Displacement Induced Joblessness

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Abstract:

Previous research examining the nonemployment of displaced workers suffers from methodological flaws which reinforce widely held but substantially incorrect views about the pattern of postseparation joblessness. In particular, adjustment difficulties have been overstated for nonwhites, long tenure workers, and those terminated during periods of high unemployment and underestimated for persons in manufacturing industries or white collar occupations.

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I. INTRODUCTION

Labor displacement is unlikely to occur randomly. When firms terminate only a portion of their workforce, they may attempt to layoff persons receiving high compensation relative to their productivity. Dislocation is also likely to be concentrated in economic sectors with weak or declining demand. To the extent that the econometrician is unable to observe these factors, the negative impact of permanent layoffs will tend to be overstated. This occurs because displaced individuals would experience higher levels of joblessness than random workers with the same observable characteristics, even had the job loss been avoided.

It is important to understand the consequences of involuntary mobility when attempting to design policies which will assist displaced workers without seriously jeopardizing economic efficiency.[1] This paper examines the extent and nature of postdisplacement joblessness. It improves on previous research by making a serious effort to control for unobserved, as well as observable, differences between dislocated and nondisplaced workers. The findings question a number of the stylized "facts" arising out of earlier studies.

Recent research examining the consequences of involuntary turnover has frequently utilized the *Displaced Worker Supplements* (DWS) to the January 1984, 1986, or 1988 *Current Population Surveys* (e.g, Addison and Portugal, 1987; Kruse, 1988; Podgursky and Swaim, 1987). Unfortunately, the DWS contains virtually no information on the previous employment conditions of nondisplaced persons, making it difficult to construct an appropriate control group with whom to compare involuntarily terminated individuals.[2] As an alternative, most studies have simply analyzed the dispersion of nonemployment experiences *within* the subsample of displaced workers. This method will generally not allow the effects of involuntary layoffs to be

***1.** Recent federal legislation pertaining to displaced workers includes the Worker Assistance Retraining and Notification Act and the Economic Dislocation and Worker Adjustment Assistance Act.*

***2.** The best attempt to construct a DWS comparison group is by Madden (1988). She matches workers displaced in 1983 to their counterparts not experiencing job loss in that year.*

accurately calculated. For example, there is no way of distinguishing whether the relatively extensive postdisplacement joblessness of nonwhites indicates more severe adjustment difficulties, or if minorities have lower employment levels (than whites) independent of displacement status.

A second body of analysis has used information from longitudinal data sets such as the *Panel Study of Income Dynamics and National Longitudinal Surveys* (e.g., Antel, 1985; Blau and Kahn, 1981; Ruhm, 1987). These studies typically estimate wage or employment regressions, controlling for a variety of observable individual and economy characteristics, with the impact of mobility ascertained from the coefficients on separation dummy variables. The resulting estimates of displacement effects will also be biased, however, if unobserved factors which influence joblessness or earnings are correlated with involuntary turnover. The typical (although not certain) result will be to overstate the adverse effects of dislocation.

The innovation of this study involves comparing individuals displaced during a base year to persons losing jobs at a later date. If the process determining layoffs is similar across time periods, subsequently displaced workers will have similar unobserved characteristics to those of base year job losers and the confounding effects of unobserved heterogeneity will be partially or completely accounted for.

II. EVALUATING THE EFFECTS OF DISPLACEMENT

Assume that joblessness is determined by

$$Z_n = X_0\alpha + S_0X_0\beta + \mu_n. \tag{1}$$

where Z is the natural log of weeks of nonemployment, X is a vector of observable characteristics, S a dummy variable indicating permanent layoffs, μ an error term, and the subscripts 0 and n refer to time periods $t + 0$ and $t + n$, respectively. The separation variable is interacted with the vector of observables because the effects of turnover may vary across population subgroups. The disturbance term can be decomposed into an individual fixed effect (f), a time-varying factor showing the effect of economic conditions (v_n), and a white noise error (ε_n) to give

$$\mu_n = f + v_n + \varepsilon\mu_n. \tag{2}$$

Regression estimates of (1) will be unbiased only if displacements occur independently of both the fixed and time-varying effects (i.e., $E(S_0 * f) = E(S_0 * v_n) = 0$). These orthogonality conditions are unlikely to hold. For example, firms will attempt to disproportionately terminate workers receiving high pay relative to their productivity. Even if they avoided loss of jobs at $t + 0$, these individuals would be likely to have elevated future joblessness because they are more likely to be temporarily laidoff or subsequently displaced. This implies that $E(S_0 * f) > 0$. Similarly, job losses will be concentrated in firms, industries, occupations, and local labor markets experiencing reductions in labor demand. These conditions would again be likely to reduce future employment levels, even in the absence of a displacement occurring at period zero. Unless complete information on economic conditions is available to the econometrician, $E(S_0 * v_n)$ will therefore also be positive.

