

Advance Notice, Job Search, and Postdisplacement Earnings

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Abstract:

Three to 5 years after job displacements, workers receiving the advance notice mandated by current law earn approximately 10% more than their nonnotified counterparts. This differential is not the result of firms systematically notifying persons with favorable reemployment prospects—early warnings are disproportionately obtained by individuals expected to earn relatively low wages in subsequent employment. It is not clear, however, whether prenotification has a causal effect. The notification differential may occur because the advance notice is frequently provided by employers offering other kinds of adjustment assistance such as job counseling, skill retraining, supplemental unemployment benefits, or outplacement assistance.

Article:

I. INTRODUCTION

Prenotification of plant closings and mass layoffs allows displaced workers to search for new positions while still employed at their old jobs and to switch investments from firm-specific to general human capital. Notified workers may therefore find new positions faster and be more productive in the subsequent employment than their nonnotified counterparts. Citing these potential benefits, the Worker Assistance and Retraining Notification Act (P.L. 100-379), which requires most firms to provide workers with 60 days advance notice of impending displacements, was enacted by Congress in 1988 and became effective in 1989.¹

Recent research, however, provides reason to question whether prenotification will yield the anticipated benefits. Using data from unemployment recipients in Arizona, Burgess and Low (1992) suggest that early notification only modestly increases predisplacement search—an extra week of notice increases predisplacement search by only 1-2 days for men and by less than 1 day for women. This may explain why prior notice leads to small or negligible reductions in postdisplacement joblessness. For instance, Ruhm (1992) finds that written announcements decrease the duration of nonemployment by less than 1 week.²

1. *Discussions of previous legislative efforts and the events leading to the passage of the current act are contained in Ehrenberg and Jakubson (1988) and Addison and Portugal (1991).*

2. *Earlier research (e.g., Addison and Portugal 1987; Ehrenberg and Jakubson 1988; or Swaim and Podgursky 1990) uncovered larger reductions in nonemployment. The benefits of written notice are overstated in this work, however, for two reasons. First, the data sources used fail to distinguish among written notice, verbal warnings, and worker expectations of job terminations, in the absence of any notice. Second, the endogeneity of voluntarily provided advance notice is not adequately accounted for. These problems are discussed in greater detail by Addison and Portugal (1992) and Ruhm (1992).*

Less attention has been paid to potential wage effects of prenotification. This is both surprising and distressing. As Ruhm (1991) and Topel (1990) have shown, whereas postdisplacement joblessness is largely transitory, the associated earnings changes are long lasting. Thus, the main potential economic benefit of early notice is to ameliorate the wage reductions that typically follow involuntary job changes. For instance, over a 10-year period, a 1% increase in discounted earnings would raise total wages by more than an initial 5-week reduction in joblessness.

Limited previous research on wage effects suggests that the earnings increases associated with advance notice are small or nonexistent. In the most comprehensive study to date, Ehrenberg and Jakubson (1988) conclude that "advance notice had very little impact on survey date . . . earnings of [displaced] workers" (p. 73).³ The analysis presented below challenges this finding. Workers receiving the type and duration of advance notice now mandated by law are shown to earn approximately 10% per week more than their counterparts, 3-5 years after the displacements.

These wage differentials do not occur because firms systematically provide lengthy written notice to individuals with favorable reemployment prospects. To the contrary, when they have the discretion to do so, employers disproportionately warn persons with relatively low expected subsequent earnings.

Despite these strong results, it is not clear that early notification causes, rather than merely correlates with, higher wages. The ambiguity occurs because there is no evidence of especially large gains for notified individuals obtaining new positions as the result of predisplacement search. Since the opportunity to engage in employed search is thought to be the major mechanism by which prenotification alleviates adjustment problems, this absence suggests that other factors may be important. In particular, firms voluntarily providing formal notice may also be relatively likely to offer other types of assistance to displaced workers (e.g., outplacement programs, job-search counseling, retraining, and extended unemployment benefits). This supplemental assistance may be responsible for the wage gains or operate in conjunction with the provision of the early notification.

The next section describes the data set, sample analyzed, and advantages of these data over those used in most previous research. Section III presents reduced-form estimates showing the relationship between advance notice and postdisplacement earnings. Section IV follows with three tests for endogeneity of voluntarily provided advance notice. Section V investigates how advance notice wage differentials vary across durations of nonemployment and uses this information to informally test whether the gains associated with early notification result from longer or more efficient pre-displacement search. Section VI concludes the paper.

3. Similarly, Podgursky and Swaim (1987) find that notification is associated with slightly higher wages for females working in white-collar occupations but has no effect for other individuals. Nord and Ting's (1991) recent study does find a positive wage benefit of advance notice. Their results are difficult to interpret, however, because they exclude individuals surprised by displacements and thus compare workers with written notice to counterparts anticipating their terminations in the absence of formal notice. The latter is a (nonrandomly) selected group of the nonnotified. (Addison, Fox, and Ruhm [1992] provide further discussion of these problems.) They also delete all persons displaced as the result of partial layoffs. As shown below, notification differentials are smaller for this group than for those losing jobs in plant closings.

II. DATA

Data for this study are obtained from the Displaced Worker Supplements (DWS) to the January 1988 and 1990 Current Population Surveys (CPS). Each DWS contains retrospective information on the previous job histories and labor market status of a nationally representative sample of workers suffering permanent job loss during the previous 5 years. Additional information on current labor force status is available from the regular monthly CPS.

The sample analyzed includes workers between the ages of 25 and 60 (at the survey date), who lost jobs as the result of a business failure, plant closure or relocation, or a layoff resulting from slack work or position or shift abolished. Persons terminating self-employment or jobs in agriculture, construction, or the armed forces are excluded. To eliminate transitory wage changes occurring during training or probationary periods, the sample used for most of the analysis is limited to persons displaced at least 2 full years before the survey date.⁴ Thus, respondents to the 1988 (1990) DWS are included if they lost jobs between 1983 and 1985 (1985 and 1987). This restriction also reduces the selection bias occurring because some workers remain out of work at the survey date.

The 1988 and 1990 DWS include three questions pertaining to advance notice. The first inquires whether the worker did "expect a layoff or had received advance notice of a layoff or plant or business closing." This question, which contains no information on either the type or timing of notice, was also incorporated in the 1984 and 1986 DWS and provided the only information on early notification available to researchers using these data sources (e.g., Addison and Portugal 1987; Ehrenberg and Jakubson 1988; Swaim and Podgursky 1990; Fallick 1991).⁵ Supplemental questions added to the 1988 and 1990 DWS inquire if the respondent had "been given written advance notice that the business would be closed or that he/ she would be laid off" and, if so, "how long before he/she was to be laid off did he/she receive that notice?" Responses permitted for the last question are: less than 1, 1-2, and greater than 2 months.⁶

Individual, job, and geographic characteristics are included as explanatory variables in the regression analysis (see App. A). In addition to data obtained from the DWS and CPS, these include regressors constructed using information from other sources. Covariates indicating residence in right- to-work states and three-digit industry and occupation unionization rates were added to proxy collective bargaining status on the predisplacement job. The state or standard metropolitan statistical area (SMSA) unemployment rate was also included, to account for differences in local labor market economic conditions.⁷

4. Sensitivity of the results to the exclusion of recently displaced workers is tested for below.

5. A few researchers have utilized non-DWS data. Folbre, Leighton, and Roderick (1984) used enterprise-level data for the state of Maine. More recently, Lazear (1990) has analyzed aggregate time-series cross-section data from 23 countries, and Deere and Wiggins (1989) have exploited a U.S. General Accounting Office random sample of establishments with mass layoffs during 1983 and 1984. Hamermesh (1989) provides a useful summary and evaluation of recent research on displaced workers, and Addison and Portugal (1991) review the literature on advance notice.

