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The Changing Face of Job Loss in the United States, 1981–1995

IN THE FIRST WEEK of January 1996, AT&T announced it was restructuring its operations and reducing its managerial work force by 40,000. This was only the latest in a string of widely publicized large labor force reductions announced by major American corporations. The public perceives that corporations are responding to increased competitive pressure by restructuring and downsizing their work forces, particularly their white-collar work forces, to an unprecedented degree and that the workers so displaced are suffering substantial economic hardship.[1] In this study I examine evidence from Displaced Workers Surveys (DWSs) from 1984 to 1996 to provide a comprehensive picture of the incidence and consequences of job loss between 1981 and 1995 to determine the extent to which labor force data support these perceptions.

Data limitations make it difficult to know what groups of workers lost jobs before the 1980s. The DWSs, which have been regular supplements to the Current Population Survey (CPS) at two-year intervals since 1984, have useful information on job loss, however.[2] Specifically, these surveys ask workers if "in the past five years" (past three years in the 1994 and 1996 DWSs) they have "lost or left a job because of a

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1. Former Secretary of Labor Robert Reich makes this argument in "How to Avoid These Layoffs," *New York Times*, January 4, 1996, p. A21.

2. The Displaced Workers Survey was part of the January CPS in even years from 1984 through 1992. It was part of the February CPS in 1994 and 1996.

plant closing, an employer going out of business, a layoff from which [the worker] was not recalled, or other similar reason.'' These data have much to say about job loss, and they form the core of my empirical analysis.

My earlier paper in this journal used the five DWSs from 1984 through 1992 to examine job loss, and the current paper is a natural extension of that earlier work in two ways.[3] First, I bring the earlier analysis up to date by using the two most recent surveys to examine job loss through 1995. Second, I focus on the distinctions regarding the stated reason for (or cause of) the job loss. The three substantively important classifications considered are job loss due to plant closing, slack work, and elimination of a position or shift. Several other, less common, options coded in the survey, including seasonal job ended, self-employment ended, and other, are combined into a fourth category, ''other.''[4] A specific model of job loss by reason is not provided here but, more generally, the evolution of the incidence and costs of job loss due to these various stated reasons is investigated during the 1981–95 period.[5]

One factor that makes investigation of job loss by stated reason interesting is that the term ''position abolished'' resonates with the well-publicized round of corporate downsizing and restructuring of the past several years. A worker's self-report of the reason for job loss is bound to be arbitrary to some degree, however. Workers who lose their jobs because of corporate downsizing or restructuring could conceivably report any of the three reasons noted here. If the employer ceased operations at the site where the worker was employed, then the worker would likely report a plant closing. If the employer reduced employment across all or many workplace functions because of a decline in demand without ceasing operations, then the worker could report either slack work or an abolished position. If the employer ''streamlined'' operations by reducing employment in certain functions—management, for example—then the worker is likely to report that the position was abolished.

3. Farber (1993).

4. I discuss the coding of the reason for job loss and how the survey design deals with these reasons in more detail later.

5. As I discuss in the next section, an earlier paper by Gibbons and Katz (1991) uses the distinction between plant closing and slack work to test an adverse-selection model.

I do not want to make too much of these distinctions in worker self-reports. These are necessarily subjective attributions, and different workers could well respond differently regarding jobs lost in identical circumstances. For example, blue-collar workers, who historically have been subject to layoff due to cyclical fluctuations in demand, may report job loss due to a restructuring as caused by slack work. In contrast, white-collar workers, who historically have not been nearly as susceptible to layoff during cyclical fluctuations, may likely report job loss due to restructuring as caused by the position being abolished.

An important part of the task at hand is to investigate the extent to which real economic differences underlie job loss due to the various stated reasons. I get some purchase on this question in several ways. First, the behavior over time and across types of workers of job-loss rates by stated reason can be informative about the cyclical sensitivity of and secular changes in the various types of job loss. Second, differences in the incidence of job loss by reason across types of workers (for example, by education and occupation) can be informative about reporting differences. Finally, the consequences of job loss by reported reason are informative about the costliness of different types of job loss and whether the distinctions by type of job loss are important.

Review of Recent Literature on Job Stability

A series of analyses of job stability have relied on mobility supplements to various January CPSs.[6] An influential early analysis was carried out by Hall, who used published tabulations from some of the January mobility supplements to compute contemporaneous job retention rates. Although any particular new job is unlikely to last a long time, Hall found that a job which has already lasted five years has a substantial probability of lasting twenty years. A substantial fraction of workers will have a ''lifetime'' job (defined as lasting at least twenty years) at some point in their life. Men are substantially more likely than women, and whites are substantially more likely than blacks to have

6. These mobility supplements, conducted in 1951, 1963, 1966, 1968, 1973, 1978, 1981, 1983, 1987, 1991, and 1996, contain information on how long workers have held their current jobs. Only the data since 1973 are available in machine-readable form.

such a lifetime job.[7] Ureta used the January 1978, 1981, and 1983 mobility supplements to recompute retention rates using artificial cohorts rather than contemporaneous retention rates.[8] Like Hall, she found that lifetime jobs are an important feature of the U.S. labor market, but she finds smaller differences by sex.

Several more recent papers have used CPS data on job tenure to examine changes in employment stability.[9] Swinnerton and Wial, using data from 1979 through 1991, analyzed job retention rates computed from artificial cohorts and found a secular decline in job stability in the 1980s.[10] In contrast, Diebold, Neumark, and Polsky, using CPS data on tenure from 1973 through 1991 to compute retention rates for artificial cohorts, found that aggregate retention rates were fairly stable over the 1980s but declined for high school dropouts and for high school graduates relative to college graduates.[11] A direct exchange between Diebold, Neumark, and Polsky and Swinnerton and Wial appears to support the view that job stability did not generally decrease during the 1979–91 period.[12] Farber, using CPS data on job tenure from 1973 through 1993, found that the prevalence of long-term employment had not declined over time, but the distribution of long jobs had shifted. Less educated men were less likely to hold long jobs than they had been previously, a finding that is offset by a substantial increase in the rate at which women hold long jobs.[13]

Rose used data from the Panel Study of Income Dynamics (PSID) to measure job stability by examining the fraction of male workers who do not report any job changes in a given time period, typically ten years. The fraction of workers who reported no job changes in a given length of time was higher in the 1970s than in the 1980s. He argued this is evidence of increasing instability of employment.[14] Jaeger and Stevens used data from the PSID and the CPS mobility and benefit

7. Hall (1982).

8. Ureta (1992).

9. In addition to the January mobility supplements, information on job tenure was collected in pension and benefit supplements to the CPS in May 1979, May 1983, May 1988, and April 1993.

10. Swinnerton and Wial (1995).

11. Diebold, Neumark, and Polsky (1997).

12. Diebold, Neumark, and Polsky (1996), Swinnerton and Wial (1996).

13. Farber (1998).

14. Rose (1995).

supplements on (roughly) annual rates of job change to try to reconcile evidence from the CPS and PSID on job stability. They found little evidence in either survey of a trend in job stability, although the estimates from the PSID are rather imprecise.[15] Unfortunately, because of the design of the PSID, neither of these studies examined the mobility experience of women.

In a paper in an earlier issue of this journal, I used the five DWSs from 1984 to 1992 to examine changes in the incidence and costs of job loss during the 1982–91 period.[16] I found slightly elevated rates of job loss for older and more educated workers in the slack labor market in the latter part of the period compared with the slack labor market of the earlier part of the period. But job-loss rates for younger and less educated workers were substantially higher than those for older and more educated workers throughout the period. These findings are consistent with the long-standing view that younger and less educated workers bear the brunt of recessions.

Gardner carried out the first analysis that incorporated the 1994 DWS. She examined the incidence of job loss from 1981 to 1992. Although she found roughly comparable overall rates of job loss in the 1981–82 and 1991–92 periods, the industrial and occupational mix of job loss changed over this period. Job loss decreased among blue-collar workers and workers in manufacturing industries and increased among white-collar workers and workers in nonmanufacturing industries.[17]

A substantial literature uses the DWS to study the postdisplacement employment and earnings experience of displaced workers.[18] This work demonstrates that displaced workers suffer substantial periods of unemployment and that earnings on jobs held after displacement are substantially lower than predisplacement earnings. In my earlier paper in this journal, I found no difference on average in the consequences of job loss between the 1982–83 recession and the 1990–91 recession.[19]

The earnings loss suffered by displaced workers is positively related to tenure on the predisplacement job. Kletzer finds further that the

15. Jaeger and Stevens (1997).
16. Farber (1993).
17. Gardner (1995).
18. See, for example, Podgursky and Swaim (1987), Kletzer (1989), and Topel (1990).
19. Farber (1993).

postdisplacement earnings *level* is positively related to predisplacement tenure, suggesting that workers displaced from long jobs are more able on average than those displaced from shorter jobs.[20] In recent work Neal, using the DWS, and Parent, using the National Longitudinal Survey of Youth, found that workers who find new employment in the same industry from which they were displaced earn more than industry switchers do.[21] This new work suggests that Kletzer's finding, that postdisplacement earnings are positively related to predisplacement tenure, results from the transferability of industry-specific capital. Workers who are reemployed in the same industry ''earn a return'' on their previous tenure; those reemployed in a different industry do not.

Gibbons and Katz take a different approach, analyzing the consequences of job loss in the context of an adverse selection model. Specifically, they argue that workers displaced because of ''slack work'' are subject to selection on the part of their employer. Within the limits of human resource management policies that give preference in retention to high tenure workers, employers are likely to lay off less productive workers when demand declines. In contrast, workers displaced because of a ''plant closing'' are not subject to such selection. Employers must lay off all workers in such situations. On this basis, Gibbons and Katz argue that workers displaced because of slack work will fare worse after displacement than workers displaced because of a plant closing. They present evidence from the 1984 and 1986 DWSs consistent with this adverse selection model.[22]

Some Data Considerations

I analyze data on individuals between the ages of 20 and 64 from the DWS supplements to the January CPSs in 1984, 1986, 1988, 1990, and 1992, and the February CPSs in 1994 and 1996. Each DWS from 1984 to 1992 asks workers if they were displaced from a job at any time in the preceding five-year period. The 1994 and 1996 DWSs ask workers if they were displaced from a job at any time in the preceding three-year period. Displacement is defined in the interviewer instructions as

20. Kletzer (1989).
21. Neal (1995), Parent (1995).
22. Gibbons and Katz (1991).

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 the individual alone'' are not considered displacement.[23] 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 that result from business decisions of firms unrelated to the performance of particular workers.

What Is a Job Loss?

Some important issues of definition are implicit in the design of this question that do not seem to have been addressed adequately in earlier work using the DWS. Job loss as measured in these data almost certainly does not represent all job loss about which we ought to be concerned. Specifically, the distinction between quits and layoffs is not always clear. Firms may wish to reduce employment without laying off workers, which they might accomplish by reducing or failing to raise wages.[24] This tactic can encourage workers (perhaps those least averse to a layoff because they have better alternatives) to quit. Other workers (perhaps those most averse to a layoff because they have worse alternatives) might be willing to continue to work at reduced wages. To the extent that these are important phenomena, the sample of individuals observed to be displaced by the definition used in the DWS is a potentially nonrandom subsample of ''truly displaced'' workers. The consequences of this are difficult to gauge, but it is worth noting that the ability of employers to offer wage decreases to their workers can be quite limited.

More important for analysis of ''involuntary'' job change is the fact that the DWS collects and reports information on at most one job loss for each individual. For workers with more than one job loss, this information refers to the longest job lost. Because it is possible (and

23. U.S. Department of Commerce, 1988, Section II, p. 4.

24. This is consistent with work by Jacobsen, LaLonde, and Sullivan (1993), who find that displaced workers suffer wage declines even before they are displaced.

not rare) for workers to have lost more than one job in a five-year (or three-year) period, the DWS cannot be used to measure the total quantity of job loss. At best, it measures the number of workers who have lost at least one job in the relevant time period.[25]

Defining the Rate of Job Loss

Even if all agree that the focus of the analysis should be on those workers who have lost at least one job, the problem remains of how to compute the job-loss rate. Consider some category of workers (defined by such characteristics as age, sex, and education). The DWS directly measures the number of workers in that category who have lost at least one job, which is a reasonable numerator for the category-specific job-loss rate. But the pool of workers who were at risk to lose a job during the relevant time period is not easily measurable. I take the straightforward approach, as I did in my earlier study,[26] of using the number of workers in the given category employed at the survey date as the measure of the relevant pool, and this number serves as the denominator in the calculation of the job-loss rate. This is likely to be a good approximation unless employment in the group is changing rapidly over the relevant time period (three years). All job-loss rates presented in the next section are computed on this employment basis.

Later I carry out multivariate analyses of the probability of job loss using disaggregated data. The sample consists of all workers employed at the survey date and all workers who reported a job loss in the relevant period (whether employed or not). This results in estimates analogous to the employment-based job-loss rates I present for specific groups.

Changes in Survey Design and Comparability: The Adjusted Job-Loss Rate

A second issue is the relevant time period over which to compute the rate of job loss. I use three-year rates of job loss, which are computed as the number of workers who report having lost a job in the three

25. There also is the commonly noted problem of recall bias because workers fail to report job loss that occurred long before the interview date. See Topel (1990) for evidence suggesting that recall bias is an important problem in the DWS. Farber (1993) also presents some evidence on this issue.

26. Farber (1993).

calendar years before the survey date divided by employment at the survey date. This calculation is straightforward using the data from the 1994 and 1996 DWSs because the central question on job loss uses a three-year recall period. But an important problem of comparability needs to be addressed because the earlier DWSs use a five-year recall period. Comparing displacement rates from a five-year period with those from a three-year period makes little sense, and it would seem reasonable to count job loss only in the most recent three years from the 1984–92 surveys. Workers who reported losing jobs four and five years ago would be counted as nonlosers. The result would be a three-year job-loss rate that could be compared with the three-year job-loss rate computed directly from the 1994 and 1996 DWSs. Call the three-year job-loss rate for group i computed this way r_{3i}.

The two sets of data are still not comparable, however. The quantity r_{3i} will be appropriate for the 1994 and 1996 DWSs, but will underestimate job loss in the earlier DWSs because some (probably non-negligible) fraction of the workers who reported losing a job four or five years ago lost at least one more job in the most recent three-year period. These subsequent job losses would not be counted.[27] The problem is that three-year job-loss rates computed from the 1994 and 1996 DWSs include jobs lost in the last three years by individuals who lost longer jobs four or five years ago. In contrast, the three-year job-loss rates computed from the 1984–92 DWSs do not include jobs lost in the last three years by individuals who also lost (longer) jobs four or five years ago.[28] Note that the change in recall period would not have been a problem in this regard had the one job loss allowed per worker been the most recent job loss rather than the longest job loss.

The solution I adopt to this problem is to adjust upward the three-year job-loss rates from the 1984–92 DWSs to account for the downward bias

27. In my earlier work using the DWS (Farber, 1993), I used two-year job-loss rates computed from each of the DWSs from 1984 to 1992. This method suffers from the general problem of missing job loss because the question asks only for one job loss in a five-year period. Because all five surveys considered in that study use the same five-year recall period, the downward bias in job-loss rates would be of roughly similar magnitude.

28. The recall period was shortened to three years in the 1994 DWS because the recall bias problem was felt to be too severe four and five years out (Topel, 1990). Although it probably it would have been better to have done the surveys from the beginning with a three-year recall period, the effect on comparability of changing from a five-year to a three-year recall period seems not to have been considered.

from the five-year recall period used in those surveys.[29] To get some idea of the magnitude of that downward bias and to provide the raw material for the adjustment factor I will apply to these DWSs, I examined data from the PSID covering the period from 1968 to 1985.[30] These longitudinal data allow me to calculate the fraction of workers who lost a job (reported an involuntary job change) four or five years ago who subsequently lost a job in the relevant three-year window. Let t represent the "current" period. I compute two fractions. First, I condition on losing a job in $t-4$ (n = 1,558) and compute the fraction of these workers who report losing at least one job in the $t-3$ to $t-1$ period. This fraction, denoted by δ_4, is 0.3017 based on the PSID. Second, I condition on losing a job in $t-5$ (n = 1,305) and compute the fraction of these workers who report losing at least one job in the $t-3$ to $t-1$ period. This fraction, denoted by δ_5, is 0.2705 based on the PSID.

This analysis suggests that a substantial fraction of workers who lost jobs four or five years ago lose jobs subsequently. These subsequent losses will not be picked up using a three-year job-loss rate. Although no perfect solution to this problem exists, in the analyses of job-loss rates in this study, I use the statistics from the PSID to adjust upward the three-year job-loss rates computed from the 1984–92 DWSs. Specifically, let r_{3i} represent the unadjusted three-year job-loss rate for group i. This is computed naively as the ratio of the number of respondents to the DWS who say they have lost a job in one of the three years before the survey year to employment at the survey date. Because the later DWSs have only a three-year recall window, this count includes all respondents who report being displaced in those surveys. The earlier DWSs have a five-year recall window, and respondents who report a displacement four or five years ago are counted as being not displaced.[31] The adjusted three-year job-loss rate for group i, denoted by r^a_{3i}, is defined as

29. The three-year period differs from the two-year period used in my earlier paper (Farber, 1993). The use of three-year job-loss rates is more straightforward in that it requires no adjustment of the data from the 1994 and 1996 DWSs. Had I adopted a two-year job-loss rate, I would have been faced with adjusting *all* of the DWSs with different factors for the earlier and later surveys due to the different recall periods.

30. This sample includes 49,922 annual observations on 6,184 individuals from the random subsample of the PSID. This sample is 86.9 percent male, due to the structure of the PSID.

31. The 1984–92 DWSs were conducted in January of the relevant years, and some

$$r^a_{3i} = r_{3i} + \delta_4\rho_{4i} + \delta_5\rho_{5i}, \tag{1}$$

where ρ_{4i} and ρ_{5i} represent, respectively, the rate of job loss in $t-4$ and $t-5$ for group i.

Here is an illustration of those calculations and the effect on comparisons of three-year job-loss rates from the 1994 DWS and earlier DWSs. The three-year job-loss rate computed for 20- to 64-year-olds from the 1994 DWS (covering job loss in the 1991–93 period) is 0.0865. This compares with an average three-year unadjusted job-loss rate of 0.0639 computed directly from the 1984–92 DWSs (covering job loss in the 1981–91 period). The average of the job-loss rates in the fourth year prior to each of the 1984–92 DWSs is 0.0138, and the average of the job-loss rates in the fifth year prior to each of the 1984–92 DWSs is 0.0110. Using the repeat job-loss fractions from the PSID, an adjusted three-year job-loss rate is computed as the sum of the unadjusted three-year job-loss rate plus the shares of the fourth-prior and fifth-prior years job-loss rates that account for repeat job losers. Based on equation 1, this rate is computed to be 0.0639 + (0.3017)(0.0138) + (0.2705)(0.0110). The resulting adjusted three-year rate is 0.0711, which is substantially (11.2 percent) higher than the unadjusted three-year rate. All three-year job-loss rates and analyses of the incidence of job loss in this study are adjusted using this technique and, necessarily, condition on the repeat job-loss fractions computed from the PSID.[32]

Several weaknesses are inherent in this approach. First, the repeat job-loss fractions do not vary with worker characteristics or time. Unfortunately, the sample size in the PSID and other longitudinal data sets is not large enough to create group- and time-specific estimates of δ_4 and δ_5. Second, this adjustment is upward biased in the sense that not all repeat job losses measured in the PSID were from shorter jobs than the "initial" job loss, as would be appropriate to make the job-loss rates comparable with those computed from the 1994 DWS. Thus δ_4

workers reported being displaced in the survey month. I do not include these displacements. Thus the three-year job loss rate refers to the three years prior to the survey year (for example, 1981–83 for the 1984 DWS). The 1994 and 1996 DWSs were conducted in February and asked explicitly about job loss in three prior calendar years.

32. In the section on job loss, I describe the analogous procedure used to adjust the data for multivariate analyses of three-year job-loss probabilities using disaggregated data.

and δ_5 are upward biased estimates of the probability of repeat job loss from a *shorter* job. Third, another upward bias could result if some individuals with multiple job losses reported the most recent job loss rather than the loss of the longest job. This could occur if the earlier job loss is forgotten or if individuals are not aware of the instruction to report the longest job lost.[33] But these upward biases are at least partly offset by recall bias making the measured rates of job loss four and five years ago (ρ_{4i}) and ρ_{5i}) smaller than they should be.

Finally, a problem occurs when adjusting reason-specific job-loss rates. My adjustment procedure effectively assumes that job losses in the most recent three years are for the same reason as the job loss four or five years ago. For example, if a worker is displaced because of a plant closing in $t-4$, then the adjustment procedure assumes some probability of another job loss due to plant closing but no probability of a job loss due to some other reason in the $t-3$ to $t-1$ time period. The evidence cited from the PSID does not address whether this assumption is appropriate. It does not have a breakdown of job loss by cause that is comparable to the DWS classification. Given a reasonably stable distribution of job loss by reason, this is not likely to be a serious deficiency, however.

Overall, although the adjustment I propose is surely not perfect, it is difficult to think of a better feasible alternative.

Another Change in Survey Design: The Reason for Job Loss

A second problem results from an unfortunate decision made in the design of the 1994 and later DWSs. In the 1984–92 DWSs, all individuals were asked if they lost a job in the last five years. If the response was yes, then the individual was asked the reason for the job loss. Six responses were allowed: (1) plant closing, (2) slack work, (3) position/

33. In fact, the DWS survey question itself does not mention what job to report in the event of multiple job losses in the relevant window. This is covered in the interviewer instructions, which are likely to come into play only when the respondent asks what an appropriate response would be. Thus respondents are free to report the most recent job loss even when they have lost other, longer jobs. The Bureau of Labor Statistics appended a set of debriefing questions to the February 1996 DWS that were asked of those displaced workers in a subset of the CPS rotation groups. One objective of these questions was to determine the extent to which multiple job losers, in fact, reported the loss of the longest job. However, the bureau has not yet released these data or any tabulations based on these data.

shift abolished, (4) seasonal job ended, (5) self-employment ended, and (6) other. Regardless of the response to this question, all individuals who suffered a job loss were then asked a series of questions regarding the job they lost and their experience since the job loss. In the 1994 and 1996 DWSs, however, these follow-up questions were asked only of those individuals who report one of the first three reasons for their job loss, making it difficult to learn much about the incidence or post-displacement costs of job loss from all causes in a way that is comparable over time.

