PLAYING HARD TO GET:
NEW EVIDENCE ON LAYOFFS, RECALLS, AND UNEMPLOYMENT

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Abstract

In my asymmetric-information model of layoffs, high-productivity workers are more likely to be recalled to their former employer and may choose to remain unemployed rather than to accept a low-wage job. In this case, unemployment can serve as a signal of productivity, and duration of unemployment may be positively related to post-laid-off wages even among workers who are not recalled. In contrast, because workers whose plant closed cannot be recalled, longer unemployment for them should not have a positive signaling benefit. Analysis of the data from the January 1988-92 Displaced Workers Supplements to the Current Population Survey reveals that the wage/unemployment duration relation differs between laid-off workers and workers displaced through plant closings in the predicted way, and finds evidence consistent with asymmetric information in the U.S. labor market.

KEYWORDS: laid-off workers, signaling, unemployment, and wages.
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INTRODUCTION

Many economists have analyzed the importance of asymmetric information in explaining labor market outcomes (Spence, 1973; Akerlof, 1976; Waldman, 1984; and Greenwald, 1986; among others). In particular, Gibbons and Katz (1991) developed and tested a model of adverse selection in the labor market. In their paper, they argue that, if employers have private information concerning employees' productivity and if they have discretion over whom to lay off, then the market infers that laid-off workers have *lower* productive ability. The authors argue that workers displaced because of plant closings, in contrast, do not suffer from such adverse inference because all workers lose their jobs when a plant closes. They predict that earnings losses associated with layoffs should be larger than earnings losses associated with plant closings. They confirm this prediction using the 1984-1986 Displaced Workers Supplements to the Current Population Survey.

In their theoretical model, Gibbons and Katz do not allow for workers returning to their former employers, even though many laid-off workers in the United States are rehired by their former employers. Lilien (1980) uses data from the U.S. Bureau of Labor Statistics (BLS) to show that about three-quarters of the workers laid off in manufacturing in the 1970s were rehired by their former employers. Katz (1986) finds that this process is also widespread outside manufacturing. Moreover, Anderson and Meyer (1994) calculate that 28 percent of turnover is temporary (defined as temporary layoffs plus recalls). Finally, the Mass Layoff Statistics program, also sponsored by BLS, reports that 68 percent of employers reporting a layoff in the second quarter of 1998 indicated that they anticipated some type of recall. It also reports that among all establishments expecting a recall, most employers expected to recall more than one-half of the separated employees and to do so within six months.

Many authors have studied the effect of the layoff-rehire process on unemployment duration in the United States (Katz, 1986; Katz and Meyer, 1990; Anderson, 1992; Fallick and Ryu, 1998). This paper adds to this literature by analyzing theoretically and empirically whether asymmetric information affects the behavior of both laid-off workers and prospective employers in the United States. My theoretical model offers a new explanation for unemployment among laid-off workers: I find that laid-off workers who are of *high* productivity may *choose* unemployment over a low-paying job as a means of signaling their productivity. Using the 1988-1992 Displaced Workers Supplements to the Current Population Survey, I offer quantitative empirical evidence consistent with this explanation.

The main idea behind this paper is that workers know their levels of productivity with their original employers, which are correlated with their probabilities of recall and with their productivity with a new employer.[1] Prospective employers may gain from using workers' private information to select among job applicants who are observationally equivalent. Thus, workers with favorable information wish to signal it to employers, and they do so by taking a costly action--unemployment--for which the expected benefit is positively correlated with their private information. The separating equilibria of this model predict a *positive* relation between post-displacement earnings and unemployment duration for laid-off workers who find a job with a new employer. [2]

The relation between post-displacement earnings and unemployment duration is determined by many factors: loss of human capital during unemployment, stigma, unobserved

[1] An underlying assumption is that employers have discretion over whom to layoff and recall.
[2] Ma and Weiss (1993) have also developed a signaling model in which least-able workers choose low-skilled jobs and more-able ones choose unemployment. Their key assumption is that workers possess private information about their own abilities, which is correlated with employers' evaluations. Ma and Weiss do not examine the layoff-rehire process, and they do not empirically test their model.

heterogeneity, and, as this paper finds, asymmetric information. Only the last element, combined with the high recall rate in the United States, leads to a positive relation between post-displacement earnings and unemployment duration for laid-off workers who get a job with a new employer. This predicted relationship provides a basis for testing the existence of asymmetric information in the labor market. To control for all unobserved heterogeneity not correlated with being a laid-off worker and with having a positive probability of being recalled, I use workers displaced through plant closings. I assume that these workers cannot be rehired by their former employers.

Using the 1988-1992 Displaced Workers Supplement, I first replicate Gibbons and Katz's results. I then test my model's empirical hypothesis and find that, after controlling for unobservable characteristics using (otherwise observationally equivalent) workers displaced through plant-closings, the post-displacement earnings of laid-off workers who find a job with a new employer *increase* with the length of their unemployment duration. Examining the relationship between earnings changes (instead of post-displacement earnings) and unemployment duration also gives a result consistent with the theoretical model's empirical hypothesis. Finally, I find that, as predicted by the model, laid-off workers have longer expected unemployment duration than (otherwise observationally equivalent) workers displaced through plant closings.

One may be concerned that the empirical findings of this paper could also be attributed to a standard search model. Thus, to further test the asymmetric-information model I distinguish between white-collar and blue-collar workers. Because blue-collar jobs are often covered by collective-bargaining agreements involving explicit layoff- and recall-by-seniority rules, the information content of a layoff and a recall is not necessarily informative of the worker's productivity. Therefore, the asymmetric-information model would predict a stronger positive relationship between post-displacement earnings and unemployment duration among workers laid off from white-collar jobs than among workers laid off from blue-collar jobs.[3] In my sample, I find that, after controlling for unobserved heterogeneity, the post-displacement earnings of laid-off workers displaced from white-collar jobs increase with the length of unemployment. No such effect is apparent for blue-collar workers.

The theoretical model is based upon the premise that laid-off workers who are of low productivity find post-displacement employment faster than higher-productivity laid-off workers. Moreover, the model assumes that low-productivity workers have lower recall expectations than higher-productivity laid-off workers. Katz and Meyer (1990) find evidence supporting this claim. Using a sample of workers whose recall expectations have been identified, Katz and Meyer find, after controlling for observable characteristics, that laid-off workers who expect to be recalled but take new jobs tend to have much longer unemployment spells than observationally equivalent workers who did not expect to be recalled at the time of layoff (page 994, and table VI). Similarly, Anderson (1992) using a different sample of workers whose recall expectations have also been identified finds that those workers who expect to be recalled have significantly lower new-job hazard rates than observationally equivalent workers who do not expect to be recalled.

This paper is organized as follows. The next section presents the theoretical model, which may be of independent interest. Section three discusses the empirical implementation and provides empirical results. Section four summarizes and interprets the findings of this paper.

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[3] However, a search model would predict the opposite result because (as found by Katz and Meyer, 1990) workers displaced from white-collar jobs are less likely to expect recall than are those displaced from blue-collar jobs.

THEORETICAL ANALYSIS

I. The Model

This is a two-period model in which initially all workers are laid off. There are two types of laid-off workers: those who were of high productivity with the original employer (G-type workers) and those who were of low productivity with the original employer (B-type workers). I assume that there is a continuum of workers of each type, t, where t = G or B. The cumulative distribution of all workers is normalized to "1." The proportion of G-type workers is α (the proportion of B-type workers is $1-a$), where $0<a<1$. Although I do not model the layoff decision, I presume that adverse selection operates here. Thus, the fraction of B-type workers among layoffs may well exceed the fraction in the population as a whole, as in Gibbons and Katz (1991).[4]

Both the worker and the original employer know the worker's type with that particular employer, where $t = B$ or G. However, laid-off workers are assumed to look identical to other potential employers. G-type workers are more likely than B-type workers to be of high productivity with a new employer. Specifically, a type-t worker will be of high productivity with a *new* employer with probability p_t, where $t = B$ or G and $0<p_B<p_G<1$. Viewed alternatively, some workers are better than others, but even good workers perform badly on some jobs and bad workers perform well on others. The productivity of a G-type worker is H and that of a B-type worker is L. I assume that $0<L<H$. After the worker remains with an employer for one period, his or her productivity with that particular employer is revealed to both the worker and the employer but not to the other firms.

At the beginning of period one, workers are laid off and enter the labor market. In this period, workers choose either to work for a new employer--accepting the highest wage offered

[4] An earlier version of this model contained both endogenous layoff- and recall-processes and generated similar results.

(randomizing in case of a tie)--or to become unemployed. An unemployed workers has a current income of zero. Prospective employers simultaneously offer laid-off workers a first-period wage. At the beginning of period two, the original employer recalls those former workers who are still unemployed with the probability of r_t, $t = B$, or G. My assumption that $r_B < r_G \leq 1$ guarantees that the employer is more likely to recall high-productivity workers than low-productivity workers. For simplicity, I set $r_B = 0$, (that is, the employer does not recall those workers who are of low productivity at his firm). I also assume that $r_G > 0$. Prospective employers observe that some unemployed workers are not recalled, and they simultaneously offer these workers a wage. Unemployed workers accept the highest wage offered (randomizing in case of a tie).

Workers work over the course of period two and retire at its end.

For notational simplicity, I assume that there is no discounting between periods. Workers are expected-lifetime-income maximizers. A large finite number of employers exist, and they maximize the present value of earnings. Therefore, each period employers offer a wage equal to workers' expected productivity. Workers and firms are risk-neutral, and they know the population parameters: a, r_t, p_t, H, and L. Because I assumed that $r_B=0$, then $r_G=r$. I assume that once a worker accepts a job offer, he is precluded from receiving a future offer from a new employer. This assumption greatly simplifies the model without modifying the main result. Similarly, after accepting an offer, workers cannot quit to return to a former employer. This assumption is consistent with the behavior of laid-off workers in the United States. As mentioned earlier, many authors have found empirical evidence consistent with the fact that laid-off workers who expect to be recall choose to wait unemployed (Katz, 1986; Katz and Meyer, 1990; Anderson, 1992).[5]

[5]This assumption could be endogenized into the model. For instance, I could assume that the employer bears a cost of hiring someone that may be recalled. The market would then offer an even lower wage to laid-off workers, and those laid-off workers who think that they will be recalled would have a higher incentive to wait unemployed.

A perfect Bayesian equilibrium in this model is a strategy combination of workers and firms and a belief structure of firms such that a worker cannot increase his total expected lifetime earnings by changing his first-period choice of being unemployed or taking a first-period job given the wage schedules being offered, and a firm cannot increase its expected profit by offering a different contingency wage schedule given workers' strategies and its beliefs.

All proofs are in the appendix.

II. The Fully Separating Equilibrium

The first theorem characterizes all equilibria in which some or all workers choose unemployment in the first period.