6. Nord and Ting (1991), Addison and Portugal (1992), and Ruhm (1992) have used the 1988 DWS to investigate postdisplacement joblessness. Nord and Ting have also briefly used this data set to examine wage effects. To my knowledge, there has been no detailed analysis of postdisplacement earnings using the 1988 or 1990 DWS.

7. The DWS identifies geographic location at the survey date, not at the time of displacement. Since approximately one-fifth of the sample changes locations, estimates of local labor market effects could be biased. To the extent that moves typically involve departing depressed areas in favor of locations with low unemployment, the impact regional conditions will be understated. Ehrenberg and Jakubson (1988) and Howland and Peterson (1988) compare estimates with and without movers included in the sample and conclude that these biases are quite small.

After the aforementioned exclusions, the sample consists of 3,650 individuals, 2,732 of whom were reemployed at the survey date. Forty-six percent of respondents reported being surprised by their job terminations and 38% anticipated permanent layoffs in the absence of written notice. The latter group is hereafter described as informally notified. While written notice was received by 16% of displaced workers, fewer than 6% obtained the 2 months of formal notice now required by law. More than two-thirds of respondents expecting their jobs to end did so without being formally notified. This makes it doubtful that previous research using the general advance notice question reveals much about the impact of lengthy written notification.

III. REDUCED-FORM ESTIMATES

This section presents estimates of the reduced-form earnings equation

$$W = X\alpha + N\beta + \varepsilon, \quad (1)$$

where W is the natural log of real weekly earnings, X is a vector of covariates, N a vector of dummy variables indicating specified types of advance notice, and ε is the regression disturbance term.

Estimates of equation (1) will indicate the effect of legally mandated advance notice under only a restricted set of circumstances. Two potential problems deserve particular attention, when considering the results of prior research. First, there is no reason to believe that persons responding affirmatively to a general question asking whether the job loss was expected will have the same subjective *ex ante* probabilities of displacements as if formal notice had been received.⁸ Data on formal notice, available in the 1988 and 1990 DWS, help to solve this problem. A second and more fundamental issue is that, even when adequate information on advance notice is available, the expected value of the error term may vary with notification status (i.e., $E(\varepsilon | N = 0) \neq (E \varepsilon | N = 1)$), with the result that the prenotification estimate could be biased either upward or downward.

There are several reasons why N and ε could be correlated. Since post-displacement wages are observed only for persons employed at the survey date, sample selection bias may exist. Ignoring nonrandom reemployment, the notification estimate will be biased if advance notice is (1) systematically provided to workers with low or high potential wages (the endogeneity problem) or (2) accompanied by other types of assistance that raise subsequent earnings (the problem of spurious correlation). These issues are the focus of Sections IV and V.

A. Demographic Covariates

The first column of table 1 shows variable means for the full sample (unbracketed) and for the subsample reemployed at the survey date (bracketed). The second and third columns display regression coefficients and associated *t*-statistics on the demographic covariates obtained by estimating equation (1), which also includes the vector of advance-notice variables.

The results are completely standard and so warrant only brief attention. Postdisplacement earnings are relatively high for married workers, whites, males, persons displaced by plant

8. For instance, one logical rule would be to respond that the termination was expected (unexpected) if the subjective probability was greater (less) than 50% over a specified time period. If mandated formal notice raises this probability closer to one, the gains to mandatory prenotification will be understated.

closings or residing in areas with low unemployment, and those with the most time to recover from the loss of jobs. Wages increase with prior earnings and health insurance coverage but decline with the proxies for unionization. Persons previously working part-time receive a positive wage differential, which is not surprising since the predisplacement wage has been controlled for. For the same reason, we expect and find a positive (negative) relationship between subsequent earnings and education (seniority). *Ceteris paribus*, survey date earnings are highest for persons with intermediate levels of work experience.

Table 1
Means and Reduced-Form Regression Coefficients for Variables Other than Advance Notice

Variable	Variable Mean	OLS Estimate	
		Coefficient	t-Statistic
Experience (in years)	19.30 [18.71]	.007	1.65
Experience Squared (in years)	473.50 [444.50]	-2.9E-4	2.90
Education (in years)	12.98 [13.15]	-.038	1.57
Education Squared (in years)	174.22 [178.65]	.003	3.17
Previous Tenure (in years)	5.30 [5.21]	-.003	.59
Previous Tenure Squared (in years)	68.39 [64.41]	-1.6E-4	.99
Married (in %)	65.26 [65.48]	.003	.14
Female (in %)	42.85 [40.03]	-.235	10.96
Black (in %)	9.45 [9.00]	-.043	1.29
Other Nonwhite (in %)	2.00 [1.94]	-.198	2.92
No. of Children	1.73 [1.71]	-4.5E-4	.05
Previous Part-Time Job (in %)	7.32 [5.97]	.005	.11
Previous Health Insurance (in %)	67.34 [69.95]	.074	3.05
Displaced by Plant Closing (in %)	58.16 [57.50]	.038	1.96
Right-to-Work State (in %)	34.03 [35.14]	-.097	4.87
Area Unemployment Rate (in %)	7.43 [7.32]	-.024	5.27
Industry Unionization Rate (in %)	18.29 [18.25]	-.002	2.77
Occupation Unionization Rate (in %)	20.02 [19.98]	-.001	2.03
Survey Date = 1990 (in %)	47.89 [49.20]	-.056	2.86
Displaced 5 Years Previous (in %)	29.51 [30.09]	.099	4.09
Displaced 4 Years Previous (in %)	33.01 [32.76]	.016	.71
Log of Previous Weekly Wage	5.73 [5.77]	.501	23.60

NOTE.—Unbracketed variable means are for the full sample ($N = 3,650$); bracketed means are for the subsample of respondents reemployed at the survey date ($N = 2,732$). Regression coefficients show the effects of the parameters on the natural log of real postdisplacement weekly wages, estimated using ordinary least squares (OLS). Wage equation also includes the regressors UNWRIT, WRIT1, WRIT2, and WRIT3. Absolute value of t-statistics are presented.

B. Advance Notice

We next discuss the relationship between prenotification and survey date earnings. The four advance notice dummy variables UNWRIT, WRIT1, WRIT2, and WRIT3 are defined to indicate, respectively, informally notified respondents and persons receiving less than 1, 1-2, and greater than 2 months written notice. One shortcoming of the DWS is that it fails to ascertain whether formally notified workers anticipated their job terminations prior to receiving written notice. Thus it is not clear whether the appropriate control group is persons surprised by displacements or those expecting the loss of jobs in the absence of written notice.

Table 2 presents results for three alternative comparisons. In model 1, the control group is all persons not receiving formal announcements, whether they are nonnotified or informally notified.⁹ This specification is appropriate if the receipt of informal and written notice are statistically independent events.¹⁰ The second model controls for both written and informal notice, implying that the formally notified are contrasted with counterparts surprised by the loss of jobs. Since the latter group is likely to possess relatively poor information about labor market conditions (and hence inferior reemployment prospects), the resulting advance-notice coefficients should be larger than in the previous model. Model 3 shows the difference in the coefficients on formal and informal notice. If workers anticipating job departures in the absence of written notice are unusually well informed about labor market conditions, this specification will produce the smallest notification effect.

