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Author(s): James N. Brown and Orley Ashenfelter

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Testing the Efficiency of Employment Contracts

James N. Brown

State University of New York at Stony Brook

Orley Ashenfelter

Princeton University

The recent literature on employment contracts emphasizes that it is in the interests of the parties to produce institutional arrangements that lead to employment contracts that we have termed "strongly efficient." Strong efficiency implies that employment is set so as to equate the marginal revenue product of workers to their alternative wage. It follows that employment in such contracts fluctuates with the determinants of a worker's marginal revenue product and with the worker's alternative wage, but not with the observed contract wage. We have examined two kinds of evidence to test the strong efficiency hypothesis. Laboratory experiments by Siegel et al. indicate that this hypothesis is strongly confirmed when the bargaining parties are required to agree on price and quantity simultaneously and is strongly rejected when the parties are required to bargain by a system of price leadership. In our field data on the printing trades, we find no convincing evidence of strong efficiency. We have also examined the evidence in support of what we have called the "weak efficiency hypothesis." According to this hypothesis, both the contract wage and the alternative wage determine employment. We have found only mixed support for this hypothesis because our measures of the alternative wage available to workers are frequently positively related to employment, precisely the contrary to the hypothesized direction of this effect in a weakly efficient contract.

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I. Introduction

In recent years there has been a rebirth of interest in the idiosyncratic nature of employment and wage bargains. The belated recognition of the importance of specific human capital has emphasized that the parties to these bargains enter them with skills or resources whose values either are, or will afterward become, unique to a particular bargaining partnership. This raises the old problem of determining the wage and employment bargain that will be struck under bilateral monopoly. A rich variety of theoretical models have now been developed to describe labor contracts in the presence of bilateral monopoly. As yet, however, little empirical work based on these models has appeared. It seems an appropriate time, therefore, to begin the process of laying out the methods by which these models might be tested and their empirical relevance assessed.

The early theoretical analyses of bilateral monopoly date to Edgeworth (1881), Marshall (1890), Cournot (1897), Pareto (1909), Pigou (1920), and Bowley (1928). According to the prediction of Cournot and Bowley, the typical case will involve bargainers of unequal "strength," and one of these bargainers will be able to dictate the price, while the other will establish the quantity traded. This leads to the simple monopoly model of wage and employment determination proposed by Dunlop (1944), which, of course, does not predict a Pareto-efficient level of employment.

Empirical models inspired by the Cournot-Bowley-Dunlop setup have been applied to labor market data by several authors, including Farber (1978) for U.S. coal mining, Dertouzos and Pencavel (1981) for U.S. printing trades, and Carruth and Oswald (1985) for British coal mining. The assumed accuracy of this monopoly model also underpins a number of analyses of union behavior, including Lewis's (1963) study of the effect of unions on employment and Rees's (1963) and more recently Freeman and Medoff's (1984) estimates of the "welfare costs" of trade union wage and employment "distortions." The essence of these models is that workers and employers end up reducing the total amount of rents available to themselves in their efforts to divide these rents.

The simple monopoly model implies that there are unexploited "potential" gains from trade available to both parties. In contrast, an alternative model inspired by Edgeworth and Pareto leaves the transacted price indeterminate but predicts the quantity transacted to be at its Pareto-efficient level. This leads to the model of employment determination suggested by Fellner (1949) but with the wage rate determined by other unspecified factors. Empirical studies inspired by the Edgeworth-Pareto-Fellner setup have been carried out by de Menil

(1968, 1971) and Svejnar (in press). The essential idea of these models is that bargaining over all factors that are not in fixed supply should lead to the Pareto-efficient employment of all factors. Consequently, there are no trade union "distortions" or "welfare costs."

These early empirical models did not carefully specify the information about technology, costs, and demand that each party carried to the negotiating table. More recent research on employment contracts explores the possibility that incomplete information may prevent the parties from reaching otherwise Pareto-efficient employment levels. In the research surveyed by Hart (1983), for example, workers have incomplete information about a firm's product demand. Since it is never in the interest of the firm to reveal states of rising demand and always in the interest of the firm to reveal states of falling demand, it is hypothesized that no institutional structure in which all available information is credibly shared by the parties can be established. The problems thus created are mitigated, however, by contracts in which employment is reduced below its otherwise Pareto-efficient level in bad times, with the reduction in employment providing the credible signal that it is *mutually* profitable for wages to be reduced.[1]

Hall and Lazear (1984) consider a simplified case in which workers have incomplete information about product demand and firms have incomplete information about workers' alternatives. In their analysis, no single institutional setup will lead to Pareto-efficient employment without excessive monitoring or information-gathering costs. Hall and Lazear conclude that Pareto-inefficient separations are a natural consequence of the presence of such costs in "free" labor markets.

In contrast to these results on "inefficient" contracts in the presence of monitoring costs, an important conclusion to emerge from the recent literature on "fully" Pareto-efficient bilateral contracts is that employment should fluctuate only with the alternative wage available to the workers who are a party to the bargain, and not with the contract wage. When workers are risk neutral, this result occurs because the opportunity cost of a worker's time is the alternative wage, and both parties agree to set employment such that the marginal product of workers is equated with their opportunity cost. When workers are risk averse, this result can still occur because it is then in the interests of the workers collectively to establish a scheme for unemployment benefits that makes it profitable for workers collectively to behave as if they were risk neutral. Finally, when workers as a group cannot be characterized as maximizing expected utility, it will

[1] Put differently, in the presence of asymmetric information, optimal contracts amount to an enforced relationship betwen wages and employment. This general idea has recently been applied to the question of strikes by Hayes (1984) and was originally spelled out by Hall and Lilien (1979).

then be optimal for the parties to engage a risk-neutral third party, perhaps an insurance company or the firm's shareholders, and, barring market imperfections, it will again be mutually profitable for employment to fluctuate only with the alternative, as opposed to the contract, wage.

It is this proposition that suggests a basis for our test of the efficient contracting hypothesis. To be concrete we shall call the hypothesis that the marginal revenue product of employment is determined *only* by the alternative wage the *strong efficiency hypothesis*. Rejection of this hypothesis implies that some obstacle impedes the establishment of an employment relationship that is first-best Pareto efficient. Of course, by itself, rejection of this hypothesis has no immediate normative implications because the precise nature of the impediment preventing efficiency has not been specified. Still, the rejection of this hypothesis should lead to a search for the nature of these impediments and their cause.

At a minimum, however, it is clear that, in any efficient bilateral contract, the alternative wage rate must determine, at least in part, the marginal revenue product of employment. If the alternative wage rate has no role to play in the determination of employment, it is apparent that, with the kind of data actually available, the simple monopoly and Pareto efficiency hypotheses are empirically indistinguishable. It is therefore worth knowing whether the available evidence supports the hypothesis that the alternative wage plays at least *some* role in the determination of employment. To be concrete, we shall call this hypothesis the *weak efficiency hypothesis*. Our purpose in this paper is to set out a variety of methods for assessing the empirical relevance of these weak and strong efficiency hypotheses.

Our motivation for this analysis is twofold. First, a considerable amount of policy-oriented research has assumed the empirical relevance of the monopoly model of employment determination. Measures of the welfare cost of trade unions and accusations of worker overmanning and featherbedding all rest on the assumed relevance of the simple monopoly model. If employment contracts are efficient, however, unions merely generate transfers to workers from those who earn rents from factors in fixed supply. From this viewpoint, featherbedding may simply be the method by which the employment contract is enforced and employers are kept from reneging, ex post, on efficient bargains. In the simple monopoly model, an increase in the bargaining power of workers leads to a higher wage rate but a lower employment level as firms move up their demand curves. As McDonald and Solow (1981) observe, however, in efficient contracts we should generally expect an increase in the bargaining power of workers to lead to a higher wage and at least as great a level of employment. Policies to weaken trade union and worker power in the first case would lead to increased employment, whereas in the second they would merely redistribute income from workers to capitalists and perhaps decrease employment as well.

Second, an extensive body of empirical work in labor demand and labor supply treats the current wage rate as the relevant measure of the price of workers' time. If contracts are efficient and marginal revenue products are determined either independently of, or only partially by, the contract wage, however, all this work may be called into question.[2] Given this fact, we think it is especially important to know whether the data warrant the role typically given to contract wages in models of employment determination.

Our appraisal of the evidence on efficient employment contracts makes use of two different types of data. We begin by discussing the results of laboratory bargaining experiments on bilateral monopoly conducted by Siegel and Fouraker (1960) and Fouraker, Siegel, and Harnett (1961) some 25 years ago. We then turn to a detailed analysis of microeconomic field data on employment and wage rates in the printing trades. Our interest in the printing industry stems from the presence of (*a*) strong unions, (*b*) considerable rents due to the precipitously increasing returns to scale already documented, and (*c*) historical allegations of featherbedding in this industry.

II. Some Evidence from Experimental Data

Although our primary interest lies in explaining observed "field" data on employment, we think that laboratory bargaining experiments provide some insights that may be helpful in the interpretation of our nonexperimental data, where the identification of supply and demand parameters becomes entangled with the identification of different behavioral hypotheses. In the bargaining experiments conducted by Siegel et al., buyers and sellers were given profit tables listing the net *monetary* gain associated with each possible price and quantity pair they might agree on. For the buyer, profits were determined by a linear average revenue schedule,

$$AR = a - bQ, \tag{1}$$

which gave the price per unit at which the buyer could resell (to the experimenter) as many units of the commodity as he purchased from

[2] One might argue, e.g., that the small labor supply elasticities typically estimated for prime-aged males derive in part from this problem.

the seller. The buyer thus profited to the extent that the price charged per unit by the seller was less than the value indicated by equation (1), and the buyer's demand curve for Q was thus the marginal revenue curve derived from (1).

For the seller, profits were determined by a linear average cost schedule,

$$AC = A + BQ, \tag{2}$$

which gave the price per unit the seller was required to pay (to the experimenter) to obtain as many units of the commodity as he sold to the buyer. The seller thus profited to the extent that the price paid per unit by the buyer was greater than the value indicated by (2), and the seller's supply curve for Q was thus the marginal cost curve derived from (2). After the negotiating pair reached agreement, they were paid (by the experimenter) the actual level of profits implied by their agreement.

