Layoffs and Lemons

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We provide theoretical and empirical analyses of an asymmetric-information model of layoffs. When firms have discretion with respect to whom to lay off, the market infers that laid-off workers are of low ability. Assuming that no such negative inference is warranted if workers are displaced in a plant closing, postdisplacement wages should be lower and postdisplacement unemployment spells should be longer for those displaced by layoffs than for those displaced by plant closings, but predisplacement wages should not differ by cause of displacement. Evidence on displaced workers from Current Population Surveys supports all three of our model's predictions.

I. Introduction

Since the seminal work of Akerlof (1976) and Spence (1973), labor economists have understood that asymmetric information about workers'

productive abilities can affect labor-market outcomes. A number of recent theoretical papers have elaborated on this theme and also have shifted attention from a worker's private information (vis-à-vis prospective employers) about his or her productive ability to an employer's private information (vis-à-vis the market) about an employee's ability. Waldman (1984), Milgrom and Oster (1987), and Ricart i Costa (1988), for instance, describe inefficient job assignments that result when an employer has private information concerning employees' abilities, and Greenwald (1986), Lazear (1986), and Riordan and Staiger (1987) describe analogous consequences for wages and mobility in the presence of such asymmetric information.

It seems plausible that a worker's current employer may be better informed about the worker's ability than prospective employers are, but the predictions generated by the existing theoretical models based on this assumption mainly concern variables that are not included in standard micro data sets (such as promotions within the firm or wage offers from prospective employers). In this article we provide theoretical and empirical analyses of an asymmetric-information model of layoffs. The model is based on the same information asymmetry as the theoretical models described above but differs in that it delivers predictions that can be tested using standard micro data. Our theoretical model offers new answers to such time-honored questions as why layoffs occur and how firms determine which workers to lay off.[1] Our empirical work offers the first quantitative evidence consistent with the burgeoning collection of asymmetric-information models described above.[2]

The main idea behind the article is simple. If a firm has discretion over whom to lay off, then the firm's desire to retain a worker signals to the market that the worker is of high ability, so the market bids up the wage of retained workers. As a result, the firm finds it unprofitable to retain low-ability workers and hence lays them off.[3] The market then infers that

[1] Our answers to these questions are of course complementary to the conventional wisdom that layoffs are caused by shocks and determined by seniority.

[2] Antel (1985) and McLaughlin (1991) find empirical support for a complementary class of asymmetric-information models concerning match quality. Our model emphasizes private information about a worker's (general-purpose) ability, so a lemons effect arises because prospective employers are wary of hiring a worker another firm does not want. In the matching model, in contrast, given the value of the match between a worker and one firm, information about the value of the match between the worker and a second firm is irrelevant, so no lemons effect arises.

[3] One might ask why high- and low-ability workers cannot be retained at high and low wages, respectively. As will become clear below, the answer is that, if the firm could retain low-ability workers at a low wage, then it would also retain high-ability workers at this low wage, thereby destroying the market's willingness to allow *any* workers to be retained at the low wage.

laid-off workers are of low ability and so offers them low wages in their next job. We assume that workers displaced by plant closings, in contrast, suffer from no such adverse inference and so receive (relatively) higher reemployment wages from the market. Our model thus predicts that the postdisplacement wages of (otherwise observationally equivalent) workers will differ according to the cause of displacement. Furthermore, in our model it is the layoff event that signals unfavorable information to the market, so the model predicts that the predisplacement wages of (otherwise observationally equivalent) workers will not differ according to the cause of displacement. Combining these predictions, we have that the wage loss at displacement should be larger for those laid off than for those displaced by plant closings.

In our empirical work, we use data from the Displaced Workers Supplements in the January 1984 and 1986 Current Population Surveys. Because our model assumes that the firm has discretion over whom to lay off, we focus on white-collar workers (rather than on blue-collar workers, whose jobs often are covered by collective-bargaining agreements involving explicit layoff-by-seniority rules). In our sample (described in Sec. III below), the estimated mean percentage wage loss from displacement is 5.5 percentage points greater for white-collar workers displaced by layoffs than for (otherwise observationally equivalent) white-collar workers displaced by plant closings. Furthermore, we find that predisplacement wages do not differ significantly by cause of displacement.

A simple extension of our lemons model yields the prediction that the average postdisplacement unemployment duration should be longer for workers displaced by layoffs than for those displaced by plant closings. We find that the evidence also is consistent with this prediction: workers laid off (and not recalled) have approximately 25% longer postdisplacement unemployment spells than do those displaced in plant closings. As we describe below, only part of this estimate should be attributed to a lemons effect, but we also report several further pieces of evidence that together suggest an important role for our lemons model in accounting for the observed variation in postdisplacement unemployment duration by cause of displacement and by occupation.

In sum, we find empirical support for all three of our model's predictions concerning the wages and unemployment experiences of displaced workers. In interpreting these empirical results, it is worth noting that they do not control for a potentially important effect in the opposite direction: if a plant is large compared to its local labor market, then the increase in the local unemployment rate following a plant closing seems likely both to depress the reemployment wages and to extend the typical unemployment duration of displaced workers.

The body of the article is organized as follows. Section II presents the theoretical model, which may be of independent interest. Stated abstractly,

our model explores how the information signaled by an informed party in the first stage of a game endogenously determines the severity of the uninformed parties' adverse-selection (or winner's-curse) problem in the second stage of the game. Sections III and IV present the empirical results on wages and unemployment duration described above. Finally, Section V summarizes and interprets our findings.

II. Theoretical Analysis

In the signaling equilibria described below, a firm lays off its least productive workers. Prospective employers then infer that these workers are of low productivity and so offer them a low wage. We assume that no such negative inference is warranted after a plant closing, so the average reemployment wage of workers who lose their jobs because of a plant closing is higher than that of workers who lose their jobs because of a layoff.

A. The Model

Our model has two periods. The major elements of the model are (1) the production technology, (2) the information structure, (3) the commitment and contracting possibilities, and (4) the timing of events between periods 1 and 2. We describe each of these elements in turn and then compare our model to related models developed by Greenwald (1986) and by Waldman (1984).

1) *The production technology.*—The first-period output of a worker of (time-invariant) productive ability η is $y_1(\eta) = \eta$. The second-period output of a worker of ability η is $y_2(\eta) = \eta + s$ (where $s > 0$) if the worker remains with the first-period employer but is $y_2(\eta) = \eta$ if the worker changes employers. The parameter s can be interpreted as firm-specific human capital and/or as one (or even the sum) of the following transaction costs: a mobility cost incurred by the worker, a hiring cost incurred by a new employer, or a firing cost incurred by the first-period employer. Given the range of these possible interpretations of s, it is difficult to specify how one might measure s. Nonetheless, it seems plausible to us that in many employment relationships at least one of these potential interpretations of s is an important consideration.

2) *The information structure.*—At the beginning of the first period, information is symmetric but imperfect: based on the observable characteristics of a given worker, all firms and the worker share the belief that the worker's productive ability is distributed according to the probability distribution $F(\eta)$ on (η_L, η_H) with density $f(\eta)$. At the end of the first period, the worker's current employer (hereafter also called the firm) observes the worker's first-period output and so perfectly infers the worker's ability, but prospective employers (hereafter also called the market) do not observe output and so do not (yet) update their beliefs about the worker's ability.

Finally, to keep things simple, we make assumptions below (on the kinds of wage and employment contracts that are feasible) to guarantee that it is immaterial whether or not the worker observes first-period output.

In the interest of clarity, we impose a (rather weak) regularity condition on the distribution of productive ability: $f(\eta)$ must be log concave (i.e., $\ln f(\eta)$ must be concave in η), which implies that $d\{E(\eta|\eta \geq x)\}/dx \leq 1$ for every x.[4] Many familiar distributions—including the uniform, Normal, exponential, and beta distributions—satisfy this condition. Furthermore, the truncation of a log-concave distribution is log concave.

We also impose an assumption relating the importance of firm-specific capital and the distribution of ability: $\eta_L + s < E(\eta)$. As will become clear below, the model is trivial without this assumption because a first-period employer can afford to retain even the lowest-ability worker for the second period, even if prospective employers offer a wage equal to the expected ability of the entire population of workers.