Table 2
Reduced-Form Estimates of the Effects of Advance Notice
on Postdisplacement Wages

Advanced Notice Regressor	Variable Mean	OLS Estimate		
		Model 1	Model 2	Model 3
Informal notice (UNWRIT)	38.36 [38.36]022 (1.08)	.022 (1.08)
Written notice: <1 month (WRIT1)	5.34 [5.53]	-.041 (1.00)	-.031 (.73)	...
Written notice: 1-2 months (WRIT2)	4.85 [4.90]	-.001 (.03)	-.009 (.20)	...
Written notice: >2 months (WRIT3)	5.56 [5.86]	.089 (2.19)	.100 (2.38)	...
WRIT1 - UNWRIT	-.053 (1.24)
WRIT2 - UNWRIT	-.013 (.30)
WRIT3 - UNWRIT078 (1.86)

NOTE.—Unbracketed variable means are for the full sample ($N = 3,650$); bracketed means are for the subsample of respondents reemployed at the survey date ($N = 2,732$). Coefficients show effects of parameters on the natural log of real postdisplacement weekly wages, estimated using ordinary least squares (OLS). Regressions include the full set of demographic, industry, occupation, and geographic covariates. Absolute values of t -statistics are in parentheses.

9. The full set of demographic, industry, occupation, and geographic covariates are included in these regressions and throughout the remainder of the article.

10. As discussed in Sec. IV, this condition is unlikely to hold.

Workers receiving lengthy written notice earn substantially more than their counterparts at the survey date. The wage premium associated with 2 or more months of formal notice ranges between 8.1% and 10.5%, depending on the comparison group used.¹¹ In contrast, the coefficients on UNWRIT, WRIT1, and WRIT2 are negative or only slightly positive, which explains why strong notification effects have not been revealed in previous work using broad definitions of advance notice.¹²

The wage differentials associated with extended written notice are sizable and could indicate an important benefit from mandatory advance notice. The next two sections attempt to determine whether early announcements cause elevated earnings or are merely associated with them, due to endogeneity bias or spurious correlation.

IV. ENDOGENOUS PROVISION OF ADVANCE NOTICE

Prior to 1989, only four states (Hawaii, Maine, South Carolina, and Wisconsin) required prenotification of impending job terminations.¹³ (Connecticut also stipulates that employers must continue health benefits for 120 days.) The reduced-form models estimated above therefore indicate the relationship between voluntarily provided advance notice and survey date earnings. If firms notify employees on a nonrandom basis, these estimates may poorly indicate the impact of mandated prenotification. For example, employers may try to minimize premature quits by disproportionately notifying workers with poor reemployment prospects. In this case, early warnings will be associated with only small increases (or even reductions) in pay. Alternatively, firms may offer advance notice in combination with other forms of adjustment assistance (e.g., retraining) that lead to higher postdisplacement earnings. If so, the reduced-form estimates will overstate the benefits of prenotification.

Three tests for the direction of endogeneity bias are presented below. The results of several "natural experiments" are considered first. Second, a search-theoretic perspective is used to show how endogeneity bias might influence the reemployment outcomes of formally notified workers. Third, a treatment/effects model is estimated, with firms assumed to provide the treatment (written notice) when the costs of failing to do so exceed the associated benefits.

Each of the three methods indicates that formal notification is disproportionately provided to workers with inferior reemployment prospects (i.e., $E(\epsilon|N = 1) < E(\epsilon|N = 0)$), which suggests that the reduced-form estimates of Section III understate the size of the written notice wage premium. The downward bias could be offset, however, if unobserved confounding factors

11. The predisplacement wage is included in the equation to control for heterogeneity not captured by the observable characteristics. Two alternative specifications were also estimated. In the first, the previous wage was excluded; in the second, changes in wages were used as the dependent variable. These specifications constrain the coefficient on previous earnings to equal zero and one, respectively. The wage differential associated with WRIT3 increases slightly when previous wages are excluded (to 11.0%, 11.3%, and 10.5% in models 1, 2, and 3) and falls somewhat when the dependent variable is wage changes (to 7.8%, 9.7%, and 5.8%).

12. For example, using a general measure, Ehrenberg and Jakubson (1988) uncover statistically insignificant prenotification differentials ranging from -3.2% to 4.6%.

13. The laws were weak in some states (e.g., a \$500 fine per employer for violations in South Carolina) but strong in others (e.g., 45 days written notice plus severance pay equaling the difference between the worker's average weekly wage and 4 weeks of unemployment benefits in Hawaii). Further information on state laws is available in Abbey (1989) and Addison and Portugal (1991).

which ease the adjustment process of dislocated workers are positively correlated with voluntarily provided advance notice. This possibility is the focus of Section V.

A. "Natural" Experiments

One method of reducing the endogeneity problem is to identify groups for whom advance notice is provided on a more random basis than for all displaced workers. This provides a natural experiment for the endogeneity of voluntarily provided prenotification. Results of three such natural experiments are presented below.

In the first case, workers involved in plant closings are compared to those losing jobs due to partial layoffs. There are several reasons to expect advance warnings to be provided on a more random basis in the former case than in the latter. First, when the entire plant is being closed, it will be difficult to selectively inform workers of impending displacements and virtually impossible to provide written notice to only a few employees (unless firms can credibly lie about the nature of the reduction in force). Conversely, when only a portion of employees are terminated, nonnotified workers may find it difficult to determine whether they will be affected. Employers also have discretion in deciding whom to lay off and may be able to combine the layoff and notification choices in ways that make the notified group even less representative of the population of job losers.¹⁴

The second natural experiment contrasts workers in highly unionized industries and occupations to their counterparts in sectors with low union densities. Contractual provisions requiring advance notice are more common for union than nonunion workers, which implies that unionized firms will be less able to notify on a discretionary basis.¹⁵ Even where collective bargaining contracts do not mandate advance notice, unions are likely to pressure firms to supply it. Furthermore, the ability of unions to disseminate information will make it more difficult for employers to notify selectively. In the analysis below, workers are classified as employed in sectors with low union densities if their three-digit (predisplacement) industry and occupation unionization rates are both below the sample average (18.3% and 20.0%, respectively) and as working in highly unionized sectors if both percentages are above the mean.

The enactment of state laws requiring advance notice or the continuation of benefits prior to the passage of federal legislation is assumed to proxy an environment where employers are strongly encouraged to prenotify workers and to be associated with widespread and less discretionary provision of advance notice.¹⁶ The final natural experiment therefore involves contrasting workers residing in states with employment regulations (i.e., Connecticut, Hawaii, Maine, South Carolina, and Wisconsin) to peers in states without similar legislation. Since notification periods are substantial in three of the four states with mandates (45 days in Hawaii and 60 days in Maine and Wisconsin), this comparison is likely be most revealing when considering lengthy written notice.

14. Gibbons and Katz (1991) present evidence that displacements are less random for partial layoffs than plant closings.

15. Brown (1987) shows that unionized and nonunionized establishments are equally likely to provide some type of advance notice, but unionized employers provide longer and more specific notice. This natural experiment should therefore focus on written notice of medium or long durations.

16. All state advance notice laws have exemptions; thus, even in these locations, some displaced workers will not receive notice.

Results for the three natural experiments, summarized in table 3, suggest that voluntary notice is disproportionately provided to workers with low expected postdisplacement wages. The evidence is especially convincing when considering lengthy formal notification. Among the three groups notified on a relatively random basis—those involved in plant closings, working in highly unionized industries and occupations, or living in states with employment dismissal regulations—the wage differential associated with 2 months written notice is 18.2%, 25.6%, and 38.1%, respectively. For the three categories likely to be notified on a more discretionary basis—workers displaced by partial layoffs, working in sectors with low union densities, or residing in states without dismissal laws—the corresponding earnings premia are a much smaller—3.2%, 3.4%, and 9.9%.

