Industry-Specific Human Capital: Evidence from Displaced Workers

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Results from the Displaced Worker Surveys show that the wage cost of switching industries following displacement is strongly correlated with predisplacement measures of both work experience and tenure. Workers apparently receive compensation for some skills that are neither completely general nor firm-specific but rather specific to their industry or line of work. Further, among displaced workers who find new jobs in their predisplacement industry, postdisplacement returns to predisplacement job tenure resemble cross-section estimates of the returns to current seniority. This suggests that firm-specific factors may contribute little to the observed slope of wage tenure profiles.

I. Introduction

Previous empirical work on human capital has focused almost exclusively on skills that are either specific to a given firm or completely general. Few studies have explored the importance of skills that are specific to firms in a given industry or sector of the economy, but, in many firms, industry-specific skills may constitute an important component of the typical worker's human capital stock.[1] For example, all firms in a given manufacturing

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[1] Kim (1992) and Kletzer (1993) are exceptions. Kim addresses industry-specific skills in an examination of interindustry wage differentials, and Kletzer examines sector-specific skills and sectoral mobility following displacement. In related work, Carrington (1993) examines links between sectoral decline and the consequences of displacement. See Willis (1986) for a review of empirical work on human capital.

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industry may value a common set of skills that are vital to the production process in that industry. However, these same skills may not be valued by firms that manufacture different product lines.

In this article, I use data from the Displaced Worker Surveys (DWS) to demonstrate that wages, in part, reflect compensation for industry-specific skills. My analyses of the wage innovations that accompany displacement yield several results that support this thesis. To begin, although most displaced workers suffer wage losses, workers who switch industries following displacement usually suffer greater losses than observationally similar workers who find new jobs in their predisplacement industry.[2] Further, among switchers, wage losses are strongly correlated with predisplacement measures of work experience and job tenure. These correlations are weaker among displaced workers who find new jobs in their predisplacement industry. As a corollary, the level of postdisplacement wages rises more sharply with predisplacement tenure and experience among workers who stay in their predisplacement industry than among workers who switch industries.

I argue that the literature on returns to job seniority is too narrowly focused on firm-specific factors. I show that among displaced workers who stay in their predisplacement industry, the profile of postdisplacement wages with respect to predisplacement tenure is quite similar to the wage tenure profile observed in a cross-section of workers. Thus, a complete explanation for the observed relationship between wages and seniority must involve factors that are not truly firm-specific but rather specific to an industry or particular line of work. Existing models of matching, firm-specific investments, and backloaded compensation schemes (that prevent shirking) provide no rationale for a strong correlation between wages on a given job and tenure on a previous job.

In the following two sections, I describe the data and present results that motivate the analyses in the balance of the article. The middle sections of the article address not only the wage innovations that follow displacement but also the decision to switch industries. In the final section, I discuss conclusions and implications for related research.

II. Data

In January of 1990, 1988, 1986, and 1984, the monthly Current Population Surveys included a supplement that sought additional information from workers who had previously suffered a job displacement. The DWS provides answers to retrospective questions concerning a respondent's employment history in the 5 years prior to the survey date. All persons 20

[2] This result is well established. Given different specifications, Podgursky and Swaim (1987), Addison and Portugal (1989), Topel (1990), and Carrington (1993) also report larger postdisplacement earnings losses for switchers.

years and older are asked if they had "lost or left a job because of plant closings, an employer going out of business, a layoff from which [the worker] was not recalled or other similar reasons" in the 5 years preceding the survey date.

For the purposes of the empirical work below, I restrict the sample to workers without predisplacement affiliation in agriculture or construction because displacement is not clearly defined for seasonal workers. Further, I also restrict the sample to workers who are employed full-time on both their predisplacement and postdisplacement jobs. This restriction is necessary because I want to measure wage rate changes that accompany displacement, and weekly earnings is the only wage measure available for both the predisplacement and postdisplacement jobs.

In these surveys, displaced workers do not represent a random sample of the labor force. Previous work by Gibbons and Katz (1991) indicates that firms lay off workers who are, on average, less productive than coworkers who are observationally similar. Therefore, in this article, I restrict my attention to displaced workers who lost their jobs because of establishment closings. As a first approximation, I consider these workers exogenously displaced.[3] There is an additional reason to exclude workers on layoff from the sample. Work by Topel (1990) shows that recall bias is a problem in the sample of laid off workers but not in the sample of workers displaced by establishment closings.[4]

III. A Simple Model of Wage Determination

The empirical strategy of this article is motivated by the following thought experiment. Imagine that a group of workers are exogenously removed from their jobs and then randomly assigned to new firms. Some workers are assigned to new firms in their original industry. Others are assigned to firms in different industries. Further, assume that we observe wages for this group just before they are displaced and just after they begin their new jobs. Finally, assume that wages are determined according to equations (1)–(3) (individual subscripts are suppressed):

[3] Nonetheless, these workers do not constitute a random sample of the labor force because the probability of being displaced by an establishment closing may differ across industries. See Carrington and Zaman (1994) for interindustry comparisons of displacement rates.

[4] Topel (1990) shows that, in the 1986 and 1984 DWS data sets, there is evidence of recall bias in the layoff sample. This is not true in the plant closing and shift elimination samples. I do not include workers displaced by shift elimination for two reasons. First, firms probably eliminate the least productive shifts. Second, if a firm chooses to eliminate its night shift and increase day shift production, the firm likely offers day shift jobs to the best night shift workers.

$$w_1 = \alpha\,\text{experience} + \theta\,\text{industry tenure} + \gamma\,\text{firm tenure} + X\beta + \varepsilon_1, \quad (1)$$

$$w_2 = \alpha\,\text{experience} + \theta\,\text{industry tenure} + X\beta + \varepsilon_2, \quad (2)$$

$$w_3 = \alpha\,\text{experience} + X\beta + \varepsilon_3, \quad (3)$$

where w_1 is the wage on the predisplacement job, w_2 is the new wage for stayers, w_3 is the new wage for switchers, and X is a vector of worker characteristics. These characteristics are the same before and after displacement. The mean zero error terms ε_1, ε_2, and ε_3 capture match-specific effects on productivity. All three are independent draws from the same distribution.