In fact, plant closing and slack work were the largest categories of job loss in the 1984–92 DWSs, but, more recently, the "other" category has been growing. The analysis here does not include loss of self-employment jobs, and seasonal employment accounts for only a small fraction of all job loss. The "other" category is somewhat larger (and growing) but poorly defined. No doubt this factor motivated the decision to follow up only the first three responses. As I show later, however, the distribution of job loss by reported reason has shifted substantially in the 1994 DWS and even more dramatically in the 1996 DWS. A much larger fraction of the job loss in the 1994 and 1996 DWS is due to "position or shift abolished," and the "other" category has grown substantially as well. In fact, the "other" category was the modal job-loss category in the 1996 DWS. It is unfortunate that only limited analysis of job loss for "other" reasons is possible.[34]

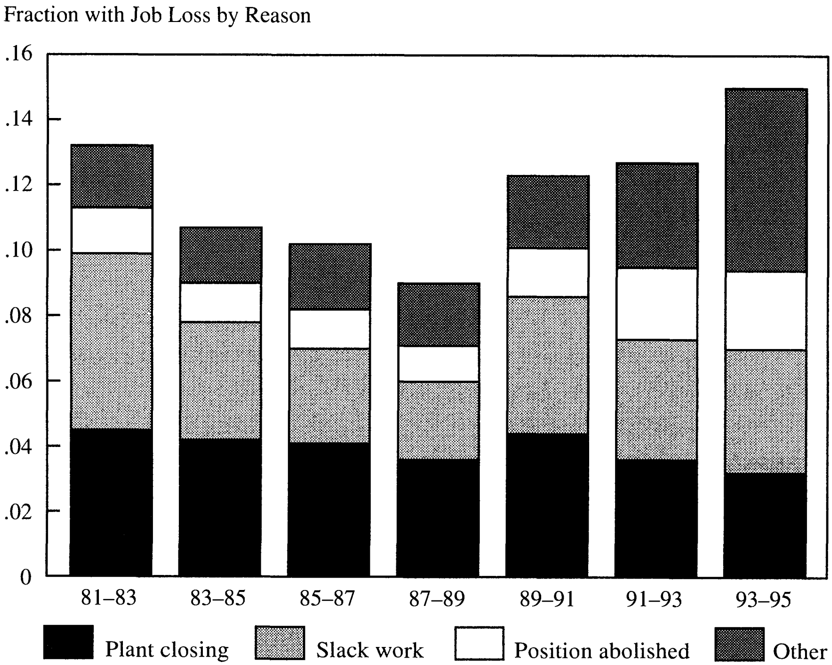
The Incidence of Job Loss: Univariate Analysis

Information on rates of job loss is presented most accessibly in graphical form, and the discussion here is organized around a series of figures.[35] Figure 1 contains plots of adjusted three-year job-loss rates

34. The debriefing questions appended to the February 1996 DWS also provide more detailed information on the reason for job loss that may shed some light on the makeup of the "other" category. However, as noted earlier, the Bureau of Labor Statistics has not yet released these data or any tabulations based on these data.

35. The numerical values underlying these figures are contained in the appendix tables.

Figure 1. Rate of Job Loss by Reason



Source: Author's calculations.

computed from each of the seven DWSs from 1984 to 1996.[36] These stacked bar graphs provide information not only on overall job-loss rates (the total height of each bar) but also on job-loss rates by reason (the shaded segments of each bar). Four classifications of reason are presented: (1) plant closing, (2) slack work, (3) position/shift abolished, and (4) other.[37]

Several interesting features of the overall job-loss rates are presented in figure 1, some of which I noted in my earlier work using the DWS.[38] The cyclical behavior of job loss is apparent at least through 1991. The

36. All adjusted job-loss rates from the 1984–92 DWSs are computed using equation 1 and the common values of δ_4 and δ_5 computed from the PSID. Group-specific values of the other rates required to compute the adjusted three-year job-loss rates (r_{3i}, ρ_{4i}, ρ_{5i}) are used. All counts are weighted using the CPS sampling weights.

37. The "other" category I use merges the "seasonal job ended," "self-employment ended," and "other" categories as coded in the DWS.

38. Farber (1993).

1981–83 job-loss rate is relatively high at about 13 percent. This is a period with a slack labor market (average unemployment rate of 9 percent, rising from 7.6 percent in 1981 to 9.6 percent in 1983). The job-loss rate then falls during the tightening labor market of the mid-1980s (average unemployment rate from 1983 to 1989 of 6.9 percent, falling from 9.6 percent in 1983 to 5.3 percent in 1989). The job-loss rate then rebounds to levels similar to the 1981–83 period as the labor market weakens after 1989 (with the unemployment rate rising from 5.3 percent in 1989 to 6.7 percent in 1991). As is clear from this comparison of unemployment rates, the latter recession is less severe than that of the early 1980s. Thus the job-loss rates are surprisingly comparable in the two slack labor markets.

The slackness in the labor market continued in the 1991–93 period despite an ongoing modest recovery (unemployment rate rising to 7.4 percent in 1992 before declining to 6.8 percent in 1993). The job-loss rate is higher in this period than even in the severe recession of the early 1980s. What is most striking is that the job-loss rate increased dramatically in the 1993–95 period despite the sustained economic expansion accompanied by a further decline in the unemployment rate to 5.6 percent in 1995. This evidence is consistent with claims of a secular decline in job security.

Some interesting patterns to job loss by specific reason are apparent. First, the rate of job loss due to plant closings seems to have been relatively constant, with a smaller secular decline over time. In contrast, the rate of job loss due to slack work seems to have a larger cyclical component combined with a smaller secular increase over time. One (speculative) interpretation of this result is that plant closings are a response to secular declines in demand for specific products, whereas job loss due to slack work is a typical response to cyclical fluctuations in demand where only marginal adjustments to output are required. Second, and more interesting from the standpoint of a secular increase in instability, is that the rates of job loss due to "position/shift abolished" and "other" were relatively constant through the 1989–91 period but have risen substantially since then.

Most striking is the dramatic increase in job loss for "other" reasons since 1991. This makes it especially unfortunate that the previously discussed design problem in the 1994 and 1996 DWSs prevents examining this increase in as much detail as would be ideal. I can only speculate on what the "other" category comprises.[39] One possibility is that it represents misreported quits. To the extent that is the case, the consequences of job loss will be smaller for these workers than for workers who lose jobs for the stated reasons. Another possibility is that the "other" category represents workers who accept buyouts or take early retirement with incentives. The latter at least would show up as occurring disproportionately for older workers and resulting in lower postdisplacement employment probabilities. Finally, job separations for health or disability reasons might be reported as "other." This too might show up as lower postdisplacement employment probabilities. To the extent possible, I examine evidence on the rate and consequences of job loss by reported reason in an attempt to learn something about the "other" category.

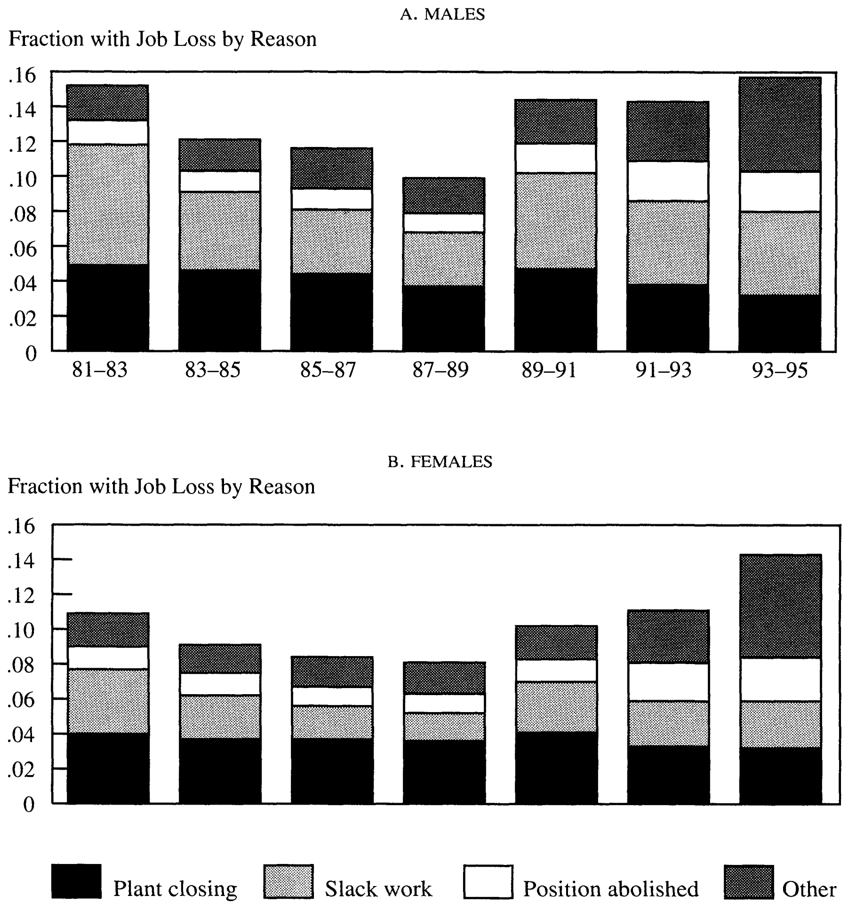
Given the increase in job loss because of "position/shift abolished" and the fact that the category has overtones of downsizing and restructuring, I also focus on the extent to which the rate and consequences of job loss for this reason vary by demographic group. To determine whether "position/shift abolished" is operationally a distinct category from "slack work" or "plant closing," I investigate how the consequences of job loss vary by the stated reason. In particular, as some have argued, the labor market may be moving toward a new organization characterized by weaker ties between workers and firms. To the extent that job loss due to these changes is classified as position abolished, it is important to understand the extent to which postdisplacement reemployment probabilities or earnings are different for workers displaced because their position was abolished than for workers displaced because of a plant closing or slack work.[40] Remember that any attribution of job loss for reasons such as downsizing or restructuring is necessarily arbitrary.

Before turning to this analysis, I investigate displacement rates by reason across sex, age, education, occupation, and industry.

39. The data from the debriefing questions appended to the February 1996 DWS would be very useful in this regard.

40. The sort of adverse selection argument made by Gibbons and Katz (1991) also needs to be taken into account.

Figure 2. Rate of Job Loss by Sex and Reason

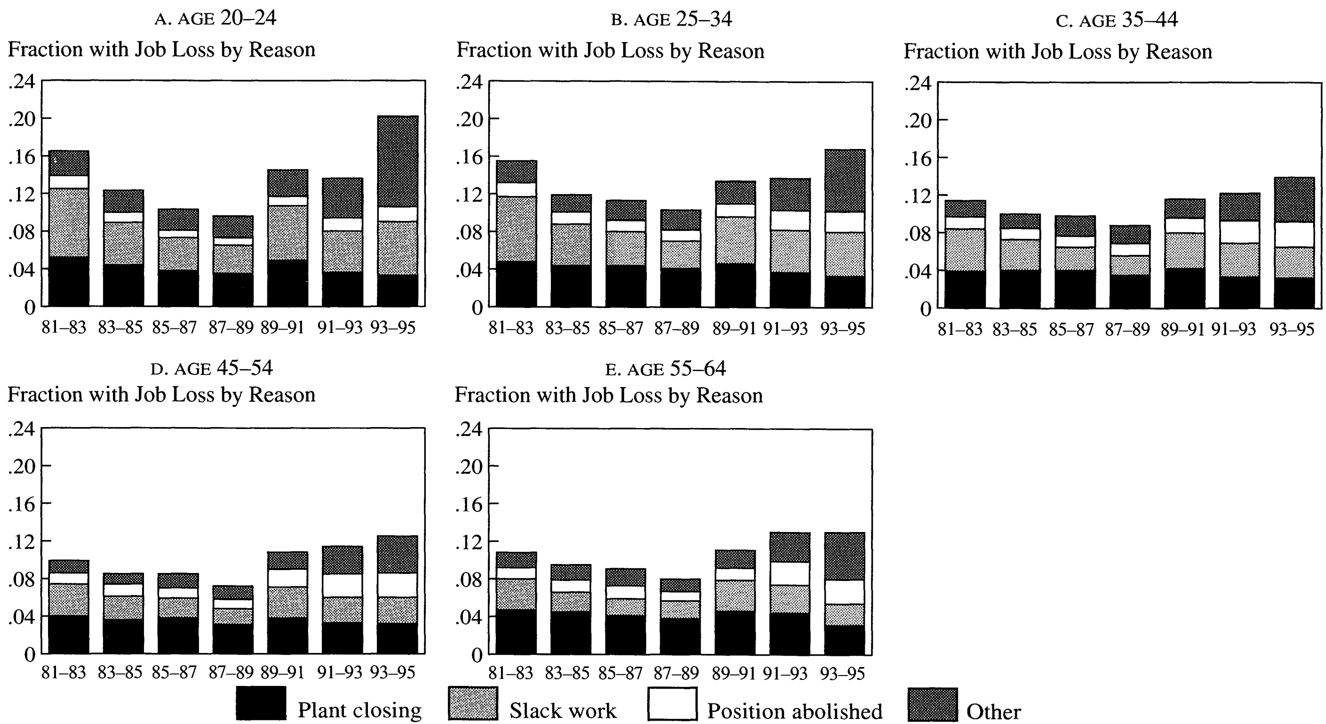


Source: Author's calculations.

Rates of Job Loss by Sex

Figure 2 contains stacked bar graphs of adjusted three-year job-loss rates by year and reason for males and females. Two facts are clear from this figure. First, women historically have had substantially lower rates of job loss than men, but the difference has declined in recent years. The male-female differential in job-loss rates was about 5 percentage points in 1981–83, and it fell to about 2 percentage points in

Figure 3. Rate of Job Loss by Age and Reason



Source: Author's calculations.

1993–95. A particularly large increase in job-loss rates for women was noted in the 1993–95 period. Between 1991–93 and 1993–95 the job-loss rate increased by about 1.5 percentage points for men and by about 3 percentage points for women. Second, the patterns of job loss over time and by reason are quite similar for men and women, despite the differences between the sexes in their distributions of employment by occupation and industry.

Rates of Job Loss by Age

Figure 3 contains stacked bar graphs of adjusted three-year job-loss rates by year and reason for five age groups.[41] Job-loss rates are highest for the youngest workers (20–24), and, until the most recent time period, these workers show the "classic" cyclical pattern to their job loss. The 1991–93 job-loss rates are lower than those in 1989–91. The older age groups show job-loss rates declining until 1987–89 and increasing thereafter. For the three groups covering workers 35 to 64 years old, job-loss rates are higher after 1989 than even in the deep recession of the early 1980s. Job-loss rates have risen substantially for workers in all age categories.

Regarding changes over time in the distribution of job loss by reason, the elevation in the rate of job loss in 1991–93 due to position abolished is substantial and largest in the older age categories. The increase in job loss for "other" reasons seems to have occurred across the age spectrum but is largest for workers in the youngest age categories. This suggests that the increase in "other" job loss is not due primarily to workers accepting buyouts and taking early retirement.

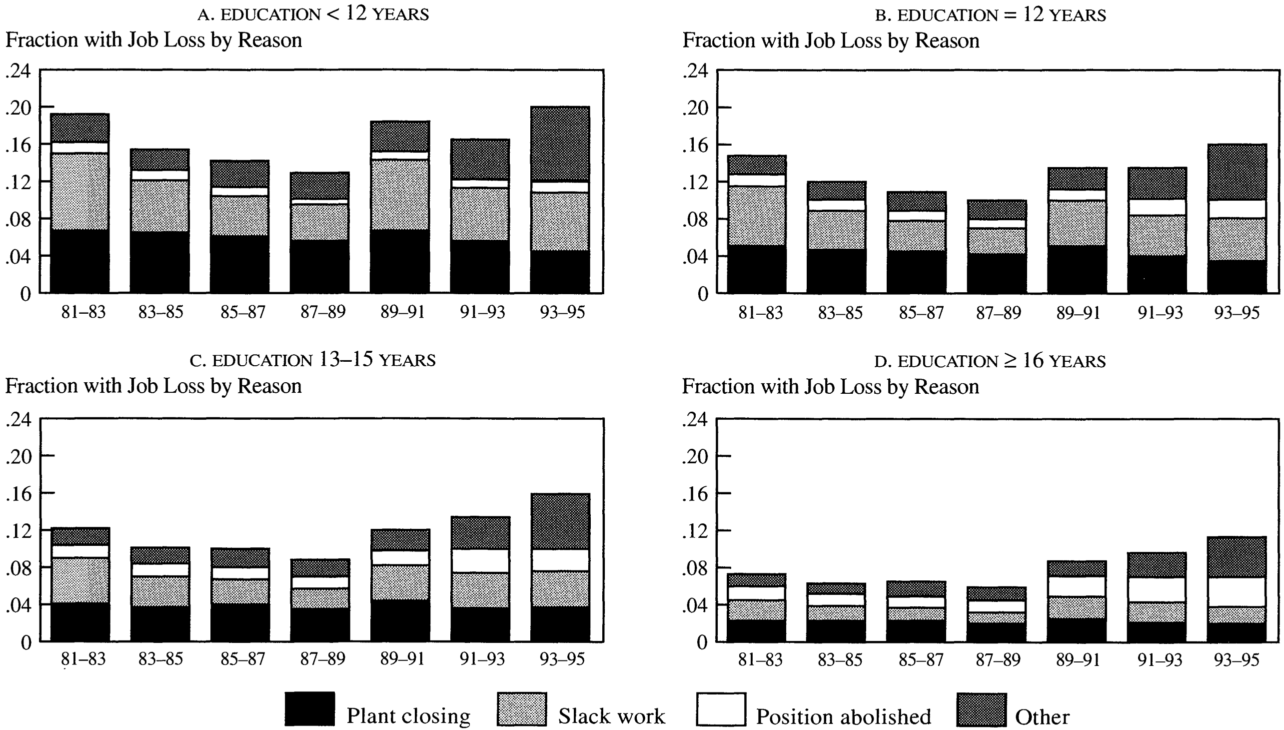
Rates of Job Loss by Education

Figure 4 contains stacked bar graphs of adjusted three-year job-loss rates by year and reason for four educational categories.[42] Not surprisingly, job-loss rates are dramatically higher for less educated workers

41. Although not presented here, I created separate sets of graphs for males and females. The patterns for both groups are basically similar, with the caveat, noted earlier, that job-loss rates are higher for men than for women.

42. Although not presented here, I created separate sets of graphs for males and females. The patterns for both groups are basically similar, with the caveat, noted earlier, that job-loss rates are higher for men than for women.

Figure 4. Rate of Job Loss by Education and Reason



Source: Author's calculations.

than for more educated workers. Job-loss rates grew over the period for workers in all educational categories. The increase was particularly large in proportional terms for workers in the higher educational categories. Rates of job loss for "other" reasons are up sharply in all educational categories.

One potentially very interesting contrast by educational category is that the increase in job loss due to position abolished has increased primarily and substantially among more educated workers but not among workers in the lower educational categories. This finding is consistent with reports of elimination of substantial numbers of white-collar jobs in some large organizations.

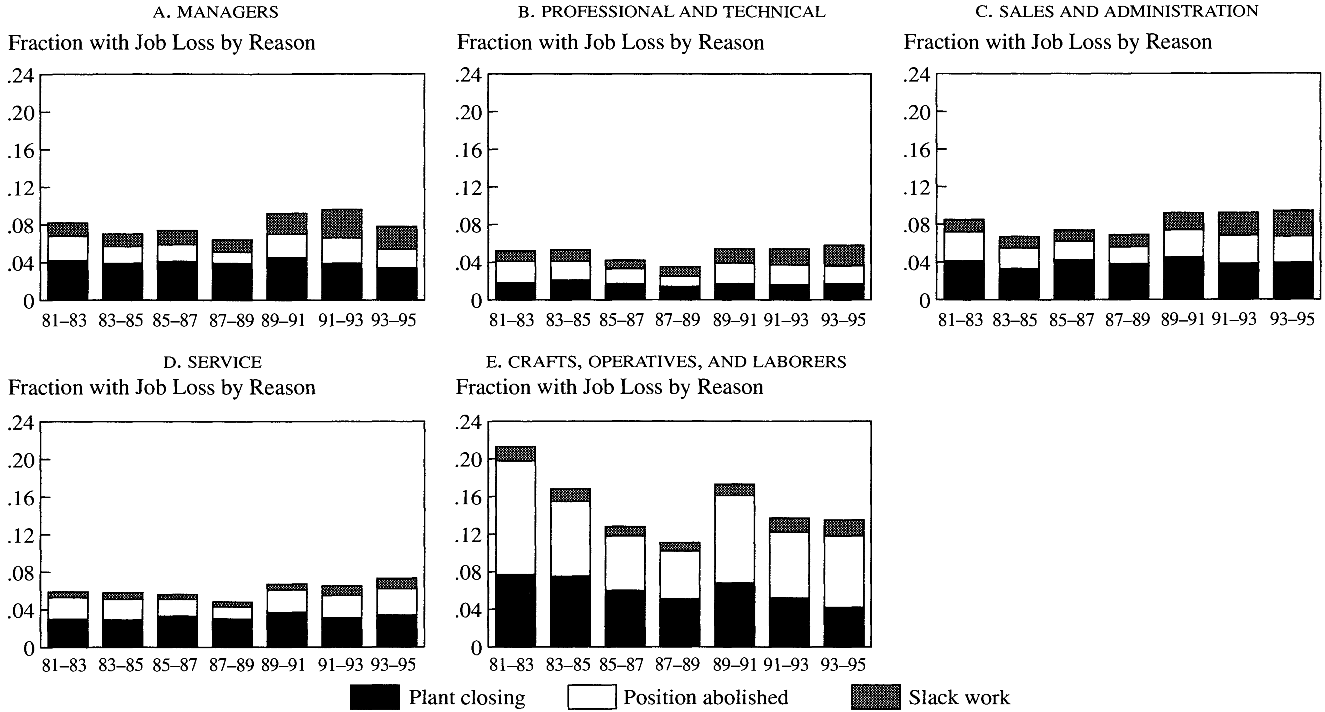
Rates of Job Loss by Occupation

Given the changes in the incidence of job loss by education, it is useful to examine rates of job loss by occupational category. Figure 5 contains stacked bar graphs of adjusted three-year job-loss rates by year and reason for five occupational categories. Only job-loss rates for displacement because of plant closing, slack work, and position/shift abolished can be examined because the 1994 and 1996 DWSs contain no information on occupation for workers displaced for "other" reasons. This is particularly unfortunate given the large increase in job-loss rates in this category.

