Displacement, Signaling, and Recall Expectations

Núria Rodríguez-Planas

April 2011

Barcelona GSE Working Paper Series

Working Paper nº 550

Displacement, Signaling, and Recall Expectations

Núria Rodríguez-Planas

Universitat Autònoma de Barcelona and Barcelona GSE

January 2010

(revised April 2011)

Abstract

This paper is the first to present empirical evidence consistent with models of signaling through unemployment and to uncover a new stylized fact using the 1988-2006 DWS, namely that, among white-collar workers, post-displacement earnings fall less rapidly with unemployment spells for layoffs than for plant closings. Because high-productivity workers are more likely to be recalled than low-productivity ones, they may choose to signal their productivity though unemployment, in which case the duration of unemployment may be positively related to post-displacement wages. Identification is done using workers whose plant closed as they cannot be recalled, and no incentives to signal arise.

KEYWORDS: Asymmetric information, laid-off workers, recalls, unemployment, and wages.

JEL Classification Numbers: J60, J30.

1. Introduction

Since the seminal work by Akerlof, 1970, many economists have analyzed how informational asymmetries affect the behavior of economic agents. This is especially true in regard to the labor market, where the effect of imperfect information about workers' productivity on labor market outcomes has long been recognized.[1] In spite of this extensive theoretical interest, empirical evidence on the extent of the problem in the labor market has, until recently, been relatively sparse.[2] In these papers, worker quality is imperfectly observed, and so potential employers must contend with the possibility of hiring a "lemon". When faced with an adverse shock, employers prefer dismissing low productivity workers. As a result, in equilibrium, employed workers are more productive than the unemployed, or certain types of displaced workers are more productive than other types. If employers believe that the least productive workers are concentrated in the pool of unemployed workers, or certain types of unemployed workers, they may use the employment history of the worker as a sorting criterion.

Although these papers provide empirical evidence that potential employers are aware of the existence of adverse selection in the labor market, they do not analyze whether workers, who are likely to have private information on their productivity, take costly actions to signal to prospective employers' favorable information. Rodríguez-Planas, 2009, develops a theoretical model that suggests that at least some aspect*s* of the search decisions of workers on temporary layoff may have a signaling component.[3] The main idea behind Rodríguez-Planas' paper is that 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

[1] Spence, 1973; Waldman, 1984; Greenwald, 1986; McCormick, 1990; Ma and Weiss, 1993; Riordan and Staiger, 1993; Gottfries and McCormick, 1995; Montgomery, 1999; Strand, 2000; and Eriksson, 2002, among others.

[2] Gibbons and Katz, 1991; Canziani and Petrolongo, 2001; Kugler and Saint-Paul, 2004; Schönberg, 2007; Hu and Taber, 2008; Khan, 2007; and Zhang, 2007.

[3] In this paper (as well as in the current one), temporary laid-off workers are laid-off workers who *initially* expected to be recalled to their former job despite not having a definite recall date. However, it is important to notice that these *ex-ante* temporary layoffs may end up *not* being recalled to their former employer, and *ex-post* becoming permanent layoffs and having to find a new job with a *new* employer.

remain unemployed rather than to accept a low-wage job. If so, unemployment can serve as a signal of productivity.[4]

The contribution of the current paper is to develop an empirical implication of this model, namely that unemployment duration is positively related to post-displacement earnings even among laid-off workers who are *not* recalled.[5] Using 1988-2006 Displaced Workers Supplements to the Current Population Survey, this paper offers quantitative empirical evidence consistent with signaling through unemployment among white-collar workers. Most importantly, it uncovers a new empirical fact about white-collar laid-off workers in the United States.

The identification strategy that we follow is to use workers displaced through plant closings to control for all factors affecting earnings and the duration of unemployment not associated with having a positive probability of recall. The assumption is that workers displaced through plant closings cannot be recalled, which, in the Rodriguez-Planas' model implies that they will not have any incentives to signal through unemployment. Therefore the empirical hypothesis is that post-displacement earnings fall *less* rapidly with unemployment spells for layoffs than for plant closings. While this identification strategy is similar to that of Gibbons and Katz, 1991, it is important to note that this paper does *not* really rely on Gibbons and Katz's lemon effect of layoffs *per se*; because even *within* each of the layoff and plant closing samples, there is heterogeneity in worker's ability unobservable to potential employers. This implies that the relevant comparative statics are the difference in the *slope* of post-displacement wage against unemployment duration between layoffs and plant closings (not the difference in level). Thus, the results from this

[4] Ma and Weiss, 1993, and Gottfries and McCormick, 1995, have also developed models in which least-able workers choose low-skilled jobs and more-able ones choose unemployment. While Rodríguez-Planas' paper analyses the effect of recalls on workers' unemployment when employers have better information about their workers than outside firms do, Ma and Weiss, and Gottfries and McCormick focus on the interaction between signaling and testing.

[5] Recalled laid-off workers return to their old employer (and most likely to their old job) and therefore their earnings losses are small relative to permanently displaced workers (Katz and Meyer, 1990). The prediction of the model is for permanently displaced workers, and the empirical analysis is also done for permanently displaced workers.

paper cannot be interpreted as evidence either for or against the presence of the type of asymmetric information studied in Gibbons and Katz, 1991.[6]

The empirical findings are consistent with the signaling model of unemployment for white-collar workers. We find that white-collar workers' post-displacement earnings do not fall as rapidly with unemployment-spell length for laid-off workers compared to workers displaced by plant closings. No such differential effect between layoffs and plant closings is found among blue-collar workers. As explained below, the lack of effects for the blue-collar sub-sample is also consistent with the model, because the information content of a recall is small when employers lack the ability to act on this information. Additional evidence that supports the model is provided. In support of the signaling explanation, we demonstrate that the estimates from the white-collar sub-sample are surprisingly robust to inclusion of region, industry, occupation dummies, and pre-displacement earnings. This suggests that the patterns encountered are not simply due to differences in the sector of the economy in which workers were employed. Finally, the paper explores whether the empirical findings are also compatible with explanations other than signaling.

The next two sections discuss the literature review and present background information on the relevance of recalls in the labor market. Section four presents a simple version of the model demonstrating that the intuition holds in equilibrium and develops the empirical implication of the model. Section five describes the data, and reports the results. Section six concludes.

2. Literature Review

While there is a vast literature on signaling through education, empirical evidence on signaling through a costly action other than education is, to the best of my knowledge, scarce. Weiss, 1995, provides a thorough review of early studies on signaling through education. More recently, several authors have developed models of symmetric employer

[6] See Krashinsky, 2002, and Song, 2007, for evidence against Gibbons and Katz's lemons effect.

learning where they assume that employers statistically discriminate among prospective workers on the basis of a signal (usually education) which is related to the (unobserved) ability of a worker. Using the NLSY, Farber and Gibbons, 1996; and Altonji and Pierret, 1997, 2001, test the symmetric employer learning model and find evidence supportive of learning being symmetric across employers. Bauer and Haisken-DeNew, 2001, and Galindo-Rueda, 2002, obtain similar findings for white-collar workers in Germany and workers in the UK, respectively. Lange, 2007, provides an estimate for the speed of employer learning, and Arcidiacono *et al.*, 2008, provide evidence that education (specifically, attending college) plays a much more direct role in revealing (as opposed to signaling) ability in the college labor market.

Several authors have made an attempt to distinguish between symmetric and asymmetric employer learning by introducing endogenous mobility in their models.[7] The first author do to this, Schönberg, 2007, finds evidence supportive of asymmetric employer learning for college graduates, but not for high-school graduates. Similarly, Zhang, 2007, also finds evidence supportive of adverse selection in the labor market by examining the impact of education on wages for individuals with different job turnover patterns using a three-period model. Using a model where firms compete for workers through bidding wars, Pinkston, 2009, finds evidence of both public learning and asymmetric information in the labor market. In contrast with the paper at hand, all of these papers exploit the empirical methodology first developed by Altonji and Pierret, that is, they use education as the identifying variable.[8] In addition, they focus their analysis on testing adverse selection in the labor market as opposed to workers' signaling (through an action other than education.)

[7] They endogenize mobility by adding non-pecuniary job characteristics to the workers' utility function following Neal, 1998, and Acemoglu and Pischke, 1998.

[8] One exception to the use of the methodology developed by Altonji and Pierret is Khan, 2007. This author also develops a model of asymmetric learning that nests the symmetric learning case and allows the degree of asymmetry to vary. The novelty of her model is that it derives a new dependent variable for identifying employer learning: the variance in pay changes. Khan, 2007, finds evidence supportive of asymmetric information.

Finally, an additional related strand of the literature is the one first developed by Waldman, 1984, and based on the idea that promotions serve as a signal of worker ability. Although this line of research has received considerable theoretical attention (Milgrom and Oster, 1987; Ricart i Costa, 1988; Waldman, 1990; Bernhardt, 1995; Chang and Wang, 1996; Zabojnik and Bernhardt, 2001; Owan, 2004; and Golan, 2005), it is only recently that the idea has been tested empirically. To the best of my knowledge, DeVaro and Waldman, 2006, were the first ones to empirically test the promotion-as-signal hypothesis and to find support for their theory. They derive two predictions consistent with their model. First, workers with high levels of education are promoted faster. Second, the wage increase associated with a promotion decreases with education. Using proprietary data from a single, large American firm in the financial services industry, they find empirical support for both predictions. Similarly, Belzil and Bognanno, 2005, find evidence consistent with the promotion-as-signal hypothesis using an eight-year panel of promotion histories of 30,000 American executives. And DeVaro, Ghosh, and Zoghi, 2008, study discrimination in promotion decisions using a promotion discrimination model based on job assignment signaling. Using personnel data from a large U.S. firm and data from the National Compensation Survey, the authors find strong empirical support for their model's predictions concerning promotion probabilities, whereas empirical support is mixed for the model's predictions concerning the wage growth attached to promotions. While in these models it is the employer who signals (through promotions) the worker ability, in the current paper it is the worker who decides whether she wants to take a costly action to signal favorable information to prospective employers.

