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The Incidence and Costs of Job Loss: 1982–91

THERE IS A PUBLIC PERCEPTION that the nature and consequences of job loss, defined here as the involuntary (from the worker's viewpoint) termination of employment with a particular firm, have changed qualitatively in recent years. The perception is that highly skilled white-collar workers and workers with more tenure (time with their current employer) are becoming increasingly vulnerable to job loss, reduced subsequent earnings, and prolonged unemployment.[1] My goal in this study is to investigate whether and how the incidence and costs of job loss have changed in the last ten years. In other words, is the public perception correct?

Data limitations make it difficult to get a perspective on who lost jobs prior to the 1980s. Since then, however, the Displaced Workers' Surveys (DWS), which have been regular supplements to the January Current Population Survey (CPS) at two-year intervals since 1984, have provided useful information on job loss. Specifically, these surveys ask workers if in the past five years they have "lost or left a job because of a plant closing, an employer going out of business, a layoff from which [they were] not recalled or other similar reason." These data

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1. One example is Alison Leigh Cowan and James Banon, "Executives the Economy Left Behind," *New York Times*, November 22, 1992, Section 3, pp. 1, 6.

have much to tell about job loss. Augmented with data from mobility supplements in the January CPS in 1983, 1987, and 1991 and the merged outgoing rotation group CPS files from 1982 to 1991, they form the basis of my empirical analysis.

I focus on two aspects of job loss. First, I examine evidence on the incidence of job loss by worker and job characteristics, including age, education, race, sex, industry, and tenure over the period 1982–91 to determine the extent to which higher-skilled workers have, in fact, become more vulnerable to job loss. This period covers an entire business cycle, with slack labor markets in 1982–83 and 1990–91 and an intervening expansion from 1984 through 1989.[2] Additionally, this is a continuing period of sectoral shifts in the composition of employment away from goods-producing industries and toward service industries.

The second aspect of job loss I focus on is its cost to the workers who are displaced. I examine evidence on the postdisplacement employment and earnings experience of displaced workers with various characteristics in order to measure not only the costs of displacement, but also if and how these costs have changed.

Potential changes in the incidence and costs of job loss are of more than passing interest. Long-term employment relationships have played a central role in the U.S. economy, particularly for highly skilled workers.[3] This is consistent with institutional arrangements that make younger, less-skilled, and less senior workers bear the brunt of downward adjustments in employment levels. Declines in labor demand are generally accommodated by laying off the least senior workers.[4] Historically, less-skilled (blue-collar) workers have been more susceptible to

2. I use the labor economists' casual definition of a recession as a period of relatively high unemployment compared with surrounding periods. The unemployment rate was 9.5 percent in 1982 and 1983 and about 8 percent in 1992. The unemployment rate increased after 1991, so that by my definition a "recession" continued through 1992.

3. Tabulations of the mobility supplement to the January 1991 Current Population Survey show that median job tenure (time with current employer) by education level for men ages 31 to 60 was 2.4 years for men with fewer than twelve years of education, 6.1 years with twelve years of education, 6.2 years with thirteen to fifteen years of education, and 8.2 years with sixteen or more years of education. Tabulations of mobility supplements to early CPSs show a similar pattern by education category. Hall (1982) and Ureta (1992) present analyses of reported job tenure based on earlier CPSs that highlight the importance of long-term jobs.

4. Although more prevalent in the union sector, inverse seniority rules for layoff are also quite common in the nonunion sector. See Abraham and Medoff (1984).

layoffs than have been more-skilled (white-collar) workers. These institutional regularities may be an efficient response to the desire to have more job-specific capital embodied in higher-tenure and more-skilled workers. To the extent that the job security of these workers has become more tenuous, the willingness and ability of firms and workers to invest in job-specific capital is reduced, with adverse consequences for productivity.

Some amount of job change plays a positive allocative role in ensuring that workers are matched with appropriate employers and that inappropriate matches can be ended. Any job change, however, has costs in a number of dimensions. Job-specific capital is lost, and the longer the relationship that ends, the more specific capital is likely to be lost. Additionally, workers (and perhaps firms) value stability per se. Finally, termination of a job may result in periods of unemployment, which has both private and social costs. More generally, long-term employment relationships may be important components of productivity growth. This productivity growth may well go beyond the usual growth in individual productivity that is associated with aging to encompass the human-capital component of new technologies.

Changes in the incidence of job loss may be related to other social and economic phenomena of broader interest. Consider the public mood, which, as the 1992 elections showed, can have serious political, and perhaps economic, ramifications. The relatively slow growth of the economy (perhaps even a continuing recession) in 1991 and 1992 has resulted in public unhappiness with the economy that some have argued is far more extreme than warranted when measured against the "objective" economic situation in earlier recessions. For example, the so-called misery index, the sum of the unemployment and inflation rates, stood at 13.4 percent in 1983 (9.6 percent unemployment plus 3.8 percent inflation), and it was only 9.8 percent in 1991 (6.7 percent unemployment plus 3.1 percent inflation).[5] One factor that might account for this difference in mood is a difference in the incidence of job loss by age, education, and tenure groups in the recent period relative to earlier recessions. Specifically, it may be the case that because those workers who have been best protected in a "typical" recession (older workers with more education) have experienced more job loss than

5. Council of Economic Advisers (1993, tables B-37 and B-59).

usual and because these same workers may be more influential, the public mood seems darker.

With increased import competition signaling dramatic sectoral shifts in employment over the last ten to twenty years, it is plausible that older high-tenure workers are more likely to be at risk to lose their jobs than was previously the case and that existing institutional arrangements may be less effective at protecting older and higher-skilled workers than they were in the past.

Using the DWS data from 1984 through 1992 to study job loss from 1982 to 1991, I find that older and more-educated workers were relatively more likely to suffer job loss in the latter part of this period than in the early part. Nonetheless, job loss remained concentrated among younger and less-educated workers. I also find that job loss became more common in some important service industries and relatively less common in manufacturing during the latter part of the period.

Supplementing the DWS data with data from the outgoing rotation groups of the CPS, I find that displaced workers, relative to nondisplaced workers, were less likely to be employed and, if employed, were more likely to be employed part-time. These effects declined with time since displacement. There is no systematic secular change in these costs of displacement, either in the aggregate or for particular groups. Finally, I examine the earnings losses of full-time reemployed displaced workers by comparing their earnings change with the earnings change of full-time employed workers who were not displaced. I find, consistent with what others have found, that these earnings losses are substantial.

Some Data Considerations: What Is a Job Loss?

I analyze data on individuals between the ages of 20 and 64 from the DWS in 1984, 1986, 1988, 1990, and 1992 in order to investigate the incidence and costs of job loss. To my knowledge, no one before has used these data to study the incidence issue. All of the work of which I am aware using these data focuses on the postdisplacement employment and earnings experience of displaced workers.[6]

6. See, for example, Podgursky and Swaim (1987); Kletzer (1989); Topel (1990); and Gibbons and Katz (1991).

Each DWS asks workers if they were displaced from a job at any time in the preceding five-year period. Displacement is defined as involuntary separation based on operating decisions of the employer. Such events as a plant closing, an employer going out of business, or a layoff from which the worker was not recalled are considered displacement. Other events including quits and being fired for "poor work performance, disciplinary problems, or any other reason that is specific to that individual alone" are not considered displacement.[7] Workers who are laid off from a job and rehired in a different position by the same employer are considered to have been displaced. Thus, the supplement is designed to focus on the loss of specific jobs resulting from business decisions of firms unrelated to the performance of particular workers.

Job loss as measured in these data almost certainly does not represent all job loss about which I am concerned. Specifically, the distinction between quits and layoffs is not always clear. Firms may wish to reduce employment without laying off workers, and they might accomplish this by reducing or failing to raise wages.[8] This can encourage workers (perhaps those least averse to the risk of a layoff) to quit. Other workers (perhaps those most averse to the risk of a layoff) might be prone to offering to continue to work at reduced wages. To the extent that these are important phenomena, the sample of displaced workers identified by the definition used in the DWS is a potentially nonrandom subsample of "truly displaced" workers. The potential consequences of this are difficult to gauge, but it is worth noting that the ability of workers to offer wage decreases to their employers is probably quite limited.

I do not pursue distinctions among displaced workers by the cause of their displacement. Specifically, recent work by Gibbons and Katz highlights the distinction between workers displaced through layoffs and those displaced through plant closings.[9] They argue that the former group is composed of less-able workers, on average, because many

7. Bureau of the Census, *CPS Interviewer Memorandum* 80-01, January 1988 Displaced Worker Supplement, January 1988.

8. This is consistent with some recent work by Jacobson, Lalonde, and Sullivan (1991) who find that displaced workers suffer wage declines even before they are displaced.

9. Gibbons and Katz (1991).

employers have some control over who is laid off, while plant closings, by definition, involve all workers. Because Gibbons and Katz find relatively small differences in postdisplacement experience by cause of displacement and because this issue is not central to my analysis, I ignore it.

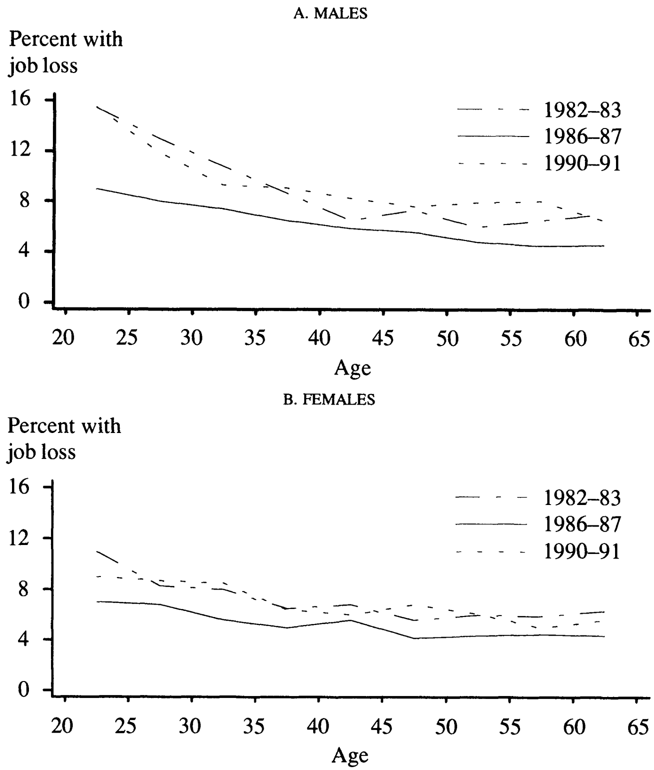
My analysis is restricted to whether a worker reports displacement within the two-year period prior to each interview. I do this for several reasons. First, if I used all five years, the recall periods would overlap. Second, workers may fail to recall job loss that occurred long before the interview date.[10] Third, the survey collects and reports information on, at most, one job loss for each individual. For workers with more than one job loss, the surveys record only the longest-held job lost. On balance, it is likely that the surveys seriously underestimate job loss that occurred long before the interview date.

The extent of this underestimation is demonstrated by a comparison of counts of overall job loss within two years of each interview (for example, 1982–83 for the January 1984 DWS) with counts of overall job loss in the same calendar period derived from interviews two years later (for example, 1982–83 for the January 1986 DWS). The weighted job loss computed for the four two-year periods from 1982 through 1989 from immediately succeeding interviews is fully 44 percent higher than the job loss computed for the same periods from interviews conducted two years later.

I use the two-year recall periods 1982–83 for the 1984 DWS, 1984–85 for the 1986 DWS, 1986–87 for the 1988 DWS, 1988–89 for the 1990 DWS, and 1990–91 for the 1992 DWS. This is a fortuitous breakdown of the 1982–91 time period. The first two years, 1982–83, represent a very slack labor market, with unemployment at 9.5 percent each year. The last two years, 1990–91, also represent a slack labor market with unemployment running above 6.5 percent by 1991. The intervening six years, 1984–90, represent a sustained expansion and a much tighter labor market. I exploit this configuration of dates to compare the incidence of job loss over the cycle and in the two periods with slack labor markets.

10. Topel (1990) presents evidence suggesting that recall bias is an important problem in the DWS.

Figure 1. Rate of Job Loss by Age and Sex



Source: Bureau of the Census, Current Population Survey, DWS, January 1984, 1988, and 1992.

The Incidence of Job Loss by Worker Characteristics

Following are summary statistics of job loss over the 1982–91 period as a function of worker characteristics, including age, sex, race, and education.

Job Loss by Age and Sex

Figure 1 contains plots of the fraction of workers by sex and age category who report a displacement in one of three two-year recall periods (1982–83, 1986–87, 1990–91).[11] These fractions are computed as the ratio of the number of workers in each age-sex-year cell who report a job loss in the relevant period divided by the number of employed individuals in the cell as of the survey date (January 1984, January 1986, or January 1988).[12] This is interpreted as the two-year rate of job loss.

The results for males, contained in the top panel, show that younger workers have substantially higher rates of job loss than do older workers. They also show that rates of job loss were generally higher for males in the two slack labor markets. The cyclical difference is particularly striking for younger males. As expected, the rate of job loss for young workers was significantly higher in both recessions than it was in the years of expansion. The cyclical differences are less pronounced for older males, though they are generally significantly different from zero.[13]

The 1990–91 recession shows a somewhat different pattern from the 1982–83 recession. The rate of job loss for younger workers in 1990–91 was higher than in the boom period but not quite as high as in 1982–83. The more striking finding is that the rate of job loss among older workers in the 1990–91 period was not only higher than in the boom period but was also much higher than in the 1982–83 recession (8 percent compared with 6.2 percent). This suggests that job loss in the recent period is, in fact, of a different character than earlier job loss. Male workers ages 45 to 60 were significantly more likely to lose their jobs in the 1990–91 period than in the 1982–83 period.

