Training Programs for Displaced Workers: What Do They Accomplish?

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A consensus appears to be building that the extensive structural changes taking place in the U.S. economy warrant the expansion of government programs to assist displaced workers. Training in particular is seen as a vital part of the adjustment process. A 1994 proposal by the Clinton Administration, for example, would have authorized extended unemployment insurance benefits for displaced workers who were making satisfactory progress in a training or education program approved by an authorized state agency (U.S. Congressional Budget Office 1994). A number of bills currently before Congress continue the call for enhanced training opportunities.

Research by economists does confirm that the structure of labor markets is changing. A rising share of unemployment is accounted for by workers who have been permanently laid off. The number of manufacturing positions is shrinking, especially in blue-collar occupations, and real wages are declining for workers with little education.

Although the "problem" is real, findings regarding the appropriate solution are murky. Research on existing training programs—such as courses to provide displaced workers with specific occupational skills or advances in general knowledge—fails to show that they enable workers to achieve higher pay at their new jobs. Less expensive government interventions such as assistance in identifying and applying for job openings may be just as effective as training, or perhaps even more effective. Thus, those who argue that greater investments in training would have beneficial effects must first confront the issue of why past investments apparently have been unsuccessful.

This article provides further analysis of the effects of training programs for displaced workers. It begins by reviewing the available research on earnings. A recurring issue has been whether assistance program participants initially have better or worse job market qualifications on average than other displaced workers. If so, observed earnings outcomes may reflect underlying differences between participants and

nonparticipants rather than indicating the effectiveness of the programs. This problem is called sample selection bias. To mitigate the possibility that sample selection could bias the findings, a series of research experiments have assigned laid-off workers randomly to different programs. Additional research has used econometric techniques, with controls for worker characteristics, to evaluate existing, nonexperimental programs. The article points out some limitations of both types of research with respect to evaluating how training programs affect reemployment pay. It goes on to argue that occupational changes by displaced workers may lead to some long-term benefits not captured in the studies to date, and that these occupational changes may be more pronounced for workers who have gone through training programs.

The remainder of the article provides evidence on which types of workers are likely to train, and on whether trainees make bigger or better job changes than non-trainees, using information on a large number of displaced workers from Massachusetts who sought government-provided reemployment assistance in the early 1990s. Training programs are found to have enabled many participants to make changes in their line of work. In addition, some relatively disadvantaged trainees were able to obtain better jobs, in terms of the new occupation's educational requirements and prestige, than they would have obtained without training. The concluding section reflects on these findings.

I. A Review of the Literature

A variety of government programs have offered adjustment assistance to displaced workers over the past two decades. Most offer job search assistance, along with formal training designed to augment or update workers' skills. Economic analyses have identified some beneficial earnings effects from adjustment assistance. However, workers going through training generally are not found to receive higher average pay upon reemployment than those who are otherwise similar but do not undergo training. Table 1 lists key studies and their findings.

The first set of studies reviewed are based on a series of experiments funded by the U.S. Department of Labor and state agencies (Leigh 1990). These experiments have featured one or more groups of workers who participated in the program, as well as a comparison group who did not receive readjustment services.

The Buffalo (New York) experiment was part of the U.S. Department of Labor Dislocated Worker Demonstration Project which examined how to reduce adjustment costs of workers displaced from manufacturing jobs. It encompassed steel and auto workers laid off from nine area plants as well as a more heterogeneous set of workers laid off from 300 area establishments in the early 1980s. Some workers were given access to various services including training; their outcomes were compared to those of others who were not offered services or who declined to accept

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services. For the "target-plant" sample (composed of workers from six of the steel and auto factories), classroom training and on-the-job training were associated with sizable earnings gains, $122 and $64 a week, respectively, compared to the results for the control group.[1] However, these were less than the increase resulting from job search assistance alone ($134), which cost the government only one-quarter as much as training. For the "nontarget-plant" sample (that is, workers from the remaining steel and auto factories and other firms), classroom training yielded an estimated earnings gain of $141 per week. While economically meaningful (and greater than the estimated gain from job search assistance alone, $15), the earnings increase was not statistically significant. On-the-job training produced a large, statistically significant gain of $136.

The Texas Worker Adjustment Demonstration focused on displaced workers who met the usual eligibility standards for federal adjustment assistance under Title III of the federal Jobs Training Partnership Act (JTPA). Workers were assigned at random to three groups: job search assistance only, job search assistance with the possibility of training, and no

[1] The pre-layoff hourly wage for target-plant workers who were offered services was $10.78. Assuming a 40-hour work week, a weekly difference of $122 amounts to 28 percent of pre-layoff pay.

readjustment services. Specific findings on training are limited to the Houston experiment. Houston men who were eligible for training were found to have an average earnings gain of $680 in the year following assignment to the program relative to those who received no adjustment services, but this gain was smaller than for those who received only job search assistance ($860 per year).[2] The researchers attributed the lack of a positive effect from training to the mismatch between the technical and vocational retraining opportunities offered and the needs of the laid-off petrochemical engineers who constituted most of the Houston sample.

Finally, the 1986–87 New Jersey Unemployment Insurance (UI) Reemployment Demonstration offered special adjustment services to UI recipients who were expected to face barriers to reemployment. The participants were assigned at random to three groups (job search assistance only, job search assistance with the possibility of training, and job search assistance with a bonus upon reemployment), and the results were compared to those for the general UI population. In contrast to the other experiments, the training was limited to upgrading workers' existing skills rather than developing entirely new skills. Earnings in the four quarters following the initial UI claim were $936 higher for the group eligible for a reemployment bonus, $608 higher for those receiving job search assistance alone, and only $345 higher for those eligible for training, compared to the control group. Some of these differences could conceivably be attributable to the speed with which workers accepted jobs; that is, workers undergoing training might be expected to delay their job search. However, an additional analysis indicated that workers eligible for training had no earnings difference in the fifth and sixth quarters following unemployment, relative to UI recipients in general. Moreover, the initial hourly wage gain at the new job was greater for participants not eligible for training than those eligible for training.

Unlike experimental studies, which are designed for research purposes and involve random assignment to treatment and control groups, nonexperimental studies focus on displaced worker programs already in operation. They measure the effects of training by controlling for as many other differences as possible between trainees and non-trainees.

[2] The other site, El Paso, involved mostly Hispanic women whose average earnings were considerably lower than those of the Houston men. The El Paso experiment indicated higher earnings gains as a result of the program, but a specific evaluation of training is not available.

Jacobson, LaLonde, and Sullivan (1994) evaluated a 1983–85 program in Allegheny County, Pennsylvania that subsidized more intensive and longer-term classroom training services than are usually available to displaced workers. The study is also unusual in that the researchers had access to detailed information regarding the content of training, as well as to subsequent earnings records for up to eight years following training for workers who remained in Pennsylvania. The authors estimated that trainees earned less than expected (on the basis of their pre-training qualifications) for about two years following their schooling, but that subsequent earnings effects were positive. By the seventh year, men's quarterly earnings increased by between $200 and $400 (5 to 10 percent) per year of training. (Women's earnings rose by a smaller amount.) Most participants trained for less than one year, however, so the total effects were smaller.

Nonexperimental studies focus on displaced worker programs already in operation. They measure the effects of training by controlling for as many other differences as possible between trainees and non-trainees.