3. Some Stylized Facts on Layoffs and Recalls

Many laid-off workers in the U.S. are rehired by their former employers. Early work by Lilien, 1980, documented that over 70% of workers laid off in U.S. manufacturing in the 1970s were subsequently rehired by their former employers. Katz, 1986, finds that this process is also widespread outside manufacturing. More recently, the Mass Layoff

Statistics program reports that over half of employers reporting a layoff in 2008 indicated that they anticipated some type of recall (US Bureau of Labor Statistics, 2009). It also reports that among all establishments expecting to recall workers, most employers (88%) expected to recall at least half of the separated employees. Finally, even in the midst of the current recession, the evidence indicates that about one fifth of laid-off workers who landed new positions within the last year were rehired by the same employer that had let them go (CNNmoney.com, 2009).

Studies by Robertson, 1989; Corak, 1996; and Raaf *et al.,* 2003, present comparable figures for Canada. Although temporary layoffs are thought to be quantitatively more important in North America than in Europe—mainly due to the tighter recruitment and dismissal regulations existing in the old continent—, empirical evidence has also found that this phenomenon exist in many European countries. For instance, Jensen and Svarer, 2003, report that about half of all unemployment spells in Denmark were due to temporary layoffs. Similarly, Jansson, 2002, calculates that about 45% of all transitions from unemployment in Sweden ended with the worker returning to the previous employer. In other European countries the recall rate has been estimated to be close to one third: 37% in Spain, 32% in Austria, and 26% in Germany (Alba-Ramirez *et al.,* 2007; Fischer and Pichelmann, 1991; and Mavroramas and Orme, 2004, respectively).

In the United States, most recalls take place within the first three months and few occur after 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. More recently, the Mass Layoff Statistics, 2009, program reports that 60% (85%) of those employers expecting to recall workers expect to do so within three (six) months.

At the same time, most layoffs in the United States find jobs within 3 months. For instance, in the sample used in this paper, 70% of laid-off workers displaced from white-collar jobs find a job within 3 months (the average unemployment spell of laid-off workers displaced from white-collar jobs is 12.31 weeks—with a standard error of 16.92—, and the median is at 6 weeks of unemployment.) Using a very different sample of displaced workers, Anderson, 1992, also finds that about 70% of workers expecting a recall have exited unemployment within the first 12 weeks of their unemployment spell.

4. Model of Signaling through Unemployment and Empirical Predictions

The main idea behind Rodríguez-Planas' 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.[9] At displacement, laid-off workers with favorable information may choose to remain unemployed rather than to accept a low-wage job, in which case, unemployment can serve as a signal of productivity. The contribution of the current paper is to develop an empirical implication of this model, namely that unemployment duration is positively related to post-displacement earnings even among laid-off workers who are *not* recalled, and to test it. For the paper to be self-contained, this section first presents the theoretical model. We then explain the empirical implication of this model and discuss the empirical implementation.

The Model

There are two periods. 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 worker). The productivity of a G-type worker with the original employer is H and that of a B-type worker is L, with $0<L<H$. I assume that there is a continuum of workers of each type, t, where $t = B$ or G. The cumulative distribution of all workers is normalized to '1'. The proportion of G-type workers (respectively, B-type workers) is α (respectively, $1-\alpha$), where $0<\alpha<1$.

Both the worker and the original employer know the worker's type, t, with that particular employer. However, laid-off workers are assumed to look identical to other potential employers. G-type workers are more likely to be of high productivity with a new

[9] An underlying assumption is that employers have discretion over whom to layoff and recall. In practice, employers may rehire according to a seniority rule.

employer than B-type workers. Specifically, a type-t worker will be of high productivity with a new employer (that is, with productivity equal to H) with probability p_t, $t = B$ 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. After the worker remains with an employer for one period, his productivity with that particular employer is revealed to both the worker and the employer, but not to other employers.

At the beginning of period one, workers are laid off. In this period, prospective employers simultaneously offer laid-off workers a first-period wage. Workers observe that wage and choose either to work for a new employer—accepting the highest wage offered (randomizing in case of a tie)—or to become unemployed. If the worker becomes unemployed, his current income is U, where $U \geq 0$. One can think of U as unemployment insurance (UI). I assume that U is financed by a constant payroll tax, ζ,, on all workers. I also assume that $U<L-\zeta$, to prevent workers always preferring unemployment over a job. To reduce the notational burden, I will set the reservation value to $U_0 \equiv U + \zeta$.

At the beginning of period two, the original employer recalls those former workers who are still unemployed with probability r_t, $t = B,$ or G. I assume that $r_B<r_G\leq 1$. This assumption 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 previous employer does not recall those workers who are of low productivity at his firm). Because I assumed that $r_B=0$, let $r_G=r$. Prospective employers observe that some unemployed workers are not recalled and they simultaneously offer them 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 maximize expected lifetime income. A large finite number of employers exist, and they maximize the present value of profits. Therefore, each period employers offer a wage equal to workers' (expected) productivity. Workers and firms are risk-neutral, and they know the population parameters: α, r, p_t, H, and L. I also assume that once a worker accepts a job offer, he is precluded from receiving a future offer from a new employer; and that after accepting an offer, workers cannot quit to return to a former employer. The latter assumption is consistent with the empirical evidence, which indicates that most workers who expect to be recalled remain unemployed instead of taking some interim job (Katz, 1986; Katz and Meyer, 1990; and Anderson, 1992, among others). Moreover, it is plausible that workers who believe that they are on temporary layoffs will consider—at least during the first few weeks—their laid-off time as time off from work to spend fixing up the house or catching up on personal things to do. Finally, this is not an unusual assumption in the theoretical literature on layoffs (Feldstein, 1976; Pissarides, 1982; Akerlof *et al.,* 1990).[10]

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 life-time 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.

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

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

Theorem 1. A necessary condition for a perfect Bayesian equilibrium in which some

[10] This assumption could be endogenized into the model. For instance, we 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. Alternatively, we could assume that workers bear a cost of generating a job offer or a once-for-all cost of changing jobs.

workers choose to remain unemployed is:

$$(1-p_B) \geq \frac{L-U_0}{H-L} \qquad (1)$$

Note that H and L are, respectively, the maximum and minimum wages that firms would offer to workers who are unemployed one period. L-U_0 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, lemmas 1-3 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 are unemployed in the first period. B-type workers may be all employed (the fully-separating equilibrium for which conditions are given in lemma 1); (b) Some employed and some unemployed (the semi-separating equilibrium for which conditions are given in lemma 2); or (c) All unemployed (the pooling equilibrium for which conditions are given in lemma 3). The parameters values uniquely determine which of these equilibria applies. For brevity, I examine below only the conditions under which Lemma 1 holds. The characterization of Lemmas 2 and 3 can be found in Rodríguez-Planas' 2009 Oxford Economic Paper.

Lemma 1. For parameter values such that:

$$r(1-p_G)-p_B \geq \frac{L-U_0}{H-L} \qquad (2a)$$

and

$$p_G - 2p_B < \frac{L-U_0}{H-L} \qquad (2b)$$

the unique perfect Bayesian equilibrium is one in which all G-type workers reject the first-period offer and all B-type workers accept it.

When conditions (2a) and (2b) hold, the minimum cost of signaling by choosing unemployment is smaller than the maximum potential gain of G-type workers, but greater than the maximum potential gain of B-type workers. 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 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 (2a) and (2b) hold, all G-type workers choose to reject the first-period market offer, whereas all B-type workers accept it.

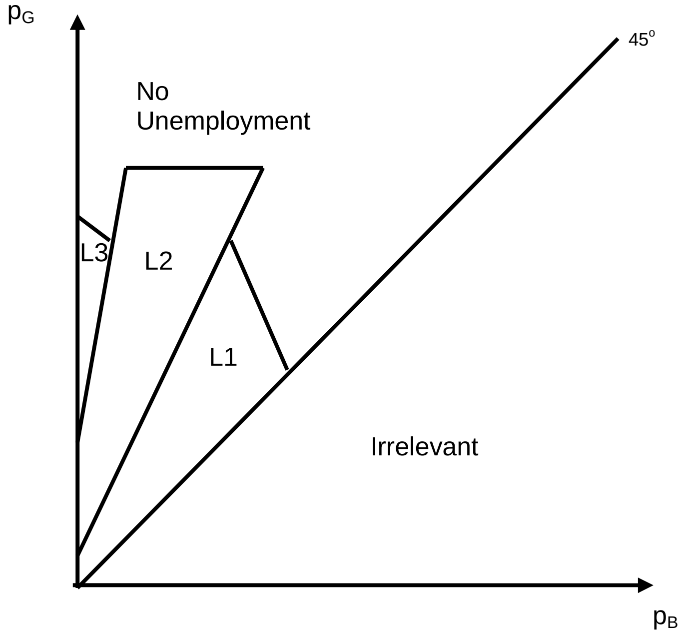
In this model, the equilibrium with no voluntary unemployment is also possible. However, under certain conditions, this equilibrium fails to satisfy the Cho-Kreps intuitive criterion. The intuitive criterion in this model is as follows: Starting from an equilibrium with no voluntary unemployment, a worker 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." [11]

Figure 1 illustrates the region where each of the equilibrium prevails.[12] L1 illustrates the equilibrium in Lemma 1, where only G-type workers choose unemployment; L2 illustrates the equilibrium in Lemma 2, where all G-type and some B-type workers choose unemployment, and L3 illustrates the equilibrium in Lemma 3, where all workers choose unemployment. The figure also shows the region where no unemployment arises. Notice that no unemployment arises in regions with high values of p_G. There are two reasons for this. In the region where the values of p_G are high relative to those of p_B (upper LHS of the figure), as the probability of a G-type worker of being a high-productivity worker with a new employer increases so does his cost of signaling relative to his potential expected gain, reducing the G-type worker's incentives to signal through unemployment.