The bottom panel of figure 1 contains the same job loss information for females and shows a pattern different from that of males. First, note

11. The other two recall periods (1984–85 and 1988–89) are not included in order to keep the graph clear. These two periods, both part of the 1980s expansion, look very much like 1986–87.

12. All cell counts are weighted by the CPS final weights. It would be preferable to divide the number of job losers by the number of workers at risk for job loss during the recall period rather than by employment at the end of the recall period, but there is no convenient measure of the number of workers at risk. I also calculated job loss rates on a population base rather than an employment basis. These rates are predictably lower and vary inversely with employment rates, but they generally show the same patterns otherwise.

13. The standard errors on these differences in rates of job loss are all less than 0.6 percent, so differences of more than 1.2 percent are statistically significantly different at the 0.05 level.

that females have significantly lower job loss rates than men (7.8 percent overall for men and 6.2 percent overall for women, p-value = 0.001). The job loss rates for females were higher for virtually all age categories in both recessions than in the intervening expansion. A joint test of the hypothesis that the 1982–83 and 1990–91 recessionary periods had job loss rates no different from the intervening expansion results in clear rejection of that hypothesis (p-value < 0.001). There seems to be no overall qualitative difference in job loss rates for females between the two recessionary periods.

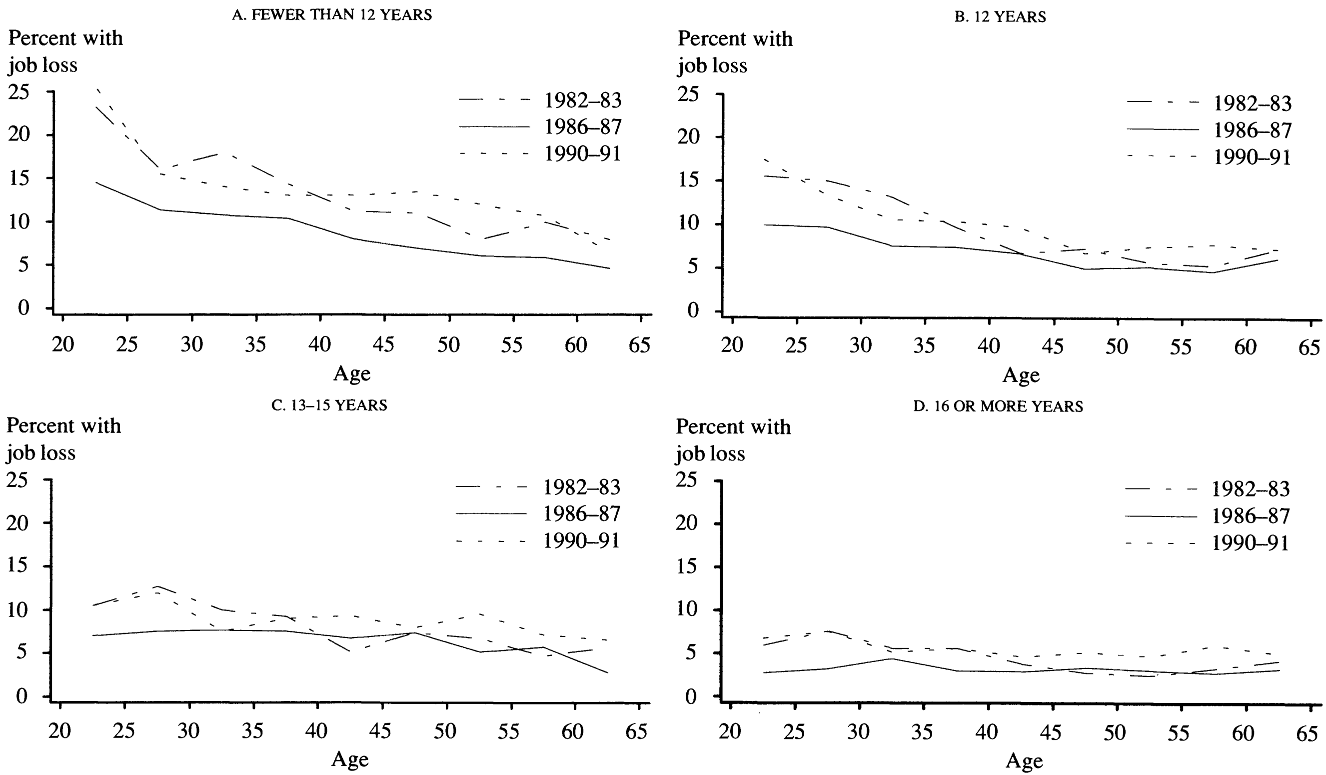
Job Loss by Level of Education

Consider next how rates of job loss vary by education level. Figure 2 shows employment-based rates of job loss by years of education and age for males in the three two-year recall periods. The most obvious fact is that rates of job loss are strongly negatively related to education level. The average rate of job loss (across all age groups and years) for males with fewer than twelve years of education is 10.8 percent, which is more than twice the job loss rate of 4.3 percent for males with at least sixteen years of education. Another important fact is that the rate of job loss declines with age most prominently for less-educated males. The rate of job loss for males with at least sixteen years of education is relatively low even at young ages and does not decline with age.

Rates of job loss for younger males in all educational categories are lower in the expansion than in either of the recessionary periods. The rate of job loss for older males in 1986–87 is substantially below the rates in the recessionary periods only for males with fewer than twelve years of education. Older men (ages 40 to 60) in all educational categories have somewhat higher rates of job loss in the 1990–91 period relative to the 1982–83 and 1986–87 periods. For example, the average rate of job loss among males 40 to 60 years of age with at least sixteen years of education was 5 percent in 1990–91, compared with 3.9 percent in 1982–83 (p-value of difference = 0.001).[14]

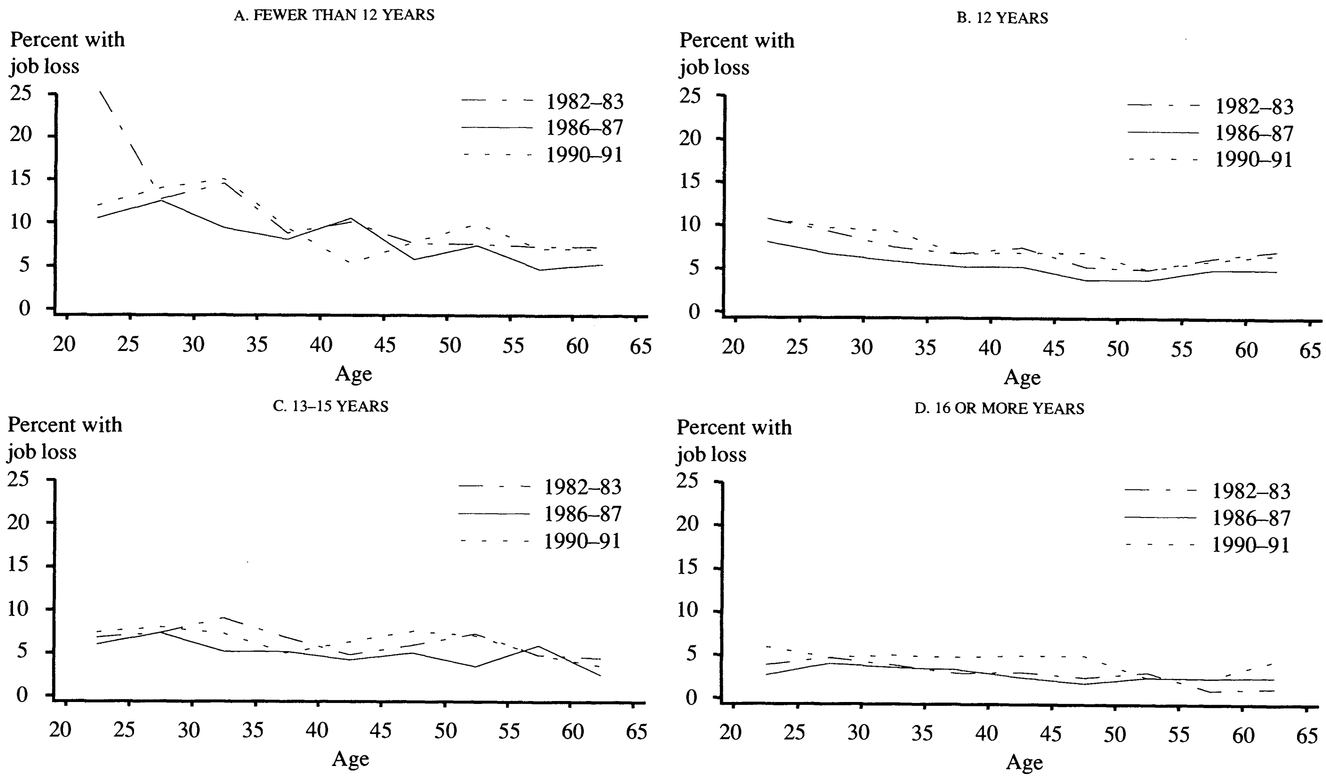
14. These differences do not look particularly large in the figures for the more-educated groups because of the requirement that the vertical scale of the graph be the same across all four educational categories. The generally low rates of job loss for highly educated workers combined with a scale that must accommodate the high rates of job loss for less-educated workers visually attenuates relatively large proportional differences for more-educated workers.

Figure 2. Rate of Job Loss Among Males by Years of Education



Source: DWS, January 1984, 1988, and 1992.

Figure 3. Rate of Job Loss Among Females by Years of Education



Source: DWS, January 1984, 1988, and 1992.

Figure 3 shows two-year employment-based rates of job loss by education group and age category for females. The patterns are less consistent than those for males. Females with twelve years of education have systematically lower rates of job loss in the expansion than in the periods with slack labor markets. Females 35 to 50 years old with at least a college education had significantly higher rates of job loss in the second recession than similar workers did in either the first recession or the expansion. This suggests that women with more education were not particularly hard hit by the earlier recession but that they suffered substantially more job loss in the recent recession.

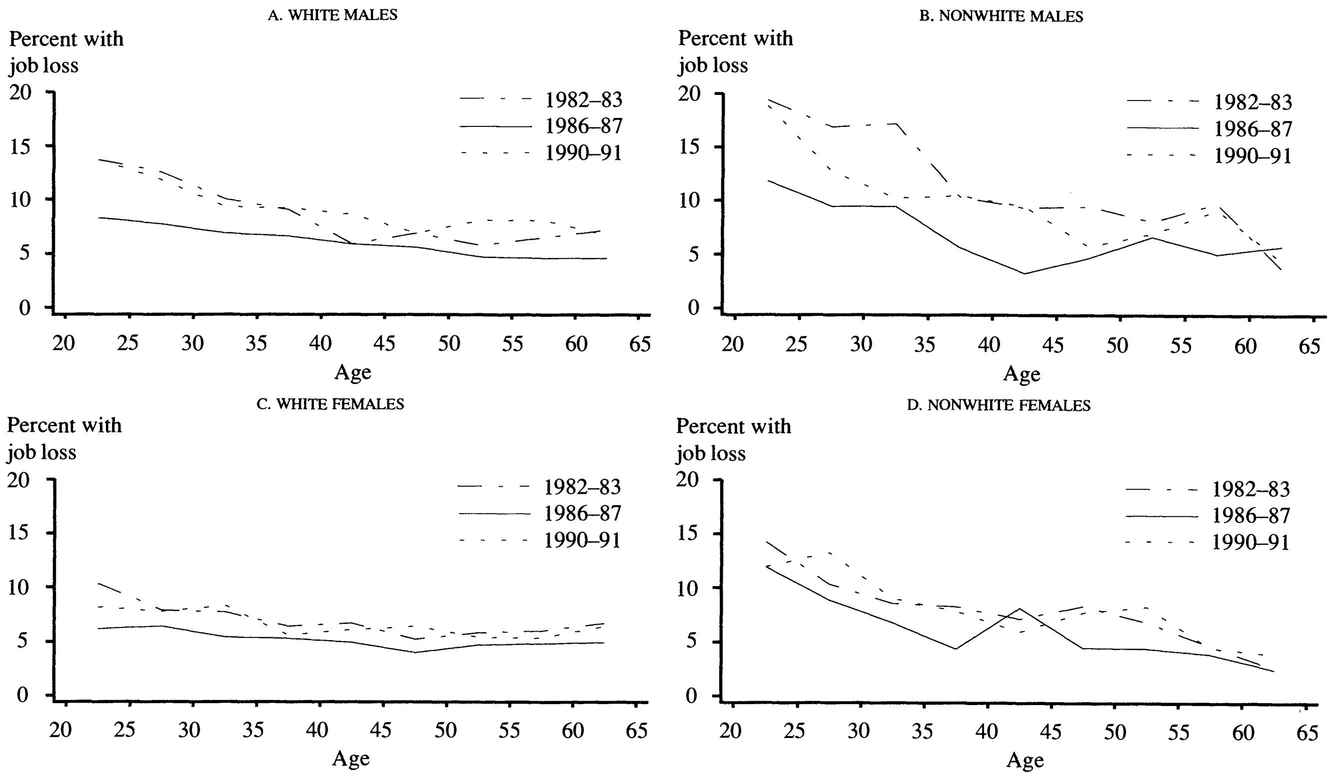
Job Loss by Race

Figure 4 contains plots of rates of job loss by sex and race category for the three two-year periods. Particular care must be taken in interpreting the plots for nonwhites because they are based on much smaller samples than the plots for whites, and, hence, the sampling errors are larger. Nonetheless, it is clear that younger nonwhites have higher rates of job loss than comparably aged whites. There is no significant difference for older workers. For example, the average job loss rate for men under age 40 is 8.9 percent for whites and 11.7 percent for nonwhites (p-value of difference = 0.025). In contrast, the average job loss rate for men 40 and older is 5.9 percent for whites and 6.4 percent for nonwhites (p-value of difference = 0.423). As noted above, women have lower rates of job loss overall, but the pattern by race and age are the same as for men. The average job loss rate for women under 40 is 6.7 percent for whites and 9.1 percent for nonwhites (p-value of difference = 0.002), while the average job loss rate for women 40 and older is 5.3 percent for whites and only 5.1 percent for nonwhites (p-value of difference = 0.665).