Because I do not know the occupation of workers who were not displaced during the period at which they were at risk of being displaced, I assume that the occupational distribution of employment does not change sharply over a three-year period. Specifically, the job-loss rate for occupation j is computed as the ratio of the weighted count of workers who reported losing a job in occupational category j to the weighted count of workers working currently (that is, at the survey date) in occupational category j.

Figure 5 illustrates that job-loss rates are substantially higher for craftsmen, operatives, and laborers than for any of the other occupational categories. Job-loss rates for these workers are strongly cyclical and were significantly lower after 1991–95 than they were in 1989–91, contrary to the experience of workers in other occupations. Note also that the peak job-loss rate for these blue-collar workers in the 1981–83

Figure 5. Rate of Job Loss by Occupation and Reason



Source: Author’s calculations.

slack period was substantially higher than their peak job-loss rate in the 1989–91 slack period. The earlier recession was a blue-collar, manufacturing-based recession, whereas the later recession was concentrated more in other sectors.

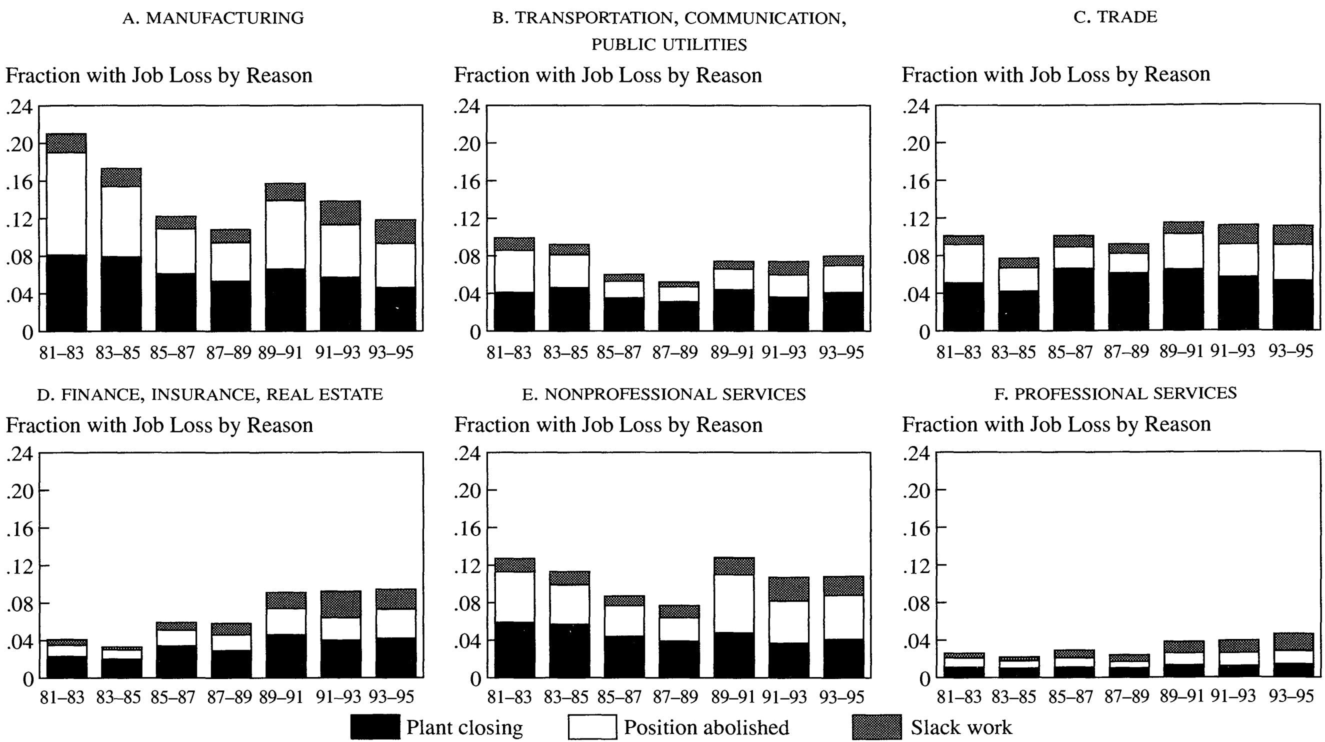
Job-loss rates for service workers and for workers in sales and administration remained relatively fixed over the sample period, although a slight cyclical pattern is evident and job loss has increased a bit since 1989. The most secure occupational category, professional and technical workers, also shows a slight elevation in job-loss rates, starting from a low base, with most of the increase coming from increased job loss due to position abolished.

The pattern of job-loss rates for managers is interesting. Rates of job loss for managers increase substantially from their 1987–89 level through 1991–93, and job loss due to position abolished accounts for all (and more) of this increase. This pattern is consistent with reports that corporations are reorganizing in ways that eliminate the jobs of significant numbers of managerial workers. But rates of job loss for managers decreased in the 1993–95 period, with the decrease accounted for by declines in job loss due to slack work and position abolished.

There is a potentially important message in this finding regarding managers. At least two interpretations of corporate restructuring have implications for job security. The first is that corporations are moving toward a mode of organization that relies less on long-term relationships with workers and, hence, less investment in workplace-specific skills. This trend would imply a permanent increase in rates of job loss. The second interpretation is that corporations are adjusting their mix of workers to reflect new production arrangements. For example, part of the managerial function might be performed more efficiently using modern information technology and fewer managers. This trend would imply a "onetime" adjustment in the number of managers, resulting in a temporary increase in rates of job loss for managers. The decline in job-loss rates for managers in the 1993–95 period is preliminary evidence consistent with this interpretation.

Again, an important caveat is that the "other" category is not considered here. Because the big increase in job loss in the 1991–95 period is in the "other" category, this omission could seriously affect the results as well as their interpretation.

Figure 6. Rate of Job Loss by Industry and Reason



Source: Author's calculations.

Rates of Job Loss by Industry

Figure 6 contains stacked bar graphs of adjusted three-year job-loss rates by year and reason for six industrial categories. Because I can only examine job-loss rates for displacement due to plant closing, slack work, and position/shift abolished, the results must be interpreted cautiously. As with occupation, I do not know the industry of workers who were not displaced during the period in which they were at risk of being displaced, and I compute the three-year job-loss rates assuming the industrial distribution of employment does not change sharply over a three-year period. The job-loss rate for industrial category j is computed as the ratio of the weighted count of workers who reported losing a job in industrial category j to the weighted count of workers currently working in industrial category j.

The secular increase found in overall job-loss rates is not apparent in all industrial categories. Specifically, job-loss rates in manufacturing show a strong cyclical pattern. The overall rate of job loss in manufacturing is much larger than in other categories and shows peaks in 1981–83 and 1989–91, with the former much higher than the latter. Once again, this finding reflects the blue-collar manufacturing locus of the 1981–83 recession. Three other categories—nonprofessional services and transportation, communication, and public utilities—show weaker evidence of cyclical behavior. Neither of these categories shows a decline in job-loss rates between 1991–93 and 1993–95. Trade (wholesale and retail) has a steady rate of job loss across the entire time period, aside from a dip in the 1983–85 period and a small increase after 1989.

Two exceptions to this pattern, one small and one large, are professional services and finance, insurance, and real estate. Professional services have *very* low rates of job loss that have increased little in absolute terms over time but are, nevertheless, proportionally much higher in 1993–95 than they were previously. Much of this growth is due to ''position abolished,'' but some is attributable to an increase in slack work as well. Finance, insurance, and real estate shows a much sharper change. Job-loss rates after 1989 are more than double their levels in 1981–83. Much of this increase is due to an increase in slack work and ''position abolished,'' but a substantial increase in job loss because of plant closings (a phenomenon not shared with any other industry) is also evident. This might reflect the substantial consolidations that have taken place in financial market sectors such as banking. The continuing elevated rates of job loss in finance, insurance, and real estate are consistent with a change in the dominant model of employment relationships in this sector to one with less emphasis on long-term employment relationships. Alternatively, the wave of consolidations in this area might not have subsided by 1995.

The Incidence of Job Loss: Multivariate Analysis

I carried out two parallel multivariate analyses of the incidence of job loss. The first analysis is a simple probit analysis of the probability of job loss as a function of age, education, sex, race, and year.[43] The second analysis consists of four separate probit analyses of the probability of job loss by stated reason: job loss due to (1) plant closing, (2) slack work, (3) position/shift abolished, and (4) other reason. Note that I am not estimating a multinomial choice model, such as multinomial logit or probit. What I am interested in here is data description and summary rather than estimates of some structural choice model. The ease of interpretation of the estimates from the binomial probit models makes them a preferred method for this purpose.

These analyses are carried out using a pooled sample of 425,816 workers from the 1984–96 DWSs. This sample consists of all workers in the DWSs who were employed at the survey date or who reported a job loss (whether employed at the survey date or not). In the probit model of the probability of job loss, the dependent variable indicates whether or not a job loss was reported within the three-year period prior to the survey year. In the reason-specific probit models, the dependent variable indicates whether or not job loss for the particular reason was reported within the three-year period prior to the survey year.

There is the problem, noted earlier, of the undercount of three-year job loss due to the five-year window used in the 1984–92 DWSs. The adjustment procedure outlined and presented formally in equation 1

43. It would also be interesting to include measures of industry and occupation, but, as discussed in the previous section, the DWS does not contain these measures (1) for nondisplaced workers during the at-risk period, and (2) for workers displaced for "other" reasons.

yields an adjusted job-loss rate for any well-defined group, but it is not directly useful in a disaggregated analysis. I implement a straightforward procedure, in the same spirit as the procedure used to adjust rates, to account for the understatement of job loss in the 1984–92 DWSs.

Recall that δ_4 is the fraction of those workers who lost a job in $t-4$ who report losing at least one more job in the $t-3$ to $t-1$ interval, and δ_5 is the fraction of those workers who lost a job in $t-5$ who report losing at least one more job in the $t-3$ to $t-1$. These conditional fractions were estimated to be 0.3017 and 0.2705 based on evidence from the PSID. Think of these fractions as probabilities of reported job loss in the relevant time period. Consider first the adjustment for repeat job loss among workers who lost a job four years ago. I create two observations for each individual in the 1984–92 DWSs who reports losing a job four years ago. I then assign one of the observations a weight of δ_4 times the CPS sampling weight. This weighted observation represents the expected number of repeat job losers among those who lost jobs four years ago. I assign the other observation a weight of $(1 - \delta_4)$ times the CPS sampling weight, and I redefine the job-loss variables as zero (no loss). This weighted observation represents the expected number of nonrepeat job losers among those who lost jobs four years ago. The analogous procedure is used for workers who lost a job five years ago. Two observations are also created for each of these workers, and they are reweighted appropriately using δ_5 rather than δ_4.[44]

Table 1 contains normalized estimates of the probit models of the probability of job loss overall and by specific reason pooling all seven survey years. The base group for the independent variables consists of 20- to 24-year-old white males with twelve years of education in the 1981–83 time period. The coefficients are normalized to represent the derivative of the probability of the outcome with respect to a change in the explanatory variable evaluated at the means of the explanatory variables. This is computed as $\hat{\beta}\phi(\bar{X}\hat{\beta})$ where $\hat{\beta}$ is the vector of estimated parameters of the probit model, $\bar{X}$ is the vector of means of the explanatory

44. This procedure increases the sample size from 425,816 to 436,002. Because the original sample size is unchanged, standard errors computed from the inflated sample should be inflated by the square root of the fractional increase in the sample. Because the sample increased by only 2.4 percent, I ignore this adjustment. It will be clear from the results that this is not a substantive problem.

Table 1. Probability of Job Loss by Reason, 1981–95, Normalized Probit Estimates

Variable	*Mean*	*Job loss*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
Constant	1.	−0.179	−0.121	−0.090	−0.079	−0.099
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age 25–34	0.306	−0.004	0.002	−0.001	0.003	−0.005
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age 35–44	0.277	−0.026	−0.003	−0.012	0.003	−0.010
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age 45–54	0.187	−0.042	−0.007	−0.019	0.003	−0.015
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age 55–64	0.109	−0.046	−0.006	−0.025	0.002	−0.014
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Ed $<$ 12	0.122	0.025	0.009	0.009	−0.004	0.007
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ed 13–15	0.248	−0.010	−0.004	−0.007	0.003	−0.001
		(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Ed $\geq$ 16	0.250	−0.047	−0.022	−0.024	0.004	−0.006
		(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Female	0.455	−0.019	−0.002	−0.016	0.000	−0.000
		(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Nonwhite	0.138	0.008	0.000	0.005	−0.002	0.004
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1983–85	0.134	−0.017	−0.001	−0.010	−0.001	−0.002
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1985–87	0.139	−0.022	−0.002	−0.017	−0.002	0.002
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1987–89	0.144	−0.031	−0.005	−0.021	−0.003	0.001
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1989–91	0.147	0.001	0.001	−0.004	0.001	0.004
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1991–93	0.152	0.003	−0.005	−0.008	0.006	0.012
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1993–95	0.156	0.023	−0.008	−0.007	0.007	0.025
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
$\overline{P}$		0.103	0.034	0.032	0.014	0.023
log L		−141879.1	−63281.5	−59759.8	−31224.3	−46907.7

Source: Author's calculations.

Note: The coefficients are normalized to represent the derviative of the probability of the outcome with respect to a change in the explanatory variable. This is computed as $\beta\phi(\overline{X}\beta)$, where β is the vector of estimated parameters of the probit model, $\overline{X}$ is the vector of means of the explanatory variables, and ϕ is the standard normal probability density function. The value of $\overline{X}$ used is contained in the first column of the table. The dependent variable in the job-loss column indicates loss for any reason. The dependent variables in the remaining columns indicate loss only for the specified reason. The quantity $\overline{P}$ is the average loss rate for the indicated reason. The data prior to the 1994 DWS are adjusted as described in the text to account for the change in recall period from five years to three years. All analyses are weighted by the adjusted CPS sampling weights. The base categories of the independent variables are 20- to 24-year-old white males with twelve years of education in the 1981–83 time period. Asymptotic standard errors are in parentheses. n = 425,816.

variables, and ϕ is the standard normal probability density function. The value of $\bar{X}$ used is contained in the first column of the table.

The estimates of the probability of job loss show, not surprisingly, that older workers and more educated workers are less likely to be displaced. These differences are substantial, with college-educated workers having a displacement rate 4.7 percentage points lower than workers with a high school education. The estimates also suggest that females are about 2 percentage points less likely to be displaced than males.

Regarding movements over time, the overall job-loss rates show a cyclical pattern through 1989, with substantially lower job-loss rates in the tight labor market of the mid- to late 1980s, bracketed by higher job-loss rates in the slack labor market of 1981–83 and 1989–91. The striking finding is that the rate of job loss in the most recent period (1993–95) is 2.3 percentage points higher than in the slack labor market of 1981–83. This was a period of sustained economic expansion with a declining unemployment rate, and one might have expected job-loss rates to fall. Instead, the job-loss rate in 1993–95 is the highest seen over the seven intervals covered, dating back to 1981–83.[45]

These movements in job-loss rates over time are adjusted for demographic characteristics using the probit model. An interesting question is how much of the movements apparent in the figures is accounted for by changes in demographic characteristics. One approach to this is to compare the estimated year effects from the probit models in table 1 that are adjusted for demographic characteristics with the estimated year effects from probit models that include only the survey-year dummy variables (so they are unadjusted for demographic characteristics). These unadjusted estimates (base year = 1981–83) are contained in table 2. Estimates are presented of probit models of the probability of job loss overall and by specific reason pooling all seven survey years. The coefficients are normalized to represent the derivative of the probability of the outcome with respect to a change in the explanatory variable evaluated at the means of the explanatory variables, as described earlier. The values of $\bar{X}$ are presented in the table. These unadjusted (for worker characteristics) differences in the probability of

45. Precise comparisons should be made cautiously given the admittedly approximate adjustment used to make the three-year loss rates from the 1984–92 DWSs comparable with the three-year loss rate from the 1994 and 1996 DWSs.

Table 2. Probability of Job Loss by Reason, 1981–95: Unadjusted Year Effects, Normalized Probit Estimates

Variable	*Mean*	*Job loss*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
1983–85	0.134	−0.018	−0.001	−0.012	−0.001	−0.002
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1985–87	0.139	−0.025	−0.003	−0.019	−0.002	0.001
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1987–89	0.144	−0.035	−0.006	−0.024	−0.002	0.000
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1989–91	0.147	−0.005	−0.000	−0.007	0.002	0.003
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1991–93	0.152	−0.004	−0.008	−0.012	0.007	0.011
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1993–95	0.156	0.014	−0.011	−0.012	0.008	0.024
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
$\bar{P}$		0.103	0.034	0.032	0.014	0.023
log L		−144054.0	−64032.4	−61788.5	−31358.5	−47386.9

Source: Author's calculations.

Note: The estimates are normalized coefficients from probit models of the probability of the indicated outcome that include only a constant and survey-year dummy variables (base year = 1981–83). The coefficients are normalized to represent the derviative of the probability of the outcome with respect to a change in the explanatory variable, computed as described in the note to table 1. All analyses are weighted by the adjusted CPS sampling weights. Asymptotic standard errors are in parentheses. n = 425,816.

the particular outcome (type of job loss) approximately reproduce the differences in job-loss rates implicit in figure 1.[46]

A comparison of the year effects in the models of the overall probability of job loss in tables 1 and 2 shows that changes in the demographic characteristics cannot account for much of the unadjusted differences over time. In fact, the only significant difference is that the adjusted increase in the rate of job loss between 1981–83 and 1993–95 is *larger* than the unadjusted estimate (2.3 percentage points adjusted compared with 1.4 percentage points unadjusted).

The probit estimates of the probability of job loss by specific reason in table 1 show important contrasts, most of which confirm the univariate results apparent in the figures. Older workers are relatively unlikely to lose jobs because of slack work or "other" reasons, but no systematic age pattern is apparent in the probability of job loss due to plant closing or to position/shift abolished. Workers with at least a college education are substantially less likely to lose jobs due to plant closing

46. The actual differences in the probabilities can be computed from the appendix tables.

or slack work and slightly more likely to lose jobs due to having their position abolished. The results confirm that females are less likely to lose jobs, largely due to a lower probability of losing jobs due to slack work. Once other demographics are controlled for, nonwhites are slightly more likely to lose jobs, mostly due to slack work and for "other" reasons.

In examining the survey-year variables, the cyclical nature of job loss due to slack work is quite clear from the estimates. Job loss due to slack work was over 2 percentage points lower in the strong labor market of the mid- to late 1980s than it was in the weak labor market of 1981–83 and over 1 percentage point lower than after 1989. In contrast, job loss due to plant closings shows only mild variation across time. Perhaps the most interesting finding is that job loss due to position/shift abolished began to grow after 1987 and is significantly more common (about 0.7 percentage points) in the 1991–93 and 1993–95 time periods than earlier. The most striking result, however, is that job loss for "other" reasons has been increasing secularly since the 1985–87 time period, and the increase was largest between 1989–91 and 1991–93 and between 1991–93 and 1993–95. The probability of job loss for "other" reasons was fully 2.7 percentage points higher in 1993–95 than it was in 1983–85. This accounts for virtually all of the increase in the rate of overall job loss over this period.

A comparison of the adjusted year effects in table 1 with the unadjusted year effects in table 2 for the probit models of job loss by specific cause clearly shows that changes in demographic characteristics account for little of the movement over time in the probability of job loss for specific reasons. In particular, none of the recent increase in job loss due to position/shift abolished or "other" can be accounted for by changes in the demographic composition of the work force.

In order to examine how the incidence of job loss has varied with education, both over time and with other demographic characteristics, I carried out separate probit analyses by educational category. Tables 3 and 4 present estimates for individuals in two educational categories, twelve years of education and sixteen or more years of education, that serve to focus the discussion on workers with high levels of education and workers with less education. The "standard" cyclical pattern in rates of job loss is clearly much stronger for the less educated workers than for more educated workers. That rates of job loss were higher than expected on

Table 3. Probability of Job Loss by Reason, 1981–95: Individuals with Twelve Years of Education, Normalized Probit Estimates

Variable	*Mean*	*Job loss*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
Constant	1.	−0.177	−0.138	−0.100	−0.070	−0.099
		(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
Age 25–34	0.310	−0.012	0.000	−0.005	0.001	−0.006
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age 35–44	0.261	−0.039	−0.003	−0.019	−0.000	−0.013
		(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Age 45–54	0.185	−0.062	−0.009	−0.031	−0.000	−0.018
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Age 55–64	0.113	−0.060	−0.007	−0.036	−0.000	−0.015
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Female	0.479	−0.022	−0.002	−0.020	0.001	−0.001
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Nonwhite	0.138	0.011	0.001	0.006	−0.001	0.004
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1983–85	0.144	−0.018	−0.001	−0.013	−0.001	−0.001
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
1985–87	0.148	−0.028	−0.004	−0.021	−0.002	0.001
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
1987–89	0.152	−0.037	−0.007	−0.027	−0.002	0.001
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
1989–91	0.143	−0.004	0.001	−0.007	−0.000	0.004
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
1991–93	0.137	−0.007	−0.009	−0.013	0.004	0.011
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
1993–95	0.137	0.014	−0.013	−0.010	0.005	0.025
		(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
$\bar{P}$		0.112	0.038	0.038	0.012	0.024
log L		−58560.4	−27454.4	−26559.4	−10875.5	−18361.3

Source: Author's calculations.