Decker and Corson (1995) evaluated the Trade Adjustment Assistance Program, which since the early 1980s has provided Trade Readjustment Allowances (extended UI benefits), job search assistance, and training to workers who lost their jobs as a result of import competition. The authors examined the earnings effects of the program both before and after a 1988 amendment that mandated training for all recipients not receiving a waiver. Earnings of the TRA trainees were compared with those of the TRA recipients who did not train. Twelve quarters after filing for unemployment, the training coefficient was estimated as −416 for the pre-1988 sample and as 152 (but not statistically significantly different from zero) for the post-1988 sample. Refining the estimation to exclude workers still unemployed or in training at the end of the sample period yielded statistically insignificant effects of −206 (for pre-1988) and 353 (for post-1988). The authors concluded that "given this uncertainty

Table 1
Effect of Displaced Worker Training Programs on Earnings

	Sample and Description of Training	Control or Comparison Group	Total Sample Size
Experimental Studies			
Leigh (1990)	1. Buffalo, NY Dislocated Worker Demonstration: 1982–83. Target-plant sample: Male workers laid off from 6 steel and auto plants. Nontarget-plant sample: Male workers laid off from 3 other steel and auto plants, and workers laid off from 300 area establishments. Treatment group eligible for job search assistance and training; 45 percent received training.	Target-plant sample: Random assignment to control group. Also included some workers who refused services. Nontarget-plant sample: Self-selected comparison group of workers who refused or did not apply for services. Both samples also included some workers receiving job search assistance only.	1,518
	2. Texas Worker Adjustment Demonstration 1983–85. Houston: Mostly male professional workers laid off from petrochemical plants eligible for Job Training Partnership Act (JTPA) programs. Treatment group eligible for job search assistance and training; "low" take-up rate for training.	Random assignment to 2 different control groups: job search assistance only and no services.	634
	3. New Jersey Unemployment Insurance project 1986–87. UI claimants with at least 3 years of tenure who did not expect to be recalled. Treatment group eligible for job search assistance and training; 15 percent received training. Limited to upgrading of existing skills.	Random assignment to 2 different control groups: job search assistance only and job search assistance plus reemployment bonus. Results compared to UI recipients not satisfying program criteria.	11,060; compared to general UI population.
Nonexperimental Studies			
Jacobson, LaLonde, and Sullivan (1994)	Displaced Workers Educational Training Program participants in Alleghany County, Pennsylvania, 1983–85, permanently laid off from a job lasting 3 or more years. Focus on participants who completed at least one class at a community college. Detailed information available on training. Additional analysis involving low-tenure workers.	Laid-off DWETP participants who did not complete any classes.	4,273
Decker and Corson (1995)	Trade Readjustment Allowance recipients from 10 states. Divided into 2 subsamples of recipients enrolled in program before and after the 1988 amendment mandating training for most TRA recipients. Focus on participants who received training for new occupations, typically lasting more than one year.	TRA recipients who did not train. Additional comparisons with UI recipients from manufacturing who had exhausted their benefits. All TRA recipients received extended UI benefits.	3,060
Kodrzycki (1996)	Displaced workers who used Economic Dislocation and Worker Assistance Act services in MA 1991–94. Workers participated in job search assistance and in education and training classes (median duration = 4 months).	Displaced workers who used only basic readjustment services.	5,492

about the returns to training, we believe that training participation should be voluntary rather than mandatory for TRA recipients."

Kodrzycki (1996) used administrative records for workers who received help at Massachusetts assistance centers funded under the Economic Dislocation

Table 1 continued
Effect of Displaced Worker Training Programs on Earnings

Earnings Concept	Findings for Training	Conclusions/Recommendations
Average weekly earnings in 6 post-program months.	Average weekly earnings increased by $122 in target sample and $141 in nontarget sample for workers receiving classroom training. On-the-job training: $64 for target, $136 for nontarget. Job search assistance only: $134 for target, $15 for nontarget.	Emphasize job search assistance, which is less expensive than training.
Earnings in 4 quarters after the JTPA assignment.	Annual earnings increased by $680 for those eligible for classroom training and by $860 for those receiving job search assistance only. Mismatch between available classes and training needs. Greater earnings gain in El Paso program (targeted at Hispanic women displaced from light manufacturing), but separate estimates for training not available.	Emphasize job search assistance. Skill training should be offered to fewer, more carefully screened participants and better matched to job interests. Cost effectiveness of targeted training remains an open issue.
Earnings in 4 quarters after initial UI claim. Additional analysis of 5th and 6th quarters. Also post UI hourly wage rate.	Annual earnings increased by $345 for those eligible for classroom training, by $608 for those receiving job search assistance only, and by $936 for those eligible for reemployment bonus. Hourly wage increased by a smaller percentage for those eligible for classroom training than for those who were not.	Longer-run, more intensive services needed for displaced workers facing major structural dislocations.
Quarterly earnings up to 8 years after participating in the program.	Training associated with negative earnings effect for approximately two years. By the 7th year, quarterly earnings per year of training rose to $200 to $400 (5 to 10 percent) for men, $100 to $200 for women. Most participants trained for less than one year.	Overall private returns to training are positive. However, the gains are small compared to the earnings losses associated with displacement. Amount spent on tuition and supplies may reduce the social benefits of the program to zero.
Average earnings in 12 quarters after initial UI claim. Some comparisons exclude workers still in training.	"No strong evidence that training enhanced earnings in quarter 12." Effects of −206 (pre-1988) and 353 (post-1988), not significantly different from zero.	Advocate voluntary participation in training instead of mandatory training for Trade Readjustment Allowance recipients.
Replacement rate = real hourly wage on new job/real hourly wage on old job.	Trainees had hourly earnings replacement rate similar to that of non-trainees. Participation had a statistically insignificant effect of −0.65 percent.	Effectiveness of training may be limited by short duration. Results may also reflect qualifications or job choice differences between trainees and non-trainees.

and Worker Assistance Act (a 1988 amendment to Title III of the JTPA) during the early 1990s. The centers offer job search assistance to all users and often fund enrollment in education and training on a case-by-case basis. The analysis compares the hourly wage replacement rate, equal to the real hourly wage on the

new job relative to that on the former job, for workers attending and not attending training classes. Participation in training was found to have a negative but not statistically significant effect of −0.65 percent. Entering the various types of training separately yielded insignificant positive and negative effects, with the exception of the dummy variable for workers receiving entrepreneurial training, which had a statistically significant effect of −13.55 percent. The author speculated that individuals starting their own business after participating in entrepreneurial training may have received nonpecuniary benefits (such as the pleasure of being their own boss) and that, for some, earnings may have increased substantially after their business became established. More generally, she noted that the small overall wage effect from training programs may reflect their short duration. To the extent returns were negative, this may indicate that training participants were at a greater disadvantage compared with others in the sample in a way that the regressions did not capture.

The generally low (if not negative) earnings effects from training programs are troublesome given the higher costs of training as compared with job search assistance. The expenses of semester-long training are typically in the vicinity of $6,000 per worker, versus overall JTPA Title III expenses per worker of about $1,375.[3] Thus, when Jacobson, LaLonde, and Sullivan compared the long-term benefits of training in the Allegheny County, Pennsylvania program (which are among the larger effects found) against workers' forgone earnings while in training and the public subsidies for tuition and supplies, they concluded that the social returns were near zero.

II. Next Steps for Research

A central question in all the studies is determining how trainees would have fared had they not undergone training. To answer this question, researchers attempt to compare the trainees to otherwise similar persons who did not train. Ideally the studies would control not only for readily observable characteristics such as labor market experience, occupation, gender and the like, but also for comparatively unobservable characteristics such as motivation and talent.