Theorem 1. The necessary condition for a perfect Bayesian equilibrium in which some workers choose to wait unemployed is

$$(1-p_B) \geq \frac{L}{H-L} \qquad (1)$$

H and *L* are, respectively, the maximum and minimum wages that firms would offer to workers who are one period unemployed. *L* is also the minimum loss incurred by a worker who refuses a first period job. Thus, when (1) does not hold, the minimum cost of signaling by choosing unemployment exceeds the maximum potential expected gain.

To establish sufficiency, in lemmas 1-3, I characterize three classes of unemployment equilibrium; one, and only one, of these exists when (1) holds. These perfect Bayesian equilibria are: (a) all G-type workers reject the first period wage, and all B-type workers take the first-period job (Lemma 1); (b) all G-type workers and some B-type workers choose unemployment (Lemma 2); and (c) all laid-off workers choose unemployment (Lemma 3). These are, respectively, fully-separating, semi-separating, and pooling equilibria. For brevity, I examine below only the conditions under which Lemma 1 holds. The characterization of Lemmas 2 and 3 can be found in the appendix. There I also examine another class of perfect Bayesian equilibrium in which only some G-type workers choose unemployment (remark 1). However, as explained in the appendix, I find this equilibrium unsatisfactory.[6] Given the above assumptions, these are the only possible equilibria with unemployment. That is, for B-type workers to choose unemployment while G-type workers choose low-paying jobs is never an equilibrium.

Lemma 1. For parameter values such that:

$$r(1-p_G)-p_B \geq \frac{L}{H-L} \tag{2}$$

and

$$p_G - 2p_B < \frac{L}{H-L} \tag{3}$$

the unique, perfect Bayesian equilibrium that survives the Cho-Kreps intuitive criterion is one in which all G-type workers reject the first-period offer and all B-type workers accept it.

Because of informational asymmetries and the existence of recalls among laid-off workers, accepting a job right away is sufficiently damaging to the future employment prospects of a laid-off worker that he may choose unemployment even if there is no disutility from work. Since

[6] Moreover, the conditions under which this equilibrium holds contradict the condition of the Cho-Kreps intuitive criterion.

G-type workers have higher productivity with their former employers and are more likely to be recalled than B-type workers, they have greater incentives to signal their productivity through unemployment. When conditions (2) and (3) hold, all G-type workers choose to reject the first-period market offer, whereas all B-type workers accept it.

In this model, the equilibrium with no voluntary unemployment is also possible and is described in the appendix. However, under certain conditions, this equilibrium fails to satisfy the Cho-Kreps intuitive criterion. The intuitive criterion in this model is as follows: Starting from an equilibrium with no voluntary unemployment, a worker choosing to wait unemployed is implicitly making the following statement: “I must have a positive probability of being recalled because those workers with no probability of being recalled would not choose unemployment, even if employers believed that only the high-productivity laid-off workers choose unemployment.”

In the appendix, I show that the outcome equilibrium that satisfies the intuitive criterion is unique and must be one with voluntary unemployment. In the separating equilibria--the fully (lemma1) and the semi-separating (lemma 2) equilibria--the post-displacement earnings of permanently laid-off workers who accept jobs at the end of period one are lower than those of observationally equivalent permanently laid-off workers who are unemployed during the first period.[7] The next section presents the empirical implementation of this prediction and tests the theoretical model using the Displaced Workers Supplement to Current Population Survey.

[7] It is unclear whether this prediction would hold when all workers choose unemployment (lemma 3) because accepting a first-period job is an out-of-equilibrium strategy. However, an equilibrium in which all laid-off workers choose unemployment is quite unlikely in the United States. For examples, in my sample more than 10 percent of laid-off workers find jobs without an intervening unemployment spell.

EMPIRICAL IMPLEMENTATION

In the signaling model described above, high-productivity laid-off workers are more likely to be recalled by their former employer than low-productivity laid-off workers. Thus, they may choose to remain unemployed rather than to accept a low-wage job. If so, unemployment can serve as a signal of productivity. In this case, unemployment duration may be positively related to post-displacement earnings even among laid-off workers who are not recalled.

However, in the real world, the relation between earnings of displaced workers and unemployment duration is determined by many factors. Among them are unobserved heterogeneity, loss of human capital, stigma, and, as I point out in this paper, asymmetric information. Most of these factors imply a negative relation between post-displacement earnings and length of unemployment. For simplicity, the theoretical model does not consider all of the above-mentioned factors that lead to the well-documented negative relationship between post-displacement earnings and unemployment duration (Farber, 1996; and Jacobson, Lalonde, and Sullivan, 1993; among others). Adapting the model to incorporate the negative effect of unemployment on earnings would not change the model's main prediction, namely that asymmetric information and the high rate of recall lead to a positive relationship between post-displacement earnings and duration of unemployment for laid-off workers, holding everything else constant.

To isolate the effects of asymmetric information in the U.S. labor market, I must control for all other factors affecting earnings and the duration of unemployment not associated with having a positive probability of recall. To do so, I use workers displaced through plant closings. I assume that workers displaced when their plant closes cannot be recalled, an assumption that, in this model, implies that they have no incentive to signal their productivity through unemployment. Thus, this model does not imply a positive relationship between unemployment duration and post-displacement earnings for workers displaced because of plant closings.[8]

My empirical hypothesis is that, after I control for unobserved heterogeneity by using (otherwise observationally equivalent) workers who were displaced through plant closings, the post-displacement earnings of laid-off workers who take new jobs right away should be lower than those of laid-off workers who remain longer unemployed. I will test this hypothesis using data from January 1988, 1990, and 1992 from the Displaced Workers Supplement (DWS) to the Current Population Survey.[9] I will also test this hypothesis against alternative hypotheses, in particular those implied by a standard search model.

The theoretical model presented assumes that some laid-off workers have a positive probability of recall in the second period. As the probability of recall converges toward zero, the expected benefits from waiting unemployed fall, decreasing the incentives to signal. In the United States, most recalls take place within six months. For instance, Katz and Meyer (1990) find that the recall hazard becomes quite low after about twenty-five weeks of unemployment. Similarly, Katz (1986) finds that almost no recalls occur after twenty-six weeks. Thus, any signaling that may occur among laid-off workers in the U.S. labor market should be observed

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[8] In this paper, workers displaced through plant closings would always accept the first-period job in equilibrium. Thus, to generate some unemployment among workers displaced through plant closings, some frictional unemployment is needed. Adding frictional unemployment for both laid-off workers and workers displaced through plant closings into this model does not alter the results of this paper.

[9] I do not use the survey years 1984 and 1986 because they did not contain the variable "*initial* unemployment spell." For the 1986 supplement, I can obtain this variable for the subsample of workers who have had only one job since displacement. For consistency purposes, I did not include this subsample in this paper, but the results are similar if the 1986 subsample is included. I do not use survey years after 1992 because, starting in 1994, the BLS decided to make some changes in the DWS questionnaire. Instead of asking "In the past five years, have you lost or left a job because of a plant closing, an employer going out of business, a layoff from which you were not recalled, or other similar reasons?" the BLS asked the same question referring only to the last three years from the survey date. Also, in 1994, there was an error in the supplement and the "initial unemployment spell" variable was not collected for all displaced workers who were re-employed at the survey date.

mainly within the time that prospective employers are most likely to infer that workers are waiting for recall. Therefore, to analyze the recall effect I focus on the target group: workers displaced for no more than nine months. [10]

Finally, this model also predicts that, laid-off workers who are not recalled should have a longer unemployment duration than that of (observationally equivalent) workers displaced through plant closings. [11] The reasoning behind this prediction is that by waiting, unemployed laid-off workers send a positive signal about their productivity to prospective employers. Such positive inference associated with their longer unemployment duration does not take place when the cause of displacement is plant closing.

I. Data Description

I examined a pooled sample of male workers between the ages of 20 and 61 who were permanently displaced from a private-sector, full-time, non-agricultural job because of a plant closing, slack work, or abolishment of a position or shift. [12] I used permanently displaced workers in an attempt to identify a sample of workers who did not return to their previous jobs (and similar wages).[13] Like Gibbons and Katz (1991), I classified as laid-off workers those

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[10] I focus on workers who were displaced up to nine months to allow for some extra search time after the first six months of displacement. The results shown below are robust to using samples of workers displaced for up to a year. 90 percent of all displaced workers in my sample had unemployment spells of 36 weeks or less, and 96 percent of all displaced workers in my sample had unemployment spells of one year or less.

[11] This prediction is consistent with the empirical findings of Gibbons and Katz (1991).

[12] I did not include agricultural workers because they tend to have a large number of jobs with a pronounced seasonal pattern. Like Gibbons and Katz, I focus on males displaced from full-time jobs in an attempt to identify a sample of workers with strong attachments to the labor force. Moreover, the information content that prospective employers infer from observing female workers' employment movements is considerably more complex than that of male workers. For instance, the U.S. society understands that women may want to leave the labor force while they have small children. However, such a choice is not as well understood when taken by a man.

[13] Katz and Meyer (1990) find that the post-displacement hourly earnings of workers with unemployment spells ending in recall are similar to their pre-displacement hourly earnings.

displaced because of slack work or a position or shift that was eliminated.[14] Workers displaced from construction jobs were eliminated from the sample because formulating an appropriate definition of permanent displacement from a construction job is difficult. The sample is restricted to those individuals who were re-employed in wage-and-salary employment at the survey date, who were no more than 36 weeks unemployed, and who had re-employment earnings of at least $40 a week.[15] Later, I address the potential sample biases that may arise from using the DWS and from the fact that I exclude from the sample the workers who were not re-employed at the survey date.

The restriction that data on all required variables be available leaves a sample of 1,501 workers displaced through plant closings and 1,358 laid-off workers who do not return to the former employer. Basic descriptive statistics for my sample of permanently displaced workers are presented in table 1. More detailed descriptive statistics by length of displacement can be found in appendix tables 1-4. Workers displaced through plant closings have, on average, significantly longer pre-displacement tenure (1.65 more years) than laid-off workers have. This finding suggests that seniority rules may be important in the layoff decision. Furthermore, workers displaced through plant closings have, on average, a significantly higher probability of finding a new job without an intervening unemployment spell (18.92 percent do not suffer unemployment compared with only 10.67 percent of the sample of workers displaced through layoffs) and shorter initial spells of unemployment (0.97 fewer weeks) than workers displaced by layoffs. Because unemployment duration usually increases with pre-displacement tenure, the fact that laid-off workers have longer unemployment spells than those of workers displaced through plant-

[14] If a worker lost more than one job in the five years before the survey, the survey questions refer to the lost job he had held the longest.

[15] The DWS does not provide current earnings information for those workers who became self-employment.

closings, despite their shorter tenure, suggests that their incentive to wait unemployed may be greater than that of workers displaced through plant closings. Finally, compared with laid-off workers, workers displaced through plant closings are older, more experienced, and less educated, a larger percentage of them receive advance notice, and a smaller percentage of them are in white-collar jobs.