Note: The coefficients are normalized to represent the derviative of the probability of the outcome with respect to a change in the explanatory variable, computed as described in the note to table 1. All analyses are weighted by the adjusted CPS sampling weights. The base categories of the independent variables are 20- to 24-year-old white males in the 1981–83 time period. Asymptotic standard errors are in parentheses. n = 164,885.

cyclical grounds in the most recent period is due largely to the substantial increase in rates of job loss for "other" reasons in both educational groups.[47] This increase began in earnest in the 1991–93 period for all groups, but it was offset in the lower educational categories by the cyclical decline in job loss due to slack work.

The increase in job loss due to position abolished is much stronger for

47. This is also true of the educational groups not shown.

Table 4. Probability of Job Loss by Reason, 1981–95: Individuals with Sixteen or More Years of Education, Normalized Probit Estimates

Variable	*Mean*	*Job loss*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
Constant	1.	−0.204	−0.093	−0.077	−0.097	−0.089
		(0.004)	(0.002)	(0.002)	(0.003)	(0.002)
Age 25–34	0.326	0.017	0.005	0.005	0.010	−0.001
		(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Age 35–44	0.324	0.007	0.001	0.001	0.011	−0.003
		(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Age 45–54	0.198	−0.004	0.000	−0.004	0.009	−0.006
		(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Age 55–64	0.093	−0.003	−0.002	−0.002	0.009	−0.005
		(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Female	0.425	−0.011	−0.003	−0.008	−0.001	0.001
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Nonwhite	0.113	−0.006	−0.001	0.002	−0.005	−0.002
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
1983–85	0.124	−0.008	0.000	−0.004	−0.002	−0.002
		(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
1985–87	0.135	−0.008	−0.000	−0.006	−0.004	0.003
		(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
1987–89	0.144	−0.012	−0.002	−0.007	−0.002	0.001
		(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
1989–91	0.149	0.014	0.002	0.003	0.006	0.004
		(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
1991–93	0.159	0.020	−0.001	0.001	0.009	0.011
		(0.003)	(0.002)	(0.001)	(0.001)	(0.002)
1993–95	0.173	0.033	−0.002	−0.002	0.012	0.020
		(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
$\bar{P}$		0.069	0.019	0.016	0.017	0.018
log L		−26994.5	−9982.9	−8668.7	−9215.6	−9448.3

Source: Author's calculations.

Note: The coefficients are normalized to represent the derviative of the probability of the outcome with respect to a change in the explanatory variable, computed as described in the note to table 1. All analyses are weighted by the adjusted CPS sampling weights. The base categories of the independent variables are 20- to 24-year-old white males in the 1981–83 time period. Asymptotic standard errors are in parentheses. n = 106,690.

the more highly educated workers. For workers with twelve years of education, job loss due to position/shift abolished was 0.4 percentage points higher in 1993–95 than in 1981–83, whereas for college graduates job loss due to position/shift abolished increased 1.2 percentage points over the same period.[48] Thus the substantial run-up in job-loss rates for

48. In fact, there was virtually no increase in job loss for this reason for workers with fewer than twelve years of education.

college graduates was due to a combination of increases in job loss due to position/shift abolished and "other" reasons.

These results raise the interesting question of whether high school graduates and college graduates simply give different names to the same experience. Perhaps high school graduates are more likely to call their job loss slack work and college graduates are more likely to call their job loss position/shift abolished. Even if this were the case, however, the 3.3 percent estimated increase in the rate of overall job loss between 1981–83 and 1993–95 for college graduates is significantly larger than the 1.4 percent estimated increase for high school graduates over the same period.

I turn now to an analysis of the consequences of job loss and consider three dimensions of labor-market experience subsequent to job loss that affect income. First, it can be difficult for individuals to find new jobs. Second, where a new job is found, it may have reduced hours relative to the lost job. To the extent the new job is part time, it is likely to pay a lower hourly rate as well as yield less total income. Third, even controlling for hours, the new job may not pay as much as the lost job paid or would pay currently had the worker not been displaced. I pay particular attention to differences in the consequences of job loss by reported reason. If job loss due to position/shift abolished is, in fact, a different phenomenon than job loss for other reasons, this could show up as differences in postdisplacement employment and earnings experience.[49]

Postdisplacement Employment Rates

In this section, I examine how the probability of survey-date employment of workers varies with the reported reason for displacement controlling for other factors including sex, race, age, education, survey year, and the number of years between the job loss and the survey date. Table 5 contains survey-date employment probabilities for displaced workers broken down by year and reason for displacement. In every period, workers displaced due to slack work have lower employment probabilities than do workers displaced due to a plant closing or posi-

49. The substantial numbers of workers at all education levels who report job loss for each reason in each year allows the identification of "reported reason" effects from the effects of other observables such as time and education.

Table 5. Fraction of Displaced Workers Employed at Survey Date, by Year and Reason

Year	*Plant closing*	*Slack work*	*Position abolished*	*Other*	*All*
1981–83	0.630	0.537	0.669	0.537	0.580
	[2,076]	[2,507]	[643]	[1,160]	[6,386]
1983–85	0.687	0.575	0.669	0.571	0.626
	[1,904]	[1,658]	[595]	[1,002]	[5,159]
1985–87	0.734	0.604	0.699	0.627	0.670
	[1,865]	[1,382]	[567]	[1,084]	[4,898]
1987–89	0.744	0.646	0.716	0.633	0.689
	[1,685]	[1,123]	[519]	[1,078]	[4,405]
1989–91	0.684	0.513	0.639	0.577	0.598
	[2,059]	[2,095]	[733]	[1,291]	[6,178]
1991–93	0.685	0.623	0.723	0.621	0.657
	[1,783]	[1,861]	[1,119]	[1,680]	[6,443]
1993–95	0.730	0.677	0.747	0.701	0.709
	[1,359]	[1,526]	[1,003]	[2,331]	[6,219]
All years	0.698	0.591	0.703	0.628	0.649
	[12,731]	[12,152]	[5,179]	[9,626]	[39,688]

Source: Author's calculations.
Note: Weighted by CPS sampling weights. The numbers in brackets are sample sizes.

tion/shift abolished.[50] Workers displaced for "other" reasons also have relatively low rates of employment. No systematic difference in employment probabilities between those displaced due to plant closings and those displaced due to position/shift abolished exists. Strong evidence indicates cyclical sensitivity of employment probabilities, with workers displaced during the slack periods (1981–83, 1989–91) having lower employment probabilities than workers in other periods. There is no evidence in this simple breakdown that employment probabilities have declined secularly for workers displaced for any reason. In fact, employment probabilities for displaced workers are at their historical (at least since 1981) high in the 1993–95 period. I turn now to a multivariate analysis of survey-date employment.

Table 6 contains probit analyses of the probability of employment at the survey date. Before discussing these results, it is necessary to discuss the solution to an estimation problem caused by the survey change introduced with the 1994 DWS that resulted in a lack of follow-up information for workers who reported job loss for "other" reasons.

50. This is consistent with Gibbons and Katz's (1991) model of adverse selection.

Table 6. Probability of Employment at Survey Date, 1981–95, Normalized Probit Estimates

Variable	*Pooled*	*Pooled*	*Ed < 12*	*Ed = 12*	*Ed 13–15*	*Ed ≥ 16*
Constant	0.188	0.056	−0.059	0.081	0.084	0.207
	(0.010)	(0.010)	(0.025)	(0.015)	(0.020)	(0.029)
Ed < 12	−0.106	−0.106	...	...	...	...
	(0.007)	(0.007)				
Ed 13–15	0.072	0.069	...	...	...	...
	(0.006)	(0.006)				
Ed ≥ 16	0.160	0.157	...	...	...	...
	(0.008)	(0.008)				
1983–85	0.036	0.051	0.041	0.059	0.042	0.052
	(0.010)	(0.010)	(0.023)	(0.014)	(0.021)	(0.022)
1985–87	0.074	0.082	0.121	0.072	0.082	0.059
	(0.010)	(0.010)	(0.023)	(0.015)	(0.021)	(0.022)
1987–89	0.093	0.109	0.136	0.105	0.130	0.065
	(0.010)	(0.010)	(0.025)	(0.015)	(0.022)	(0.022)
1989–91	−0.005	0.016	−0.006	0.015	0.026	0.019
	(0.009)	(0.009)	(0.023)	(0.014)	(0.018)	(0.019)
1991–93	0.044	0.061	0.053	0.073	0.060	0.038
	(0.009)	(0.009)	(0.024)	(0.014)	(0.018)	(0.020)
1993–95	0.102	0.127	0.154	0.154	0.106	0.077
	(0.009)	(0.009)	(0.025)	(0.015)	(0.018)	(0.020)
Slack work	−0.118	−0.089	−0.069	−0.102	−0.099	−0.049
	(0.006)	(0.006)	(0.016)	(0.010)	(0.012)	(0.014)
Position abolished	−0.044	−0.023	−0.018	−0.026	−0.031	−0.001
	(0.008)	(0.008)	(0.029)	(0.014)	(0.015)	(0.015)
Other reason	−0.113	−0.084	−0.085	−0.082	−0.096	−0.053
	(0.008)	(0.008)	(0.021)	(0.013)	(0.016)	(0.017)

(*continued*)

Although most variables are available from the basic CPS questions, no information is available on the particular year of job loss for workers in the 1994 and 1996 DWSs who report job loss for "other" reasons. This variable is used to compute a measure of time since displacement, which turns out to be an important predictor of the probability of survey-date employment.

Assuming no data problem, I would estimate the probit model of the probability of survey-date employment including the "usual" set of variables (dummy variables for education category, age category, sex, and race) as well as dummy variables for the reported reason for job

Table 6 (*continued*)

Variable	*Pooled*	*Pooled*	*Ed* < *12*	*Ed* = *12*	*Ed 13–15*	*Ed* ≥ *16*
Other reason	0.030	0.126	0.117	0.096	0.145	0.130
(1994–96 DWS)	(0.012)	(0.012)	(0.033)	(0.019)	(0.022)	(0.023)
Lost job 2	. . .	0.189	0.179	0.191	0.192	0.160
years ago		(0.006)	(0.016)	(0.010)	(0.012)	(0.013)
Lost job 3	. . .	0.243	0.255	0.259	0.223	0.187
years ago		(0.007)	(0.017)	(0.010)	(0.013)	(0.013)
Age 25–34	0.036	0.024	0.025	0.004	0.054	−0.033
	(0.007)	(0.008)	(0.020)	(0.011)	(0.013)	(0.025)
Age 35–44	0.022	0.008	0.043	−0.010	0.035	−0.074
	(0.008)	(0.008)	(0.021)	(0.012)	(0.014)	(0.025)
Age 45–54	−0.020	−0.035	−0.007	−0.053	0.001	−0.111
	(0.009)	(0.009)	(0.022)	(0.014)	(0.016)	(0.026)
Age 55–64	−0.164	−0.187	−0.167	−0.206	−0.162	−0.227
	(0.010)	(0.010)	(0.024)	(0.016)	(0.021)	(0.028)
Female	−0.078	−0.082	−0.140	−0.088	−0.056	−0.053
	(0.005)	(0.005)	(0.014)	(0.008)	(0.009)	(0.010)
Nonwhite	−0.129	−0.130	−0.130	−0.166	−0.114	−0.037
	(0.007)	(0.007)	(0.017)	(0.011)	(0.012)	(0.016)
Number	39,632	39,632	6,330	16,600	10,001	6,701
log L	−24146.5	−23301.1	−4065.4	−10203.4	−5703.0	−3277.8

Source: Author's calculations.

Note: The coefficients are normalized to represent the derviative of the probability of the outcome with respect to a change in the explanatory variable. This is computed as $\hat{\beta}\phi(\bar{X}\hat{\beta})$ where $\hat{\beta}$ is the vector of estimated parameters of the probit model, $\bar{X}$ is the vector of means of the explanatory variables, and ϕ is the standard normal probability density function. The dependent variable equals 1 if the individual is employed at the DWS survey date and equals 0 otherwise. All analyses are weighted by the adjusted CPS sampling weights. The base categories of the independent variables are 20 to 24-year-old white males with twelve years of education who lost a job in the 1981–83 period in the year before the survey date due to a plant closing. The specific year of job loss is missing for all who report job loss in the 1994 and 1996 DWSs due to "other" (4,011 observations). The variable "other (1994–96 DWS)" is an indicator variable for these 4,011 observations, and the missing variables are set to zero for these observations. Asymptotic standard errors are in parentheses.

loss (omitted category of plant closing) and dummy variables for job loss two and three years prior to the survey year (omitted category of job loss in the year prior to the survey). Because job loss for "other" reasons is a significant category, however, I need some method to carry out the analysis including the "other" observations in 1994 and 1996. So I set the time-since-job-loss variables to zero for the "other" losers in 1994 and 1996 and include an additional indicator variable for "other" losers in these years. The coefficient of this indicator variable accounts for the "average" effect of time since job loss for the "other" losers in 1994 and 1996, and allows the effect of time-since-job-loss on the probability of employment to be determined by the observations for which the time since job loss is available.

In order to provide a baseline, the first column of table 6 contains normalized probit estimates of a model without including the time-since-job-loss variables but including the 1994–96 "other" indicator in order to determine the extent to which employment probabilities are different for these workers.[51] Without discussing the specifics, the results suggest that, controlling for survey year and the usual demographic characteristics, workers displaced for "other" reasons as reported in the 1984–92 DWSs are 11.3 percentage points less likely to be employed at the survey date than workers who were displaced due to a plant closing. The estimates of the coefficients of the survey-year dummy variables show that reemployment probabilities have been growing since the late 1980s (by 3.9 percentage points between 1989–91 and 1991–93 and by another 5.8 percentage points between 1991–93 and 1993–95). The estimated coefficient on the 1994–96 "other" indicator implies that the employment rate grew by an additional 3 percentage points higher for workers displaced for "other" reasons in 1991–96 than for the remaining displaced workers.

The second column of table 6 contains normalized probit estimates of the model including the time-since-displacement variables, and these variables have a strong positive effect on the probability of employment at the survey date. Workers displaced two and three years prior to the survey data are about 19 and 24 percentage points more likely to be employed at the survey date, respectively, than workers displaced in the year prior to the survey date. This most likely reflects time spent unemployed or searching for a new job or both. The estimated coefficient of 12.6 percentage points on the 1994–96 "other" variable reflects a combination of the approximately 3 percent differential in reemployment rates for this group and the average effect of the missing time since displacement variables (recorded as zero for these observations). I focus my discussion of employment rates on the model that includes the time-since-displacement variables.

These estimates show a strong cyclical component in reemployment

51. The normalization of the probit estimates is the same as that used in the earlier probit analysis of the probability of job loss. It is described earlier and in the footnote to table 6.

probabilities, with the lowest employment rates in the slack labor markets of 1981–83 and 1989–91. Reemployment rates rose through the 1980s expansion, fell in 1989–91, and then increased in 1991–93 and 1993–95.

Regarding the demographic variables, workers in the oldest age group (55–64 years of age) have a substantially lower probability of postdisplacement employment (a differential of about 19 percentage points relative to younger workers). This could reflect a move toward early retirement by these workers or greater difficulty for older workers in finding a new job. Simple tabulation of the CPS labor force status variable provides evidence consistent with the retirement explanation: The fraction of displaced workers who report being unemployed at the survey date does not vary substantially with age, whereas the fraction who report being out of the labor force is dramatically higher in the oldest age category (23.9 percent for displaced workers ages 55 to 64 compared with less than 10 percent for younger workers).

A similar finding is noted with regard to sex differences in postdisplacement employment rates. Females are estimated to have about an 8 percentage point lower employment rate than males. This seems to be due to lower labor force participation rates after displacement for women than for men. The fraction of displaced workers who are not in the labor force at the survey date is about 13.9 percent for women and only about 7.5 percent for men. At the same time, there is virtually no difference by sex in the fraction of displaced workers who are unemployed at the survey date. This may reflect time-use options other than work that are available to women when they lose their jobs involuntarily.

The opposite conclusion can be drawn with regard to differences by race in postdisplacement employment rates. Nonwhites are about 13 percentage points less likely than whites to be employed at the survey date following job loss. Nonwhite job losers are substantially more likely to be unemployed than whites (34.5 percent compared with 23.2 percent) but only moderately more likely than whites to be out of the labor force (12.0 percent compared with 9.8 percent).

An important result is that workers with more education have substantially higher postdisplacement employment rates. The college/high school differential is estimated to be 16 percentage points. Workers with less than twelve years of education are about 10 percentage points less likely than high school graduates to be employed at the survey date.

Given the increased job-loss rates for more educated workers already documented, it is worth examining employment probabilities separately by educational category. The last four columns of table 6 contain estimates of probit models of the probability of employment by educational category. Note several contrasts. First, the cyclicality of job loss is much greater in the lower educational categories. Second, the race and sex differences are also greater in the lower educational categories. Third, the age patterns are roughly similar across educational categories.[52]

Finally, consider the effect of the reported reason for job loss. Consistent with the simple tabulations, workers displaced due to slack work and workers displaced for "other" reasons are significantly less likely than workers displaced for the remaining reasons to be employed at the survey date. The pattern of these differences persists but is muted among workers with more education.

This analysis was partially motivated by the question of whether displacement due to position/shift abolished is a distinct phenomenon compared with displacement for other reasons. The results do not show any clear difference. Workers displaced due to position/shift abolished have postdisplacement employment probabilities that are intermediate between those displaced due to slack work or "other" reasons (the lowest) and those displaced due to a plant closing (the highest). This ordering exists in all educational categories, although the difference between workers in the position/shift abolished category and workers in the plant-closing category is nil for college graduates. No evidence indicates that college graduates who have lost jobs due to position/shift abolished have fared worse in the dimension of postdisplacement employment probabilities than have other job losers.

Postdisplacement Full-Time/Part-Time Status

Part-time workers typically have substantially lower wage rates than full-time workers. The DWSs collect information on part-time status (fewer than 35 hours per week) on the lost job, and it is straightforward

52. Although the coefficients on the age dummies are consistently larger in absolute value for college graduates than for those with less education, this reflects primarily the use of 20- to 24-year-olds as the base category rather than any structural difference.

to compute part-time status on postdisplacement jobs from the standard CPS hours information. The top panel of table 7 contains a breakdown of the fraction of workers reporting being displaced from a part-time job by year and reason for displacement. The bottom panel of the table contains a similar breakdown for the fraction of employed workers reporting working part time at the survey date. Note that there is the problem of temporal comparability of the data on part-time employment at the survey date. The new survey instrument, first used in the 1994 CPS, asks a different battery of questions about hours of work on the current job, which may have the effect of raising the fraction of workers reporting they are currently working part time.[53]

The striking fact is that the part-time rate on the current job is about 6 percentage points higher on average than the part-time rate on the lost jobs. Some of this might be the result of individual labor supply decisions. Currently employed workers who were displaced from part-time jobs have a much higher probability of working part time at the survey date than workers who were displaced from full-time jobs (43.8 percent compared with 12.8 percent). It is difficult to pick out patterns over time or by the stated reason for job loss from these tabulations. It does appear that workers displaced for "other" reasons are more likely to be working part time both on the job they lost and after displacement, however.

Following the outline of the earlier analysis, table 8 contains normalized estimates of probit models of the probability of part-time employment among workers employed at the survey date. The first two columns of the table contain estimates for all reemployed displaced workers. The first column does not include an indicator variable for part-time status on the old job because it was not available for workers displaced for "other" reasons in the 1994 and 1996 DWSs. It does include an indicator for 1994–96 "other" job loss, and its coefficient is not significantly different from zero. The second column contains the preferred specification with the indicator for part time on the old job (set to zero for the missing observations for "other" job loss in the 1994 and 1996 DWSs) along with the 1994–96 "other" job-loss indicator. Not surprisingly, the strongest effect on the probability of

53. Polivka and Miller (1994). The survey question regarding whether the lost job was part time is unchanged in the 1994 and 1996 DWSs.

Table 7. Full- and Part-Time Status, by Year and Reason

Year	*Plant closing*	*Slack work*	*Position abolished*	*Other*	*All*
Fraction Part Time on Lost Job					
1981–83	0.129	0.110	0.104	0.152	0.117
	[2,072]	[2,503]	[643]	[1,158]	[6,376]
1983–85	0.116	0.093	0.113	0.148	0.114
	[1,904]	[1,656]	[595]	[999]	[4,155]
1985–87	0.115	0.099	0.118	0.140	0.110
	[1,865]	[1,382]	[567]	[1,084]	[3,814]
1987–89	0.113	0.077	0.111	0.132	0.100
	[1,681]	[1,121]	[518]	[1,075]	[3,320]
1989–91	0.115	0.099	0.092	0.139	0.111
	[2,054]	[2,092]	[732]	[1,286]	[4,878]
1991–93	0.110	0.125	0.113	...	0.116
	[1,723]	[1,810]	[1,109]	[4,642]	
1993–95	0.122	0.143	0.108	...	0.127
	[1,290]	[1,462]	[990]	[3,742]	
All years	0.117	0.109	0.108	0.142	0.117
	[12,589]	[12,026]	[5,154]	[5,602]	[35,371]
Fraction Part Time at Survey Date					
1981–83	0.176	0.166	0.183	0.205	0.178
	[1,319]	[1,375]	[421]	[605]	[3,720]
1983–85	0.155	0.151	0.176	0.168	0.158
	[1,302]	[951]	[395]	[568]	[3,216]
1985–87	0.132	0.118	0.127	0.144	0.130
	[1,366]	[833]	[396]	[680]	[3,275]
1987–89	0.146	0.120	0.118	0.151	0.137
	[1,243]	[721]	[366]	[680]	[3,010]
1989–91	0.156	0.173	0.149	0.197	0.168
	[1,374]	[1,055]	[463]	[735]	[3,627]
1991–93	0.162	0.173	0.196	0.228	0.187
	[1,223]	[1,157]	[816]	[1,044]	[4,240]
1993–95	0.176	0.178	0.156	0.198	0.181
	[990]	[1,037]	[748]	[1,636]	[4,411]
All years	0.157	0.159	0.163	0.191	0.166
	[8,817]	[7,129]	[3,605]	[5,948]	[25,499]

Source: Author's calculations.