The rate of job loss for nonwhites was not noticeably higher in the recent slack period compared with the earlier slack period. The plots in figure 4 show that the higher rates of job loss in 1990–91 relative to 1982–83 were concentrated among older white males.

Overall, the figures demonstrate that the strong cyclical nature of rates of job loss is concentrated largely among younger and less-educated workers. The plots also show that rates of job loss are strongly inversely related to education and age. Younger males have lower rates

Figure 4. Rate of Job Loss by Race and Sex



Source: DWS, January 1984, 1988, and 1992.

of job loss than do younger females, and younger nonwhites have higher rates of job loss than younger whites.

It is true that rates of job loss for older, more-educated workers were proportionally much higher in the most recent recession than in the earlier recession. Although this increase in vulnerability to job loss of a group of workers with historically low rates of job loss has attracted a lot of attention, it must be kept in perspective with the historically high rates of job loss suffered by younger and less-educated workers. The new higher rates of job loss among older, more-educated workers in the 1990–91 period were still substantially lower than the rates of job loss suffered by younger, less-educated workers in the 1980s boom. Job loss continues to fall disproportionately on workers at the low end of the skill distribution.

The Locus of Job Loss by Industry

It is difficult to use the DWS data to study the relationship between job attributes and the probability of job loss because the data are not truly longitudinal. Specifically, all workers are not asked about their jobs at one point in time and then reinterviewed at a future date to find out about job loss. Rather, workers are interviewed at one point in time and asked whether they have lost a job in the recent past (the recall period). Only if workers had been displaced were they asked about the job that they were at risk to lose. Thus, no information is available at the individual level on characteristics of jobs (for example, industry, occupation, tenure) held during the recall period unless the workers either were displaced or did not change jobs voluntarily.

This is a weakness of using purely cross-sectional data to study job loss. However, it is possible to construct the probability of job loss by industry using Bayes's rule to combine industry employment shares, industry job-loss shares (the fraction of job loss in a given year that comes from each industry), and the overall probability of displacement. On this basis, the probability that worker i in industry j is displaced in period t is

$$(1) \quad Pr(D_{it}=1|I_{it}=j) = PR(I_{it}=j|D_{it}=1)\cdot Pr(D_{it}=1)/Pr(I_{it}=j),$$

where D_i represents a binary variable that equals 1 if worker i suffers a job loss in period t and I_{it} equals j if worker i is employed in industry j in period t.

Each of the three probabilities required to calculate $Pr(D_{it}=1|I_{it}=j)$ are easily computed. The first piece, $Pr(I_{it}=j|D_{it}=1)$, is computed directly as the industrial distribution of workers displaced in period t. The second piece, $Pr(D_{it}=1)$, is computed as the unconditional probability that an individual is displaced in period t. The last piece, $Pr(I_{it}=j)$, is computed as the industrial distribution of employment, and, for convenience, I use the industrial distribution of employment as of the January CPS containing the relevant DWS (for example, January 1984 for the 1982–83 recall period). Although the results will differ somewhat from the basically unobservable (and variable) industrial distribution of employment during the recall period, the differences are likely to be minor.

Table 1 contains data for each of the five two-year periods on industry employment shares, the industrial distribution of displaced workers, and the probability of job loss by industry as computed using equation 1.[15] It is clear from this table that the recent years are different from the earlier years. First, consider the share of job loss by industry in the middle panel of table 1. Throughout the 1982–91 period, manufacturing workers disproportionately lost their jobs, in that manufacturing's share of total job loss exceeded manufacturing's employment share in every period.[16] However, job loss was substantially less concentrated in manufacturing after 1985. In the 1982–85 period, about three-eighths of all job loss was from manufacturing employment. In the 1986–91 period about one-fourth of all job loss was from manufacturing. Three industry groups—trade; finance, insurance, and real estate; and professional services—each had an increase in the share of job loss of three to four percentage points to make up the difference.

Next examine the probability of job loss by industry in the bottom

15. The overall probabilities of displacement, $Pr(D_{it}=1)$, are 0.0858 for 1982–83, 0.0689 for 1984–85, 0.0608 for 1986–87, 0.0550 for 1988–89, and 0.0832 for 1990–91. These are computed from weighted tabulations of the DWSs each year as the ratio of the two-year job loss rate to the employment rate.

16. This is not the same thing as saying that manufacturing's share of total employment fell in every period. Although this is also true, the job loss measure does not account for new hiring or for voluntary job change. Davis and Haltiwanger (1992) present an analysis of net job change in manufacturing using establishment data.

Table 1. Incidence of Job Loss by Industry
Percentage

Industry	Employment shares (January CPS)				
	1984	*1986*	*1988*	*1990*	*1992*
Manufacturing	.2010	.1982	.1852	.1836	.1745
Transportation, communications	.0728	.0733	.0751	.0728	.0736
Trade	.1891	.1858	.1875	.1871	.1871
Financial services	.0651	.0686	.0714	.0693	.0693
Nonprofessional services	.0976	.1027	.1085	.1104	.0951
Professional services	.2181	.2189	.2251	.2251	.2607
Other	.1563	.1525	.1471	.1491	.1396
	Share of job loss (January CPS-DWS)				
Industry	*1982–83*	*1984–85*	*1986–87*	*1988–89*	*1990–91*
Manufacturing	.3663	.3561	.2457	.2525	.2485
Transportation, communications	.0692	.0817	.0647	.0609	.0569
Trade	.1692	.1615	.2345	.2330	.2194
Financial services	.0242	.0290	.0488	.0566	.0626
Nonprofessional services	.1051	.1157	.1103	.1146	.1098
Professional services	.0602	.0636	.0933	.0977	.1075
Other	.2057	.1924	.2027	.1848	.1954
	Probability of job loss (Equation 1)				
Industry	*1982–83*	*1984–85*	*1986–87*	*1988–89*	*1990–91*
Manufacturing	.1564	.1238	.0807	.0756	.1185
Transportation, communications	.0816	.0768	.0524	.0460	.0643
Trade	.0768	.0599	.0760	.0685	.0976
Financial services	.0319	.0291	.0416	.0449	.0752
Nonprofessional services	.0924	.0776	.0618	.0571	.0961
Professional services	.0237	.0200	.0252	.0239	.0343
Other	.1129	.0869	.0838	.0682	.1165

Source: Based on tabulations of DWS, January 1984, 1986, 1988, 1990, and 1992. See text for details.

panel of table 1. Although job loss is predictably higher in the two recessionary periods, the probability of job loss in manufacturing was almost four percentage points lower in 1990–91 than in 1982–83. The probability of job loss was more than two percentage points higher in trade in 1990–91 than in 1982–83, and more than four percentage points higher in finance, insurance, and real estate (more than doubling). Thus, job loss has become relatively more common in important and growing non-goods-producing industries.

Job Loss by Tenure, Education, Sex, and Age: A Multivariate Analysis

A key component of the perceived change in the locus of job loss is that individuals with higher tenure are now more at risk for job loss than they were. Unfortunately, as with industry, tenure during the recall period is not observed for workers who do not report that they have been displaced. I can use the same Bayes's rule approach to recover the probability of job loss by tenure category that I used to compute the probability of job loss by industry. The appropriate relationship defining the probability that worker i in tenure category j is displaced in period t is

$$(2)\quad Pr(D_{it}=1|T_{it}=j) = Pr(T_{it}=j|D_{it}=1)\cdot Pr(D_{it}=1)/Pr(T_{it}=\mathrm{j}),$$

where D_{it} equals 1 if worker i in period t suffers a job loss and $T_{it}=j$ if worker i is in tenure category j in period t.

The only piece of equation 2 that is not available directly from the DWS is $Pr(T_{it}=j)$, the unconditional tenure distribution. However, the January CPS in the years immediately prior to the 1984, 1988, and 1992 DWS contains mobility supplements that have information on job tenure for all workers, and I use this information to compute unconditional tenure distributions in January 1983, 1987, and 1991. These mobility supplements are ideally timed because they fall almost precisely in the middle of the 1982–83, 1986–87, and 1990–91 recall periods associated with the 1984, 1988, and 1992 DWSs, respectively. Unfortunately, I have no information on the unconditional distribution of job tenure that is appropriate for the 1984–85 or 1988–89 recall periods.

Calculations identical to those performed above for industry yield the unsurprising result that the probability of job loss is strongly monotonically declining in tenure. Although not presented here, the probability of job loss (averaged over the six years) falls from about 10 percent for workers in their first year on the job to less than 3 percent for workers with more than ten years on the job. The results are more remarkable for what is not found. There seems to be no substantial difference in the tenure-specific probability of job loss between the two recessionary periods. The earlier finding, that older workers fared worse in the recent downturn than in the 1982–83 downturn, suggested that workers with more tenure (who are also older on average) would also fare worse in the more recent period. That does not seem to be the case, which implies indirectly that the older workers with relatively low tenure conditional on their age were relatively more likely to suffer job loss in the most recent period than such workers were in the earlier period.

Covariation among age, education, and tenure in determining the rate of job loss suggests that a multivariate analysis of job loss would be useful. Without longitudinal data on all variables, however, a microlevel analysis is not feasible. I can nevertheless compute cell mean level rates of job loss using the Bayes's rule approach. I compute job loss rates for groups defined by education, tenure, age, sex, and time period. There are 1,608 cells, defined by these five variables, for which I have computed job loss rates using information on individual characteristics and job displacement from the 1984, 1988, and 1992 DWSs combined with the data from the mobility supplements to the January CPSs in 1983, 1987, and 1991. Unfortunately, cell sizes are too small if I expand the breakdown to include race or industry.

The job loss rates for each category are computed using Bayes's rule applied to the conditional probabilities associated with job loss, tenure, sex, and education. It is straightforward to show that

$$(3) \quad Pr(D_{it}=1|T_{it}=j, S_{it}=k, A_{it}=r, E_{it}=m)=D_{ijkrmt}$$
$$= Pr(T_{ikrmt}=j|D_{it}=1) \cdot Pr(D_{ikrmt}=1)/Pr(T_{ikrmt}=j),$$

where $T_{it}=j$ if individual i is in tenure category j in year t, $S_{it}=k$ if individual i is of sex k (in year t for symmetry), $A_{it}=r$ if individual i is in age category r in year t, and $E_{it}=m$ if individual i is in educational category m in year t. The quantity $D_{ijkrmt}=1$ if individual i in tenure category j, of sex k, in age category r, and with education level m in year t is displaced from his or her job. $T_{ikrmt}=j$ if individual i of sex k in age category r with education level m is in tenure category j in period t. Finally, $D_{ikrmt}=1$ if individual i of sex k with education level m in age category r is displaced in period t. This particular representation of the conditional probability of job loss is used because it allows computation of the probability of displacement conditional on tenure and the other controls without direct information on the joint distribution of displacement and tenure.

Preliminary analysis of these data shows the usual result that the intermediate time period, because it is in the middle of an expansion, has lower rates of job loss, particularly for workers with lower tenure levels. The interesting question is whether the incidence of job loss changed systematically over the ten-year period. For this reason, the analysis in this subsection focuses only on the 1982–83 and 1990–91 periods.

Table 2 contains estimates of weighted least squares (WLS) regressions of the log of the probability of job loss in a cell on the characteristics defining the cell. The weights used for each cell are based on estimates of the variance of the log probability of job loss for that cell. The log of the probability of job loss in equation 3 is a linear combination of the logs of its component probabilities. Specifically,

$$\ln\left(D_{ijkrmt}\right) = \ln\left[Pr(T_{ikrmt}=j|D_{it}=1)\right] + \ln\left[Pr(D_{ikrmt}=1)\right] - \ln\left[Pr(T_{ikrmt}=j)\right]. \tag{4}$$

The variance of each component probability is $p(1-p)/n$, where p is the relevant probability and n is the sample size on which the estimate of the probability is based. The variance of the log of each probability is computed using the delta method as $(1-p)/pn$, and the variance of $\ln(D_{ijkrmt})$ is computed as the sum of the variances of the log of the three component probabilities.

Specification 1 in table 2 contains estimates of a model of the log probability of job loss with main effects for sex, age category, education level, tenure category, and time period. I have also allowed for interaction between education level and time period. These results demonstrate the overwhelming negative relationship between the probability of job loss and tenure. Workers with more than fifteen years' tenure have a probability of job loss that is less than 25 percent of the rate of job loss for workers in their first year on the job. The results also corroborate the earlier finding that females have lower rates of job loss than men (about 70 percent). The estimates suggest that workers with at least some college have a lower rate of job loss than do workers with less education. The rate of job loss for college graduates is only 53 percent of the rate of job loss of otherwise equivalent high school graduates (the base group).