Table 1 also provides information on the post-displacement earnings relative to the pre-displacement earnings. The earnings loss for the typical displaced worker is substantial: Being displaced reduces the earnings of the “average” worker by $70.97 per week (or $3,974.32 per year).[16] I find that the mean loss in the log of real weekly earnings for workers displaced through layoffs (-.159) is significantly greater than that experienced by workers displaced through plant closings (-.118). Since much evidence indicates that the earnings losses of displaced workers rise substantially with pre-displacement tenure (Podgursky and Swaim, 1987; Kletzer, 1989; and Topel, 1991), the fact that workers displaced through plant closings have smaller earnings losses than workers displaced through layoffs, despite their higher average pre-displacement tenure, suggests that a “lemon effect” may be operating.

Similar results hold when one classifies workers by length of displacement (tables 1-4 in the appendix). The mean loss in the log of real weekly earnings for the “average” worker increases with unemployment duration. Comparing layoff to plant closings, I find that the mean loss in the log of real weekly earnings for laid-off workers with less than one month of unemployment is significantly greater than that for similar workers displaced through plant

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[16] The average pre-displacement deflated weekly earnings for the sample are $553.76. The measure of pre-displacement wages is the usual weekly earnings before deductions that the worker earned at his job before he became displaced. The measure of post-displacement wages is the usual weekly earnings at his current job (that is, the job he holds at survey date.) Unfortunately, neither the pre-displacement hourly wage nor the pre-displacement hours worked are available in the 1988, 1990, and 1992 DWS.

closings. However, this finding is reversed for workers who are unemployed from five to twelve weeks, suggesting that there may be a positive effect of some unemployment for laid-off workers versus workers displaced through plant closings. There is no statistical difference by cause of displacement in the workers' earnings losses when they experience thirteen to thirty-six weeks of unemployment.

II. Earnings Equation

II.1. Previous results

Table 2 replicates Gibbons and Katz's results and shows that workers displaced through layoffs experience 4.6 percent greater wage losses than workers displaced through plant closings.[17] Like them, I find that the greater wage loss is explained by lower post-displacement earnings and by higher pre-displacement earnings. Thus, as in Gibbons and Katz, a "lemons effect" seems to be associated with being displaced through a layoff relative to being displaced through plant closings.

II.2. Specification

The theoretical model predicts that, after controlling for unobserved heterogeneity by using observationally equivalent workers who were displaced through plant closings, the post-displacement earnings of laid-off workers who take new jobs right away should be lower than those of laid-off workers who remain longer unemployed. I can test the these predictions by estimating the following equation:

$$Y_i = g + b_1 L_i + a_2 D_i + a_3 D_i^2 + b_2 Z_i + b_3 Z_i^2 + X'_i d + x_i \qquad (4)$$

[17] Using January 1984 and 1986 DWS, Gibbons and Katz (1991) find that workers displaced through layoffs experience 4 percent greater wage losses than workers displaced through plant closings.

where: Y_i is the log real post-displacement weekly earnings for worker i for $i=1,...N$;

L_i is a dummy for cause of displacement (L_i = 1 if the worker is laid off, and 0 if the worker is displaced through plant closings);

D_i is the length of unemployment between the time the worker was displaced and the time he found his first job;

D_i^2 is the square of the length of unemployment;

Z_i is the interaction between the layoff dummy and the length of unemployment variable*;*

Z_i^2 is the interaction between the layoff dummy and the square of the length of unemployment*;* and

X_i is a vector of observable pre-displacement characteristics.[18]

I assume that prospective employers know the workers' employment history. Besides controlling for workers' observable characteristics, in particular the log real pre-displacement weekly earnings, I also control for workers' pre-displacement industry and occupation, region of displacement, year of displacement, and year of survey. These variables aim to control for macroeconomic and regional effects. All regressions use the Huber/White estimator of variance.

Alternatively, I have examined a similar equation in which the LHS variable is the change in earnings before and after displacement. The results from this specification are

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[18] The covariates are log(previous earnings deflated by GDP deflator), a spline function in previous tenure (with breaks at one, two, three, and six years); five dummies for education (one for "less than twelve years completed;" one for "twelve years completed;" one for "some college but less than four years of college completed;" one for "college degree but no graduate degree;" and one for "more than four years of college"); three "year of survey" dummies; one "years since displacement" variable; eight "year-of-displacement" dummies; one "advance notification" dummy; six "previous-industry" dummies; five "previous-occupation" dummies; one "experience at survey date" variable and its square; one "marital status in year *t*" dummy; one "non-white" dummy; and four "region" dummies.

consistent with the prediction of the theoretical model and can be found in the tables 10-15 of the appendix. [19]

II.3. Results

Column 1 of table 3 displays the results from equation (4). After controlling for unobserved heterogeneity not correlated with having a positive probability of recall, I find that the post-displacement earnings of laid-off workers *increase* with the length of unemployment. Laid-off workers with no unemployment spell experience 8.8 percent lower post-displacement earnings than observationally equivalent workers displaced through plant closings (this finding is consistent with Gibbons and Katz "lemons" effect); however, this differential decreases and becomes positive as the length of unemployment increases. After being unemployed for four weeks, laid-off workers' post-displacement earnings are only 4.1 percent lower than those of similar workers displaced through plant closings; after four months of unemployment, their post-displacement earnings are 5 percent *higher*. These effects might understate the true signaling effect of unemployment for the following two reasons. First, some laid-off workers included in the sample could end up returning to their original employer and thus they should have higher re-employment wages and shorter initial spells of joblessness than workers who do not return to the

[19] An extended version of this model, with both endogenous layoff and rehire processes, yields the following prediction: "Relative to observationally equivalent workers displaced through plant closings, the earnings losses of laid-off workers decrease as their unemployment spell lengthens." This result occurs because, following Gibbons and Katz, competition among employers and symmetric but imperfect information about workers' productivity the first time workers enter the market yield a single pre-displacement wage, independent of workers' type and the cause of displacement. The prediction regarding the post-displacement earnings is identical to that presented in this paper.

original employers.[20] Second, many of the layoffs in the sample are likely to be determined by strict seniority systems.[21]

The results in column 1 also show that workers displaced through plant closings experience increasing losses in earnings as their unemployment spell lengthens. As mentioned earlier, others have found that workers who are unemployed longer (especially those exhausting unemployment insurance benefits) tend to have larger wage losses than short-term unemployed workers. Loss of human capital, stigma associated with being unemployed, or decreases in the workers' reservation wage are some of the arguments put forth to explain this negative relationship between unemployment duration and earnings of displaced workers. As I pointed out earlier, my theoretical work is not contrary to these arguments. It focuses on the effects of asymmetric information and the high probability of recall on post-displacement earnings.

While the results so far are consistent with my theoretical model, they cannot distinguish between my model and a standard search model combined with a "lemons" effect among laid-off workers at displacement.[22] Thus, to test this model further I distinguish between white- and blue-collar workers. Because blue-collar jobs are often covered by collective-bargaining agreements

[20] The DWS is known to overstate what would be considered job displacement because some laid-off workers end up returning to their original employer after the survey date. This occurs despite the fact that workers entering my sample are re-employed at survey date and have answered "yes" to the question: "In the past 5 years, have you left or lost a job because of a plant closing, an employer going out of business, or a layoff from which you were *not* recalled, or other similar reasons?"

[21] The initial negative stigma should not apply for these workers because the employer does not have discretion over whom to lay off. Also, these workers are usually recalled by seniority rules; therefore, those who are more likely to accept a job right away are those with less seniority (but not necessarily lower productivity).

[22] Such a model would also explain the differential search activity between workers displaced through layoffs and those displaced through plant closings through the different recall expectations. Workers with higher recall expectations would have a higher reservation wage than those workers with lower expectations of recall and thus would search longer. As the unemployment spell increased, the recall expectations would fade, generating a steeper post-displacement earnings profile for laid-off workers than that for similar workers displaced through plant-closings and thus leading to a differential earnings pattern similar to that of the one observed in the asymmetric-information model.

involving explicit layoff- and recall-by-seniority rules, the information content of a possible recall is not necessarily informative of the worker's productivity. Therefore, the asymmetric information model would predict a stronger positive relationship between post-displacement earnings and duration of unemployment among workers laid off from white-collar jobs than among those laid off from blue-collar jobs.[23] Columns 2 and 3 of table 3 display separate estimates for the two groups.[24] I find that, after controlling for unobserved heterogeneity, the post-displacement earnings of laid-off workers displaced from white-collar jobs increase with the unemployment spell. No such effect is apparent for blue-collar workers. This is fairly strong evidence supporting the asymmetric-information model.

An alternative approach to determining whether workers were displaced from jobs that were likely to be governed by formal layoff-by-seniority rules is to classify workers by the likelihood that their pre-displacement jobs were unionized. Since the DWS does not provide information as to whether a worker's pre-displacement job was unionized, I classify workers by whether they were displaced from industries with high or low rates of unionization.[25] Estimates are shown in columns 4 and 5 of table 3. I find that, relative to observationally equivalent workers displaced through plant-closings, the post-displacement earnings of laid-off workers displaced from industries with low-unionization rates increase with the length of unemployment. However, when workers are displaced from industries with high-unionization rates, this effect is smaller and

[23] However, a search model would predict the opposite result because (as found by Katz and Meyer, 1990) workers laid off from white-collar jobs are less likely to expect recall than are those laid off from blue-collar jobs.
[24] Descriptive statistics can be found in the appendix. Descriptive statistics by length of displacement are available from the author upon request.
[25] For a given year of displacement, I define industries with high-unionization rates as those having a unionization rate above the sample mean rate for that year. In 1983, the sample mean rate was 20.1 percent, and in 1992, the sample mean was 15.7 percent (Current Population Survey: Annual averages 1983-92).

not statistically significant. Again, these findings are consistent with the asymmetric-information model.

Some employers use layoffs to adjust to demand fluctuations (Feldstein, 1975; Medoff, 1979; and Lilien, 1982). Thus, one can expect that those workers who lost their jobs because of slack work are more likely to be recalled than those whose positions were abolished. In columns 1 and 2 of table 4, I present separate estimates for laid-off workers who lost their jobs because of slack work and those whose shifts or positions were abolished. As expected, I find that the differential pattern is stronger among workers who lost their jobs because of slack work than among those who report losing their jobs because their shifts or positions were abolished.