3) *Contracting possibilities.*—We assume that neither contingent nor long-term contracts are possible: the first-period wage is determined at the beginning of the first period, the second-period wage is determined at the beginning of the second period, and the layoff decision is made at the beginning of the second period. The firm cannot *commit* at the beginning of the first period to pay a particular second-period wage or to make a particular second-period layoff decision, whether or not the wage or layoff decision is contingent on first-period output. Once the second period arrives, however, the firm *chooses* to pay wages and make layoff decisions that depend on output (i.e., ability), as described below. Since there are no contingent or long-term contracts, firms have no way to offer insurance, so it is immaterial whether workers are risk neutral or risk averse.

The assumption that contracts contingent on output cannot be enforced fits naturally with our assumption that output is not observable by prospective employers (and so plausibly also is unobservable to a court). The assumption that long-term contracts cannot be enforced seems natural because of the possibility that firm-specific productivity shocks may occur: if a bad enough shock occurs at the beginning of period 2, the current employer will go bankrupt rather than live up to the contract. (Our empirical focus on permanently displaced workers is consistent with productivity shocks playing an important role.) More generally, both of these assumptions are in keeping with the view that output and compensation are complex, multidimensional quantities that are difficult to specify in a

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[4] See Heckman and Honore (1990) and Caplin and Nalebuff (1991) for proofs of this result. We assume log concavity for two reasons. First, it greatly simplifies several derivations (but weaker assumptions would also be sufficient; see Caplin and Nalebuff). Second, although the predictions we examine in our empirical work do not depend on log concavity, we find that one's intuitive grasp of the model is enhanced by comparative static results that do rely on log concavity.

contract: output can be produced under favorable or unfavorable circum-
stances and can be of high or low quality; compensation includes not only
wages but working conditions and task assignments.

4) *Timing.*—The crux of the model is the sequence of events between
the two periods. After observing a given worker's first-period output, the
current employer decides whether to lay off the worker. If the worker is
laid off, then the current employer has no further contact with the worker.
In particular, there is no possibility of recall. Following a layoff, prospective
employers observe that the worker was laid off and then simultaneously
offer the worker a second-period wage. The worker accepts the highest
wage offered (randomizing in case of a tie). If the current employer does
not lay off the worker, however, then the following wage-setting game
ensues. First, prospective employers observe that the worker was not laid
off by the first-period employer. Second, prospective employers simulta-
neously offer the worker a second-period wage. Third, the current employer
observes the offers from prospective employers and then makes its own
second-period wage offer to the worker. Finally, the worker chooses the
highest of the wages offered (choosing the current employer's offer in case
of a tie), except that with probability μ the worker is constrained to move—
in order to accompany a spouse, for example—and so accepts the highest
offer from the market (randomizing among tied offers).

Our model of layoffs and wage-setting integrates two leading models
from the literature: the adverse-selection model developed by Greenwald
and the signaling model developed by Waldman. There are two major
differences between these models. First, Greenwald assumes that the mar-
ket's second-period wage offer to a worker the firm does not lay off equals
the worker's expected ability conditional on the worker choosing to accept
the market's offer rather than the firm's. Waldman, in contrast, assumes
that the market's offer is the (unconditional) expectation of the worker's
ability. In the terminology of auction theory, Greenwald allows for the
winner's curse, while Waldman assumes it away.[5] Second, Greenwald as-
sumes that the firm has no opportunity to signal its private information,
whereas Waldman does allow the firm to signal (through job assignments
rather than through layoffs as here).

Our model has a continuum of equilibria, ranging from an equilibrium
analogous to the unique equilibrium in Greenwald's model to an equilib-
rium analogous to the unique equilibrium in Waldman's model. All of
these equilibria formalize the intuition given at the beginning of this section:

[5] The winner's curse arises in an auction when the good being sold has a common
value to all the bidders (such as an oil field) and each bidder has a privately known
unbiased estimate of the value of the good (such as from a geologist's report): the
winning bidder will be the one who most overestimated the value of the good; this
bidder's estimate itself may be unbiased but the estimate conditional on the knowl-
edge that it is the highest of n unbiased estimates is not.

if the firm lays off any one at all, it lays off its least productive workers; the market infers that laid-off workers are of low productivity and so offers them a low wage; assuming that no such negative inference is warranted after a plant closing, laid-off workers thus earn lower reemployment wages than do workers displaced by plant closings. In addition to formalizing the intuition we subsequently study in our empirical work, our equilibria also shed new light on the results of and the relation between the Greenwald and Waldman models. See the discussion at the end of this section.

B. Computing the Equilibria

We use the following notation to describe the players' (pure) strategies. Let $L(\eta)$ represent the firm's layoff decision for a worker of ability η: $L(\eta) = 1$ if the current employer lays off a worker with ability η; $L(\eta) = 0$ if the firm does not lay off such a worker. Let $w_{mi}(L)$ denote the wage offer made by the ith prospective employer if the current employer lays off a worker; let $w_{mi}(R)$ denote the analogous offer in the event that the firm attempts to retain (i.e., does not lay off) the worker. It suffices to consider a market of two prospective employers, denoted by $i = 1, 2$. Finally, let $w_f[\eta, w_{m1}(R), w_{m2}(R)]$ be the wage the firm offers to a worker whom it did not lay off, given that the worker's ability is η and the offers the worker has received from prospective employers are $w_{m1}(R)$ and $w_{m2}(R)$.

To construct an equilibrium in our model, we work backwards. We first compute the firm's optimal wage offer to a worker it did not lay off, given the worker's ability and the offers made by prospective employers. We then compute the market's optimal wage offer to a worker who was not laid off, given that the firm's subsequent offer will be the best response just derived and given the market's conjecture about the layoff rule used by the firm. Finally, we compute the firm's optimal layoff rule, given that the subsequent wage offers will be the best responses just derived. In equilibrium, the firm's chosen layoff rule must be identical to the market's conjecture.[6]

Consider a worker who was not laid off. Let w_m denote the worker's best offer from the market (hereafter also called the market wage): w_m

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[6] We have just (loosely) defined a perfect Bayesian (or, roughly, sequential) equilibrium of our signaling game. See Kreps and Wilson (1982) and Fudenberg and Tirole (1991) for more precise definitions of these equilibrium concepts. In many signaling games one must strengthen the equilibrium concept so as to rule out equilibria that rely on unreasonable beliefs off the equilibrium path; see, e.g., Cho and Kreps (1987). The signal space in our model, however, includes only two possible signals: whether or not the worker is laid off. Furthermore, in all but one of the equilibria we describe below, both signals are used (with positive probability), in which case it is neither necessary nor feasible to strengthen the equilibrium concept by restricting beliefs off the equilibrium path.

$= \max\{w_{m1}(R),\ w_{m2}(R)\}$. If the worker's ability satisfies $\eta + s \geq w_m$, the firm's best response is to try to retain the worker by offering the market wage, in which case the worker remains with the firm for the second period with probability $1 - \mu$ but is constrained to move with probability μ. If the worker's ability satisfies $\eta + s < w_m$, however, the firm's best response is to offer less than the market wage, in which case with probability one the worker leaves the firm to accept a higher second-period wage offer from a new employer.

In equilibrium, prospective employers anticipate that the firm will play the best response just derived. In order to compute their optimal wage offers, prospective employers must also have a conjecture about the layoff rule used by the firm. Suppose that prospective employers believe that the firm lays off a worker if and only if the worker's ability is less than some cutoff, η_R. Then Bertrand wage competition among prospective employers bids up the second-period wage the market offers to workers who are not laid off until the market earns zero expected profit on the workers it attracts with this offer. That is, w_m satisfies

$$0 = \mu[E(\eta | \eta \geq \eta_R) - w_m] + (1 - \mu)\text{prob}\{\eta + s < w_m | \eta \geq \eta_R\} \qquad (1)$$
$$\times\ [E(\eta | \eta_R \leq \eta < w_m - s) - w_m]$$

because prospective employers understand that with probability μ the worker will be constrained to move and so will accept w_m independent of the offer received from the firm but that with probability $1 - \mu$ the worker will not be constrained to move and so will accept w_m only if it exceeds the wage offer received from the firm (i.e., only if $\eta < w_m - s$, given the firm's best response derived above). We show in the Appendix that for each of the relevant values of η_R, (1) has a unique solution.