The best way of controlling for these differences is to assign workers at random to "treatment" and "control" groups. However, opportunities for experimentation are inherently limited. Only a small number of projects can be funded, and these usually involve small (if not idiosyncratic) samples. And in fact, none of the experiments reviewed above randomly assigned individuals to *training*; at best, they assigned individuals to a group that received job search assistance plus the *possibility of training*. Thus, even in the context of experimental data, it is difficult to isolate the effects of training, since participants who select (or are selected for) training may have different initial qualifications than participants who turn down the opportunity to train.

Given these limitations, the nonexperimental studies inevitably will continue to play a role in the evaluation of training programs, even though one can never be assured that they deal satisfactorily with sample selection issues.[4] The nonexperimental studies cited above use somewhat different approaches for controlling for "unobservables." Decker and Corson reach qualitatively similar conclusions before and after training became mandatory for most TRA recipients, which provides some indication that selection is not driving their results. Jacobson, LaLonde, and Sullivan include grade point average in classroom training, which may contain information about a worker's pre-training ability and degree of effort during the training process. Kodrzycki includes the worker's score on a reading test administered by the worker assistance center; this is included as an indicator of ability, but not training effort.

In a related study, Bartel (1995) examines the effect of company training programs on the pay of employees. She posits that an employee's relatively unobservable qualities, as evidenced by his or her pay relative to others performing the same job at the company, may influence both whether the employee is selected for training and subsequent growth in pay. In a regression analysis, Bartel finds that "core" training (managerial and leadership) and technical training

[3] The JTPA Title III estimate is from LaLonde (1995). He also estimates that training under the 1960s Manpower Development and Training Programs cost $6,500 when measured in 1994 dollars. This is consistent with informal estimates for the EDWAA programs gathered by Kodrzycki (1995). In the Buffalo and Houston experiments, the costs of providing training and job search assistance were reported to be between 3.3 and 3.8 times those of job search assistance alone. Training costs in the New Jersey experiment were relatively low because the courses were limited in scope.

[4] An influential article by LaLonde (1986) compared the effect of an employment program that was run as a field experiment where disadvantaged job seekers were randomly assigned to treatment and control groups with the estimates that would have been produced by an econometrician. He found that many of the econometric procedures did not yield accurate or precise estimates.

tend to involve employees with high relative pay. By contrast, employee development training (designed to improve communications and time management skills) is likely to be remedial—that is, reserved for employees with low relative pay. Bartel then uses the fitted values from these regressions (which reflect how a worker's ability and effort as well as other, more readily observable characteristics affect the probability of training) in a second-stage regression for subsequent wage growth. Bartel concludes that both remedial and non-remedial training have a significant positive effect on wage growth; worker quality as measured by previous relative pay also has a separate positive impact. The beneficial effect of employee development training is masked when Bartel uses the simpler (one-step) procedure of regressing the growth in pay on participation in training and measures of education and experience; this is because the participants selected for this type of training are of lower quality in terms of "unobservables."

To date, discussions of selection bias have revolved exclusively around whether individuals who train are of the same "quality" as those who do not. A question of considerable importance is whether the two groups make similar occupational choices.

Previous wage could be an additional control variable for analyzing training for displaced workers. In contrast to grades and test scores, it is a more general measure of a worker's performance. Since pay scales vary across companies, previous wage is a more accurate measure of ability when limited to a single company. Including company-specific effects may or may not be possible to implement, depending on the sample. If splitting the sample by company is not possible, previous pay may still provide information, to the degree that other factors that help to determine pay (such as industry, occupation, job tenure, and union status) also enter the regression.

To date, discussions of selection bias have revolved exclusively around whether individuals who train are of the same "quality" as those who do not. A neglected question, but one that may be of considerable importance, is whether the two groups make similar occupational choices. Individuals with comparable aptitude and dedication to working may be paid quite differently. A large body of literature indicates, for example, that displaced workers who change occupation or industry suffer larger wage losses than those who find jobs in their previous occupation and industry. And in particular, Kodrzycki (1996) found that in the wake of recent defense downsizing, former employees of defense contractors faced especially large pay reductions upon reemployment.

None of the studies cited above considers the potential interactions between training and occupational choice. Presumably, displaced workers who decide to make a more substantial job change are more likely to undergo training to prepare for this change. Thus any wage gains resulting from training may be masked by a negative "switching" effect. Furthermore, wage gains may be difficult to detect to the extent they are not reflected within the period of time used for evaluation. It is possible that trainees are more likely to take jobs that offer greater potential for advancement or less likelihood of subsequent layoff. Although most data sets do not offer the possibility of long-term follow-up, research can at least examine the extent to which the new jobs appear to place workers on a promising job ladder.

III. The Massachusetts Data Set

The data set used in this paper is described at length in Kodrzycki (1996). In brief, the sample consists of workers laid off between 1991 and 1994 who used assistance centers in Massachusetts operating under the provisions of the EDWAA amendment to Title III of the Job Training and Partnership Act. These centers offer basic readjustment services such as counseling and job market information to all users. In addition, some displaced workers received funding to enroll in education and/or job training programs at local colleges, universities, and specialized training facilities. The training programs had to fit into the worker's job search strategy (although in fact some workers ended up in jobs unrelated to the training programs), and the allocation of funds was subject to budgetary guidelines.[5] For the purposes of this paper,

[5] It is not clear to what extent center personnel constrained worker decisions on whether or not to train.

the sample is restricted to workers who had previously been employed full-time and who became reemployed at a new job. That is, workers who were recalled to their previous job or who were not employed at the end of the observation period are excluded.

IV. Worker Characteristics, Training, and Job Changes

This section presents information on which types of workers were more or less likely to receive training, and on how job changes varied by participation in training programs. The subsequent section tests hypotheses more rigorously through use of regression analysis.

Who Trained?

Of those who had been employed full time in their previous job and who found a new job, about 46 percent went through training (Table 2). As indicated by the relatively high concentrations in the column headed "job training," most trainees took vocational courses. These consisted of preparation for a specific occupation or to become an independent entrepreneur, generally in a classroom setting.[6] Only about 5 percent of the sample took general education classes and 3 percent combined job training and general education. General education includes adult basic education classes to improve reading, writing, mathematics, and computer literacy skills; English as a second language; and GED classes, to obtain a high-school equivalency diploma. The "both" category includes individuals who took separate job training and education classes, as well as some who participated in an integrated program of job training and education.

Workers who participated in training programs clearly were not a random sample of those who received EDWAA assistance. Furthermore, participants in job training and general education programs differed from each other. Women and nonwhites were far more likely to receive training than men and whites. Nonwhites were less likely than whites to receive job training, however. In general, the characteristics of those who received both job training and general education were more similar to the "education only" than the "job training only" group.

[6] Very few displaced workers in this sample received on-the-job training.

Overall training propensities varied inversely with education, although a closer inspection of the data reveals differences across different types of training. A relatively high proportion of high school dropouts took general education classes; relatively few had job training. High school graduates who had not completed college had the highest participation rates in job training.

Training propensities did not vary much between the ages of 25 and 54. However, workers younger than 25 had above-average and workers 55 and older below-average training rates. Those who had been at their previous job for no more than five years were more likely to receive training—especially job training—than workers with a longer work history at their last employer.

Comparatively few high school dropouts and older workers trained for a specific occupation. Also, job training rates were low among former defense workers, despite the fact that opportunities in that industry were declining.

Training patterns also varied by occupation and industry. Former sales workers were more likely to receive job training than workers in other occupations. Production, service, and "other" workers had high rates of participation (and professional, technical, and managerial workers low rates of participation) in education courses. Workers from defense-related and computer manufacturing firms had far lower training rates than workers from other, typically lower-paying, manufacturing industries, and workers from professional services industries trained less than those from other services industries. The highest overall training percentage was for finance, insurance, and real estate employees.