In this framework, wages rise with experience, industry tenure, and firm tenure because workers acquire general skills, industry-specific skills, and firm-specific skills through on-the-job training. All displaced workers begin their postdisplacement jobs with zero firm tenure and forfeit the returns to seniority in their predisplacement firm. However, workers who switch industries following displacement forfeit both the returns to seniority in their previous firm and the returns to tenure in their previous industry. Thus, among displaced workers who are otherwise observationally similar, we expect switchers to suffer greater wage losses than stayers following displacement, and we expect the switcher-stayer differential in losses to increase with predisplacement industry tenure.

Among workers with the same seniority prior to displacement, the expected wage loss for a given switcher exceeds the expected wage loss for stayers by an amount proportional to the switcher's industry tenure. Therefore, with data from the experiment described above, one could estimate θ, the return to industry tenure, using a standard difference in differences approach.[5]

Unfortunately, I do not have data from an experiment. Further, even if one is willing to treat the wage and employment changes that follow establishment closings as outcomes of natural experiments, the data provided by the DWS do not accommodate a straightforward difference in differences analysis. To begin, the DWS does not provide data on industry tenure. Further, because the surveys are retrospective, the intervals between wage observations may be as great as 5 years, and productive worker characteristics may change during these intervals. Finally, it is impossible to fully control for these changes in worker characteristics because the surveys do

[5] For each switcher, form the difference between his actual wage loss, $(w_1 - w_3)$, and the mean wage loss among stayers with the same firm tenure. Then, regress this difference in differences on the industry tenure of each switcher.

not provide both predisplacement and postdisplacement observations on all worker characteristics.[6]

In light of these data limitations, I adopt the following approach. I regress the changes in log wages that accompany displacement on potential predisplacement experience, predisplacement job tenure, years since displacement, weeks unemployed following displacement, controls for occupational change, dummies for year of displacement, and several demographic characteristics. Further, I run these quasi-first-difference regressions separately for both male and female stayers and switchers.[7]

These regressions do not share the exact structure of the heuristic model outlined in equations (1)–(3), but the model does suggest several likely results. The difference between switchers and stayers is that switchers forfeit compensation for their industry-specific skills. Therefore, following displacement, the wage cost of switching industries should vary positively with predisplacement industry tenure. In the absence of direct controls for industry tenure, we expect to observe positive correlations between the wage cost of switching industries and predisplacement measures of both experience and firm tenure.[8]

The anticipated results are borne out in table 1. Among displaced men who switch industries, wage losses increase with experience and tenure at roughly twice the rates observed among stayers. Consider two workers. One is displaced after working 10 years for the same employer. The other is displaced during the first year of his career. If both are switchers, the former's expected loss in log wages is more than .27 greater than the loss

[6] Given the structure of the DWS, I cannot tell how long a worker has been on his postdisplacement job, exactly how much he worked between the date of displacement and the interview date, or how many jobs he held following displacement. In the analyses below, I use years since displacement and weeks unemployed following displacement as controls for the composition of each worker's labor market experience following displacement. Further, the surveys do not provide predisplacement observations on demographic variables like education and marital status. Therefore, I use postdisplacement values as proxies for predisplacement demographic characteristics.

[7] All wages are measured in 1990 dollars. With the exception of a few service industries, I define industries at the two-digit levels. See appendix table A2 for a list of the industries.

[8] Throughout most of the article, I discuss industry-specific skills as acquired attributes. This facilitates exposition but is not central to the analysis. My results imply an important link between wages and industry tenure, but they do not necessarily imply that workers invest in industry-specific skills. Variants of Jovanovic's (1979*a*) matching model could easily generate a correlation between wages and industry tenure. In such models, workers would search across industries, and wages would be determined by idiosyncratic matches between worker attributes and the tasks performed in a given industry.

Table 1
Determinants of Changes in Log Wages for Displaced Workers: Estimates from Ordinary Least Squares Regressions

	Men		Women	
	Switcher (1)	Stayer (2)	Switcher (3)	Stayer (4)
Constant	.141	.030	.050	−.110
	(.100)	(.105)	(.119)	(.150)
Experience (predisplacement)	−.017	−.008	.001	.004
	(.004)	(.004)	(.004)	(.006)
Experience^2	.0003	.0002	−.0001	−.0001
	(.0001)	(.0001)	(.0001)	(.0001)
Tenure (predisplacement)	−.015	−.007	−.025	−.015
	(.005)	(.005)	(.007)	(.009)
Tenure^2	.0002	−.00002	.0006	.0005
	(.0002)	(.00019)	(.0003)	(.0004)
Years of schooling	−.004	−.004	−.006	.007
	(.005)	(.005)	(.007)	(.008)
Occupation:*				
Manager	.045	.168	.268	.197
	(.044)	(.069)	(.067)	(.123)
Professional	.069	.114	.120	.266
	(.056)	(.085)	(.081)	(.147)
Technician	.188	.042	.216	−.036
	(.060)	(.105)	(.089)	(.167)
Sales	−.012	.158	.090	.105
	(.041)	(.062)	(.069)	(.127)
Clerk	.052	.093	.155	.070
	(.048)	(.070)	(.065)	(.118)
Service worker	−.026	.104	−.063	.018
	(.043)	(.090)	(.067)	(.133)
Crafts worker	.096	.081	.133	.264
	(.034)	(.053)	(.073)	(.133)
Operative	.100	.088	.141	.122
	(.032)	(.052)	(.064)	(.130)
White	−.050	−.040	.004	.018
	(.038)	(.049)	(.041)	(.057)
Married (current)	.060	.013	−.044	−.028
	(.025)	(.030)	(.027)	(.036)
Years since displacement	.010	.020	.039	.018
	(.010)	(.011)	(.012)	(.017)
Weeks unemployed (postdisplacement)	−.0046	−.0020	−.0013	−.0010
	(.0005)	(.0007)	(.0006)	(.0010)
R^2	.168	.063	.124	.090
Mean of dependent variable	−.140	−.062	−.085	−.048
N	1,685	956	1,014	477

NOTE.—Standard errors are in parentheses. All specifications include year of displacement dummies. The samples contain workers displaced from full-time jobs by plant closings and are restricted to persons between the ages of 20 and 61 who were employed at the survey dates and worked at least 35 hours per week. Further, all workers earning less than $40 per week are eliminated. Earnings are measured in 1990 dollars. Inflation is measured by the implicit gross national product deflator. See appendix tables A1 and A3 for descriptive statistics. Experience (predisplacement) is measured as age, during the year of displacement, minus years of schooling minus six.