[11] See Rodriguez-Planas, 2009, for proofs on the existence of the equilibrium with no voluntary unemployment, uniqueness of the equilibrium, and equilibrium refinements.

[12] Figure 1 has been computed for parameter values $\alpha = 0{,}4;$ $r = 0{,}5;$ $H = 5;$ $L = 1;$ $U = 0{,}6;$ and $\zeta = 0{,}2$. For other parameter values, some region may cease to exist but the sorting is always the same.

Fig. 1 Region where each of the equilibrium prevails



In contrast, in the region where the values of p_G approach those of p_B (upper RHS of the figure), the information content of the signal decreases since the comparative advantage of G-type workers in the spot market relative to B-types is reduced, making it less worthwhile to signal through unemployment. Figure 1 also shows that as the probability of a B-type worker of being a high-productivity worker with a new employer, p_B, decreases relative to p_G, B-type worker's incentive to behave strategically and to choose unemployment instead of a low-wage job increases.

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.[13] The next section presents

[13] 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 example, in the DWS sample of

the empirical implementation of this prediction and finds evidence consistent with the theoretical model using the Displaced Workers Supplement to Current Population Survey.

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 other factors, such as unobserved heterogeneity, loss of human capital, or stigma. 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. 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 the 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

---

laid-off workers used in the next section, more than 10 percent of laid-off workers find jobs without an intervening unemployment spell.

relationship between unemployment duration and post-displacement earnings for workers displaced because of plant closings.[14]

Therefore the empirical hypothesis is that post-displacement earnings fall *less* rapidly with unemployment spells for layoffs than for plant closings. To formally test this hypothesis, the following regression is estimated, separately for white-collar and blue-collar workers:

$$Y_i = \alpha_0 + \sum_{j=1}^{4} \alpha_j D_i^j + \beta_0 L_i + \sum_{j=1}^{4} \beta_j Z_i^j + X_i^{'} \delta + \xi_i \qquad (3)$$

where: D^j_i are four dummies for worker i initial length of joblessness, for $i=1,...N$: $D^1_i = 1$ if the worker's initial length of joblessness is 1 to 4 weeks long, and 0 elsewhere; $D^2_i = 1$ if the worker's initial length of joblessness is 5 to 12 weeks long, and 0 elsewhere; $D^3_i = 1$ if the worker's initial length of joblessness is 13 to 24 weeks long, and 0 elsewhere; and $D^4_i =$ 1 if the worker's initial length of joblessness is more than 24 weeks long, and 0 elsewhere; L_i is a dummy for cause of displacement for worker i for $i=1,...N$ ($L_i = 1$ if the worker is laid off, and 0 if the worker is displaced through plant closings); Z^j_i is the interaction between the layoff dummy and D^j_i dummies; and X_i is a vector of observable pre-displacement characteristics, that includes the log real pre-displacement weekly earnings, a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years), three dummies for completed education (one for "high school graduate"; one for "some college"; and one for "college graduate or above"), an "advance notice" dummy, year-of-displacement dummies; previous-industry and previous-occupation dummies; experience and its square; a gender dummy; marriage dummy; a non-white dummy; and three region dummies. All regressions use the Huber/White estimator of variance. The LHS variable is the logarithm of the post-displacement weekly earnings.

[14] 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.

Using the notations from equation (3), the prediction would translate to: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0.$

Because many fewer white- than blue-collar jobs are covered by collective-bargaining agreements involving explicit layoff- and recall-by-seniority rules, the degree of discretion over whom to lay off and recall is likely to be higher in white- than blue-collar jobs, and thus, the information content of a layoff and a recall is considerably higher in white- than blue-collar jobs (Gibbons and Katz, 1991, and Hu and Taber, 2008). Therefore, the 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. As in Gibbons and Katz, 1991, and Hu and Taber, 2008, the analysis is done separately for blue- and white-collar workers.

The results presented below are robust to modifying the length of the joblessness dummies, and to using an alternative specification including length of joblessness and its square (instead the dummies).

5. The Data and Results

5.1. The Data

The data used is from the Displaced Workers Supplements (DWS) to the Current Population Survey (CPS) between 1988 and 1992 and between 1996 and 2006 (all years included), and restricts the analysis to individuals who *permanently* lost a job within three years prior to the survey date. The reason for excluding the supplements from 1984, 1986 and, 1994 is that they do not include one of the key variables for the analysis: the variable 'initial length of unemployment spell'.[15] Prior to 1994, the DWS asked respondents if, in the prior five years, they had lost or left a job owing to a plant closing, slack work, a position or shift abolished, or other reasons. However, because many researchers highlighted the problem of recall bias when using the DWS, starting in 1994, the BLS

[15] The variable 'initial unemployment spell' was added starting in 1988, however, due to an error this variable was *not* collected for all displaced workers who were re-employed at the survey date in the 1994 supplement.

decided to ask about employment status in the past three years instead of the past five years.[16] For consistency purposes, I only used workers who had reported losing a job in the last three years in the 1988, 1990 and 1992 DWS, but the results presented below are robust to including workers who reported losing a job in the last five years.[17]

The DWS only asks follow-up questions about at most one lost job. If an individual lost multiple jobs, she was only asked about the job that had been held the longest. The post-displacement wage is for the current job held at the survey date, which is not necessarily the first job since displacement. To guaranty that the key variable "Initial unemployment spell" is from the spell *immediately* preceding the current job, I have excluded multiple job losers included in the sample. The sample is further restricted to workers between the ages of 20 and 61 who were permanently displaced from a private-sector, full-time job because of a plant closing, slack work, or abolishment of a position or shift. 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).[18] I focus on workers displaced from full-time jobs in an attempt to identify a sample of workers with strong attachments to the labor force. Like in Gibbons and Katz, 1991, I classified as laid-off workers those displaced because of slack work or a position or shift that was eliminated. The sample is restricted to those individuals who were re-employed in wage-and-salary employment at the survey date and who had re-employment earnings of at least $40 a week. Earnings are deflated by the gross domestic product deflator (base year = 2000). 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, while the blue-collar sample consists of workers who, in their pre-

[16] Recall bias arises because respondents forget less salient events from the distant past and fail to report them. Carrington, 1990; Topel, 1990; Evans and Leighton, 1995; Oyer, 2004; and Song, 2004, are some of the researchers who have found evidence of recall bias when using the DWS.

[17] In order to explore whether recall bias was affecting the results, the analysis was also done using workers who had reported losing a job in the last *two* years. The results in this paper are also robust to such sensitivity analysis. Estimates are available from the author upon request.

[18] 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.

displacement job, were craft and kindred workers, operatives, laborers, transport operatives, or service workers. Workers in agriculture and construction industries are excluded.[19]

The main focus of the present analysis is to analyze how the post-displacement earnings vary by cause of displacement and with the length of the unemployment spell. Descriptive statistics of the sample are reported in Tables 1A and 1B. The data are divided in twenty groups, classifying by blue- / white-collar, length of unemployment spell, and layoff / plant closing. The length of unemployment spell is divided in five groups: (1) no unemployment; (2) 1 to 4 weeks unemployed; (3) 5 to 12 weeks unemployed; (4) 13 to 24 weeks unemployed; (5) more than 24 weeks unemployed. Sample means and standard deviations for all of the variables are displayed in the cells. The key variables are displayed in the first three rows: (1) the logarithm of the previous weekly earnings; (2) the logarithm of the real weekly current earnings; and (3) the change in the logarithm of real weekly earnings. Focusing first on white-collar workers, while the post-displacement earnings of workers displaced through plant closings fall with their unemployment spell, no such pattern is observed among layoffs within the first 6 months of the unemployment spell. For blue-collar workers, the post-displacement earnings fall with the unemployment spell for both layoffs and plant closings, but they fall *less* rapidly for layoffs than for plant closings—this (raw) differential pattern between layoffs and plant closings is statistically significant as can be seen in the first column of Appendix Tables A.1 for white-collar workers and A.2 for blue-collar workers.

5.2. The results

White-Collar Workers

The first and second columns of Table 2 display the α_j coefficients for white-collar workers displaced through plant-closings, and the β_j coefficients for white-collar laid-off workers,

[19] I did not include agricultural workers because they tend to have a large number of jobs with a pronounced seasonal pattern. Workers displaced from construction jobs were eliminated from the sample because formulating an appropriate definition of permanent displacement from a construction job is difficult.

respectively, for j =1 to 4. Table 2 also reports the p-value for the joint Z test of the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0.$[20] Columns 1 and 2 of Table 2 show that, for white-collar workers, post-displacement earnings fall *less* with the unemployment spell for layoffs than for plant closings. All of the four β_j coefficients, for j =1 to 4, are sizeable and positive. Moreover, although only β_2 is statistically significant, the p-value for the Z test of the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0$ is significant at the 5% level, indicating that there is a differential effect by cause of displacement in the relationship between post-displacement earnings and unemployment duration for white-collar workers. Notice that 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 original employers.[21] Second, some of the layoffs in the sample are likely to be determined by strict seniority systems.

The stronger effect is found between 1 and 3 months, which is when most recalls occur in the United States.[22] As the probability of recall converges toward zero, the expected benefits from waiting unemployed fall, decreasing the incentives to signal. Thus, any signaling that may occur among laid-off workers in the U.S. labor market should be observed mainly within the time that prospective employers are most likely to infer that workers are waiting for recall.

Table A.1 in the Appendix highlights the robustness of these results to inclusion of control variables for white-collar workers. While parameters change some when introducing demographic variables—moving from columns (1) to (2) in Table A.1—, and when

[20] To test this one-sided alternative, I have used bootstrapping with 1000 iterations.

[21] 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 3 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?"

[22] See evidence presented earlier in Section 3.

introducing schooling—moving from columns (2) to (3), all of the relevant coefficients change very little thereafter. It is particularly striking that controls for occupation, industry, region, and pre-displacement earnings—columns (8) through (10) of Table A.1—seem to make little difference in the final result, which suggest that the patterns encountered are not simply due to differences in the sector of the economy in which workers were employed.