To summarize, in some respects, training was aimed at displaced workers who are usually thought of as less advantaged—those with little job experience or education, those from less prestigious occupations or industries, as well as women and minorities. But in

other respects, the results indicate that disadvantaged groups were less likely to select or be selected into job training (as opposed to general education). Comparatively few high school dropouts and older workers trained for a specific occupation. Also, job training rates were low among former defense workers, despite the fact that opportunities in that industry were declining.

Table 2
Percent of the Sample Receiving Training

	Job Training or General Education	Job Training	General Education	Both Job Training and General Education
Full Sample	45.7	37.4	5.1	3.2
Men	39.5	32.6	4.5	2.3
Women	53.2	43.1	5.9	4.2
Whites	44.6	38.4	3.6	2.6
Nonwhites	54.2	29.2	17.1	8.0
High School Dropouts	55.9	13.8	29.5	12.6
High School Graduates	47.0	41.3	2.7	2.9
Some College	44.3	42.0	1.1	1.3
College Graduates	39.4	38.5	.5	.4
More than College	32.7	30.2	2.2	.4
Age (years)				
Less than 25	51.4	39.3	6.2	5.9
25–34	47.1	38.5	5.2	3.3
35–44	45.0	37.3	4.6	3.1
45–54	46.3	37.9	5.7	2.7
55 and over	40.0	32.2	4.8	2.8
Job Tenure (years)				
0–5	49.6	41.3	5.4	2.8
6–10	43.8	34.7	6.2	2.9
11–19	41.6	35.8	3.7	2.1
20 and over	40.5	30.1	4.7	5.7
Previous Occupation				
Professional, Technical and Managerial	40.2	37.4	1.9	.8
Sales	51.5	45.8	2.7	3.1
Production	45.9	29.9	10.5	5.5
Service	45.2	30.5	8.9	5.7
Other	43.9	30.0	8.3	5.7
Not Known	49.8	44.5	3.0	2.4
Previous Industry				
Manufacturing	42.5	31.8	6.7	3.9
Defense-Related	38.1	33.3	3.5	1.3
Computers	27.7	24.1	2.9	.7
Other	47.0	32.9	8.6	5.5
Construction	49.5	38.0	8.7	2.7
Transportation, Communication, and Utilities	45.6	42.6	2.0	9.8
Trade	49.1	44.0	3.0	2.1
Finance, Insurance, and Real Estate	52.8	48.8	.5	3.6
Professional Services	38.5	33.6	2.6	2.3
Other Services	52.3	46.3	4.4	1.7
Government	46.8	40.5	3.6	2.8
Other and Not Known	61.0	53.7	4.8	2.5

Note: Sample includes full-time workers reemployed in a new job.

Big Leaps or Little Leaps?

The next four tables examine job changes. Over three-fifths of all workers in the sample changed industries, using the fairly broad industry definitions in Table 3.[7] The industry-switching rate was much higher among trainees, 73 percent, than among those who did not train, 58 percent. This pattern of a substantially higher rate of industry change among trainees holds up across workers from all industries except defense-related manufacturing. Almost all the former defense workers moved into non-defense work, regardless of whether they trained or not. Workers undergoing job training were slightly more likely to switch industries than those who took only general education classes.

[7] The percent of manufacturing workers switching industry reflects shifts between subcategories of manufacturing (defense-related, computers, and other) as well as shifts out of manufacturing.

Table 3
Percent of the Sample Who Changed Industry, by Former Industry and Training Status

	All Workers	No Training	Job Training and/or General Education	Job Training	General Education	Both Job Training and General Education
Full Sample	62.3	57.5	73.4	76.3	43.3	74.0
Previous Industry						
Manufacturing	64.1	60.0	74.9	79.9	36.9	75.3
Defense-Related	95.4	94.5	98.1	98.0	100.0[a]	100.0[a]
Computers	82.2	79.5	95.6	96.3	85.7[a]	100.0[a]
Other	48.9	41.6	65.5	70.9	27.4	72.7
Construction	46.0	37.5	60.9	82.4	0.0[a]	0.0[a]
Transportation, Communication, and Utilities	77.3	76.3	80.0	81.8	50.0[a]	n.a.
Trade	66.3	59.8	77.8	77.9	62.5	92.9
Finance, Insurance, and Real Estate	69.9	63.9	78.6	77.6	100.0[a]	88.9[a]
Professional Services	35.8	31.3	48.6	50.3	50.0	28.6
Other Services	71.1	67.2	77.5	76.8	77.8[a]	100.0[a]
Government	66.4	60.9	75.7	76.1	55.6[a]	90.0

[a]Fewer than 10 observations.
n.a. = Not available—no observations.
Note: Sample includes full-time workers reemployed in a new job. Percentages exclude workers whose former or new occupational categories were unknown.

Table 4 looks at broad occupational categories. A higher share of trainees than non-trainees changed occupations—48 percent compared to 33 percent. Those taking general education classes were most likely to switch.

These broad measures, which are typical of those used in analyses of displaced workers, treat occupational change as a dichotomous variable, and they involve rather arbitrary definitions of what constitutes a change. In actuality, workers who stay in the same broad occupational category may experience a considerable change in job content. For example, a mail room supervisor who becomes a secretary would tend to use a different set of skills even though he or she remains a "clerical and services" worker. And among those who change occupations, some job changes are more dramatic than others. For example, a customer service representative (a clerical and sales occupation) may make a larger job change in becoming a fumigator (a service occupation) than in taking an office position classified as "professional."

To provide a richer analysis of occupational change, Table 5 uses job ratings developed by the U.S. Department of Labor for approximately 500 occupations.[8] The measures labeled "job functions" indicate the levels of complexity in relation to data, people, and things. For example, the most complex jobs in terms of "data" (rating = 0) involve "synthesizing: integrating analyses of data to discover facts and/or develop knowledge concepts or interpretations." The least complex data jobs (rating = 6) involve "comparing: judging the readily observable functional, structural, or compositional characteristics (whether similar to or divergent from obvious standards)."[9] "Job traits" refers to the physical aspects of the work. While the job functions categories are scored on a reverse scale (with the highest ratings assigned to the least challenging jobs), job traits are scored on an ascending scale. Job traits include the strength required to perform the work (ranging from 1 = sedentary to 5 = very heavy), as well as a composite measure of other physical demands, primarily motor and visual. The

[8] The appendix describes how the displaced workers in the sample were matched up to these job characteristics and provides ratings for selected occupations.

[9] The most complex "people" jobs involve mentoring; the least complex involve "taking instructions—helping." The top level "things" category requires "adjusting machines or equipment . . . to prepare them to perform their functions, change their performance, or restore their proper functioning if they break down." The bottom level rating is reserved for handling objects or materials.

Table 4
Percent of the Sample Who Changed Occupational Category, by Former Occupation and Training Status

	All Workers	No Training	Job Training and/or General Education	Job Training	General Education	Both Job Training and General Education
Full Sample	38.2	32.6	47.5	47.0	39.0	61.9
Professional, Technical and Managerial	34.4	30.7	41.6	40.8	63.0	46.7
Clerical and Sales	33.8	31.1	37.3	36.5	42.9	48.6
Production	40.5	31.5	55.5	58.4	27.3	65.3
Service	52.1	41.5	67.9	71.4	42.1	78.6
Other	61.2	55.0	72.7	72.7	72.7	72.7

Note: Sample includes full-time workers reemployed in a new job. Percentages exclude workers whose former or new occupational categories were unknown.