* The occupation dummies do not control for predisplacement occupation. Rather, they measure net changes in occupational affiliation. For example, a person leaving a management job and entering a sales position would be coded as follows: management = −1; sales = 1; other occupations = 0. A person who does not change occupations receives zeros for all categories. Laborer is the omitted occupation.

expected by the latter. If both are stayers, the corresponding differential is less than .13.[9]

The results are not so dramatic for women. Losses are much more strongly correlated with tenure among displaced women who switch industries than among those who find new jobs in their original industry, but potential predisplacement experience is uncorrelated with wage losses for both switchers and stayers. I conjecture that the results for men and women differ in large part because potential experience is a noisier measure of total work experience for women, and thus, more weakly correlated with industry tenure. In the balance of the article, I focus on results for the male sample only.[10]

The results in table 1 suggest that, among experienced workers, a significant component of wages may reflect compensation for industry-specific skills. However, the magnitudes of the estimated coefficients may be misleading because these regressions do not address an obvious selection bias problem. Among displaced workers with a given set of predisplacement characteristics, those who possess comparatively few industry-specific skills are more likely to switch industries. Therefore, the relationship between industry tenure and industry-specific compensation in a sample of switchers may be quite different than the relationship observed in the labor force as a whole.[11]

In the following sections, I describe versions of the above regressions that include explicit controls for selection bias. Further, I present alternative specifications that address competing explanations for the results in table 1.

[9] The standard errors on these expected losses are .035 and .040, respectively. Although stayers should retain compensation for both their general and industry-specific skills, wage changes are negatively correlated with predisplacement experience among stayers. I offer two possible explanations for this result. First, given the structure of the DWS, postdisplacement wage growth affects the wage change observations, and human capital theory suggests that younger workers may experience faster wage growth. Further, Jovanovic (1979*b*) and other models that include outside offers demonstrate that, holding tenure constant, the expected value of job matches increases with market experience.

[10] Below, I present results that include controls for selection bias and controls for industry wage rents. I have conducted similar analyses for women. The results follow the pattern observed in table 1. Among women, job tenure but not potential experience appears to be correlated with industry-specific compensation.

[11] Assume that the data permitted a direct regression of log wage changes on predisplacement industry tenure using the sample of switchers. The sign of the bias on the slope coefficient would be given by the covariance between industry tenure and the error term, conditional on a voluntary switch. The discussion above implies that this covariance would be positive. If industry-specific skills are important, the costs of switching rise with industry tenure. Thus, conditional on a voluntary switch, the expected value of the error term in the switcher's equation should be an increasing function of predisplacement tenure.

IV. Selection Corrections and Postdisplacement Wage Changes

I use a variant of Heckman's (1979) two-stage procedure to produce corrected versions of the above regressions. To implement this method, I must estimate the following selection equation using a standard probit model:

$$s_i^* = Z_i\beta + v_i, \tag{4}$$

where s_i^* is the latent value of switching industries for displaced worker i. We observe a switch if $s_i^* > 0$, and we observe a new job in the predisplacement industry otherwise. Vector Z_i captures worker and industry characteristics, v_i captures unobserved individual specific costs of switching, and $v_i \sim N(0, 1)$.

If Z_i does not contain at least one element that is excluded from the wage regressions, the identification of the model hinges completely on functional form assumptions. Therefore, satisfactory identification requires data on factors that affect the value of switching industries but do not directly affect wages.

My goal is to construct variables that, given a worker's characteristics, are unrelated to wage offers either prior to or following displacement but are still related to switching behavior through the costs of search. In this effort, I employ information from the Employment and Earnings Reports of the Bureau of Labor Statistics. The reports provide yearly levels of employment for each industry. From these data, I construct, for each worker, both the total number of jobs and the rate of job growth in the worker's predisplacement industry during the year the worker is displaced.[12]

I hypothesize that search costs associated with finding a job in one's predisplacement industry decrease with the number of jobs and the rate of job growth in that industry. Total employment in an industry should be inversely related to the cost of applying for a job in that industry. Workers displaced from retail trade can easily apply for new jobs in other retail stores, but workers displaced from small industries may have to relocate in order to apply for jobs in their original industry. Further, given the level of employment in an industry, the costs of finding a job opening should fall with the rate of job growth in the industry.

Is it reasonable to assume that total industry employment and industry employment growth are uncorrelated with wage offers? I see no direct link between wage offers and the level of industry employment, but the rate of employment growth in an industry may be correlated with product demand shifts or shocks to technology that affect the marginal product of

[12] See table A4 in the appendix for industry employment data.

labor in that industry.[13] Therefore, in the analyses below, I include both industry employment levels and industry employment growth rates as determinants of the latent value of switching industries, but I exclude only the employment levels from the wage change equations.[14]

Table 2 presents results from the probit analysis of industry switching. Both industry employment levels and employment growth have the expected impact on the probability of industry switching. Evaluate Z_i at the sample means of worker characteristics. The predicted probability of switching industries is approximately 64%. Given this base, a 1 SD increase in predisplacement industry employment translates into an approximately 7% decrease in predicted industry switching. A corresponding increase in the employment growth rate for the predisplacement industry yields a reduction of roughly 5%.[15]

Using the estimated coefficients from the probit model, I estimate selection corrected regressions of changes in log wages on worker characteristics for displaced men.[16] Columns 1 and 2 of table 3 contain specifications that are analogous to those in table 1. Here, the estimated coefficients on predisplacement tenure and experience reveal an even sharper contrast between stayers and switchers. An additional 10 years of experience with the same employer implies an increase in log wage losses of .31 for switchers but only .1 for stayers.[17] Therefore, the corrected results suggest that the ordinary least squares (OLS) results in table 1 may understate the effects of both experience and tenure on the wage cost of switching industries.

I offer these corrected results as additional support for the hypothesis that workers receive compensation for industry-specific skills, but others

[13] I do not address the fact that all search costs may influence reservation wages and thus may influence distributions of accepted wages. Here, I implicitly assume that workers know the wage they would receive in their predisplacement industry and in other industries. Therefore, workers stay in their original industry whenever the wage gains associated with staying exceed the additional costs associated with locating a new job in that particular industry.

[14] Number of kids is also included in the probit equation but excluded from the wage change equations. Family size may affect mobility costs, but, conditional on the other observed worker characteristics, family size should not influence wage offers.