As explained earlier, the results presented below are robust to modifying the length of the joblessness dummies, and to using an alternative specification including length of joblessness and its square (instead the dummies). Finally, the results are also robust to performing the analysis separately by gender.

Blue-Collar Workers

For blue-collar workers, I do not find evidence that post-displacement earnings of laid-off workers fall *less* with unemployment spell than for workers displaced through plant closings, as one would expect from the model if most recalls in blue-collar jobs are driven by seniority rules, and therefore lack of information content. Notice that only two of the four β_j coefficients, for j =1 to 4, are positive (although not statistically significant), as shown in column 4 of Table 2, and that the p-value for the Z test of the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0$ is not statistically significant, indicating that there is no evidence of a differential effect by cause of displacement in the relationship between post-displacement earnings and unemployment duration for blue-collar workers.

In contrast with the robustness of the results to inclusion of control variables for white-collar workers, the estimates for blue-collar workers are quite sensitive to the introduction of region and year dummies, and pre-displacement wages, as shown in Appendix Table A.2. According to the first four columns of Table A.2, all of four β_j coefficients, for j =1 to 4, are positive, and although they are not as large as those observed in the white-collar sub-sample, the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0$ is rejected at the 5% level, indicating a differential pattern between layoffs and plant-closings (similar to the one observed for white-collar workers). However, moving from columns (4) to (5) in Table A.2 cuts the coefficient of β_1 by half and also reduces the size of the other coefficients of interest (reversing the sign of β_3) leading to an insignificant p-value for the Z test of the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0$. The size of all of four β_j coefficients changes further (becoming negative for three of the β_j) when pre-displacement wage is added as a control. This suggest that workers' heterogeneity explains most of the differential pattern between layoffs and plant closings displaced from blue-collar jobs—notice that no such effect was found for white-collar workers.

5.3. Alternative Explanations

An important question is: Are there alternative explanations other than signaling for the findings among white-collar workers? One possibility is that instead of information being transmitted through the recall probabilities and the unemployment spell of laid-off workers, the observed empirical findings are due to human-capital accumulation and differential changes in occupation between laid-off workers and workers displaced through plant closings. Given the occupational specificity of human capital (Neal, 1999; Parent, 2000; Gibbons *et al.*, 2005; Kambourov, and Manovskii, 2009a), a cause of concern would occur if changes in occupation that occur between employment spells take place at different points of the unemployment spell for layoffs and plant closings.[23] In that case, what now appears to be evidence of employer learning over the length of an unemployment spell could actually be due to the accumulation of occupation-specific human capital over time in

[23] Although earlier research by Neal, 1995, and Parent, 1995, emphasized the relevance of industry-specific human capital in the US, these findings have recently been questionste by Kambourov and Manovskii, 2009a, among others. More specifically, these authors have found that when occupational experience is also taken into account, it is occupational experience rather than industry experience that is of primary importance in explaining wages. Others have also found evidence consistent with a substantial fraction of workers' human capital being occupation specific in the United States (Shaw, 1984, and 1987; and McCall, 1990). Evidence of human capital being occupation specific has also been found in other countries, such as Sweden (Kwon, and Meyersson Milgrom, 2004), the United Kingdom (Zangelidis, 2008), or Canada (Kambourove, Manovskii, and Plesca, 2005).

an occupation and differential switching pattern over the unemployment spell for laid-off workers versus workers displaced through plant closings.

A simple comparison of the percentage of occupation change in my data that take place among workers displaced through plant closings with the percentage in which the same type of change occurs among laid-off workers at different points of the unemployment spell does not suggest that occupation changes are more common for layoffs than plant closings between 1 and 4 weeks of unemployment spell or that they are more common for plant closings than layoffs after 5 weeks of unemployment. Table A.3 in the Appendix shows the probability of staying in the same occupation for white-collar displaced workers by cause of displacement and length of the unemployment spell. While laid-off workers are more likely to switch occupations without an intervening unemployment than workers displaced through plant closings, no statistically significant differences are found thereafter. As a more direct test of the possibility that these results are driven by occupation changes, we compare the basic results with results from regressions that restrict observations to cases in which no occupation change has occurred.[24] The first column of Table 3 presents the results of this analysis. They indicate that post-displacement earnings fall *less* with the unemployment spell for layoffs who find a job in the same occupation than for plant closings who also find a job in the same occupation. All of the four β_j coefficients, for j =1 to 4, are positive, and (although not statistically significant at the individual level) the p-value for the joint Z test of the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0$ is significant at the 5% level, indicating that there is a differential effect by cause of displacement in the relationship between post-displacement earnings and unemployment duration for white-collar workers who stay in the same occupation. While no such effect is found when restricting the sample to those cases in which occupation change has occurred (column 2 of Table 3), this is not necessarily evidence against the signaling model as the occupation-specific human capital is lost with

[24] This approach has been used by others, such as Pinkston, 2008.

the switch of occupation, and therefore the signaling content of the layoff is not as relevant for the new employer.[25]

A related concern arises if the main results of the paper are driven by differences in the composition of the pool of laid-off workers and that of workers displaced through plant closings. We shall explore this from several perspectives, such as, in terms of advance notice receipt, unemployment insurance receipt, and worker's tenure and experience.

Much evidence suggests that advance notice yields a productive pre-displacement search (Addison and Blackburn 1995; Podgursky and Swaim, 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 (Ruhm, 1992). In such a case, one would want to distinguish between those workers who were notified in advance and those who were not. While the prediction should hold for workers who do not receive advance notice, it is unclear whether such result ought to hold for workers who received 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 a differential pattern by cause of displacement. 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 signaling model would not necessarily predict a positive relationship between post-displacement earnings and the length of unemployment among laid-off workers. Column 3 of Table 3 display the estimates for workers who did not receive advance notice. These results are consistent with the signaling model of unemployment. As shown in column 4 of Table 3, this pattern is not observed among workers who received advance notice of displacement. As mentioned earlier, this unobserved pattern may result from complex reasons. Despite its interest, the

[25] See Kambourov and Manovskii's (2009a and b) discussion on the relevance of occupation-specific human capital as well as of its transferability across employers within the same occupation.

topic lies beyond the scope of the present paper.

Because the search behavior of unemployment-insurance (UI) recipients may differ from that of non-recipients, or because UI recipients may differ from their non-recipients counterparts in some unmeasured way, we distinguish between those workers who received UI benefits and those who did not in columns (1) and (2) of Table 4. For similar reasons, the analysis is also done by distinguishing between those who exhausted UI and those who did not in columns (3) and (4) of Table 4. For all subgroups, the p-value for the Z test of the null hypothesis: $\beta_1 > 0, \beta_2 > 0, \beta_3 > 0, \beta_4 > 0$ is significant at the 5% level, providing evidence consistent with the signaling model of unemployment.

Finally, we explore whether there are differences in composition in terms of workers' tenure and whether these affect the results. To do so, the analysis is estimated separately for workers with less than two years of tenure and those with at least one year of tenure, on the one hand. Table 5 display the results. The estimates from columns (1) and (2) reveal that the differential effect by cause of displacement in the relationship between post-displacement earnings and unemployment duration holds *only* among white-collar workers who have at least one year of tenure with the former employer, suggesting that some time may have to elapse before the current employer can accurately evaluate workers' productivity (perhaps because the employer cannot learn the workers' productivity until the workers learns the job), implying that layoffs (and subsequently recalls) after brief employment spells signal little information to prospective employers.

Another concern is that this result may be explained by alternative signaling stories. For example, one could assume that, contrary to the model in this paper, longer unemployment after any kind of displacement signals lower ability. This is consistent with post-displacement wages falling with the duration of the initial unemployment spell. If a layoff is also a signal of productivity (as in Gibbons and Katz, 1991), then the market knows more about workers who were laid off than they know about workers displaced by a plant closing, and consequently, future signals of productivity (unemployment) will then have less of an effect on wages of workers who were laid off. However, such alternative signaling model relies on the assumption that layoffs are lemons. While this result has been questioned by many (see Krashinsky, 2002, and Song, 2007, among others), it is easy to check whether it holds with the current data set. When estimating Gibbons and Katz's1991 specification using the data from the current paper, I find that white-collar workers displaced through layoffs did *not* have lower post-displacement wages than workers displaced through plant closings—the coefficient on the layoff dummy is -.008 (standard error = .028), ruling out that the results of this paper are another test of the "layoffs and Lemons" story.

6. Conclusion

This paper is particularly timely given the recent waves of layoffs in the economy, especially because it uncovers a new empirical fact in the United States, namely that, among white-collar workers, post-displacement earnings do not fall as rapidly with unemployment-spell length for laid-off workers compared to workers displaced by plant closings.[26, 27] These findings are consistent with an asymmetric information model of layoffs that explicitly considers the possibility of recall, and therefore suggest that at least some aspect*s* of the search decisions of white-collar workers on temporary layoff in the United States may have a signaling component. Finally, the paper explores alternative explanations for these results. It finds that the results are robust across several subgroups of white-collar workers—regardless of their UI receipt status, or their work experience—, and that they are driven by those who experience no occupation change, suggesting that signaling is more relevant when the amount of human capital transmitted is higher.

[26]Every day one can read several articles on layoffs in the press. For instance, on December 22, 2008, CNNMoney.com reported that: "As the recession has worsened, companies have ratcheted up layoffs. (….) Reports show that nearly 1 in 4 companies plan layoffs, and 1 million job cuts are forecasted."

[27]As explained in Section 23, recalls continue to be important in the current recession. According to the Mass Layoff Statistics program over half of employers reporting a layoff in 2008 indicated that they anticipated some type of recall (US Bureau of Labor Statistics, 2009). It also reports that among all establishments expecting to recall workers, most employers (88%) expected to recall at least half of the separated employees. Finally, even in the midst of the current recession, the evidence indicates that about one fifth of laid-off workers who landed new positions within the last year were rehired by the same employer that had let them go (CNNmoney.com, 2009).