Table 5
Occupational Content, by Training Status

				Types of Training		
	Total	No Training	Training	Job Training	General Education	Both Job Training and General Education
Characteristics of Former Occupation						
Job Functions						
Data (6–0 scale)	2.98*	2.91	3.07*	2.89	3.87*	3.74*
People (8–0 scale)	6.26	6.23	6.30*	6.19	6.85*	6.69*
Things (7–0 scale)	4.29	4.31	4.28	4.26	4.35	4.30
Job Traits						
Strength (1–5 scale)	2.19	2.20	2.18	2.09*	2.58*	2.44*
Physical Demands (0–4 scale)	1.73	1.72	1.73	1.68*	1.99*	1.89*
Environmental Hazards (0–6 scale)	.48	.48	.47	.43*	.72*	.57*
Size of Change from Old Job to New Job (Absolute Magnitudes)						
Job Functions						
Data	1.05*	1.01	1.12*	1.10*	1.20*	1.33*
People	.97	.94	1.02*	1.03*	.87*	1.01*
Things	1.60*	1.50	1.77*	1.78*	1.63*	1.73*
Job Traits						
Strength	.52*	.48	.58*	.57*	.53*	.69*
Physical Demands	.52*	.50	.55*	.56*	.48	.53*
Environmental Hazards	.39	.38	.42*	.40*	.50*	.55*
Change from Old Job to New Job						
Job Functions (Negative change indicates increased complexity for data/people/things)						
Data	.07	.09	.03*	.02*	.13	−.04*
People	.03	.05	−.0004*	−.003*	.07	−.04*
Things	−.09	−.05	−.14*	−.14*	−.33*	−.05
Job Traits						
Strength	−.03*	−.01	−.08*	−.07*	−.10*	−.10*
Physical Demands	.04	.04	.04	.04	.004*	.01*
Environmental Hazards	−.01	.01	−.03*	−.03*	−.04*	−.02*

*Different from the corresponding measure for no training at 95 percent confidence level.

final job traits concept indicates the physical challenges of the work environment, in terms of temperature variation, noise levels, hazards, and the like.

The first section of Table 5 confirms the impression that trainees and non-trainees have different backgrounds. On average, displaced workers who decided to further their education had held jobs that were less demanding in terms of the "data" and "people" measures but more demanding in terms of physical challenges than those who decided not to train or who had only job training. (The asterisks in Table 5 indicate that a particular value for a category is significantly different from the value for "no training.") Those undergoing job training had the least challenging jobs with respect to strength, physical demands, and environmental hazards.

The next section of the table provides a look at the degree of occupational change by indicating the (absolute) size of the difference in these job functions and traits between the old and new jobs. For all of the indicators and almost all types of training, trainees made bigger job changes than non-trainees. These differences were also statistically significant.

Moving Up or Moving Down?

The next question is whether trainees moved to better or worse occupations. The functions and traits measures indicate something about the nature of job changes, but they are only suggestive of whether new jobs were better or worse. All else equal, jobs that involve more complexity or greater physical challenges should be more highly compensated than others. But, in general, the highest-paying jobs in recent years have involved great complexity in terms of data and people, but little complexity in terms of things and little physical challenge.

Workers who took a job after training were more likely to obtain more complex jobs (or less likely to take less complex jobs) than those who did not train. Trainees tended to take jobs requiring less strength and fewer environmental hazards than the non-trainees.

To provide further evidence on whether training was associated with better or worse shifts, Table 6 shows the U.S. Department of Labor measures of the amounts of general educational development (GED) on a 1 to 6 scale and specific vocational preparation (SVP) on a 1 to 9 scale required of a worker to "acquire the knowledge and abilities necessary for average performance in a particular job-worker situation."[10] In addition, it includes prestige rankings on a 1 to 100 scale. (Educational requirements and prestige were each measured on an ascending scale; that is, 1 represents the lowest value.) In contrast to the other measures, which were developed by professional job evaluators, prestige refers to public opinions as elicited in studies conducted by sociologists. Prestige may reflect a broad set of pecuniary and non-pecuniary attributes of the job. Finally, the table compares median pay in the old and new occupations, as measured by Census data for 1989 for Massachusetts. The information is shown separately by gender, as men's median hourly pay was about four dollars (36 percent) higher than women's.

On average, displaced workers who went through education programs had been employed in occupations that called for less rigorous preparation and were less prestigious than the occupations of those who did not train. The average statistics for the "job training" and "no training" groups are similar. People who attended education programs had been in occupations that paid less than those who received only job training and those who received no job training or education after losing their job.

The direction of occupational movement differed across training categories. Those who did not train tended to move downward in terms of their new job's general educational and specific vocational requirements and prestige. By contrast, job training was associated with positive moves in terms of educational content, with the largest improvement for those who had undergone job training in combination with education. Prestige declined much less for those with training than without, and those having both job training and education showed a gain. The job shifts of those who availed themselves of education programs alone were similar to those in the no training category.

On the whole trainees fared similarly to non-trainees in terms of occupational pay changes. For men who trained, median pay in the new occupations averaged 43 cents per hour less than in their previous occupations—versus a decline of 41 cents for men who did not train. For women, the pay cuts were 33 cents for trainees and 29 cents for non-trainees. The more detailed information indicates no clear pattern across the different training categories.

Thus, the pay data contradict the other findings showing that trainees move to somewhat more attractive occupations, and that trainees fare better than non-trainees. It is not entirely clear why occupational pay provides different results. To some extent, this pattern may reflect wage premia in manufacturing;

[10] Bishop (1996) cautions that these are only general guidelines; more education tends to improve productivity across a broad spectrum of occupations.

Table 6
Occupational Training Time, Prestige, and Wage, by Training Status

	Total	No Training	Training	Types of Training: Job Training	Types of Training: General Education	Types of Training: Both Job Training and General Education
Characteristics of Former Occupation						
Training Time						
General Educational Development (1–6 scale)	3.75*	3.80	3.70*	3.82	3.17*	3.29*
Specific Vocational Preparation (1–9 scale)	5.40*	5.49	5.29*	5.45	4.60*	4.68*
Prestige (1–100 scale)	43.86*	44.55	43.02*	44.33	37.22*	37.86*
Median Hourly Wage, 1989, Dollars						
Men	14.87	14.92	14.80*	15.16*	13.10*	13.27*
Women	10.95	11.21	10.71*	10.98	9.59*	9.69*
Change from Old Job to New Job						
Educational Requirements						
General Educational Development	.01*	−.02	.06*	.06*	.02*	.19*
Specific Vocational Preparation	−.02	−.05	.03*	.02*	−.04	.10*
Prestige	−.88*	−1.35	−.09*	−.18*	−1.27	2.34*
Median Hourly Wage, 1989, Dollars						
Men	−.42	−.41	−.43	−.38	−.82*	−.77*
Women	−.31	−.29	−.33	−.40	−.22	.33*

*Different from the corresponding measure for no training at 95 percent confidence level.

almost 15 percent of former manufacturing workers who switched out of manufacturing had an increase in occupational prestige but a decrease in occupational pay, versus only 9 percent for those who stayed in manufacturing. The combination of higher prestige and lower pay also was much more common for women than men.

To summarize, the available data indicate that, on the whole, trainees were more likely than non-trainees to move to better occupations, as indicated by an increased complexity of work, reduced hazards in the workplace, the educational background required to achieve average performance, and public perception. Those who received job training tended to make greater progress than those who had only academic training. However, there were no clear differences between trainees and non-trainees in terms of occupational pay; the two groups experienced similar declines.