Note: Weighted by CPS sampling weights. The numbers in brackets are sample sizes. The survey date sample consists of job losers employed at the survey date.

part-time employment comes from part-time work on the lost job. Workers who lost a part-time job are 19 percentage points more likely than full-time job losers to be working part time on their new job. This finding could reflect labor supply preferences or some other unmeasured characteristic that makes full-time work difficult to get for some workers.

Females are substantially more likely (about 11 percentage points) to be working part time after displacement, even after controlling for part-time status on the lost job. Some females may have preferred fewer hours of work than were required on the lost job, perhaps due to hours constraints. This is consistent with the finding of lower postdisplacement employment probabilities for females documented in table 6. No difference emerges in part-time rates by race. Compared with the youngest workers, prime-age workers (age 25–54) are less likely to be working part time. The oldest workers (age 55–64) are more likely than even the youngest workers to be working part time, perhaps reflecting a move toward partial retirement. This is consistent with the finding of lower postdisplacement employment probabilities for older workers documented in table 6. The probability of working part time is inversely related to education, with college graduates about 3 percentages points less likely than high school graduates to be working part time. The last four columns of table 8 contain normalized estimates from separate probit analyses by educational category. The age, race, and sex patterns are roughly similar across the four groups.

The estimates suggest that workers often take a part-time job temporarily, perhaps until they can find a full-time job. Workers displaced two and three years ago are significantly less likely (about 4 percentage points) to be employed part time than workers displaced in the year prior to the survey. This is true for workers in all educational categories.

Given the lack of comparability of the data on part-time status between the 1984–92 DWSs and the 1994 and 1996 DWSs, I draw substantive inferences only with caution regarding changes over time in the part-time rate. The estimates suggest that there was a cyclical component to part-time rates, with relatively high part-time rates in the slack labor market of 1981–83 and much lower part-time rates in the 1983–89 period. The increase in 1989–91 probably reflects the slack labor market of that period, but the continuing relatively high part-time rate after 1991 could reflect either a real increase in part-time work

Table 8. Probability of Part-Time Employment at Survey Date, 1981–95, Normalized Probit Estimates

Variable	*Pooled*	*Pooled*	*Ed < 12*	*Ed = 12*	*Ed 13–15*	*Ed ≥ 16*
Constant	−0.238	−0.240	−0.251	−0.263	−0.187	−0.287
	(0.009)	(0.010)	(0.026)	(0.014)	(0.019)	(0.024)
Ed < 12	0.035	0.035	. . .	. . .	. . .	. . .
	(0.007)	(0.007)				
Ed 13–15	0.009	0.003	. . .	. . .	. . .	. . .
	(0.006)	(0.006)				
Ed ≥ 16	−0.026	−0.031	. . .	. . .	. . .	. . .
	(0.007)	(0.007)				
1983–85	−0.022	−0.023	−0.034	−0.030	−0.033	0.012
	(0.009)	(0.009)	(0.025)	(0.014)	(0.021)	(0.020)
1985–87	−0.050	−0.050	−0.055	−0.035	−0.091	−0.032
	(0.009)	(0.009)	(0.025)	(0.014)	(0.021)	(0.021)
1987–89	−0.045	−0.045	−0.077	−0.035	−0.078	−0.009
	(0.010)	(0.010)	(0.026)	(0.014)	(0.021)	(0.020)
1989–91	−0.008	−0.010	−0.039	−0.018	0.003	0.001
	(0.009)	(0.009)	(0.026)	(0.013)	(0.018)	(0.019)
1991–93	0.008	0.002	−0.051	0.020	−0.012	0.017
	(0.009)	(0.009)	(0.027)	(0.013)	(0.018)	(0.018)
1993–95	−0.008	−0.016	−0.031	−0.023	−0.031	0.020
	(0.009)	(0.009)	(0.026)	(0.014)	(0.018)	(0.018)
Slack work	0.013	0.009	0.025	0.013	0.004	−0.001
	(0.006)	(0.006)	(0.017)	(0.009)	(0.012)	(0.014)
Position abolished	0.009	0.010	0.019	0.008	0.007	0.006
	(0.007)	(0.007)	(0.030)	(0.012)	(0.014)	(0.013)
Other reason	0.030	0.022	0.044	0.008	0.028	0.023
	(0.008)	(0.008)	(0.023)	(0.012)	(0.016)	(0.016)

(*continued*)

among displaced workers or a survey-redesign comparability problem. Some differences appear across educational groups in movements in the time dimension. For the lower educational categories, the part-time rate in the 1990s remained below that in the 1981–83 period, although not as low as in the mid-1980s. In contrast, displaced college graduates have had higher and increasing part-time rates since 1991.

Finally, consider the differences in the part-time rate by reason for displacement. Consistent with the tabulations in table 7, workers displaced for "other" reasons are significantly more likely to be working part time (2.2 percentage points). This finding is true even after controlling for their higher part-time rate on the predisplacement job and

Table 8 (*continued*)

Variable	*Pooled*	*Pooled*	*Ed < 12*	*Ed = 12*	*Ed 13–15*	*Ed ≥ 16*
Other reason	−0.001	0.016	0.024	0.018	0.027	−0.009
(1994–96 DWS)	(0.011)	(0.011)	(0.035)	(0.018)	(0.022)	(0.021)
Part-time	. . .	0.190	0.221	0.197	0.198	0.143
old job		(0.007)	(0.023)	(0.011)	(0.013)	(0.013)
Age 25–34	−0.070	−0.044	−0.021	−0.022	−0.089	−0.005
	(0.007)	(0.007)	(0.021)	(0.010)	(0.013)	(0.019)
Age 35–44	−0.074	−0.046	−0.042	−0.027	−0.091	0.004
	(0.007)	(0.007)	(0.022)	(0.011)	(0.014)	(0.019)
Age 45–54	−0.070	−0.041	−0.010	−0.012	−0.126	0.016
	(0.008)	(0.008)	(0.024)	(0.013)	(0.016)	(0.021)
Age 55–64	0.006	0.035	0.044	0.050	−0.011	0.091
	(0.010)	(0.010)	(0.026)	(0.015)	(0.022)	(0.023)
Lost job 2	. . .	−0.039	−0.018	−0.043	−0.042	−0.036
years ago		(0.006)	(0.018)	(0.009)	(0.012)	(0.012)
Lost job 3	. . .	−0.042	−0.027	−0.054	−0.031	−0.037
years ago		(0.006)	(0.018)	(0.009)	(0.012)	(0.012)
Female	0.138	0.114	0.111	0.131	0.082	0.120
	(0.005)	(0.005)	(0.015)	(0.007)	(0.009)	(0.009)
Nonwhite	0.006	0.008	0.033	0.018	0.003	−0.027
	(0.007)	(0.007)	(0.019)	(0.011)	(0.013)	(0.015)
Number	25,265	25,265	3,070	10,146	6,851	5,198
log L	−10733.7	−10305.4	−1362.5	−4081.8	−2940.1	−1846.8

Source: Author's calculations.

Note: The coefficients are normalized to represent the derviative of the probability of the outcome with respect to a change in the explanatory variable. This is computed as $\hat{\beta}\phi(\bar{X}\hat{\beta})$ where β is the vector of estimated parameters of the probit model, $\bar{X}$ is the vector of means of the explanatory variables, and ϕ is the standard normal probability density function. The dependent variable equals 1 if the individual is employed part time (<35 hours a week) at the DWS survey date and equals 0 if the individual is employed full time. Displaced workers not employed at the survey date are not included in the sample. All analyses are weighted by the adjusted CPS sampling weights. The base categories of the independent variables are 20- to 24-year-old white males with twelve years of education who lost a job in the 1981–83 period in the year before the survey date due to a plant closing. Part-time status on the old job and the specific year of job loss are missing for all who report job loss in the 1994 and 1996 DWSs due to "other" (2,680 observations). The variable "other (94–96 DWS)" is an indicator variable for these 2,680 observations, and the missing variables are set to zero for these observations. The asymptotic standard errors are in parentheses.

across educational categories. No significant difference in part-time rates across the remaining three categories of reason for job loss is found.

The Change in Earnings

I begin this analysis by examining the difference in real weekly earnings between the postdisplacement job and the job from which the worker was displaced.[54] This is a straightforward measure, but it only gets at a part of the effect of displacement on earnings for several reasons.

First, it is appropriate to ask what earnings would have been had the worker not been displaced. In order to answer this question, the earnings change over the same period for a control group of nondisplaced workers is required. Later in this section, I provide such a control group using data from the CPS outgoing rotation groups.

Second, as I showed earlier, job loss produces strong negative effects on hours of work. In the analysis of earnings change, I generally control for part-time status on the survey-date job, even though it is another outcome strongly related to displacement. Data on the hourly wage on the lost job are not available, and including an indicator of part-time status provides a crude adjustment for hours worked. But when considering the overall effect of displacement on earnings, there is not only the direct wage effect but also the indirect negative effect through the increased probability of part-time work measured in the previous section and the lower wage rate earned by part-time workers.

Third, a similar issue arises because the analysis of earnings, of necessity, includes only displaced workers who are employed at the survey date. But part of the effect of job loss on earnings comes from the fact that a substantial fraction of displaced workers are *not* employed at the survey date.[55] Thus, when considering the overall effect of displacement on earnings, the direct wage effect needs to be augmented with the nonemployment effect as well as with the part-time effect.[56]

Difference Estimates of the Change in Earnings as a Result of Job Loss

The top panel of table 9 contains average changes in log real weekly earnings between the lost job and the survey-date job broken down by survey year and reason for displacement. The sample used here does

54. 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 the DWS survey month is used to deflate current earnings.

55. About 35 percent of displaced workers, on average, are not employed at the survey date. See tables 5 and 6 and the related discussion.

56. Not considered here are the earnings losses associated with any period of unemployment suffered by displaced workers who ultimately find employment.

Table 9. Postdisplacement Change in Earnings, by Year and Reason

Year	Plant closing	Slack work	Position abolished	Other	All
Average Change in Log Real Weekly Earnings for All Transitions					
1981–83	−0.109	−0.190	−0.191	−0.145	−0.155
	(0.020)	(0.020)	(0.038)	(0.035)	(0.012)
1983–85	−0.127	−0.147	−0.102	−0.129	−0.131
	(0.019)	(0.022)	(0.038)	(0.036)	(0.013)
1985–87	−0.101	−0.145	−0.113	−0.105	−0.115
	(0.018)	(0.024)	(0.037)	(0.031)	(0.012)
1987–89	−0.071	−0.135	−0.127	−0.091	−0.098
	(0.019)	(0.025)	(0.040)	(0.031)	(0.013)
1989–91	−0.137	−0.203	−0.215	−0.086	−0.157
	(0.017)	(0.021)	(0.034)	(0.030)	(0.011)
1991–93	−0.174	−0.122	−0.212	. . .	−0.164
	(0.018)	(0.019)	(0.024)		(0.012)
1993–95	−0.128	−0.020	−0.187	. . .	−0.103
	(0.019)	(0.019)	(0.024)		(0.012)
All years	−0.122	−0.134	−0.176	−0.108	−.133
	(0.007)	(0.008)	(0.011)	(0.013)	(0.005)
Average Change in Log Real Weekly Earnings for Full-Time to Full-Time Transitions					
1981–83	−0.103	−0.105	−0.111	−0.003	−0.090
	(0.017)	(0.017)	(0.033)	(0.034)	(0.011)
1983–85	−0.076	−0.074	−0.058	−0.104	−0.077
	(0.016)	(0.020)	(0.033)	(0.034)	(0.011)
1985–87	−0.091	−0.116	−0.089	−0.110	−0.101
	(0.015)	(0.020)	(0.032)	(0.029)	(0.011)
1987–89	−0.043	−0.059	−0.082	−0.050	−0.054
	(0.016)	(0.021)	(0.033)	(0.029)	(0.011)
1989–91	−0.100	−0.140	−0.145	−0.045	−0.109
	(0.015)	(0.018)	(0.029)	(0.029)	(0.010)
1991–93	−0.133	−0.085	−0.150	. . .	−0.120
	(0.015)	(0.017)	(0.021)		(0.010)
1993–95	−0.068	−0.009	−0.107	. . .	−0.058
	(0.016)	(0.017)	(0.020)		(0.010)
All years	−0.089	−0.083	−0.113	−0.063	−0.088
	(0.006)	(0.007)	(0.010)	(.012)	(0.004)

Source: Author's calculations.

Note: The change in log real weekly earnings is computed as the difference between postdisplacement log real weekly earnings and predisplacement log real weekly earnings. Earnings are deflated by the 1982–84 = 100 CPI. All means are weighted by CPS sampling weights. The overall sample size is 18,616 including all transitions and 14,504 for full-time to full-time transitions. Standard errors are in parentheses.

not include workers who report being displaced for "other" reasons in 1994 and 1996 DWSs because they are not asked about earnings on the lost job.[57] On average, real weekly earnings on the postdisplacement job are about 13 percent lower than predisplacement earnings. An important part of the earnings losses suffered by displaced workers can be attributed to the substantial incidence of part-time employment after displacement (see table 6). In order to focus on full-time workers, the bottom panel of table 9 contains average changes in log real full-time weekly earnings between the lost job and the survey-date job broken down by survey year and reason for displacement. These changes are computed using the subsample of displaced workers who were displaced from full-time jobs and are employed at the survey date on another full-time job. The earnings losses are substantial, even for these workers. The average decline in weekly earnings is about 9 percent for currently full-time workers displaced from full-time jobs.

Some variation over time in average earnings loss emerges, both overall and for full-time workers. Historically, earnings loss has been greater in the slack labor markets (1981–83 and 1989–91) than in the intervening tighter labor market (1983–89) and the subsequent tighter labor market (1993–95). There is considerably less variation over time in the real earnings changes of workers making full-time to full-time transitions. The cyclicality in overall earnings changes may reflect the cyclicality of part-time work in response to job loss. In slack labor markets, more displaced workers are making full-time to part-time transitions, which results in a larger average earnings decline.

The patterns with regard to the reason for job loss are interesting. In particular, it appears that the earnings loss suffered by workers displaced due to position/shift abolished increased secularly between 1983–85 and 1991–93 before moderating somewhat in the 1993–95 period. The average loss for full-time workers increased from about 6 percent to about 15 percent before declining to about 11 percent. The fluctuations in earnings losses for job losers due to slack work appears to have a more "standard" cyclical pattern; those for job losers due to plant closing are intermediate. Although only available for the 1981–91

57. Note also that the "trick" used in the analysis of part-time rates to get around the problem of missing data for "other" job losers in the 1994 and 1996 DWSs will not work here. In this case, the dependent variable cannot be calculated for these observations because of missing data on earnings on the predisplacement job.

time period, the wage loss of job losers for "other" reasons appears to be smaller than job losers in the remaining categories.

Table 10 contains estimates of a regression of the difference in log real weekly earnings between the job held at the survey date and the predisplacement job for employed workers, both pooled across educational categories and estimated separately for each educational category. Note that these difference estimates do not take into account the extent to which wages would have grown had the worker not been displaced. The estimates in the first column do not include controls for whether the old or new jobs were part time. Because the earnings measure is weekly earnings, it is likely to be strongly correlated with the part-time variables. But, as noted earlier, part-time status on the new job is a consequence of job loss. Thus, although most of the regression analyses of earnings will include these measures, it is worth investigating how sensitive the results are to their inclusion. The earnings change model without the part-time measures has a relatively small R^2 (not surprising for a difference model) of 0.024. Inclusion of part-time status on the lost job (column 2 of table 10) raises the R^2 to 0.064, and the old job part-time status indicator is strongly positively correlated with the change in earnings. This is expected because part-time status on the old job is strongly negatively related to earnings in the initial period. Finally, the model in column 3 of table 10 includes part-time status on the current job as well. The R^2 of this model is fully 0.219, and part-time status on the current job is strongly negatively related to the change in earnings.[58] For the most part, the estimated coefficients on the other variables in the model are not affected in important ways by whether or not the part-time status variables are included in the model. (Note exceptions in the discussion that follows.)

This analysis of earnings change for displaced workers confirms the standard finding that older workers suffer substantially larger wage declines than younger workers.[59] This decline is larger when part-time status is not controlled, reflecting the movement toward part-time work by older job losers. Race differences are not significant. No relationship

58. The coefficient on the old job part-time indicator becomes more strongly negative when part-time status on the new job is included in the model because workers who lose part-time jobs are also more likely to be reemployed on a part-time job.

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

Table 10. Regression Analysis of Change in Log Real Weekly Earnings at Survey Date, 1981–95

Variable	*Pooled*	*Pooled*	*Pooled*	*Ed < 12*	*Ed = 12*	*Ed 13–15*	*Ed ≥ 16*
Constant	−0.057	−0.128	−0.026	−0.125	−0.028	0.012	0.034
	(0.019)	(0.019)	(0.017)	(0.042)	(0.024)	(0.036)	(0.054)
Ed < 12	−0.043	−0.042	−0.020	. . .	. . .	. . .	. . .
	(0.014)	(0.014)	(0.013)				
Ed 13–15	0.012	−0.002	−0.003	. . .	. . .	. . .	. . .
	(0.011)	(0.011)	(0.010)				
Ed ≥ 16	0.079	0.067	0.042	. . .	. . .	. . .	. . .
	(0.013)	(0.013)	(0.012)				
1983–85	0.031	0.033	0.019	0.051	−0.004	0.037	0.030
	(0.017)	(0.017)	(0.016)	(0.038)	(0.022)	(0.035)	(0.040)
1985–87	0.048	0.050	0.018	0.046	0.024	0.056	−0.072
	(0.017)	(0.017)	(0.015)	(0.037)	(0.022)	(0.034)	(0.039)
1987–89	0.063	0.067	0.037	0.127	0.019	0.027	0.025
	(0.018)	(0.017)	(0.016)	(0.039)	(0.023)	(0.035)	(0.040)
1989–91	0.008	0.011	0.004	0.050	0.004	−0.029	0.020
	(0.017)	(0.016)	(0.015)	(0.040)	(0.022)	(0.031)	(0.037)
1991–93	0.016	0.010	0.011	0.152	−0.040	0.019	0.009
	(0.017)	(0.017)	(0.015)	(0.043)	(0.023)	(0.031)	(0.037)
1993–95	0.078	0.072	0.066	0.210	0.082	0.049	−0.007
	(0.017)	(0.017)	(0.015)	(0.043)	(0.023)	(0.032)	(0.037)
Slack work	−0.021	−0.021	−0.015	−0.027	. . .	−0.009	−0.020
	(0.011)	(0.011)	(0.010)	(0.025)		(0.020)	(0.026)
Position abolished	−0.065	−0.058	−0.054	−0.001	−0.041	−0.075	−0.047
	(0.014)	(0.013)	(0.012)	(0.045)	(0.020)	(0.023)	(0.025)
Other reason	0.011	0.005	0.018	0.063	−0.010	0.050	0.013
	(0.015)	(0.015)	(0.014)	(0.035)	(0.020)	(0.028)	(0.032)

Part-time	. . .	0.420	0.621	0.424	0.615	0.596	0.767
old job		(0.015)	(0.014)	(0.042)	(0.022)	(0.026)	(0.033)
Part-time	. . .	. . .	−0.716	−0.573	−0.663	−0.766	−0.867
new job			(0.012)	(0.031)	(0.018)	(0.023)	(0.031)
Lost job 2	0.020	0.028	−0.000	0.058	−0.021	0.006	−0.010
years ago	(0.011)	(0.011)	(0.010)	(0.027)	(0.015)	(0.020)	(0.023)
Lost job 3	0.020	0.034	0.005	0.032	−0.009	0.007	0.013
years ago	(0.011)	(0.011)	(0.010)	(0.027)	(0.015)	(0.020)	(0.024)
Age 25–34	−0.070	−0.019	−0.040	−0.078	−0.025	−0.070	−0.025
	(0.014)	(0.014)	(0.013)	(0.033)	(0.018)	(0.024)	(0.045)
Age 35–44	−0.156	−0.103	−0.123	−0.098	−0.094	−0.174	−0.137
	(0.015)	(0.015)	(0.013)	(0.036)	(0.019)	(0.026)	(0.046)
Age 45–54	−0.190	−0.136	−0.154	−0.150	−0.130	−0.180	−0.178
	(0.017)	(0.017)	(0.015)	(0.038)	(0.022)	(0.030)	(0.049)
Age 55–64	−0.338	−0.288	−0.247	−0.314	−0.210	−0.267	−0.239
	(0.022)	(0.021)	(0.020)	(0.045)	(0.028)	(0.044)	(0.058)
Female	−0.004	−0.054	0.023	−0.006	0.021	0.018	0.038
	(0.009)	(0.009)	(0.009)	(0.025)	(0.013)	(0.017)	(0.021)
Nonwhite	−0.015	−0.015	−0.009	−0.016	−0.018	−0.018	0.035
	(0.014)	(0.014)	(0.012)	(0.032)	(0.019)	(0.024)	(0.032)
Number	18,595	18,595	18,595	2,397	7,720	4,917	3,561
R^2	0.024	0.064	0.219	0.184	0.202	0.242	0.264

Source: Author's calculations.

Note: The dependent variable is the difference in real log weekly earnings between the postdisplacement job held at the survey date and the job from which the worker was displaced. All analyses are weighted by the CPS sampling weights. The base categories of the independent variables are 20- to 24-year-old white males working full time with twelve years of education who lost a full-time job in the 1981–83 period in the year before the survey date due to a plant closing. Standard errors are in parentheses.

apparently exists between time since displacement and the change in earnings. Females suffer somewhat smaller earnings losses than do males on average (about 2.3 percentage points) when part-time status is controlled, but there is no difference by sex when part-time status is not controlled. A combination of factors including higher part-time rates among female job losers both before and after job loss, and lower postdisplacement employment rates among females are probably responsible. Labor supply response to job loss is a more important factor for females than for males.

The results also show that the earnings loss declines with education. The advantage of college-educated workers over high school workers in this dimension is larger when part-time status is not controlled. College-educated workers suffer an average earnings decline that is about 7.9 percentage points smaller than that experienced by high school graduates when part-time status is not controlled. This differential falls to 4.2 percentage points when part-time status is controlled.