Table 2. WLS Estimates of Log Probability of Job Loss by Sex, Education, Tenure, Age, and Year Cells, 1982–83 and 1990–91

Variable	Weighted mean	Specification (1)	Specification (2)
Constant	1.0	−1.54 (0.0337)	−1.52 (0.0384)
1990–91	0.504	−0.0095 (0.0339)	−0.0508 (0.0539)
Female	0.448	−0.354 (0.0225)	−0.352 (0.0224)
Education < 12 yrs.	0.219	0.311 (0.0413)	0.325 (0.0414)
Education 13–15 yrs.	0.241	−0.179 (0.0415)	−0.184 (0.0414)
Education ≥ 16 yrs.	0.203	−0.638 (0.0482)	−0.636 (0.0482)
Education < 12 yrs. ·1990–91	0.0987	0.0130 (0.0628)	−0.0080 (0.0629)
Education 13–15 yrs. ·1990–91	0.130	0.0195 (0.0570)	0.0229 (0.0568)
Education ≥ 16 yrs. ·1990–91	0.108	0.146 (0.0656)	0.138 (0.0657)
Tenure 1 yr.	0.146	−0.406 (0.0347)	−0.405 (0.0346)
Tenure 2 yrs.	0.130	−0.350 (0.0384)	−0.348 (0.0383)
Tenure 3–4 yrs.	0.142	−0.641 (0.0367)	−0.642 (0.0365)
Tenure 5–9 yrs.	0.140	−0.952 (0.0381)	−0.952 (0.0380)
Tenure 10–14 yrs.	0.0821	−1.22 (0.0541)	−1.22 (0.0539)
Tenure 15–19 yrs.	0.0538	−1.38 (0.0738)	−1.37 (0.0736)
Tenure 20–24 yrs.	0.0377	−1.45 (0.0952)	−1.45 (0.0950)
Tenure ≥ 25 yrs.	0.0359	−1.62 (0.0885)	−1.61 (0.0883)
Age 25–29	0.128	−0.0162 (0.0356)	0.0023 (0.0479)

Table 2. (continued)

Variable	*Weighted mean*	*Specification* (1)	(2)
Age 30–34	0.132	−0.0048 (0.0380)	0.0049 (0.0516)
Age 35–39	0.126	−0.0662 (0.0411)	−0.0746 (0.0573)
Age 40–44	0.118	−0.0745 (0.0450)	−0.169 (0.0652)
Age 45–49	0.111	0.0047 (0.0492)	−0.0896 (0.0706)
Age 50–54	0.0986	0.0402 (0.0544)	−0.0486 (0.0755)
Age 55–59	0.0917	0.0776 (0.0582)	−0.0260 (0.0779)
Age 60–64	0.0772	0.208 (0.0672)	0.187 (0.0881)
Age 25–29 ·1990–91	0.0619	—	−0.0365 (0.0703)
Age 30–34 ·1990–91	0.0652	—	−0.0140 (0.0736)
Age 35–39 ·1990–91	0.0647	—	0.0240 (0.0792)
Age 40–44 ·1990–91	0.0645	—	0.171 (0.0866)
Age 45–49 ·1990–91	0.0601	—	0.173 (0.0939)
Age 50–54 ·1990–91	0.0509	—	0.173 (0.103)
Age 55–59 ·1990–91	0.0453	—	0.213 (0.109)
Age 60–64 ·1990–91	0.0366	—	0.0403 (0.127)
R^2		0.701	0.705
Standard error of estimation		0.361	0.360

Source: Author's tabulation based on DWS, January 1984, 1991, and 1992; and January Mobility Supplements to Bureau of the Census, Current Population Survey, January 1983, 1991, and 1992. The sample consists of 1,072 sex-education-age-tenure-year cells derived using data on individuals aged 20–64. The base cell contains males 20–24 years old with less than one year of tenure and twelve years of education in 1982–83. The means are weighted by the square root of the inverse variance of the log probability of job loss in each cell. This is the estimation weight as described in the text. All variables are dummy variables. The weighted mean of the dependent variable is −2.21. The numbers in parentheses are standard errors.

— Not applicable.

An interesting result is that when tenure is controlled for, the relationship between age and the rate of job loss is not strong. The finding in figures 1 through 4 that older workers have lower rates of job loss seems to be due largely to the positive correlation between age and tenure.

The results indicate that for the base group of high school graduates, the rate of job loss in the 1990–91 period was not significantly different from the rate in the 1982–83 period (the base period). However, there is a significant positive interaction between the 1990–91 dummy variable and the dummy for sixteen or more years of education. The point estimate suggests that, other things equal, college graduates were about 15 percent more likely to lose their jobs in the 1990–91 period than in the 1982–83 period. None of the other time-education interactions are significant. Thus, workers who were not college graduates fared about the same in the two recessions, other things equal.

Specification 2 in table 2 contains estimates of a model that includes age-year interactions along with the main effects and the education-year interactions. I present these estimates because of the finding in table 1 that older workers had higher rates of job loss in the 1990–91 period than in the 1982–83 period. The multivariate analysis strongly supports this result. Workers between ages 40 and 60 had a significantly higher probability of job loss in 1990–91 than otherwise equivalent workers in 1982–83. The point estimates suggest that turnover rates among older workers were about 18 percent higher in the recent period than in the earlier period. Older workers were at greater risk for job loss in 1990–91, even controlling for tenure.

I also estimated a model that included tenure-year interactions along with the main effects, the education-year interactions, and the age-year interactions. The point estimates of the tenure-year interactions are not presented, but none of them is significantly different from zero. A test of the null hypothesis that the tenure-year interactions are all zero cannot be rejected (p-value $= 0.530$). None of the substantive results alluded to above is altered by the inclusion of the tenure-year interactions, and this suggests that older workers, per se, were at higher risk for job loss in 1990–91 relative to 1982–83 and that higher tenure workers (controlling for age) were not at greater risk.

Remarks on Changes in the Incidence of Job Loss

What can be concluded from the analysis in preceding sections? First, there is clear evidence that workers who were older and had more education were relatively more likely to lose their jobs in the most recent period than in the earlier years. Second, although the probability of job loss is highly negatively correlated with tenure, there is no evidence that workers with more tenure were more vulnerable to job loss in the most recent period than they were in the earlier recession.

This pattern of results raises as many questions as it answers. The public perception that the incidence of job loss was different in the 1990–91 recession than it was in at least the immediately preceding recession has some basis in fact. To the extent that older workers and workers with more education have relatively more influence on the public mood, it would not be surprising for the public to be more "unhappy" with this recession than the last. This is consistent with weighted counts of the 1984 and 1992 DWS, which show that about 14 percent more jobs were lost in the 1990–91 period than in the 1982–83 period (8.6 million compared with 7.5 million). In the 1982–83 recession, however, the unemployment rate was much higher (9.5 percent compared with 6.0 percent) and the rate of employment growth was much lower than in the 1990–91 period (−1.0 percent compared with 0.0 percent).[17]

The finding that job loss in the 1982–83 period was relatively concentrated in manufacturing, while the job loss in the 1990–91 period was relatively concentrated in a collection of service industries, suggests that adjustments have been concentrated in different sectors in the two periods. Employment in goods-producing industries fell by 8.8 percent between 1978 and 1983 and by only 3.0 percent between 1986 and 1991. Employment in service-producing industries grew by 9.4 percent between 1978 and 1983 and by 12.7 percent between 1986 and 1991.[18] Clearly, the adjustment in the goods-producing sector was much more severe in the earlier period, and, given the skill composition of em-

17. Council of Economic Advisers (1992, p. 344).
18. Council of Economic Advisers (1993, p. 395).

ployment by industry, its effects were disproportionately borne by less-skilled workers.

The finding that the rate of job loss by tenure category was no different in the two recessions suggests that plant shutdowns are not contributing more to job loss than they did in the past. Some direct evidence is available from the DWS that is consistent with this. The DWS asks displaced workers the cause of their job loss. Plant closings accounted for 30.7 percent of job loss in 1982–83, approximately 34 percent in 1984–89, and 29.4 percent in 1990–91. There is no significant difference between the two recessions in the fraction of job loss that was due to plant closings (p-value > 0.3). It is interesting to note that the fraction of job loss due to plant closings was significantly higher during the expansion than in either recession. Because total job loss is higher in the recessionary periods, however, the absolute number of jobs lost to plant closings is higher in the recessionary periods than in the expansion.

Costs of Job Loss: Postdisplacement Labor Force Status

Earlier work using the DWS data concludes that displaced workers, on average, suffer earnings losses in the form of both unemployment and lower wages upon reemployment.[19] In this section I examine the labor force status at the survey date of more than 420,000 individuals between the ages of 20 and 64 who were part of one of the five January CPSs containing the DWSs from 1984 to 1992, whether they were displaced or not. Approximately 20,000 of these workers reported being displaced within two years of one of the urveys.[20] I use these data to examine how displacement is related to labor force status by comparing the distribution of labor force status among workers who reported being displaced to the distribution of labor force status among workers who did not report being displaced.[21]

19. See, for example, Podgursky and Swaim (1987); and Topel (1990).

20. I delete a small number of workers who report being displaced in the month of the survey because they have had little time to find a new job.

21. Another outcome that would be interesting to investigate is time spent unemployed after being displaced. Unfortunately, a change in the relevant question asked in the DWS between the 1986 and 1988 surveys makes a meaningful comparison impossible. In the

Comparing the probabilities of displaced workers being in the various labor force states with the same probabilities for individuals who were not displaced provides a useful basis for judging the effects of displacement. There is an important difficulty with this approach, however. Workers who were displaced had to have been employed at some point within the last two years, while those individuals who were not displaced may be consistently out of the labor force. The comparison is thus biased toward finding a smaller adverse employment effect of displacement, and the results must be interpreted with this in mind.

Table 3 contains tabulations of labor force status at the time of the DWSs for all individuals, broken down by sex, survey year, and whether the individual reported being displaced within two years of the survey date. Notwithstanding the potential bias noted above, I find dramatically lower employment probabilities in every year for displaced males compared with nondisplaced males. The difference in employment probabilities for females is also negative, although by a smaller amount. Unemployment probabilities for both sexes in all years are almost an order of magnitude higher for displaced workers than for nondisplaced workers. Nondisplaced workers of both sexes are more likely to be out of the labor force than displaced workers, and this is almost certainly due to the bias induced by not controlling for past employment among the nondisplaced workers.

The tabulations in table 3 also clearly show that the probability of employment (controlling for displacement status) for both males and females is significantly higher in the survey years following periods of expansion than in the survey years following recessionary periods. Not surprisingly, the probability of unemployment moves inversely with the probability of employment. The probability of being out of the labor force is rather small for men, and it does not vary much over time. Displaced women are much more likely to be out of the labor force than men, but this probability is falling over time, a pattern consistent with the increasing commitment of women to the labor force.

A comparison of the displacement-nondisplacement difference in the probabilities of employment and unemployment at the 1984 and 1992

1984 and 1986 DWSs, displaced workers were asked how much time they had spent unemployed since being displaced. In the 1988, 1990, and 1992 DWSs, displaced workers were asked how much time they were unemployed before they started another job.

Table 3. Labor Force Status by Displacement, Sex, and Year as of January of Survey Year

Weighted column percentage

	Males — *Displaced within two years of January?*									
Labor force status	*1984*		*1986*		*1988*		*1990*		*1992*	
	No	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Employed	0.828	0.577	0.841	0.599	0.843	0.646	0.843	0.665	0.826	0.550
Unemployed	0.052	0.358	0.043	0.322	0.038	0.286	0.039	0.269	0.048	0.380
Not in labor force	0.121	0.065	0.117	0.079	0.119	0.068	0.118	0.066	0.126	0.070
Number	37,519	3,028	37,901	2,443	37,594	2,135	37,763	1,855	36,704	2,918

	Females — *Displaced within two years of January?*									
Labor force status	*1984*		*1986*		*1988*		*1990*		*1992*	
	No	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Employed	0.593	0.486	0.619	0.550	0.644	0.590	0.664	0.595	0.660	0.531
Unemployed	0.037	0.303	0.034	0.238	0.030	0.223	0.028	0.221	0.032	0.293
Not in labor force	0.370	0.210	0.347	0.213	0.326	0.188	0.308	0.184	0.308	0.176
Number	42,527	1,806	42,608	1,493	42,391	1,447	42,115	1,410	41,522	1,879

Source: Author's tabulations based on data reported in DWS, January 1984, 1986, 1988, 1990, and 1992. The CPS final sampling weights are used as weights.

interview dates shows that males' penalty for displacement in this dimension was larger in 1992 than it was in 1984. The male 1992–84 difference in employment probabilities is -0.026 (p-value $= 0.007$). The difference in unemployment probabilities precisely offsets this at 0.026 (p-value < 0.001). Thus, displacement is estimated to result in lower employment probabilities and higher unemployment probabilities for males in the recent recession relative to the 1982–83 period. None of these differences in differences are significantly different from zero for females, so that the penalty for job loss was the same for females in both recessions.[22]

While a multinomial logit specification would normally be appropriate to analyze labor force states because of the unordered discrete outcomes being considered (employment, unemployment, out of the labor force), my preliminary analyses suggest that the relevant variation in the data is summarized efficiently by a simple logit on the probability of employment. Because these estimates are easier to interpret than multinomial estimates, the remainder of the analysis in this section focuses on the probability of employment using the simple logit model.

Table 4 contains estimates of simple logit models of the probability of employment separately for males and females. The sample includes all individuals between the ages of 20 and 64, whether displaced or not. The model includes main effects for race, education, age, and year, along with interactions of these variables with whether the individual suffered a job loss. By fully interacting the worker characteristics with the job loss variable, I get the same fit as if I had estimated separate logit models for displaced and nondisplaced workers. The advantage to the combined estimation is that estimates are produced directly of the difference between displaced and nondisplaced workers in the relationship of the probability of employment with each of these characteristics. The "main effect" columns contain the estimates of the relationship for nondisplaced workers of worker characteristics with the probability of employment. The "interact with loss" columns contain estimates of the difference between displaced and nondisplaced workers

22. In fact, the probability of employment for displaced women is larger in 1991–92 than in 1982–83, but the increase is smaller than the increase in employment probabilities for nondisplaced women over the same period.