The conception of these experiments was remarkably modern. In their many bargaining games, the authors varied (*a*) bargainers' information sets (complete vs. incomplete information regarding opponents' objective functions), (*b*) the number of transactions per bargaining pair (one-shot vs. repeated games), and (*c*) the price and quantity at which profits were split equally between the two parties. In addition, the authors went to great lengths to control for other factors that might influence bargaining outcomes. For example, random assignment was used whenever possible, and all bargaining was conducted in total anonymity. Finally, the authors also varied the institutional framework within which bargaining took place. In a first set of experiments (Siegel and Fouraker 1960), bargainers were required to make joint offers for a price *and* a quantity. A second set of experiments (Fouraker, Siegel, and Harnett 1961), however, required the seller to dictate a price to the buyer, who then chose a quantity to buy.

From the point of view of a bargaining pair taken jointly, it is apparent that an efficient bargain should extract the maximum possible rents from the experimenter and somehow allocate those rents to the bargainers. In particular, the specific division of the returns between the two parties should not influence the total profits obtained. In this sense the *pair* of bargainers should act as a single individual.

Total revenues received by the bargainers from the experimenter were $Q \cdot (AR - AC)$. It follows that, if the bargainers' utility levels were linear in income, an efficient contract would select Q to maximize

$$\text{joint profits} = Q(AR - AC). \tag{3}$$

Simple differentiation, after the substitution of (1) and (2) into (3), indicates that this requires the bargainers to exchange a quantity

$$Q^P = \frac{a - A}{2(b + B)}, \tag{4}$$

in which the superscript P denotes the Pareto-efficient outcome, determined by the intersection of the bargainers' marginal revenue and marginal cost curves. In a Pareto-efficient bargain, this quantity should occur regardless of the price the bargainers agree on. The price should determine only how the joint profits are split by the two parties.

In most negotiations between workers and firms, we do not observe joint decision making over the wage and employment level. This does not necessarily imply that Pareto-inefficient trades are being made, but it does at least suggest an alternative prediction for the employment level that is not Pareto efficient. To proceed with the example, suppose that the seller is given the right to unilaterally determine only the price and that the buyer may then select the quantity purchased. How will the seller determine the price to propose?

The classical solution due to Cournot and Bowley is to suppose that the seller knows the buyer's demand curve. If the seller sets price P, the seller may suppose that the buyer will maximize his own profits, which are

$$\Pi^b = (AR - P) \cdot Q. \tag{5}$$

Taking P as fixed, as the buyer is assumed to do, substituting (1) into (5), and differentiating shows that the buyer will respond by setting marginal revenue equal to price, so that

$$a - 2bQ = P. \tag{6}$$

The seller is then expected to maximize the seller's own profits subject to the reaction function (6). This leads to the seller's maximizing

$$\Pi^s = (P - AC) \cdot Q, \tag{7}$$

which, after substituting (2) and (6) and differentiating, leads to

$$Q^M = \frac{a - A}{4b + 2B}, \tag{8}$$

in which the superscript M denotes the monopoly outcome, a predicted outcome different from Q^P.

In the Fouraker et al. (1961) experiments, the average revenue and average cost curves (1) and (2) were known. It was thus possible to test (4) and (8) as alternative predictors for the quantity transacted by means of a simple comparison of predicted versus actual quantities traded. In addition, because the simple monopoly model predicts that observed price and quantity combinations will satisfy (6) and lie on the buyer's marginal revenue schedule, whereas the model of Pareto-efficient transactions does not, a test of (4) and (8) could also be based on the observed correlation of prices and quantities transacted.

Listed below in table 1, parts A and B, are summary statistics for the bargaining outcomes presented in Siegel and Fouraker (1960) and Fouraker et al. (1961). The first three columns of part A list, for the case of simultaneous bargaining over prices and quantities, average observed values (across bargaining pairs) for the ratio of transacted quantities to the Pareto-optimal quantity (Q/Q^P); the ratio of transacted quantities to the quantities that would be profit maximizing for sellers as monopolistic price setters (Q/Q^M); and the ratio of transacted quantities to those quantities that lie on buyers' demand curves at actual transacted prices, $Q/Q_b(P)$. The remaining columns of part A present results from regressions of transacted quantities on transacted prices across bargaining pairs. With Pareto-efficient contracts, these regression coefficients should all be zero, while in the simple monopoly model, the absence of price variability would leave these coefficients undefined. Failure by some sellers to charge the monopoly profit-maximizing price might still leave (6) intact, in which case the regression coefficient would be negative and equal to the slope of the buyer's demand curve. Part B of the table presents information similar to that in part A but for the case in which the seller dictates a price to the buyer, who then chooses quantity unilaterally.

Beginning with part A, it is clear that, when bargaining takes place jointly over price and quantity, the outcome very closely approximates the Pareto-optimal solution. All observed values in column 1 are essentially unity and in no case differ statistically significantly from unity. In contrast, no observed value in column 2 lies within three standard errors of unity, indicating that transacted quantities are not well predicted by the simple monopoly model. As column 3 shows, transacted quantities do not lie on the buyers' marginal revenue curve. A simple regression of quantity on price yields a slope estimate ($\beta_{q \cdot p}$) that is positive and far different from the known slope of the buyer's marginal revenue curve for these experiments.

Consider now the summary statistics presented in part B. These data describe the results of similar experiments conducted by Fouraker et al. (1961) but in which sellers acted as price leaders, unilaterally choosing a price, for which buyers then unilaterally chose a corresponding quantity. As can be seen from column 1, transacted quantities in this set of experiments no longer approximate the Pareto-efficient value. It is also apparent that the information available to the bargainers influences the outcome. In experiments 1 and

TABLE 1

CROSS-SECTION RESULTS

Experimental Group	Mean Ratio of Actual Quantities to Quantities Predicted for: Pareto-efficient Transactions Q/Q^P (1)	Monopoly Seller Transactions Q/Q^M (2)	Buyer Takes Seller's Price as Parametric $Q/Q_b(P)$ (3)	Results from Regression of Transacted Quantities on Transacted Prices: $\beta_{q \cdot p}$ (4)	R^2	Number of Pairs	Number of Bargains	Comments
	A. Simultaneous Bargaining over Price and Quantity*							
1	1.061 (.072)	1.320 (.090)	.783 (.150)	4.636 (4.324)	.577	11	...	No party has any information about opponent's profit function.
2	1.015 (.021)	1.263 (.027)	.670 (.032)	2.469 (1.421)	.188	15	...	One randomly assigned party has information about opponent's profit function. The opponent does not know that the first party has this information.
3	1.007 (.016)	1.254 (.020)	.665 (.011)	...†	...	8	...	Both parties have information about opponent's profit function. Each party knows that the other party has this information.
1–3	1.028 (.025)	1.280 (.031)	.706 (.050)	4.006 (.813)	.4317	34	...	

	B. Final Transactions: Seller as Price Leader[‡]							
1	.693 (.088)	1.039 (.057)	1.000 (.000)	−1.000 (.000)	1.000	9	18	Only the seller knows the opponent's profit function. Multiple transactions occur. Reported values are those for twentieth and twenty-first transactions pooled.
2	.643 (.019)	.965 (.028)	.984 (.021)	−1.030 (.264)	.459	20	20	Both parties have complete information about opponent's profit function. Only one transaction occurs.
3	.849 (.042)	1.274 (.003)	.963 (.038)	−.930 (.219)	.361	22	34	Both parties have complete information about opponent's profit function. Multiple transactions occur. Reported values are those for the twentieth and twenty-first transactions pooled.
1–3	.753 (.025)	1.129 (.037)	.978 (.019)	−.926 (.095)	.575	51	72	

SOURCE.—Part A is based on experimental results reported in Siegel and Fouraker (1960). Part B is based on experimental results reported in Fouraker et al. (1961).

NOTE.—Standard errors of means are in parentheses.

* For the experiments summarized here, buyers' demand curves were given by $p = 2.4 - .066Q$, and sellers' supply curves were given by $p = .2Q$. Each bargaining pair conducted only one bargaining session.

† No variation in transacted prices.

‡ For the experiments summarized here, buyers' demand curves were given by $p = 19 - Q$, and sellers' supply curves were given by $p = -11 + q$. Each bargaining pair conducted 20 transactions and then conducted one final, unexpected transaction for triple stakes. For some experiments, buyers' and sellers' joint profits could be equally split only at $Q = Q^M$. For other experiments, buyers' and sellers' joint profits could be split only at $Q = Q^P$. The effects of triple stakes and of the specific equal-split quantity were found to be minor and so are subsumed in the grouping scheme used for this table.

2, the simple monopoly seller outcome is clearly a good description of the data. The quantity transacted is about 30 percent reduced from the efficient quantity but about equal to the prediction of the seller-as-monopoly model. Likewise, the observed transaction price apparently lies on the buyer's demand schedule. It is important to recognize that experiment 1 involves multiple transactions, so considerable opportunity is provided for the parties to build the long-term relationship that might result in an efficient bargain. The bargainers apparently did not establish such a relationship, perhaps because of incomplete information, even though the results reported are for the final transactions in the round.

The results of the third experiment, which involved complete information and repeated bargaining, support neither the efficiency prediction nor the simple monopoly prediction. Quantities transacted are greater than what the simple monopoly model predicts but less than the Pareto-efficient level. Nevertheless, the price and quantity combinations observed fall along the buyer's demand curve, as indicated by the results in columns 3 and 4 of part B. (The slope of the buyer's marginal revenue curve is -1 in this experiment.)

One might reasonably argue that little weight should be placed on these experimental results, especially since the amount at stake in these bargains was typically quite small. Moreover, these bargains between two individuals may give only a poor indication of the relationship that exists between many employees and a single employer. As is always the case, it is necessary to find the parallels between the laboratory setting and the observed institutions before firm conclusions can be drawn. Nevertheless, we believe that these experiments illustrate several important points.

First, the results indicate that differences in institutional settings may have enormous effects on bargaining outcomes. This suggests the importance of investigating models in which the determinants of institutional types may be explained and of establishing empirically when different institutional types are producing different outcomes.