In equilibrium, the firm anticipates that the subsequent market wage offer will be w_m, the solution to (1), and so understands that it will find it unprofitable to employ a worker of ability satisfying $\eta + s < w_m$. The firm is indifferent between the two ways it can rid itself of such a worker: it can lay off the worker, or it can induce the worker to quit by offering less than the market wage. Thus, provided that $\eta_R + s \leq w_m$, one of the firm's optimal layoff rules is to lay off a worker if $\eta < \eta_R$ and to induce the worker to quit if $\eta_R \leq \eta < w_m - s$. If the firm chooses this rule, then the market's conjecture we assumed above is correct. Thus, we have constructed (most of) an equilibrium, provided η_R satisfies

$$\eta_R + s \leq w_m, \qquad (2)$$

where w_m is in turn a function of η_R as described by (1).

We show in the Appendix that (2) holds provided $\eta_R \leq \eta^*$, where η^* is the unique solution to

$$\eta^* + s = E(\eta \mid \eta \geq \eta^*). \qquad (3)$$

To complete the description of equilibrium, it remains only to specify the equilibrium behavior by prospective employers if the firm lays off a worker: Bertrand wage competition yields a second-period wage equal to the worker's expected ability. If prospective employers believe that the firm lays off a worker if and only if the worker's ability is less than η_R, then the reemployment wage of a laid-off worker will be

$$w_2(L) = E(\eta \mid \eta < \eta_R). \qquad (4)$$

Since $\eta_R < \eta^* < \eta_H$, we have that $w_2(L) < E(\eta).$[7]

For workers who lose their jobs because of a plant closing, in contrast, we assume that no adverse inference about ability is warranted, so that competition among prospective employers yields the reemployment wage

$$w_2(PC) = E(\eta). \qquad (5)$$

Comparing (4) and (5) shows that $w_2(PC) > w_2(L)$. This inequality is the main prediction of our model: the reemployment wages of laid-off workers are less than those of (otherwise observationally equivalent) workers displaced by plant closings. In our empirical work we consider wage changes as well as reemployment wages. In our model, competition among employers and the symmetric information before period 1 yield a single first-period wage w_1 for all workers, independent of ability. Therefore, the wage changes experienced by displaced workers satisfy $w_2(PC) - w_1 > w_2(L) - w_1$.

To conclude this section, we describe some of the properties of our continuum of equilibria parameterized by η_R, and we relate these equilibria to the Greenwald and Waldman equilibria. First, η^* in (3) decreases with s. (To see this, implicitly differentiate [3] and use the fact that $d\{E(\eta \mid \eta \geq \eta^*)\}/d\eta^* \leq 1$.) In the limits, η^* approaches η_L as s approaches $E(\eta) - \eta_L$ and approaches η_H as s approaches zero. These results follow from the fact that—as in any lemons problem, from Akerlof (1970) on—the current employer considers the productivity (in that firm) of the marginal retained worker, while prospective employers consider the productivity (in their firms) of the average retained worker. Some level of firm-specific human capital $(s > 0)$ is necessary for these two productivities to be equal.

Second, η_L and η^* are independent of μ. Thus, the range of layoff rates associated with our continuum of equilibria (i.e., $F(\eta_R)$, for η_R ranging

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[7] We show in the Appendix that any equilibrium, including those not belonging to the continuum of equilibria we have just described, yields the qualitative conclusion that $w_2(L) < E(\eta)$.

from η_L to η^*) is independent of μ. The range of quit rates associated with these equilibria does vary with μ, however, and also varies inversely with the layoff rate: the maximal quit rate occurs when the layoff rate is zero (i.e., when $\eta_R = \eta_L$) and the minimal quit rate (namely, μ) occurs when the layoff rate is maximal (i.e., when $\eta_R = \eta^*$).

Third, as described above, our equilibria range from an equilibrium analogous to Waldman's (namely, $\eta_R = \eta^*$, in which $w_m = E(\eta | \eta \geq \eta_R)$—there is no winner's curse) to an equilibrium analogous to Greenwald's (namely, $\eta_R = \eta_L$, in which turnover vanishes as μ approaches zero). The higher the ability of the most able laid-off worker (namely, η_R), the weaker the winner's curse the market suffers when it makes a second-period wage offer to a worker the firm does not lay off (in the sense that this wage offer becomes closer to the unconditional expectation of the worker's ability). One interpretation of these equilibria is that we relax Waldman's assumption that the winner's curse is absent but show that the assumption holds in one of the limiting equilibria (and is approximately correct in nearby equilibria). A second interpretation is that we allow the ability distribution in Greenwald's model to be determined endogenously by the firm's layoff decision and show that even if μ is arbitrarily small (as casual reflection suggests it might be) there exist equilibria in which turnover does not vanish: the layoff rate can be as high as $F(\eta^*)$, independent of μ. Under either interpretation, however, our main result is that in all the equilibria the reemployment wages of laid-off workers are low. We turn next to an empirical investigation of this prediction.

III. Empirical Analysis of the Wages of Displaced Workers

In this section, we provide evidence on the wages of male displaced workers,[8] using data from the January 1984 and January 1986 Displaced Workers Supplements (DWS) to the Current Population Survey.[9] We examine how the change in wages, the predisplacement wage, and the post-

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[8] We focus on males displaced from full-time jobs in an attempt to identify a sample of workers with strong attachments to the labor market. This allows us to focus on the impact of the lemons effect on wages alone rather than jointly modeling the impact on labor-force participation.

[9] Workers in the January 1984 DWS permanently lost a job between January 1979 and January 1984. Workers in the January 1986 DWS permanently lost a job between January 1981 and January 1986. Individuals enter the DWS if they lost a job in the 5 years prior to the survey because of a plant closing, an employer going out of business, or a layoff from which he or she was not recalled. Interviewers for the DWS were instructed that, if an individual was fired from a job for cause, then the individual should not be included in the DWS. If a worker lost more than one job in the 5 years prior to the survey, the survey questions refer to the lost job he or she had held the longest. See U.S. Department of Commerce (1986) for further details on the design and implementation of the DWS surveys.

displacement wage vary with the cause of displacement and with predisplacement occupation.

Both our model and our data concern layoffs and plant closings. Existing work has focused on the distinction between quits (voluntary separations) and layoffs (involuntary separations). Bartel and Borjas (1981) and Mincer (1986), for example, find that quitters typically experience wage increases while laid-off workers typically experience wage losses. This fact suggests that a laid-off worker might try to escape the lemons inference described in our model by claiming to have quit rather than admit to having been laid off. Similarly, a laid-off worker could claim to have been displaced by a plant closing. We assume that plant closings are not only widely observed but also verifiable. Thus, a personnel officer could check an applicant's claim concerning a plant closing. Likewise, we assume that a personnel officer could check an applicant's claim concerning current or previous employment.

A. Data Description

We examine a pooled sample of male workers between the ages of 20 and 61 who were permanently displaced from a private-sector, full-time, nonagricultural job because of a plant closing, slack work, or a position or shift that was eliminated; we classify as layoffs those displaced because of slack work or a position or shift that was eliminated. Workers displaced from construction jobs were eliminated from the sample since it is difficult to formulate an appropriate definition of permanent displacement from a construction job. For most of this section, the sample is restricted to those individuals who were reemployed in wage and salary employment[10] at the survey date and who had reemployment earnings of at least $40 a week; at the end of the section, we address the potential sample-selection bias arising from the fact that we exclude from the sample workers not reemployed at the survey date.

Basic descriptive statistics for our sample of displaced workers are presented in table 1. The sample is approximately evenly split between those displaced through plant closings and those displaced by layoffs. The vast majority (79%) of those whom we classify as displaced by layoffs were displaced because of slack work. The major measured difference between workers displaced by plant closings and those displaced by layoffs is that, on average, the former had been on their predisplacement jobs substantially longer (2.2 more years). This suggests that seniority rules may be important in layoff decisions. Other measured differences between workers displaced by plant closings and those displaced by layoffs are, on average, the former have shorter spells of joblessness following displacement, are more likely

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[10] Unfortunately, the CPS does not provide current earnings information for those workers who entered self-employment.