Table 4. Logit Estimates of Probability of Employment: Effect of Job Loss

	Males			*Females*		
Variable	*Weighted mean*	*Main effect*	*Interact with loss*	*Weighted mean*	*Main effect*	*Interact with loss*
Constant	1.0	1.23 (0.0217)	−1.31 (0.0683)	1.0	0.428 (0.0170)	−0.616 (0.0818)
Displaced in second year prior to survey	—	—	1.12 (0.0430)	—	—	0.720 (0.0508)
Nonwhite	0.134	−0.648 (0.0172)	−0.106 (0.0606)	0.152	−0.0756 (0.0134)	−0.637 (0.0801)
Education < 12 yrs.	0.176	−0.719 (0.0170)	0.395 (0.0556)	0.170	−0.865 (0.0133)	0.282 (0.0740)
Education 13–15 yrs.	0.215	−0.0699 (0.0179)	0.392 (0.0563)	0.219	0.254 (0.0127)	0.216 (0.0639)
Education ≥ 16 yrs.	0.238	0.631 (0.0211)	0.101 (0.0679)	0.183	0.689 (0.0146)	0.203 (0.0287)
Age 25–29	0.150	0.913 (0.0239)	−0.731 (0.0701)	0.147	0.123 (0.0181)	−0.105 (0.0856)
Age 30–34	0.150	1.13 (0.0208)	−1.10 (0.0729)	0.147	0.0980 (0.0181)	−0.219 (0.0856)
Age 35–39	0.134	1.23 (0.0271)	−1.18 (0.0778)	0.132	0.235 (0.0189)	−0.317 (0.0913)

Age 40–44	0.112	1.20	−1.30	0.111	0.355	−0.319
		(0.0285)	(0.0847)		(0.0200)	(0.0965)
Age 45–49	0.091	1.11	−1.16	0.091	0.287	−0.418
		(0.0294)	(0.0931)		(0.0209)	(0.106)
Age 50–54	0.079	0.787	−1.11	0.080	0.0038	−0.559
		(0.0282)	(0.0981)		(0.0210)	(0.120)
Age 55–59	0.076	0.180	−0.724	0.079	−0.405	−0.182
		(0.0251)	(0.104)		(0.0207)	(0.126)
Age 60–64	0.073	−0.901	−0.341	0.079	−1.15	−0.0227
		(0.0231)	(0.125)		(0.0214)	(0.152)
1986	0.196	0.0792	0.105	0.197	0.0926	0.219
		(0.0214)	(0.0657)		(0.0152)	(0.0801)
1988	0.201	0.0554	0.297	0.201	0.186	0.233
		(0.0212)	(0.0676)		(0.0153)	(0.0809)
1990	0.204	0.0304	0.486	0.204	0.249	0.211
		(0.0212)	(0.0710)		(0.0153)	(0.0816)
1992	0.211	−0.134	0.0664	0.209	0.195	−0.0700
		(0.0208)	(0.0614)		(0.0153)	(0.0751)
Sample size		201,870			214,767	
Log likelihood		−81,595.1			−130,301.6	

Source: Author's calculations based on data reported in DWS, January 1984, 1986, 1988, 1990, and 1992. All data are weighted by the CPS final sampling weights. The weighted means of the dependent variable are given in table 3. The base group consists of white individuals from the January 1984 CPS who are 20 to 24 years old, have twelve years of education, and who had not been displaced within two years of the survey date. The sample includes all individuals ages 20–64. A total of 714 individuals who reported a job loss in the survey month were deleted. This was approximately 3 percent of displaced workers and less than 0.2 percent of the total sample. The numbers in parentheses are asymptotic standard errors.

— Not applicable.

in these relationships. Division of the appropriate logit coefficient by five results in a good estimate of the effect of the variable on the probability of employment.[23]

The discussion focuses on the interaction estimates because they represent the relevant difference in employment probabilities between displaced and nondisplaced workers. The multivariate analysis shows a somewhat different picture of the effect of displacement on the probability of employment than was evident in the simple tabulations in table 3. Relative to nondisplaced workers, the baseline probability of employment is substantially lower for individuals who were displaced in the year prior to the survey date. This difference is about twenty-five percentage points for men and about twelve percentage points for women. The apparent penalty for displacement is virtually zero for workers who were displaced in the second year prior to the survey date, however. The difference is reduced to about four percentage points for men and is actually positive (though insignificant) for women.

This finding is consistent with workers who were displaced more than a year in the past having had more time to find a new job than recently displaced workers. It is unlikely to be due to the observation that the probability of an individual recalling a job loss declines with the time since displacement.[24] It is natural to expect individuals who become employed to be less likely to recall the job loss than individuals who remain unemployed. This would bias the results toward finding larger negative employment effects of displacement as the time since job loss increases.

Nonwhites and particularly nonwhite females suffer larger negative employment effects of job loss than do whites. The race difference is relatively small for men (about two percentage points), but it is larger (about twelve percentage points) for women.

The data clearly show that individuals with more education are more likely to be working. The interactions with job loss indicate that, relative to displaced workers with twelve years of education, displaced workers with other levels of education have higher probabilities of employment. The finding for workers who have not completed high school is likely

23. The derivative of the probability in the logit model is $\beta P(1-P)$. The sample average value of P is approximately 0.75, so that $P(1-P)$ is approximately 0.20.

24. Topel (1990).

due to the selection induced by the fact that displaced workers had to be working at some point in the past. The finding for the higher education categories may reflect a greater ability by highly educated workers to find jobs after displacement. Adding the main effects for education to their respective interactions with job loss verifies that the probability of reemployment for displaced workers is monotonically increasing in education.

One of the goals of this study is to investigate how the costs of job loss have changed over time. The estimates of the main year effects and their interactions with job loss show that the displacement penalty in employment probabilities is significantly lower in the expansionary period than in the two recessions. This difference is large, averaging about six percentage points for men and about four percentage points for women. There is no significant difference in the displacement penalty in reemployment probabilities between the 1982–83 downturn and the later recession.

Given the findings that older and more educated workers were more likely to suffer a job loss in the 1990–91 period than in the 1982–83 period, it is interesting to investigate whether changes occurred in the distribution of postdisplacement labor force states over time. I investigated this by reestimating the logit model of the probability of employment separately for each education category and for each age group. These estimates, which are not presented here, show no evidence of any systematic change in the displacement penalty in employment probabilities other than the cyclical effects noted above. Specifically, there is no evidence that the displacement-nondisplacement difference in employment probabilities for older or more-educated workers has gotten larger (negatively).

Overall, there is clear evidence that workers who were displaced within one year of the survey date are less likely to be employed at the survey date than workers who were not displaced or who were displaced more than one year before the survey date. I find that nonwhite females suffer a larger displacement penalty in employment probabilities than do white females. The race difference is smaller for males. I further find that the displacement penalty in employment probabilities is inversely related to education level. There is no evidence of secular changes in the displacement-nondisplacement difference in employment probabilities, even for particular age and education groups.

Costs of Job Loss: Full-Time Employment Among Displaced Workers

Although most displaced workers have found new employment, it may be that the quality of the job found is lower than the quality of the job lost. Of course, some workers may find better jobs, but the earlier literature on the consequences of job loss suggests that displaced workers, on average, find jobs that are inferior to the jobs they lost.[25] I consider the obvious job attribute, wages, in the next section. Another important dimension of job quality is hours worked.[26] The DWSs do not have detailed information on hours on the jobs displaced workers lost, but they do ask if the job was part-time (fewer than thirty-five hours a week). Hours on the job held at the survey date are available, and I use this information to determine if the displaced workers are currently working full- or part-time. Table 5 contains calculations of the fraction of workers who are currently working full-time by year and by whether the worker was displaced and, if displaced, by whether the worker was reemployed and whether the worker was displaced from a full-time or a part-time job.

The first two columns of table 5 show that about 80 percent of employment is full-time. This is lower than the approximately 88 percent of displaced workers who were displaced from full-time jobs. Thus, workers are more likely to be displaced from full-time jobs than from part-time jobs.

Columns 3, 4, and 5 provide a breakdown by year of the probability of full-time employment for displaced workers. Displaced workers who are not working full-time are either not employed or employed part-time. Clearly, displaced workers are not very likely to be working full-time even if they were working full-time prior to displacement. It is true, however, that workers displaced from full-time jobs were substantially more likely to be working full-time at the survey date than were those who were displaced from part-time jobs. This may reflect persistent individual differences in labor supply preferences. Another finding is that workers displaced in the two recessionary periods had a

25. See, for example, Podgursky and Swaim (1987); and Topel (1990).

26. See Altonji and Paxson (1988) for an interesting analysis of hours constraints, wages, and labor supply.

Table 5. Full-Time/Part-Time Status by Displacement and Previous Status for Displaced Workers by Year

Fraction reporting full-time work

			All displaced workers			*Reemployed displaced workers*		
Year	*All workers (1)*	*Workers not displaced (2)*	*All (3)*	*Old FT (4)*	*Old PT (5)*	*All (6)*	*Old FT (7)*	*Old PT (8)*
1984	0.787	0.790	0.381	0.406	0.208	0.721	0.758	0.434
	(0.002)	(0.002)	(0.007)	(0.008)	(0.018)	(0.009)	(0.009)	(0.030)
1986	0.804	0.806	0.415	0.439	0.234	0.754	0.789	0.473
	(0.002)	(0.002)	(0.008)	(0.008)	(0.020)	(0.009)	(0.010)	(0.033)
1988	0.792	0.794	0.458	0.479	0.310	0.763	0.789	0.564
	(0.002)	(0.002)	(0.008)	(0.009)	(0.020)	(0.009)	(0.009)	(0.031)
1990	0.806	0.807	0.462	0.483	0.285	0.758	0.789	0.493
	(0.002)	(0.002)	(0.008)	(0.009)	(0.022)	(0.010)	(0.010)	(0.033)
1992	0.790	0.794	0.372	0.393	0.211	0.716	0.752	0.424
	(0.002)	(0.002)	(0.007)	(0.007)	(0.017)	(0.008)	(0.009)	(0.029)
Number	292,691	281,187	20,785	18,228	2,557	11,481	10.183	1,298

Source: Authors tabulations based on data reported in DWS, January 1984,1986, 1988, 1990, 1992. All averages are weighted by the CPS final sampling weights. Full-time is defined as at least thirty-five hours of work a week. Displacement is defined as a job loss within two years of the DWS survey date. The numbers in parentheses are standard deviations of the means.

Note: FT = full-time, PT = part-time.

significantly lower probability of working in full-time jobs at the survey date. This is true regardless of whether the lost job was full-time or part-time.

The last three columns of table 5 provide a breakdown by year of the probability of full-time employment for reemployed displaced workers. These statistics show the same general pattern as the full-time employment probabilities for all displaced workers in columns 3-5, although (not surprisingly) at a uniformly higher level. Note that reemployed displaced workers, even those who were displaced from full-time jobs, have significantly lower probabilities of full-time employment than do workers who were not displaced.

Table 6 contains estimates of a logit model of the probability of full-time employment among those displaced workers who were reemployed at the survey date. The results must therefore be interpreted conditional on reemployment. This model includes a measure of whether the lost job was part- or full-time in order to account for gross and persistent differences in hours preferences. Consistent with the tabulations in table 5, workers who lost part-time jobs were less likely to be employed in full-time jobs at the survey date. The point estimate on the "part-time on old job" variable suggests that workers who lost part-time jobs were about twenty-five percentage points less likely to hold full-time jobs at the survey date than were workers who had lost full-time jobs. As with the logit estimates of the probability of employment, division of the appropriate coefficient by five results in a good estimate of the effect of a variable on the probability of full-time employment.[27]

The logit estimates also show that females are about eleven percentage points less likely than otherwise equivalent men to hold a full-time job after displacement. Note that this result is not because women are more likely to hold part-time jobs generally, since the eleven percentage point estimate is derived controlling for part-time status of the lost job. Nonwhites are about five percentage points less likely to hold a full-time job after displacement than are otherwise equivalent whites.

The correlation between education and the probability of full-time employment after displacement is strongly positive. For example, college graduates are about ten percentage points more likely than high school graduates to be employed full-time after displacement. Workers

27. See footnote 22.

with less than a high school education are about seven percentage points less likely than high school graduates to be employed full-time after displacement.

Older workers (ages 55 to 64) are significantly less likely than younger workers to be employed full-time after displacement. For example, employed workers ages 60 to 64 who are displaced are about seventeen percentage points less likely than workers twenty years younger to be working full-time after displacement. This suggests that older displaced workers may ''partially'' retire. These workers may have a pension available to them upon displacement.

Workers displaced in the periods of expansion are about five percentage points more likely to be reemployed full-time than are workers displaced in either recessionary period. There seems to be no significant difference between 1984 and 1992, suggesting that full-time employment was not harder to find after displacement in the recent recession relative to the earlier recession (conditional on reemployment). Estimates of a model that allows for different education and age effects by year (not shown) provide no evidence (other than the cyclical factors) that the relationship of the probability of full-time employment with age and education varies systematically over time. In particular, the two recessionary periods are not significantly different.

The final variables included in the model crudely measure the length of time since displacement. The base group consists of those workers who were displaced in January of the survey year (immediately before the survey). It is interesting that the probability of full-time employment rises with time since displacement. This is consistent with displaced workers accepting part-time employment relatively quickly while continuing to search for a full-time job. It is also consistent with the role that time since displacement played in determining the probability of employment. The passage of time provides an opportunity not only to find a job, but also to find a good (that is, full-time) job.