V. Regressions

The remaining tables explore the links between worker attributes and selection into training and between job outcomes and training, adjusting for a range of other variables. Table 7 is based on a multinomial logit model of selection into training, with displaced workers electing job training only, education only, or both. The coefficients indicate the probabilities of selecting each of these options relative to no training.

The regressions confirm some findings in the raw data. Previous education level matters, with those who have finished high school more likely to undertake job training but less likely to take general education courses. Also, women are more disposed to enroll in training classes (especially those with an occupational focus) than men, and nonwhites enroll in general education classes more often than whites. In addition, the regressions indicate that economic conditions matter, regardless of the worker's socioeconomic group. At times of high or rising unemployment rates, workers are more likely to take job training (either alone or in conjunction with education).

The regressions also shed light on the ability and past success of trainees and non-trainees. As indicated by a coefficient on the real wage that is greater than one, job training alone tends to attract more successful workers.[11] By contrast, the less successful workers

[11] Similar findings were obtained using the worker's former real wage relative to others in the same company or in the same occupation.

Table 7
Training Probabilities: Multinomial Logit Results
Coefficients indicate probabilities relative to base category = no training.

	(1) Job Training		(2) General Education		(3) Both	
Independent Variable	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Age (Omitted = Less than 25)						
25–34	1.02	.13	.74	.22	.75	.21
35–44	.97	.12	.78	.23	.86	.24
45–54	1.01	.13	1.05	.32	.67	.20
55 and Over	.83	.12	.49**	.18	.55*	.19
Tenure (Omitted = 5 Years or Less)						
6–10	.89	.06	.93	.19	1.53	.34
11–19	.93	.07	.69	.16	.99	.25
20 or More	.68**	.05	.86	.18	1.92*	.38
Education (Omitted = Less than High School)						
High School	1.75**	.20	.06**	.01	.16**	.03
Some College	1.69**	.20	.04**	.01	.10**	.02
College Degree	1.34*	.18	.02**	.01	.04**	.02
More than College	1.29	.25	.03**	.02	.06**	.07
Reading Test Score	1.01	.01	1.09**	.03	1.06	.04
Demographic Characteristics						
Gender and Marital Status (Omitted = Unmarried Male)						
Married Male	1.03	.07	.99	.19	.98	.23
Married Female	2.07**	.17	1.11	.25	2.80**	.63
Unmarried Female	1.77**	.13	1.33	.27	2.37**	.51
Nonwhite	.99	.09	2.77**	.52	2.52**	.48
Unemployment Rate at Time of Layoff						
County Unemployment Rate	1.95**	.16	1.17	.28	2.67*	.66
12-Month Difference in County UR	1.31**	.07	1.04	.16	1.43	.22
State Minus County Unemployment Rate	1.76**	.15	1.35*	.33	2.24**	.58
12-Month Difference in State Minus County UR	1.27**	.07	1.38	.24	1.14	.20
Former Industry/Occupation						
12-Month Industry Employment Growth Rate	1.03**	.01	1.14**	.05	1.03	.05
Industry Dummies?	yes[a]		yes[b]		yes	
Occupation Dummies?	yes[c]		yes		yes[d]	
Log Real Wage at Time of Layoff	1.28*	.11	.54**	.15	.41**	.12
Year Dummies?	yes[e]		yes		yes	
Pseudo R^2 = .14						
Number of Observations = 8,338						

[a]Omitted = construction. Coefficient for professional services significantly less than 1.
[b]Omitted = construction. Coefficient for finance, insurance, and real estate significantly less than 1.
[c]Omitted = services. Coefficient for sales significantly greater than 1.
[d]Omitted = services. Coefficient for professional, technical, and managerial significantly less than 1.
[e]Omitted = 1991. Coefficients for 1992 and 1993 significantly greater than 1. Coefficient for 1994 significantly less than 1.
*Significantly different from 1 at 5 percent level.
**Significantly different from 1 at 1 percent level.

tend to be drawn into education programs (and education combined with job training).[12]

The next set of regressions examines the magnitude of job changes (Table 8). The dependent variable refers to the degree of change in work functions (PEOPLE, DATA, and THINGS) and work traits (STRENGTH, PHYSDEM, and ENVIRON). It is computed by taking the sum of absolute differences in these six job indicators.[13] The first regression uses

[12] This impression of the education trainees may be too negative, however. Once one controls for their low level of past educational attainment, this group has above-average reading ability, as indicated by the coefficient for reading test score.

[13] The job change variable averages 5.06, with a standard deviation of 3.43.

Table 8
Size of Job Change: Ordinary Least Squares Regression Results[a]

Independent Variable	(1) Results Using Training Dummies		(2) Results Using Fitted Training Probabilities	
	Coefficient	Standard Error	Coefficient	Standard Error
Age (Omitted = Less than 25)				
25–34	.03	.22	.14	.24
35–44	−.09	.22	.004	.24
45–54	−.09	.22	−.01	.24
55 and Over	.07	.25	.26	.27
Tenure (Omitted = 5 Years or Less)				
6–10	−.21	.12	−.18	.13
11–19	.03	.12	.04	.13
20 or More	.14	.13	.29	.17
Education (Omitted = Less than High School)				
High School	.46*	.19	−.10	.49
Some College	.31	.20	−.30	.51
College Degree	.08	.22	−.47	.52
More than College	.18	.31	−.27	.56
Reading Test Score	−.03	.02	−.01	.02
Demographic Characteristics				
Gender and Marital Status (Omitted = Unmarried Male)				
Married Male	.08	.12	.04	.12
Married Female	−.67**	.14	−.96**	.20
Unmarried Female	−.42**	.13	−.67**	.17
Nonwhite	.10	.15	.10	.18
Former Industry/Occupation				
12-Month Industry Employment Growth Rate	−.03	.02	−.04	.02
Industry Dummies?	yes		yes	
Occupation Dummies?	yes[b]		yes[b]	
Log Real Wage at Time of Layoff	−.64**	.15	−.80**	.17
Training				
Job	.62**	.09	2.33**	.82
General Education	−.10	.33	−.80	1.60
Both	.46	.30	−.05	1.46
Year Dummies?	yes[c]		yes	
Constant	6.97**	.46	6.97**	.71
Adjusted R^2	.07		.07	
Number of Observations	6,341		5,987	

[a]Dependent variable indicates the overall change in DATA, PEOPLE, THINGS, STRENGTH, PHYSDEM, and ENVIRON.
[b]Omitted = services. Coefficients for professional, technical, and managerial and sales significantly less than 0.
[c]Omitted = 1991. Coefficient for 1994 significantly less than zero.
*Significantly different from zero at 5 percent level.
**Significantly different from zero at 1 percent level.

dummy variables for the three categories of training, while the second uses fitted training probabilities.[14] The regressions are able to explain relatively little about which kinds of displaced workers make major job changes (the adjusted R^2 is no higher than 0.07).

[14] The fitted values are based on separate logit models for the three types of training, using the same independent variables as in the multinomial logit model presented in Table 7. The standard errors shown in the second set of results in Table 8 (and the equivalent results in Tables 9 and 10) were corrected using the procedure shown as equation 15′ in Murphy and Topel (1985). Arguably, their procedure for the more general case of nonindependent random components (equation 24) might be appropriate. Yet Murphy and Topel's numerical examples indicate little added benefit from using this more complicated error correction technique.