Table 1
**Descriptive Statistics for Displaced Workers Data Set from January 1984
and 1986 CPS Displaced Workers Surveys, Males Reemployed
at Survey Date in Wage and Salary Employment**

| | Means | | |
| | | Reason for Displacement | |
Variable	Entire Sample	Plant Closing	Layoff*
Layoff = 1	.53	.00	1.00
Previous tenure in years	4.73	5.87	3.72
	(6.00)	(7.03)	(4.68)
Change in log real weekly earnings	−.164	−.160	−.168
	(.50)	(.49)	(.51)
Log of previous weekly earnings	5.94	5.94	5.93
	(.49)	(.49)	(.48)
Log of current weekly earnings	5.77	5.78	5.76
	(.54)	(.54)	(.48)
Weeks of joblessness after displacement	21.35	19.61	22.89
	(25.71)	(25.21)	(26.05)
No unemployment after displacement			
= 1	.14	.19	.10
Advance notification of displacement = 1	.51	.56	.47
Years of schooling	12.62	12.41	12.81
	(2.40)	(2.46)	(2.33)
(Age − education − 6) at displacement	12.38	13.67	11.23
	(10.33)	(10.85)	(9.71)
White collar in previous job = 1	.34	.34	.35
Previous job in manufacturing = 1	.53	.51	.54
N	3,427	1,614	1,813

NOTE.—Standard deviations are in parentheses. All weekly earnings figures are deflated by the gross national product (GNP) deflator.
  * Reason for displacement was slack work or elimination of shift or position.

to have found new jobs without an intervening spell of unemployment, and are more likely to have received advance notification of job loss.

The earnings loss for the typical displaced worker in the sample is substantial. The mean change in the log of real weekly earnings is −0.16 for the whole sample and does not differ much between those displaced by plant closings and those displaced by layoffs. Much evidence indicates, however, that the earnings losses of displaced workers rise substantially with predisplacement tenure (e.g., Podgursky and Swaim 1987; Kletzer 1989; and Topel 1991). Thus, the fact that the earnings losses of workers displaced by layoffs and by plant closings are similar despite the higher average predisplacement tenure of those displaced by plant closings suggests that a lemons effect may be operating.

The information content of a layoff depends on whether the employer has any discretion with respect to whom to lay off. In the presence of a layoff-by-seniority rule, for example, there may be little or no information concerning a worker's ability revealed by the fact that the worker was laid

off. Most jobs covered by collective-bargaining agreements are governed by layoff-by-seniority rules, but many nonunion jobs are not governed by such rules.[11] This suggests examining whether the gap between the wage losses from layoffs versus those from plant closings is larger in subsamples where employers are likely to have more discretion with respect to whom to lay off.[12]

Because many fewer white- than blue-collar jobs are covered by collective-bargaining agreements,[13] we presume that the degree of discretion over whom to lay off is likely to be higher in white- than in blue-collar jobs. Differences in the characteristics and displacement experiences of displaced workers by cause of displacement are presented separately for white- and blue-collar workers in table 2. The difference in average pre-displacement tenure between workers displaced by plant closings and those displaced by layoffs is significantly smaller for white-collar workers than it is for blue-collar workers (1.3 vs. 2.6 years). This suggests that strict layoff-by-seniority rules are less important and employer discretion is more important for layoff decisions concerning white-collar workers than for those involving blue-collar workers.[14] The significantly lower fraction of white- than of blue-collar layoff victims who received advance notification

[11] Abraham and Medoff (1984), for instance, find that (1) 92% of union firms have written rules to deal with permanent layoffs while only 24% of nonunion firms have such written layoff policies, and that (2) 58% of nonunion firms have a practice of sometimes laying off a more senior worker if a junior worker is believed to be worth more on net, as compared to 17% of union employers.

[12] Unfortunately, the Displaced Workers Supplements do not provide information on whether a worker's predisplacement job was covered by a collective-bargaining agreement.

[13] We used all 12 outgoing rotation groups from the 1983 Current Population Survey to tabulate unionization rates by occupation for a sample of workers comparable to our DWS sample. We included in our sample 20–61-year-old, male, full-time, private-sector employees not working in agriculture or construction. Workers were classified as unionized if they were union members and/or working in employment covered by a collective-bargaining agreement. We find that 10.4% of white-collar workers were unionized, compared to 38.5% of blue-collar workers; see table 3 for the mapping from one-digit occupations to these white-collar and blue-collar aggregates.

[14] We also computed the difference in average predisplacement tenure between workers displaced by plant closings and those displaced by layoffs for one-digit predisplacement occupations rather than for our white- and blue-collar occupational aggregates. Among white-collar workers, the average difference in tenure (in years) is: managers and administrators = 1.4, professional and technical workers = 1.6, clerical workers = 1.2, and sales workers = 1.2. Among blue-collar workers, the average (in years) is: craft and kindred workers = 2.7, operatives (except in transportation) = 2.8, transport operatives = 2.2, laborers = 3.1, and service workers = 1.4. Thus, with the exception of service workers, the white- vs. and blue-collar division of the sample closely matches the division of the sample in terms of this average difference in predisplacement tenure.

Table 2
Descriptive Statistics by Broad Occupation from January 1984 and 1986
CPS Displaced Workers Surveys, Males Reemployed at Survey Data
in Wage and Salary Employment

	Means			
	White Collar		Blue Collar	
Variable	Plant Closing	Layoff*	Plant Closing	Layoff*
Previous tenure in years	5.17	3.84	6.23	3.66
	(.28)	(.20)	(.22)	(.13)
Change in log real weekly earnings	−.068	−.125	−.208	−.191
	(.02)	(.02)	(.02)	(.02)
Log of previous weekly earnings	6.06	6.05	5.88	5.87
	(.02)	(.02)	(.01)	(.01)
Log of current weekly earnings	5.99	5.93	5.67	5.68
	(.02)	(.02)	(.02)	(.02)
Weeks of joblessness after displacement	13.96	18.36	22.54	25.29
	(.84)	(.85)	(.84)	(.81)
No unemployment after displacement = 1	.25	.11	.16	.09
	(.02)	(.01)	(.01)	(.01)
Advance notification of displacement = 1	.55	.41	.56	.51
	(.02)	(.02)	(.02)	(.01)
Years of schooling	13.87	14.21	11.65	12.07
	(.10)	(.09)	(.06)	(.06)
(Age − education − 6) at displacement	13.04	11.82	13.99	10.92
	(.44)	(.40)	(.34)	(.28)
Previous job in manufacturing = 1	.35	.39	.60	.62
N	552	627	1,062	1,186

NOTE.—Standard errors of the means are in parentheses. The white-collar sample consists of workers with predisplacement jobs as managers and administrators, professional and technical workers, clerical workers, or sales workers. The blue-collar sample consists of workers with predisplacement jobs as craft and kindred workers, operatives, laborers, transport operatives, or service workers. Weekly earnings figures are deflated by the GNP deflator.
* Reason for displacement was slack work or elimination of shift or position.

of displacement (0.41 vs. 0.51) also suggests that white-collar layoffs are less likely to be governed by collective-bargaining agreements, which often include formal prenotification requirements and formally limit employer choice with respect to whom to lay off.

Table 2 also reveals that the pattern of (raw) earnings losses for white-collar workers fits the predictions of our model: predisplacement earnings do not differ much by cause of displacement, while postdisplacement earnings are significantly lower (by 6%) for those displaced by layoffs. Furthermore, for blue-collar workers the analogous difference in postdisplacement earnings is not significantly different from zero, again as predicted by the model.

Table 3
Coefficients on Layoff Dummy in Earnings Equations from January 1984
and 1986 CPS Displaced Workers Surveys, Males Reemployed
at Survey Date

		Dependent Variable*		
Sample	N	Wage Change (1)	Predisplacement (2)	Postdisplacement (3)
Whole sample	3,427	−.040 (.017)	.017 (.014)	−.021 (.017)
White collar	1,179	−.055 (.028)	−.0094 (.024)	−.064 (.029)
Blue collar	2,248	−.024 (.022)	.022 (.017)	.0023 (.021)
Low union	1,716	−.040 (.023)	−.007 (.020)	−.046 (.024)
High union	1,711	−.031 (.026)	.030 (.020)	.002 (.004)

NOTE.—The reported regressions include a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years), education, a dummy for advance notification of displacement, year-of-displacement dummies, seven previous-industry dummies, eight previous-occupation dummies, experience (age − education − 6) and its square, a marriage dummy, a nonwhite dummy, and three region dummies. Columns 1 and 3 also include years since displacement. The white-collar sample consists of workers with predisplacement jobs as managers and administrators, professional and technical workers, clerical workers, or sales workers. The blue-collar sample consists of workers with predisplacement jobs as craft and kindred workers, operatives, laborers, transport operatives, or service workers. The low-union sample consists of workers in industry-occupation cells with unionization rates of less than 25.5% in 1983; all workers in industry-occupation cells with higher unionization rates are in the high-union sample. Earnings are deflated by the GNP deflator. The numbers in parentheses are standard errors.
   * Dependent variable: col. 1 = log (current wage/previous wage); col. 2 = log (previous wage); col. 3 = log (current wage).