Overall, the results in this section demonstrate that reemployed displaced workers are less likely to be employed full-time after displacement than they were before displacement. Since the wage rate on part-time jobs is substantially less than the wage rate on full-time jobs, other things equal, the increased incidence of part-time employment among displaced workers is an important component of the costs of job loss. I also find substantial differences across reemployed displaced workers

Table 6. Logit Estimates of Probability of Full-Time Employment for Reemployed Displaced Workers, 1984–91

Variable	*Weighted mean*	*Coefficient estimate*
Constant	1.0	0.411 (0.163)
Female	0.382	−0.552 (0.0530)
Nonwhite	0.116	−0.243 (0.0766)
Part-time on old job	0.110	−1.20 (0.0739)
Education $<$ 12 yrs.	0.151	−0.370 (0.0731)
Education 13–15 yrs.	0.248	0.164 (0.0651)
Education $\geq$ 16 yrs.	0.175	0.510 (0.0797)
Age 25–29	0.209	0.0959 (0.0819)
Age 30–34	0.176	0.137 (0.0863)
Age 35–39	0.149	0.0985 (0.0899)
Age 40–44	0.109	0.224 (0.101)
Age 45–49	0.0754	0.255 (0.113)
Age 50–54	0.0490	0.0831 (0.130)
Age 55–59	0.0381	−0.354 (0.134)
Age 60–64	0.0183	−0.625 (0.180)
1986	0.197	0.200 (0.0849)
1988	0.209	0.263 (0.0846)
1990	0.186	0.264 (0.0871)
1992	0.243	−0.0412 (0.0804)

Table 6. (continued)

Variable	*Weighted mean*	*Coefficient estimate*
Displaced in year prior to survey	0.496	0.696 (0.144)
Displaced in second year prior to survey	0.479	1.07 (0.144)

Sample size: 8,886
Log likelihood: −4,641.9

Source: Author's tabulations based on data reported in DWS, January 1984, 1986, 1988, 1990, and 1992. The data are weighted by the CPS final sampling weights. The weighted mean of the dependent variable is 0.749. The base group consists of white individuals from the 1984 DWS who are 20 to 24 years old, have twelve years of education, and who were displaced in January of the survey year. Asymptotic standard errors are in parentheses.

in their probability of full-time employment. Most of these differences are not likely to be due to differences in desired hours because the estimates control for the full-time/part-time status of the lost job. Workers displaced during the recessionary periods have a lower probability of full-time employment than workers displaced during the expansion, but there is no evidence that the relationship between the probability of full-time employment and worker characteristics varies systematically over time. Finally, the reduction in the probability of full-time employment declines with time since displacement as displaced workers have sufficient time to find full-time jobs.

Costs of Job Loss: Earnings Losses of Reemployed Workers

The final piece of the analysis of the costs of job loss relates to the earnings changes associated with job loss. Earlier work using the DWS shows that displaced workers suffer earnings losses on average.[28] Unfortunately, the DWS contains information only on usual weekly earnings on the lost job. No hours information is collected other than the full-time/part-time distinction, so it is not possible to compute an hourly wage rate. The same information is available for the current job along with usual hours worked so that an hourly wage can be calculated for the current job. Because I am interested in wage *changes*, however, I

28. See, for example, Podgursky and Swaim (1987); Kletzer (1989); Topel (1990); and de la Rica (1992).

Table 7. Average Log Real Change in Weekly Earnings for Reemployed Displaced Workers by Year

Year	*All*	*FT-FT*	*FT-PT*	*PT-FT*	*PT-PT*
1984	−0.123	−0.0683	−0.511	0.577	−0.0178
	(0.0163)	(0.0152)	(0.0430)	(0.0795)	(0.0790)
1986	−0.144	−0.0653	−0.599	0.436	−0.127
	(0.0149)	(0.0136)	(0.0411)	(0.0731)	(0.0780)
1988	−0.122	−0.0938	−0.523	0.614	−0.0044
	(0.0144)	(0.0132)	(0.0408)	(0.0641)	(0.0799)
1990	−0.0972	−0.0559	−0.451	0.430	0.0063
	(0.0153)	(0.0139)	(0.0439)	(0.0762)	(0.0796)
1992	−0.169	−0.105	−0.585	0.543	0.0225
	(0.0134)	(0.0125)	(0.0348)	(0.0686)	(0.0661)
Number	8,886	6,117	1,765	463	541

Source: Author's tabulations based on data on workers displaced within two years before the survey date and reemployed at the time of the survey reported in DWS, January 1984, 1986, 1988, 1990, and 1992. All averages are weighted by the CPS final sampling weights. Part-time is defined as less than thirty-five hours of work a week. "Current" earnings are deflated by the Consumer Price Index for January of the survey year. Base-period earnings are deflated by the CPI for the year of reported displacement. The numbers in parentheses are standard deviations of the means.

Note: FT = full-time; PT = part-time.

am forced to use the weekly earnings measure controlling for full-time/part-time status on both the old and new jobs.

Table 7 contains weighted average log real earnings changes (changes in reported weekly earnings) by year for displaced workers who are reemployed as of the date of the survey. The log real earnings change is computed as the difference between reported weekly earnings as of the survey date and the reported weekly earnings on the lost job as of the date of displacement. Earnings are deflated by the 1982–84 = 100 consumer price index (CPI). The CPI in the reported year of displacement is used to deflate earnings on the old job. The CPI for January of the survey year is used to deflate current earnings.

The first column of table 7 contains these averages for all reemployed displaced workers, and they show an average loss in weekly earnings of about 13 percent. The analysis above demonstrated, however, that reemployed job losers are less likely to hold a full-time job after displacement than they were before displacement. Thus, the remaining four columns of table 7 contain the mean log earnings changes for the four combinations of full-time/part-time status on the old and new jobs. Workers who lost a full-time job but found a new full-time job suffered earnings losses that average about 8 percent. Workers who lost a part-time job and found a new part-time job on average suffered earnings losses insignificantly different from zero. There is more scope for hours variation within part-time jobs (one to thirty-four hours a week), however, and these comparisons suffer from unmeasured hours variation. The two other categories show the not-surprising result that displaced workers who move from full-time to part-time suffer huge losses in weekly earnings, while those who move from part-time to full-time enjoy huge gains (of approximately equal magnitude). These reflect changes in labor supply and are not directly informative about the costs of displacement other than showing that full-time to part-time transitions are over three times more common than part-time to full-time transitions.

Table 8 contains estimates of log earnings-change regressions that control explicitly for the full-time/part-time status of both the old and the new jobs. The R^2 is 0.167, which is quite high for a wage-change regression, but most of the explanatory power is coming from the part-time status variables, which represent labor supply factors more than wage-change factors.[29]

The results suggest that older workers have lower earnings changes than younger workers. Each ten years of age represents about 3 percent lower earnings growth. This is consistent with the overwhelming evidence that age-log earnings profiles are concave so that earnings changes are decreasing in age for workers generally. Tenure on the previous job is also significantly related to earnings changes, with more senior displaced workers having substantially larger negative earnings changes. Each year of previous tenure is related to about a 1 percent larger negative earnings change. This may be a result of the destruction of job-specific human capital that results from job loss.[30]

The estimates suggest that workers who were displaced in either of the two years prior to the survey have about a fifteen percentage point

29. The R^2 for the regression without the part-time status variables is about 0.03.

30. Abraham and Farber (1987) make the point that the return to tenure may at least partially result from a correlation between tenure and unobserved worker ability. This would tend to moderate the negative relationship between previous tenure and wage changes for displaced workers. Consistent with this, Kletzer (1989) presents evidence from earlier DWSs that workers displaced from high tenure jobs have higher earnings (levels, not changes) on their postdisplacement jobs. Topel (1991) and Altonji and Williams (1992) present thorough analyses of the returns to tenure.

Table 8. OLS Regression Estimates of Log Change in Real Weekly Earnings for Reemployed Displaced Workers, 1984–91

Variable	*Weighted mean*	*Coefficient estimate*
Constant	1.0	0.232 (0.0460)
Female	0.382	−0.0178 (0.0129)
Nonwhite	0.116	0.0230 (0.0189)
Education < 12 yrs.	0.151	−0.0321 (0.0183)
Education 13–15 yrs.	0.248	−0.0118 (0.153)
Education ≥ 16 yrs.	0.175	0.0164 (0.0173)
Age	34.6	−0.00313 (0.00065)
Tenure on lost job (years)	3.87	−0.0105 (0.0013)
Previous job part-time	0.110	0.575 (0.0276)
Current job part-time	0.251	−0.462 (0.0158)
Both jobs part-time	0.0571	−0.0775 (0.0396)
1986	0.197	−0.0248 (0.0202)
1988	0.209	−0.0062 (0.0199)
1990	0.186	0.0187 (0.0205)
1992	0.243	−0.0327 (0.0194)
Displaced in year prior to survey	0.496	−0.147 (0.0389)
Displaced in second year prior to survey	0.479	−0.138 (0.0391)
Sample size: 8,886		
R^2: 0.167		
Standard error of estimation: 0.569		

Source: Author's tabulations of data for workers displaced within two years before the survey data and reemployed at the time of the survey reported in DWS, January 1984, 1986, 1988, 1990, and 1992. The data are weighted by the CPS final sampling weights. The weighted mean of the dependent variable is −0.133. The base group consists of white males from the 1984 survey who have twelve years of education, whose previous and current jobs were full-time, and who were displaced in January of the survey year. "Current" earnings are deflated by the Consumer Price Index for January of the survey year. Base-period earnings are deflated by the CPI for the year of reported displacement. Standard errors are in parentheses.

larger negative earnings change than do those workers who were displaced in the month of the survey (the base group). This is likely due to selection in that only the most able (or most fortunate) of the workers who were displaced in the month of the survey were likely to have found a new job before the survey was taken. Additionally, any reasonable job search model with a declining reservation earnings would have the property that searches that end quickly will yield higher wages.

There is no consistent pattern by calendar year in earnings changes among displaced workers. Earnings losses for workers displaced in the 1990–91 recession are marginally significantly more negative than those displaced in the 1982–83 recession, but even workers displaced in the stronger labor market of 1984–85 have larger negative wage changes than those displaced in 1982–83. Additional regressions, not presented here, that include interactions of year with education and age do not find any significant interactions. Thus, workers who are displaced in recessions do not seem to suffer systematically larger earnings declines (or smaller earnings increases) than do workers who are displaced in expansions. One caveat to this conclusion is that workers displaced in recessions are more likely to be reemployed in part-time jobs, which pay substantially lower wages generally.[31]

An important weakness of this analysis is that earnings changes for displaced workers ought to be benchmarked against earnings changes for workers who were not displaced (or against the population as a whole, since displaced workers represent a relatively small fraction of the work force). Only by differencing the earnings changes of nondisplaced workers (a control group) from the earnings changes of displaced workers can a proper measure of the earnings penalty associated with job loss be made.

I generate a control group using a 5 percent random sample from the merged outgoing rotation group (OGRG) files of the CPS for the two calendar years prior to each DWS, together with all nondisplaced workers from the OGRG files of the January CPSs containing the DWSs.[32] The OGRG files contain information on weekly earnings and

31. Remember that the estimates here cannot be used to draw the conclusion that part-time jobs pay lower wages because the part-time measures are dominated by hours differences and greatly exaggerate any wage penalty to part-time work.

32. The OGRG data are used by de la Rica (1992) as a control group for earnings

hours for a random sample of the work force each year. The two-year period prior to each DWS covers the entire period of displacement for workers, and, as such, provides base-period weekly earnings for workers who were not displaced during this period.[33] Weekly earnings as of the "current" period (the date of each DWS) for nondisplaced workers are derived from the OGRG files of the relevant January CPS.

Since these data are neither longitudinal nor retrospective, I cannot compute earnings changes for individual workers. I can, however, measure whether the observation represents base-period or current-period earnings. This allows me to estimate the average changes in weekly earnings of nondisplaced workers.[34] My sample includes full-time workers (thirty-five hours a week or more) between the ages of 20 and 64. A comparable sample of displaced workers is derived using the workers who report having been displaced within two years of each DWS survey date.

These data are used to estimate an earning function of the form

$$\ln E_j = \gamma_0 + X_j\beta + \gamma_1 D_j + \gamma_2 C_j + \gamma_3 D_j C_j + \varepsilon_j, \tag{5}$$

where

E_j = real earnings for cell j,
X_j = dummy variables for main effects for worker characteristics,
D_j = a dummy variable for job displacement,
C_j = a dummy variable for current period, and
ϵ_j = an error term.

The vectors β and γ are parameters to be estimated. The X vector contains controls for sex, race, four education categories, and nine age categories. A set of calendar year dummies is also included when equation 5 is estimated pooling across the five DWSs.

losses suffered by workers who report being displaced in the 1986 DWS. Jacobson, Lalonde, and Sullivan (1991) also use a control group to estimate earnings losses of displaced workers.

33. In fact, workers who are or will be displaced are included in the merged OGRG files, but, since only a small fraction of workers are displaced in any year (less than 10 percent), I ignore this slight distortion.

34. The OGRG files contain enough information to compute hourly earnings, but, because the DWSs do not contain this information, the analysis here uses only weekly earnings for full-time workers.

This specification allows separate intercepts for four groups: initial earnings of nondisplaced workers (γ_0); initial earnings of displaced workers ($\gamma_0 + \gamma_1$); current earnings of nondisplaced workers ($\gamma_0 + \gamma_2$); and current earnings of displaced workers ($\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3$). The usual estimate of the earnings effect of displacement is the earnings change of displaced workers ($\gamma_2 + \gamma_3$). The difference-in-difference estimate subtracts the normal earnings change of nondisplaced workers over the same period (γ_2) from the earnings change of displaced workers. The resulting estimate is simply γ_3. To the extent that the earnings of nondisplaced workers are growing, the difference-in-difference estimate will be smaller (more negative) than the simple earnings change of displaced workers.