While the empirical results are consistent with a signaling through unemployment model, I acknowledge that the nature of asymmetric information makes it difficult to conduct direct empirical tests.[28] In addition, I recognize that frequently the data available are not rich enough to precisely distinguish between all potential explanations. Superior data, when they exists, usually restrict the analysis to very specialized settings, such a single firm—as in DeVaro and Waldman, 2004—, or to a narrowly defined group of workers—as in Acemoglu and Pischke, 1998.[29] Nonetheless, this paper provides additional evidence suggestive that explanations of asymmetric information are important. In "Layoffs and Lemons," Gibbons and Katz showed that prospective employers understood adverse selection in the labor market. The results in this paper indicate that workers may also be aware of the existence of adverse selection and of its consequences on their behavior. This finding implies a need for differential unemployment policies by cause of displacement and type of job (blue-collar versus white-collar).

[28] This has also been acknowledged by Gibbons and Katz, 1991; Hu and Taber, 2008; Schönberg, 2007; Khan, 2008; Pinkston, 2009; and Zhang, 2007, among others.
[29] Acemoglu and Pischke, 1998, analyze the German Apprenticeship labor market.

Table 1A. Descriptive Statistics for Displaced Workers Reemployed at Survey date, DWS 1988-2006 (White-Collar Workers at Displacement)

Weeks unemployed	0 weeks		1 to 4 weeks		5 to 12 weeks		13 to 24 weeks		25 + weeks	
	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff
Log of previous weekly	6.538	6.452†	6.411	6.456	6.508	6.517	6.484	6.552	6.420	6.600 †††
earnings	(.595)	(.675)	(.649)	(.645)	(.601)	(.634)	(.620)	(.629)	(.633)	(.664)
Log of current weekly	6.476	6.336††	6.292	6.320	6.282	6.371	6.244	6.304	5.970	6.125
earnings	(.790)	(.943)	(.921)	(.895)	(.890)	(.834)	(.965)	(.954)	(.911)	(1.156)
Change in log real	-.062	-.116	-.119	-.134	-.226	-.146	-.240	-.248	-.455	-.471 †††
weekly earnings	(.588)	(.785)	(.769)	(.797)	(.798)	(.719)	(.893)	(.848)	(.788)	(1.032)
Previous tenure (years)	7.529	6.612†	4.476	3.863 †	6.463	4.332 ††	5.520	5.476	7.565	6.120††
	(7.390)	(7.140)	(5.775)	(4.636)	(7.474)	(5.637)	(6.637)	(6.011)	(7.520)	(7.007)
Unemployment spell	0	0	2.240	2.400 ††	8.832	8.676	18.714	18.923	48.378	45.103
(weeks)	(0)	(0)	(1.180)	(1.188)	(2.392)	(2.415)	(3.533)	(3.640)	(23.921)	(24.600)
Advance notice	.619	.337†††	.518	.302 †††	.485	.304	.484	.354 †††	.632	.273 †††
(percent)	(.486)	(.473)	(.500)	(.459)	(.501)	(.460)	(.501)	(.479)	(.483)	(.446)
Male (percent)	.519	. 446††	.462	.469	.467	.477††	.460	.434	.353	.516 †††
	(.500)	(.498)	(.499)	(.500)	(.500)	(.500)	(.500)	(.497)	(.479)	(.501)
Age	40.703	40.264	37.728	38.449	40.069	39.772	40.031	42.099 ††	41.813	42.529 ††
	(9.685)	(10.162)	(9.711)	(9.861)	(10.028)	(9.767)	(9.963)	(9.607)	(9.814)	(9.290)
Currently married	.692	.617††	.596	.528 ††	.609	.610	.540	.605	.679	.637
(percent)	(.462)	(.487)	(.491)	(.500)	(.489)	(.488)	(.500)	(.490)	(.468)	(.482)
Black (percent)	.043	.063	.058	.099 ††	.080	.053	.056	.073	.114	.093
	(.204)	(.243)	(.234)	(.300)	(.272)	(.224)	(.230)	(.261)	(.319)	(.292)
High-school dropout	.011	.010	.025	.025	.033	.013 ††	.012	.022	.047	.028
(percent)	(.104)	(.098)	(.155)	(.157)	(.179)	(.112)	(.111)	(.147)	(.211)	(.164)
High-school graduate	.241	.218	.261	.228	.245	.186 ††	.217	.179 †††	.295	.204
(percent)	(.428)	(.413)	(.440)	(.420)	(.431)	(.389)	(.414)	(.384)	(.457)	(.404)
Some College (percent)	.319	.349	.342	.331	.328	.313	.354	.336	.389	.329
	(.467)	(.477)	(.475)	(.471)	(.471)	(.464)	(.480)	(.473)	(.489)	(.471)
College graduate or	.430	. 424	.373	.417	.394	.488 ††	.416	.464	.269	.439†††
above (percent)	(.496)	(.495)	(.484)	(.493)	(.490)	(.500)	(.494)	(.500)	(.445)	(.497)
Number of observations	370	413	448	593	274	549	161	274	193	289

Note.- The numbers in parenthesis are standard deviations. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 2000). † Difference in the means between layoff and plant closing are significantly different at the 90% confidence level. †† Difference in the means between layoff and plant closing are significantly different at the 95% confidence level. ††† Difference in the means between layoff and plant closing are significantly different at the 99% confidence level

Table 1B. Descriptive Statistics for Displaced Workers Reemployed at Survey date, DWS 1988-2006 (Blue-Collar Workers at Displacement)

Weeks unemployed	0 weeks		1 to 4 weeks		5 to 12 weeks		13 to 24 weeks		25 + weeks	
	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff
Log of previous weekly	6.302	6.163 †††	5.967	5.996	6.050	6.076	6.071	6.219 ††	6.063	6.120
earnings	(.514)	(.563)	(.550)	(.540)	(.542)	(.524)	(.557)	(.558)	(.521)	(.508)
Log of current weekly	6.222	6.087 †	5.936	5.935	5.896	5.986 †	5.866	5.873	5.741	5.765
earnings	(.712)	(.961)	(.604)	(.664)	(.615)	(.555)	(.653)	(1.079)	(.622)	(.777)
Change in log real	-.080	-.076	-.031	-.061	-153	-.090	-.205	-.345	-.322	-.354
weekly earnings	(.625)	(.918)	(.491)	(.588)	(.546)	(.428)	(.520)	(1.092)	(.576)	(.722)
Previous tenure (years)	8.119	5.051 †††	4.699	3.293 †††	5.214	3.568 †††	6.552	4.300 †††	7.517	5.473 †††
	(8.777)	(6.863)	(6.119)	(4.896)	(6.578)	(4.943)	(7.4653)	(5.419)	(7.782)	(6.617)
Unemployment spell	0	0	2.301	2.358	8.878	8.322 †††	19.130	19.023	48.541	48.540
(weeks)	(0)	(0)	(1.140)	(1.212)	(2.388)	(2.380)	(3.753)	(3.886)	(25.263)	(23.841)
Advance notice	.505	.235 †††	.475	.235 †††	.441	.240 †††	.435	.282 †††	.559	.338 †††
(percent)	(.501)	(.425)	(.500)	(.424)	(.498)	(.428)	(.498)	(.451)	(.498)	(.474)
Male	.783	.772	.626	.687†	.664	.610	.536	.706 †††	.571	.636
	(.413)	(.421)	(.485)	(.464)	(.473)	(.488)	(.500)	(.456)	(.496)	(.482)
Age	39.986	37.837	37.187	35.389 †††	39.013	37.913	39.572	39.563	41.847	40.091
	(10.384)	(10.787)	(10.875)	(9.741)	(9.901)	(11.208)	(10.802)	(10.545)	(10.362)	(10.280)
Currently married	.712	.630 †	.618	.559†	.605	.569	.601	.592	.635	.586
(percent)	(.454)	(.484)	(.486)	(.497)	(.490)	(.496)	(.491)	(.493)	(.483)	(494)
Black (percent)	.061	.073	.096	.114	.097	.099	.109	.092	.106	.111
	(.240)	(.260)	(.295)	(3187)	(296)	(.299)	(312)	(.290)	(.309)	(.315)
High-school dropout	.085	.087	.210	.168	.160	.112	.196	.184	.182	.207
(percent)	(.279)	(.282)	(.408)	(.374)	(.367)	(.316)	(.398)	(.389)	(.387)	(.460)
High-school graduate	.491	.408 †	.462	.456	.450	.447	.406	.368	.488	.384 ††
(percent)	(.501)	(.492)	(.499)	(.498)	(.498)	(.498)	(.493)	(.484)	(.501)	(.488)
Some College (percent)	.307	.377	.260	.282	.286	.294	.297	.276	.265	.298
	(.462)	(.486)	(.439)	(.450)	(.453)	(.456)	(.459)	(.448)	(.442)	(.454)
College graduate or	.118	.128	.068	.094	.105	.147	.101	.172 †	.065	.111
above (percent)	(.323)	(.335)	(.251)	(.292)	(.307)	(.355)	(.303)	(.379)	(.247)	(.315)
Number of observations	212	289	385	553	238	313	138	174	170	198

Note.- The numbers in parenthesis are standard deviations. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 2000). † Difference in the means between layoff and plant closing are significantly different at the 90% confidence level. †† Difference in the means between layoff and plant closing are significantly different at the 95% confidence level. ††† Difference in the means between layoff and plant closing are significantly different at the 99% confidence level.