Table 3
“Natural Experiments” for Endogeneity of Advance Notice

Subgroup	Informal Notice (UNWRIT)	Advanced Notice Coefficient Written Notice (Months)			N
		<1 (WRIT1)	1–2 (WRIT2)	>2 (WRIT3)	
All workers	.022 (1.08)	−.031 (.73)	.009 (.20)	.100 (2.38)	2,732
Plant closings	6.9E−5 (.00)	.033 (.53)	.052 (.91)	.167 (3.24)	1,571
Partial layoffs	.062 (1.87)	−.075 (1.29)	−.064 (.89)	−.033 (.45)	1,161
High union density	.076 (1.90)	−.051 (.72)	.059 (.69)	.228 (3.18)	716
Low union density	.003 (.08)	.014 (.18)	−.022 (.30)	.033 (.42)	1,183
State dismissal law	.077 (.74)	−.394 (1.97)	−.255 (1.31)	.323 (1.86)	132
No state dismissal law	.022 (1.05)	−.013 (.30)	.031 (.68)	.094 (2.16)	2,600

NOTE.—Coefficients show effects of advance notice on the log of real postdisplacement weekly wages, estimated using ordinary least squares. Regressions include the full set of demographic, industry, occupation, and geographic covariates. Absolute values of *t*-statistics are in parentheses. Union density is “high” if the industry unionization rate is greater than 18.3% and the occupation unionization rate exceeds 20.0%. Union density is low if the industry unionization rate is below 18.3% and the occupation unionization rate is less than 20.0%. States with dismissal regulations include Connecticut, Hawaii, Maine, South Carolina, and Wisconsin.

Conversely, the coefficients on WRIT1 and WRIT2 never differ significantly across comparison groups and the pattern of disparities is ambiguous. For example, intermediate (1–2 months) written notice appears most beneficial for workers displaced by plant closings or leaving unionized sectors, where warnings are provided relatively randomly, but also for persons residing in states without dismissal regulations, where firms exercise greater discretion in notification decisions. Given these results and the small size of the associated reduced form coefficients, a reasonable working assumption is that short and intermediate periods of written notice have no impact on subsequent wages.

B. Nonemployment Survival Rates

The natural experiments suggest that lengthy formal notice is systematically provided to workers with poor reemployment prospects. Further evidence is provided by a comparison of the nonemployment durations of notified and nonnotified workers.

Following Lancaster (1979), we can specify the probability that an individual leaves joblessness over the short interval $[t, t + dt]$ by

$$h(t)dt = \{1 - G[\tilde{w}(t)]\} r(t)dt,$$

where $h(\bullet)$ is the hazard rate, $G(\bullet)$ the distribution function of wage offers, $r(\bullet)$ the rate at which wage offers are received, and \tilde{w} , the reservation wage (optimal stopping point), which is obtained from a model of search.

The hazard rate, expressed in terms of the underlying density and distribution functions, $f(\bullet)$ and $F(\bullet)$, is

$$h(t) = \frac{f(t)}{1 - F(t)} \quad (2)$$

and, integrating (2), the probability that search (S) exceeds T periods is

$$\text{pr}(S > T) = 1 - F(T) = \exp - \int_0^T h(v)dv = \exp[-H(T)], \quad (3)$$

where $H(\bullet)$ is the integral of the hazard function.

If nonnotified workers do not begin searching until the displacement occurs and notified individuals initiate search as soon as the job termination is announced (and do so with the same intensity as if they were already out of work), the probability that the nonemployment duration exceeds T periods, conditional on receiving N periods of advance notice, is

$$\text{pr}(S > T/N) = 1 - F(T + N) = \exp[-H(T + N)]. \quad (4)$$

The difference between (3) and (4) represents the maximum decrease in survival probabilities associated with N periods of prenotification. Smaller reductions occur if (1) notified workers search less intensively for new jobs or have higher reservation wages during the period of predisplacement search or (2) some nonnotified individuals anticipate displacements and begin searching for new positions while still employed at the old job. In the extreme case, prenotification would have no effect on survival rates.

The search model therefore predicts that advance notice should either speed or have no effect on reemployment. There is no theoretical reason to expect prenotification to increase spell durations; evidence that it does so provides a clear indication that early warnings are disproportionately provided to workers with low reemployment hazards. If hazard rates and expected earnings are positively correlated, reduced-form wage regressions will then underestimate the earnings differential associated with advance notice.

Figure 1 displays the difference in cumulative reemployment probabilities $F(\bullet)$, at various durations of joblessness, for notified workers as compared to individuals surprised by the loss of jobs. Positive (negative) percentages imply higher (lower) rates of reemployment for persons receiving notice. For example, 15.0% of nonnotified individuals are jobless for 1 week or less, before finding a new position. The corresponding percentages are 20.8% for the informally notified and 14.4%, 15.3%, and 21.7%, respectively, for workers with less than 1, 1-2, and over 2 months written notice. The difference in cumulative reemployment probabilities, compared to

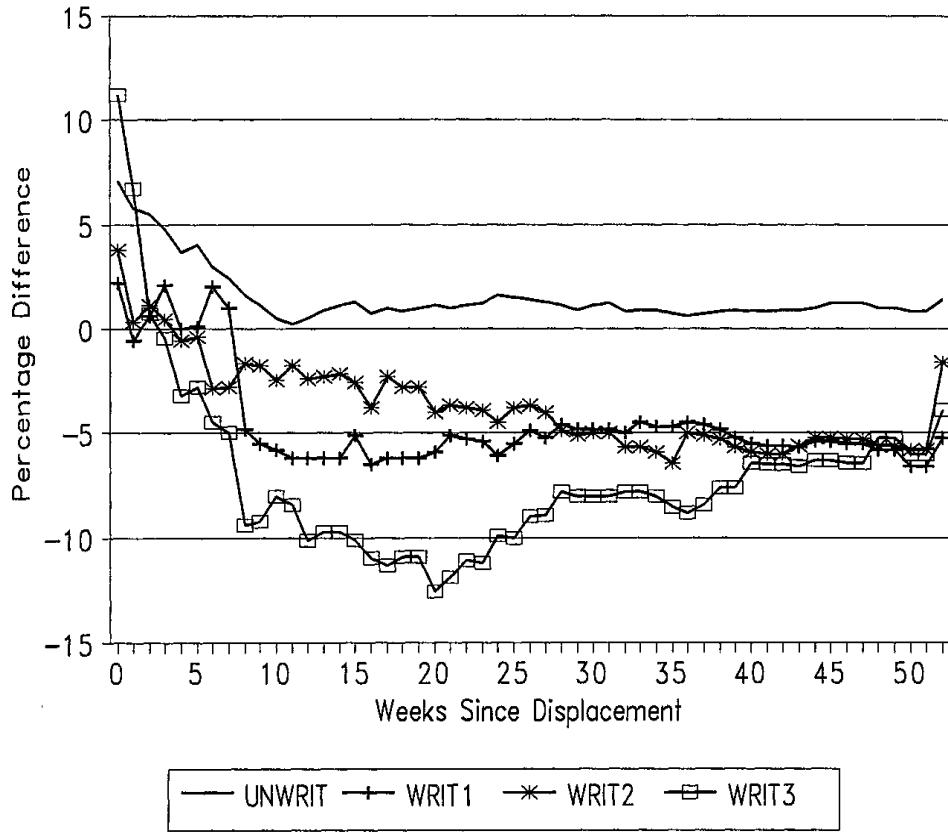


FIG. 1

workers failing to anticipate displacements, therefore is 5.8%, —0.6%, 0.3%, and 6.7% for the four types of advance notice.

Reflecting their more extensive opportunities for predisplacement search, notified workers are more likely than nonnotified individuals to avoid joblessness or be out of work for only a short period of time. Among the formally notified, however, this does not translate into a long-term advantage. Persons obtaining written warnings are more likely to remain jobless beyond 7 weeks, and the propensity to experience extended joblessness is particularly pronounced for individuals with lengthy written notice. For example, this group is 12.6% more likely than persons surprised by displacements to remain jobless for more than 20 weeks (50.7% vs. 63.3%).