One may also be concerned that the results found in column 1 of table 3 may be driven by differences in the composition of the pool of laid-off workers and that of workers displaced through plant closings. For example, much evidence suggests that advance notice yields a productive pre-displacement search (Addison and Blackburn, 1995; Swaim and Podgursky, 1990). If so, one may be concerned that a pre-displacement search among laid-off workers may be affecting the above results. Moreover, notified workers may differ from their non-notified counterparts in some unmeasured way (Addison and Portugal, 1992a; Ruhm, 1992; and Fallick, 1994). In such a case, one would want to distinguish between those workers who were notified in advance and those who were not. With a sample of workers who do not received advance notice, the theory predicts that, after one controls for unobserved heterogeneity with workers displaced through plant closing, the post-displacement earnings of laid-off workers should increase with their length of unemployment. Yet the predictions of the asymmetric-information model are not so straightforward when workers receive advance notice. Assuming that (1) productive pre-displacement search occurs among workers who receive advance notice, (2) prospective employers observe the pre-displacement search time, and (3) the longer the pre-displacement notice the more productive the worker's search, the model would predict that, after controlling for unobserved heterogeneity using workers displaced through plant closings, laid-off workers' post-displacement earnings increase with the workers' total search time (instead of with the duration of unemployment). Unfortunately, Addison and Blackburn' s results (1995) provide no evidence of monotonically increasing benefits from longer pre-displacement written notice. Moreover, they do not find evidence of any incremental value to receiving extended written notice rather than informal notice. Thus, the asymmetric-information model would not necessarily predict a positive relationship between post-displacement earnings and the length of unemployment among laid-off workers. Column 3 in table 4 displays the estimates for workers who, in my sample, did not receive advance notice. These results are consistent with the theoretical model: Relative to observationally equivalent workers displaced through plant closings, laid-off workers' post-displacement earnings increase with the length of unemployment. As shown in column 4, this pattern is not observed among workers who receive advance notice of displacement. As mentioned earlier, this unobserved pattern may result from complex reasons. Despite its interest, the topic lies beyond the scope of the present paper.

Finally, because the search behavior of unemployment-insurance UI recipients may differ from that of nonrecipients, or because UI recipients may differ from their nonrecipients counterparts in some unmeasured way, I distinguish between those workers who received UI benefits and those who did not. The last two columns of Table 4 display the results for the two samples. In both samples, after controlling for unobserved heterogeneity with observationally equivalent workers displaced by plant closings, laid-off workers who are unemployed longer receive higher post-displacement earnings.

III. Sensitivity Analysis

III.1. Robustness

The results above are robust to model specifications, to changes in the definition of the sample, and to various changes of the covariates. As mentioned earlier, these results are also consistent to using the change in earnings before and after displacement as the LHS variables. [26]

III.2. Retrospection bias

The CPS supplements are retrospective in as much as respondents are asked to describe events that may have occurred up to five years in the past. This characteristic is a problem when the errors are not random, such as if a worker recalls only an especially traumatic or costly displacement that occurred four to five years previously. A priori, it is not clear whether this bias is worse for plant closings than for layoffs.[27] Looking at the raw data, I find that, for a given year, many more layoffs are reported in the 1988 survey than in the 1990 survey, which in turn

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[26] The results shown in tables 3 and 4 are robust to alternative functional forms of the unemployment-spell variable, the inclusion of interactions between the log real pre-displacement weekly earnings and the length-of-unemployment-spell variables, and interactions between tenure and the length-of-unemployment-spell variables. The results were also robust to the inclusion of workers with longer spell of initial unemployment, part-time workers, workers earning less than $40 a week, and public-sector workers. I also re-run the regressions dropping outliers. The estimates are available from the author upon request.

[27] For instance, plant closings that were easier to remember than layoffs and those that occurred in more-depressed areas might bias upward both the coefficient on the layoff dummy--because plant closings in particular depressed areas would be over-represented--and the coefficient on the interaction between layoff and unemployment--because these plant closings would be more likely have long-term unemployment and lower re-employment wages if the area were still depressed—assuming constant labor supply. On the other hand, if particularly “traumatic” layoffs were remembered and over-represented, then I would expect both the coefficient on the layoff dummy and on the coefficient of the interaction between being laid-off and unemployment duration to be biased downward. The bias in the former would be of concern. A different type of retrospective bias may emerge if respondents do not accurately recall the exact date of their layoff or the length of the initial duration of unemployment. For instance, the understatement by laid-off workers, especially non-traumatized ones, about the length of time they were unemployed in earlier years, would bias downward the coefficients of the interaction between being a laid-off worker and the unemployment duration.

has more layoffs reported than the 1992. The analogous comparison of plant closings reported at the three survey dates reveals a much smaller difference. Yet, looking at the averages of tenure fails to indicate whether the retrospection bias is worse for layoffs or for plant closings (see appendix). To analyze the dimension of this problem, I re-estimate the equations dropping the first and second earliest years from each DWS. The results are similar to the ones previously found, thus showing that the retrospection bias does not seem to affect the results.

III.3. Sample selection bias

The regressions above are based on a sample of displaced workers who were re-employed at the survey date, and therefore, the estimates may reflect sample-selection bias because some of the workers have had little time to find a new good job match. To probe the importance of this problem, I re-estimate the equations using a sample of workers who were displaced at least a year before survey date as these workers should have had plenty of time to find a new job. Again, the results accord with the findings.

IV. Unemployment Duration and Cause of Displacement

The theoretical model also predicts that laid-off workers who are not recalled have longer unemployment duration than workers displaced through plant closings. As table 1 shows, among permanently displaced workers who were re-employed at a survey date, workers displaced through plant closings have average initial unemployment spells similar to those of workers displaced through layoffs. Table 5 shows my analysis of the duration of initial spells of joblessness for this sample using semi-parametric proportional hazard-model techniques, and controlling for observable characteristics. I find that workers permanently displaced by layoffs have significantly longer initial unemployment spells than do those displaced by plant closings.

An alternative explanation to this result is the depressing effect of recall expectations on job search as mentioned by Katz (1986). The asymmetric information model predicts that the higher the information content of a recall, all else being equal, the greater the incentive to signal through unemployment. Thus, one would expect laid-off workers to have relatively longer unemployment spells than those of similar workers displaced by plant closings in those sectors in which employers have more discretion over whom to recall.

Rows 2 and 3 in table 5 show the estimation of the effect of being laid-off on the duration of unemployment spell for the white- and blue-collar samples. I find that workers laid off from white-collar jobs have longer unemployment spells than observationally equivalent workers displaced through plant closings. No effect is found among workers displaced from blue-collar jobs. These results, combined with those from section II.3., are fairly strong evidence supporting the asymmetric-information model.

Rows 4 and 5 in table 5 display the effects of being laid off on duration in samples of workers displaced from low- and high-unionized jobs. Again, as predicted by the model, laid-off workers displaced from low-unionized industries have longer unemployment spells than those of similar workers displaced through plant closings. No effect is found among workers displaced from blue-collar jobs.

Rows 6 and 7 separate the estimates of the layoff dummy in the duration equation by whether workers were laid off because of slack work or abolishment of position or shift. As expected, relative to workers displaced through plant closings, workers laid off because of slack work have longer unemployment than similar workers laid off because of abolishment of position or shift.

I also analyze the effect of being laid off on duration by distinguishing whether or not the workers received advance notice, and whether of not they receive unemployment insurance benefits. I find that laid-off workers who did not receive advance notice have longer unemployment spells than observationally equivalent workers displaced by plant closings. The same is true for workers who did not receive unemployment insurance benefits. However, I do not find a differential effect between being laid-off and being displaced by a plant closing when workers received advance notice or when they received unemployment insurance benefits. As mentioned earlier, the lack of result in the sample of notified workers may be related to productive pre-displacement search and the inability to accurately measure total search time. The estimate on the layoff dummy obtained from the sample of workers who received unemployment insurance may be inaccurate because the effects of unemployment insurance benefits on workers' search behavior are likely to vary across time.[28]

Overall, the results above were robust to changes in the covariates, the functional form and the subsamples.

CONCLUSION

In the United States, many laid-off workers are recalled to their original employer. If employers have discretion over whom to recall, high-productivity workers are more likely to be recalled and may choose to remain unemployed rather than to accept a low-wage job offered early in their unemployment spell. If so, unemployment can serve as a signal of productivity. In

[28] Fallick (1990) finds that empirical specifications that do not allow for variation in the effects of unemployment insurance over time may be inadequate to measure hazard rates of displaced workers who receive unemployment insurance benefits.

this case, unemployment duration may be positively related to post-displacement wages even among workers who are not recalled. In contrast, because workers displaced through plant closings cannot be recalled, a longer duration of unemployment should not have a positive signaling benefit for such workers. Analysis of the 1988-1992 Displaced Workers Supplements to the Current Population Survey reveals that the earnings and unemployment duration experiences of the two groups behave in the predicted way.

This paper offers a new test of the importance of asymmetric information in the labor market. Some evidence has been provided against the “most natural” alternative model, the standard search model. More important, the theoretical model provides the basis for a new empirical finding regarding laid-off workers. After one controls for unobserved heterogeneity, the post-displacement earnings of laid-off workers who do not return to their former employers increase with the length of unemployment.

In “Layoffs and Lemons,” Gibbons and Katz showed that prospective employers understood adverse selection in the labor market. The results in my paper indicate that workers are also aware of the existence of adverse selection and of its consequences on their behavior. This implies a need for differential unemployment policies by cause of displacement. Further research will analyze the effects of displaced workers’ differential behavior by cause of displacement on policies oriented to reduce unemployment duration.

Table 1. Descriptive statistics for displaced workers using the DWS (1988-90-92), males re-employed at survey date

	Means		
		Reason for displacement	
Variable	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	47.49	0	100
Previous tenure (years)	4.96	5.75	4.10
	(6.18)	(6.73)	(5.37)
Change in log real weekly earnings	-.137	-.118	-.159
	(.487)	(.473)	(.502)
Log of previous weekly earnings	6.160	6.153	6.168
	(.554)	(.552)	(.557)
Log of current weekly earnings	6.023	6.035	6.009
	(.575)	(.548)	(.603)
Length of unemployment (weeks)	8.68	8.22	9.19
	(9.33)	(9.47)	(9.16)
No unemployment after displacement = 1 (percent)	15.00	18.92	10.67
Advance notice = 1 (percent)	51.38	58.69	43.29
Current education (years)	12.94	12.71	13.20
	(2.42)	(2.43)	(2.37)
Current (age-education-6) (years)	17.44	18.20	16.60
	(10.20)	(10.31)	(10.02)
White collar in previous job = 1 (percent)	42.70	40.37	45.28
Previous job in manufacturing = 1 (percent)	41.69	41.10	42.34
Current age (years)	36.39	36.92	35.81
	(10.08)	(10.03)	(10.10)
Currently married = 1 (percent)	68.66	69.88	67.30
Non white = 1 (percent)	10.03	10.92	9.05
N	2,859	1,501	1,358

Note.- The numbers in parenthesis are standard deviations. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales, and administrative support specialties.

Table 2. Gibbons and Katz (1991) earnings equation using the DWS (1988-90-92), males reemployed at survey date

	Dependent variables: Earnings♣		
N = 2,859	Weekly change	Pre-displacement	Post-displacement
Layoff	-.046	.017	-.028
	(.018)	(.017)	(.018)
R-squared	.0914	.3915	.2984

Note: The number in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are: a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years); four dummies for education (one for "twelve years completed"; one for "some college but less than four years of college completed"; one for "college degree but no graduate degree"; and one for "more than four years of college"); nine "year-of-displacement" dummies; one "advance notification" dummy; five "previous-industry" dummies; four "previous-occupation" dummies; one "experience at survey date" variable and its square; one "pre-displacement marital status" dummy; one "non-white" dummy; and three "region" dummies. Columns (1) and (3) also include three "year of survey" dummies; and one "years since displacement" variable.