Age categories are defined as of the current date rather than the base-period date to ensure that calculations of earnings changes between the base and current periods use a consistent group of workers. For example, nondisplaced workers 20 to 24 years old in the January 1984 CPS contribute current earnings observations. The base-period earnings for these workers are drawn from the OGRG files using workers 18 to 22 in 1982 and 19 to 23 in 1983.

The annual average CPI for each year was used to deflate base-period earnings (the year of the OGRG file for nondisplaced workers and the year of job loss for displaced workers). The appropriate January CPI was used to deflate current earnings.

Table 9 contains ordinary least squares estimates of equation 5 using the data on full-time real weekly earnings. No part-time earnings observations (either base or current period) are used. Although only the estimates of the relevant differences (γs) are shown, all regressions also include main effects for sex, race, education level, and age. Note that the base period is (randomly) up to two years prior to the current period. Since the average time difference between the base and current periods is one year, it is reasonable to interpret the earnings changes as annual rates of change.

The estimates of γ_1 show that base-period earnings are significantly lower for displaced workers relative to nondisplaced workers. This is consistent with existing evidence from other sources that displaced workers suffer earnings losses even before they lose their jobs. Most convincing on this point is the work by Jacobson, Lalonde, and Sullivan

Table 9. Estimates of Effect of Displacement on Log Full-Time Weekly Earnings

Parameters	*Pooled*	*1984*	*1986*	*1988*	*1990*	*1992*
γ_1	−0.0415	−0.396	−0.0285	−0.0048	−0.0660	−0.0773
	(0.0050)	(0.0102)	(0.0112)	(0.0113)	(0.0123)	(0.0113)
γ_2	0.0413	0.0321	0.0334	0.0354	0.0324	0.0186
	(0.0035)	(0.0059)	(0.0059)	(0.0060)	(0.0059)	(0.0059)
$\gamma_2 + \gamma_3$	−0.0777	−0.0779	−0.0836	−0.119	−0.0598	−0.131
	(0.0074)	(0.0151)	(0.0153)	(0.0156)	(0.0169)	(0.0211)
γ_3	−0.119	−0.110	−0.117	−0.154	−0.0922	−0.115
	(0.0075)	(0.0163)	(0.0163)	(0.0166)	(0.0179)	(0.0167)
$\gamma_1 + \gamma_3$	−0.161	−0.150	−0.146	−0.159	−0.158	−0.192
	(0.0056)	(0.0127)	(0.0120)	(0.0123)	(0.0131)	(0.0123)
Number of observations	128,714	23,630	24,581	24,801	26,943	28,759
R^2	0.336	0.339	0.348	0.344	0.334	0.329

Source: Author's tabulations based on data reported in DWS, January 1984, 1986, 1988, 1990, and 1992. These estimates are derived from regressions of log real weekly earnings on dummy variables for sex, race, nine age categories, four educational categories, whether the worker was displaced from his/her job, whether the observation represents "current" earnings (at the time of the displaced workers survey), and the interaction of the displacement dummy and the current observation indicator. The pooled model also includes a set of year dummies. The "current" observations for nondisplaced workers (n = 50,090) are from the outgoing rotation groups of the above-listed January CPS's. The "prior" observations for nondisplaced workers (n = 62,578) are a 5 percent random sample from the merged outgoing rotation group CPS files for the two years preceding each displaced worker survey. All observations for displaced workers (8,839 "prior," 7,207 "current") are from those surveys. The numbers in parentheses are standard errors.

Note: Estimates are from the regression

(5) $\ln E_j = X_j\beta + \gamma_1 D_j + \gamma_2 C_j + \gamma_3 L_j C_j + \varepsilon_j,$

γ_1 Prior earnings (displaced—not displaced)

γ_2 (Current earnings—prior earnings) not displaced

$\gamma_2 + \gamma_3$ (Current earnings—prior earnings) displaced

γ_3 Difference-in-difference estimate of displacement effect

$\gamma_1 + \gamma_3$ Current earnings (displaced—not displaced)

using administrative data with workers' quarterly earnings histories.[35] There are at least two other potential explanations for this finding. First, the finding may result from displaced workers having less tenure than nondisplaced workers. Second, displaced workers may simply be of lower (unobserved) quality than nondisplaced workers.[36]

The estimates of γ_2 show that real earnings of nondisplaced workers

35. Jacobson, Lalonde, and Sullivan (1991). Blanchflower (1991), using data from the United Kingdom, and de la Rica (1992), using the 1986 DWS, also find displaced workers suffer earnings losses prior to displacement.

36. Being of lower tenure and of lower ability may be related if part of what makes a high ability worker is stability. See Abraham and Farber (1987).

grew by about 3 to 4 percent on average over the sample period. In contrast, the estimates of earnings change for displaced workers ($\gamma_2 + \gamma_3$) show that their earnings fell substantially (by more than 10 percent in some periods).

The difference-in-difference estimates (γ_3) show a large and significant negative effect of displacement on real earnings of about 11 percent. Thus, displaced workers suffer a substantial real earnings decline relative to nondisplaced workers. With regard to changes over time in the earnings loss associated with displacement, the point estimate of γ_3 does vary somewhat from year to year, but there is no systematic pattern to movements in γ_3.

Although the results are not presented here, the earnings function was reestimated separately for each of the four education categories. Estimated differences in γ_3 by education category were generally not statistically significant. The analogous exercise was carried out by age category, and differences in γ_3 were generally not statistically significant. There was no consistent pattern over time in the estimated displacement effects by either education category or age category. Note specifically that older and/or more-educated displaced (and full-time reemployed) workers did not suffer larger (proportional) earnings losses relative to younger and/or less-educated displaced workers in the most recent period relative to earlier periods.

Finally, the earnings function was reestimated allowing the earnings losses to differ with time since displacement. Some evidence was found that the full-time earnings penalty for displacement does decline with time since displacement. Specifically, workers who were displaced in the year prior to the DWS had a full-time earnings penalty (γ_3) of 13.2 percent, compared with a penalty of 10.7 percent for workers displaced in the second year prior to the DWS (p-value of difference $= 0.015$).

Overall, these results document the substantial full-time earnings losses (on the order of twelve percentage points) suffered by displaced workers relative to nondisplaced workers. This twelve-point relative decline is magnified by the fact that displaced full-time workers are less likely to be working full-time after displacement than are nondisplaced workers. The earnings penalty for displacement is moderated by the passage of time, however, because the full-time earnings penalty falls somewhat and the probability of postdisplacement full-time employment rises with time since displacement. There is no evidence that full-time earnings losses are larger in the recessionary period other than through the lower probability of working full-time, that the age difference in earnings losses suffered by displaced workers has grown over time, or that earnings losses are falling relatively more heavily on highly educated workers.

Conclusion

The perception that the nature and consequences of job loss are different than they used to be seems generally correct. Job loss was relatively more common in important service industries and relatively less common in manufacturing in 1990–91 compared with 1982–83. It is indeed the case that older and more-educated workers were more vulnerable to job loss in the 1990–91 period than they were in the 1982–83 period. There is, however, no evidence that the costs of job loss have increased. None of the postdisplacement labor market outcome measures show significant differences in the costs of displacement between the 1982–83 and 1990–91 periods, either overall or by age or education categories.

It is clear that the costs of job loss, though they have not changed systematically, are substantial and work through several channels. First, displaced workers are less likely to be employed than are otherwise equivalent nondisplaced workers. Second, displaced workers who are reemployed are less likely to be employed full-time after displacement relative either to otherwise equivalent nondisplaced workers or to their own predisplacement hours. Third, even if reemployed on a full-time job, displaced workers earn substantially less than either otherwise equivalent nondisplaced workers or what they themselves earned prior to displacement.

More formally, the expected current period earnings of workers is

$$(6)\quad E(W) = Pr(EMP)\Big[Pr(FT|EMP)\cdot E(W|EMP,FT) + Pr(PT|EMP)\cdot E(W|EMP,PT)\Big],$$

where

$Pr(EMP)$	=	probability of reemployment,
$Pr(FT \mid EMP)$	=	conditional probability of full-time employment,
$Pr(PT \mid EMP)$	=	conditional probability of part-time employment,
$E(W \mid EMP, FT)$	=	conditional expectation of full-time earnings, and
$E(W \mid EMP, PT)$	=	conditional expectation of part-time earnings.

Job loss adversely affects all terms in this expectation.[37] Additionally, the earnings (and wage rates) of full-time workers are higher than those of part-time workers so that a shift toward part-time employment adversely affects expected earnings.

There is fairly strong evidence that some of the costs of displacement are temporary. The probability-of-employment and the part-time employment penalties for displacement decline with the time since displacement. Additionally, there is evidence that the full-time earnings penalty for displacement narrows slightly with time since displacement. Thus, the unconditional expected earnings penalty declines with time since displacement. The full-time earnings penalty is likely to be persistent, however, at least in part because the wage loss is directly related to tenure, and lost tenure is never fully recovered.

Overall, the costs of job loss to displaced workers are substantial and come in several forms. However, the public perception that the sluggish economy of the last two years is worse than earlier downturns may reflect more who has lost jobs recently rather than either increased overall job loss or increased costs to those who are losing jobs.

37. In fact, little evidence is presented regarding the effect of displacement on part-time earnings. The simple statistics in table 7 suggest that displacement has only a small effect on this value.

Comments and Discussion

Comment by Robert Hall: Farber provides a fascinating compilation of data on job losses. He wisely avoids presentation of a formal model of the process, given the complexity of any reasonable model. Where necessary, he provides the appropriate piece of the model to help the reader understand the findings.

Farber is candid about the heterogeneity of the phenomenon he studies in the paper. He measures job loss from the displaced workers supplement to the Current Population Survey. Information comes from a household's respondent—generally, an adult who happens to be home during the day—about the employment experiences of everyone in the household. A worker is recorded as having suffered a job loss, in the sense of Farber's data, if the respondent thinks that the worker lost a job as the result of the employer's decision unrelated to the performance of the worker. Presumably, most quits are thereby excluded. Many ambiguities remain. What if the family member is a programmer, hired on a term contract? How does the mother or wife of a construction worker answer these questions?

One sign of the importance of heterogeneity is the rapid decline in the job-finding rate as a function of the duration of job-seeking that Farber and all other investigators find. Construction and contract workers, for whom turnover is a way of life, find jobs relatively quickly and so drop out of the body of job seekers. Among job seekers who are more than a month or two into the process, those who have unexpectedly lost permanent jobs predominate, and their job-finding rates are much lower than those for the others.

One of the challenges of a descriptive piece of research such as this is to tell readers what they really want to know. Farber is a master of this craft, and I am glad to see that he adopted suggestions of mine for the published paper that make his results even more useful.

A good example is the comparison of probabilities of job loss between men and women. The incidence of earlier job loss among the respondents does not tell readers what they want to know. Women have lower job loss rates because, on average, they have a substantially lower likelihood of having a job to lose. Farber deals with this fact neatly by calculating rates of earlier job loss relative to current employment in the same demographic category. The incidence of job loss per woman is a little more than half the incidence per man. But the rate per employed woman—6.2 percent—is fairly close to the rate per employed man, 7.8 percent.

Farber's handling of displacement rates by industry is similarly adroit. His exposition, with its invocation of Bayes's rule, hides the simplicity and common sense of what he has done to generate usable results. In effect, he measures relative rates of job loss for an industry as the ratio of job losers in the survey to outside measures of employment in the industry.

Farber gives strong support to earlier findings about the importance of tenure in job loss. Rates of job loss are vastly lower among workers who have survived ten or fifteen years in a job than they are for otherwise similar workers who are new to a job. Tenure accounts for most of the age differences in job loss rates.

The paper is less successful in delivering what readers want to know about the effects of displacement. Table 3 reports the fractions of the population who are employed, unemployed, and out of the labor force for workers who have suffered a displacement and those who have not. Here the problems of interpreting the difference associated with displacement become disabling. The displaced workers have the crucial difference of having had a job to lose sometime recently, whereas many of the people in the nondisplaced group did not have a job to lose. As Farber candidly informs us, "the comparison is thus biased toward finding a smaller adverse employment effect of displacement, and the results must be interpreted with this in mind." Farber wants readers to make some kind of adjustment in their minds, but surely he would have a comparative advantage in that task, which he displays so well in other parts of the paper. He should compare the displaced workers to people in general who have worked at some time in the past two years. Absent that comparison, it is hard to see how his results shed much light on the question of the "effect" of displacement.

Farber's discussion of the likelihood of full-time employment after displacement is another place where readers do not learn what they would like to learn. Displaced workers who previously worked full-time and are now reemployed have almost exactly the same likelihood of full-time work as does the population as a whole. But two conditioning issues confuse the interpretation. As before, displaced workers are special because they had a job to be displaced from, which is not true of everyone in the population. Second, and more important here, the displaced workers who have succeeded in becoming reemployed by the time of the survey are quite selected—almost half the displaced workers are not yet reemployed. Because the reemployed are a select group, it is not very informative to look at their likelihood of full-time employment. Again, the full Farber treatment would call for some kind of comparison group to eliminate the two conditioning influences.