Table 9
Direction of Job Change: Ordinary Least Squares Regression Results

	Results Using Training Dummies					
	(1) Educational Requirements		(2) Prestige		(3) Median Wage	
Independent Variable	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Age (Omitted = Less than 25)						
25–34	−.20	.14	−.29	.79	−.28	.27
35–44	−.35*	.14	−1.69*	.79	−.45	.28
45–54	−.41**	.15	−1.77*	.81	−.33	.28
55 and Over	−.51**	.16	−2.23*	.91	−.33	.31
Tenure (Omitted = 5 Years or Less)						
6–10	−.05	.08	−.59	.44	−.04	.15
11–19	−.02	.08	−.60	.45	.01	.16
20 or More	.28**	.09	1.00*	.48	.30	.17
Education (Omitted = Less than High School)						
High School	.06	.12	.27	.70	−.13	.24
Some College	.33*	.13	2.26**	.74	.39	.25
College Degree	.87**	.15	5.70**	.81	1.38**	.28
More than College	.89**	.21	5.14**	1.14	2.29**	.40
Reading Test Score	.02	.01	.16*	.08	.07**	.03
Demographic Characteristics						
Gender and Marital Status (Omitted = Unmarried Male)						
Married Male	.13	.08	.10	.42	.02	.15
Married Female	.42**	.10	3.02**	.52	.19	.18
Unmarried Female	.24**	.08	2.02**	.46	.22	.16
Nonwhite	−.03	.10	.69	.56	−.13	.19
Former Industry/Occupation						
12-Month Industry Employment Growth Rate	−.02	.01	−.03	.08	−.04	.03
Industry Dummies?	yes[a]		yes[a]		yes[a]	
Occupation Dummies?	yes[b]		yes[b]		yes[b]	
Log Real Wage at Time of Layoff	.59**	.10	4.27**	.54	1.42**	.19
Training						
Job	.17**	.06	1.14**	.33	−.13	.12
General Education	−.02	.22	1.07	1.26	.09	.42
Both	.28	.20	3.10**	1.08	.43	.37
Year Dummies?	yes		yes		yes	
Constant	.06	.30	−2.59	1.67	−2.13**	.58
Adjusted R^2	.10		.11		.07	
Number of Observations	6,341		6,296		6,450	

[a]Omitted = construction. Coefficient for defense-related manufacturing significantly less than zero.
[b]Omitted = services. Coefficients for professional, technical, and managerial, sales, and production significantly less than zero.
*Significantly different from zero at 5 percent level.
**Significantly different from zero at 1 percent level.

However, job training continues to be associated with larger job changes, even when adjusting for all the other variables and for the training selection process. In addition, men who lose their job make significantly larger job changes than women, other things equal. High ability, as indicated by previous wage, also contributes to job mobility.

Table 9 examines the quality of job changes, as indicated by the U.S. Department of Labor measures of educational requirements (GED plus SVP) and the public impressions of prestige, as well as by the difference in the median pay in the new and old occupations.[15] The regressions are consistent in vari-

[15] The mean of the education variable is −0.05, indicating very little average difference in typical requirements on the old and new jobs, with a standard deviation of 2.3. Prestige falls slightly on average, by 0.60 point, with a standard deviation of 12.7. The

Table 9 continued

Results Using Fitted Training Probabilities					
(4) Educational Requirements		(5) Prestige		(6) Median Wage	
Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
−.19	.16	−.01	.83	−.15	.29
−.37*	.16	−1.59*	.83	−.40	.29
−.38*	.17	−1.31	.85	−.20	.30
−.54*	.19	−2.19*	.95	−.23	.33
−.08	.09	−.84*	.46	−.12	.16
−.04	.09	−.72	.47	.02	.17
.22	.12	.19	.60	.17	.21
.22	.34	3.92*	1.75	.90	.60
.53	.36	6.18**	1.82	1.47**	.63
1.06**	.36	9.59**	1.84	2.48**	.63
1.06**	.39	9.00**	1.99	3.30**	.69
.02	.02	.09	.09	.07*	.03
.12	.09	.17	.44	.07	.15
.39**	.14	3.45**	.70	.43*	.25
.24*	.12	2.42**	.59	.45*	.21
−.04	.13	−.02	.64	−.29	.23
−.02	.02	−.05	.08	−.05*	.03
yes[a]		yes[a]		yes[a]	
yes[b]		yes[b]		yes[b]	
.67**	.12	4.84**	.60	1.63**	.21
.04	.57	−3.63	2.86	−1.34	1.02
−.78	1.11	2.12	5.62	2.24	1.95
2.78**	1.01	21.03**	5.26	2.75	1.80
yes		yes		yes	
−.29	.49	−5.57*	2.53	3.26**	.88
.11		.11		.07	
5,987		5,944		6,087	

ous respects. More educated workers move to improved jobs, as do higher "quality" workers (as indicated by previous wage and reading test score). Former defense industry workers suffer setbacks; this dummy variable has a significant negative coefficient in all cases. When job change is measured according to education requirements or prestige (but not average wage), older workers make negative changes, while women make positive changes, all else equal.

The training results vary. General education alone never has a significant positive effect. Job training alone has a positive effect as measured by changes in the education and prestige measures, but not after taking into account the training selection process. That is, Table 7 demonstrated that this type of training tends to draw more educated and "higher quality" workers; the training classes themselves do not appear to improve the outcome. Those who took both job training and general education, who were shown on average to be relatively disadvantaged in terms of criteria such as previous education and pay, did reap benefits from training. The effect is visible only after adjusting for the characteristics of the participants. The job improvement for those who had job training combined with general education is indicated clearly in the results for education requirements and prestige; the training coefficient in the wage regression is positive, but not strongly significant.

A final set of regressions included the degree of job change as an additional explanatory variable (Table 10). All else equal, larger changes tend to be associated with a move to occupations with lower educational requirements, prestige, and average pay. These results were more significant for prestige and pay, and when the actual rather than the fitted degree of job change was used. The training variables continued to perform as before: combined training contributed to positive job changes, after adjusting for selection into training.

The data set lacks the information needed to determine why combined training was more effective

occupational wage falls by an average of 45 cents, with a standard deviation of $4.40.

Table 10
Direction of Job Change: Selected Ordinary Least Squares Regression Results Including Size of Job Change as an Explanatory Variable

	Results Using Training Dummies						Results Using Fitted Training Probabilities					
	(1) Educational Requirements		(2) Prestige		(3) Median Wage		(4) Educational Requirements		(5) Prestige		(6) Median Wage	
Independent Variable	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Training												
Job	.17**	.06	1.33**	.34	−.09	.12	.05	.57	−3.50	2.90	−1.16	1.02
General Education	−.02	.22	.84	1.29	.09	.43	−.78	1.11	.32	5.78	2.02	1.99
Both	.28	.20	3.01**	1.11	.29	.38	2.78**	1.01	20.58**	5.35	2.45	1.81
Size of Job Change	−.003	.008	−.19**	.05	−.07**	.02	−.0007	.009	−.18**	.05	−.07**	.02

	Results Using Training Dummies						Results Using Fitted Training Probabilities					
	(1) Educational Requirements		(2) Prestige		(3) Median Wage		(4) Educational Requirements		(5) Prestige		(6) Median Wage	
Independent Variable	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Training												
Job	.78	.50	3.61	2.11	.68	.75	.96	.79	−1.58	3.96	−.74	1.41
General Education	−.12	.40	.20	1.94	−.04	.61	−.84	1.11	1.96	5.63	2.20	1.95
Both	.74	.50	4.94*	2.35	.87	.76	2.97**	1.02	21.42**	5.28	2.87	1.81
Fitted Size of Job Change[a]	−.98	.78	−3.92	3.36	−1.31	1.18	−.36	.22	−.81	1.08	−.23	.38

Note: The regressions also included the same independent variables shown in Table 9.
*Significant at the 95 percent confidence level.
**Significant at the 99 percent confidence level.
[a]An additional regressor, the number of dependents, was added in the first stage equation in order to provide identification.

than job training or general education alone. Perhaps combined training simply was better at closing the gap between the skills possessed by this group of displaced workers and the skills required at available jobs than was the case for the other types of training and the other types of workers. Interestingly, the participants in combined training tended to spend longer in these training programs—about eight months on average—compared to other trainees; the job only and general education only groups trained for four to five months on average. So perhaps the intensity of training was also a factor.