Second, like all good experiments, the Siegel et al. results raise questions that suggest the usefulness of further experiments. Two areas for future investigation seem especially promising. In the repeated transaction experiments, strikes (a failure to transact) were observed. Casual observation suggests that these strikes may be a part of the mechanism by which information is exchanged and bargains are struck. Further experiments that explore the role of strikes in bargaining may be useful since union-management bargaining appears to result in similar behavior.[3] Also, in the Siegel et al. experi-

[3] See Coursey (1982) for a first step in such work.

ments, the institutional framework was determined by the experimenter. It would be interesting to see how parties behave under the same variety of information conditions but where they are also required to agree on, or one party is allowed to select, the institutional framework for subsequent bargaining. It would likewise be interesting to explore the role that third parties, such as arbitrators or insurance brokers, might play in these bargaining systems.

Finally, the Siegel et al. experiments provide an example of the type of test one might apply to "field" data on wages and employment if direct observations on firms' marginal revenue products and workers' marginal utilities were available. With such data, determining whether quantity transactions are efficient would be a relatively simple matter, since tests could then be based on precise predictions for quantities traded. In practice, however, the information on demand, technology, and worker preferences required to implement such tests with nonexperimental data is unlikely ever to be available. It follows that "field" tests of the efficiency of employment determination must be based on something else. To us, the best hope appears to lie in observed correlations between wages and employment. As we have seen, the Siegel et al. findings suggest that efficient contracts lead, if anything, to negligible (or positive) correlations between price and quantity across bargaining pairs, while inefficient contracts are associated with a well-defined negative correlation between price and quantity across bargaining pairs. In the following section we explore this possibility more explicitly, with an eye toward subsequent empirical application.

III. A Framework for Inference

As a prelude to our empirical work, we begin our analysis of employment contracts with the traditional case in which unions are assumed unable to provide insurance for unemployed workers and in which third parties cannot serve this purpose either. We then consider the alterations in our conclusions that are required when these assumptions are relaxed.

A. Contracts without Worker Insurance

Consider a bargaining pair composed of an employer whose objective is to maximize profits, defined as the difference between revenue $R(L)$ and the wage bill wL:

$$\pi(w, L) = R(L) - wL, \tag{9}$$

and a union, whose objective function is

$$u(w, L).^{4} \tag{10}$$

Our starting point is the observation that a Pareto-efficient contract must equate the marginal rates of substitution between wages and employment for both the firm and the union. That is,

$$\frac{w - R_L}{L} = \frac{\partial u(\cdot)/\partial L}{\partial u(\cdot)/\partial w}, \tag{11}$$

where R_L denotes the marginal revenue product of labor.[5] Our objective is to test whether observed data on wages and employment are consistent with this condition.[6]

To develop our procedure, consider the following restatement of condition (3):

$$R_L = w(1 + \epsilon_{w \cdot L}), \tag{12}$$

where $\epsilon_{w \cdot L} = [(\partial u/\partial L)/(\partial u/\partial w)](L/w)$ denotes the elasticity of wages with respect to employment along the relevant iso-utility locus for the union. As can be seen from condition (12), the efficient contract chooses a level of employment such that the marginal revenue product of labor is equated not with the contract wage but rather with a measure of the marginal factor cost of labor, taking into account the fact that the union will accept lower wages in exchange for higher levels of employment. Because the union will generally place a positive value on higher levels of employment, $\epsilon_{w \cdot L}$ will generally be negative, implying that employment will generally exceed the level that the firm would voluntarily choose at the contract wage.[7]

[4] In what follows we assume that expressions (9) and (10) display strict concavity and twice differentiability. With regard to the firm's profit function, these assumptions are probably innocuous. With regard to the union "utility" function, however, these assumptions seem less clearly justified. Establishing the mere presence of the function $u(w, L)$ in a bargaining unit with considerable heterogeneity in worker skills and wage rates is akin to establishing the presence of a social welfare function for that unit. This problem has been addressed by many writers. The famous dispute between Dunlop (1944) and Ross (1948) may be interpreted as motivated by this and other issues. Dunlop's view that it is useful to act as if union preferences may be represented as a simple function $u(w, L)$ is most plausible under those conditions in which all the workers in the bargaining unit receive the same wage rate and face the same probability of employment. Under these circumstances there is no social choice problem, and any worker may be chosen at random to represent all other workers. We believe that the printing and construction trades, from which many of Dunlop's examples are taken, come closest to these conditions.

[5] That is, we require that w and L be selected so as to maximize $\pi(w, L) - \lambda[u(w, L) - u^0]$ for any u^0, which leads to condition (11).

[6] For a similar treatment of this question, see MaCurdy and Pencavel (this issue).

[7] The gap between the contract wage and employees' marginal value product is no doubt related to what employers describe as "featherbedding." Strong trade unions that can enforce featherbedding are often said to exist in the printing and construction

In contrast, consider now the case in which the union acts as a monopolistic price setter, with the firm then choosing a most preferred level of employment at the union's stated wage. In this case, employment will be determined according to the condition that

$$R_L = w. \tag{13}$$

In comparison with condition (12), condition (13) provides an alternative hypothesis that might allow a test for Pareto efficiency of employment contracts. Conditions (12) and (13) suggest that one might study the tendency for employment levels to equate marginal revenue products either with (*a*) the contract wage or with (*b*) the marginal factor cost of labor (where the latter is defined in condition [12]), in much the same way that Fouraker et al. studied the tendency for transacted quantities to approach either the Pareto-optimal value or that value implied by a model of monopolistic price setting.[8]

Since we have no direct information about firms' profit functions and unions' utility functions, however, we are unable to make any direct comparison of actual and optimal employment levels. Instead, we must focus our attention on the different implications of conditions (12) and (13) for the correlations between employment levels, contract wages, and alternative wages. In doing so, we are forced to adopt some specific functional forms, and, not surprisingly, the manner in which we interpret the data depends heavily on these assumptions.

To elaborate, suppose that the union utility function is such that $\epsilon_{w \cdot L}$ is a constant. Suppose also that workers' marginal revenue products are given by

$$\log(R_L) = \alpha_0 + \alpha_1 \mathbf{X} - \alpha_2 \log(L), \tag{14}$$

where $\mathbf{X}$ indicates other (unspecified) variables. In this case, conditions (12) and (13) imply

$$\log(L) = \left[\frac{\alpha_0 - \log(1 + \epsilon_{w \cdot L})}{\alpha_2}\right] + \left(\frac{\alpha_1}{\alpha_2}\right)\mathbf{X} - \left(\frac{1}{\alpha_2}\right)\log(w), \tag{15}$$

$$\log(L) = \left(\frac{\alpha_0}{\alpha_2}\right) + \left(\frac{\alpha_1}{\alpha_2}\right)\mathbf{X} - \left(\frac{1}{\alpha_2}\right)\log(w). \tag{16}$$

It is clear from conditions (15) and (16) that, as long as α_0 is unknown and $\epsilon_{w \cdot L}$ is a constant, data on wages and employment cannot be used to distinguish between the implications of conditions (12) and (13).

trades and in the railroad and entertainment industries. These industries therefore seem like especially interesting candidates for testing efficiency in employment determination.

[8] This approach is also taken by MaCurdy and Pencavel (1984).

Clearly, more structure must be imposed before any test based on conditions such as (15) and (16) will be possible.

One method of providing such structure is to specify the union's utility function in a way that $\epsilon_{w \cdot L}$ varies with observable variables that do not enter firms' profit functions directly. The most obvious candidate variables for this role at an intuitive level are alternative wages and employment opportunities for disemployed union members. If we assume that these variables influence $\epsilon_{w \cdot L}$, equation (15) will then include terms that are excluded from equation (16). In this case, a test based on exclusion restrictions is possible.

If, for example, union preferences over wages and employment are given by the Stone-Geary function

$$u(w, L) = k(w - \bar{w})^{\beta} L^{1-\beta}, \tag{17}$$

where $\bar{w}$ denotes some minimum acceptable contract wage, perhaps equal to union members' alternative wage, condition (11) becomes

$$\frac{w - R_L}{L} = \left(\frac{1 - \beta}{\beta}\right)\left(\frac{w - \bar{w}}{L}\right). \tag{18}$$

Equations (14) and (18) in turn imply an analogue to (15) given by

$$\log(L) = \left(\frac{\alpha_0}{\alpha_2}\right) + \left(\frac{\alpha_1}{\alpha_2}\right)\mathbf{X} - \left(\frac{1}{\alpha_2}\right)\log[\gamma\bar{w} + (1 - \gamma)w], \tag{19}$$

where $\gamma = (1 - \beta)/\beta$. Notice that, unlike equation (16), equation (19) includes the alternative wage, $\bar{w}$.

Equation (19) provides a useful framework within which to test the hypotheses of strong efficiency and weak efficiency in employment determination. To illustrate, consider the case where γ is found to be unity in the data. In this case, the alternative wage acts as the sole determinant of employment (given $\mathbf{X}$), with no influence from the contract wage. Given such a finding, we would conclude that the data are consistent with strong efficiency in employment determination. Alternatively, if γ were found to be less than unity but positive, both the contract wage and the alternative wage would act as determinants of employment. In this case, we would conclude that the data are consistent with weak efficiency in employment determination. Finally, if γ were found to be zero in the data, the contract wage would act as a sufficient statistic (given $\mathbf{X}$) in determining employment, with no influence from the alternative wage. In this case, we would conclude that a model of monopolistic price setting by the union, with subsequent unilateral employment determination by the employer, is the most appropriate model for the data.

Because we can never know the exact forms of firm's production functions and unions' utility functions, we can never be sure that the data we study reflect the optimal correlation of wages and employment implied by those particular functions. Nevertheless, except in the extreme case in which unions place no value on employment, we should find at least some negative employment effect of alternative wages. Thus equation (19) provides one basic implication that might be tested and on which normative conclusions might be based: namely, that estimates of γ from (19) should be strictly positive. If we maintain the assumption that unions place some positive value on employment, we can argue that Pareto efficiency requires at least weak efficiency in employment determination. Thus, if we fail to find at least weak efficiency in employment determination, we can reject the null hypothesis of Pareto-efficient employment determination.