A set of probit models was estimated to investigate whether the patterns observed in figure 1 result from differences in observable characteristics. In each equation, the dependent variable was set to one (zero) if joblessness exceeded (was less than or equal to) the specified number of weeks and the full set of explanatory variables was controlled for. The resulting coefficient estimates, which are summarized in Appendix table A1, are entirely consistent with figure 1. Lengthy written notice is associated with higher probabilities of both quick reemployment and

extended joblessness. The notification effect is maximized at 20 weeks and is substantial between 15 and 45 weeks. Brief periods of advance notice also tend to be correlated with relatively high joblessness survival rates, while informally notified individuals become reemployed relatively rapidly.

These results indicate that firms systematically supply lengthy written notice to workers with low reemployment hazards. The relationship between nonemployment durations and postdisplacement earnings was next examined by estimating the reduced-form wage equation, with weeks of joblessness included as a supplementary covariate. These regressions (not shown) reveal that each additional week of nonemployment is associated with a statistically significant 0.3% reduction in survey date earnings.¹⁷ Thus, formal notice is provided to workers with slow rates of reemployment who, on average, have poor earnings prospects. If the relationship between the speed of reemployment and subsequent earnings is independent of notification status, reduced-form wage equations, such as those estimated in Section III, therefore will underestimate the wage differentials obtained by formally notified workers.¹⁸

C. Sample Selection and Treatment/Effects Estimates

This section provides several types of additional information on the nature of the endogeneity bias. It first examines whether the estimated notification effects suffer from reemployment selection bias, which could be problematic since wage data are available only for respondents who are working at the survey date. Next we consider whether the findings are sensitive to the exclusion of recently displaced workers from the analysis. Finally, a treatment/effects framework is used to model the process determining who receives early notice.

Column 1 of table 4 repeats results of the ordinary least squares (OLS) wage equation originally displayed in model 2 of table 2. Column 2 provides corresponding estimates corrected for reemployment selection bias using Heckman's (1979) two-step method. This involves including the inverse Mills ratio (λ_w) from a first-stage probit model as a supplemental regressor in the second-stage wage equation. In addition to the full set of demographic and advance-notice variables, the probit reemployment equation controls for the average state weekly unemployment benefit paid in 1986.¹⁹

The coefficient on λ_w provides weak evidence of a positive correlation. The coefficient on λ_w provides weak evidence of a positive correlation between potential wages and reemployment probabilities. The selection effect is imprecisely measured, however, and the null hypothesis of no selection bias cannot be rejected. The two-step estimates also imply slightly larger earnings

17. Complementary evidence indicating that well-paid individuals have relatively high rates of reemployment at the survey date is provided in Sec. IVC.

18. Evidence casting doubt on the assumption of independence is presented and discussed in Sec. V.

19. Whereas unemployment insurance benefits have either a small effect on subsequent wages (Ehrenberg and Oaxaca 1976; Burgess and Kingston 1976) or no effect at all Classen (1977), virtually all studies find a strong positive association between benefit levels and unemployment durations. Thus, it may be reasonable to exclude the UI benefit from the wage equation, while including it in the selection equation. Jones (1988) has previously used the assumption that UI influences reservation wages but has no independent impact on reemployment outcomes as an identifying restriction when testing the predictions of search theory.

Table 4
Advance-Notice Estimates Using Alternative Samples and Specifications

Regressor	Model 1	Model 2	Model 3	Model 4
Informal notice (UNWRIT)	.022 (1.08)	.029 (1.01)	.023 (1.11)	.029 (1.44)
Written notice: <1 month (WRIT1)	−.031 (.73)	−.015 (.26)	−.024 (.56)	.030 (.94)
Written notice: 1–2 months (WRIT2)	.009 (.20)	.003 (.05)	.012 (.28)	.017 (.51)
Written notice: >2 months (WRIT3)	.100 (2.38)	.130 (2.05)	.095 (2.24)	.104 (3.18)
λ_w689 (1.08)
Estimation technique	OLS	two-stage	OLS	OLS
Area unemployment rate	included	included	excluded	excluded
Recently displaced workers	excluded	excluded	excluded	included

NOTE.—Regression coefficients show the effects of the parameters on the natural log of real postdisplacement weekly wages. Absolute values of *t*-statistics are in parentheses. The two-stage estimates include the inverse Mills ratio (λ_w) from a probit reemployment equation estimated by maximum likelihood. The selection equation contains the same regressors as the wage equation plus a variable indicating the natural log of the average state weekly UI benefit paid in 1986. "Recently" displaced workers are defined as persons losing jobs in the 2 calendar years immediately preceding the survey date. Sample sizes are 2,732 (4,612) when recently displaced workers are excluded (included).

differentials associated with extended written notice than were obtained by OLS (13.9% vs. 10.5%). Thus, correcting for reemployment selection bias either has no effect or slightly raises the estimated benefits of formal notice.

Columns 3 and 4 examine whether the notification estimates are sensitive to the exclusion of recently displaced workers. The regression in model 3 is identical to that in model 1 except that the area unemployment rate is not controlled for. This exclusion has no effect on the notice coefficients. Model 4 estimates the same wage equation as model 3 but adds to the sample respondents losing jobs during the 2 calendar years immediately preceding the survey date (e.g., persons terminated in 1988 and 1989 for the 1990 DWS). A comparison of columns 3 and 4 demonstrates that the addition of recently displaced individuals has virtually no impact on the estimated notification effects.

To provide more precise information on the nature of the endogenous notification decision, a treatment/effects model of advance notice is specified and estimated next. Firms are assumed to offer workers the "treatment" (advance notice) if the net cost (cost minus benefit) of failing to do so is positive. Thus, notification status is

$$N = \begin{cases} 1 & \text{if } C = Z\delta + \mu \begin{cases} > 0 \\ \leq 0 \end{cases} \end{cases} \quad (5)$$

where C is the net cost of withholding notice, Z is a vector of covariates which affect net costs, N indicates advance notice, and μ is an error term.

Rewriting equation (1), postdisplacement wages are specified by

$$W = X\alpha + N\beta + \varepsilon, \quad (6)$$

where W , X , and ε are as previously defined. If μ and ε are distributed bivariate normal with mean zero and covariance $\sigma_{\mu\varepsilon}$,

$$E(W | N = 0) = X\alpha + E(\varepsilon | N = 0),$$

and

$$E(W | N = 1) = X\alpha + \beta + E(\varepsilon | N = 1),$$

where

$$E(\varepsilon | N = 0) = \frac{\sigma_{\varepsilon\mu}}{\sigma_\mu} \left| \frac{-\phi(\cdot)}{1 - \Phi(\cdot)} \right|$$

and

$$E(\varepsilon | N = 1) = \frac{\sigma_{\varepsilon\mu}}{\sigma_\mu} \left| \frac{\phi(\cdot)}{\Phi(\cdot)} \right|,$$

for $\phi(\cdot)$ and $\Phi(\cdot)$ the probability distribution function and cumulative distribution function of the standard normal distribution evaluated at $Z\delta$.

Consistent estimates for the treatment/effects model are obtained using a two-stage technique. In the first stage, the probit equation implied by (5) is estimated to obtain $\hat{\phi}(\cdot)$ and $\hat{\Phi}(\cdot)$. Next, the wage equation

$$W = X\alpha + N\beta + \lambda\gamma + \eta \quad (7)$$

is estimated, with $\lambda = -\hat{\phi}(\cdot)/[1 - \hat{\Phi}(\cdot)]$ for nonnotified individuals and $\lambda = \hat{\phi}(\cdot)/\hat{\Phi}(\cdot)$ for persons receiving notice. The reported standard errors are corrected for heteroscedasticity, and the coefficient $\hat{\gamma}$ provides an estimate of $\sigma_{\varepsilon\mu}/\sigma_\mu$.²⁰

Employers failing to provide advance notice may be subject to costs in the form of damage to their reputations, productivity reductions by continuing employees, and legal expenses. The costs vary across geographic locations (e.g., being higher in states with mandatory or voluntary programs encouraging prenotification) and, as evidenced by the recent federal legislation, have probably increased over time. State and time dummy variables are included in the probit notification equation to capture these variations. Industry and occupation unionization rates are similarly incorporated, since contractual advance notice provisions are most common in unionized firms and unions increase the information available to workers. The reason for displacement is also controlled for because it is more difficult (and hence more costly) to conceal information on plant closings than on selective layoffs.