Table 2. Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006

	White-collar workers at displacement		**Blue-collar workers at displacement**	
	Plant closing	**Layoff**	**Plant closing**	**Layoff**
	(1)		**(2)**	
No unemployment (β_0 for layoffs)		-.050 (.049)		-.013 (.062)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.082* (.048)	+.050 (.066)	-.045 (.050)	-.022 (.069)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.158*** (0.58)	+.131* (.074)	-.151*** (0.54)	+.113 (.073)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.192** (.073)	+.065 (.095)	-.181*** (.061)	-.104 (.106)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.407*** (.064)	+.052 (.096)	-.327*** (.060)	+.005 (.086)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z= 4.82 Prob>Z = 0.000		Z= 2.64 Prob>Z = 0.008	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z= 2.00 Prob>Z = 0.045		Z= 0.00 Prob>Z = 1.000	
Sample size	3,564		2,670	

Note: The numbers in parentheses are robust standard errors. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 2000). Additional covariates include: the log real pre-displacement weekly earnings, a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years), three dummies for completed education (one for "high school graduate"; one for "some college"; and one for "college graduate or above"), an "advance notice" dummy, year-of-displacement dummies; previous-industry and previous-occupation dummies; experience and its square; a gender dummy; marriage dummy; a non-white dummy; and three region dummies. Column (1) matches column (10) in Appendix Table A.1; and column (2) matches column (10 in Appendix Table A.2.

* Estimate significantly different from zero at the 90% confidence level

** Estimate significantly different from zero at the 95% confidence level

*** Estimate significantly different from zero at the 99% confidence level

Table 3. Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(White-Collar Workers at Displacement)

Unemployment spell	**No change in occupation**		**Occupation change**		**No advance notice**		**Received advance notice**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**
	(1)		**(2)**		**(3)**		**(4)**	
No unemployment (β_0 for layoffs)		-.044 (.039)		-.089 (.145)		-.065 (.079)		-.025 (.054)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.044 (.038)	+.029 (.058)	-.105 (.136)	+.090 (.172)	-.048 (.070)	+.045 (.092)	-.110* (.065)	+.080 (.087)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.148*** (.057)	+.099 (.073)	-.142 (.137)	+.225 (.175)	-.113 (.073)	+.098 (.095)	-.215** (.093)	+.167 (.111)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.244*** (.094)	+.144 (.115)	-.104 (.137)	-.065 (.187)	-.264* (.129)	+.216* (.149)	-.114* (.068)	-.176 (.136)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.452*** (.078)	+.099 (.114)	-.302** (.140)	+.004 (.201)	-.487*** (.137)	+.191 (.168)	-.385*** (.067)	-.102 (.106)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z= 2.87 Prob>Z = 0.004		Z= 2.07 Prob>Z = 0.038		Z = 2.22 Prob>|Z| = 0.026		Z = 3.35 Prob>|Z| = 0.001	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z= 1.99 Prob>Z = 0.046		Z= 0.00 Prob>Z = 1.000		Z = 2.00 Prob>|Z| = 0.045		Z = 0.00 Prob>|Z| = 1.000	
Sample size	2,358		1,206		2,109		1,455	

Note.- See Table 2.

Table 4. Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(White-Collar Workers at Displacement)

Unemployment spell	**UI receipt**		**No UI receipt**		**Exhaust UI**		**Did not exhaust UI**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**
	(1)		**(2)**		**(3)**		**(4)**	
No unemployment (β_0 for layoffs)		-.327 (.273)		-.374 (.049)		-.354 (.312)		-.040 (.050)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	+.001 (.218)	+.510* (.300)	-.049 (.050)	+.025 (.068)	-.544 (.290)	+.715* (.408)	-.041 (.047)	+.019 (.066)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	+.170 (.202)	+.498* (.280)	-.169 (.098)	+.073 (.115)	-.248 (.370)	+.607 (.448)	-.132** (.056)	+.100 (.073)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	+.231 (.198)	+.319 (.279)	-.446* (.230)	+.218 (.271)	+.156 (.220)	+.069 (.341)	-.221*** (.083)	+.122 (.108)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	+.026 (.199)	+.347 (.282)	-.659*** (.198)	+.136 (.248)	-.164 (.194)	+.345 (.321)	-.431*** (.110)	+.0522 (.147)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z= 0.00 Prob>Z = 1.000		Z= 2.45 Prob>Z = 0.014		Z = 0.00 Prob>|Z| = 1.000		Z = 2.48 Prob>|Z| = 0.013	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z= 2.69 Prob>Z = 0.007		Z= 2.18 Prob>Z = 0.029		Z = 2.01 Prob>|Z| = 0.044		Z = 2.04 Prob>|Z| = 0.042	
Sample size	1,454		2,100		441		3,087	

Note.- See Table 2. Sample sizes do not add to 3,564 because for several observations information on UI receipt or UI exhaustion was missing.

Table 5. Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(White-Collar Workers at Displacement)

Unemployment spell	**Less than 2 years of tenure**		**Tenure greater than 1 year**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**
	(1)		**(2)**	
No unemployment (β_0 for layoffs)		-.018 (.138)		-.050 (.052)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.161 (.126)	+.121 (.158)	-.075 (.050)	-.002 (.078)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	+.094 (.120)	-.043 (.155)	-.215*** (.069)	+.174** (.088)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.017 (.179)	+.087 (.197)	-.222*** (.081)	+.042 (.111)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.440* (.249)	+.206 (.295)	-.394*** (.060)	+.010 (.105)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z= 0.00 Prob>Z = 1.000		Z= 4.18 Prob>Z = 0.000	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z= 0.00 Prob>Z = 1.000		Z= 2.05 Prob>Z = 0.040	
Sample size	945		2,619	

Note.- See Table 2. In column (2) the null hypothesis tested was H_0: $\beta_2>0$, $\beta_3>0$, $\beta_4>0$ (instead of H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$).

REFERENCES

Acemoglu, D. and J. Pischke, "Why Do Firms Train? Theory And Evidence." *The Quarterly Journal of Economics*, 113, 1998: 78-118.

Addison J., and M. Blackburn, "The Worker Adjustment and Retraining Notification Act," *Journal of Economic Perspectives*, vol. 8(1), 1994:181-90.

Akerlof, G., "The Market for Lemons: Qualitative Uncertainty and the Market Mechanism." *Quarterly Journal of Economics 84,* 1970: 488-500.

Akerlof G., A. Rose, and J. Yellen, "Waiting for Work." *NBER Working Paper, No. 3385,* May 1990.

Alba-Ramírez, A., Arranz-Muñoz, J.M., and Muñoz-Bullón F., "Exits from unemployment: recall or new job", *Labour Economics,*14, 2007: 788-810.

Altonji, J., and C. Pierret. "Employer Learning and the Signaling Value of Education". In *Industrial Relations, Incentives and Employment*, edited by I. Ohashi and T. Tachibanaki. London: Macmillan Prese Ltd., 1997: 159-95.

Altonji J. and C. Pierret, "Employer Learning and Statistical Discrimination." *Quarterly Journal of Economics 116,* 2001:313-350.

Anderson, P., "Time-varying Effects of Recall Expectations, a Reemployment Bonus, and Job Counseling on Unemployment Durations." *Journal of Labor Economics*, 1992: 99-115.

Arcidiacono, P., P. Bayer, and A. Hizmo, "Beyond Signaling and Human Capital: Education and the Revelation of Ability." NBER Working Paper No. 13951.

Bauer, T. and J. Haisken-DeNew, "Employer Learning and the Returns to Schooling." *Labour Economics*, 8, 2001: 161-180.

Bedard K., "Human Capital versus Signaling Models: University Access and High School Dropouts." *Journal of Political Economy,* vol. 109, n. 4, 2001: 749-775.

Belzil, C. and M. Bognanno, "The Promotion Dynamics of American Executives." mimeo, Temple University, 2005.

Bernhardt, D. “Strategic Promotion and Compensation.” *Review of Economic Studies*, 62, 1995, pp. 315—339.

Canziani P. and B. Petrolongo, “Firing Costs and Stigma: A Theoretical Analysis and Evidence from Microdata.” *European Economic Review 45,* 2001: 1877-1906.

Carrington, W.J., “Specific human capital and worker displacement.” Ph.D Dissertation. Chicago: University of Chicago, 1990.

Chang, C. and Y. Wang, “Human Capital Investment Under Asymmetric Information: The Pigovian Conjecture Revisited.” *Journal of Labor Economics*, 14, 1996, 505-519.

Cho, I., and, D. Kreps, “Signaling Games and Stable Equilibria.” *Quarterly Journal of Economics 102,* 1987: 179-221.

Corak, M., “Unemployment insurance, temporary layoffs and recall expectations.” *Canadian Journal of Economics*, 29, 1996: 1-7.

DeVaro, Jed, and Michael Waldman. “The Signaling Role of Promotions: Further Theory and Empirical Evidence.” Working paper, Cornell University, 2006.

DeVaro, J., S. Ghosh, and C. Zoghi. “Job Characteristics and Labor Market Discrimination in Promotions: New Theory and Empirical Evidence.” Cornell Working Paper, August 4th, 2007.

Dickler, J. “Laid-off Workers Get their Jobs Back.” *CNNmoney.com*, July 28, 2009.

Farber H., “Job Loss and the Decline in Job Security in the United States”. Princeton Working Paper, September 2007.

Farber, H.S. and R. Gibbons, "Learning and Wage Dynamics," *Quarterly Journal of Economics*, 111, 1996:1007-1047.

Feldstein, M., “Temporary Layoffs in the Theory of Unemployment.” *Journal of Political Economy, 84*, 1976: 937-957.

Eriksson, S., “Imperfect Information, Wage Formation, and the Employability of the Unemployed", *Working Paper 2002:17, Institute for Labour Market Policy Evaluation.*

Evans, D.S., and L.S. Leighton, “Retrospective bias in the displaced worker surveys.” *Journal of Human Resources* 30 (2), 1995: 386-396.

Fischer, G. and Pichelmann, K., "Temporary layoff unemployment in Austria: empirical evidence from administrative data", *Applied Economics*, 23, 1991: 1447-52.

Galindo-Rueda, F., "Employer Learning and Schooling-Related Statistical Discrimination in Britain." Discussion Paper, IZA, 2003.