At this stage we have a usable and yet still fairly general framework within which to interpret the data. This framework focuses on the employment effects of alternative wages (and other determinants of unions' marginal rate of substitution between wages and employment) in testing for Pareto efficiency in employment determination. This framework has no testable implications, however, regarding the employment effect of contract wages. It is worth asking, therefore, what assumptions are required in order to base inferences on the estimated effects of contract wages as well as alternative wages in tests of Pareto efficiency in employment determination.

To develop such a framework, we take the union's objective function to be the expected utility of the "typical" union member, given by

$$u(w, L) = \left(\frac{L}{\overline{L}}\right)g(w) + \left[1 - \left(\frac{L}{\overline{L}}\right)\right]g(\overline{w}), \tag{20}$$

where L denotes the number of union members actually employed by the firm at a wage of w, $\overline{L}$ denotes the total number of union members, $\overline{L} - L$ of whom are disemployed and must work elsewhere at a wage of $\overline{w}$, and $g(\cdot)$ denotes a twice-differentiable, concave function relating worker's utility to realized earnings.[9] When (20) is used in place of the more general functions given by (10) or (17), condition (12) becomes

$$R_L = w + \left[\frac{g(\overline{w}) - g(w)}{g'(w)}\right].^{10} \tag{21}$$

Taking a second-order Taylor series, expressions (14) and (21) imply an employment equation given by

[9] For simplicity, we assume that hours of work do not vary across jobs.

[10] This is the condition used throughout by McDonald and Solow (1981) in their analysis of efficient employment contracts.

$$\log(L) \approx \left(\frac{\alpha_0}{\alpha_2}\right) + \left(\frac{\alpha_1}{\alpha_2}\right)\mathbf{X} - \left(\frac{1}{\alpha_2}\right)\log(\overline{w}) + \frac{\rho}{2\alpha_2}[\log(w) - \log(\overline{w})]^2, \tag{22}$$

where ρ denotes the Arrow-Pratt measure of relative risk aversion, $-wg''(w)/g'(w)$.

In comparison with equation (19), equation (22) provides a somewhat less general but potentially more powerful framework within which to test the null hypothesis of Pareto-efficient employment determination. Like equation (19), equation (23) implies a negative employment effect for alternative wages. Unlike equation (19), however, equation (22) also provides implications regarding the Pareto-efficient employment effect of contract wages. In particular, it can be seen that equation (22) involves no first-order term in $\log(w)$. Within this framework, the contract wage influences employment only through higher-order terms in the logarithmic difference between w and $\overline{w}$. Thus, within this framework, one might base a test of Pareto-efficient employment determination on the absence of any first-order employment effect of the contract wage, in addition to the previously required negative employment effect of the alternative wage.

Further restrictions on the form of the union utility function suggest an even more powerful test. In the special case where $g(w)$ is linear, so that ρ is equal to zero, equation (22) reduces to

$$\log(L) = \left(\frac{\alpha_0}{\alpha_2}\right) + \left(\frac{\alpha_1}{\alpha_2}\right)\mathbf{X} - \left(\frac{1}{\alpha_2}\right)\log(\overline{w}), \tag{23}$$

which is just the strong efficiency hypothesis. Thus, if we maintain the assumption that workers are risk neutral, we can then argue that Pareto efficiency requires what we have called strong efficiency in employment determination. Granted, it is unlikely that workers are risk neutral, and so it may seem that rejection of Pareto efficiency on the basis of (23) is not very significant. In the next section, however, we provide an argument that suggests a more general justification and interpretation of this test.

B. *Contracts with Uncertainty and Insurance*

Much of the literature on "implicit contracts" explores the extent to which uncertainty and imperfect information alter the form of efficient contracts in circumstances of bilateral monopoly. One important prediction from such models is the increased likelihood that employment contracts will be *strongly efficient.* It is instructive to see why this is the case because it shows how providing greater institutional flexibility can change the predictions of the simplest bilateral monopoly models.[11]

To be precise, suppose again that the union's objective function takes the expected utility form in (20). Suppose further that we allow the union to tax employed workers an amount $\tilde{w}$ in order to make payments of $\hat{w}$ to disemployed workers. Financing this unemployment insurance scheme requires that $\tilde{w}L = \hat{w}(\bar{L} - L)$, and maximization of (20) with respect to $\tilde{w}$ and $\hat{w}$ subject to this constraint leads to the first-order conditions

$$g'(\overline{w} + \hat{w}) - \left(\frac{L}{\bar{L}}\right)g'(\overline{w} + \hat{w}) = \lambda(\bar{L} - L), \tag{i}$$

$$-\left(\frac{L}{\bar{L}}\right)g'(w - \tilde{w}) = \lambda L. \tag{ii}$$

Eliminating λ by taking the ratio of these two equations establishes that $g(\overline{w} + \hat{w}) = g(w - \tilde{w})$, which implies that union members are fully insured. Moreover, since $\overline{w} + \hat{w} = w - \tilde{w}$, it follows that each worker's utility is $g(\overline{w} + \hat{w}) = g\{(L/\bar{L})w + [1 - (L/\bar{L})]\overline{w}\}$, which by condition (21) implies that $R_L = \overline{w}$, the *strong efficiency hypothesis*. Thus there is some basis, beyond arbitrary functional form restrictions, for expecting employment contracts to display strong efficiency.

The presence of worker risk aversion in a model of bilateral monopoly should lead to worker demands for income insurance. If this insurance is complete, workers will be indifferent to the risk of disemployment, and observed wage-employment combinations will be consistent with the strong efficiency hypothesis. Therefore, if one maintains a utility function for workers of the form (20), it follows that rejection of the strong efficiency hypothesis in the presence of Pareto efficiency requires more than just the absence of risk neutrality. Such a rejection requires also that there exist some human or technical obstacle to the contractual provision of income insurance.

Finally, it is worth considering how these conclusions may be modified if workers are unable to observe R_L, the value of their work to the firm. It is tempting to conjecture that this might lead the parties to a contract that contains some elements of both the efficient contract and monopoly price-setting models, but as far as we are aware, this has not been established in any formal model. This may serve as a reminder that there is still considerable research necessary before the relationship between the various models and their empirical implications is fully established. Perhaps this also implies that our empirical approach should at this stage proceed without forcing unnecessary structure onto our data analysis.

[11] We are indebted to Oliver Hart for suggesting the following argument.

IV. Empirical Implementation

Our empirical work makes use of data on membership and contract wages for 10 locals belonging to the International Typographical Union (ITU) for the period 1948–65.[12] The choice of this union was motivated by several practical considerations. First, the union itself publishes data on membership and wages for each of its member locals, and related data on output, product prices, and factor prices for the newspapers employing ITU members are also readily available. Second, the ITU is remarkably democratic in its operation and decision making, while the nature of members' jobs is fairly homogeneous across locals. In addition, although the ITU sets some bargaining guidelines at the national level, individual locals bargain independently with regard to wages, hours, and corresponding levels of local employment. As a result, the ITU data appear to provide an opportunity for measuring the employment effects of contract wages and alternative wages in a setting in which unobservable job-specific and person-specific characteristics do not vary much across locals but in which wages and employment do vary across locals. Third, newspaper industry disputes over employment levels are both commonplace and long-standing. This suggests that the newspaper industry may be a good candidate for testing the efficiency of employment determination since the presence of joint bargaining over wages and employment may indicate some separation between the distributional and allocative effects of contracted wages.

As is typically the case with empirical work, however, our data fail to perfectly match the theoretical constructs we have emphasized in equations such as (19) and (22). We have thus been required to maintain a number of assumptions in our interpretation of the data. The most important of these assumptions concern the measurement of employment, alternative wages, and demand-related variables in our analysis. These assumptions are discussed in detail in Appendix B.

On the basis of the many considerations listed in Appendix B, we have used several alternative specifications in our tests for employment efficiency. With regard to employment, we have used four alternative measures. These are (i) the number of journeyman printers who are members of a union local; (ii) the number of "active" members of a union local, defined as the number of journeyman members plus the number of apprentices minus the number of retired journey-

[12] The time period 1948–65 was chosen to avoid complications arising from technological innovations that occurred in the printing industry after 1965. Summary statistics and definitions of variables are presented in App. A. We are indebted to John Pencavel for providing us with a substantial portion of these data. For previous analyses based on these data, see Dertouzos and Pencavel (1981) and Pencavel (1984).

man members; (iii) an imputed measure of total local hours, $\hat{h}$, defined as the ratio of local assessments to the product of the local wage and the aggregate ITU assessment rate; and (iv) local assessments.

With regard to alternative wages, we have used 11 different measures of the alternative wage relevant to ITU printers. These are (i) the real average hourly earnings of production workers in all U.S. manufacturing industries; (ii) the real average hourly earnings of production workers in all manufacturing industries in the local's census region; (iii) the real average hourly earnings of production workers in all manufacturing industries in the local's state; (iv) the real average hourly earnings of production workers in all U.S. printing and publishing industries; (v) the real average hourly earnings of production workers in all printing and publishing industries in the local's census region; (vi) the real average hourly earnings of production workers in all printing and publishing industries in the local's state; (vii) the real average weekly contract wage for ITU journeyman printers in all U.S. locals; (viii) the real average weekly contract wage for ITU journeyman printers in the local's state; (ix) the real average hourly contract wage for ITU journeyman printers in an "adjacent" local;[13] (x) the real average hourly earnings of nonsupervisory workers in all U.S. retail trade establishments; and (xi) the real average hourly earnings of production workers in all U.S. durable goods manufacturing industries.

A. *Instrumental Variables Estimates*

For each measure of employment and for each measure of the alternative wage, we first estimated the Stone-Geary specification (19) and the expected utility of employment specification (22). In these specifications, the vector **X** included local-specific fixed effects, local-specific linear time trends, a common quadratic time trend, the logarithm of the average real advertising rate for the newspaper(s) employing local printers, the logarithm of the total advertising linage for the newspaper(s) employing local printers, and, as discussed in Appendix B, the lagged value of the dependent variable.[14] Finally,

[13] For this purpose, locals were matched according to geographical proximity. The exact pairings are given in App. A.