Regarding changes over time in the earnings decline, it appears the regression-adjusted earnings decline was significantly and substantially smaller in the 1993–95 period than in any earlier period. This is true regardless of whether or not part-time status is controlled. The extent of this decrease in the earnings decline in the 1993–95 period varies by educational category (see the estimates in the last four columns of table 10). A comparison of the estimates of the constants in these four regressions verifies that the average earnings decline was larger in the base period for the least educated workers in the base group (white male workers 20 to 24 years old who lost their job in the year preceding the survey because of a plant closing). By the most recent period, however, the regression-adjusted proportional earnings decline was larger for college-educated workers than for those with a high school education or less.

Note some interesting findings regarding the relationship between the reported reason for displacement and the change in earnings. Workers displaced due to position/shift abolished suffer a substantially larger decline in earnings (about 5.4 percentage points when part-time status is controlled) than workers displaced due to plant closing. No significant difference in earnings change between workers displaced due to slack work or "other" reasons and workers displaced due to a plant closing is evident. Although the standard errors are relatively large, these findings are confirmed by the education-category specific estimates. It is clearly the case that job loss due to position/shift abolished has the most negative consequences for earnings for workers in all educational categories other than for those with less than a high school education.

This analysis has potentially important implications for evaluating the impact of corporate restructuring and downsizing. First, the rate of job loss due to position/shift abolished has been increasing, particularly among more educated workers. Second, the consequences for full-time earnings of job loss due to position/shift abolished are particularly severe. To the extent that job loss due to position/shift abolished, in fact, reflects job loss due to corporate restructuring and downsizing (by no means a small leap), it appears that recent changes in the employment relationship have imposed costs on the affected workers that are larger than those borne by workers who have lost jobs in other situations.

Difference-in-Difference Estimates of the Change in Earnings as a Result of Job Loss

An important weakness of the difference analysis of the effect of job loss on earnings is that it does not take into account the extent to which earnings might have grown had the workers not been displaced. But the appropriate counterfactual is not clear, even conceptually, because it depends on the interpretation given to the cause of displacement, even abstracting from poor work performance on an individual basis. It is almost a tautology to say that the job loss occurred because of a shock that caused the value of output to fall below the wage (interpreted to include all variable labor costs associated with the worker). I consider two extreme interpretations that lead to different counterfactuals.

In the first interpretation, the counterfactual is that the shock occurred, but the response to the shock was such that the firm lowered wages and did not displace the worker. In this case, the worker might have quit to find a better paying job or the worker might have stayed with the firm at the reduced wage. With either response, the worker's wage would have evolved "naturally" subsequent to the initial adjustment. With this interpretation, the shock itself is not counted as part of the effect of job loss on the wage. An appropriate estimate of the effect of job loss is the difference between the wage at the survey date and the wage the firm would have been willing to pay the worker rather than terminate him or her (the firm's reservation wage). Note at least two problems with this interpretation. First, an operational problem: The firm's reservation wage is not observable, and no obvious control group exists from which to calculate the reservation wage.[60] Second, it may be that the direct negative effect of the shock itself ought be part of the cost of job loss. Otherwise, in many cases, job loss would appear to have a positive effect on the wage. For example, consider a worker with particular skills useful in a variety of industries but whose current industry of employment is hit with a substantial negative demand shock. This worker is likely to find comparable employment in other industries, but the current employer's reservation wage is considerably lower than either the predisplacement wage or the wage on the new job. The "effect" of job loss on this worker appears positive.

In the second interpretation, the counterfactual is that the shock never occurred. The worker would have had the option of remaining with the firm at the old wage, which would then have evolved "naturally" between the date of pseudo-displacement and the survey date. In this case, it is easier to conceive of a (somewhat imperfect) control group of workers whose employers did not suffer job-ending shocks. This control group consists of workers who were not displaced. A difference-in-difference estimate of the cost of job loss in this case would be computed as

$$DID = (\ln W_{dt} - \ln W_{d0}) - (\ln W_{ct} - \ln W_{c0}) \quad (2)$$

where d refers to displaced workers (the "treatment" group), c refers to nondisplaced workers (the "control" group), t refers to "current" (postdisplacement) period, and 0 refers to the "initial" (predisplacement) period. The first difference $(\ln W_{dt} - \ln W_{d0})$ is the difference estimate of the earnings effect of job loss analyzed earlier. This includes the direct negative effect of the shock as well as the effect of the job loss itself. The second difference $(\ln W_{ct} - \ln W_{c0})$ is the estimate, based on the control group, of the amount earnings would have grown over the period had the worker not been displaced.

I use this approach to compute the difference-in-difference estimate

60. Such a control group would include workers who sustained a similar negative shock to the value of their output but whose employers reduced wages rather than terminating them.

of the effect of job loss on earnings. But the results need to be interpreted appropriately. First, this estimate counts the effect of the initial shock as part of the wage effect of job loss.[61] Second, it might be that some of the nondisplaced workers in the control group also worked for firms that suffered negative shocks but whose employers chose to reduce wages rather than to displace workers. In this case, the wage trajectory of the control group is also affected by shocks to the economy. This will tend to understate earnings growth of the control group, and the difference-in-difference estimate of the cost of job loss will be understated in absolute value. Essentially, some of the direct cost of the shock is being subtracted out.

I generate a control group using a random sample from the merged outgoing rotation group (MOGRG) files of the CPS for the three calendar years prior to each DWS together with all nondisplaced workers from the outgoing rotation groups of the CPSs containing the DWSs.[62]

In order to get ''initial'' earnings for the control group ($\ln W_{c0}$), I take a random sample from the MOGRG file each year from 1981 to 1995. The size of the random sample was set so (1) the size of the sample with initial earnings on the control group was expected to be the same size as the current earnings on the control group (two rotation groups), and (2) the distribution of years since the associated DWS survey date roughly mimicked the distribution of years since displacement in the sample of displaced workers. This is 45 percent from the year prior to the DWS, 30 percent from two years prior to the DWS, and 25 percent from three years prior to the DWS. In other words, a

61. Although including the effect of the initial shock is not wrong or inappropriate, it needs to be clearly understood.

62. Note that the random sample from MOGRG files, which I use to compute ''initial'' period earnings for the control group, contains displaced as well as not-displaced workers. As such, this sample understates initial period earnings of nondisplaced workers because displaced workers earn somewhat less than nondisplaced workers, even before displacement. Although I cannot eliminate workers who were displaced from this sample, I could have used a sample of ''current'' period earnings for the control group that does *not* eliminate displaced workers. The result would be a more comparable sample for the control group at both points in time, but calculation of appropriate standard errors would have become more difficult. My preliminary calculation is that the regression-adjusted difference in initial period full-time earnings between displaced and not-displaced workers is about 5 percent. Given a maximum job-loss rate among employed workers of about 10 percent, this suggests that the initial period earnings of the control sample might be understated by as much as 0.5 percent. This does not appear to be a first-order problem.

separate control sample was drawn for each DWS from the three MOGRGs for the years immediately prior to the DWS. Each MOGRG file has twenty-four rotation groups (two per month for twelve months). For the year immediately prior to the DWS, I needed 45 percent of two rotation groups from the twenty-four rotation groups in the relevant MOGRG file. In order to get this expected sample size, I took a random sample with probability (0.45)(2)/24. Similarly, for the second and third years prior to the DWS, I took random samples with probability (0.30)(2)/24 and (0.25)(2)/24, respectively. The resulting sample of earnings for full-time workers contains 74,836 observations.

The CPSs containing the DWSs have two outgoing rotation groups (OGRGs) with earnings data for all workers. These provide the observations on current earnings for the control group of nondisplaced workers ($\ln W_{ct}$). This sample contains observations on full-time earnings for 67,865 workers who did not report a job loss in the last three years.

The source of data for the treatment group earnings is clear. These data come from the DWSs, where $\ln W_{dt}$ is survey-date earnings for displaced workers and $\ln W_{d0}$ is earnings on the lost job. Because there is heavy selection regarding which workers are employed full time (and I am only considering wage changes for workers working full time at both dates), I only use treatment group earnings for workers employed full time *both* before and after displacement.[63] This results in a sample of 14,504 earnings observations for the treatment group at each date.

Estimation of the difference-in-difference estimates proceeds in two stages. In the first stage an earnings function is estimated for the treatment group sample of the form

$$\ln W_{is} = X_{is}\beta_d + \gamma_d T_{is} + \epsilon_{is}, \tag{3}$$

where $\ln W_{is}$ measures full-time earnings for individual i in period s (either 0 or t), X is a vector of individual characteristics, β_d is a vector of coefficients, and ϵ is an error term. The variable T_{is} is a dummy variable for the postdisplacement period, and γ_d measures the change

63. This differs from my similar analysis of earnings change using the 1984–92 DWSs (Farber, 1993) where I included observations on full-time earnings on the lost jobs for displaced workers regardless of whether or not full-time earnings were observed at the survey date. Similarly, I included observations on full-time earnings at the survey date for displaced workers regardless of whether or not full-time earnings were observed on the lost job. That analysis suffers from the selection problem noted in the text.

in earnings for displaced workers ($\ln W_{dt} - \ln W_{d0}$).[64] In order to complete the first stage, I compute an earnings function for the control groups of the analogous form. This is

$$\ln W_{is} = X_{is}\beta_c + \gamma_c T_{is} + \epsilon_{is}, \quad (4)$$

where γ_c measures the change in earnings for the control group ($\ln W_{ct} - \ln W_{c0}$).

In the second stage, the estimates of $\ln W_{dt} - \ln W_{d0}$ and $\ln W_{ct} - \ln W_{c0}$ are combined to yield the difference-in-difference estimates of the earnings effect of job loss as $\gamma_d - \gamma_c$.[65] These estimates are contained in table 11.

The top panel of table 11 contains the overall regression-adjusted difference-in-difference estimates for the pooled sample and separately by year.[66] The results show that displaced workers earned 9 percent less on average after displacement than before, whereas earnings for the control group rose 3.1 percent over the same period. The difference-in-difference estimate of the earnings loss is the difference between these numbers, which is a loss of 12.1 percent. A few interesting findings emerge when examining these results by year. First, there is some movement year to year in the earnings decline for displaced workers, with a relatively small loss (6.5 percent) in the 1993–95 period. Additionally, the rate of earnings growth for the control group is smaller

64. Note that I do not calculate first-differenced estimates (as in table 10) even though the observations are paired. This method keeps the estimates comparable to those I derive for the control group, where the observations are not paired. The standard errors I compute are adjusted to account for a random-effects error structure within individuals, however.

65. This differs from my similar analysis of earnings change using the 1984–92 DWSs (Farber, 1993) where I estimated a single earnings function for all workers and included dummy variables for the current period (C_{is}), the treatment group (D_i), and the interaction of the dummy variables for the current period and the treatment group ($C_{is}D_i$). The coefficient of $C_{is}D_i$ is the difference-in-difference estimate of the effect of job loss on earnings in this context. The approach used in the current study has the advantage of allowing for differences in the determination of earnings between the treatment and control groups while still providing a summary measure of the effect of job loss on earnings.

66. These difference estimates incorporate the effect of normal growth along the age-earnings profile because the age variables in the regression are measured at the DWS survey date for both the treatment and control groups. Thus, it was important that the sample fractions in the initial-earnings control group mimic the fractions in the treatment group with respect to the time until the DWS survey date. This is appropriate when considering the loss in earnings due to displacement.

Table 11. Difference-in-Difference Analysis of Effect of Displacement on Log Real Weekly Earnings

Group	*Pooled*	*1981–83*	*1983–85*	*1985–87*	*1987–89*	*1989–91*	*1991–93*	*1993–95*
All Full-Time Workers								
ΔEarnings	−0.090	−0.092	−0.084	−0.107	−0.056	−0.106	−0.113	−0.065
displaced	(0.004)	(0.009)	(0.010)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)
ΔEarnings	0.031	0.037	0.041	0.037	0.042	0.023	0.007	0.026
control	(0.002)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Job-loss	−0.122	−0.131	−0.125	−0.146	−0.098	−0.129	−0.120	−0.091
effect	(0.005)	(0.013)	(0.012)	(0.013)	(0.013)	(0.012)	(0.013)	(0.014)
Full-Time Workers with <12 Years of Education								
ΔEarnings	−0.126	−0.163	−0.150	−0.174	−0.060	−0.148	−0.085	0.000
displaced	(0.011)	(0.025)	(0.026)	(0.028)	(0.026)	(0.028)	(0.037)	(0.042)
ΔEarnings	0.007	0.002	0.028	0.015	0.031	−0.018	−0.027	0.019
control	(0.007)	(0.015)	(0.016)	(0.018)	(0.018)	(0.018)	(0.020)	(0.030)
Job-loss	−0.133	−0.165	−0.178	−0.189	−0.091	−0.131	−0.058	−0.019
effect	(0.013)	(0.030)	(0.031)	(0.033)	(0.031)	(0.034)	(0.042)	(0.052)
Full-Time Workers with 12 Years of Education								
ΔEarnings	−0.094	−0.098	−0.091	−0.093	−0.052	−0.124	−0.136	−0.058
displaced	(0.006)	(0.013)	(0.015)	(0.018)	(0.019)	(0.014)	(0.019)	(0.019)
ΔEarnings	0.025	0.025	0.017	0.041	0.037	0.021	0.013	0.021
control	(0.004)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.012)
Job-loss	−0.119	−0.123	−0.108	−0.134	−0.088	−0.145	−0.148	−0.079
effect	(0.007)	(0.016)	(0.017)	(0.020)	(0.021)	(0.017)	(0.021)	(0.023)

Full-Time Workers with 13–15 Years of Education								
ΔEarnings displaced	−0.084	−0.072	−0.066	−0.096	−0.066	−0.102	−0.103	−0.071
	(0.009)	(0.022)	(0.023)	(0.021)	(0.024)	(0.023)	(0.023)	(0.020)
ΔEarnings control	0.035	0.073	0.073	0.048	0.035	0.017	0.007	0.018
	(0.005)	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
Job-loss effect	−0.119	−0.145	−0.139	−0.144	−0.101	−0.119	−0.111	−0.089
	(0.011)	(0.026)	(0.026)	(0.025)	(0.027)	(0.026)	(0.026)	(0.024)
Full-Time Workers with ≥16 Years of Education								
ΔEarnings displaced	−0.067	−0.029	−0.031	−0.100	−0.052	−0.053	−0.105	−0.089
	(0.009)	(0.023)	(0.024)	(0.023)	(0.029)	(0.027)	(0.022)	(0.026)
ΔEarnings control	0.050	0.066	0.070	0.044	0.064	0.052	0.020	0.044
	(0.005)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.014)
Job-loss effect	−0.117	−0.095	−0.101	−0.145	−0.116	−0.105	−0.124	−0.133
	(0.011)	(0.026)	(0.028)	(0.026)	(0.032)	(0.030)	(0.026)	(0.030)

Note: The numbers in parentheses are standard errors. These estimates are derived from separate regressions for displaced workers (the DWS sample) and nondisplaced workers (the control sample) of log real weekly earnings on dummy variables for sex, race, nine age categories, four educational categories, and whether the job represents earnings at a DWS survey date ("current" earnings as contrasted with "prior" earnings). The pooled model also includes a set of dummy variables for DWS survey year. The sample of displaced workers includes those for whom full-time earnings are reported both before displacement and at the DWS survey date (n = 14,504 at each point). The standard errors for the samples of displaced workers are corrected to account for the fact that the wage observations are paired. The "current" observations for the nondisplaced workers (n = 66,210) are from the outgoing-rotation groups of the CPSs that contain the DWSs. The "prior" observations for the nondisplaced workers (n = 74,836) are a random subsample from the merged outgoing rotation group annual files of the CPS for the three years preceding each DWS with the fraction from each year taken to reflect the timing of job loss among displaced workers. The reported wage difference is the coefficient on the DWS survey-date dummy variable. The estimate of the job-loss effect is the difference between the dummy variables from the DWS and control samples.

ΔEarnings (displaced): The regression-adjusted difference in earnings between the current period (the DWS survey date) and the base period (the average of the three previous years) for displaced workers.

ΔEarnings (control): The regression-adjusted difference in earnings between the current period (the DWS survey date) and the base period (the average of the three previous years) for nondisplaced workers.

Job-loss effect: The difference-in-difference estimate of the effect of job loss on earnings, computed as the difference between Δ Earnings (displaced) and Δ Earnings (control).

since 1989 than it was in 1989 or earlier. As a result, the difference-in-difference estimate of the earnings loss in the most recent period is the smallest measured in the sample at 9.1 percent.

The remainder of table 11 contains difference-in-difference estimates of the earnings loss by education category. The results are rather interesting. Aside from the lowest educational category, the earnings losses estimated from the pooled sample are virtually identical at about 11.8 percent. But this masks differences in the sources of this earnings loss. The difference estimate of the earnings decline for displaced workers decreases monotonically with education: 9.4 percent for high school graduates and 6.7 percent for college graduates. At the same time, control group earnings growth increases monotonically with education: 2.5 percent for high school graduates and 5.0 percent for college graduates. The differences net out to almost the same figure. The college graduates forgo more earnings growth while taking a smaller earnings decline than do workers with less education.

Examining the year-by-year estimates by education level, the structure of earnings losses have altered dramatically over time. As noted before for the overall sample, earnings growth for the control group fell for all education categories. The average earnings decline for displaced workers with a college education have increased substantially since 1991. At the same time, the earnings decline for workers with less education has decreased somewhat. Thus, in the 1993–95 period, the difference-in-difference estimate of the earnings loss associated with job loss is *largest* for college graduates. The standard errors are relatively large, and it is not clear whether this pattern will continue in any case. But it does appear that earnings losses for less educated workers are significantly smaller than they had been, while there has been no such decline for college graduates.

Table 12 presents difference estimates of the change in earnings for displaced workers by reason for job loss. The difference-in-difference estimates of the wage loss are not shown as they were in table 11, but the estimates of the change in earnings for the control group of nondisplaced workers are precisely the same as those used to compute the estimates in table 11. That is, I use the same control group, based on the CPS outgoing rotation groups, for each of the four treatment subgroups used for the overall treatment group in table 11. Thus in table 12 the rows from table 11 that contain the difference estimates of the wage change for the control group are reproduced, and sufficient information is provided in the table to compute the difference-in-difference estimates.

The difference estimates for the treatment groups are derived from a common regression for the displaced workers that additionally include a set of dummy variables for the reason for job loss as well as the interaction of the postdisplacement dummy variable with the set of dummy variables for reason. This regression is

$$\ln W_{is} = X_{is}\beta_d + \sum_{k=1}^{4} R_{ik}\delta_k + T_{is}\sum_{k=1}^{4} R_{ik}\gamma_{dk} + \epsilon_{is}, \tag{5}$$

where k indexes the reason for job loss, the R_{ik}s are the reason dummy variables, and the γ_{dk}s are the difference estimates of reason-specific decline in earnings for displaced workers. The difference-in-difference estimates of the reason-specific earnings loss are computed as $\gamma_{dk} - \gamma_c$.

The estimates in table 12 verify that workers displaced due to position/shift abolished suffer the largest earnings decline (and, hence, earnings loss), and those displaced for "other" reasons suffer the smallest earnings decline. This is true for every educational category. The overall earnings loss in the most recent time period has lessened dramatically for workers displaced due to plant closing and slack work, but it only declined slightly (from a high level) for those who lost jobs due to position/shift abolished. The breakdowns by educational category in table 12 show particularly interesting results for college graduates. The large wage loss for these workers in the 1993–95 period that we saw in table 11 seems due entirely to the large wage loss that college graduates suffer when they lose a job because of position/shift abolished. A caveat to these results is that the "other" category is the modal category in the 1993–95 period, and no information on wage loss is available for "other" losers since 1991. However, it is suggestive that jobs losers for "other" reasons historically have had smaller than average earnings losses.