The direction of bias for the individual elements of $\widehat{ß}$ is uncertain, depending on the relationship between X and S. On average, however, the adverse impact of displacements will be overstated. This can be seen by noting that, in a model where displacement affects only the intercept term,

$$Z_n = X_0\alpha + S_0 ß_0 + \mu_n , \qquad (3)$$

$\widehat{ß}_0$ will be biased upwards if $E(S_0 * f)$ or $E(S_0 * v_n)$ are positive. This occurs because the separation parameter combines the effects of the displacement (state dependence) with those of unobserved differences (heterogeneity).[3]

One method of separating the effects of state dependence and heterogeneity is to find a comparison group whose unobserved characteristics are closer to those of period zero job losers than are those of the random nondisplaced worker. Following Mincer (1986), this paper uses individuals who involuntarily terminate jobs in a subsequent time period.[4] To see how this is helpful, add the following structure to the error components:

$$f = S_0\gamma_0 + S_q\gamma_q + \lambda \qquad (4)$$

and

$$v_n = S_0\delta_0 + S_q\delta_q + \tau_n. \qquad (5)$$

with $q > n$ and λ and τ_n independent of X_0, S_0, and S_q.

Again consider the intercept-only model. Substituting (4) and (5) into (3) gives

$$\begin{aligned} Z_n = {} & X_0\alpha + S_0 (ß_0 + \gamma_0 + \delta_0) \\ & + S_q(\gamma_q + \delta_q) + (\lambda_n + \tau_n + \epsilon_n). \end{aligned} \qquad (6)$$

We can then estimate

$$Z_n = X_0\alpha + S_0 b_0 + (S_0 + S_q)c + u_n \qquad (6')$$

to obtain $\widehat{b}_0 = ß_0 + (\gamma_0 - \gamma_q) + (\delta_0 - \delta_q)$. An unbiased estimate of the displacement effect will be

3. *An additional problem arises if the sample is restricted to displaced workers. Since So equals one for all sample members, the model estimated is*

$$Z_n = X_0 (\alpha + ß) + \mu_n. \qquad (1')$$

Coefficients on the observable characteristics therefore confound the covariate and displacement effects. With a white noise error term, the coefficient on X_0 will understate (overestimate) the displacement effect if α is negative (positive).

4. *Mincer used t + 1 job changers to control for unobserved heterogeneity when calculating the wage effects of year t job separations. Analogously, Bound (1989) utilized rejected applicants to partially control for heterogeneity when evaluating the labor supply impact of disability insurance.*

TABLE 1.—SAMPLE MEANS BY DISPLACEMENT STATUS

Variable	Not Displaced	Displaced in Year Zero	Displaced in Year Three
Personal Characteristics			
Age	35.88 yrs.	33.56 yrs.	34.63 yrs
Experience	16.57	13.98	15.61
Education	11.78	11.01	11.10
Married	79.68%	75.26%	77.70%
Female	14.48	13.41	11.85
Nonwhite	33.14	43.01	37.11
Pre-Displacement Job Characteristics			
Tenure (years):			
< 1	17.70%	34.80%	29.44%
1–3	27.27	31.79	28.75
3–9	29.26	23.12	22.82
10–19	17.72	7.17	12.72
≥ 20	8.03	3.12	6.27
Blue Collar	52.31%	73.41%	68.64%
Manufacturing	27.85	29.36	29.44
Professional	25.32	12.49	15.16
Log Wage at Year $t - 2$ (1974 $)	5.176	4.967	5.039
N	2,776	864	574

provided if $\gamma_0 = \gamma_q$ and $\delta_0 = \delta_q$. If $\gamma_0 > \gamma_q$ or $\delta_0 > \delta_q$, heterogeneity will only be partially controlled for. There will still be an improvement over the conventional model, however, as long as γ_q (δ_q) is between zero and γ_0 (δ_0).

III. Data and Sample

Data for this study were obtained for heads of households from the *Panel Study of Income Dynamics* (PSID). Displacement status was ascertained for the five base years 1972 through 1976, with the base year hereafter referred to as period *t*, $t + 0$, or zero.[5] Household heads were included in the sample if they were between the ages of 21 and 50, at time t, and participated in the labor force during some part of each of years t through $t + 3$.[6] The inquiry is therefore restricted to prime-age workers with consistent attachments to the labor force.