[14] In addition, we also included a dummy variable to account for newspaper mergers and dummy variables for two observations that we think reflect strike activity (Memphis, Tenn., 1952, and Augusta, Ga., 1965). Also, for the equations using assessments as the dependent variable, current and lagged values of the contract wage and of the aggregate ITU assessment rate were initially included in the vector **X**, along with lagged values of assessments. It was readily apparent, however, that these three lagged variables were duplicating the effect of lagged imputed hours since the estimated

following our discussion in Appendix B, we have used current and lagged values of the consumer price index and lagged values of the contract wage as instruments for the current contract wage.

Our results from estimating equations (19) and (22) were uniformly poor. In the case of (19), the parameter γ was repeatedly found to be negative and large, while, in the case of (22), the estimated coefficient for the squared term in $[\log(w) - \log(\bar{w})]$ was typically much larger than one in absolute value and frequently of the wrong sign to support the hypothesis of risk aversion. In addition, the standard errors for these estimates were quite large.

At this stage, it was apparent to us that the framework provided by equations (19) and (22) would not allow us to study the data in an informative way. We therefore chose to focus instead on a first-order approximation to (19) and (22) given by

$$\log(L) \approx \left(\frac{\alpha_0}{\alpha_2}\right) + \left(\frac{\alpha_1}{\alpha_2}\right)\mathbf{X} - \left(\frac{\gamma}{\alpha_2}\right)\log(\bar{w}) - \left(\frac{1-\gamma}{\alpha_2}\right)\log(w). \quad (24)$$

Equation (24) can also be interpreted as a general nesting equation for the two models given by (16) and (23), where the parameter γ measures the relative weight the data give to each model.

Listed in table 2, parts A–D, are the results of fitting equation (24) to our pooled data from 10 ITU locals. Part A presents estimated equations for which the dependent variable is the (log) total number of journeyman printers. Eleven equations are presented, one for each measure of the alternative wage. Parts B–D present similar equations for which the dependent variables are (log) "active" membership, imputed hours, and assessments, respectively.[15]

Beginning with part A, we find no consistent evidence that alternative wages negatively influence employment (as measured by journeyman membership). With the one (marginal) exception of row 9, the

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coefficients for lagged wages and assessment rates were approximately equal and both approximately equal to the negative of the estimated coefficient for lagged assessments. Given this fact, to improve the precision of the instrumental estimates, we substituted lagged imputed hours in place of lagged assessments, wages, and assessment rates in the assessment equations.

[15] The columns headed $\hat{\rho}$ in these tables present estimated coefficients from supplementary equations based on the residuals taken from our employment equations. For each employment equation we retrieved the residuals ($\hat{\epsilon}_t$) and then estimated the equation

$$\hat{\epsilon}_t = \delta_0 + \delta_1 X_t + \delta_2 \log(\bar{w}_t) + \delta_3 \log(w_t) + \delta_4 \log(L_{t-1}) + \rho\hat{\epsilon}_{t-1} + v_t. \quad (*)$$

We use our estimates of ρ from this supplementary equation as a basis for a test of first-order autocorrelation in our error terms. A more straightforward approach would be to use the Dubin h-statistic, but for our data the h-statistic is typically not defined since its computation requires us to take the square root of a negative number. Finally, we should mention that there is no evidence of any serial correlation among the v_t from eq. (*).

estimated coefficient for the alternative wage is never statistically significantly negative. We also find, however, no clear evidence that contract wages influence employment. Indeed, in every equation presented in part A, the estimated effect of contract wages on employment is positive rather than negative. Although this finding is consistent with efficient employment determination (with Stone-Geary utility, e.g., γ will exceed unity and employment will be positively related to contract wages whenever β is less than .5) and is reminiscent of the findings of Siegel and Fouraker (1960), we suspect this finding derives as much from sampling error as from anything else. Apparently, the instruments we have at our disposal will not allow us the precision with which any clear conclusions might be drawn from these data.

On the basis of the alternative measure of employment that excludes retired journeymen but includes apprentices, there is perhaps a bit more evidence of efficiency in part B than in part A. The difference is very small, however. The estimated coefficients for alternative wages are more uniformly negative in part B than in part A, while the estimated coefficients for contract wages remain predominantly positive. Unfortunately, however, as was true for part A, the coefficient estimates in part B are too imprecise to allow any substantive conclusions.

Consider now the estimates presented in part C, which are based on earnings-related data rather than membership counts (as discussed in App. B, we take local-specific assessments to be a measure of local-specific earnings). In contrast to the estimates presented in parts A and B, these estimates are more consistent with monopolistic wage setting by the union and unilateral employment determination by the firm than with joint employment determination. It is now the alternative wage that frequently has a positive estimated coefficient, while the contract wage effect is consistently negative although generally statistically insignificant.

Part D of table 2 presents results very similar to those in part C. In this case, since the dependent variable is the logarithm of assessments, the effect of contract wages on employment is measured by the difference between the estimated contract wage coefficient and unity. As was true for part C, the implied contract wage effect in part D is typically negative but not significantly different from zero. The estimated employment effect of alternative wages is never significantly negative and frequently of the wrong sign, as was true in part C.

Given the inconclusive results presented in table 2, we have reconsidered our measurement of alternative wages. In particular, we have relaxed our implicit assumption that disemployed union members can find certain and instantaneous employment at the alternative

TABLE 2

INSTRUMENTAL VARIABLE ESTIMATES

A. $\log(J_t) = a_0 + a_1 X_t + a_2 \log(\overline{w}_t) + a_3 \log(w_t) + a_4 \log(J_{t-1})$;
DEPENDENT VARIABLE = log(Journeyman Members)

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\overline{w}_t)$	$\log(w_t)$	$\log(J_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\overline{w}$
1	−.430 (.451)	.456 (.365)	.361 (.086)	.999	.058 (.197)	137	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.273 (.281)	.108 (.179)	.392 (.085)	.999	.128 (.212)	123	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	.039 (.212)	.067 (.211)	.372 (.083)	.999	.101 (.206)	126	State average for real hourly earnings of production workers in all manufacturing industries.
4	−.103 (.163)	.233 (.198)	.377 (.081)	.999	.054 (.194)	137	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	.007 (.100)	.167 (.185)	.400 (.086)	.999	.080 (.219)	123	Regional average for hourly earnings of production workers in all printing and publishing.
6	.008 (.088)	.039 (.185)	.431 (.097)	.999	.055 (.258)	98	State average for real hourly earnings of production workers in all printing and publishing.
7	−.590 (.436)	.757 (.473)	.335 (.094)	.999	.090 (.195)	137	National average for weekly real contract wage of ITU journeyman printers.
8	−.019 (.256)	.222 (.314)	.381 (.081)	.999	.117 (.194)	137	State average for weekly real contract wage of ITU journeyman printers.
9	−.223 (.144)	.394 (.226)	.370 (.082)	.999	.165 (.195)	137	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	−.309 (.494)	.380 (.359)	.367 (.085)	.999	−.009 (.196)	137	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.539 (.446)	.435 (.275)	.371 (.083)	.999	.143 (.194)	137	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

B. $\log(M_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(M_{t-1})$;
DEPENDENT VARIABLE = log("Active" Membership)

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\bar{w}_t)$	$\log(w_t)$	$\log(M_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\bar{w}$
1	−.366 (.485)	.377 (.402)	.418 (.087)	.999	.150 (.198)	7	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	−.024 (.306)	.132 (.201)	.433 (.091)	.999	.089 (.207)	123	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	−.180 (.226)	.170 (.234)	.383 (.087)	.999	.170 (.212)	126	State average for real hourly earnings of production workers in all manufacturing industries.
4	−.189 (.174)	.266 (.216)	.411 (.083)	.999	.162 (.196)	137	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	−.044 (.106)	.132 (.203)	.428 (.090)	.999	.104 (.208)	123	Regional average for hourly earnings of production workers in all printing and publishing.
6	−.018 (.096)	−.042 (.209)	.411 (.101)	.999	−.051 (.241)	98	State average for real hourly earnings of production workers in all printing and publishing.
7	−.203 (.431)	.369 (.478)	.420 (.089)	.999	.149 (.194)	137	National average for weekly real contract wage of ITU journeyman printers.
8	.019 (.293)	.157 (.366)	.405 (.090)	.999	.154 (.194)	137	State average for weekly real contract wage of ITU journeyman printers.
9	−.101 (.155)	.264 (.248)	.415 (.084)	.999	.224 (.195)	137	Real hourly contract wage for ITU journeyman printers in an adjacent local.
10	.104 (.526)	.093 (.392)	.400 (.086)	.999	.155 (.197)	137	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.551 (.496)	.426 (.314)	.432 (.088)	.999	.195 (.189)	137	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

TABLE 2 (*Continued*)

C. $\log(H_t) = a_0 + a_1X_t + a_2\log(\overline{w}_t) + a_3\log(w_t) + a_4\log(H_{t-1})$;
DEPENDENT VARIABLE = log[Assessments/(Wages · ITU Assessment Rate)]

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\overline{w}_t)$	$\log(w_t)$	$\log(H_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\overline{w}$
1	.865 (.484)	−.686 (.394)	.345 (.064)	.999	.090 (.149)	137	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.223 (.332)	−.170 (.207)	.393 (.079)	.999	−.213 (.206)	123	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	−.135 (.250)	−.091 (.253)	.339 (.072)	.999	.059 (.161)	126	State average for real hourly earnings of production workers in all manufacturing industries.
4	.053 (.200)	−.078 (.242)	.369 (.071)	.999	.103 (.151)	137	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	−.046 (.117)	−.140 (.214)	.405 (.079)	.999	−.265 (.214)	123	Regional average for hourly earnings of production workers in all printing and publishing.
6	.203 (.101)	−.503 (.212)	.448 (.090)	.999	−.318 (.209)	98	State average for real hourly earnings of production workers in all printing and publishing.
7	.825 (.428)	−.836 (.467)	.340 (.063)	.999	.148 (.152)	137	National average for weekly real contract wage of ITU journeyman printers.
8	.286 (.321)	−.326 (.391)	.348 (.072)	.999	.108 (.153)	137	State average for weekly real contract wage of ITU journeyman printers.
9	.064 (.173)	−.073 (.272)	.371 (.070)	.999	.108 (.153)	137	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	.250 (.588)	−.191 (.430)	.365 (.070)	.999	.052 (.151)	137	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	.006 (.546)	−.030 (.338)	.373 (.072)	.999	.106 (.151)	137	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