[15] The results also show that the probability of switching declines with predisplacement tenure. Topel (1990) reports similar results.

[16] In this work, I address only the decision to switch industries. I do not model selection into reemployment. In unreported analyses, I explore this issue by further restricting the sample to workers who were displaced at least 1 year before the survey date. Although this restriction eliminates approximately 17% of the sample, the estimated tenure and experience effects are almost identical to those reported in table 3. See Fallick (1993) for an analysis of reemployment decisions following displacement.

[17] The standard errors on these expected losses are .039 and .042, respectively.

Table 2
Dependent Variable: Switch = 1, Stay = 0

	Worker/Job Characteristics
Constant	.977
	(.259)
Industry employment growth (predisplacement)	−1.460
	(.309)
Industry employment in millions of workers (predisplacement)	−.032
	(.005)
Experience (predisplacment)	−.010
	(.009)
Experience^2	.0001
	(.0003)
Tenure (predisplacment)	−.024
	(.011)
Tenure^2 (predisplacment)	.001
	(.0004)
Years of schooling	−.007
	(.013)
Manager	.008
	(.133)
Professional	−.390
	(.151)
Technician	−.259
	(.183)
Sales	−.134
	(.128)
Clerk	−.186
	(.153)
Service worker	.053
	(.157)
Crafts worker	−.266
	(.115)
Operative	−.188
	(.114)
White	−.168
	(.095)
Married (current)	−.151
	(.065)
Kids	.016
	(.018)
Years since displacement	.051
	(.023)
Mean of dependent variable	.64
N	2,641

NOTE.—Standard errors are in parentheses. The specification includes year of displacement dummies. The occupation variables reflect occupation on the predisplacement job. The omitted occupation is laborer. The industry employment data comes from the Employment and Earnings Reports of the Bureau of Labor Statistics. See table A4. Employment growth for year t is defined as employment in year $t + 1$ minus employment in year $t - 1$ divided by employment in year t. Table A3 provides descriptive statistics for switchers and stayers. The samples contain workers displaced from full-time jobs by plant closings and are restricted to persons between the ages of 20 and 61 who were employed at the survey dates and worked at least 35 hours per week. Further, all workers earning less than $40 per week are eliminated. Earnings are measured in 1990 dollars. Inflation is measured by the implicit gross national product deflator.

may argue that the results simply demonstrate that workers in some industries earn rents. It is well known that wages differ substantially across industries for observationally similar workers.[18] If industry wage premiums represent labor market rents, and jobs are rationed in high-wage industries, two results might be expected. First, because rents impede mobility, average industry tenure may be higher for workers in high-wage industries.[19] Second, wage losses associated with displacement should be particularly large among workers who leave high-wage industries. Thus, the presence of labor market rents in high-wage industries may contribute to the apparent correlation between industry tenure and the wage cost of switching industries.

I investigate this issue by estimating alternative specifications of both the selection equation and the wage change equations that include controls for the industry wage premiums and union coverage rates associated with each worker's predisplacement job. The industry wage premiums come from a standard cross-section wage regression. The regression does not include controls for union status. Therefore, the wage premiums capture industry level rents generated by union activity as well as any rents associated with the payment of efficiency wages.[20] Because the surveys do not provide individual union status on predisplacement jobs, I use the union coverage rate in a worker's three-digit industry as a control for individual union status prior to displacement.[21] Results from the alternative model are displayed in columns 3 and 4 of table 3.[22]

[18] See Krueger and Summers (1988) and Katz and Summers (1989). Both present numerous empirical results concerning the pattern of interindustry wage differentials, and both conclude that workers in high-wage industries earn substantial rents.

[19] Krueger and Summers (1988) provide evidence that, among observationally similar workers, average tenure is higher in high-wage industries.

[20] Appendix table A2 presents the industry wage premiums and details their construction.

[21] The data come from Curme, Hirsch, and MacPherson (1990). Because the data do not provide coverage rates for all years covered by the DWS, I define one coverage rate for each industry as the average rate over the period 1983–86. To further investigate the importance of predisplacement union status, I reestimated the specifications in table 3 using a sample of workers who were likely not in union jobs prior to displacement. Table A5 details the results and the sample selection rules. The losses for switchers are smaller in this "nonunion" sample, but the estimated experience and tenure profiles are quite similar to those in table 3.

[22] The alternative model included controls for coverage rates and industry wage premiums in both the selection equation and the wage change regressions. The additional controls increase the explanatory power of the probit model. Workers displaced from high-wage industries are more reluctant to switch industries than similar workers displaced from low-wage industries. Nonetheless, the probit results from the alternative specification are quite similar to the baseline results in table 2.

The additional controls do weaken the correlations between log wage losses for switchers and predisplacement measures of both experience and tenure, but the results still indicate important profile differences between switchers and stayers. Among switchers, losses in log wages rise roughly twice as fast with both predisplacement experience and tenure. The wage cost of switching industries is clearly greatest among senior workers with considerable labor market experience. For displaced males, the first 10 years of work experience with a given employer im-

Table 3
Determinants of Changes in Log Wages for Displaced Workers: Men (Estimates from Heckman's Two-Stage Procedure)

	Baseline		Alternative Specification	
	Switcher (1)	Stayer (2)	Switcher (3)	Stayer (4)
Constant	.024	−.116	.292	−.137
	(.115)	(.153)	(.113)	(.150)
Experience (predisplacement)	−.018	−.007	−.014	−.007
	(.004)	(.004)	(.004)	(.004)
Experience^2	.0003	.0002	.0003	.0002
	(.0001)	(.0001)	(.0001)	(.0001)
Tenure (predisplacement)	−.019	−.005	−.012	−.006
	(.005)	(.005)	(.005)	(.005)
Tenure^2	.0003	−.00008	.0001	−.00006
	(.0002)	(.00020)	(.0002)	(.00020)
Industry employment growth (predisplacement)	−.313	.169	−.137	.195
	(.162)	(.202)	(.151)	(.197)
Industry wage premium (predisplacement)	...	...	−.551	−.018
			(.100)	(.108)
Union coverage rate (predisplacement)	...	...	−.105	.085
			(.081)	(.095)
Years of schooling	−.005	.005	−.003	.006
	(.005)	(.005)	(.005)	(.005)
Occupation:*				
Manager	.039	.167	.042	.165
	(.044)	(.069)	(.043)	(.069)
Professional	.091	.097	.035	.094
	(.057)	(.085)	(.056)	(.085)
Technician	.200	.031	.156	.032
	(.061)	(.104)	(.060)	(.104)
Sales	.006	.145	−.005	.141
	(.042)	(.062)	(.041)	(.062)
Clerk	.062	.084	.038	.081
	(.049)	(.070)	(.047)	(.070)
Service worker	−.023	.106	−.024	.107
	(.043)	(.089)	(.042)	(.089)
Crafts worker	.108	.075	.073	.075
	(.035)	(.052)	(.034)	(.052)
Operative	.106	.084	.077	.082
	(.033)	(.051)	(.032)	(.051)