20. Further information on the treatment/effects model is provided by Greene (1990).

Firms also obtain benefits from withholding advance notice. Chief among these is likely to be the reduction in premature quits among workers with favorable employment alternatives.²¹

Premature quits will be relatively uncommon (and so notification costs relatively low) for workers receiving wage premia or generous fringe benefits, with substantial investment in firm-specific skills, or those losing jobs in depressed local labor markets—since these individuals will have difficulty finding new positions that are comparable or superior to those they are departing. Such differences are accounted for by including the area unemployment rate and a variety of demographic and predisplacement job characteristics in the first-stage probit equation.²²

Two specifications of the treatment/effects model are estimated below. In the first, the treatment includes written notice of any duration (WRITTEN). Persons expecting job terminations in the absence of advance notice are deleted from this analysis to avoid a more complicated choice process which includes the possibility of informal notice. Thus, formally notified individuals are contrasted with counterparts surprised by the loss of jobs. In the second specification, the choice is between lengthy written notice (WRIT3) versus no notice of any sort, with workers obtaining briefer formal notification (WRIT1 and WRIT2) or informal notice excluded from the sample. The first two columns of table 5 show results for the first-stage probit notification equation; the third and fourth columns display coefficients from the second-stage wage equation.

Table 5
Treatment/Effects Estimates of Written Notice and Postdisplacement Earnings

Regressor	Probit Estimates on Notice		Survey Date Earnings	
	Any Written (1)	Written >2 Months (2)	Any Written (3)	Written >2 Months (4)
Experience	-.008 (.50)	.037 (1.39)	.004 (.64)	-8.4E-5 (.01)
Experience Squared	-6.2E-5 (.16)	-8.8E-4 (1.50)	-.002 (1.78)	-1.3E-4 (.94)
Education	-.027 (.28)	-.107 (.73)	-.030 (.92)	-.024 (.69)
Education Squared	.003 (.77)	.008 (1.42)	.003 (2.05)	.002 (1.67)
Previous Tenure	.057 (3.47)	.064 (2.69)	-.001 (.23)	-.001 (.22)
Previous Tenure Squared	-.001 (1.91)	-9.0E-4 (1.08)	-6.7E-5 (.31)	-2.0E-4 (.83)
Married014 (.47)	.022 (.66)
Female	.262 (3.25)	.565 (4.44)	.223 (7.76)	-.203 (6.48)
Black	-.122 (.95)	-.355 (1.71)	.014 (.34)	.012 (.27)
Other Nonwhite	-.310 (1.18)	-.288 (.67)	-.240 (2.92)	-.292 (3.22)
No. of Children002 (.14)	.008 (.68)
Previous Part-Time Job	.289 (1.63)	.678 (2.30)	.033 (.57)	.040 (.64)
Previous Health Insurance	.504 (5.24)	1.023 (5.34)	.055 (1.57)	.043 (1.23)
Displaced by Plant Closing	.281 (3.84)	.495 (4.31)	.067 (2.53)	.038 (1.35)
Right-to-Work State	-.077 (3.05)	-.108 (3.90)
Area Unemployment Rate	-.012 (.39)	.058 (1.25)	-.025 (4.27)	-.026 (4.13)
Industry Unionization Rate	.008 (3.27)	.009 (2.46)	-.003 (3.30)	-.005 (4.70)
Occupation Unionization Rate	.011 (4.03)	.013 (3.56)	-.001 (1.35)	-2.2E-4 (.21)

21. Fallick (1991) has studied this issue in detail.

22. Marital status and family size are excluded from the probit model under the assumption that they have no effect on the ratio of predisplacement to alternative wages.

Table 5 (Continued)

Regressor	Probit Estimates on Notice		Survey Date Earnings	
	Any Written (1)	Written >2 Months (2)	Any Written (3)	Written >2 Months (4)
Survey Date = 1990	.013 (.15)	.206 (1.61)	-.059 (2.33)	-.072 (2.60)
Displaced 5 Years Previous	-.030 (.30)	-.207 (1.35)	.092 (3.02)	.095 (2.79)
Displaced 4 Years Previous	-.141 (1.61)	-.241 (1.79)	.025 (.88)	.026 (.82)
Log of Previous Wage	.036 (.43)	.122 (.91)	.529 (19.55)	.524 (17.77)
Written notice: any duration (WRITTEN)070 (.54)	...
Written notice: >2 months (WRIT3)292 (2.25)
λ	-.028 (.36)	-.115 (1.54)
State effects	yes	yes	no	no

NOTE.—Respondents receiving informal notice are excluded from all regressions ($N = 1,684$). Persons receiving less than 2 months written notice are also excluded from estimates where WRIT3 is the treatment ($N = 1,399$). Columns 1 and 2 display results for probit models, estimated by maximum likelihood, where the dependent variables are WRITTEN and WRIT3, respectively. Columns 3 and 4 show estimates of "treatment/effects" regressions of the form $W = X\alpha + N\beta + \lambda y + \mu$, where $\lambda = \phi(\cdot)/\Phi(\cdot)$ for notified respondents and $\lambda = -\phi(\cdot)/[1 - \Phi(\cdot)]$ for nonnotified individuals. Absolute values of t -statistics are in parentheses.

The probit estimates generally conform to the predictions of the "employer-cost" model described above. Workers involved in plant closings, leaving highly unionized settings, or displaced late in the sample period receive notice relatively frequently, which is consistent with our expectation that these factors are associated with large costs of failing to notify. High pay or substantial fringe benefits (as proxied by health insurance coverage) on the predisplacement job are difficult to replicate and so are positively correlated with the receipt of advance warnings. Similarly, holding weekly earnings constant, part-time workers and women have low probabilities of finding comparable new employment and they are prenotified relatively often. Written notice is disproportionately furnished to workers with substantial seniority, reflecting their high levels of firm-specific skills, and lengthy notice is most common in local labor markets with elevated unemployment rates, where new jobs are hard to obtain.

The education effect is less easily explained. If schooling provides general human capital, highly educated workers will have favorable employment alternatives and, if notified, may be expected to have high rates of premature quits. Firms will therefore find it costly to notify educated persons. The probit results indicate, however, that notification probabilities rise with years of schooling. One possibility is that the reputation costs of failing to provide advance notice are particularly large for educated workers and outweigh the benefits associated with reducing early departures.

If firms minimize peremptory quits by disproportionately notifying workers with poor reemployment prospects, the reduced-form results in Section III will understate the wage

differentials associated with randomly provided written advance notice. Results of the second-stage wage equations verify this expectation. The inverse Mills coefficient is negative for both WRITTEN and WRIT3, although small and imprecisely measured in the former case, which implies that $E(\varepsilon|N=0) > E(\varepsilon|N=1)$. The treatment/ effects estimates of the advance notice effect will therefore be larger than when the wage equation is estimated by OLS. As shown in table 5, the wage differential predicted by the two-stage model is 7.3% for WRITTEN and 33.6% for WRIT3; the corresponding OLS estimates (not shown) are a much smaller —2.5% and 10.8%, respectively.²³

V. PREDISPLACEMENT SEARCH AND REEMPLOYMENT ASSISTANCE PROGRAMS

The reduced-form estimates of Section III indicate that lengthy written notice is associated with an 8.1%-13.9% survey date earnings differential, and the three tests for exogeneity, detailed in Section IV, suggest that these estimates actually understate the size of the wage premix.