D. $\log(A_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(\theta_t) + a_5 \log(H_{t-1})$;
DEPENDENT VARIABLE = log(Assessments)

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\bar{w}_t)$	$\log(w_t)$	$\log(\theta_t)$	$\log(H_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\bar{w}$
1	.659 (.563)	.447 (.435)	1.013 (.019)	.356 (.066)	.999	.046 (.126)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.215 (.341)	.822 (.207)	.992 (.049)	.393 (.079)	.999	−.252 (.169)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	−.159 (.248)	.861 (.251)	1.023 (.021)	.342 (.071)	.999	−.138 (.132)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	.036 (.198)	.894 (.240)	1.022 (.018)	.376 (.071)	.999	−.057 (.129)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	−.046 (.117)	.857 (.215)	.994 (.049)	.406 (.079)	.999	−.291 (.166)	122	Regional average for hourly earnings of production workers in all printing and publishing.
6	.204 (.101)	.451 (.212)	.987 (.048)	.447 (.091)	.999	−.280 (.191)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	.812 (.415)	.141 (.452)	1.022 (.016)	.347 (.063)	.999	.093 (.126)	136	National average for weekly real contract wage of ITU journeyman printers.
8	.263 (.317)	.664 (.384)	1.023 (.017)	.356 (.071)	.999	−.002 (.130)	136	State average for weekly real contract wage of ITU journeyman printers.
9	.066 (.170)	.880 (.269)	1.020 (.018)	.376 (.069)	.999	.059 (.124)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	−.025 (.648)	.951 (.459)	1.024 (.020)	.381 (.074)	.999	.047 (.127)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.198 (.575)	1.027 (.342)	1.022 (.020)	.384 (.073)	.999	.065 (.126)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

NOTE.—Source and definition of variables are in App. A.

wage. This was done by including as a regressor in our previous equations the logarithm of one minus the relevant state-insured unemployment rate. In the context of a monopoly model of union-firm interaction, this variable should, like alternative wages, have no influence on employment except insofar as it influences the contract wage. Thus, if the monopoly union model is appropriate, this unemployment variable should have an estimated coefficient of zero. Alternatively, if an efficient contracting model of employment determination is appropriate, this variable, like alternative wages, should have a negative effect on local employment of ITU printers.[16]

The results of estimating these augmented equations are presented in table 3, parts A–D. As can be seen from this table, the basic findings of table 2 remain. In particular, on the basis of membership data (table 2, pts. A–B), alternative wages typically have a negative but statistically insignificant effect on employment, while contract wages typically have a positive (also insignificant) effect on employment. On the basis of earnings-related data, however, these findings are reversed. In table 3, parts C and D, the estimated coefficients for alternative wages are frequently positive, while estimated contract wage effects are always negative (but generally statistically insignificant).

In contrast to the absence of any systematic wage effects in these data, consider now the estimated coefficients for alternative employment probabilities. These estimated coefficients are negative in every row of table 3. They are relatively small (and statistically insignificant) in parts A and B, but they are much larger (and frequently statistically significant) in parts C and D. These estimated coefficients are consistent with efficiency in employment determination, for they indicate that, as alternative employment opportunities (and wages expected over states of unemployment and employment) improve, employment at ITU locals is reduced. Only if the demand for printers' services were countercyclical would these estimated effects of alternative employment probabilities be consistent with a monopolistic model of union-firm interaction. Thus, in table 3, parts C and D, the data provide at least some limited evidence of (weak) efficiency in employment determination.

[16] This statement abstracts, of course, from any other sources of correlation between statewide unemployment rates and the error term in our employment equations. It is worth noting that if alternative employment status were a binomial variable with probability $1 - U$ and if workers were risk neutral, the appropriate alternative wage measure to include in our regressions would be the (logarithm of the) product $\bar{w}(1 - U)$. A simple test of this specification could then be based on the difference between the separate coefficients estimated for $\log(\bar{w})$ and $\log(1 - U)$. As will be seen, the estimated coefficients for $\log(\bar{w})$ and $\log(1 - U)$ are nowhere near equal.

At this point it is useful to investigate more closely the characteristics of our data that have generated the instrumental estimates presented in tables 2 and 3. As it turns out, there are a few simple facts in the data that appear to be responsible for our findings. In particular, on inspection of the reduced-form equations underlying tables 2 and 3, we repeatedly find that the three variables used as instruments for the contract wage (current and lagged consumer prices and the lagged contract wage) all have large and statistically significant reduced-form effects on wages but have no statistically significant reduced-form effects on employment. Thus another (and perhaps more straightforward) way to describe the findings of tables 2 and 3 is to say that lagged wages and prices have large positive effects on current (real) wages and that current prices have large negative effects on current (real) wages, but that none of these variables has any statistically discernible effect on current employment. Given this fact and given that these three variables are the only instruments we use, it is not surprising that our estimated structural equations show no statistically discernible employment effect of contract wages.

What can we infer from tables 2 and 3? Because membership-based data cannot capture variation in hours per employed member or employed members per total membership, we are inclined to place greater weight on estimates derived from earnings-related data. We are encouraged, moreover, that the estimated contract wage effects based on imputed hours (discussed in App. B) are so similar to those based directly on assessments. These considerations, along with the apparent statistical importance of alternative employment probabilities, lead us to emphasize the results presented in table 3, parts C and D. On the basis of these parts of table 3, we find some support for at least weak efficiency in employment determination. The data suggest that contracts do take workers' alternatives into account in determining employment. The data are less informative, however, as to the employment effects of contract wages and alternative wages.

B. Ordinary Least Squares Estimates

The conclusions that we draw from tables 2 and 3 derive in large part from the fact that lagged prices and wages influence current wages but do not influence current employment. It is a large step to go from this finding to a statement about the efficiency of employment contracts, and we cannot be certain that this step is justified. It may be instructive, therefore, to compare our instrumental variables estimates with ordinary least squares (OLS) estimates of the same equations.

Presented in table 4, parts A–D, are OLS estimates analogous to the

TABLE 3

INSTRUMENTAL VARIABLE ESTIMATES

A. $\log(J_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(J_{t-1})$;
DEPENDENT VARIABLE = log(Journeyman Members)

Equation Number	Estimated Coefficient for $\log(\bar{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(J_{t-1})$	R^2	$\hat{\rho}$	df	Definition of $\bar{w}$
1	−.471 (.445)	.460 (.357)	−.140 (.236)	.356 (.086)	.999	−.003 (.203)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.287 (.283)	.089 (.182)	−.122 (.276)	.390 (.085)	.999	.174 (.220)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	.038 (.213)	.064 (.216)	−.031 (.264)	.371 (.084)	.999	.116 (.217)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	−.106 (.163)	.216 (.203)	−.115 (.229)	.374 (.081)	.999	.087 (.201)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	.004 (.101)	.158 (.186)	−.087 (.279)	.399 (.086)	.999	.117 (.228)	122	Regional average for hourly earnings of production workers in all printing and publishing.
6	.004 (.088)	.023 (.186)	−.203 (.287)	.428 (.097)	.999	.136 (.263)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	−.640 (.442)	.776 (.476)	−.159 (.252)	.328 (.095)	.999	−.045 (.199)	136	National average for weekly real contract wage of ITU journeyman printers.
8	−.043 (.262)	.225 (.316)	−.109 (.234)	.378 (.081)	.999	.090 (.201)	136	State average for weekly real contract wage of ITU journeyman printers.
9	−.226 (.144)	.374 (.230)	−.123 (.232)	.367 (.082)	.999	.132 (.201)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	−.344 (.494)	.386 (.361)	−.110 (.234)	.363 (.086)	.999	.077 (.200)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.530 (.455)	.421 (.290)	−.056 (.239)	.370 (.083)	.999	.052 (.196)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

B. $\log(M_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(M_{t-1})$;
DEPENDENT VARIABLE = log("Active" Membership)

Equation Number	Estimated Coefficient for $\log(\bar{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(M_{t-1})$	R^2	$\hat{\rho}$	df	Definition of $\bar{w}$
1	−.424 (.469)	.353 (.389)	−.320 (.251)	.407 (.087)	.999	.206 (.198)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.018 (.307)	.078 (.204)	−.344 (.298)	.419 (.091)	.999	.148 (.210)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	−.181 (.226)	.143 (.239)	−.155 (.281)	.379 (.087)	.999	.229 (.217)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	−.194 (.172)	.211 (.223)	−.297 (.247)	.399 (.083)	.999	.202 (.193)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	−.061 (.107)	.093 (.204)	−.371 (.299)	.416 (.090)	.999	.133 (.210)	122	Regional average for hourly earnings of production workers in all printing and publishing.
6	−.026 (.095)	−.083 (.210)	−.422 (.313)	.392 (.101)	.999	−.010 (.237)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	−.282 (.429)	.384 (.477)	−.302 (.254)	.412 (.080)	.999	.201 (.198)	36	National average for weekly real contract wage of ITU journeyman printers.
8	−.045 (.296)	.166 (.366)	−.293 (.249)	.401 (.090)	.999	.233 (.198)	136	State average for weekly real contract wage of ITU journeyman printers.
9	−.106 (.153)	.206 (.254)	−.295 (.248)	.402 (.084)	.999	.261 (.200)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	.052 (.521)	.073 (.394)	−.293 (.245)	.391 (.087)	.999	.247 (.200)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.461 (.503)	.330 (.331)	−.241 (.259)	.417 (.089)	.999	.214 (.199)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

TABLE 3 (*Continued*)

C. $\log(H_t) = a_0 + a_1X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(H_{t-1})$;
DEPENDENT VARIABLE = log[Assessments/(Wages · Aggregate ITU Assessment Rate)]