Table 3 (*Continued*)

	Baseline		Alternative Specification	
	Switcher (1)	Stayer (2)	Switcher (3)	Stayer (4)
White	−.074	−.026	−.029	−.026
	(.041)	(.050)	(.039)	(.050)
Married (current)	.042	.019	.077	.018
	(.027)	(.030)	(.026)	(.031)
Years since displacement	.016	.016	−.006	.017
	(.010)	(.011)	(.010)	(.011)
Weeks unemployed (postdisplacement)	−.0046	−.0021	−.0044	−.0022
	(.0005)	(.0007)	(.0005)	(.0007)
Selection correction term	.265	−.128	−.142	−.132
	(.113)	(.097)	(.108)	(.097)
R^2	.171	.065	.192	.067
Mean of dependent variable	−.140	−.062	−.140	−.062
N	1,685	956	1,685	956

NOTE.—Standard errors are in parentheses. All specifications include year of displacement dummies. With the exception of the controls for selection bias and industry employment growth, the baseline specification is the same specification used in table 1. Ordinary least squares versions of the baseline yield estimated coefficients on the experience and tenure terms that are almost identical to those in table 1. The samples contain workers displaced from full-time jobs by plant closings and are restricted to persons between the ages of 20 and 61 who were employed at the survey dates and worked at least 35 hours per week. Further, all workers earning less than $40 per week are eliminated. Earnings are measured in 1990 dollars. Inflation is measured by the implicit gross national product deflator. The selection correction terms are constructed by using estimated coefficients from the corresponding probit models of industry switching. These terms take on different values depending on whether the worker is a switcher or stayer. The coefficient on each selection term is the covariance between the error in the relevant wage equation and the error term in the selection equation. The standard errors do account for the sampling error introduced by the selection correction term, but the standard errors in cols. 3 and 4 do not account for the fact that the industry wage premiums are estimated in a separate regression. Appendix table A2 presents the industry wage premiums and details their construction.

* The occupation dummies do not control for predisplacement occupation. Rather, they measure net changes in occupational affiliation. For example, a person leaving a management job and entering a sales position would be coded as follows: management = −1; sales = 1; other occupations = 0. A person who does not change occupations receives zeros for all categories. Laborer is the omitted occupation.

plies an addition to log wage losses of .21 for switchers but less than .11 for stayers.[23]

The results from both specifications suggest that workers receive industry-specific compensation and that levels of industry-specific compensation are correlated with industry tenure. If the controls, in columns 3 and 4, for industry wage premiums and union coverage rates are actually controls for rents, then, on average, a portion of industry-specific compensation reflects rents and not compensation for industry-specific skills. However, there is considerable debate about whether or not workers in

[23] The standard errors on these expected losses are .038 and .042, respectively. The corrections for selection bias contribute little to the results. Estimated coefficients from OLS versions of these wage change equations imply almost identical results for both stayers and switchers.

high-wage industries and/or unions actually earn rents. Several studies document the sorting of workers on ability across industries and argue that industry wage premiums, in part, reflect differences across industries in unobserved dimensions of worker skill.[24] Further, some researchers contend that, within a given industry, unmeasured differences in worker quality account for a portion of the wage gap between union and nonunion workers.[25]

The results in table 3 show that displaced workers maintain their predisplacement industry wage premiums if they find new jobs in their predisplacement industry. However, displaced workers who leave high-wage industries lose over half of their predisplacement premiums. Are these switchers forfeiting industry-specific rents, compensation for industry-specific skills, or both? The existing literature on interindustry wage differentials does not provide a clear answer to this question.

V. Returns to Predisplacement Job Tenure

The results in the previous section have important implications for existing research on the returns to job seniority. Most explanations for the observed correlation between wages and seniority stress firm-specific factors. Matching models and models of investment in firm-specific human capital focus on the firm-specific aspects of worker productivity.[26] Further, models that address shirking problems offer backloaded compensation schemes as solutions to particular agency problems between a firm and its workers.[27] However, the results in table 3 indicate that the observed correlation between job tenure and wages is driven in part by industry-specific factors.

To further highlight the link between industry-specific compensation and the returns to job seniority, I present three additional regressions. The first is a standard cross-section wage regression of log wages on tenure and other worker characteristics using data from the predisplacement jobs. The second and third are regressions of log postdisplacement wages on predisplacement tenure and other worker characteristics. I estimate one for switchers and the other for stayers, again using Heckman's method to control for selection bias. Table 4 presents the results.

For both stayers and switchers, postdisplacement wages are positively correlated with predisplacement tenure. However, a comparison of columns

[24] See Murphy and Topel (1990), Gibbons and Katz (1991), and Keane (1993). Kim (1992) and Neal (1995) specifically address the link between industry-specific skills and industry wage premiums.

[25] See Lewis (1986), chap. 4, for a discussion of union wage premiums and selectivity bias. Hirsch (1993) addresses the magnitude of this bias in the trucking industry.

[26] See Becker (1975) and Jovanovic (1979*a*).

[27] See Lazear (1981).