The primary dependent variable analyzed below is the natural log of weeks of joblessness occurring during years *t* and $t + 1$. This measure includes periods of labor force nonparticipation, as well as unemployment, reflecting evidence that the distinction between the two states may be largely arbitrary for recently laidoff workers.[7] The comparison group includes individuals

5. A worker was defined to be displaced if they terminated a job because of a plant closure or layoff (excluding departures from seasonal or temporary positions) and failed to return to the original employer by the end of the second subsequent calendar year.

6. Respondents were not required to work during the survey years, only to participate in the labor force.

7. See Clark and Summers (1979) for discussion of this issue. Preliminary estimates were also obtained with unemployment as the dependent variable. They indicate shorter relative durations for groups, such as low tenure workers, who often temporarily depart the labor force following layoffs.

permanently losing jobs in year $t + 3$. Data were used for each base year in which the respondent was displaced (at either t or $t + 3$) and for up to one (randomly chosen) base period at which no job loss occurred.[8]

As discussed below, joblessness rises substantially in the year prior to permanent layoffs. The decision to use workers losing jobs at $t + 3$, rather than $t + 2$, as the comparison group is based on the belief that this increase is part of the displacement process itself, rather than representing the effects of time-varying factors which would also be experienced by equivalent nondisplaced workers. To the extent this judgment is incorrect, the effects of unobserved factors will not be fully accounted for.

Observable differences were controlled for by dummy variables indicating base year values of experience, education, marital status, race, sex, city size, tenure, industry, occupation. National economic conditions were also taken into account by including continuous measures of the adult male (over age 20) unemployment rate at periods $t + 0$ and $t + 1$.

IV. RESULTS

Displaced workers possess *observable* characteristics associated with heightened joblessness. They are younger, less experienced, less educated, and have lower seniority than nondisplaced workers. They are also more often unmarried or nonwhite, earn lower weekly wages, and are more likely to have worked in blue collar or manufacturing jobs (see table 1). Characteristics of persons displaced at $t + 3$ are typically intermediate between those of base year job losers and nondisplaced workers but closer to the former.

The possibility that displaced workers have unobserved characteristics associated with elevated joblessness was examined in two ways. First, a specification test, of the type described by Hausman and Taylor (1981), was implemented to reject (at the 1% level of significance) the null hypothesis that there were no differences between the fixed effects of terminated and nondisplaced workers. Second, regressions were estimated which indicate that base year job losers were out of work 10% to 12% (1.5 to 2 days per year) more than their counterparts in periods $t - 3$ and $t - 2$. These predisplacement differentials are attributed to unobserved factors. The size of the employment disparity almost doubles at time $t - 1$, which probably indicates errors in the reported timing of displacements and an increase in temporary layoffs immediately preceding permanent reductions in force. A similar pattern of predisplacement adjustments is also observed for workers terminated at $t + 3$.[9]

Regression estimates of the correlates of postdisplacement joblessness are presented in table 2. The coefficients show nonemployment differentials for the various demographic groups and are obtained from OLS estimates of

8. *If the respondent was displaced in both t + 0 and t + 3, they were randomly assigned to the treatment or control group.*

9. *An appendix detailing regression results and procedures of the specification test is available upon request*.

TABLE 2.—PREDICTED EFFECTS OF COVARIATES ON POSTDISPLACEMENT JOBLESSNESS

	Sample Excludes Nondisplaced Workers	No Controls for period $t + 3$ Displacements	Includes Controls for period $t + 3$ Displacements
INTERCEPT	2.212[c]	0.761[c]	1.005[c]
	(8.46)	(2.78)	(2.67)
EXPERIENCE (≤ 10 years)	0.103	−0.022	−0.084
	(1.36)	(0.28)	(0.73)
SCHOOL (≤ 12 years)	0.089	0.071	0.034
	(0.85)	(0.65)	(0.22)
MARRIED	−0.469[c]	−0.328[c]	−0.282[a]
	(4.16)	(2.66)	(1.70)
NONWHITE	0.178[b]	0.069	−0.130
	(2.36)	(0.85)	(1.14)
FEMALE	−0.166	−0.308[b]	−0.239
	(1.16)	(2.02)	(1.13)
CITY SIZE: > 100,000	7.5E-3	−0.054	8.0E-3
	(0.08)	(0.56)	(0.06)
< 25,000	−0.216[b]	−0.069	0.013
	(1.99)	(0.61)	(0.07)
BLUE COLLAR	1.44	−0.035	−0.138
	(1.36)	(0.32)	(0.88)
MANUFACTURING	−0.036	−0.126	0.122
	(0.46)	(1.50)	(1.03)
PROFESSIONAL	−8.5E-3	−0.182	−0.174
	(0.06)	(1.24)	(0.85)
TENURE 1–3 years	−0.091	−0.052	−0.236[a]
	(1.03)	(0.54)	(1.75)
4–9 years	−0.132	−0.114	−0.070
	(1.35)	(1.08)	(0.47)
10–19 years	−0.091	−0.119	−0.149
	(0.62)	(0.79)	(0.75)
≥ 20 years	0.253	0.220	−0.073
	(1.22)	(1.06)	(0.27)
UNEMPLOYMENT RATE Year 0	0.053[b]	0.021	9.6E-3
	(1.96)	(0.72)	(0.23)
Year 1	0.196[c]	0.062[b]	0.023
	(4.11)	(2.08)	(0.52)
	(1)	(2)	(3)