Gibbons, R., and L. Katz, "Layoffs and Lemons," *Journal of Labor Economics,* October 1991: 351-380.

Gibbons, Robert, Lawrence F. Katz, Thomas Lemieux, and Daniel Parent. "Comparative advantage, learning, and sectoral wage determination." *Journal of Labor Economics* 23, no. 1:681–724.

Golan, L., "Counteroffers and Efficiency in Labor Markets with Asymmetric Information," *Journal of Labor Economics*, 23, 2005, 373-393.

Gottfries, N. and McCormick, B., "Discrimination and open unemployment in a segmented labour market", *European Economic Review,* 39, 1995: 1-15.

Greenwald, B., "Adverse Selection in the Labor Market," *Review of Economic Studies,* 1986: 325-347.

Hu, L., and C. Taber, "Displacement, Asymmetric Information, and Heterogeneous Human Capital," Federal Reserve Bank of Chicago Working Paper, 2008.

Jansson, F., "Rehires and unemployment duration in the Swedish labour market – new evidence of temporary layoffs", *Labour*, 16, 2002: 311-45.

Jensen, P. and M. Svarer, "Short- and long-term unemployment: How do temporary layoffs affect this distinction?", *Empirical Economics*, 28, 2003: 23-44.

Kahn L., "Asymmetric Information between Employers", November 17, 2007.

Kambourov, G., I. Manovskii, and M. Plesca, "Returns to Government Sponsored Training," mimeo, University of Pennsylvania (2005).

Kambourov G., and I. Manovskii, "Occupational Specificity of Human Capital", *International Economic Review*, 50, 2009a: 63-112.

Kambourov G., and I. Manovskii, "Occupational Mobility and Wage Inequality," *The Review of Economic Studies*, 76, 2009b: 731-759.

Katz, L., “Layoffs, Recall and the Duration of Unemployment,” *NBER Working Paper No. 1825,* 1986.

Katz, L., and B. Meyer, “Unemployment Insurance, Recall Expectations, and Unemployment Outcomes,” *Quarterly Journal of Economics,* November 1990: 973-1002.

Krashinsky, H., 2002. Evidence on adverse selection and establishment size in the labor market. Industrial and Labor Relations Review 56 (1), 84-96.

Kugler, A. and G. Saint-Paul, “How Do Firing Costs Affect Worker Flows in a Wolrd with Adverse Selection?”, *Journal of Labor Economics,* 2004, vol. 22, no. 3.

Kwon, I., and E. Meyersson Milgrom, “Boundaries of Internal Labor Markets: The Relative Importance of Firms and Occupations,” mimeo, Stanford University Graduate School of Business (2004).

Laing D., “Involuntary Layoffs in a Model with Asymmetric Information Concerning Worker Ability.” *The Review of Economic Studies,* Vol. 61, No. 2, 1994:375-392.

Lange, F. (2007), “The Speed of Employer Learning." Journal of Labor Economics, Vol. 25, 2007: 1-35.

Lilien, D, “The Cyclical Importance of Temporary Layoffs,” *Review of Economics and Statistics, LVII,* 1980: 24-31.

Ma, A., and A. Weiss, “A Signaling Theory of Unemployment,” *European Economic Review 37,* 1993: 135-157.

Mavromaras, K.G. and Orme, C.D., “Temporary layoffs and split population models,” *Journal of Applied Econometrics*, 19, 2004: 49-67.

Milgrom, P., and S. Oster, “Job Discrimination, Market Forces, and the Invisibility Hypothesis,” *Quarterly Journal of Economics*, 102, 1987, 453-476.

McCall B.P., “Occupational Matching: A Test of Sorts,” *Journal of Political Economy*, 98(1), 1990: 45–69.

McCormick, B., “A theory of signaling during job search, employment efficiency and “stigmatized” jobs”, *Review of Economic Studies,* vol. 57, 1990: 299-313.

Montgomery, J., "Adverse Selection and Employment Cycles", *Journal of Labor Economics, 17*, 1999: 281-297.

Neal, D., "Industry-Specific Human Capital: Evidence from Displaced Workers," *Journal of Labor Economics*, 13(4), 1995: 653–677.

Neal, D., "The Link between Ability and Specialization: An Explanation for Observed Correlations between Wages and Mobility Rates." Journal of Human Resources, 33, 1998: 173-200.

Neal, D., "The complexity of job mobility among young men". *Journal of Labor Economics* 17, no. 2, 1999::237–61.

Oyer, P., "Recall bias among displaced workers." Economics Letters 82 (3), 2004: 397-402.

Owan, H., "Promotion, Turnover, Earnings, and Firm-Sponsored Training," *Journal of Labor Economics*, 22, 2004, 955-978.

Parent D., "Industry-Specific Capital and the Wage Profile: Evidence from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics," *Journal of Labor Economics*, 18(2), 2000: 306–323.

Pinkston J., "A Model of Asymmetric Employer Learning with Testable Implications", *Review of Economic Studies*, Vol. 76, Issue 1, pp. 367-394, January 2009.

Pissarides C., "Job Search and the Duration of Layoff Unemployment." *The Quarterly Journal of Economics,* vol. 97, no.4, 1982: 595-612.

Podgursky M., and P. Swaim, "Job Displacement and Earnings Loss: Evidence for the Displaced Worker Survey," *Industrial and Labor Relations Review 41,* 1987:17-29.

De Raaf, S., Motte, A., and Vincent, C., "The dynamics of reliance on EI benefits: evidence from the SLID The earnings supplement project, Social Research and Demonstration Corporation (SDRC)" Working Paper Series 03-0, 2003.

Ricart i Costa, J., "Managerial Task Assignment and Promotion," *Econometrica*, 56, 1988: 449-466.

Riordan M., and R. Staiger, "Sectoral Schocks and Structural Unemployment" *International Economic Review 34,* 1993: 611-629.

Robertson, M., “Temporary layoffs and unemployment in Canada”, *Industrial Relations,* 28, 1989: 82-90.

Rodriguez-Planas N., “A Signaling Model of Temporary Layoffs”, *Oxford Economic Papers,* 61(3), 2009: 566-585.

Ruhm, C., “Advance Notice and Post-Displacement Joblessness.” *Journal of Labor Economics,* vol. 10, no.1, January 1992.

Shaw K. “A Formulation of the Earnings Function Using the Concept of Occupational Investment,” *Journal of Human Resources*, 14, 1984: 319–40.

Shaw K. “Occupational Change, Employer Change, and the Transferability of Skills,” *Southern Economic Journal*, 53, 1987: 702–19.

Schönberg U., “Testing for Asymmetric Employer Learning”, Journal of Labor Economics, Vol. 25, 2007: 651-691.

Song Y., “Recall bias in the displaced workers survey: Are layoffs really lemons?”, Economics Letter, Vol. 82, Issue 3, 2004: 397-402.

Spence, M., “Job Market Signalling.” *Quarterly Journal of Economics 87,* 1973: 355-74.

Strand J., “Wage Bargaining and Turnover Costs with Heterogeneous Labor Asymmetric Information.” *Labour Economics 7*, 2000: 95-116.

Topel, R., “Specific capital and unemployment: measuring the costs and consequences of job loss.” Carnegie-Rochester Conference Series in Public Policy. 33 (Autumn), 1990: 181-214.

United States Bureau of Labor Statistics, “Extended mass layoff in 2008”, United States Department of Labor, Report, 2009.

Waldman, M., “Job Assignment, Signaling, and Efficiency,” *Rand Journal of Economics 15,* 1984: 255-267.

Waldman, M., “Up-or-Out Contracts: A Signaling Perspective,” *Journal of Labor Economics*, 8, 1990, 230-250.

Weiss, A., “Human Capital vs Signaling Explanation of Wages.” *Journal of Economic Perspectives, 9:4,* 1995: 133-154.

Zabojnik, J. and D. Bernhardt, "Corporate Tournaments, Human Capital Acquisition, and the Firm Size-Wage Relation," *Review of Economic Studies*, 68, 2001, 693-716.

Zangelidis, A., "Occupational and Industry Specificity of Human Capital in the British Labour Market," *Scottish Journal of Political Economy*, 2007.

Zhang Y., "Employer Learning Under Asymmetric Information: The Role of Job Mobility" (November 1, 2007).

APPENDIX

(*Not for Necessarily for Publication*)

Table A.1. Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(White-Collar Workers at Displacement)

Unemployment spell	**(1)**		**(2)**		**(3)**		**(4)**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**
No unemployment (β_0 for layoffs)		-.140** (.062)		-.099* (.060)		-.110* (.058)		-.103 (.057)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.184*** (.060)	+.170** (.084)	-.148** (.057)	+.131 (.081)	-.125** (.055)	+.122 (.078)	-.103* (.054)	+.104 (.077)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.194*** (.068)	+.230** (.089)	-.158** (.066)	+.180** (.087)	-.140** (.064)	+.139 (.085)	-.134** (.063)	+.133 (.084)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.232*** (.086)	+.200* (.113)	-.190** (.085)	+.165 (.112)	-.196** (.081)	+.157 (.108)	-.194** (.081)	+.133 (.108)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.508*** (.077)	+.298*** (.113)	-.427*** (.074)	+.195* (.109)	-.360*** (.071)	+.134 (.106)	-.369*** (.070)	+.110 (.105)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z = 12.27 Prob>|Z| = .0000		Z = 10.03 Prob>|Z| = .0000		Z = 5.85 Prob>|Z| = .0000		Z = 5.09 Prob>|Z| = .0000	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z = 4.57 Prob>|Z| = .0000		Z = 3.18 Prob>|Z| = .0001		Z = 2.44 Prob>|Z| = .015		Z = 2.24 Prob>|Z| = .025	
Constant	6.476*** (.041)		Yes		Yes		Yes	
Married, race, gender			Yes		Yes		Yes	
Education					Yes		Yes	
Exp., exp^2							Yes	
R-squared	.0151		.0738		.1341		. 1485	

Note.- See Table 2. Column (10) matches column (1) in Table 2.