B. Earnings Equations

The raw earnings changes suggest that some stigma is attached to being laid off when employers are likely to be able to pick whom to lay off but that no stigma is attached to being laid off from jobs where formal rules are more likely to govern layoff decisions. To continue to assess the empirical support for the predictions of our model, we present estimates in table 3 of the coefficient on a layoff dummy in regressions of (i) the change in earnings; (ii) predisplacement earnings; and (iii) postdisplacement earnings on a standard set of worker characteristics, year-of-displacement dummies, region dummies, a dummy variable for advance notification of displacement, and one-digit predisplacement occupation and industry dummies.[15]

The estimates for the whole sample presented in table 3 provide some support for the model's basic prediction: the estimate in column 1 reveals that workers displaced through layoffs experience approximately 4% larger wage reductions than do workers with the same measured predisplacement

[15] The wage-change and postdisplacement earnings equations also include years since displacement.

characteristics who were displaced in plant closings. This effect is perhaps stronger than it might appear because it is net of two effects in the opposite direction: first, workers involved in plant closings seem more likely to be located in distressed local labor markets, and, second, many of the layoffs in the sample are likely to have been determined by strict seniority systems. Columns 2 and 3 reveal, however, that the estimate in column 1 arises both from the slightly higher predisplacement earnings and from the slightly lower postdisplacement earnings of those displaced by layoffs. Our model predicts only the lower postdisplacement earnings.

Separate estimates of the effect of cause of displacement on the wage changes of white- and blue-collar workers are also presented in table 3. Among white-collar workers, reemployment earnings are estimated to be more than 6% lower for those displaced by layoffs than for those displaced by plant closings; no similar difference is apparent for blue-collar workers. Thus, for white-collar workers there is fairly strong evidence supporting the lemons effect.

An alternative approach to determining whether workers were displaced from jobs that were likely to be governed by formal layoff-by-seniority rules is to classify workers by the likelihood that their predisplacement jobs were unionized. Since the DWS does not provide information on whether a worker's predisplacement job was unionized, we used the extent of unionization in a worker's predisplacement industry-occupation cell to determine whether the worker's predisplacement job was likely to be covered by a collective-bargaining agreement. We used all 12 outgoing rotation groups from the 1983 Current Population Survey (the full-year sample) to generate a sample of workers comparable to our DWS sample and then to compute unionization rates for white- and blue-collar workers in each three-digit industry (as defined in the 1980 Census of Population).[16] We then classified workers into high- and low-union subsamples depending on whether their predisplacement industry-occupation cell had a unionization rate above or below the sample median rate of 25.5%. The mean unionization rates in the high- and low-union subsamples are 50.3% and 9.7%, respectively.

Estimates of the effect of cause of displacement on the earnings of workers by unionization class are presented in the last two rows of table 3. The layoff coefficients for the low-union sample are qualitatively similar to but not as large as the analogous coefficients for the white-collar sample. In

[16] Unionization rates were computed for 420 industry-occupation cells using a 210-industry by two-occupation (i.e., white- and blue-collar) classification scheme. The sample was restricted to 20–61-year-old, male, full-time, private-sector employees not working in agriculture or construction. The sample consisted of 53,972 observations satisfying these criteria. Workers were classified as unionized if they were union members and/or employed in a job covered by a collective-bargaining agreement.

particular, the reemployment earnings of low-union workers displaced by layoffs are estimated to be approximately 4.5% lower than are those of low-union workers displaced by plant closings, while no such gap is apparent in the high-union sample.

In sum, the estimates in table 3 are consistent with the view that the information content of a layoff is greater where employers have more discretion over whom to lay off. According to (a simple extension of) our model, however, the information content of a layoff also should be greater where employers have better information about workers' abilities. Since some time may have to elapse before the current employer can accurately evaluate a worker's ability (perhaps because the employer is unable to learn the worker's ability until the worker learns the job), it may be that layoffs after brief employment spells signal little information to prospective employers. To study this possibility, we explored whether the lemons effect associated with layoffs differs for workers with more versus less predisplacement job tenure. We reestimated the regressions in rows 1–3 of table 3 after replacing the layoff dummy with two interactions between the layoff dummy and two predisplacement tenure dummies—a low-tenure dummy for predisplacement tenure less than 2 years and a high-tenure dummy for predisplacement tenure of at least 2 years. The results of these regressions are presented in table 4.[17]

The estimates in table 4 reveal that the effect of a layoff (vs. a plant closing) on a displaced worker's wages varies substantially with predisplacement tenure. For the whole sample and for white- and blue-collar workers considered separately, workers with less than 2 years of tenure experience essentially no (statistically significant) differential effect from a layoff. Not surprisingly, therefore, the estimates for workers with at least 2 years of tenure are amplified versions of the estimates that do not allow for tenure differences (presented in table 3). For high-tenure white-collar workers, for example, the extra loss in postdisplacement earnings following a layoff rather than a plant closing is 8.2% rather than the 6.4% for all white-collar workers reported in table 3. As before, the analogous loss for high-tenure blue-collar workers is zero, as is the influence of a layoff on the *pre*displacement earnings of high-tenure white-collar workers. Unlike in table 3, however, the influence of a layoff on the *pre*displacement earnings of high-tenure blue-collar workers is now (a statistically significant) 4% rather than (an insignificant) 2%.

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[17] We also computed the analogous estimates using low- and high-tenure dummies defined as less than and at least 1 year of predisplacement tenure, respectively. Because the sample of such low-tenure workers is extremely small, the estimates for the low-tenure subsample are quite imprecise and the estimates for the high-tenure subsample are very similar to the estimates for the whole sample presented in table 3.

Table 4
Coefficients on Interaction of Layoff Dummy with Low- and High-Tenure Dummies in Earnings Equations from January 1984 and 1986 CPS Displaced Workers Surveys, Males Reemployed at Survey Date

		Dependent Variable*		
Sample	N	Wage Change (1)	Predisplacement (2)	Postdisplacement (3)
Whole sample:	3,427			
Layoff dummy				
× low-tenure dummy		−.011	−.022	−.031
		(.030)	(.024)	(.029)
Layoff dummy				
× high-tenure dummy		−.054	.036	−.016
		(.021)	(.017)	(.021)
White collar:	1,179			
Layoff dummy				
× low-tenure dummy		.011	−.038	−.026
		(.047)	(.042)	(.050)
Layoff dummy				
× high-tenure dummy		−.087	.004	−.082
		(.033)	(.029)	(.035)
Blue collar:	2,248			
Layoff dummy				
× low-tenure dummy		−.010	−.020	−.027
		(.038)	(.030)	(.037)
Layoff dummy				
× low-tenure dummy		−.030	.043	.017
		(.027)	(.021)	(.026)

NOTE.—Low tenure = less than 2 years of tenure on predisplacement job; high tenure = at least 2 years of tenure on predisplacement job. The reported regressions include a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years), education, a dummy for advance notification of displacement, year-of-displacement dummies, seven previous-industry dummies, eight previous-occupation dummies, experience (age − education − 6) and its square, a marriage dummy, a nonwhite dummy, and three region dummies. Columns 1 and 3 also include years since displacement. The white-collar sample consists of workers with predisplacement jobs as managers and administrators, professional and technical workers, clerical workers, or sales workers. The blue-collar sample consists of workers with predisplacement jobs as craft and kindred workers, operatives, laborers, transport operatives, or service workers. Earnings are deflated by the GNP deflator. The numbers in parentheses are standard errors.
  * Dependent variable: col. 1 = log (current wage/previous wage); col. 2 = log (previous wage); col. 3 = log (current wage).

C. Sensitivity Analyses

Since the equations presented in table 3 were estimated on the sample of displaced workers who were reemployed at the survey date, the estimates potentially reflect sample-selection bias. We have taken two approaches to probe the importance of this problem. First, we reestimated the models presented in table 3 using the two-stage sample-selection bias correction approach of Heckman (1979).[18] The Heckit estimates of the layoff-dummy

[18] The reemployment probit equations in the first stage of the Heckit procedure were estimated on our basic sample plus displaced males who were unemployed, self-employed, or employed and earning less than $40 a week at the survey date.