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\bar{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(H_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\bar{w}$
1	.631 (.466)	−.612 (.379)	−.613 (.243)	.338 (.063)	.999	.029 (.149)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.309 (.327)	−.259 (.205)	−.560 (.323)	.359 (.080)	.999	−.181 (.206)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	−.138 (.246)	−.149 (.255)	−.365 (.306)	.336 (.070)	.999	.044 (.160)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	.057 (.191)	−.204 (.239)	−.626 (.265)	.352 (.069)	.999	.054 (.147)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	−.063 (.115)	−.203 (.211)	−.545 (.326)	.377 (.079)	.999	−.261 (.214)	122	Regional average for hourly earnings of production workers in all printing and publishing.
6	.199 (.099)	−.601 (.211)	−.609 (.332)	.400 (.092)	.999	−.330 (.208)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	.679 (.420)	−.809 (.458)	−.589 (.237)	.331 (.062)	.999	.037 (.151)	136	National average for weekly real contract wage of ITU journeyman printers.
8	.162 (.320)	−.318 (.386)	−.597 (.260)	.344 (.071)	.999	.049 (.156)	136	State average for weekly real contract wage of ITU journeyman printers.
9	.056 (.165)	−.195 (.267)	−.613 (.265)	.356 (.067)	.999	.067 (.148)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	.196 (.564)	−.274 (.418)	−.620 (.260)	.351 (.068)	.999	.065 (.150)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	.244 (.579)	−.277 (.344)	−.648 (.268)	.351 (.067)	.999	.067 (.149)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

D. $\log(A_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(\theta_t) + a_6 \log(H_{t-1})$;
Dependent Variable = log(Assessments)

Equation Number	Estimated Coefficient for $\log(\bar{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(\theta_t)$	$\log(H_{t-1})$	R^2	$\hat{\rho}$	df	Definition of $\bar{w}$
1	.737	.315	−.667	.992	.331	.999	.066	135	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
	(.531)	(.418)	(.275)	(.020)	(.065)		(.132)		
2	.303	.743	−.599	.997	.359	.999	−.204	121	Regional average for real hourly earnings of production workers in all manufacturing industries.
	(.337)	(.205)	(.325)	(.048)	(.080)		(.168)		
3	−.152	.833	−.285	1.014	.338	.999	−.143	124	State average for real hourly earnings of production workers in all manufacturing industries.
	(.246)	(.253)	(.332)	(.023)	(.070)		(.134)		
4	.055	.795	−.601	1.004	.354	.999	−.023	135	National average for real hourly earnings of production workers in all U.S. printing and publishing.
	(.192)	(.239)	(.295)	(.020)	(.069)		(.129)		
5	−.064	.800	−.544	.995	.378	.999	−.273	121	Regional average for hourly earnings of production workers in all printing and publishing.
	(.116)	(.212)	(.328)	(.048)	(.080)		(.166)		
6	.199	.397	−.609	.988	.401	.999	−.244	96	State average for real hourly earnings of production workers in all printing and publishing.
	(.099)	(.211)	(.332)	(.047)	(.093)		(.192)		
7	.671	.201	−.548	1.006	.334	.999	.075	135	National average for weekly real contract wage of ITU journeyman printers.
	(.412)	(.447)	(.265)	(.018)	(.063)		(.134)		
8	.168	.674	−.555	1.006	.346	.999	−.039	135	State average for weekly real contract wage of ITU journeyman printers.
	(.317)	(.381)	(.290)	(.019)	(.071)		(.130)		
9	.059	.797	−.600	1.002	.357	.999	−.017	135	Real hourly contract wage of ITU journeyman printers in an adjacent local.
	(.165)	(.266)	(.294)	(.020)	(.068)		(.128)		
10	.153	.753	−.595	1.004	.354	.999	.004	135	National average for real hourly earnings of nonsupervisory workers in retail trade.
	(.617)	(.448)	(.300)	(.021)	(.071)		(.131)		
11	.268	.714	−.655	.998	.349	.999	−.009	135	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.
	(.574)	(.351)	(.316)	(.021)	(.069)		(.131)		

Note.—Source and definition of variables are in App. A.

TABLE 4

ORDINARY LEAST SQUARES ESTIMATES

A. $\log(J_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(J_{t-1})$;
DEPENDENT VARIABLE = log(Journeyman Members)

Equation Number	Estimated Coefficient for $\log(\bar{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(J_{t-1})$	R^2	$\hat{\rho}$	df	Definition of $\bar{w}$
1	−.058 (.256)	.078 (.131)	−.156 (.228)	.384 (.080)	.999	.127 (.210)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.316 (.278)	.009 (.125)	−.143 (.273)	.389 (.085)	.999	.219 (.227)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	.062 (.201)	.001 (.121)	−.047 (.260)	.373 (.083)	.999	.126 (.222)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	−.047 (.146)	.074 (.116)	−.151 (.224)	.383 (.080)	.999	.137 (.209)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	.026 (.098)	.024 (.127)	−.017 (.277)	.401 (.086)	.999	.196 (.241)	122	Regional average for hourly earnings of production workers in all printing and publishing.
6	−.000 (.087)	.067 (.141)	−.196 (.287)	.429 (.097)	.999	.136 (.265)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	−.049 (.198)	.080 (.133)	−.159 (.230)	.383 (.080)	.999	.139 (.212)	136	National average for weekly real contract wage of ITU journeyman printers.
8	.100 (.167)	.020 (.129)	−.107 (.232)	.385 (.080)	.999	.118 (.208)	136	State average for weekly real contract wage of ITU journeyman printers.
9	−.121 (.119)	.109 (.118)	−.176 (.224)	.382 (.080)	.999	.160 (.211)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	.061 (.283)	.046 (.131)	−.136 (.227)	.387 (.080)	.999	.128 (.209)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.114 (.315)	.084 (.125)	−.145 (.223)	.385 (.080)	.999	.133 (.207)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

B. $\log(M_t) = a_0 + a_1 X_t + a_2 \log(\overline{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(M_{t-1})$;
DEPENDENT VARIABLE = log(Active Membership)

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\overline{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(M_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\overline{w}$
1	−.026 (.346)	−.028 (.137)	−.323 (.242)	.376 (.080)	.999	.253 (.199)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.048 (.301)	.003 (.139)	−.365 (.295)	.408 (.088)	.999	.170 (.209)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	−.098 (.210)	−.071 (.130)	−.210 (.273)	.354 (.083)	.999	.255 (.215)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	−.107 (.155)	−.008 (.124)	−.363 (.238)	.375 (.080)	.999	.250 (.196)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	−.050 (.104)	.020 (.139)	−.384 (.297)	.407 (.088)	.999	.149 (.209)	122	National average for hourly earnings of production workers in all printing and publishing.
6	−.026 (.094)	−.085 (.157)	−.423 (.312)	.392 (.099)	.999	.010 (.234)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	.088 (.209)	−.068 (.141)	−.323 (.244)	.377 (.080)	.999	.236 (.199)	136	National average for weekly real contract wage of ITU journeyman printers.
8	.135 (.179)	−.093 (.140)	−.298 (.246)	.370 (.080)	.999	.255 (.197)	136	State average for weekly real contract wage of ITU journeyman printers.
9	−.015 (.127)	−.029 (.128)	−.312 (.239)	.376 (.080)	.999	.250 (.201)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	.250 (.298)	−.099 (.140)	−.314 (.240)	.375 (.079)	.999	.218 (.199)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	−.022 (.336)	−.031 (.135)	−.348 (.237)	.377 (.080)	.999	.257 (.200)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

TABLE 4 (*Continued*)

C. $\log(H_t) = a_0 + a_1 X_t + a_2 \log(\overline{w}_t) + a_3 \log(w_t) + a_4 \log(1 - U_t) + a_5 \log(H_{t-1})$;
DEPENDENT VARIABLE = log[Assessments/(Wages · Aggregate ITU Assessment Rate)]

EQUATION NUMBER	ESTIMATED COEFFICIENT FOR $\log(\overline{w}_t)$	$\log(w_t)$	$\log(1 - U_t)$	$\log(H_{t-1})$	R^2	$\hat{\rho}$	df	DEFINITION OF $\overline{w}$
1	1.035 (.264)	−.988 (.135)	−.641 (.235)	.322 (.060)	.999	.089 (.127)	136	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
2	.473 (.309)	−.697 (.138)	−.685 (.308)	.346 (.076)	.999	−.069 (.170)	122	Regional average for real hourly earnings of production workers in all manufacturing industries.
3	.110 (.212)	−.788 (.129)	−.526 (.276)	.316 (.064)	.999	.006 (.140)	125	State average for real hourly earnings of production workers in all manufacturing industries.
4	.303 (.159)	−.775 (.125)	−.791 (.241)	.318 (.063)	.999	.022 (.132)	136	National average for real hourly earnings of production workers in all U.S. printing and publishing.
5	.016 (.108)	−.667 (.140)	−.624 (.311)	.367 (.076)	.999	−.196 (.169)	122	Regional average for hourly earnings of production workers in all printing and publishing.
6	.220 (.097)	−.810 (.157)	−.659 (.327)	.382 (.090)	.999	−.146 (.187)	97	State average for real hourly earnings of production workers in all printing and publishing.
7	.856 (.202)	−1.019 (.136)	−.596 (.235)	.322 (.059)	.999	.104 (.128)	136	National average for weekly real contract wage of ITU journeyman printers.
8	.623 (.178)	−.960 (.137)	−.602 (.241)	.298 (.061)	.999	−.014 (.128)	136	State average for weekly real contract wage of ITU journeyman printers.
9	.299 (.128)	−.812 (.127)	−.752 (.240)	.333 (.062)	.999	.018 (.128)	136	Real hourly contract wage of ITU journeyman printers in an adjacent local.
10	1.000 (.295)	−.951 (.137)	−.694 (.237)	.320 (.061)	.999	.061 (.128)	136	National average for real hourly earnings of nonsupervisory workers in retail trade.
11	1.006 (.334)	−.891 (.132)	−.825 (.235)	.322 (.061)	.999	.055 (.129)	136	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.