Table 4
The Relationship between Wages and Job Tenure: Men
Dependent Variable = Displacement Wage

	Log Predisplacement Wage: Full Sample (1)	Log Postdisplacement Wage	
		Switchers (2)	Stayers (3)
Constant	4.594	4.667	3.890
	(.075)	(.107)	(.223)
Experience (predisplacement)	.033	.016	.027
	(.003)	(.004)	(.006)
Experience^2	−.0006	−.0003	−.0004
	(.0001)	(.0001)	(.0002)
Tenure (predisplacement)	.030	.011	.030
	(.004)	(.005)	(.008)
Tenure^2	−.0007	−.0004	−.0010
	(.0001)	(.0002)	(.0003)
Years of schooling	.077	.075	.087
	(.004)	(.005)	(.008)
White	.185	.131	.200
	(.031)	(.037)	(.070)
Married (current)	.064	.092	.165
	(.020)	(.025)	(.043)
Industry employment growth	...	.054	.851
		(.144)	(.283)
Years since displacement	...	.029	.043
		(.009)	(.016)
Weeks unemployed (postdisplacement)	...	−.0040	−.0025
		(.0005)	(.0008)
Selection correction term	...	−.029	−.627
		(.099)	(.137)
R^2	.28	.23	.29
Mean of dependent variable	6.14	5.96	6.14
N	2,641	1,685	956

NOTE.—Standard errors are in parentheses. All specifications include dummies for year of displacement and region of current residence. The samples contain workers displaced from full-time jobs by plant closings and are restricted to persons between the ages of 20 and 61 who were employed at the survey dates and worked at least 35 hours per week. Further, all workers earning less than $40 per week are eliminated. Earnings are measured in 1990 dollars. Inflation is measured by the implicit gross national product deflator. The selection correction terms are constructed using the coefficients from the probit analysis in table 2. The negative coefficient on the selection term in the stayers equation indicates a negative covariance between the error in the switching equation and the error in the wage equation for stayers. When observed characteristics are held constant, workers who benefit greatly from staying are unlikely to switch. See table 3 for more details about the corrections for selection bias.

2 and 3 shows that the link between predisplacement tenure and postdisplacement wages is much stronger among displaced workers who stay in their predisplacement industry. Previous studies of displaced workers have documented a positive correlation between postdisplacement wages and predisplacement tenure, but these studies did not analyze stayers and switchers separately.[28] Therefore, this correlation has previously been in-

[28] See Addison and Portugal (1989), Kletzer (1989), and Ruhm (1990).

terpreted as evidence that job tenure is a signal of general worker ability and that cross-sectional estimates of the returns to job seniority are contaminated by heterogeneity bias. The results for switchers do provide support for the heterogeneity bias hypothesis, but the results for stayers indicate that job tenure is correlated with not only general ability but also an industry-specific component of wages.

The most striking result in table 4 is the magnitude of the tenure effects for stayers. The results in column (3) show that stayers receive a postdisplacement log wage return of .20 for 10 years of predisplacement job seniority.[29] The cross-section results in column 1 indicate only a slightly greater return of .23 for 10 years of current job seniority.[30] These results are not easily reconciled with models of firm-specific investments, job matching, or backloaded compensation schemes. All imply that wages should be correlated with current tenure, but none implies that seniority in a given firm should be highly valued by other firms in the same industry.[31]

Nonetheless, these results should not be taken as evidence that workers possess few firm-specific skills or that firms never backload compensation. The analyses presented here rely on weekly wages as the sole measure of worker compensation. No attention is given to fringe benefits or pensions that may be tied to a worker's tenure in a particular firm. Further, there

[29] The standard error on this expected return is .053. Results from OLS versions of the postdisplacement wage equations do indicate smaller tenure effects for stayers but approximately the same effects for switchers. The implied log wage returns to 10 years of predisplacement tenure are .13 for stayers and .6 for switchers. However, in the two-stage model, the strong tenure effects for stayers are quite robust to specification. For example, whether the occupation controls are included in only the probit model, in both the probit model and the wage equations, or in neither, the results for stayers imply (postdisplacement) log wage returns of .20–.24 for 10 years of predisplacement tenure. The difference between the OLS and two-stage estimates is consistent with the view that, in the equation for stayers, selection bias creates a negative correlation between predisplacement tenure and the error term. In other words, those who report short tenure on their predisplacement job yet still incur the costs required to locate a new job in their predisplacement industry constitute quite a select group of workers.

[30] Although these cross-section estimates (of returns to seniority) do not come from a random sample of the workforce, the estimates are comparable to those in previous studies. Altonji and Shakotko (1987) and Topel (1991) both use data from the Panel Survey of Income Dynamics to estimate similar specifications. According to their estimates, the first 10 years of seniority imply additions to log wages of .3 and .24. The corresponding figure, given the estimated coefficients in col. 1, is .23. The standard error on this expected return is .024.

[31] The results in table 4 also indicate that the convention of interpreting returns to experience as returns to general human capital may be misleading. Postdisplacement wages rise much more sharply with predisplacement experience among stayers than among switchers. This suggests that, in the absence of controls for industry tenure, measured returns to experience in part capture returns to skills that are not completely general.

is a sample selection problem. By definition, every worker in the sample lost his predisplacement job before the end of his career. Shared investments in firm-specific training and implicit contracts that backload compensation may be most common in jobs where the probability of displacement is low.

VI. Conclusions and Implications for Future Research

The results presented here show that the wage cost of switching industries following displacement is strongly correlated with predisplacement measures of both work experience and tenure. Further, displaced workers who find new jobs in their predisplacement industry earn significantly greater returns to both their predisplacement experience and tenure than observationally similar workers who switch industries following displacement. I interpret these results as evidence that workers receive compensation for some skills that are neither completely general nor firm-specific but rather specific to a set of firms that produce similar products and services.

Further, it appears that the literature on returns to seniority focuses too narrowly on firm-specific factors. Displaced workers who find new jobs in their predisplacement industries earn substantial returns to their predisplacement tenure. In fact, the returns approximate standard cross-section estimates of the returns to current job tenure. This finding suggests that firm-specific factors may contribute little to the observed slope of wage tenure profiles.

Previous work on the costs of displacement clearly establishes that senior, experienced workers suffer larger wage losses following displacement than workers displaced early in their careers. Further, the literature also shows that switchers suffer greater wage losses than stayers.[32] The results presented here highlight an interaction between these previously established results. The switcher-stayer differential in wage losses is not a constant. Like the overall level of losses, this differential increases with predisplacement experience and tenure. Thus, the private wage costs of displacement may be greatest for experienced workers who are displaced during a large decline in total industry employment. These workers may find it costly to obtain new jobs in their predisplacement industry, and they will suffer large wage losses if they switch industries.[33]

In closing, I must acknowledge the possibility that the results outlined above reflect the importance of skills that are not truly specific to given

[32] See Podgursky and Swaim (1987), Addison and Portugal (1989), Topel (1990), and Jacobsen, LaLonde, and Sullivan (1993).