Table 5
Coefficients on Layoff Dummy in Earnings Equations from January 1984 and 1986 CPS Displaced Workers Surveys, Males Displaced at Least 2 Years before the Survey Date and Reemployed at the Survey Date

		Dependent Variable*		
Sample	N	Wage Change (1)	Predisplacement (2)	Postdisplacement (3)
Whole sample	1,875	−.017	.0028	−.013
		(.030)	(.019)	(.023)
White collar	602	−.047	−.030	−.075
		(.039)	(.034)	(.040)
Blue collar	1,273	.0081	.013	.021
		(.030)	(.024)	(.028)

NOTE.—The reported regressions include a spline function in previous tenure (with breaks at 1, 2, 3, and 6 years), education, a dummy for advance notification of displacement, year-of-displacement dummies, seven previous-industry dummies, eight previous-occupation dummies, experience (age − education − 6) and its square, a marriage dummy, a nonwhite dummy, and three region dummies. Columns 1 and 3 also include years since displacement. The white-collar sample consists of workers with predisplacement jobs as managers and administrators, professional and technical workers, clerical workers, or sales workers. The blue-collar sample consists of workers with predisplacement jobs as craft and kindred workers, operatives, laborers, transport operatives, or service workers. Earnings are deflated by the GNP deflator. The numbers in parentheses are standard errors.
  * Dependent variable: col. 1 = log (current wage/previous wage); col. 2 = log (previous wage); col. 3 = log (current wage).

coefficient are similar to the ordinary least squares (OLS) estimates presented in table 3 in all cases. For example, the Heckit estimates of the layoff-dummy coefficients for white-collar workers are −0.055 in the wage change equation, −0.0093 in the predisplacement earnings equation, and −0.064 in the postdisplacement earnings equation.

As a second approach to the sample-selection problem, we examined the subsample of workers who were displaced at least 2 years prior to the survey date. These workers have had a substantial amount of time to find a new job. Also, the effects of short-lived bad matches and temporary jobs after displacement should be reduced. Estimates of the layoff coefficient for this subsample of displaced workers are presented in table 5. The layoff effects for white-collar workers displaced at least 2 years before the survey date are similar to the analogous effects for the entire white-collar sample, and the findings for the whole sample and for blue-collar workers are in the spirit of the analogous results in table 3.

In addition to sample-selection bias, a second potential bias arises because

The reemployment probits included the same variables as the independent variables in the wage regressions in table 3. Since we could not make a convincing exclusion restriction (i.e., identify a variable that affects the probability of reemployment at the survey date but does not affect earnings), the Heckit models are identified only by the nonlinearity of the sample-selection bias correction term (the inverse Mills ratio).

the DWS asks respondents about events that occurred as long as 5 years prior to the survey date. Thus, some respondents may have either completely forgotten events that occurred in the distant past or remembered such events but misreported the dates at which they occurred, or both. Robert Topel has alerted us to one possible manifestation of this kind of retrospection bias: a comparison of the layoffs reported in the 1984 and 1986 DWS's for the years these surveys have in common (1981–83) reveals that many more layoffs are reported in the 1984 survey than in the 1986 survey, while the analogous comparison of the plant closings reported at the two survey dates reveals a much smaller difference.[19] One might hypothesize that (i) this large difference between the layoffs reported in 1984 versus those reported in 1986 results because some 1986 respondents have simply forgotten layoffs that occurred between 1981 and 1983, *and* (ii) these forgotten layoffs were disproportionately those that did not result in large earnings losses. (Implicit in this pair of hypotheses is the notion that plant closings are memorable, even if they do not result in large earnings losses.)

In brief, we find three reasons why retrospection bias appears *not* to be an important problem here.[20] First, while we cannot reject the possibility that such a bias explains our results for the sample as a whole, the data do not support the conjecture that such a bias explains our results for white- versus blue-collar workers. Second, the presence of such a bias would seem to suggest that the lemons effect should grow (in absolute value) with years since displacement, but it does not. And third, the effect of such a bias would seem to be reduced for workers displaced from jobs with long predisplacement tenure, but (as reported in table 4) the qualitative properties of our empirical results are not only preserved but even strengthened by focusing on workers with long predisplacement tenure.

IV. Unemployment and Reason for Displacement

Like wage changes at displacement, the unemployment experiences of displaced workers also appear to differ substantially by the reason for displacement. In table 1 we found that, among permanently displaced workers who were reemployed at the survey date, those displaced by plant closings were less likely to have experienced a spell of unemployment after

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[19] In our sample (including those not reemployed at the survey date), the layoffs reported in the 1984 DWS are 314, 504, and 484 for 1981–83, respectively, while those reported in the 1986 DWS are 142, 258, and 188 for the same years. The plant closings reported in the 1984 DWS, in contrast, are 238, 298, and 272 for 1981–83, respectively, while those reported in the 1986 DWS are 227, 247, and 242 for the same years.

[20] In app. 2 of an earlier version of this article (Gibbons and Katz 1989), we present a detailed investigation of the extent to which retrospection bias may affect our empirical results.

displacement and had fewer weeks of joblessness following displacement than did workers displaced by layoffs.[21] Table 2 shows that these differences in the probability and duration of unemployment between those displaced by layoffs and those displaced by plant closings are more pronounced for white- than for blue-collar workers.

We know of two potential explanations for the fact that workers permanently displaced by plant closings experience shorter unemployment spells than do workers permanently displaced by layoffs. The first potential explanation, due to Katz (1986), focuses on the importance of recall expectations in the job-search behavior of the unemployed: workers displaced by layoffs are more likely to think they may be recalled to their preunemployment jobs than are workers displaced in plant closings; higher recall expectations are likely to reduce the new-job-finding rate by reducing search intensity and making workers choosier about new jobs. Katz and Meyer (1990) find evidence in support of this view: in a sample of unemployment insurance (UI) recipients in Missouri, workers who expected to be recalled at the time of job loss have much lower (approximately 50% lower) new-job-finding rates than do workers who did not expect to be recalled.

The second potential explanation involves a simple extension of our lemons model. The model developed in Section II yields predictions about wage changes at displacement but also predicts that there will be no postdisplacement unemployment. The latter prediction arises for a simple reason: for any belief about a worker's ability, there is always a wage low enough that firms will be willing to hire the worker.

To generate postdisplacement unemployment, the model could be changed so that for sufficiently pessimistic beliefs about a worker's ability either no firm is willing to hire the worker or it takes time for the worker to find a firm that is willing to hire him or her. One could imagine, for example, that some firms use technologies that are extremely sensitive to the worker's ability, so that hiring a bad worker results in a net loss (the cost of broken equipment exceeds the value of output produced), while other firms use technologies such as that described in Section II, so that there always exists a wage low enough to make the latter firms willing to hire a bad worker. Adding such technological heterogeneity and a job-search mechanism to our lemons model would yield positive (but finite) postdisplacement unemployment durations. The expected duration would be longer for workers displaced by layoffs (again, because of the lemons effect) than for workers displaced by plant closings.

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[21] These results are consistent with the earlier findings of Kruse (1988) using the 1984 DWS. Similarly, Katz (1986) finds using a sample of household heads from the Panel Study of Income Dynamics that workers who enter unemployment through layoffs have lower escape rates from unemployment through the finding of *new* jobs than do those displaced through plant closings.

   In an attempt to assess the extent to which a lemons effect contributes to our finding that workers permanently displaced by layoffs experience longer unemployment spells than do those displaced by plant closings, we used the January 1986 DWS to construct a sample of first spells of joblessness for 20–61 year-old males permanently displaced from full-time, private-sector jobs not in agriculture or construction.[22] The sample contains 830 complete initial spells of joblessness and 498 censored spells.

   We analyze the duration of initial spells of joblessness for this sample using formal hazard-model techniques. We parameterize the hazard rate (i.e., the escape rate from joblessness) using a Weibull specification. The hazard rate for individual i at time t is specified as

$$\lambda_i(t) = \alpha t^{\alpha-1} \exp(X_i \delta), \tag{6}$$

where X_i is a vector of time-invariant covariates for individual i, α is the Weibull duration-dependence parameter, and δ is a vector of parameters. (See Kalbfleisch and Prentice [1980, pp. 23–25 and 30–32] for a discussion of the properties of the Weibull model.) Let T_i be the length of individual i's unemployment spell. The Weibull specification of the hazard function implies that the log of the failure time for i (i.e., $Y_i = \log T_i$) can be written as a regression model of the form

$$Y_i = X_i \beta + \sigma \varepsilon_i, \tag{7}$$

where $\sigma = 1/\alpha$ is known as the Weibull scale parameter, $\beta = -\sigma\delta$, and ε_i is an error term with an extreme-value distribution (Kalbfleisch and Prentice 1980, pp. 22–24).