Lengthy advance notice could be correlated with high subsequent pay for a variety of reasons. Most directly, early notification allows predisplacement search, which could raise wages by reducing liquidity constraints and thereby allowing workers to be more selective in their pursuit of jobs. Controlling for search durations, it may also be more efficient to search while employed than when jobless (e.g., employers might believe that unemployment is a signal of low productivity).

Prenotification also allows workers to switch training investments from firm-specific to general human capital. However, as Hamermesh (1987) has emphasized, the information would need to be received many months (or even years) in advance to significantly reduce the specific investment which is rendered worthless by the loss of jobs. Similarly, while early warnings might permit expenditures to be modified (in preparation for a period of lower incomes) or facilitate intrahousehold labor supply reallocations, the size of these adjustments is likely to be modest and so probably does not explain the wage differentials associated with lengthy notice.²⁴

Alternatively, prior notice may be correlated with higher wages, without having a causal effect. For instance, this could occur if notified individuals disproportionately receive other types of reemployment assistance such as outplacement benefits, job search counseling, skill retraining, or extended unemployment insurance.²⁵ Moreover, advance notice and these supplemental forms of assistance could jointly produce larger benefits than would either in isolation. Lengthy written announcements might also be obtained, as part of a panoply of rents, by workers with strong bargaining power. If the gap between reservation and offer wages is especially wide for these

23. Even stronger evidence of negative selection is obtained when recently displaced workers are included in the sample. For example, the treatment/effects coefficient on WRIT3 implies a 58.4% wage differential, as compared to 11.0% in the OLS model, and the coefficient (t-statistic) on λ is —0.206 (2.99). The treatment/effects model was also estimated with informally notified workers grouped with the non- notified and included in the analysis. Using this more inclusive sample raises the estimated return to short periods of formal notice and has little impact on lengthy warnings. The coefficient on WRITTEN indicates an earnings differential of 25.0% and that on WRIT3 a premium of 32.9% (the associated t-statistics are 1.45 and 1.88, respectively).

24. For example, Seitchik (1991) shows that extra work by wives typically replaces only a small fraction of the earnings lost when their husbands are displaced.

25. The exogeneity tests of Sec. IV implicitly assume that the same package of supplemental assistance that accompanies voluntary advance notice would be obtained with randomly provided prenotification.

individuals, they are likely to obtain reemployment slowly but receive relatively high wages in their new jobs

By examining how the notification wage premia vary with the duration of joblessness, indirect inferences can be drawn on the role of predisplacement search. If the early search is important, the largest differentials should be obtained by notified workers who avoid joblessness altogether or line up new positions during the notification period and start them shortly thereafter. Conversely, predisplacement job search is not expected to raise the pay of individuals who remain out of work for several months.

If the wage gains associated with lengthy notice are concentrated among the subgroup with short spells of joblessness, it is therefore plausible to attribute a major role to predisplacement search. Conversely, differentials that are evenly distributed across nonemployment durations or disproportionately received by displaced workers with protracted joblessness indicate that employed search is less important while other mechanisms, such as employer-provided supplemental assistance programs, are responsible for the observed wage differentials.²⁶ Unfortunately, the DWS contains no information on firm-sponsored displacement assistance efforts and so direct estimates of the effects of these programs cannot be obtained.

The top panel of table 6 displays advance notice coefficients from OLS wage equations for subsamples stratified by weeks of joblessness. The pattern of earnings differentials is inconsistent with explanations placing primary importance on the role of predisplacement search. Workers who receive lengthy written notice and are out of work less than 5 weeks are predicted to earn only (a statistically insignificant) 2.6% more than their counterparts surprised by the loss of jobs. Among those jobless between 5 and 8 weeks, the coefficient on WRIT3 is actually negative! Thus, notification earnings differentials are small or absent for those workers predicted to receive the greatest benefit from predisplacement search.

In contrast, the wage premium associated with the longest written notice ranges from 21 % to 49% for nonemployment spells lasting between 2 and 18 months. The extended joblessness belies the possibility that these earnings differences are due to predisplacement search. Instead, the results suggest that firms providing lengthy advance notice also offer various types of supplemental adjustment assistance. The negative coefficient on WRIT3, beyond 18 months, may indicate that the assistance efforts are either exhausted or of no help at very long durations.²⁷

Postdisplacement wages decline with the duration of joblessness, raising the possibility that predisplacement search could benefit notified workers by raising exit rates out of nonemployment even without earnings differentials at given spell lengths. A series of probit models was estimated to investigate this prospect. In each case, the dependent variable indicates whether nonemployment spells were within or above the specified range of durations. For example, when considering the reemployment hazard rate at 5-8 weeks of joblessness, the dependent variable was set equal to one (zero) for respondents out of work between 5 and 8

26. These programs may benefit workers with prolonged as well as brief spells. For example, skill retraining is likely to be most useful for persons who complete (possibly lengthy) programs.

27. The large positive coefficient on WRIT2 for workers taking 6-12 months to find jobs suggests that adjustment assistance may also be important for some persons receiving shorter periods of written notice.

Table 6
**Wage and Reemployment Regressions at Various Durations
of Joblessness**

Weeks of Joblessness	Informal Notice (UNWRIT)	Advance Notice Coefficient Written Notice (Months)			N
		<1 (WRIT1)	1–2 (WRIT2)	>2 (WRIT3)	
Wage regressions:					
All workers	.022 (1.08)	-.031 (.73)	.009 (.20)	.100 (2.38)	2,732
≤4	-.006 (.18)	-.044 (.64)	.008 (.11)	.026 (.36)	1,023
5–8	-.009 (.18)	-.059 (.51)	.088 (.84)	-.012 (.10)	345
9–13	.069 (1.12)	-.129 (1.03)	.050 (.35)	.290 (2.35)	285
14–26	.022 (.45)	-.071 (.77)	-.006 (.52)	.217 (2.21)	470
27–39	-.029 (.31)	-.027 (.13)	.113 (.51)	.212 (1.33)	169
40–52	-.006 (.07)	-.083 (.43)	.231 (1.57)	.218 (1.45)	226
53–78	.021 (.13)	.297 (1.19)	-.222 (.87)	.397 (1.61)	86
≥79	-.131 (.91)	-.086 (.40)	-.459 (1.44)	-.419 (1.83)	128
Reemployment regressions:					
≤4	.127 (2.29)	.048 (.43)	.153 (.13)	-.042 (.36)	2,732
5–8	-.068 (.86)	.224 (1.35)	.056 (.34)	-.261 (1.57)	1,709
9–13	-.020 (.23)	-.160 (.91)	-.191 (.97)	-.158 (.94)	1,364
14–26	.018 (.21)	-.036 (.21)	-.257 (1.38)	-.326 (2.00)	1,079
26–39	.081 (.63)	-.512 (1.89)	-.487 (1.78)	-.017 (.08)	609
40–52	.180 (1.24)	-.449 (1.70)	.437 (1.63)	.155 (.64)	440
53–78	.041 (.19)	.341 (1.00)	.416 (.98)	-.015 (.04)	214

NOTE.—The top panel shows wage regressions for individuals with specified weeks of joblessness. The bottom panel shows estimates of probit models where the dependent variable equals one for persons reemployed at specified weeks of joblessness and zero for those remaining out of work for a greater number of weeks. All regressions include the full set of demographic, industry, occupation, and geographic covariates. Absolute values of *t*-statistics are in parentheses.

(longer than 8) weeks, with persons finding jobs in less than 5 weeks excluded from the sample. The notification coefficients obtained from the reemployment probits are displayed in the second panel of table 6.