[33] See Carrington and Zaman (1994) for recent work on interindustry differences in the cost of displacement. In their attempts to explain these differences, they include controls for trends in industry employment and other industry characteristics, but they do not analyze stayers and switchers separately.

industries, but rather specific to a set of jobs that are associated with the intersection of certain occupations and industries. Helwege (1992) points out that only 11 three-digit occupations are significantly represented in as many as 20 two-digit industries. She concludes that for some purposes, "The distinction between industry and occupation is not very clear."[34] Further, Shaw (1984) demonstrates that occupational skills are an important determinant of earnings.[35] Future research in this area must confront the task of defining job categories that directly capture important skill specificities.

Appendix

Table A1
Descriptive Statistics for Current Population and Displaced Worker Surveys: Males

	(1)	(2)
Age	36.37	36.58
White	.90	.92
Married	.67	.72
Experience	16.55	17.02
	(11.50)	(10.41)
Experience^2	406.28	398.00
	(471.91)	(443.90)
Experience (predisplacement)	...	14.03
		(10.29)
Experience^2 (predisplacement)	...	302.63
		(381.34)
Tenure (predisplacement)	...	6.29
		(7.13)
Tenure^2 (predisplacement)	...	90.46
		(191.34)
Years of schooling	13.83	13.56
	(2.69)	(2.46)
Northeast	.24	.21
North-central	.25	.25
South	.30	.31
Years since displacement	...	3.03
		(1.37)
Displaced:		
1979	...	.04
1980	...	.05
1981	...	.11
1982	...	.12
1983	...	.15
1984	...	.10
1985	...	.16
1986	...	.11

[34] See Helwege (1992), p. 77.

[35] Shaw uses longitudinal data to develop a measure of occupational investment. Compared to standard measures of experience, Shaw's proxy for occupation-specific human capital explains a greater portion of the observed variation in individual earnings.

Table A1 (*Continued*)

	(1)	(2)
1987	...	.09
1988	...	.05
1989	...	.03
1990	...	.00
Weeks without work	...	16.04
		(21.97)
N	20,025	2,641

NOTE.—The statistics in col 1. describe full-time male workers in the January 1984, 1986, 1988, and 1990 Current Population Surveys. All workers are privately employed. The statistics in col. 2 describe the sample of male workers displaced by establishment closings.

Table A2
Estimated Industry Wage Premiums

	Estimated Wage Premium
Petrol	.458
	(.038)
Mining	.474
	(.021)
FIRE	.247
	(.009)
Transportation	.347
	(.012)
Broadcasting	.243
	(.038)
Transportation equipment	.389
	(.014)
Machinery	.311
	(.013)
Instruments	.263
	(.022)
Primary metals	.308
	(.021)
Chemicals	.369
	(.017)
Professional services	.243
	(.011)
Utilities	.416
	(.013)
Health services	.198
	.010)
Wholesale	.219
	(.011)
Rubber	.229
	(.022)
Food	.213
	(.016)
Tobacco	.415
	(.069)

Table A2 (*Continued*)

	Estimated Wage Premium
Print	.200 (.016)
Electrical machinery	.256 (.014)
Paper	.363 (.022)
Stone, clay, glass	.234 (.026)
Durables	.128 (.028)
Metals	.250 (.017)
Lumber	.143 (.023)
Furniture	.135 (.024)
Retail trade	. . .
Textiles	.173 (.022)
Repair services	.099 (.018)
Apparel	.045 (.019)
Personal services	.022 (.014)
Leather	.140 (.046)
Other services	−.025 (.014)
Education services	−.025 (.017)
N	34,115

NOTE.—The estimated wage premiums come from a cross-sectional regression that includes dummies for industry affiliation. The dependent variable is log current weekly earnings. Other regressors include a constant, sex, race, marital status, experience, and experience^2, years of education, three region dummies, nine occupation dummies, and interaction terms between sex and marriage, sex and experience, and sex and experience^2. The data come from full-time workers in the January Current Population Surveys for 1990, 1988, 1986, and 1984.

Table A3
Descriptive Statistics for Displaced Male Workers

	Switchers (1)	Stayers (2)
Log weekly earnings (current)	5.960 (.483)	6.148 (.509)
Change in log weekly earnings	−.140 (.485)	−.062 (.381)

Table A3 (*Continued*)

	Switchers (1)	Stayers (2)
Age	36.050	37.505
	(9.943)	(10.044)
Years of school	13.545	13.575
	(2.410)	(2.536)
Weeks without work	18.081	12.442
	(23.282)	(18.929)
Experience	16.506	17.931
	(10.285)	(10.561)
Experience^2	378.183	432.929
	(427.355)	(469.847)
Experience (predisplacement)	13.429	15.084
	(10.142)	(10.468)
Experience^2 (predisplacement)	283.143	336.975
	(363.006)	(409.628)
Tenure (predisplacement)	6.011	6.793
	(6.998)	(7.338)
Tenure^2 (predisplacement)	85.083	99.937
	(187.495)	(197.674)
Married	.706	.748
White	.907	.928
Northeast	.208	.205
North-central	.256	.240
South	.306	.319
West	.230	.236
N	1,685	956

NOTE.—Sample selection rules are described in table 3.