♣ Dependent variable: col. 1 = log(current earnings/previous earnings); col. 2 = log (previous earnings deflated by GDP deflator); and col. 3 = log(current earnings deflated by GDP deflator)

Table 3. Post-displacement earnings equation using the DWS (1988-90-92), males re-employed at survey date

	Dependent variable: Post-displacement earnings ♣				
	Whole sample N = 2,859	White-collar N = 1,221	Blue-collar N = 1,638	Low-unionization N = 1,272	High-unionization N = 1,587
Unemployment spell	-.015	-.020	-.010	-.019	-.012
	(.003)	(.006)	(.004)	(.006)	(.004)
Unemployment spell	.0003	.0005	.0001	.0005	.0002
Squared	(.0001)	(.0002)	(.0001)	(.0002)	(.0001)
Layoff Dummy	-.088	-.071	-.087	-.082	-.101
	(.028)	(.044)	(.039)	(.045)	(.037)
Layoff dummy	.015	.022	.007	.021	.012
x Unemployment spell	(.005)	(.008)	(.007)	(.009)	(.007)
Layoff dummy	-.0004	-.0008	-.0001	-.0007	-.0003
x Unemployment spell	(.0001)	(.0003)	(.0002)	(.0003)	(.0002)
squared					
R-squared	.4728	.5063	.3660	.5018	.4589

Note: The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are: log (previous earnings deflated by GDP deflator); a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years); four dummies for education (one for "twelve years completed"; one for "some college but less than four years of college completed"; one for "college degree but no graduate degree"; and one for "more than four years of college"); nine "year-of-displacement" dummies; one "advance notification" dummy; five "previous-industry" dummies; four "previous-occupation" dummies; one "experience at survey date" variable and its square; one "pre-displacement marital status" dummy; one "non-white" dummy; and three "region" dummies.

♣ Dependent variable: log(current earnings deflated by GDP deflator)

Table 4. Post-displacement earnings equation using the DWS (1988-90-92), males re-employed at survey date

	Dependent variable: Post-displacement earnings ♣					
	Slack work N = 2,412	Shift abolished N = 1,948	No advance notice N = 1,390	Advance Notice N = 1,469	No UI benefits N = 1,378	UI benefits N = 1,481
Unemployment spell	-.015	-.014	-.021	-.009	-.016	-.010
	(.003)	(.003)	(.006)	(.004)	(.006)	(.005)
Unemployment spell squared	.0003	.0003	.0005	.0001	.0005	.0001
	(.0001)	(.0001)	(.0002)	(.0001)	(.0002)	(.0001)
Layoff dummy	-.102	-.063	-.155	-.022	-.072	-.126
	(.033)	(.044)	(.046)	(.036)	(.034)	(.056)
Layoff dummy x unemployment spell	.018	.010	.030	-.002	.020	.017
	(.006)	(.008)	(.008)	(.007)	(.010)	(.008)
(Layoff dummy x unemployment spell) squared	-.0005	-.0002	-.0009	.0000	-.0008	-.0004
	(.0002)	(.0002)	(.0008)	(.0002)	(.0003)	(.0002)
R-squared	.4724	.4770	.4636	.4993	.5263	.4328

Note: The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are: log (previous earnings deflated by GDP deflator); a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years); four dummies for education (one for "twelve years completed"; one for "some college but less than four years of college completed"; one for "college degree but no graduate degree"; and one for "more than four years of college"); nine "year-of-displacement" dummies; one "advance notification" dummy; five "previous-industry" dummies; four "previous-occupation" dummies; one "experience at survey date" variable and its square; one "pre-displacement marital status" dummy; one "non-white" dummy; and three "region" dummies.

♣ Dependent variable: log(current earnings deflated by GDP deflator).

Table 5. Semi-parametric hazard model estimates using the DWS (1988-90-92), males
Dependent variable = Log (month of joblessness)
Cox proportional hazard model specification

		Layoff dummy	Log likelihood
1. Whole sample	N=2,859	-.099 (.035)	-20,088.30
2. White-collar workers	N=1,221	-.197 (.056)	-7,518.65
3. Blue-collar workers	N=1,638	-.031 (.046)	-10,596.77
4. Non-unionized workers	N=1,272	-.153 (.054)	-7,898.81
5. Unionized workers	N = 1,587	-.062 (.048)	-10,219.79
6. Slack work	N = 2,412	-.104 (.039)	-16,539.21
7. Shift abolished	N = 1,948	-.082 (.049)	-12,946.52
8. No advance notice	N = 1,390	-.123 (.052)	-8,737.45
9. Advance notice	N = 1,469	-.071 (.049)	-9,350.98
10. No UI benefits	N = 1,378	-.139 (.049)	-8,733.90
11. UI benefits	N = 1,481	.013 (.049)	-9,437.13

Note.- Earnings are deflated by GDP deflator. The numbers in parentheses are standard errors.
The covariates are: log (previous earnings deflated by GDP deflator); one "previous years in tenure" variable; four dummies for education (one for "twelve years completed"; one for "some college but less than four years of college completed"; one for "college degree but no graduate degree"; and one for "more than four years of college"); nine "year-of-displacement" dummies; one "advance notification" dummy; five "previous-industry" dummies; four "previous-occupation" dummies; one "experience at survey date" variable and its square; one "pre-displacement marital status" dummy; one "non-white" dummy; one "age" variable; and three "region" dummies.

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Technical Appendices

(not necessarily for publication)

PLAYING HARD TO GET:
NEW EVIDENCE ON LAYOFFS, RECALLS, AND UNEMPLOYMENT

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APPENDIX

A. Characterization of equilibria

Lets define $h = \frac{(1-r)a}{(1-r)a + (1-a)}$, where η is the probability that a G-type worker accepts a new job offer in the second period when all workers choose to reject the first period offer. Let w_G and w_B be the expected productivity of a G-type worker and a B-type worker, respectively, at a new job, where w_G and w_B are defined as:

$$w_G = p_G H + (1 - p_G)L$$

and

$$w_B = p_B H + (1 - p_B)L$$

Proof of theorem 1. Suppose that there is an equilibrium in which some workers prefer to wait unemployed than to accept the job right away. Then,

$$rH + (1-r)w(2,U,h,l) \geq w(1,W,h,l) + w_G \qquad (5)$$

$$w(2,U,h,l) \geq w(1,W,h,l) + w_B \qquad (6)$$

where $w(1,W,h,l)$ and $w(2,U,h,l)$ are the wages offered to displaced workers who accept a job in the first period and in the second period, respectively. Consistency requires that $w(1,W,h,l) \geq L$ and $w(2,U,h,l) \leq H$. Inequalities (5) and (6) become

$$H \geq L + p_G H + (1 - p_G)L \qquad (7)$$

$$H \geq L + p_L H + (1 - p_L)L \qquad (8)$$

respectively, and since $p_L < p_H$, inequality (1) follows.

Proof of lemma 1. Expressions (2) and (3) can be rewritten as

$$rH + (1-r)w_G \geq w_B + w_G \qquad (9)$$

and

$$w_G < 2w_B \qquad (10)$$

respectively. Expressions (9) and (10) say that if prospective employers offer a wage of w_G to workers who reject the first period offer, and a wage of w_B to workers who accept the first period offer, then it is optimal for G-type workers to reject the first-period offer and for B-type workers to accept it. Given these worker's equilibrium strategies, prospective employers choose a second-period wage of w_G for laid-off workers who are unemployed during the first period and a wage of w_B for laid-off workers who accept a job at the beginning of the first period. Thus, the strategies just described are equilibrium strategies.

<u>Lemma 2</u>. *There is a perfect Bayesian equilibrium in which all G-type workers and a proportion t of B-type workers wait unemployed when:*

$$h(p_G - p_B) - p_B \leq \frac{L}{H-L} \leq p_G - 2p_B \tag{11}$$

$$r \geq \frac{(p_G - p_B)[H-L]}{2(1-p_B)[H-L]-H} \tag{12}$$

and t is given by the following equation

$$t = \frac{a(1-r)}{(1-a)} \frac{(p_G - 2p_B)[H-L]-L}{p_B[H-L]+L} \tag{13}$$

<u>Proof of lemma 2.</u> Suppose (11) is true. Since, $V(2,U,l,t)$ ranges from $\left[hw_G + (1-h)w_B\right]$ to w_G and is continuous, there must exist a $\tau \in [0,1]$ that satisfies (11). Reordering expression (12) and (13), it is easy to see that they say the following: If prospective employers offer $V(2,U,l,t)$ to laid-off workers who are unemployed during the first period and w_B to laid-off workers who accept the first-period job offer, G-type workers will strictly prefer to reject the offer while B-type workers will be indifferent between rejecting the second period job offer or accepting it. Supposing that all G-type workers and a fraction τ of B-type workers reject the first-period job offer, then prospective employers will offer a wage of $V(2,U,l,t)$ to laid-off workers who wait one period unemployed and a first period wage of w_B to workers who accept the second period job. Therefore, the strategies described in this lemma are the equilibrium strategies.

<u>Lemma 3.</u> *There is a perfect Bayesian pooling equilibrium in which all types of laid-off workers reject job offers in the second period when the following conditions hold:*

$$\frac{L}{H-L} \leq r(1-p_B) + h(p_G - p_B)(1-r) - p_G \qquad (14)$$

$$\frac{L}{H-L} \leq h(p_G - p_B) - p_B \qquad (15)$$

<u>*Proof of lemma 3*</u>. Notice that (14) and (15) imply that

$$rH + (1-r)\left[hw_G + (1-h)w_B\right] \geq w_B + w_G \qquad (16)$$

$$hw_G + (1-h)w_B \geq 2w_B \qquad (17)$$

Suppose that prospective employers offer $\left[hw_G + (1-h)w_B\right]$ to laid-off workers who were unemployed during the first period, and w_B to laid-off workers who accept a job in the first period. Inequalities (16) and (17) say that both types of workers reject the first period job offer and choose to wait one period unemployed. Supposing that all laid-off workers choose to wait unemployed one period, and observing an out-of-equilibrium employment history of accepting a second-period job by a worker, it is possible that prospective employers believe that they were observing a B-type worker. Those beliefs would lead them to offer that worker the following wage: w_B .

Under condition (1), I can also construct another hybrid equilibrium, described in Lemma 4. However, I find this equilibrium to be unsatisfactory

<u>Remark 1</u>. *There is a perfect Bayesian equilibrium in which all B-type workers accept a first period offer, and a proportion g of G-type workers choose to wait one period unemployed when*

$$r(1-p_G) - a(p_G - p_B) - p_B \leq \frac{L}{H-L} \leq r(1-p_G) - p_B \qquad (18)$$

$$r > \frac{p_G - p_B}{1-p_B} \qquad (19)$$

and g is given by:

$$(1-g)=\frac{(1-a)}{a}\frac{[r(1-p_G)-p_B][H-L]-L}{[r(1-p_G)-p_G][H-L]-L}$$

<u>Proof of remark 1.</u> The proof is similar to lemma 2 and thus omitted.
This completes the proof of theorem 1.