Concluding Remarks

The results are clear. Rates of job loss are up substantially relative to the standard of the last decade, particularly when we consider the

Table 12. Regression-Adjusted Δln Real Weekly Earnings, Displaced Full-Time Workers by Reason for Job Loss: Difference Estimates

Δ Earnings	*Pooled*	*1981–83*	*1983–85*	*1985–87*	*1987–89*	*1989–91*	*1991–93*	*1993–95*
All Full-Time Workers								
Plant closing	−0.093	−0.109	−0.082	−0.101	−0.039	−0.099	−0.130	−0.090
	(0.006)	(0.015)	(0.016)	(0.015)	(0.017)	(0.014)	(0.017)	(0.018)
Slack work	−0.091	−0.102	−0.087	−0.132	−0.067	−0.135	−0.076	−0.025
	(0.007)	(0.015)	(0.018)	(0.021)	(0.024)	(0.017)	(0.017)	(0.021)
Position abolished	−0.120	−0.120	−0.078	−0.103	−0.102	−0.136	−0.142	−0.134
	(0.010)	(0.026)	(0.028)	(0.025)	(0.027)	(0.028)	(0.028)	(0.022)
Other	−0.057	−0.003	−0.086	−0.087	−0.051	−0.046	. . .	. . .
	(0.015)	(0.027)	(0.027)	(0.034)	(0.032)	(0.037)		
Control	0.031	0.037	0.041	0.037	0.042	0.023	0.007	0.026
	(0.002)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Full-Time Workers with < 12 Years of Education								
Plant closing	−0.137	−0.211	−0.165	−0.115	−0.072	−0.143	−0.143	−0.055
	(0.015)	(0.037)	(0.037)	(0.037)	(0.032)	(0.041)	(0.057)	(0.061)
Slack work	−0.124	−0.114	−0.127	−0.283	−0.060	−0.198	−0.026	0.045
	(0.020)	(0.043)	(0.044)	(0.067)	(0.058)	(0.044)	(0.053)	(0.073)
Position abolished	−0.146	−0.334	−0.113	−0.227	−0.101	−0.048	−0.080	0.061
	(0.039)	(0.144)	(0.101)	(0.063)	(0.063)	(0.099)	(0.102)	(0.076)
Other	−0.095	−0.075	−0.176	−0.133	−0.025	−0.083	. . .	. . .
	(0.032)	(0.054)	(0.085)	(0.065)	(0.067)	(0.093)		
Control	0.007	0.002	0.028	0.015	0.031	−0.018	−0.027	0.019
	(0.007)	(0.015)	(0.016)	(0.018)	(0.018)	(0.018)	(0.020)	(0.030)
Full-Time Workers with 12 Years of Education								
Plant closing	−0.090	−0.092	−0.088	−0.087	−0.020	−0.087	−0.168	−0.113
	(0.009)	(0.021)	(0.022)	(0.023)	(0.024)	(0.019)	(0.027)	(0.031)
Slack work	−0.097	−0.108	−0.107	−0.131	−0.072	−0.154	−0.077	0.010
	(0.010)	(0.021)	(0.026)	(0.030)	(0.041)	(0.024)	(0.028)	(0.031)
Position abolished	−0.131	−0.159	−0.052	−0.088	−0.172	−0.132	−0.194	−0.111
	(0.018)	(0.040)	(0.042)	(0.044)	(0.047)	(0.076)	(0.053)	(0.037)
Other	−0.072	−0.030	−0.090	−0.050	−0.036	−0.151	. . .	. . .
	(0.022)	(0.039)	(0.043)	(0.062)	(0.052)	(0.034)		

Control	0.025	0.025	0.017	0.041	0.037	0.021	0.013	0.021
	(0.004)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.012)
Full-Time Workers with 13–15 Years of Education								
Plant closing	−0.091	−0.094	−0.013	−0.101	−0.086	−0.116	−0.105	−0.089
	(0.012)	(0.036)	(0.037)	(0.030)	(0.033)	(0.029)	(0.031)	(0.032)
Slack work	−0.088	−0.094	−0.110	−0.102	−0.039	−0.127	−0.087	−0.049
	(0.014)	(0.031)	(0.042)	(0.040)	(0.037)	(0.035)	(0.032)	(0.036)
Position abolished	−0.126	−0.069	−0.094	−0.065	−0.143	−0.196	−0.121	−0.151
	(0.022)	(0.050)	(0.053)	(0.051)	(0.056)	(0.047)	(0.058)	(0.039)
Other	−0.021	0.017	−0.071	−0.099	0.000	0.040	. . .	. . .
	(0.035)	(0.073)	(0.054)	(0.069)	(0.085)	(0.086)		
Control	0.035	0.073	0.073	0.048	0.035	0.017	0.007	0.018
	(0.005)	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
Full-Time Workers with ≥ 16 Years of Education								
Plant closing	−0.066	−0.041	−0.058	−0.126	0.019	−0.067	−0.090	−0.071
	(0.016)	(0.045)	(0.045)	(0.035)	(0.067)	(0.034)	(0.041)	(0.038)
Slack work	−0.047	−0.073	0.050	−0.017	−0.094	−0.043	−0.082	−0.052
	(0.018)	(0.039)	(0.039)	(0.053)	(0.050)	(0.055)	(0.034)	(0.060)
Position abolished	−0.103	−0.044	−0.085	−0.108	−0.003	−0.108	−0.132	−0.152
	(0.017)	(0.043)	(0.059)	(0.048)	(0.043)	(0.037)	(0.037)	(0.041)
Other	−0.044	0.080	−0.037	−0.123	−0.141	0.049	. . .	. . .
	(0.032)	(0.059)	(0.054)	(0.055)	(0.059)	(0.102)		
Control	0.050	0.066	0.070	0.044	0.064	0.052	0.020	0.044
	(0.005)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.014)

Note: The numbers in parentheses are standard errors. The estimates for the displaced workers are derived from regressions for displaced workers (the DWS sample) of log real weekly earnings on dummy variables for sex, race, nine age categories, four educational categories, dummies for the reason for the job loss, and interactions of the four reason-for-job-loss dummies with an indicator of whether the job represents earnings at a DWS survey date ("current" earnings as contrasted with "prior" earnings). The pooled model also includes a set of dummy variables for DWS survey year. The sample of displaced workers includes those for whom full-time earnings are reported both before displacement and at the DWS survey date (n = 14,504 at each point). The standard errors for the samples of displaced workers are corrected to account for the fact that the wage observations are paired. The reported earnings changes for displaced workers are the coefficients of these four interactions. The earnings change for the control group are based on data from the CPS outgoing rotation groups. These estimates are identical to those in table 11, and their derivation is described in the note to that table.

state of the labor market. The increase has not been uniform. More educated workers, although continuing to have lower rates of job loss than less educated workers, have seen their rates of job loss increase more than those of other groups. We also see interesting temporal variation in job-loss rates by reported reason. The rate of job loss due to plant closing has been fairly steady over time. In contrast, job loss due to slack work has a substantial cyclical component. Job loss due to position/shift abolished has been increasing in recent years, largely among more educated workers. Finally, job loss for "other" reasons has increased dramatically across the board.

The costs of job loss are substantial. Displaced workers have a large probability of not being employed at the survey date after displacement (about 35 percent on average). This probability is substantially smaller for workers with a college education than for workers with a high school education (22 percent compared with 38 percent).

A substantial fraction of those reemployed are working part time after displacement (about 17 percent on average compared with only 12 percent working part time prior to displacement). The college educated are significantly less likely than high school graduates to be working part time (13.5 percent compared with 16.6 percent), even though about 11 percent of both educational groups are working part time on the predisplacement job.

The decline in real weekly earnings between the predisplacement job and the postdisplacement job averages about 13 percent for all reemployed displaced workers and about 9 percent for workers displaced from full-time jobs who are reemployed on full-time jobs. The college educated suffer smaller proportional earnings declines on average, even accounting for full-time/part-time status. Among those displaced from full-time jobs who are reemployed full time, the average wage decline is 6.4 percent for college graduates and 9.3 percent for high school graduates. Difference-in-difference estimates of the earnings loss associated with job loss, computed using a sample of workers from the outgoing rotation groups of the CPS as a control, show larger regression-adjusted earnings losses for more educated workers due to foregone earnings growth. The average overall regression-adjusted earnings loss is about 11.8 percent for both high school and college graduates. Although the overall loss is the same, the more educated workers have a smaller earnings decline but forgo more growth in earnings than the less educated.

Clearly job loss adversely affects workers' earnings in many ways. Employment probabilities are reduced, and an increased probability of working part time yields lower earnings both through shorter hours and lower wage rates. These costs are larger for those workers with less education. Even those reemployed full time suffer substantial earnings losses on average, regardless of education level. Fairly strong evidence indicates that some of the costs of displacement are temporary, however. The probability-of-employment penalty and the part-time-employment penalty for displacement decline with time since displacement. However, we have little evidence that the full-time earnings penalty for displacement narrows with time since displacement. The costs due to foregone earnings growth are not likely to be recouped. An additional cost of job loss not accounted for in this framework is earnings loss during the period of nonemployment before a new job is located.

The costs of job loss are clearly countercyclical, with larger costs of job loss in slack labor markets and relatively smaller costs in tight labor markets. Postdisplacement employment probabilities and the probability of full-time employment among reemployed workers are both lower in slack labor markets. We have no evidence that the costs of job loss have increased systematically over time, however, and, in fact, the proportional wage losses are lower in the most recent period for all workers with fewer than sixteen years of education.

One of the goals of this analysis was to investigate how the incidence and consequences of job loss varied by stated reason for the loss. In particular, I wanted to determine whether job loss due to position/shift abolished, perhaps capturing job loss due to corporate restructuring and downsizing, could be characterized as a distinct phenomenon from job loss due to slack work and plant closing. The evidence is clear that the rate of job loss due to position/shift abolished has increased, particularly for more educated workers. And there is clear evidence that job loss due to position/shift abolished has more serious negative consequences for earnings than does job loss for other reasons. Again, this is a particularly important factor for college graduates.

A remaining mystery is that job loss due to "other" reasons has shown a dramatic rise in recent years. Workers who lose jobs for this reason are more likely to work part time but suffer smaller earnings losses than other displaced workers. Unfortunately, very little information is available regarding exactly what sort of job loss comprises the other category.[67]

In conclusion, the rate of job loss has increased in recent years, and job loss for all reasons imposes substantial costs on the affected workers. There is not very clear evidence that the consequences of job loss have changed systematically. But it is the case that job loss due to position/shift abolished is on the increase, particularly for more educated workers, and this form of job loss has particularly large negative earnings effects. Does this mean that corporate downsizing and restructuring is causing the increase in job loss for this reason and, hence, causing an increase in the cost of job loss? That remains an open question given the tenuous link between any specific stated reason for job loss and the reality of downsizing and restructuring.

67. The February 1996 DWS contained debriefing questions asked of respondents in the CPS outgoing rotation groups, and some of these questions asked for more detail on the reason for job loss. Unfortunately, these data have not yet been made available to researchers outside the Bureau of Labor Statistics. Katharine Abraham, in her comments on this paper, does present some interesting preliminary tabulations of these data that shed some light on job loss for "other" reasons.

Appendix

The following six tables contain the numerical values for figures 1 through 6.

Table A-1. Three-Year Rate of Job Loss by Reason, 1981–95 (Numbers for figure 1)

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
All Individuals					
1981–83	0.133	0.045	0.054	0.014	0.019
1983–85	0.107	0.042	0.036	0.012	0.017
1985–87	0.101	0.041	0.029	0.012	0.020
1987–89	0.090	0.036	0.024	0.011	0.019
1989–91	0.124	0.044	0.042	0.015	0.022
1991–93	0.128	0.036	0.037	0.022	0.032
1993–95	0.151	0.032	0.038	0.024	0.056

Source: Author's calculations.

Table A-2. Three-Year Rate of Job Loss by Reason, 1981–95 (Numbers for figure 2)

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
Males					
1981–83	0.152	0.049	0.069	0.014	0.020
1983–85	0.122	0.046	0.045	0.012	0.018
1985–87	0.116	0.044	0.037	0.012	0.023
1987–89	0.099	0.037	0.031	0.011	0.020
1989–91	0.143	0.047	0.055	0.017	0.025
1991–93	0.144	0.038	0.048	0.023	0.034
1993–95	0.158	0.032	0.048	0.023	0.054
Females					
1981–83	0.110	0.040	0.037	0.013	0.019
1983–85	0.090	0.037	0.025	0.013	0.016
1985–87	0.084	0.037	0.019	0.011	0.017
1987–89	0.081	0.036	0.016	0.011	0.018
1989–91	0.103	0.041	0.029	0.013	0.019
1991–93	0.110	0.033	0.026	0.022	0.030
1993–95	0.143	0.032	0.027	0.025	0.059

Source: Author's calculations.

Table A-3. Three-Year Rate of Job Loss by Reason, 1981–95 (Numbers for figure 3)

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
Age 20–24					
1981–83	0.165	0.052	0.073	0.014	0.026
1983–85	0.123	0.044	0.045	0.011	0.023
1985–87	0.103	0.038	0.035	0.008	0.022
1987–89	0.096	0.035	0.030	0.008	0.023
1989–91	0.145	0.049	0.058	0.010	0.028
1991–93	0.135	0.036	0.044	0.014	0.042
1993–95	0.202	0.033	0.057	0.016	0.096
Age 25–34					
1981–83	0.155	0.048	0.069	0.015	0.023
1983–85	0.120	0.044	0.044	0.013	0.018
1985–87	0.113	0.044	0.036	0.012	0.021
1987–89	0.103	0.041	0.029	0.012	0.021
1989–91	0.135	0.046	0.050	0.014	0.024
1991–93	0.138	0.037	0.045	0.021	0.034
1993–95	0.168	0.033	0.047	0.022	0.066
Age 35–44					
1981–83	0.113	0.039	0.045	0.013	0.017
1983–85	0.100	0.040	0.033	0.012	0.015
1985–87	0.098	0.040	0.025	0.012	0.021
1987–89	0.088	0.035	0.021	0.013	0.019
1989–91	0.117	0.042	0.038	0.016	0.020
1991–93	0.122	0.033	0.036	0.024	0.029
1993–95	0.138	0.032	0.033	0.027	0.047
Age 45–54					
1981–83	0.100	0.040	0.034	0.012	0.013
1983–85	0.085	0.036	0.025	0.013	0.011
1985–87	0.085	0.038	0.021	0.011	0.015
1987–89	0.072	0.031	0.017	0.010	0.014
1989–91	0.108	0.038	0.033	0.019	0.018
1991–93	0.115	0.033	0.027	0.025	0.029
1993–95	0.125	0.032	0.028	0.026	0.039
Age 55–64					
1981–83	0.109	0.047	0.033	0.012	0.016
1983–85	0.094	0.045	0.021	0.013	0.016
1985–87	0.090	0.041	0.018	0.014	0.018
1987–89	0.080	0.038	0.019	0.010	0.013
1989–91	0.111	0.046	0.033	0.013	0.019
1991–93	0.130	0.044	0.030	0.025	0.031
1993–95	0.130	0.031	0.023	0.026	0.050

Source: Author's calculations.

Table A-4. Three-Year Rate of Job Loss by Reason, 1981–95 (Numbers for figure 4)

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*	*Other*
Education < 12 years					
1981–83	0.193	0.067	0.083	0.012	0.030
1983–85	0.154	0.065	0.056	0.011	0.022
1985–87	0.142	0.061	0.043	0.010	0.028
1987–89	0.128	0.056	0.039	0.006	0.028
1989–91	0.184	0.067	0.076	0.009	0.032
1991–93	0.166	0.056	0.057	0.009	0.043
1993–95	0.203	0.045	0.063	0.012	0.079
Education = 12 years					
1981–83	0.148	0.051	0.064	0.013	0.020
1983–85	0.120	0.047	0.042	0.012	0.019
1985–87	0.109	0.045	0.033	0.011	0.020
1987–89	0.099	0.042	0.028	0.010	0.020
1989–91	0.136	0.051	0.049	0.012	0.023
1991–93	0.135	0.040	0.044	0.018	0.033
1993–95	0.161	0.035	0.046	0.020	0.059
Education 13–15 years					
1981–83	0.123	0.041	0.049	0.014	0.018
1983–85	0.100	0.037	0.033	0.014	0.017
1985–87	0.100	0.040	0.027	0.013	0.020
1987–89	0.088	0.035	0.022	0.013	0.018
1989–91	0.119	0.044	0.038	0.016	0.022
1991–93	0.135	0.036	0.038	0.026	0.034
1993–95	0.160	0.037	0.039	0.024	0.059
Education ≥ 16					
1981–83	0.074	0.023	0.022	0.015	0.013
1983–85	0.063	0.023	0.016	0.013	0.011
1985–87	0.064	0.023	0.014	0.012	0.016
1987–89	0.059	0.020	0.012	0.013	0.014
1989–91	0.087	0.025	0.024	0.022	0.016
1991–93	0.095	0.021	0.022	0.027	0.026
1993–95	0.113	0.020	0.018	0.032	0.043

Source: Author's calculations.

Table A-5. Three-Year Rate of Job Loss by Reason, 1981–95 (Numbers for figure 5)

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*
Managers				
1981–83	0.082	0.042	0.026	0.014
1983–85	0.070	0.039	0.018	0.013
1985–87	0.074	0.041	0.018	0.015
1987–89	0.064	0.039	0.012	0.013
1989–91	0.093	0.045	0.025	0.022
1991–93	0.097	0.039	0.027	0.030
1993–95	0.078	0.034	0.020	0.024
Professional and Technical				
1981–83	0.051	0.018	0.023	0.011
1983–85	0.053	0.021	0.020	0.012
1985–87	0.043	0.017	0.016	0.009
1987–89	0.035	0.014	0.011	0.010
1989–91	0.054	0.017	0.022	0.015
1991–93	0.055	0.016	0.021	0.017
1993–95	0.059	0.017	0.019	0.022
Sales and Administration				
1981–83	0.085	0.041	0.031	0.013
1983–85	0.067	0.033	0.022	0.012
1985–87	0.075	0.042	0.020	0.012
1987–89	0.069	0.038	0.018	0.013
1989–91	0.092	0.045	0.029	0.018
1991–93	0.091	0.038	0.030	0.024
1993–95	0.093	0.039	0.028	0.027
Service				
1981–83	0.059	0.030	0.023	0.006
1983–85	0.057	0.029	0.022	0.007
1985–87	0.056	0.033	0.018	0.005
1987–89	0.048	0.030	0.013	0.005
1989–91	0.068	0.037	0.024	0.006
1991–93	0.065	0.031	0.024	0.010
1993–95	0.073	0.034	0.028	0.011
Craftsworkers, Operatives, and Laborers				
1981–83	0.212	0.077	0.121	0.015
1983–85	0.168	0.075	0.080	0.013
1985–87	0.128	0.060	0.058	0.010
1987–89	0.111	0.051	0.051	0.009
1989–91	0.173	0.068	0.093	0.012
1991–93	0.137	0.052	0.070	0.015
1993–95	0.135	0.042	0.076	0.017

Source: Author's calculations.

Table A-6. Three-Year Rate of Job Loss by Reason, 1981–95 (Numbers for figure 6)

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*
Manufacturing				
1981–83	0.210	0.081	0.109	0.020
1983–85	0.174	0.079	0.075	0.019
1985–87	0.122	0.061	0.048	0.013
1987–89	0.107	0.053	0.041	0.014
1989–91	0.156	0.066	0.073	0.018
1991–93	0.138	0.057	0.056	0.025
1993–95	0.118	0.046	0.047	0.025
Transportation, Communication, Public Utilities				
1981–83	0.099	0.041	0.045	0.013
1983–85	0.092	0.046	0.035	0.011
1985–87	0.060	0.035	0.018	0.007
1987–89	0.051	0.031	0.016	0.005
1989–91	0.074	0.044	0.022	0.008
1991–93	0.074	0.036	0.024	0.014
1993–95	0.081	0.041	0.029	0.010
Trade				
1981–83	0.101	0.051	0.041	0.009
1983–85	0.077	0.042	0.025	0.010
1985–87	0.101	0.066	0.023	0.012
1987–89	0.092	0.061	0.021	0.010
1989–91	0.115	0.065	0.038	0.012
1991–93	0.112	0.057	0.035	0.020
1993–95	0.110	0.053	0.038	0.020

(*continued*)

Table A-6 *(continued)*

Year	*Total*	*Plant closing*	*Slack work*	*Position abolished*
Finance, Insurance, Real Estate				
1981–83	0.040	0.023	0.012	0.006
1983–85	0.033	0.020	0.010	0.003
1985–87	0.058	0.034	0.017	0.008
1987–89	0.058	0.029	0.017	0.012
1989–91	0.091	0.046	0.028	0.017
1991–93	0.091	0.040	0.024	0.028
1993–95	0.095	0.042	0.031	0.021
Nonprofessional Services				
1981–83	0.127	0.059	0.054	0.014
1983–85	0.113	0.057	0.042	0.014
1985–87	0.087	0.044	0.033	0.010
1987–89	0.077	0.039	0.025	0.013
1989–91	0.127	0.048	0.062	0.018
1991–93	0.107	0.037	0.045	0.025
1993–95	0.108	0.041	0.047	0.020
Professional Services				
1981–83	0.026	0.011	0.010	0.005
1983–85	0.023	0.010	0.008	0.004
1985–87	0.028	0.011	0.010	0.008
1987–89	0.024	0.010	0.007	0.007
1989–91	0.038	0.013	0.013	0.012
1991–93	0.039	0.012	0.014	0.013
1993–95	0.046	0.014	0.014	0.018

Source: Author's calculations.

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Comments

Comment by John Haltiwanger: Eyewitnesses to the same event often have a very different view on what, when, how, and why the event happened. The paper by Hank Farber presents an excellent analysis based on some of the key eyewitnesses to the continuing job loss in the U.S. economy; namely, he uses the Displaced Worker Survey (DWS) supplement to the CPS to obtain the perspective of the worker. In recent work several researchers, including myself, have been examining the process of job destruction from the perspective of the employer, using longitudinal establishment-level data. Much of my comment is aimed at trying to reconcile some of what I think we know about this phenomenon from the employer side with the information that Farber is providing from the employee side.

Although the job-loss terminology that Farber uses and job destruction sound similar, they are distinct concepts. The job-loss concept is based on the number of workers who have "lost" at least one job in the relevant time period. Job loss is defined as involuntary separation based on operating decisions of the employer. This number is converted to a rate by dividing it by the total number of workers at risk (at the survey date). Multiple job losers are not double counted. Thus, the Farber job-loss rate might be better described as a worker displacement rate. According to the survey question, a worker is displaced if the job loss is due to a plant closing, the employer going out of business, a layoff without recall, or some similar reason.

Conversely, the job destruction rate is a measure of the number of jobs that have been eliminated in the relevant time period. Job elimination in practice is measured by looking at changes in the total number of employees at an establishment because the relevant datasets usually have only limited information about restructuring within establishments.

Farber is able to construct job-loss rates over a three-year horizon and finds that the average job-loss rate is approximately 12 percent. Interpreted literally, this means that 12 percent of the work force experiences at least one separation over a three-year horizon that is classified as a displacement. In contrast, the annual rate of job destruction in the U.S. economy is approximately 10 percent in manufacturing industries and slightly higher in nonmanufacturing industries.[1] These figures indicate that at least one in ten jobs is destroyed in a typical year. It is not quite appropriate to simply cumulate the annual job destruction rate to generate a three-year job destruction rate because some fraction of the annual job destruction is reversed (although in the current context such reversals may not be relevant because they may occur too late for affected workers to be recalled). Davis, Haltiwanger, and Schuh calculate that roughly 74 percent of job destruction persists for more than two years.[2] The job destruction rate for U.S. manufacturing over a five-year horizon calculated by Baldwin, Dunne, and Haltiwanger is approximately 26 percent.[3] Putting these figures together and taking into account that job destruction rates are higher for nonmanufacturing suggests that the three-year job destruction rate likely exceeds 20 percent—a rate that is substantially greater than the corresponding three-year job-loss rate.

What factors are responsible for this large difference in magnitude? First, as noted, the job-loss rate does not permit workers who experience more than one job loss during a three-year horizon to be counted multiple times. In contrast, a worker who moves from one declining establishment to another could be counted multiple times in the cumulative job destruction figure (even if all of the job destruction is permanent). Second, a major difficulty in interpreting the displacement measure is whether all workers who experience an employer-initiated separation consider themselves displaced and whether changes in the

1. See, for example, Davis, Haltiwanger, and Schuh (1996) and Anderson and Meyer (1994).
2. Davis, Haltiwanger, and Schuh (1996).
3. As calculated by Baldwin, Dunne, and Haltiwanger (1995).