Farber also studies the earnings changes that follow displacement, the main focus of previous research based on the displaced workers survey. Again, he does not solve major problems of interpretation. For example, workers who had lost full-time jobs and had found new full-time jobs by the time of the survey suffered earnings losses of about 8 percent. But this is surely an understatement of the effect of displacement on earnings, because those who found not only new jobs but also new full-time jobs are unusually lucky or proficient. Farber shows earnings changes for those who moved to part-time work, but he cannot give any meaningful answer to the question of the effect of displacement on earnings. Again, about half the displaced workers had not found any work by the time of the survey. Farber does put a good deal of effort into providing a benchmark for earnings changes of nondisplaced workers, but this is well down the list of confounding factors in the interpretation of earnings changes for displaced workers.

Let me turn to some issues of macroeconomics where I find Farber's results illuminating. I think that for many purposes the concept of job loss in the displaced workers survey is superior to others studied in macroeconomics, such as the old manufacturing turnover data and gross flows from the Current Population Survey. The other data are completely dominated by short-term job loss. Farber is able to concentrate on permanent job loss, which, although only a fraction of the flow of job losers, is the main source of unemployment and hardship.

In the old days, macroeconomists thought that separations were constant over the cycle and that job-finding rates varied. Farber joins others in showing the opposite. He finds that the incidence of job loss for men ages 35–39, for example, rose from about 7 percent in a strong labor market (1986–87) to about 9 percent in a weak market (1982–83 or 1990–91) (see figure 1). Reemployment rates fell from about 60 percent in the strong market to about 55 percent in the weak market (see table 3). Changes in job loss rates are several times larger than changes in reemployment rates. Thus, slack markets occur mainly because there are more job losses. Job losers find it only slightly more difficult to find work in slack times than in good times. Farber's evidence supports the view that slack periods are those when the labor market is processing a large volume of rematching, because an unusually large number of workers have been let go. The efficiency of rematching remains about the same.

Farber's evidence seems unfavorable to the idea that sluggish wage adjustment accounts for changes in labor-market conditions. With slow adjustment, there would be queuing for jobs in slack times, and job-finding rates would be lower. Farber's numbers suggest very little difference in queuing between strong and slack markets. In addition, Farber finds that labor-market conditions have almost no effect on the earnings changes of job changers. Although the new jobs found by displaced workers typically pay quite a bit less than the lost job, the differential is about the same in weak and strong periods.

The paper nicely documents the dramatic difference between source industries for the 1981–82 recession and the 1990–91 recession. Table 1 shows that job losses in finance, insurance and real estate, trade, professional services, and construction are more important, and manufacturing less important, in the recent recession.

It is a disappointment to me as a macroeconomist that Farber has insisted on retaining footnote 2, which defines a recession as a period of high unemployment. Both in the English language and in the discourse of macroeconomics, a recession means a contraction—the period when output and employment are falling. This paper is a comparison of slack periods to strong periods, not recessions to expansions. It only limits the audience to misuse the term recession. The problem goes beyond the semantic, however. Farber remarks that the fact that the displacement survey covers 1982–83 and 1990–91 "is a fortuitous breakdown of the 1982–91 period." I would call it unfortunate. U.S. economic activity peaked in mid-1981, reached a trough in mid-1982, and expanded rapidly in 1983. By contrast, the economy expanded until mid-1990, contracted until early 1991, and then remained flat. Although Farber is right that both periods had high unemployment, they differ completely in terms of dynamics.

The overwhelming general conclusion of the paper is that the processes of job loss and job finding remained structurally unchanged during the 1980s. That is, the important differences in labor-market conditions in the recession of 1990–91 relative to the recession of 1981–82 can be traced to large differences in the industries responsible for job losses. There do not seem to be any important changes in the way the labor market assimilated the displaced workers.

Despite problems in the displaced workers survey, Farber's paper provides a wealth of new data on the operation of the labor market. Farber makes no grand inferences about alternative theories of the labor market, but his evidence gives a lot of help to those who venture into the dangerous territory of inference.

Comment by John Pencavel: This is a most informative paper on a topical issue: the loss of jobs. Two questions are addressed:

(1) What types of workers have lost their jobs during the past ten years?
(2) What has been the monetary cost of these job losses to these workers?

First, one must define who is a job loser. The research reported in this paper considers job losers to be those who left their job in the preceding two years because they were laid off, because a plant closed, or because an employer went out of business. One question that may be asked of this definition is whether the people so categorized constitute a well-defined and homogeneous group.

As is well known, the distinction between employment terminations initiated by the employer (layoffs) and terminations initiated by the employee (quits) is blurred. In addition, there is a question whether labor markets operate in such a way that workers whose jobs are at risk can and do reduce the likelihood of their layoff by offering their employers wage concessions to maintain their jobs. If so, the group of job losers consists in part of a self-selected sample—those who accept the higher risk of being laid off rather than accept wage reductions to increase the probability of maintaining an existing job. If there is anything to this argument, comparisons between these job losers and those who did not so lose their jobs are compromised by questions about who chooses to be in which group.

In fact, although some highly publicized wage concessions occurred in the early 1980s, their extent appears to be small, perhaps because many workers are not in a position of offering wage reductions to their employers.[1] If so, the problem of sample endogeneity is moot, as Farber presumes. This may well be correct, but I would have welcomed a more thorough examination of the issue.

Having thus addressed the question of whether the sample of job losers is well defined, the next question is whether this group of people is homogeneous. In fact, evidence already exists showing that the labor market experiences of people who lost their jobs through plant closings have been different from those experienced by workers laid off from plants continuing operations. Farber describes these differences as "small," but that is not quite accurate. The change in earnings for laid-off blue-collar workers is similar to the change for blue-collar workers displaced through plant closings, but the reduction in earnings among laid-off white-collar workers was about twice that experienced by white-collar workers displaced through plant closings. For both blue- and white-collar workers, those displaced through plant closings were unemployed about a month less than those displaced through layoff.[2] These magnitudes are not "small" by comparison with the effects of job loss measured by Farber later in his paper.

Setting aside these questions about the definition of job losers and

1. The self-selection argument would suggest that, other things equal, those who retain their jobs have lower wages than those who lose them. In fact, holding constant a number of observable attributes of these workers, it appears that job losers tend to have lower wages than those who did not lose their jobs. (Hamermesh 1988).) This may be interpreted either as questioning the importance of the self-selection argument or as a reflection on our ability to measure these effects.

2. These numbers are taken from table 2 of Gibbons and Katz (1991).

the selection of the samples, let me turn to the two questions taken up in this paper. First, what types of workers have lost their jobs during the past ten years?

In examining this question, Farber focuses on the difference between job losers in the most recent recession and those in the recession of the early 1980s. He finds that the composition of job losers in 1990–91 includes more older and more well-educated workers compared with the 1982–83 recession, and he conjectures that this difference accounts for the greater disquiet recently over the state of the economy.[3] I would have liked to have learned whether the incidence of job loss by predisplacement earnings is also different in the current recession: should I infer that job losers in 1990–91 are more heavily concentrated among high-wage workers than was the case for those who lost their jobs in 1982–83?

This finding about the difference between the two recessions should be of special interest to macroeconomists, some of whom appear to operate as if all recessions are alike. In fact, it would surely be remarkable if recessionary shocks in different markets were the same over time, and I would conjecture that, if Farber could apply his work to recessions before the 1980s, he would find job losers in each recession to be different from another. I believe that this recent recession is much more of a New England and Californian phenomenon, while the 1982–83 recession hit the Midwest and Mid-Atlantic states hardest. Is this true? I expected Farber to examine the regional incidence of job losers.

The second question that Farber addresses is the monetary cost of the job losses to the job losers. To answer this, Farber examines (a) the probability of a job loser being reemployed, (b) the probability of a job loser working at a part-time job, and (c) the difference between the job loser's predisplacement earnings and his earnings on his subsequent job.

These measurements are made by comparing the job losers' experiences with those of the workers who did not lose their jobs in this manner. Farber shows quite convincingly that job losers suffer on all

3. Is it true, as Farber writes, that "the public [is] more 'unhappy' with this recession than the last"? Both the coincidence of this recession with a presidential election and the fact that experiences in the past are usually perceived through a telescope viewed through the wrong end tend to make one lose perspective.

three dimensions: they are less likely to be employed, they are more likely to be working at a part-time job, and they suffer about a 9 percent wage loss on average.[4] There is evidence, however, that these losses decrease with time, but because the author restricts his attention to the experiences of these workers over a two-year period, one does not know whether the gap is totally closed.

There are several comments to be made about this work. First, my understanding is that the tapes that Farber is working with also contain information on whether the job losers received unemployment insurance after losing their jobs and whether they have lost health insurance coverage. These factors should surely appear in cataloging the monetary costs from job loss. In fact, in the first half of the 1980s, at least, about two-thirds of job losers received unemployment compensation,[5] and in several cases they received company-funded supplemental unemployment benefits or trade adjustment assistance. In some instances, "some of the workers had their pre-layoff earnings almost entirely replaced by benefits, at least for a time."[6]

This raises an issue of considerable importance. All of Farber's research in this paper addresses the experiences of the average job loser. However, my expectation is that there is a considerable dispersion of experiences and that public concern may be less directed to the average experience and more directed to those job losers who fared badly. My guess is that some job losers ended up better off than they were before they were displaced. These may include those receiving unemployment benefits and other forms of financial assistance. They may have gone on to find high-paying jobs.

4. Farber computes this wage loss by a difference-in-difference estimator, where the change in earnings for displaced workers is compared with the change in earnings of those workers who have not been displaced (after holding constant other factors). Other researchers investigating this issue have simply computed the change in earnings of the displaced workers. Although Farber does not remark on it, usually this change in earnings of the displaced workers comes close to his difference-in-difference estimator, and there is little extra information gleaned from comparing this difference with that experienced by nondisplaced workers. (I am comparing here the row $\gamma_2 + \gamma_3$ in table 9 with the simple change in earnings of full-time workers in the second column of table 7.)

5. This number comes from table 9 of Flaim and Sehgal (1985). Note that they define job losers more narrowly than Farber. In particular, they exclude people who had not held their predisplacement job for less than three years, a restriction that probably results in increasing the fraction who received unemployment insurance.

6. Flaim and Sehgal (1985), p. 12.

I also suspect that some job losers have suffered very large losses, receiving no form of compensation for their displacement, experiencing long bouts of unemployment, or taking a job at substantially lower pay than their former jobs. This is the group that a compassionate public policy might want to help. How large is this group? I think Farber's research would be much more interesting if he gave some idea of the size of this group and of the change in their welfare.

If this analysis had been conducted in a European country, it would have been essential to take into account the severance payments that most displaced workers would have received. Such severance payments are believed to be unusual in this country, but I do not know whether this belief is correct. From a survey that Jim Dertouzos undertook several years ago of workers displaced from the newspaper industry, I learned that many more workers received severance pay than I would have expected, but I do not know whether the experience of these newspaper workers can be extended to other industries. It does raise the public policy question, however, of whether severance payments should be mandated, as most other industrialized countries do.

I think the case for mandatory severance pay has yet to be demonstrated. There is reason to believe that by increasing the expected cost of labor, such mandated payments discourage hiring, whereas the more appealing policy, I should have thought, would be one that encouraged the hiring of labor and especially the hiring of workers who have been unemployed a long time. Ways to achieve this are obvious: an employer who hires a worker who has been unemployed for x weeks could have his payroll taxes on that worker excused for 3x weeks.

Farber writes that, overall, the costs of job loss to the average displaced worker "are substantial," but he does not really show this. His research shows the reduction and the probability of full-time employment of job losers growing smaller over time, while the two-year wage loss to be on the order of 9 percent, roughly equivalent to the wage returns to one more year of schooling. Even this wage loss may diminish further with time. I would characterize the findings as indicating that the average job loser suffers substantial monetary costs in the short term unless he receives severance payments or unemployment compensation, but these costs dissipate over time. I fear that the monetary costs for a minority of job losers are very high, and I would like Farber to tell me whether this is the case.

General Discussion: John Haltiwanger noted that the author's measures of job loss were similar in their cyclical properties to measures of job destruction found from looking at establishment-level data. Paradoxically, although the data presented in the paper showed that the rates of job loss among displaced workers were higher in manufacturing than in nonmanufacturing industries, establishment-level data seemed to show that overall rates of job creation, destruction, and reallocation were higher for nonmanufacturing than for manufacturing industries. Attempting to explain this contradiction, he suggested that nonmanufacturing industries might be able to accommodate job reallocation without inducing displacement. Haltiwanger also said that, according to establishment-level data, job destruction in manufacturing was concentrated within individual establishments: plants that decreased employment by more than 20 percent on an annual basis accounted for more than two-thirds of all job destruction in manufacturing. He wondered whether the concentration of job destruction was different in nonmanufacturing industries and whether such concentration might be different during different time periods. In particular, it would be interesting to know whether job loss in nonmanufacturing industries was more concentrated during the most recent recession than it was in earlier downturns.

Katharine Abraham remarked on the importance of the paper's results showing an increase in the probability of job loss among more-educated workers during the 1980s. She suggested that, to some extent, this phenomenon might have occurred because more-educated workers were disproportionately employed by those industries that were experiencing an increasing proportion of total job loss. In addition, she argued, this phenomenon might be partially a reflection of a change in the way firms view and treat their white-collar workers. In earlier postwar recessions, according to Abraham, the drop in white-collar employment in the manufacturing sector was quite small relative to the drop in blue-collar employment. But in the most recent recession, the white-collar drop was almost as large as the blue-collar one. It is not clear, she said, whether the employment changes in manufacturing during the last recession represented a one-time restructuring or, alternatively, a process that would be repeated, the latter case possibly indicative of a permanent change in the way that employers view their white-collar workers.

Ariel Pakes suggested a novel approach for defining the returns to education. He said that more-educated people might be able to get jobs more quickly than less-educated workers. As a result, he said, one might regard the return to education not only in terms of wages, but also in terms of employment flexibility. He said the data presented in the paper might allow for calculations along those lines.

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