Table A4
Industry Employment Levels, 1978–91

	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991
Mining	828	865	940	1,079	1,028	921	957	939	880	818	753	719	730	733
Lumber	724	730	669	663	627	658	713	693	692	750	758	792	780	714
Furniture	554	567	510	510	461	488	549	597	650	634	685	664	686	622
Glass	679	706	637	632	539	588	593	577	616	600	610	640	625	578
Primary metals	1,220	1,262	1,169	1,148	925	804	855	810	779	806	802	838	857	787
Fabricated metals	1,634	1,697	1,712	1,671	1,545	1,257	1,317	1,304	1,303	1,285	1,332	1,319	1,264	1,205
Machine	2,485	2,747	2,790	2,818	2,558	2,426	2,663	2,626	2,503	2,441	2,532	2,587	2,471	2,406
Electrical machinery	2,144	2,293	2,294	2,317	2,295	2,108	2,289	2,232	2,153	2,115	2,039	2,067	2,046	1,943
Transportation equipment	2,230	2,298	2,100	2,107	1,931	2,247	2,471	2,586	2,679	2,642	2,645	2,638	2,554	2,407
Instruments	560	584	604	609	600	655	654	686	700	672	695	675	711	777
Durables	591	567	502	499	487	508	503	475	531	533	415	451	434	388
Food	1,874	1,789	1,763	1,745	1,733	1,643	1,692	1,720	1,737	1,714	1,701	1,821	1,830	1,726
Tobacco	73	64	57	67	74	67	60	63	63	58	58	54	47	58
Textiles	871	823	782	720	688	742	760	731	714	713	714	688	692	688
Apparel	1,285	1,279	1,250	1,224	1,150	1,154	1,209	1,176	1,176	1,174	1,182	1,172	1,076	1,044
Paper	705	726	706	723	689	677	637	660	698	737	735	749	734	736
Print	1,429	1,507	1,554	1,572	1,621	1,657	1,729	1,699	1,762	1,802	1,899	1,932	1,887	1,829
Chemicals	1,189	1,217	1,286	1,265	1,213	1,153	1,162	1,168	1,200	1,221	1,257	1,330	1,320	1,338
Petrol	242	256	225	227	229	202	204	189	169	170	179	179	170	187
Rubber	708	731	687	657	643	700	731	723	688	723	813	771	731	736
Leather	283	275	267	256	251	243	205	164	149	145	140	152	139	137
Transportation	3,548	3,706	3,596	3,630	3,590	4,066	4,319	4,459	4,608	4,815	4,959	5,051	5,035	5,022
Radio	185	180	193	211	243	221	231	237	252	279	280	255	245	272
Utilities	2,429	2,519	2,604	2,657	2,718	2,700	2,808	2,852	2,790	2,784	2,825	2,788	2,856	2,910
Wholesale	3,616	3,775	3,827	3,920	4,120	4,314	4,212	4,341	4,416	4,580	4,578	4,611	4,651	4,640
Retail	15,636	15,898	15,900	16,129	16,638	16,832	17,767	17,955	18,397	18,812	19,085	19,618	19,618	19,415
FIRE	5,406	5,779	5,860	5,997	6,270	6,510	6,750	7,005	7,401	7,763	7,921	7,988	8,021	7,786
Product services	4,062	4,250	4,507	4,763	5,208	5,533	6,153	6,640	7,030	7,569	8,006	8,384	8,483	8,562
Repair	1,362	1,432	1,448	1,577	1,655	1,688	1,828	1,971	1,905	1,930	1,961	2,046	2,083	2,082
Personal services	3,826	3,800	3,738	3,810	3,993	4,077	4,174	4,352	4,472	4,598	4,727	4,664	4,667	4,675
Health	6,673	6,849	7,186	7,379	7,810	7,874	7,934	7,910	8,129	8,478	8,781	9,110	9,447	9,817
Education	7,620	7,851	7,971	7,865	8,084	8,019	8,088	8,255	8,425	8,547	8,802	9,156	9,301	9,335
Services	3,074	3,049	3,090	3,221	3,285	3,732	3,890	4,034	4,187	4,428	4,610	4,688	4,932	5,075

SOURCE.—Data are from the "Total Employed" column of "Household Data Annual Averages," in the Bureau of Labor Statistics, Employment and Earnings Reports (1978–91).
NOTE.—Entries are in thousands of workers.

Table A5
Determinants of Changes in Log Wages for "Nonunion" Displaced Workers: Men

	Baseline		Alternative	
	Switcher (1)	Stayer (2)	Switcher (3)	Stayer (4)
Constant	.029	−.116	.220	−.144
	(.118)	(.156)	(.121)	(.169)
Experience (predisplacement)	−.016	−.007	−.012	−.006
	(.005)	(.005)	(.004)	(.005)
Experience^2	.0003	.0002	.0002	.0002
	(.0001)	(.0001)	(.0001)	(.0001)
Tenure (predisplacement)	−.017	−.004	−.010	−.004
	(.006)	(.006)	(.0057)	(.006)
Tenure^2	.0003	−.00019	.00005	−.0002
	(.0002)	(.00023)	(.00020)	(.0002)
Industry employment growth (predisplacement)	−.267	.122	−.123	.150
	(.181)	(.228)	(.176)	(.229)
Industry wage premium (predisplacement)	...	...	−.579	−.067
			(.109)	(.119)
Union coverage rate (predisplacement)	...	...	.034	.099
			(.108)	(.128)
Years of schooling	−.005	.005	−.001	.006
	(.005)	(.006)	(.005)	(.006)
Occupation:				
Manager	.079	.182	.071	.180
	(.047)	(.074)	(.047)	(.074)
Professional	.147	.095	.086	.095
	(.060)	(.093)	(.060)	(.094)
Technician	.215	.026	.158	.028
	(.064)	(.111)	(.638)	(.111)
Sales	.031	.153	−.012	.149
	(.046)	(.066)	(.045)	(.066)
Clerk	.061	.103	.038	.098
	(.052)	(.075)	(.052)	(.075)
Service worker	−.003	.114	−.007	.113
	(.048)	(.096)	(.047)	(.096)
Crafts worker	.085	.081	.057	.078
	(.039)	(.060)	(.039)	(.060)
Operative	.122	.096	.087	.091
	(.036)	(.059)	(.036)	(.059)
White	−.062	−.032	−.031	−.029
	(.044)	(.054)	(.043)	(.054)
Married (current)	.054	.027	.076	.029
	(.028)	(.033)	(.028)	(.033)
Years since displacement	.014	.013	−.007	.013
	(.011)	(.012)	(.011)	(.012)
Weeks unemployed (postdisplacement)	−.0048	−.0025	−.0049	−.0025
	(.0006)	(.0008)	(.0006)	(.0008)
Selection correction term	.189	−.134	−.146	−.158
	(.112)	(.102)	(.119)	(.111)
R^2	.150	.075	.167	.077
Mean of dependent variable	−.098	−.059	−.098	−.059
N	1,358	818	1,358	818

NOTE.—Estimates are from Heckman's two-stage procedure. This table replicates the analyses in table 3. However, workers displaced from blue-collar jobs in three-digit industries with union coverage rates over one-third are eliminated from the sample. This is an attempt to estimate the models on a sample of "nonunion" workers. It is not possible to perform such analyses directly because the DWS does not provide union status prior to displacement. See table 3 for further details. The occupation dummies do not control for predisplacement occupation. Rather, they measure net changes in occupational affiliation. For example, a person leaving a management job and entering a sales position would be coded as follows: management = −1; sales = 1; other occupations = 0. A person who does not change occupations receives zeros for all categories. Laborer is the omitted occupation.

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