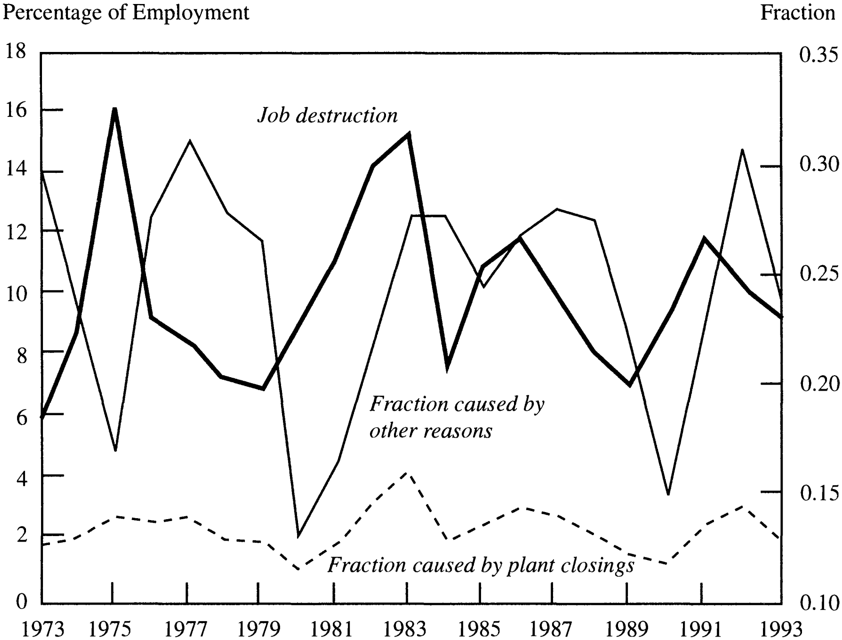
questionnaire over time (see accompanying comments by Katharine Abraham) yield spurious changes in the pace of measured job loss. Third, and perhaps most important, establishments accomplish job destruction by a variety of means. The introduction of the paper opens with news reports of massive corporate downsizing. In practice, companies accomplish much of this downsizing through attrition and accompanying hiring freezes. Of course, the role of attrition depends critically on the concentration of the employment reduction over spatial and time dimensions. An establishment that shuts down or contracts sharply and quickly will not be able to use normal attrition, whereas an establishment that gradually downsizes can potentially use attrition to accomplish the downsizing.

The spatial and time concentration of contractions varies over time and across employers of different characteristics. For example, the five-year job destruction rate due to plant shutdowns varies from 5.6 percent in the petroleum industry (standard industrial classification 29) to 22.2 percent in the apparel industry (SIC 23).[4] Moreover, as figure 1 shows, job destruction due to plant shutdowns varies considerably over the cycle. Figure 1 shows that overall job destruction and job destruction due to plant closings in U.S. manufacturing are countercyclical. Interestingly, the share of overall destruction caused by plant closings usually rises during recessions (the mid-1970s are an exception) but tends to stay high well into a recovery and only falls at the final stages of an expansion. For present purposes, the implication is that the role of plant closings in overall destruction varies considerably over time, and this variation influences the measured displacement associated with the destruction.

To shed further light on the sources of the differences between job destruction and job-loss rates, it is instructive to take advantage of the detailed tables in the paper on the rates of job loss by industry and by reason. Figure 2 provides a comparison of the time series fluctuations in the job-loss and job destruction rates. The job-loss rates depicted are the overall rate and the rate for manufacturing. Note that the job-loss rates by industry do not include the important "other" category. Still, one can see that the job-loss rate for manufacturing tends to exceed that for the whole economy. The job destruction rate depicted is the simple

4. Baldwin, Dunne, and Haltiwanger (1995).

Figure 1. Annual Job Destruction Rates for U.S. Manufacturing, 1973–93



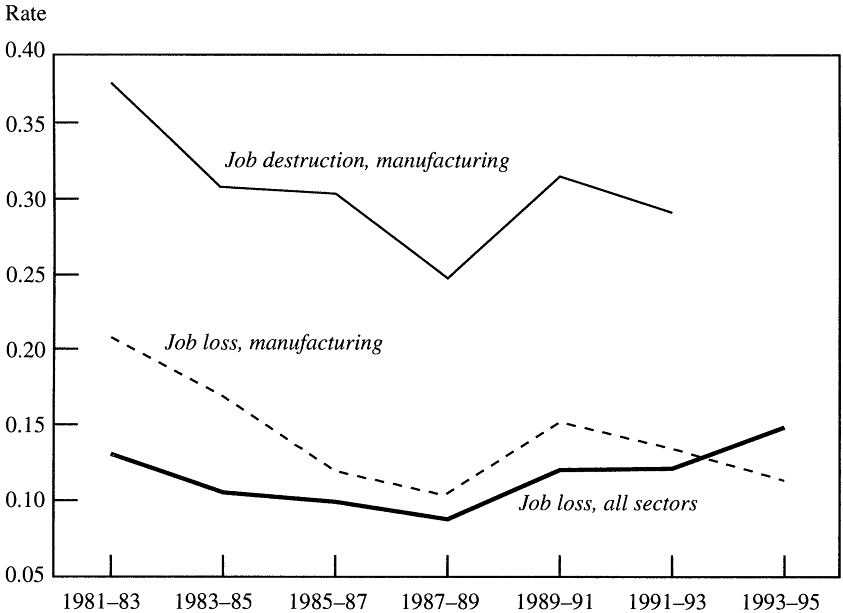
Source: Author's calculations extending the work of Davis, Haltiwanger, and Schuh (1996).

cumulative three-year rate of destruction for U.S. manufacturing, where the years are matched up with the appropriate three-year horizons used for the job-loss rates (note that the destruction rates terminate in 1993).[5] The much larger magnitude for job destruction is evident, but interestingly the time series fluctuations in the manufacturing job destruction and job-loss rates exhibit quite similar patterns.

Figure 3 compares job-loss job destruction rates due to plant closings. The rates are relatively close for manufacturing; however, the sharp divergence in the time series patterns is somewhat troubling. In principle, these rates for manufacturing should be directly comparable—a plant that shuts down should yield measured job destruction that equals measured job loss because all affected workers would presumably report themselves as displaced. Of course, sampling error, differ-

5. See Davis, Haltiwanger, and Schuh (1996).

Figure 2. Comparison of Job-Loss Rates and Three-Year Cumulative Job Destruction Rate



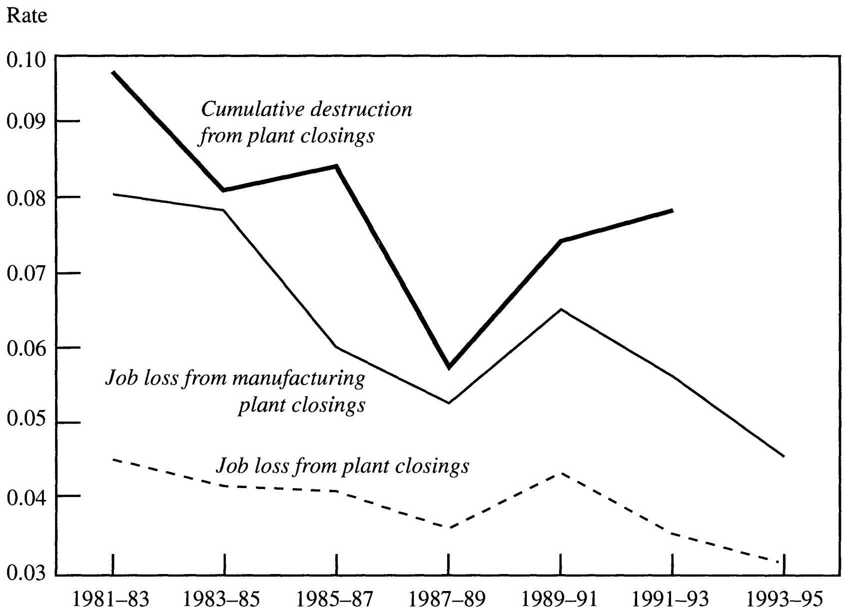
Source: Author's calculations extending the work of Davis, Haltiwanger, and Schuh (1996).

ences in timing, classification error, and other measurement problems can contribute to the observed differences.

The general point is that many factors (such as measurement problems; the effect of the mix of attrition, plant closings, and the like on job destruction; the role of multiple job losers; and the stage of the business cycle) influence the relationship between job destruction and job-loss rates. These figures suggest that these factors matter—that is, the magnitudes differ substantially and the time series fluctuations exhibit different patterns. Unfortunately, at this point, further study is required to understand the precise sources of these differences. Understanding these differences is critical for our understanding of the connection between downsizing and the effect of this downsizing on workers.

The second half of the paper focuses on the earnings losses of those workers who experience displacement. The analysis is cleverly and

Figure 3. Comparison of Job Loss and Destruction from Plant Closings



Source: Author's calculations extending the work of Davis, Haltiwanger, and Schuh (1996).

carefully done with attention to separating out the effects of employment probabilities, part-time probabilities, and earnings losses for full-time workers. Although much is learned from this analysis, one important feature from the perspective of the worker is missing. As some recent work emphasizes, employment separations beget further separations.[6] Thus, a full understanding of the earnings losses requires characterizing the chain of separations that occur following an initial displacement.

To put the losses for workers into perspective, it is useful to consider the losses and gains to the firms. An important recent result from a study of longitudinal establishment data is that the reallocation of workers and other inputs across establishments is a critical contributing factor to overall productivity growth. I have estimated that roughly half of the increase in total factor productivity for the average U.S. manufacturing industry during the 1980s was caused by the reallocation of

6. Hall (1995); Schoeni and Dardia (1996).

output from less productive to more productive establishments.[7] In this regard, it is not primarily the downsizing plants that exhibit productivity gains—indeed, there is a strong positive covariance between changes in output shares and changes in plant-level productivity.

Bringing together the perspectives of the firm and the workers highlights one of the fundamental tensions that characterize market-based economies. On the one hand, large continuing reallocation of outputs and inputs across production sites is a critical component of productivity growth. The allocation of resources to their highest valued use is apparently a noisy and complex process with widely varying rates of success and failure across individual producers. On the other hand, the continuing reallocation yields a nontrivial rate of worker displacement that is often accompanied by substantial earnings losses. This inherent tension makes clear the fundamental importance of considering both the workers' and the firm's perspective on reallocation for our understanding growth and welfare. Unfortunately, relatively little theoretical or empirical work has been done that formally brings these perspectives together. A key part of the problem empirically is the lack of appropriate data. Ideally, data are needed that track the interaction of the movement of workers; the movement of jobs; the impact on the workers of these dynamics in terms of outcomes such as job loss, earnings, and unemployment; and the impact on firms in terms of outcomes such as productivity, output, and employment growth. As should be clear from the preceding comments, the data currently permit researchers to make only crude comparisons across worker-based and employer-based studies. The core importance of the underlying issues suggests that the development of the requisite integrated data that link worker and firm behavior should be high priority.

Comment by Katharine G. Abraham: Henry Farber's paper provides a comprehensive analysis of data from the first seven Displaced Worker supplements to the Current Population Survey (CPS), conducted in even-numbered years beginning in 1984. He examines both the pattern of job displacement and the consequences of displacement for affected workers, with an emphasis in both cases on changes that have occurred

7. Haltiwanger (forthcoming), extending the work of Baily, Hulten, and Campbell (1992) and Olley and Pakes (1997).

over time. The paper is, in most respects, unusually careful in its treatment of the displaced worker data. A fairly obvious difference between the earlier and the later Displaced Worker supplements, for example, is the use of a five-year versus a three-year reference period. On the surface, it might appear that one would need only to exclude reports of displacement occurring more than three years before the interview date from the earlier Displaced Worker files to make those data comparable to the later surveys' data. Farber correctly recognizes, however, that if respondents are reporting for the *longest* job from which they were displaced during the reference interval rather than the *most recent* job, there may be a more subtle comparability problem. He then develops a rather clever approach to addressing that problem empirically. In contrast to most earlier papers that have sought to assess the consequences of displacement for workers' earnings, Farber also takes seriously the task of constructing an appropriate comparison group.

The data that Farber analyzes span the period from 1981 through 1995. I suspect that most students of the economic history of this period would have expected a smaller 1993–95 job displacement rate than that observed for the 1981–83 period: The period from the beginning of 1993 through the end of 1995 was one of steady employment growth and modest unemployment, whereas the period from the beginning of 1981 through the end of 1983 included an interval of sharp employment decline and much higher average unemployment.[8] In fact, however, Farber reports a 1993–95 displacement rate that is larger than that for 1981–83. For many readers, this may be the paper's most surprising finding.

Part of a discussant's obligation, of course, is to raise questions. I do have some questions about the present paper, and in particular about the finding just described. A relatively large share of 1993–95 displacement, as captured by the 1996 Displaced Worker supplement, is accounted for by individuals in an "other" category that Farber includes

8. Payroll employment rose by 8.1 percent between December 1992 and December 1995; in contrast, it was only 1.5 percent higher in December 1983 than it had been in December 1980, and actually fell by 3.0 percent between July 1981 and November 1982. Unemployment averaged 6.2 percent during the 1993–95 period, compared with 9.0 percent during the 1981–83 period. (Data are from the Bureau of Labor Statistics website. Employment data: http:/stats.bls.gov/cgi-bin/dsrv; unemployment data: http:/ stats.bls.gov/cgi-bin/srgate.)

among the displaced; this ''other'' category also appears to be somewhat larger in the 1991–93 data drawn from the 1994 Displaced Worker supplement than in the data for earlier years. With the ''other'' group excluded, however, displacement in the 1993–95 period is markedly reduced. Clearly, understanding why the size of this ''other'' group has grown is important to understanding Farber's findings.

My own sense is that changes in the questions asked as part of the Displaced Worker Survey are an important part of the explanation for the growth in this ''other'' category. The first question asked, the first of two screening questions designed to identify those individuals of whom additional questions should be asked, was unchanged (except for the reference time period) from the first supplement, conducted in 1984, through the fifth supplement, conducted in 1992. That question was as follows: ''*In the past 5 years, that is, since January 19xx, has . . . lost or left a job because of a plant closing, an employer going out of business, a layoff from which . . . was not recalled, or other similar reasons*?''

As noted in Farber's paper, the reference period for which displacement events were to be reported was reduced from five years to three years, effective with the sixth supplement conducted in 1994. Both in 1994 and then again in 1996, however, there also were other changes in the precise wording of the first supplement question that could have prompted more people to respond affirmatively. The 1994 question was as follows: ''*During the last 3 calendar years, that is, January 1991 through December 1993, did (name/you) lose or leave a job because a plant or company closed or moved, (your/his/her) position or shift was abolished, insufficient work, or another similar reason*?'' The most recent version of the question, asked on the 1996 supplement, was as follows: ''*During the last 3 calendar years, that is, January 1993 through December 1995, did (name/you) lose a job, or leave one because: (your/his/her) plant or company closed or moved, (your/his/her) position or shift was abolished, insufficient work, or another similar reason*?''

Two aspects of the changes in question wording might have contributed to producing a larger number of positive responses than were received in response to the pre-1994 questions. First, in contrast to the earlier questions, both the 1994 and the 1996 question included ''insufficient work'' in the list of things specifically mentioned as possible reasons why an individual might have lost or left a job. It may not have been clear to respondents what ''insufficient work'' means. This term, for example, might have led some individuals who had chosen to leave jobs they did not find sufficiently challenging to answer ''yes'' to these new questions, although further probing would have revealed that they were not truly displaced. Second, rather than asking whether an individual ''lost or left a job,'' the 1996 question more clearly identifies both losing a job and leaving a job as potentially relevant events, asking ''did (name/you) lose a job, or leave one'' for any of the listed reasons or similar reasons. We know that even small changes in the way that questions are worded can make an important difference to the answers received, and the 1996 question may have identified a larger number of people who left their job in anticipation of the job ending.

The expansion in the size of the group responding affirmatively to the initial screening question on the supplement most likely has had little effect on the displaced worker counts published by the Bureau of Labor Statistics (BLS). That is because the BLS counts as displaced only those individuals who, in response to a second question, give certain specific reasons for why they had left the job: The plant closed or moved, work was slack, or position or shift was abolished.[9] In contrast, Farber treats everyone who responded affirmatively to the initial screening question as displaced. In addition to those counted as displaced by the BLS, these include workers reporting that they had left a seasonal job that ended; those whose self-operated business had failed; and a residual group giving some other answer. Tabulations of the displaced worker data indicate that workers whose seasonal job or self-employment ended account for a small and relatively stable share of the total group Farber counts as displaced. In contrast, the remainder of the ''other'' category has become much more important, rising from 13.5 percent of those counted as displaced in 1989–91 to 18.6 percent in 1991–93 and to 29.9 percent in 1993–95.

Who are those in this residual category? Should they be counted as displaced? I have a temporary advantage in trying to answer these questions, insofar as I recently have obtained access to preliminary data

9. In most BLS publications on the subject of worker displacement, the displaced worker population also is restricted to those reporting three or more years of tenure on the job they left, although data without this tenure restriction also are routinely made available.

from a debriefing of displaced worker supplement respondents conducted in February 1996. These data were derived from questions asked of members of the CPS outgoing rotation groups. The tabulations are based on unweighted debriefing survey responses.

Among the 501 respondents from the outgoing rotation groups assigned to the "other" category, 452 were in the universe eligible to be asked the debriefing questions about why they lost, left, or retired from the job. Nearly two-thirds gave reasons that were assigned prespecified codes by the interviewer. These included codes for several displacement reasons, as well as for a variety of personal reasons.[10] More than a third of the debriefing supplement respondents, however, gave reasons to which the interviewer could not assign one of the prespecified codes. These reasons were recorded verbatim and are available for internal BLS review. A team of BLS staff members has classified these verbatim responses into those that clearly represent displacement reasons, those that might perhaps represent displacement reasons, and those that clearly were nondisplacement reasons. All told, taking both the coded and the verbatim responses into account, between 24 and 31 percent of those assigned to the "other" category in the supplement itself gave displacement reasons for departing from their job when asked the debriefing questions. By this criterion, most of those in the "other" category should not be counted as displaced.

There is, unfortunately, no obvious way to construct a "corrected" displacement time series based on the 1996 debriefing survey data. Changes in the opening question on the Displaced Worker supplement likely have affected the characteristics of the pool of persons responding affirmatively to that question, with a corresponding impact on the proportion of those in the "other" category who properly should be counted as displaced. Because Farber's presentation of his findings is so thorough, however, it is easy to determine how those findings would have been affected had he excluded the "other" group (inclusive of

10. Coded displacement reasons included cases in which the person's company or plant had insufficient work, was about to close down, was about to move away, was downsizing or restructuring, was filing for bankruptcy, or suffered a natural disaster, together with cases in which the individual's position or shift was about to be abolished or new technology made the individual's job unnecessary. Coded personal reasons included not liking one's job or boss, child care problems or family obligations, own illness or injury, going back to school, moving away, not enough pay, poor benefits, and too long of a commute.

those whose seasonal job or self-employment ended) from his displaced worker counts.

Recalculating the job displacement rate with the "other" group excluded produces a smaller rate for 1993–95 than for 1981–83. The 1993–95 rate, however, is comparable to that for 1991–93, a period when the economy was much weaker on average, and a good bit higher than that for 1987–89, a period of arguably comparable general economic conditions.[11] This suggests to me that something indeed has changed with respect to the relationship between aggregate labor market conditions and the rate of worker displacement. Importantly, Farber's findings regarding changes in the pattern of displacement over time also are generally robust to redefining displacement to exclude those in the "other" category. As was true using Farber's more expansive definition, women's displacement rates move closer to those for men between 1981–83 and 1993–95, and the relative displacement rates of more educated individuals rise over the period. Because the full battery of supplement questions was not asked of those in the "other" category in either 1994 or 1996, Farber's analysis of the industry and occupational pattern of displacement, together with his analysis of the employment and earnings consequences of employment, already exclude that group from the displacement definition.

My main comment on the paper, then, pertains to the proper treatment of Farber's "other" category in the identification of the displaced worker population. While agreeing that some adjustment is appropriate, I also might quibble with some of the specifics of Farber's efforts to use information from the Panel Study of Income Dynamics (PSID) to adjust the displacement rates derived from the earlier Displaced Worker supplements to be more comparable to those derived from the later supplements. For starters, both the PSID population and the PSID displacement concept may be sufficiently different from those in the Displaced Worker supplements that the intertemporal distribution of displacement events for PSID respondents poorly approximates that

11. Nonfarm payroll employment grew by 8.1 percent during the December 1992 to December 1995 period, which is fairly close to the 8.3 percent growth observed during the December 1986 to December 1989 period but nearly three times as large as the 2.8 percent growth registered between December 1990 and December 1993. Unemployment averaged 6.2 percent during the 1993–95 period, which is not too different from the 5.7 percent average for 1987–89 but well below the 7.1 percent average for 1991–93.

underlying the Displaced Worker supplement responses. The validity of the assumption that respondents to the earlier Displaced Worker supplements who had been displaced from multiple jobs over a five-year period always reported for the earliest such incident also might be questioned. If they did not, the upward adjustment Farber applies to the displacement rates for earlier years may be too large. Applying a smaller adjustment, I might note, would have the consequence of making recent displacement rates look even larger relative to those for earlier years, strengthening the conclusion that the labor market has changed. All of this, however, is speculative, and I do not have a better approach to suggest in addressing the comparability problem Farber identifies.

At another point in the paper, in discussing the finding that part-time employment is higher on the current job than on the lost job, Farber comments that some of it might result from ''individual labor supply decisions.'' He does not, however, exploit the information in the basic CPS questionnaire on the voluntary versus involuntary nature of part-time workers' status, which could be helpful in sorting out this question.

These latter thoughts, however, involve relatively minor points concerning what I generally would characterize as a careful and informative paper from which I learned a great deal.

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