   Table 6 presents maximum-likelihood estimates of Weibull duration models for the initial spell of joblessness following displacement for our 1986 DWS sample. For ease of interpretation, we present the estimates in the form of the regression model in equation (7): we report the parameter β_j for each covariate X_j. The estimates presented in table 6 thus can be interpreted as the effects of the covariates on the expected log duration of joblessness.

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[22] The January 1984 DWS only provides information on total weeks of joblessness since displacement and so does not allow one to determine the length of the initial spell of joblessness. The January 1986 DWS, in contrast, provides information on the number of jobs held by a worker since displacement. This variable and information on total weeks of joblessness since displacement allow one to determine both the length of the initial spell of joblessness for those employed in their first job since displacement at the survey date and the censored length of the initial spell for those who had not worked since displacement. The variable that measures weeks of joblessness since displacement is top-coded at 99. We treat initial spells of joblessness top-coded at 99 as being censored at 99 weeks.

Table 6
Effects of Selected Variables on the Duration of the First Spell
of Joblessness following Displacement from January 1986 CPS
Displaced Workers Survey, Males with Only One Spell of
Joblessness since Displacement
Dependent Variable = Log (Weeks of Joblessness)
Weibull Duration Model Specification

Variable	(1)	(2)	(3)	(4)
Layoff = 1	.248	.244	.352	.323
	(.086)	(.108)	(.106)	(.126)
Layoff × white collar	. . .	−.049	. . .	. . .
		(.168)		
Layoff × high union	. . .	. . .	−.299	. . .
			(.147)	
Layoff × fraction union	. . .	. . .	. . .	−.358
				(.345)
Fraction union	. . .	1.173	1.363	1.326
		(.266)	(.294)	(.033)
Previous tenure in years	.037	.034	.034	.033
	(.007)	(.007)	(.007)	(.007)
Log of previous real weekly earnings	−.301	−.339	−.331	−.333
	(.100)	(.099)	(.099)	(.099)
Weibull scale parameter (σ)	1.146	1.139	1.137	1.139
	(.033)	(.032)	(.032)	(.032)
Log likelihood	−1,831.3	−1,822.2	−1,820.2	−1,821.7

NOTE.—The reported models were estimated by maximum likelihood with left censoring explicitly treated using the LIFEREG procedure in SAS. The sample size is 1,228. The reported specifications include education, a dummy for advance notification of displacement, year-of-displacement dummies, seven previous-industry dummies, eight previous-occupation dummies, experience (age − education − 6) and its square, a marriage dummy, a nonwhite dummy, and three region dummies. Fraction union is the 1983 fraction unionized of the worker's predisplacement industry-occupation cell. High union equals one for workers displaced from industry-occupation cells where the fraction unionized was greater than 25.5% in 1983; it equals zero otherwise. Earnings are deflated by the GNP deflator. The numbers in parentheses are asymptotic standard errors.

The coefficient estimate for the layoff dummy in column 1 of table 6 indicates that workers permanently displaced by layoffs have approximately 25% longer initial unemployment spells than do those displaced in plant closings. This finding is consistent with our (extended) lemons model but of course also is consistent with a recall-expectations model. In columns 2–4 of table 6 we attempt to isolate the effect due solely to the lemons model.

The estimates in column 2 indicate that the effect observed in column 1 for the whole sample also appears in both the white- and blue-collar subsamples of our data set: workers permanently displaced by layoffs experience significantly longer initial unemployment spells than do workers displaced by plant closings, regardless of whether the displacement is from a white- or blue-collar job. (More precisely, the point estimates in col. 2 suggest that the effect is slightly smaller for white-collar workers but far from statistically significantly so.) Since white-collar workers are much

less likely to expect to be recalled than are blue-collar workers,[23] the similarity in the impact of the layoff dummy on unemployment durations for the two groups suggests that a lemons effect may be influencing the post-displacement unemployment duration of white-collar workers permanently displaced by layoffs.

The estimates in columns 3 and 4 are analogous to those in column 2, except that we use a second approach to attempt to define a subsample of jobs that are likely to be governed by formal layoff-by-seniority rules and therefore not likely to be subject to a lemons effect. As in Section IIIB, we classify each worker in terms of a measure of the extent of unionization in the worker's predisplacement industry and occupation (as described in n. 16 above).

In column 3 our measure of the extent of unionization is a dummy variable equal to one if the unionization rate exceeds 25.5% (the median of the sample described in Section IIIB). We find that, for workers from low-unionization predisplacement industries and occupations, those displaced by layoffs experience significantly longer (approximately 35% longer) postdisplacement unemployment durations than do those displaced by plant closings. We also find that, for workers from high-unionization predisplacement industries and occupations, those displaced by layoffs experience only 5% longer unemployment spells than do those displaced by plant closings; this difference in unemployment duration between those laid off from low- versus high-unionization industries and occupations is statistically significant. In column 4 we interact the layoff dummy with the unionization rate (i.e., the continuous variable that underlies the unionization dummy described above) of the worker's predisplacement industry and occupation. The results are qualitatively similar to those reported in column 3.

In sum, the evidence presented in table 6 shows that workers permanently displaced by layoffs experience significantly longer initial unemployment durations following displacement than do workers displaced by plant closings. While both our (extended) lemons model and a recall-expectations model are capable of explaining this fact, we find that the result persists for subsamples that seem likely to fit the lemons model but unlikely to fit the recall-expectations model.[24]

[23] Katz and Meyer (1990) find for a sample of UI recipients from Missouri and Pennsylvania in 1979–80 that 58% of blue-collar workers who had spells that ended in a new job initially expected to be recalled by their previous employer, while only 25% of the comparable sample of white-collar workers expected to be recalled.

[24] Furthermore, Alba and Freeman (1990) find for a sample of displaced workers from Spain in the early 1980s that those "individually fired" had substantially longer unemployment durations than those displaced by plant closings ("firm shutdowns"). Since temporary layoffs are quite unimportant in Spain, this is suggestive evidence of a lemons effect from layoffs operating in the Spanish labor market.

V. Summary and Interpretation

In this article we develop and find empirical support for an asymmetric-information model of layoffs. The model is based on a seemingly plausible form of asymmetric information: a worker's current employer is assumed to be better informed about the worker's productive ability than prospective employers are. The key feature of the model is that, when firms have discretion with respect to whom to lay off, the market infers that laid-off workers are of low ability. Under the assumption that no such negative inference is warranted if workers are displaced in a plant closing, our model predicts that the postdisplacement wages of otherwise observationally equivalent workers will be lower for those displaced by layoffs than for those displaced by plant closings. Furthermore, in our model it is the layoff event that signals unfavorable information to the market, so the model predicts that the *pre*displacement wages of (otherwise observationally) equivalent workers will *not* differ according to the cause of displacement. Finally, a simple extension of our model predicts that the average postdisplacement unemployment spell of otherwise observationally equivalent workers will be shorter for those displaced by plant closings than for those permanently displaced by layoffs.

Using data on a large sample of permanently displaced workers, we find two kinds of evidence consistent with the lemons effect predicted by our model. First, with respect to pre- and postdisplacement earnings, the postdisplacement earnings of white-collar workers who are displaced by layoffs are significantly lower than those of white-collar workers displaced by plant closings, but predisplacement earnings do not vary with cause of displacement. Second, white-collar workers permanently displaced by layoffs endure postdisplacement unemployment spells that are significantly longer than those endured by white-collar workers displaced by plant closings. The fact that both of these findings are consistent with our model may bode well for future theoretical and empirical work on wages and mobility based on the information asymmetry we analyze. Alternatively, our empirical results can be interpreted as support for certain symmetric-information models.

In order for a symmetric-information model to match our empirical findings, the model must first provide descriptions of why layoffs occur (rather than wage changes) and of which workers are laid off. Having done this, the model must then explain why laid-off workers fare worse than those displaced by plant closings, in terms of postdisplacement wages and unemployment durations, but are no different in terms of predisplacement wages.