There is no evidence that lengthy written notice increases the proportion of short spells. To the contrary, early announcements are associated with little or no change in the probability of reemployment within 1 month, with much lower rates of job finding between 1 and 2 months, and with somewhat to dramatically reduced hazard rates between 2 and 6 months.

The combined pattern of wage and reemployment differentials is consistent with the receipt of supplemental unemployment benefits which raise reservation wages and lower rates of reemployment. Supplemental unemployment insurance (UI) does not provide the entire explanation however. At durations of 6-18 months, the reemployment hazard rates associated with lengthy advance notice are as high as or higher than those receiving no notice, while the former group continues to receive large positive wage differentials. Wages and reemployment hazard rates are similarly elevated, at durations of 9-12 months, for individuals receiving 1-2 months written notice. The simultaneous increase in wages and nonemployment exit rates suggests the importance of assistance other than supplemental UI benefits (e.g., skill retraining).²⁸

VI. CONCLUSION

Workers receiving 2 or more months of advance notice obtain sizable wage premia in subsequent employment. These higher earnings do not result because firms disproportionately notify workers with favorable reemployment prospects. To the contrary, three separate tests for endogeneity indicate that the differentials would be still larger if notification were provided randomly and accompanied by the same types of adjustment assistance currently received by notified workers.

Nor are the findings favorable to the hypothesis that advance notice raises wages by allowing predisplacement search. Were this the case, the prenotification differentials would be concentrated among workers with short nonemployment spells or avoiding joblessness altogether. Instead, the wage premia are primarily observed among persons reemployed after fairly long durations of joblessness.

The positive correlation between lengthy written notice and subsequent earnings may be the result of spurious correlation, where firms offering formal notice also frequently provide adjustment assistance such as job search counseling, skill retraining, supplemental unemployment insurance, or outplacement programs. Similarly, there may be an interaction between advance notice and reemployment assistance such that the combined effect is greater than either would be in isolation.

Based on these results, it is premature to conclude that the recent passage of federal legislation mandating advance notice will ease the adjustment problems of displaced workers. Although prenotification may have an independent effect on subsequent wages, the mechanism of influence is unclear and is unrelated or only weakly related to predisplacement search behavior. The results are consistent, however, with the possibility that advance notice raises the effectiveness of other types of displacement assistance. Thus, there is little doubt about the desirability of voluntarily provided reemployment assistance, of which prenotification is only one part.

28. To further test the role of predisplacement search, wage equations were estimated with potential search duration, measured in a variety of ways, controlled for. In no case were unusually large gains obtained by workers receiving lengthy written notice and quickly finding new employment. To cite one example, potential search durations were set equal to weeks of joblessness plus 0, 1, 5, and 9 weeks, respectively, for persons receiving no formal notice, less than 1, 1-2, or greater than 2 months of written notice. Wage equations were then estimated for the group with less than 14 weeks of potential search (corresponding to fewer than 5 weeks of nonemployment for workers receiving lengthy written notice). The resulting coefficient (t-statistic) on WRIT3 was 0.076 (1.10). This implies a wage differential of 7.9%, which is 2.6% less than that obtained when persons with lengthier search were retained in the sample.

The findings may also have implications for evaluating the relative efficiency of employed versus unemployed search. Workers aware that their jobs will soon end have a greater incentive to engage in job search than do their counterparts in stable employment. Thus, the gains from employed search among soon-to-be-displaced persons are likely to be larger than for the typical worker. Nonetheless, this study finds little evidence that pre- displacement search is more efficient than searching while out of work. This may lend credence to macroeconomic theories emphasizing search unemployment.

Appendix A

Description of Variables Used in Analysis

Continuous variables:

- Log of Survey Date Wage = natural log of real weekly wage at survey date
- Log of Previous Wage = natural log of real weekly wage at time of displacement
- Experience = years of potential labor market experience (age — education — 6)
- Experience Squared = experience × experience
- Education = years of schooling (censored at 17 years)
- Education Squared = education × education
- Tenure = number of years working for the predisplacement employer
- Tenure Squared = tenure × tenure
- No. of Children = number of children under the age of 18
- Weeks of Joblessness = duration of initial spell of joblessness (censored at 99 weeks)
- Area Unemployment Rate = unemployment rate in displacement year for 50 largest SMSAs or state (U.S. Department of Labor, May 1982-88)
- Industry Unionization Rate = average 1987 unionization rate by three- digit industry (Curme, Hirsch, and Macpherson 1990)
- Occupation Unionization Rate = average 1987 unionization rate by three-digit occupation (Curme, Hirsch, and Macpherson 1990)
- Log of Unemployment Benefit = natural log of average 1986 weekly state UI benefit (U.S. Department of Commerce 1988)

Dummy Variables (equal to one if):

- Married = married with spouse living in household
- Female = female
- Black = black
- Other Nonwhite = Hispanic, Asian, or other (nonblack) nonwhite
- Previous Part-Time = part-time work (self-defined) in predisplacement job
- Previous Health Insurance = included in group health insurance plan in predisplacement job
- Displaced by Plant Closing = job terminated due to plant closure or relocation
- Survey Date = 1990 = survey date is January 1990
- Displaced N Years Previous = displacement occurred N years prior to survey date
- Informal Notice = displacement "expected" in absence of written advance notice
- WRITTEN = received written advance notice of displacement
- WRIT1 = received < 1 months written advance notice of displacement
- WRIT2 = received 1-2 months written advance notice of displacement

WRIT3 = received > 2 months written advance notice of displacement Right-to-Work
 State = resides in "right-to-work" state (U.S. Department of Commerce 1988)

Table A1
Probit Estimates of Joblessness Survival Probabilities

Weeks of Joblessness	Informal Notice (UNWRIT)	Advanced Notice Coefficient Written Notice (Months)		
		<1 (WRIT1)	1-2 (WRIT2)	>2 (WRIT3)
0	-.387 (6.05)	-.220 (1.62)	-.179 (1.29)	-.568 (4.79)
2	-.177 (3.45)	-.071 (.65)	-.034 (.30)	-.094 (.86)
5	-.127 (2.64)	-.069 (.68)	-.022 (.21)	-.063 (.62)
10	-.035 (.74)	.087 (.89)	.028 (.27)	.048 (.48)
15	-.056 (1.18)	.082 (.83)	.033 (.32)	.117 (1.19)

Table A1 (Continued)

Weeks of Joblessness	Informal Notice (UNWRIT)	Advanced Notice Coefficient Written Notice (Months)		
		<1 (WRIT1)	1-2 (WRIT2)	>2 (WRIT3)
20	-.048 (.98)	.119 (1.20)	.080 (.77)	.218 (2.21)
25	-.052 (1.05)	.128 (1.27)	.092 (.87)	.192 (1.92)
30	-.042 (.80)	.139 (1.32)	.171 (1.57)	.186 (1.78)
35	-.030 (.56)	.135 (1.26)	.219 (1.99)	.194 (1.84)
45	-.050 (.91)	.180 (1.66)	.216 (1.92)	.163 (1.50)
55	-.082 (1.32)	.219 (1.87)	.114 (.91)	.107 (.90)
65	-.100 (1.58)	.201 (1.67)	.087 (.67)	.111 (.91)
75	-.101 (1.59)	.200 (1.64)	.082 (.62)	.112 (.92)
85	-.145 (2.19)	.182 (1.45)	.105 (.78)	-.003 (.02)

NOTE.—Dependent variable indicates whether joblessness exceeds specified number of weeks. Probit estimates obtained using maximum likelihood. Sample includes persons not working at survey date ($N = 3,650$). Regressions include full set of demographic, industry, occupation, and geographic covariates. Absolute values of t -statistics are in parentheses.

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