The equilibrium characterized by remark 1 is unsatisfactory for the following reason. Suppose that fewer than g of *G*-type workers choose unemployment, then the expected productivity of workers accepting a first period market offer would exceed $V(1,W,g,0)$. Then, more *G*-type workers would accept the first-period market offer, and the equilibrium would not be sustained. Conversely, if more than g choose to reject the first-period market offer, the expected productivity of laid-off workers who accept the first-period job offer would be lower, and fewer G-type workers would choose to accept the first-period job. Thus, it seems unlikely that an economy would ever converge to the equilibrium described in remark 1.[29]

B. Equilibrium with no voluntary unemployment

<u>Theorem 2</u>. There is a perfect Bayesian equilibrium in which there is no voluntary unemployment when

$$\frac{L}{H-L}>r[1-a(p_G-p_B)-p_B]-p_G \qquad (20)$$

<u>Proof of theorem 2</u>. Inequality (20) can be rewritten as:

$$[aw_G+(1-a)w_B]+w_G>rH+(1-r)[aw_G+(1-a)w_B] \qquad (21)$$

On the other hand, since $w_B>0$, I have that

$$[aw_G+(1-a)w_B]+w_B>[aw_G+(1-a)w_B] \qquad (22)$$

Inequalities (21) and (22) say that all laid-off workers choose to accept the first period wage.
Since all workers choose to work during the first period, firms offer them a first-period wage of

[29] Notice that the other hybrid equilibrium does not have this instability problem. If the fraction of *B*-type workers who rejected the offer increased (or decreased), this would lead to lower (or higher) earnings for

$\left[a w_G + (1-a) w_B\right]$. Firms can have consistent beliefs that if the out-of-equilibrium action "being unemployed for one period" is observed, it would have been taken by a randomly selected worker. Thus, they can set the wage offered to workers who are unemployed during the first period to $\left[a w_G + (1-a) w_B\right]$.

C. Equilibrium refinements

In this section, I apply the Cho Kreps (1987) intuitive criterion to my model. Under certain conditions, the equilibrium in theorem 2 fails to satisfy the intuitive criterion.

Theorem 3. The equilibrium with no voluntary unemployment described in theorem 2 fails to satisfy the intuitive criterion if and only if

$$\frac{L}{H-L} \leq r(1-p_G) - a(p_G - p_B) - p_B \qquad (23)$$

and

$$\frac{L}{H-L} > p_G - a(p_G - p_B) \qquad (24)$$

Proof of theorem 3. I first show that (23) and (24) are sufficient. Inequalities (23) and (24) can be rewritten

$$\left[a w_G + (1-a) w_B\right] + w_G \leq rH + (1-r) w_G \qquad (25)$$

and

$$\left[a w_G + (1-a) w_B\right] + w_B > w_G \qquad (26)$$

Inequality (26) implies that it is an out-of-equilibrium strategy for a B-type worker to reject a first-period job, whereas inequality (25) implies that it is not an out-of-equilibrium strategy for a G-type worker to reject a first-period job. Thus, if a laid-off worker chooses to wait unemployed, he must be a G-type laid-off worker. Inequality (23) implies that the equilibrium with no voluntary unemployment fails to satisfy the intuitive criterion.

Inequalities (23) and (24) are not only a sufficient condition, but also necessary ones.

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those who wait unemployed. Thus, this would bring these workers back to the postulated distribution of actions.

If $\left[aw_G + (1-a)w_B\right] + w_B > w_G$, then a B-type worker would not benefit from waiting unemployed one period even when by doing so he would be identified as an G-type laid-off worker. And if $\left[aw_G + (1-a)w_B\right] + w_G \leq rH + (1-r)w_G$ then it would be optimal for a G-type worker to reject a first-period offer.

<u>Theorem 4</u>. All equilibria with some voluntary unemployment satisfy the intuitive criterion.

<u>Proof of theorem 4</u>. Since the separating and the two hybrid equilibria do not involve an unreached information set, they satisfy the intuitive criterion. I only need to show that the equilibrium in which all laid-off workers choose to wait unemployed for one period satisfies the intuitive criterion. The only way the intuitive criterion would rule out the equilibrium where everyone chooses to wait unemployed would be if, when observing an out-of-equilibrium action from a worker, prospective employers would believe this worker was a G-type worker. However, since G-type workers have a positive probability of being recalled, this restriction on prospective employers' beliefs is not possible. Thus, when prospective employers observe a worker accepting a firsts-period offer, they believe that he is a B-type worker. This will dissuade workers from accepting an offer in the first period. □

<u>Corollary 1.</u> If (23) and (24) hold, the equilibrium outcome that satisfies the intuitive criterion is unique and must be one with voluntary unemployment.

<u>Proof of corollary 1</u>. Inequalities (23) and (24) only contradict condition (18) in theorem 1. Together with theorems 3 and 4, I know that an equilibrium that satisfies the intuitive criterion must be one with voluntary unemployment. Because the equilibria in lemmas 1-3 in theorem 1 are mutually exclusive, the conclusion follows.

D. Retrospection Bias

In my sample (including those not re-employed at survey date), the layoffs reported in the 1988 DWS are 230, 256, and 353 for 1985-7, respectively, while those reported in the 1990 DWS are 122, 173, and 148 for the same years, and those reported in the 1992 DWS are 113 for

1987. The plant closings reported in the 1988 DWS are 282, 248, and 244 for 1985-87, respectively, while those reported in the 1990 are 194, 206, and 222 for the same years, and those reported in the 1992 DWS are 201 for 1987. Similarly, the layoffs reported in the 1990 DWS are 144 and 306 for 1988-89, respectively, while those reported in the 1992 DWS are 137 and 216. The plant closings reported in the 1990 DWS are 196 and 204 for 1988-89, respectively, while those reported in the DWS 1992 are 200 and 256 for the same years.

The average tenure reported by laid-off workers in the 1988 DWS are 4.39 (5.81), 4.19 (5.50), and 3.56 (6.04) -standard errors are in parenthesis- for 1985-87, respectively, while those reported in the 1990 DWS are 4.40 (5.37), 4.79 (5.98), and 5.03 (6.08) for the same years, and those reported in the 1992 DWS are 6.16 (7.14) for 1987. The average tenure reported for workers displaced through plant closings are in the 1988 DWS are 7.32 (9.00), 7.44 (8.26), and 6.31 (7.86) for 1985-87, respectively, while those reported in the 1990 are 6.97 (7.30), 7.54 (8.60), and 5.28 (6.77) for the same years, and those reported in the 1992 DWS are 7.25 (7.64) for 1987. Similarly, the layoffs reported in the 1990 DWS are 4.02 (5.47) and 3.57 (5.03) for 1988-89, respectively, while those reported in the 1992 DWS are 4.78 (6.74) and 4.47 (5.97). The plant closings reported in the 1990 DWS are 7.13 (8.38) and 5.27 (6.87) for 1988-9, respectively, while those reported in the DWS 1992 are 5.12 (6.15) and 6.29 (7.66) for the same years.

Table 1. Descriptive statistics for displaced workers who get re-employed without unemployment spell using the DWS (1988-90-92), males re-employed at survey date

	Means		
		Reason of displacement	
Variables	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	33.79	0	100
Previous tenure (years)	5.94	6.34	5.16
	(6.96)	(7.08)	(6.67)
Change in log real weekly earnings	-.088	-.060	-.143
	(.481)	(.468)	(.503)
Log of previous weekly earnings	6.209	6.201	6.224
	(.580)	(.568)	(.603)
Log of current weekly earnings	6.120	6.141	6.080
	(.613)	(.568)	(.692)
Length of unemployment (weeks)	0	0	0
No unemployment after displacement = 1 (percent)	100	100	100
Advance notice = 1 (percent)	65.03	67.95	59.31
Current education (years)	13.20	12.99	13.60
	(2.24)	(2.20)	(2.28)
Current (age-education-6) (years)	17.21	17.54	16.56
	(10.11)	(10.15)	(10.02)
White collar in previous job = 1 (percent)	48.25	48.94	46.89
Previous job in manufacturing = 1 (percent)	37.99	34.50	44.82
Current age (years)	36.41	36.53	36.17
	(10.13)	(10.11)	(10.20)
Currently married = 1 (percent)	70.16	70.07	70.34
Non white = 1 (percent)	7.22	6.69	8.27
N	429	284	145

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 2. Descriptive statistics for displaced workers who are unemployed between one and four weeks using the DWS (1988-90-92), males re-employed at survey date

	Means		
		Reason of displacement	
Variables	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	47.30	0	100
Previous tenure (years)	4.06	4.79	3.24
	(5.36)	(5.91)	(4.53)
Change in log real weekly earnings	-.084	-.047	-.125
	(.480)	(.441)	(.517)
Log of previous weekly earnings	6.079	6.107	6.048
	(.559)	(.568)	(.547)
Log of current weekly earnings	5.994	6.060	5.922
	(.584)	(.544)	(.618)
Length of unemployment (weeks)	2.36	2.34	2.39
	(1.15)	(1.17)	(1.13)
No unemployment after displacement = 1 (percent)	0	0	0
Advance notice = 1 (percent)	49.74	56.06	42.70
Current education (years)	12.74	12.59	12.92
	(2.44)	(2.53)	(2.32)
Current (age-education-6) (years)	16.36	17.49	15.10
	(10.07)	(10.18)	(9.80)
White collar in previous job = 1 (percent)	39.08	38.53	39.69
Previous job in manufacturing = 1 (percent)	42.63	41.04	44.42
Current age (years)	35.11	36.08	34.03
	(9.86)	(9.76)	(9.87)
Currently married = 1 (percent)	68.22	68.59	67.81
Non white = 1 (percent)	10.65	12.13	9.01
N	985	519	466

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 3. Descriptive statistics for displaced workers who are between five and twelve weeks unemployed using the DWS (1988-90-92), males re-employed at survey date

	Means		
		Reason of displacement	
Variables	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	52.35	0	100
Previous tenure (years)	4.89	5.89	3.98
	(6.16)	(7.08)	(5.02)
Change in log real weekly earnings	-.142	-.177	-.111
	(.473)	(.498)	(.448)
Log of previous weekly earnings	6.168	6.164	6.171
	(.549)	(.571)	(.530)
Log of current weekly earnings	6.025	5.987	6.059
	(.573)	(.552)	(.590)
Length of unemployment (weeks)	8.56	8.618	8.52
	(2.36)	(2.35)	(2.37)
No unemployment after displacement = 1 (percent)	0	0	0
Advance notice = 1 (percent)	47.24	55.93	39.33
Current education (years)	12.96	12.69	13.21
	(2.51)	(2.59)	(2.41)
Current (age-education-6) (years)	17.49	17.76	17.25
	(10.14)	(10.23)	(10.06)
White collar in previous job = 1 (percent)	42.66	38.41	46.52
Previous job in manufacturing = 1 (percent)	40.64	43.22	38.30
Current age (years)	36.46	36.45	36.46
	(10.00)	(10.01)	(10.01)
Currently married = 1 (percent)	68.23	70.33	66.32
Non white = 1 (percent)	9.42	11.01	7.96
N	743	354	389

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 4. Descriptive statistics for displaced workers who are unemployed between thirteen and thirty-six weeks using the DWS (1988-90-92), males re-employed at survey date.