In a symmetric-information model in which a worker of a given ability is equally productive in all firms, the equilibrium response to bad news about a worker's ability is a wage reduction, not a layoff. Our asymmetric-

information model, in contrast, strongly motivates a layoff rather than a wage reduction in response to bad news about a worker's ability. In equilibrium, the firm cannot retain low-ability workers at a low wage: if it could, it would also retain high-ability workers at the low wage, which would destroy the market's willingness to allow any workers to be retained at the low wage.

In a symmetric-information model in which firm-specific human capital and (general-purpose) ability are complements, it could be that a firm's optimal response to a demand shock is to lay off low-ability workers because the return on such workers' specific capital is low. In such a model, however, laid-off workers should have low *pre*displacement wages as well as low postdisplacement wages. We find no empirical support for the former prediction.

One symmetric-information model that matches our empirical findings involves learning and sticky wages. Suppose that, at the beginning of a worker's career, information about the worker's productive ability is imperfect but symmetric: the worker, the firm, and the market all hold the same (imprecise) belief.[25] As the worker's career progresses, the worker, the firm, and the market all observe the same information about the worker's performance, so at any point in the worker's career, the worker, the firm, and the market all hold a common belief about the worker's ability. If wages are sticky (perhaps because wages are attached to jobs, as in an internal labor market), layoffs may occur if the worker's productivity turns out to be much lower than was at first expected. Because information is symmetric, the market does not learn from the layoff per se (unlike in our lemons model), but the market does take the opportunity that a layoff presents to reduce the worker's wage.

A second reason layoffs might occur in a symmetric-information model involves matching.[26] Suppose, for instance, that there are two industries: in industry A, output is very sensitive to ability, while in industry B, output is relatively insensitive to ability. Suppose further that, if information about

[25] Such an information structure would arise if, e.g., the worker's schooling is a noisy indicator of the worker's productive ability and, unlike the familiar Spence formulation, the combination of the worker's lack of experience in the labor market and firms' wealth of experience in evaluating new workers implies that the worker and all the firms are equally able to predict the worker's productive ability.

[26] Matching can yield layoffs in asymmetric-information models, as well; see Antel (1985) and McLaughlin (1991). These models focus on the distinction between quits and layoffs but are silent concerning plant closings. If layoffs occur when match quality at the current firm is relatively low (i.e., lower than match quality at an outside firm) but plant closings occur when match quality at the current firm is absolutely low (i.e., at the bottom of the distribution of match quality), then the distribution of postdisplacement match quality will have more weight in the lower tail after a plant closing. Such a model seems unlikely to reproduce our finding that laid-off workers have lower postdisplacement wages.

workers' productive abilities were perfect, then high-ability workers would be employed in industry A and low-ability workers would be employed in industry B. As described above, however, information about workers' abilities is imperfect but improves over time. As a result, at the beginning of their careers, workers who appear to be of high-(respectively, low-) ability are employed in industry A (respectively, B). Over time, as new information about workers' productive abilities becomes available, mobility endogenously improves the matching of workers with industries.[27]

Two kinds of mobility occur in this matching model: workers who perform surprisingly well move from industry B to A, while those who perform surprisingly poorly move from industry A to B. If we call the former a quit and the latter a layoff, then this matching model also generates the main empirical prediction of our lemons model—laid-off workers receive low reemployment wages. The data do not provide further support for this matching model, however. We reestimated the postdisplacement earnings equation for white-collar workers (table 3, row 2, col. 3) with the addition of both a dummy variable equal to one if the worker's reemployment industry differs from the worker's predisplacement industry (at the one-digit level) and the interaction of this dummy with the layoff dummy. We found that (i) the coefficient on the switch-industry dummy is negative, large (in absolute value), and statistically significant; (ii) the coefficient on the layoff dummy is essentially identical to the coefficient reported in table 3 (i.e., −.064), but the standard error is large enough that the coefficient is no longer significant at conventional levels; and (iii) the coefficient on the interaction between the switch-industry and layoff dummies is extremely small (but positive). This third result is hard to reconcile with the matching model sketched above, which suggests that laid-off workers who switch industries should receive especially low reemployment wages.

In sum, the predictions of our lemons model can be generated by some (but by no means all) symmetric-information models. Our empirical results therefore are not conclusive proof that asymmetric information plays an important role in the labor market. Rather, we interpret the results as a necessary (but not sufficient) condition for confidence in models based on asymmetric information about workers' abilities: had our estimates rejected our model, it would have cast doubt on the entire family of models based on this kind of asymmetric information (as well as on the symmetric-information models that generate the same predictions). Unfortunately, the nature of asymmetric information seems to imply that direct empirical

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[27] See Gibbons and Katz (1991) for a precise statement of this model, which is akin to a dynamic version of Roy's (1951) model, extended to include imperfect information, learning, and endogenous mobility.

tests of its importance are not possible, so indirect tests of the kind presented here may be all that is possible.

Appendix
Construction and Characterization of Equilibria

This appendix provides the details of two arguments left incomplete in the text. First, we complete the proof that there exists a continuum of equilibria parameterized by η_R, which ranges from η_L to η^* given in (3). Second, we show that, even in equilibria not belonging to the continuum of equilibria parameterized by η_R (if such alternative equilibria exist), our main qualitative conclusion continues to hold: $w_2(L) < E(\eta)$.

Part 1.—We first establish that there exists a unique η^* satisfying (3) and that $\eta^* < \eta_H$. At $\eta^* = \eta_L$, the right-hand side of (3) exceeds the left; at $\eta^* = \eta_H$, the left exceeds the right; and the derivative of the left-hand side with respect to η^* is one, which exceeds the derivative of the right-hand side because the fact that $f(\eta)$ is log concave implies that $d\{E(\eta|\eta \geq x)\}/dx \leq 1$ for every x. Note (for use below) that the same monotonicity argument implies that, if $\eta_R < \eta^*$, then $\eta_R + s < E(\eta|\eta \geq \eta_R)$.

We show next that given a value of $\eta_R < \eta^*$, there exists a unique solution to (1). At $w_m = \eta_L$, the right-hand side of (1) is positive because the first term is positive and the probability term in the second term is zero. At $w_m = \eta_H$, the right-hand side is negative because the first term is negative and the second term is either negative or zero (depending on whether the probability term is positive or zero). Finally, the derivative of the right-hand side with respect to w_m is negative because $d\{F(x)[E(\eta|\eta < x) - x]\}/dx = -F(x) < 0$ for every x. Thus, given η_R, (1) has a unique solution. Furthermore, this solution is a best response for prospective employers, given that the firm lays off a worker if and only if the worker's ability is less than η_R. Offering a lower wage attracts no workers and so earns no profit. Offering a higher wage attracts all rather than half of the pool of workers that would otherwise accept the market's offer and also attracts the lowest-ability workers that the firm would otherwise retain, but the prospective employer earns negative profit on both groups.

Finally, we show (by contradiction) that, if $\eta_R < \eta^*$, then (2) holds: $\eta_R + s \leq w_m$, where w_m is the solution to (1). If $\eta_R + s > w_m$, then prob$\{\eta + s < w_m|\eta \geq \eta_R\} = 0$, so the solution to (1) is $w_m = E(\eta|\eta \geq \eta_R)$, but then $\eta_R + s > E(\eta|\eta \geq \eta_R)$, which contradicts the assumption that $\eta_R < \eta^*$ (because of the last sentence of the first paragraph of part 1 above).

Part 2.—We show next that any equilibrium layoff rule leads to a reemployment wage for laid-off workers satisfying $w_2(L) < E(\eta)$. Just as in the argument leading to (2) in the text, if the firm's layoff rule has $L(\eta) = 1$ for some η, then it must be that $\eta + s \leq w_m$, so $E[\eta|L(\eta) = 1] < w_m$. But the analog of (1), modified to account for the layoff rule under consideration, is

$$0 = \mu \{ E[\eta \,|\, L(\eta) = 0] - w_m \} + (1 - \mu) \text{prob} \{ \eta + s < w_m \,|\, L(\eta) = 0 \}$$

$$\times \{ E[\eta \,|\, \eta + s < w_m, L(\eta) = 0] - w_m \}, \qquad (A1)$$

which implies that

$$w_m \leq E[\eta \,|\, L(\eta) = 0], \text{ so } E[\eta \,|\, L(\eta) = 1] < E[\eta \,|\, L(\eta) = 0].$$

Therefore,

$$w_2(L) = E[\eta \,|\, L(\eta) = 1] < E(\eta) < E[\eta \,|\, L(\eta) = 0].$$

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