Note: Absolute value of t-statistics are in parentheses.
[a] Pr < 0.10.
[b] Pr < 0.05
[c] Pr < 0.01.

$$Z' = X_0a + S_0X_0b + \phi(S_0 + S_3)X_0c + u', \tag{7}$$

where $Z' = Z_0 + Z_1$, $u' = u_0 + u_1$, and ϕ is an indicator variable which is constrained to equal either zero or one, as explained below.[10]

Column 1 of the table displays estimates of $\hat{a} + \hat{b}$ obtained for the subsample of workers displaced at $t + 0$. This corresponds to analyses using the *Displaced Workers Supplements* or case studies, where the sample includes only job losers. Column 2 shows $\hat{b}$ for the full sample of displaced and nondisplaced workers, with ϕ constrained to zero. These estimates, which control for observable differences but not for unobservables, are equivalent to earlier estimates using panel data. Finally, column 3 presents the estimates for $\hat{b}$, with ϕ set to one. In this case, unobserved heterogeneity has been controlled for to the extent that persons displaced at t and t + 3 are similar.

10. *To test the robustness of this specification, gamma distributed accelerated failure time (AFT) equations were also estimated. The AFT model can easily account for censored nonemployment spells and has a more straightforward search-theoretic interpretation than the log-linear model. Results of the AFT and OLS regressions are virtually identical.*

Results in the first two columns are presented for comparison purposes only, to show the biases in earlier econometric work. Substantial differences between these coefficients and those in column 3 indicate that the errors are frequently large. For instance, when the sample is restricted to displaced workers (column 1), minorities are predicted to be out-of-work 19.5% (3.5 weeks) more than their white counterparts. The extra joblessness is not *caused* by the termination, however, but rather reflects the relative employment instability expected for all nonwhites. The column 3 coefficient suggests that the displacement-induced increase in nonemployment is actually larger for whites than for minorities, although the difference is not statistically significant.

The exclusion of nondisplaced workers also leads to a substantial overstatement of the impact of permanent layoffs on blue collar workers, persons with more than 20 years seniority, and those terminated during recessions. On the other hand, the elevated joblessness of males and manufacturing workers is somewhat underestimated. The biases are generally smaller when persons terminated in the base year are compared to random nondisplaced workers (compare columns 2 and 3). Nonetheless, the nonemployment differentials of blue collar and long tenure workers continue to be overstated, while those of whites and persons in manufacturing are still biased downwards.

V. CONCLUSION

Previous econometric estimates of postdisplacement joblessness are likely to be seriously flawed because they either incompletely control for differences between displaced and nondisplaced workers or exclude a control group altogether. At least where nonemployment is concerned, these methodological problems reinforce widely held but largely incorrect assumptions about who suffers the most from economic dislocation. Contrary to popular belief, involuntary separations do not have particularly adverse impacts on the employment levels of nonwhites, blue collar occupations, or long tenure workers. Nor do displacements have a more severe impact when they occur during recessions, although it is true that persons employed by firms facing declining demand are likely to experience heightened joblessness *whether or not a permanent termination is the end result*.

This does not deny the potential harm of economic dislocation. It is associated with high levels of joblessness for all types of workers and, even for the groups just mentioned, may result in large and lasting wage reductions.[11] There is also clear evidence that increased joblessness frequently precedes displacements. This presumably occurs as firms use temporary layoffs, in an unsuccessful attempt to prevent permanent reductions in force. These preseparation losses suggest that research which focuses exclusively on postdisplacement changes misses at least one important component of the adjustment problems faced by dislocated workers.

11. *See Ruhm (1991) for evidence on the permanence of wage changes.*

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