	Means		
		Reason of displacement	
Variables	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	50.99	0	100
Previous tenure (years)	5.71	6.55	4.90
	(6.57)	(7.06)	(5.97)
Change in log real weekly earnings	-.236	-.211	-.260
	(.501)	(.474)	(.525)
Log of previous weekly earnings	6.236	6.170	6.300
	(.524)	(.490)	(.548)
Log of current weekly earnings	6.000	5.958	6.040
	(.532)	(.518)	(.543)
Length of unemployment (weeks)	22.98	23.47	22.50
	(6.39)	(6.53)	(6.23)
No unemployment after displacement = 1 (percent)	0	0	0
Advance notice = 1 (percent)	49.71	57.84	41.89
Current education (years)	13.05	12.68	13.40
	(2.37)	(2.29)	(2.39)
Current (age-education-6) (years)	19.05	20.28	17.86
	(10.34)	(10.50)	(10.06)
White collar in previous job = 1 (percent)	44.44	38.08	50.55
Previous job in manufacturing = 1 (percent)	43.73	44.47	43.01
Current age (years)	38.10	38.96	37.27
	(10.20)	(10.17)	(10.17)
Currently married = 1 (percent)	68.80	71.22	66.48
Non white = 1 (percent)	11.53	12.50	10.61
N	702	344	358

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 5. Descriptive statistics for displaced workers by whether they were displaced from white- or blue-collar jobs using the DWS (1988-90-92), males re-employed at survey date

	Means					
	White-collar			Blue-collar		
		Reason of displacement			Reason of displacement	
Variables	Entire sample	Plant closing	Layoff	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	50.36	0	100	45.36	0	100
Previous tenure (years)	5.12	5.47	4.78	4.85	5.94	3.53
	(6.31)	(6.57)	(6.02)	(6.08)	(6.83)	(4.70)
Change in log real weekly earnings	-.128	-.109	-.146	-.144	-.124	-.169
	(.484)	(.466)	(.501)	(.489)	(.477)	(.503)
Log of previous weekly earnings	6.354	6.315	6.392	6.016	6.043	5.983
	(.569)	(.581)	(.555)	(.497)	(.504)	(.487)
Log of current weekly earnings	6.226	6.206	6.245	5.871	5.919	5.814
	(.580)	(.581)	(.579)	(.522)	(.493)	(.550)
Length of unemployment (weeks)	8.77	7.65	9.87	8.62	8.61	8.63
	(9.28)	(9.07)	(9.35)	(9.38)	(9.71)	(8.98)
No unemployment after displacement = 1 (percent)	16.95	22.93	11.05	13.55	16.20	10.36
Advance notice = 1 (percent)	49.30	60.06	38.69	52.93	57.76	47.10
Current education (years)	14.26	14.01	14.51	11.96	11.83	12.12
	(2.16)	(2.22)	(2.07)	(2.11)	(2.16)	(2.03)
Current (age-education-6) (years)	17.52	17.64	17.40	17.38	18.58	15.94
	(10.12)	(10.04)	(10.21)	(10.27)	(10.48)	(9.82)
White collar in previous job = 1 (percent)	100	100	100	0	0	0
Previous job in manufacturing = 1 (percent)	28.66	26.07	31.21	51.40	51.28	51.54
Current age (years)	37.79	37.66	37.91	35.35	36.41	34.07
	(10.03)	(9.86)	(10.20)	(9.99)	(10.12)	(9.69)
Currently married = 1 (percent)	70.02	70.13	69.91	67.64	69.72	65.14
Non white = 1 (percent)	8.27	8.74	7.80	11.35	12.40	10.09
N	1,221	606	615	1,638	895	743

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 6. Descriptive statistics for displaced workers by whether they were displaced from industries with high-unionization rates or not using the DWS (1988-90-92), males re-employed at survey date

	Means					
	Non-unionized			Unionized		
	Reason of displacement			Reason of displacement		
Variables	Entire sample	Plant closing	Layoff	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	47.72	0	100	47.32	0	100
Previous tenure (years)	4.01	4.39	3.58	5.73	6.83	4.51
	(5.34)	(5.47)	(5.17)	(6.67)	(7.41)	(5.50)
Change in log real weekly earnings	-.091	-.073	-.111	-.174	-.153	-.197
	(.499)	(.480)	(.518)	(.475)	(.464)	(.486)
Log of previous weekly earnings	6.092	6.084	6.101	6.215	6.207	6.223
	(.586)	(.594)	(.578)	(.522)	(.511)	(.534)
Log of current weekly earnings	6.001	6.011	5.990	6.040	6.053	6.025
	(.596)	(.571)	(.623)	(.556)	(.529)	(.586)
Length of unemployment (weeks)	7.98	7.33	8.69	9.24	8.93	9.59
	(8.78)	(8.95)	(8.55)	(9.72)	(9.81)	(9.62)
No unemployment after displacement = 1 (percent)	16.03	21.95	9.55	14.17	16.50	11.58
Advance notice = 1 (percent)	46.93	57.29	35.58	54.94	59.80	49.53
Current education (years)	13.31	13.11	13.54	12.65	12.39	12.93
	(2.48)	(2.50)	(2.43)	(2.32)	(2.33)	(2.28)
Current (age-education-6) (years)	16.09	16.15	16.02	18.53	19.83	17.07
	(9.96)	(9.75)	(10.19)	(10.27)	(10.46)	(9.86)
White collar in previous job = 1 (percent)	59.11	56.84	61.61	29.55	27.27	32.09
Previous job in manufacturing = 1 (percent)	0	0	0	75.11	73.80	76.56
Current age (years)	35.41	35.26	35.56	37.18	3823	36.01
	(9.93)	(9.62)	(10.28)	(10.13)	(10.17)	(9.97)
Currently married = 1 (percent)	63.99	64.06	63.92	72.40	74.52	70.03
Non white = 1 (percent)	9.43	10.52	8.23	10.52	11.24	9.72
N	1,272	665	607	1,587	836	751

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 7. Descriptive statistics for displaced workers by cause of displacement using the DWS (1988-90-92), males re-employed at survey date

	Means		
	Reason of displacement		
Variables	Plant closing	Slack work	Shift or position abolished
Layoff = 1 (percent)	0	100%	100%
Previous tenure (years)	5.75	3.41	5.50
	(6.73)	(4.59)	(6.47)
Change in log real weekly earnings	-.118	-.141	-.193
	(.473)	(.496)	(.514)
Log of previous weekly earnings	6.153	6.080	6.347
	(.552)	(.541)	(.547)
Log of current weekly earnings	6.035	5.939	6.153
	(.548)	(.595)	(.593)
Length of unemployment (weeks)	8.22	9.21	9.15
	(9.47)	(9.19)	(9.12)
No unemployment after displacement = 1 (percent)	18.92	9.33	13.42
Advance notice = 1 (percent)	58.69	45.22	39.37
Current education (years)	12.71	12.88	13.86
	(2.43)	(2.35)	(2.28)
Current (age-education-6) (years)	18.20	15.78	18.27
	(10.31)	(9.86)	(10.14)
White collar in previous job = 1 (percent)	40.37	36.99	62.19
Previous job in manufacturing = 1 (percent)	41.10	45.88	35.12
Current age (years)	36.92	34.67	38.13
	(10.03)	(10.01)	(9.91)
Currently married = 1 (percent)	69.88	64.10	73.82
Non white = 1 (percent)	10.92	10.20	6.71
N	1,501	911	447

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 8. Descriptive statistics for displaced workers by whether they received advance notice using the DWS (1988-90-92), males re-employed at survey date

	Means					
	No advance notice			Advance notice		
		Reason of displacement			Reason of displacement	
Variables	Entire sample	Plant closing	Layoff	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	55.39	0	100	40.02	0	100
Previous tenure (years)	4.32	4.85	3.90	5.57	6.38	4.36
	(5.64)	(6.18)	(5.14)	(6.59)	(7.03)	(5.65)
Change in log real weekly earnings	-.153	-.135	-.166	-.122	-.105	-.148
	(.517)	(.534)	(.503)	(.457)	(.424)	(.502)
Log of previous weekly earnings	6.142	6.122	6.158	6.178	6.175	6.182
	(.579)	(.586)	(.573)	(.530)	(.527)	(.536)
Log of current weekly earnings	5.988	5.986	5.991	6.055	6.069	6.033
	(.601)	(.561)	(.631)	(.547)	(.536)	(.563)
Length of unemployment (weeks)	9.10	8.512	9.57	8.29	8.02	8.69
	(9.32)	(9.40)	(9.24)	(9.33)	(9.52)	(9.05)
No unemployment after displacement = 1 (percent)	10.79	14.67	7.66	18.99	21.90	14.62
Advance notice = 1 (percent)	0	0	0	100	100	100
Current education (years)	12.95	12.59	13.24	12.94	12.79	13.15
	(2.47)	(2.45)	(2.45)	(2.36)	(2.42)	(2.26)
Current (age-education-6) (years)	17.52	18.01	17.12	17.37	18.33	15.91
	(10.40)	(10.55)	(10.26)	(10.02)	(10.15)	(9.66)
White collar in previous job = 1 (percent)	44.53	39.03	48.96	40.98	41.31	40.47
Previous job in manufacturing = 1 (percent)	37.55	36.45	38.44	45.60	44.38	47.44
Current age (years)	36.48	36.61	36.37	36.31	37.13	35.07
	(10.23)	(10.13)	(10.33)	(9.94)	(9.97)	(9.76)
Currently married = 1 (percent)	68.05	67.74	68.31	69.23	71.39	65.98
Non white = 1 (percent)	10.28	10.80	9.87	9.80	11.01	7.99
N	1,390	620	770	1,469	881	588

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 9. Descriptive statistics for displaced workers by whether they received unemployment insurance benefits using the DWS (1988-90-92), males re-employed at survey date