VI. Conclusions

Using evidence from Massachusetts in the early 1990s, this study indicates that different types of training are used by different types of displaced workers and have different degrees of effectiveness. The extent of job change is measured by a continuous variable that captures job content and working conditions (rather than the typical dichotomous indicators of occupational and industry change). The direction of job change is measured in three different ways: the differences between the old and new occupations in general education and specific preparation requirements, in prestige, and in median pay.

Job training allowed displaced workers to make more fundamental changes in the nature of their work than would have been possible without such training. However, job training only (as opposed to job training combined with general education classes) tended to draw the most promising workers. It was this group's higher reading ability and previous earnings, as well as their demographic composition and work history,

rather than training per se that accounted for these workers moving into occupations that require more preparation and are more prestigious than their former jobs. Job training combined with education tended to draw less promising candidates. Adjusting for worker qualifications and past experiences, this group made positive job shifts as a result of training. By contrast, academic coursework alone did not enable trainees to make either bigger or better job changes than would have been expected on the basis of these workers' pre-training qualifications.

The results deserve further scrutiny. Even if some training programs can be shown to provide positive job changes that eventually result in higher job satisfaction or greater income for displaced workers, they may still turn out not to be socially beneficial. The existing literature has pointed out that training programs are more costly than job search assistance, and this paper shows a rather modest degree of occupational change even among displaced workers who trained.

On the other hand, the results point to some potential social and private benefits that are not typically acknowledged. In an economy with changing types of job opportunities, it is in the public interest to help workers move to jobs that are more closely aligned with future needs. For example, when manufacturing production jobs are shrinking, it makes sense to have workers change to other occupations, even if there is zero immediate pecuniary or non-pecuniary payoff. To the extent that job training was found in this study to be associated with larger job changes, some degree of continued government support may be warranted.

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Appendix Table 1
Characteristics of 20 Common Occupations for Displaced Workers, in Order of Prestige

Occupational Title	Data (6–0 scale)	People (8–0 scale)	Things (7–0 scale)	Strength (1–5 scale)
Maids and Housemen	5.4	7.6	6.6	3.2
Grinding, Abrading, Buffing, and Polishing Occupations	4.9	7.9	3.5	2.8
Small Engine Repairers	2.5	7.5	1.5	2.9
Truck Drivers, Heavy	4.9	6.6	3.5	2.7
Sales Occupations, Other Business Services	2.9	5.4	6.3	1.9
Helpers, Mechanics and Repairers	2.6	7.3	1.9	3.2
Molding and Casting Machine Operators	4.2	6.6	4.6	2.6
Supervisors, Cleaning and Building Services	3.9	6.3	5.1	3.0
Fabricating Machine Operators, n.e.c.	5.0	7.2	4.6	2.7
Advertising and Related Sales Occupations	2.5	5.0	6.7	1.8
Heat Treating Equipment Operators	4.7	7.2	2.5	2.7
Technicians, n.e.c.	2.1	5.7	3.4	2.0
Secretaries	3.0	6.0	2.1	1.0
Designers	.8	6.2	2.2	1.8
Management Related Occupations, n.e.c.	1.8	4.7	6.3	1.6
Electrical and Electronic Engineers	.5	6.0	2.2	1.9
Dentists	1.0	.4	1.0	1.2
Architects	.3	6.0	1.5	1.9
Chemists, except Biochemists	.4	6.0	1.3	2.0
Postsecondary Teachers, Subject Not Specified	2.0	2.4	6.6	2.0

Source: See Appendix text.

Appendix: Measurement of Job Quality and Skill Requirements

by Margaret E. Enis

Prestige ratings for 1980 Census detailed occupations came from the study, "Occupational Prestige Ratings from the 1988 General Social Survey." All other skill measurements can be found in "Occupational Measures from the Dictionary of Occupational Titles for 1980 Census Detailed Occupations." Both data sets are available from the Inter-University Consortium for Political and Social Research (ICPSR). Values for a selected sample of occupations are shown in Appendix Table 1.

The displaced workers data set, which was provided by the Massachusetts Industrial Services Program, initially included 3-digit occupational codes from the Dictionary of Occupational Titles (DOT) for the previous job, and complete 9-digit DOT codes for the new job. To merge the skill and prestige ratings into the data set, the DOT codes had to be translated into the 1980 Census detailed occupational codes found in the other data sets. The National Occupational Information Coordinating Committee (NOICC) Crosswalk and Data Center supplied a file containing the 1990 Census detailed occupational codes corresponding to each 9-digit occupational code from the Dictionary of Occupational Titles. They also provided another file translating the 1990 Census codes into those used in 1980 (very few changes were made in occupational classification between 1980 and 1990.) The 9-digit DOT codes for the new job translated directly into the 504 Census detailed occupations. To assign Census occupational codes to the previous job, the first three digits of the DOT codes in the translation file were used. Since the Census codes show less detail than the DOT codes, most of the DOT codes for the previous occupation translated into only one Census code. For the few that could translate into more than one Census code, the DOT occupational code was assigned several possible Census codes, which were weighted by employment. Weights were assigned using the employment in that occupation in Massachusetts taken from the 1990 Census of Population and Housing Subject Summary Tape File, "Earnings by Occupation and Education." The median hourly wage by occupation came from the Census tape as well. The 1990 Census detailed occupational codes were then translated into 1980 codes using the NOICC translation file. Prestige ratings and skill measure-ments were merged by 1980 Census code for both the new and old jobs in the displaced workers data set using similar procedures.

Appendix Table 1 continued

Characteristics of 20 Common Occupations for Displaced Workers, in Order of Prestige

Physical Demands (0–4 scale)	Environmental Hazards (0–6 scale)	General Educational Development (1–6 scale)	Specific Vocational Preparation (1–9 scale)	Prestige (1–100 scale)	Median Wage (dollars)
2.8	.1	1.9	2.5	20.1	7.65
2.1	1.4	2.6	4.1	22.8	11.01
2.8	.9	3.6	6.2	27.8	12.49
2.3	.4	2.9	3.4	30.1	12.07
.7	.0	3.8	4.9	32.3	18.40
2.9	1.5	3.7	6.3	33.4	10.47
1.9	.6	3.5	4.7	33.7	10.63
2.4	.5	2.9	4.5	35.6	12.15
2.0	.7	2.9	3.8	37.8	9.24
.2	.0	4.2	6.0	39.3	17.45
1.8	2.4	3.2	4.5	39.9	11.81
1.7	.2	4.4	6.0	40.9	14.69
2.0	.0	4.0	6.0	46.1	10.60
1.8	.4	4.5	6.6	46.5	14.99
.8	.3	4.4	6.5	48.7	12.44
1.8	.1	5.0	7.8	64.2	21.89
2.0	.0	5.9	7.9	71.8	40.68
1.8	.1	5.7	7.9	73.2	20.71
1.9	.1	5.7	7.7	73.3	16.66
.6	.0	5.6	7.7	73.5	20.73