Table A.1. (*Continued*) Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(White-Collar Workers at Displacement)

Unemployment spell	(5)		(6)		(7)		(8)	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**
No unemployment (β_0 for layoffs)		-.071 (.057)		-.065 (.057)		-.045 (.056)		-.060 (.057)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.100* (.055)	+.082 (.077)	-.082 (.055)	+.079 (.076)	-.078 (.055)	+.075 (.076)	-.064 (.055)	+.062 (.075)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.147** (.063)	+.137 (.083)	-.136** (.063)	+.143* (.082)	-.127** (.064)	+.135 (.082)	-.130** (.063)	+.138* (.082)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.209*** (.081)	+.107 (.106)	-.199** (.080)	+.103 (.106)	-.190** (.080)	+.092 (.105)	-.186** (.077)	+.086 (.103)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.398*** (.071)	+.082 (.105)	-.401*** (.071)	+.097 (.105)	-.401*** (.070)	+.102 (.105)	-.401*** (.070)	+.102 (.104)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z = 4.20 Prob>|Z| = .0000		Z = 3.61 Prob>|Z| = .0000		Z = 3.11 Prob>|Z| = .002		Z = 2.77 Prob>|Z| = .006	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z = 2.04 Prob>|Z| = .041		Z = 2.10 Prob>|Z| = .036		Z = 2.06 Prob>|Z| = .039		Z = 2.00 Prob>|Z| = .045	
Constant	Yes		Yes		Yes		Yes	
Married, race, gender, education, exp., and exp^2	Yes		Yes		Yes		Yes	
Yr dummies, Yrs since displacement, region	Yes		Yes		Yes		Yes	
Pre-displacement tenure spline			Yes		Yes		Yes	
Advance notice					Yes		Yes	
Industry							Yes	
Occupation								
Pre-displacement wage								
R-squared	.1726		.1761		.1774		.1909	

Note.- See Table 2. Column (10) matches column (1) in Table 2.

Table A.1. (*Continued*) Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(White-Collar Workers at Displacement)

Unemployment spell	**(9)**		**(10)**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**
No unemployment (β_0 for layoffs)		-.056 (.057)		-.050 (.049)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.062 (.055)	+.058 (.075)	-.082* (.048)	+.050 (.066)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.133** (.064)	+.140* (.082)	-.158*** (0.58)	+.131* (.074)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.189** (.078)	+.087 (.103)	-.192** (.073)	+.065 (.095)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.405*** (.070)	+.099 (.104)	-.407*** (.064)	+.052 (.096)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z = 2.75 Prob>|Z| = .006		Z= 4.82 Prob>Z = 0.000	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z = 2.01 Prob>|Z| = .044		Z= 2.00 Prob>Z = 0.045	
Constant	Yes		Yes	
Married, race, gender, education, exp., and exp^2	Yes		Yes	
Yr dummies, Yrs since displacement, region	Yes		Yes	
Pre-displacement tenure spline	Yes		Yes	
Advance notice	Yes		Yes	
Industry	Yes		Yes	
Occupation	Yes		Yes	
Pre-displacement wage			Yes	
R-squared	.1922		.3193	

Note.- See Table 2. Column (10) matches column (1) in Table 2.

Table A.2. Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(Blue-Collar Workers at Displacement)

Unemployment spell	**(1)**		**(2)**		**(3)**		**(4)**	
***N*=2,670**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**
No unemployment (β_0 for layoffs)		-.136* (.075)		-.101 (.077)		-.119 (.076)		-.105 (.075)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.286*** (.058)	+.135 (.086)	-.219*** (.055)	+.108 (.084)	-.163*** (.055)	+.094 (.082)	-.144*** (.055)	+.073 (.081)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.326*** (.063)	+.226* (.090)	-.276*** (.060)	+.228* (.088)	-.243*** (.059)	+.209** (.082)	-.245*** (.059)	+.214** (.085)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.357*** (.074)	+.144 (.124)	-.258*** (.071)	+.068 (.122)	-.224*** (.068)	+.054 (.118)	-.219*** (.067)	+.031 (.117)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.481*** (.068)	+.160 (.104)	-.408*** (.067)	+.130 (.103)	-.358*** (.066)	+.128 (.101)	-.360*** (.065)	+.106 (.099)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z = 32.73 Prob>|Z| = .0000		Z = 31.62 Prob>|Z| = .0000		Z = 11.22 Prob>|Z| = .0000		Z = 10.05 Prob>|Z| = .0000	
H_0: $\beta_1 >0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z = 4.57 Prob>|Z| = .0000		Z = 2.09 Prob>|Z| = .036		Z = 2.05 Prob>|Z| = .040		Z = 2.00 Prob>|Z| = .045	
Constant	6.222*** (.049)		Yes		Yes		Yes	
Married, race, gender			Yes		Yes		Yes	
Education					Yes		Yes	
Exp., exp^2							Yes	
R-squared	.0270		.0816		.1377		. 1513	

Note.- See Table 2. Column (10) matches column (2) in Table 2.

Table A.2. (*Continued*) Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(Blue-Collar Workers at Displacement)

Unemployment spell	**(5)**		**(6)**		**(7)**		**(8)**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**	**PC**	**Layoff**
No unemployment (β_0 for layoffs)		-.046 (.066)		-.041 (.066)		-.041 (.066)		-.043 (.065)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.142*** (.055)	+.032 (.074)	-.135*** (.056)	+.033 (.075)	-.135*** (.056)	+.033 (.075)	-.132*** (.056)	+.032 (.075)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.241*** (.059)	+.199** (.080)	-.235*** (.060)	+.200** (.081)	-.235*** (.060)	+.200** (.081)	-.230*** (.059)	+.183** (.080)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.233*** (.068)	-.023 (.111)	-.233*** (.068)	+.003 (.111)	-.233*** (.068)	-.003 (.111)	-.229*** (.067)	-.014 (.111)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.405*** (.065)	+.072 (.092)	-.406*** (.065)	+.073 (.092)	-.406*** (.065)	+.073 (.092)	-.409*** (.064)	+.075 (.092)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z = 9.18 Prob>|Z| = .0000		Z = 8.22 Prob>|Z| = .0000		Z = 8.22 Prob>|Z| = .0000		Z = 8.8 Prob>|Z| = .0000	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z = 0.00 Prob>|Z| = 1.000		Z = 2.08 Prob>|Z| = .037		Z = 2.08 Prob>|Z| = .037		Z = 0.00 Prob>|Z| = 1.000	
Constant	Yes		Yes		Yes		Yes	
Married, race, gender, education, exp., and exp^2	Yes		Yes		Yes		Yes	
Yr dummies, Yrs since displacement, region	Yes		Yes		Yes		Yes	
Pre-displacement tenure spline			Yes		Yes		Yes	
Advance notice					Yes		Yes	
Industry							Yes	
Occupation								
Pre-displacement wage								
R-squared	.2123		.2145		.2145		.2294	

Note.- See Table 2. Column (10) matches column (2) in Table 2.

Table A.2. (*Continued*) Post-Displacement Earnings Equation
Workers Reemployed at Survey Date
DWS 1988-2006
(Blue-Collar Workers at Displacement)

Unemployment spell	**(9)**		**(10)**	
***N*=3,564**	**PC**	**Layoff**	**PC**	**Layoff**
No unemployment (β_0 for layoffs)		-.038 (.065)		-.013 (.062)
One-to-four weeks (α_1 for plant-closings and β_1 for layoffs)	-.128** (.055)	+.023 (.074)	-.045 (.050)	-.022 (.069)
Five-to-twelve weeks (α_2 for plant-closings and β_2 for layoffs)	-.227*** (.059)	+.175** (.079)	-.151*** (0.54)	+.113 (.073)
Thirteen-to-twenty-four weeks (α_3 for plant-closings and β_3 for layoffs)	-.231*** (.067)	-.018 (.111)	-.181*** (.061)	-.104 (.106)
More than twenty-four weeks (α_4 for plant-closings and β_4 for layoffs)	-.409*** (.065)	+.067 (.092)	-.327*** (.060)	+.005 (.086)
H_0: $\alpha_1<0$, $\alpha_2<0$, $\alpha_3<0$, $\alpha_4<0$	Z = 8.82 Prob>|Z| = .000		Z= 2.64 Prob>Z = 0.008	
H_0: $\beta_1>0$, $\beta_2>0$, $\beta_3>0$, $\beta_4>0$	Z = .000 Prob>|Z| = 1.000		Z= .000 Prob>|Z| = 1.000	
Constant	Yes		Yes	
Married, race, gender, education, exp., and exp^2	Yes		Yes	
Yr dummies, Yrs since displacement, region	Yes		Yes	
Pre-displacement tenure spline	Yes		Yes	
Advance notice	Yes		Yes	
Industry	Yes		Yes	
Occupation	Yes		Yes	
Pre-displacement wage			Yes	
R-squared	.2337		.3193	

Note.- See Table 2. Column (10) matches column (2) in Table 2.

Table A.3. Probability of staying in the same occupation for Displaced Workers, Workers Reemployed at Survey date, DWS 1988-2006 (White-Collar Workers at Displacement)

Weeks unemployed	0 weeks		1 to 4 weeks		5 to 12 weeks		13 to 24 weeks		25 + weeks	
	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff	Plant closing	Layoff
Probability of staying in the same occupation	.746 (.436)	.676 †† (.469)	.667 (.472)	.675 (.469)	.602 (.490)	.663 (.473)	.665 (.474)	.653 (.477)	.620 (.488)	.580 (.495)
Number of observations	370	413	448	593	274	549	161	274	193	289

Note.- The numbers in parenthesis are standard deviations. All weekly wages are deflated by the gross domestic product (GDP) deflator (base year = 2000).

† Difference in the means between layoff and plant closing are significantly different at the 90% confidence level

†† Difference in the means between layoff and plant closing are significantly different at the 95% confidence level