	Means					
	No UI benefits			UI benefits		
		Reason of displacement			Reason of displacement	
Variables	Entire sample	Plant closing	Layoff	Entire sample	Plant closing	Layoff
Layoff = 1 (percent)	44.33	0	100	50.43	0	100
Previous tenure (years)	4.30	4.79	3.69	5.58	6.75	4.43
	(5.78)	(5.92)	(5.54)	(6.47)	(7.36)	(5.22)
Change in log real weekly earnings	-.077	-.061	-.097	-.193	-.177	-.209
	(.489)	(.480)	(.499)	(.479)	(.457)	(.499)
Log of previous weekly earnings	6.11	6.13	6.08	6.207	6.176	6.238
	(.600)	(.597)	(.604)	(.504)	(.501)	(.505)
Log of current weekly earnings	6.03	6.06	5.98	6.014	5.998	6.029
	(.609)	(.573)	(.649)	(.541)	(.519)	(.561)
Length of unemployment (weeks)	4.29	3.76	4.96	12.76	12.88	12.65
	(6.48)	(6.17)	(6.80)	(9.73)	(10.06)	(9.40)
No unemployment after displacement = 1 (percent)	28.01	33.76	20.78	2.90	3.40	2.40
Advance notice = 1 (percent)	51.37	57.88	43.20	51.38	59.53	43.37
Current education (years)	13.06	12.87	13.30	12.84	12.54	13.12
	(2.51)	(2.49)	(2.52)	(2.32)	(2.36)	(2.23)
Current (age-education-6) (years)	16.55	16.86	16.17	18.27	19.61	16.95
	(10.20)	(9.97)	(10.48)	(10.14)	(10.49)	(9.63)
White collar in previous job = 1 (percent)	45.86	44.19	47.95	39.77	36.37	43.10
Previous job in manufacturing = 1 (percent)	36.79	34.81	39.27	46.25	47.68	44.84
Current age (years)	35.62	35.73	35.47	37.11	38.15	36.08
	(10.21)	(9.81)	(10.69)	(9.91)	(10.12)	(9.60)
Currently married = 1 (percent)	66.90	66.88	66.93	70.29	73.02	67.60
Non white = 1 (percent)	9.57	10.56	8.34	10.46	11.30	9.63
N	1,378	767	611	1,481	734	747

Note.- Standard deviations are in parenthesis. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 1992). The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties.

Table 10. Earnings equation using the DWS (1988-90-92), males re-employed at survey date

	Dependent variables: Earnings♣		
N = 2,859	Weekly change	Pre-displacement	Post-displacement
Unemployment spell	-.016	.002	-.013
	(.004)	(.003)	(.004)
Unemployment spell squared	.0003	-.0001	.0002
	(.0001)	(.005)	(.0001)
Layoff dummy	-.086	-.005	-.091
	(.031)	(.029)	(.033)
Layoff dummy	.016	-.003	.013
x unemployment spell	(.006)	(.005)	(.006)
Layoff dummy	-.0006	.0002	-.0003
x unemployment spell squared	(.0002)	(.0001)	(.0002)
R-squared	.1039	.3941	.3056

Note: The number in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are: a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years); four dummies for education (one for "twelve years completed"; one for "some college but less than four years of college completed"; one for "college degree but no graduate degree"; and one for "more than four years of college"); nine "year-of-displacement" dummies; one "advance notification" dummy; five "previous-industry" dummies; four "previous-occupation" dummies; one "experience at survey date" variable and its square; one "pre-displacement marital status" dummy; one "non-white" dummy; and three "region" dummies.

♣ Dependent variable: col. 1 = log(current earnings/previous earnings); col. 2 = log (previous earnings deflated by GDP deflator); and col. 3 = log(current earnings deflated by GDP deflator)

Table 11. Earnings equation using the DWS (1988-90-92), workers displaced from white- and blue-collar jobs, re-employed at survey date

	Dependent variables: Earnings♣					
	White-collar (N=1,221)			Blue-collar (N=1,638)		
	Weekly change	Pre-displacement	Post-displacement	Weekly change	Pre-displacement	Post-displacement
Unemployment spell	-.022	.003	-.018	-.012	.002	-.009
	(.006)	(.007)	(.007)	(.005)	(.004)	(.005)
Unemployment spell squared	.0005	-.0001	.0003	.0002	-.0001	.0001
	(.0002)	(.0002)	(.0002)	(.0001)	(.0001)	(.0001)
Layoff dummy	-.090	.047	-.043	-.070	-.032	-.102
	(.048)	(.047)	(.052)	(.043)	(.037)	(.043)
Layoff dummy	.029	-.017	.012	.004	.004	.009
x unemployment spell	(.009)	(.009)	(.010)	(.008)	(.007)	(.008)
Layoff dummy	-.001	.0007	-.0003	-.0001	-.0000	-.0001
x unemployment spell squared	(.0003)	(.0003)	(.0003)	(.0002)	(.0002)	(.0002)
R-squared	0.1597	0.3697	0.2815	.0918	0.3362	0.2291

Note: The white-collar sample consists of workers whose pre-displacement occupations were in the managerial and professional specialties or in the technical, sales and administrative support specialties. The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are described in table 10.

♣ Dependent variable: col. 1 and 4 = log(current earnings/previous earnings); col. 2 and 5 = log (previous earnings deflated by GDP deflator); and col. 3 and 6 = log(current earnings deflated by GDP deflator)

Table 12. Earnings equation using the DWS (1988-90-92), workers displaced from non-unionized and unionized jobs, re-employed at survey date

	Dependent variables: Earnings♣					
	Non-unionized (N=1,272)			Unionized (N=1,587)		
	Weekly change	Pre-displacement	Post-Displacement	Weekly change	Pre-displacement	Post-displacement
Unemployment spell	-.018	-.002	-.020	-.016	.007	-.008
	(.007)	(.007)	(.007)	(.005)	(.004)	(.005)
Unemployment spell squared	.0004	-.0000	.0005	.0003	-.0003	.0000
	(.0002)	(.0002)	(.0002)	(.0001)	(.0001)	(.0001)
Layoff dummy	-.058	-.054	-.112	-.120	.041	-.078
	(.050)	(.048)	(.052)	(.041)	(.036)	(.042)
Layoff dummy	.020	.002	.023	.016	-.009	.007
x unemployment spell	(.010)	(.010)	(.011)	(.007)	(.006)	(.008)
Layoff dummy	-.0008	.0002	-.0006	-.0005	-.0004	-.0000
x unemployment spell squared	(.0003)	(.0003)	(.0003)	(.0002)	(.0002)	(.0002)
R-squared	0.1141	0.3876	0.3246	.1061	0.4003	0.3038

Note: The unionized sample consists of workers whose pre-displacement industries had unionization rates above the sample mean rate. The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are described in table 10.

♣ Dependent variable: col. 1 and 4 = log(current earnings/previous earnings); col. 2 and 5 = log (previous earnings deflated by GDP deflator); and col. 3 and 6 = log(current earnings deflated by GDP deflator)

Table 13. Earnings equation by cause of displacement using the DWS (1988-90-92), workers re-employed at survey date

	Dependent variables: Earnings♣					
	Slack work and plant closings (N=2,412)			Shift or position abolished and plant closings (N=1,948)		
	Weekly change	Pre-displacement	Post-displacement	Weekly change	Pre-displacement	Post-Displacement
Unemployment spell	-.016	.002	-.013	-.016	.002	-.013
	(.004)	(.003)	(.004)	(.004)	(.004)	(.004)
Unemployment spell squared	.0003	-.0001	.0002	.0003	-.0001	.0002
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Layoff dummy	-.079	-.051	-.131	-.105	.089	-.015
	(.036)	(.033)	(.037)	(.048)	(.043)	(.050)
Layoff dummy	.016	.003	.020	.018	-.016	.001
x unemployment spell	(.007)	(.006)	(.007)	(.009)	(.008)	(.009)
Layoff dummy	-.0006	.0000	-.0005	-.0006	.0007	.0001
x unemployment spell squared	(.0002)	(.0002)	(.0002)	(.0003)	(.0002)	(.0003)
R-squared	0.1007	0.3857	0.3016	.1230	0.3894	0.3026

Note: The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are described in table 10.

♣ Dependent variable: col. 1 and 4 = log(current earnings/previous earnings); col. 2 and 5 = log (previous earnings deflated by GDP deflator); and col. 3 and 6 = log(current earnings deflated by GDP deflator)

Table 14. Earnings equation using the DWS (1988-90-92), workers who did and did not receive advance notice, re-employed at survey date

	Dependent variables: Earnings[♣]					
	No advance notice (N=1,390)			Advance notice N=(1,469)		
	Weekly change	Pre-displacement	Post-displacement	Weekly change	Pre-displacement	Post-displacement
Unemployment spell	-.022	.001	-.020	.010	.003	-.007
	(.007)	(.006)	(.006)	(.004)	(.004)	(.005)
Unemployment spell squared	.0005	-.0001	.0004	.0002	-.0001	.0000
	(.0002)	(.0002)	(.0002)	(.0001)	(.0001)	(.0001)
Layoff dummy	-.139	-.033	-.172	-.028	.013	-.014
	(.052)	(.046)	(.051)	(.039)	(.038)	(.042)
Layoff dummy x unemployment spell	.031	-.0003	.030	.0005	-.006	-.005
	(.009)	(.008)	(.009)	(.008)	(.008)	(.009)
Layoff dummy x unemployment spell squared	-.001	.0002	-.0007	-.0001	.0003	.0002
	(.0003)	(.0002)	(.0003)	(.0003)	(.0002)	(.0003)
R-squared	0.1134	0.4215	0.3140	.1223	.3835	.3184

Note: The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are described in table 10.

♣ Dependent variable: col. 1 and 4 = log(current earnings/previous earnings); col. 2 and 5 = log (previous earnings deflated by GDP deflator); and col. 3 and 6 = log(current earnings deflated by GDP deflator)

Table 15. Earnings equation using the DWS (1988-90-92), workers who received unemployment insurance benefits or not, re-employed at survey date

	Dependent variables: Earnings[♣]					
	No unemployment benefits (N=1,378)			Unemployment benefits (N=1,481)		
	Weekly change	Pre-displacement	Post-displacement	Weekly change	Pre-displacement	Post-displacement
Unemployment spell	-.017	.0006	-.016	-.014	.007	-.006
	(.007)	(.007)	(.007)	(.005)	(.005)	(.006)
Unemployment spell squared	.0006	-.0001	.0004	.0003	-.0002	.0000
	(.0002)	(.0002)	(.0002)	(.0001)	(.0001)	(.0001)
Layoff dummy	-.065	-.016	-.081	-.166	.088	-.078
	(.038)	(.036)	(.039)	(.060)	(.052)	(.063)
Layoff dummy x unemployment spell	.025	-.012	.013	.023	-.012	.010
	(.011)	(.011)	(.011)	(.009)	(.008)	(.009)
Layoff dummy x unemployment spell squared	-.001	.0006	-.0004	-.0006	.0004	-.0001
	(.0004)	(.0004)	(.0004)	(.0002)	(.0002)	(.0002)
R-squared	.0887	0.4327	0.3632	0.1293	0.3631	0.2701

Note: The numbers in parentheses are standard errors. All regressions use the White estimator of variance. The covariates are described in table 10.

♣ Dependent variable: col. 1 and 4 = log(current earnings/previous earnings); col. 2 and 5 = log (previous earnings deflated by GDP deflator); and col. 3 and 6 = log(current earnings deflated by GDP deflator)