D. $\log(A_t) = a_0 + a_1 X_t + a_2 \log(\bar{w}_t) + a_3 \log(w_t) + a_4 \log(\theta_t) + a_5 \log(1 - U_t) + a_6 \log(H_{t-1})$;
Dependent Variable = log(Assessments)

Equation Number	Estimated Coefficient for $\log(\bar{w}_t)$	$\log(w_t)$	$\log(\theta_t)$	$\log(1 - U_t)$	$\log(H_{t-1})$	R^2	$\hat{\rho}$	df	Definition of $\bar{w}$
1	1.101	−.004	.985	−.730	.314	.999	.095	135	National average for real hourly earnings of production workers in all U.S. manufacturing industries.
	(.276)	(.137)	(.018)	(.258)	(.060)		(.128)		
2	.491	.304	1.013	−.687	.345	.999	−.078	121	Regional average for real hourly earnings of production workers in all manufacturing industries.
	(.318)	(.138)	(.046)	(.309)	(.077)		(.171)		
3	.080	.211	1.021	−.401	.320	.999	−.040	124	State average for real hourly earnings of production workers in all manufacturing industries.
	(.214)	(.129)	(.021)	(.303)	(.064)		(.139)		
4	.300	.225	1.003	−.772	.319	.999	.021	35	National average for real hourly earnings of production workers in all U.S. printing and publishing.
	(.160)	(.125)	(.018)	(.270)	(.064)		(.132)		
5	.016	.333	.998	−.624	.367	.999	−.197	121	Regional average for hourly earnings of production workers in all printing and publishing.
	(.108)	(.141)	(.046)	(.313)	(.077)		(.170)		
6	.220	.189	.996	−.659	.382	.999	−.159	121	State average for real hourly earnings of production workers in all printing and publishing.
	(.097)	(.158)	(.047)	(.329)	(.091)		(.187)		
7	.857	−.020	1.007	−.551	.326	.999	.100	135	National average for weekly real contract wage of ITU journeyman printers.
	(.202)	(.136)	(.017)	(.262)	(.060)		(.128)		
8	.624	.039	1.007	−.556	.301	.999	−.020	135	State average for weekly real contract wage of ITU journeyman printers.
	(.179)	(.137)	(.018)	(.269)	(.062)		(.128)		
9	.299	.188	1.005	−.720	.335	.999	.013	135	Real hourly contract wage of ITU journeyman printers in an adjacent local.
	(.128)	(.127)	(.018)	(.269)	(.062)		(.128)		
10	1.028	.043	.992	−.741	.316	.999	.063	135	National average for real hourly earnings of nonsupervisory workers in retail trade.
	(.304)	(.138)	(.018)	(.262)	(.061)		(.130)		
11	1.098	.093	.986	−.920	.314	.999	.062	135	National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries.
	(.356)	(.134)	(.019)	(.267)	(.062)		(.131)		

Note.—Source and definition of variables are in App. A.

instrumental variable (IV) estimates presented in table 3.[17] In comparison with these IV estimates, two basic findings are apparent. First, for membership-based measures of employment, the OLS point estimates are not substantially different from the previous IV point estimates. There is a clear tendency for the OLS coefficients on both the contract wage and the alternative wage to be smaller in absolute value than the corresponding IV estimates (see, e.g., rows 1, 7, and 11 of table 4, pts. A–B), but the basic nature of the results is unchanged. There is little evidence of any wage effects on employment in parts A and B and only marginal evidence that alternative employment probabilities have any influence on employment. Perhaps the wage bargains struck by the locals in our sample are not greatly influenced by membership levels. If so, this might explain the congruence between table 3, parts A–B, and table 4, parts A–B. An alternative explanation, of course, is that these membership-based measures of employment are so poor that, regardless of our estimation method, no clear relationship can be uncovered.

The second finding in table 4 is that, for earnings-based measures of employment, there is a substantial difference between OLS and IV estimated wage effects. In particular, the OLS estimates of the employment effects of contract wages tend to be much larger in absolute value in table 4, parts C–D, than the corresponding IV estimates in table 3, parts C–D. In addition, the standard errors for the estimated contract wage coefficients are substantially reduced. The consequence of these two facts is that, for every measure of the alternative wage in table 4, parts C and D, the contract wage maintains a statistically significant negative effect on employment. In contrast, the estimated alternative wage effect is consistently positive and frequently statistically significant. At the same time, it remains true that greater alternative employment probabilities reduce local employment. Indeed, both the coefficient estimates and standard errors for the employment probability variable are changed only slightly in going from IV to OLS.

One obvious explanation for the pronounced contract wage effects in table 4, part C, is that the dependent variable is based on a ratio, the denominator of which is the contract wage. If the contract wage is measured with any error, this measurement error will bias the estimated contract wage coefficients toward negative one. The estimated contract wage effects in part C certainly suggest that this problem is present.

Consider, however, the estimated contract wage coefficients in part

[17] For the sake of brevity, we discuss only those equations including the employment probability variable, $\log(1 - U)$.

D. There is no mechanical or definitional tendency for these coefficients to be biased, and yet they imply virtually the same contract wage effects as indicated by part C. In particular, the estimated wage coefficients in part D are typically several standard errors below unity. Put simply, increases in contract wages are not associated with anything near equal proportionate increases in assessments, with the ITU assessment rate held constant. Our reading of this fact is that hours per employed member or employed members per total membership must be falling as the contract wage increases. We are inclined, therefore, to interpret part D as providing additional evidence that employment contracts are not strongly efficient. At the very least, we find no overwhelming evidence consistent with strong efficiency.

We do continue to find evidence consistent with weak efficiency, however. As was true in table 3, parts C–D, higher alternative employment probabilities reduce local employment by a statistically significant amount. This fact could be explained by a negative correlation between general levels of demand and unmeasured elements of the demand for printers' services, but such a negative correlation seems quite unlikely to us.[18] Our interpretation of the ITU data, therefore, is that strong efficiency does not seem to be present, but weak efficiency cannot be rejected. We therefore cannot reject the null hypothesis of Pareto efficiency in employment determination.

V. Concluding Remarks

A meaningful distinction between efficient and inefficient employment contracts under bilateral monopoly exists only when employees (or their unions) value the overall level of employment in their firm. When a meaningful distinction exists between efficient and inefficient employment contracts, public policies that weaken trade unions will have different effects on employment according to whether employment contracts are struck as efficient bargains. Likewise, the welfare cost of trade union "wage distortions" is a meaningful concept only when employment bargains are inefficient since the absence of the welfare cost of union wage distortions is precisely what characterizes an efficient employment bargain. It follows that the empirical determination of whether employment bargains are struck efficiently is of considerable practical importance.

[18] For the 10 locals contained in our data set, the correlation between log(linage) and $\log(1 - U)$, corrected for trends and fixed effects, is .316, with a probability value of .0001. For the ITU as a whole, the corresponding correlation is .464, with a probability value of .052. Given the clear positive correlation between printers' output and alternative employment probabilities, it seems unlikely to us that a negative correlation exists between unmeasured elements of demand and alternative employment probabilities.

Much of the recent literature on employment contracts emphasizes that it is in the interests of the parties to produce institutional arrangements that lead to employment contracts that we have termed "strongly efficient." Strong efficiency implies that employment is set so as to equate the marginal revenue product of workers to their alternative wage. It follows that employment in such contracts fluctuates with the determinants of a worker's marginal revenue product and with the worker's alternative wage, but not with the observed contract wage.

We have examined two kinds of evidence to test the strong efficiency hypothesis. Laboratory experiments by Siegel et al. indicate that this hypothesis is strongly confirmed when the bargaining parties are required to agree on price and quantity simultaneously and is strongly rejected when the parties are required to bargain by a system of price leadership. In our field data on the printing trades, we find no convincing evidence of strong efficiency. This is also MaCurdy and Pencavel's (this issue) finding. When we use membership-based measures of employment, we typically find no statistically significant employment effect of contract wages. We also find, however, no statistically significant employment effect of alternative wages or alternative employment probabilities. Thus we are hesitant to make too much of the fact that contract wages have no statistically discernible effect on membership-based measures of employment. When we make use of earnings-related measures of employment, we find some evidence that contract wages do influence employment negatively. When instrumental variables techniques are used, the estimated employment effect of contract wages is always negative although imprecisely measured. When ordinary least squares is used, the estimated employment effect of contract wages is uniformly negative and always statistically significant.

We have also examined the evidence in support of what we have called the "weak efficiency hypothesis." According to this hypothesis, both the contract wage *and* the alternative wage determine employment. To examine this hypothesis, we have searched for convincing evidence that some measure of the alternative wage is a determinant of employment in our field data on the printing trades. We have found some support for this hypothesis in the negative effects of alternative employment probabilities on local employment of ITU printers. However, we have also found that our measures of the alternative wage available to workers are frequently positively related to employment, precisely the contrary of the hypothesized direction of this effect in a weakly efficient contract.[19]

[19] MaCurdy and Pencavel (this issue) report negative effects for the alternative wage in their study of employment in the printing trades, but it is difficult to judge the

It seems clear that further research will be required before any of the simple contracting models of employment determination that we have examined here are likely to be useful positive tools in the analysis of public policies toward the labor market.

Appendix A

Definitions and Sources for Variables Used in the Empirical Analysis

Measures of Employment, Earnings, and Contract Wages

Data for the following variables were taken from annual statistical supplements to the *Typographical Journal* and from various issues of the *ITU Journal.*

w = local hourly contract minimum-wage scale (as of May 20);
M = local "active" membership, defined as total journeyman members plus apprentices minus journeyman members on pension (as of May 20);
J = local journeyman membership (as of May 20);
A = local assessments during the fiscal year ending May 20;
θ = ratio of the total ITU assessments during the fiscal year ending May 20 to total ITU earnings during the fiscal year ending May 20.

Measures of Demand for Printers' Services (**X**)

Data for the following variables were taken from April issues of *Editor and Publisher* and from annual issues of *Editor and Publisher's International Year Book*: (i) average real advertising rate per line charged by the newspaper(s) employing local printers (measured by calendar year); (ii) average yearly advertising linage for the newspaper(s) employing local printers (measured by calendar year).

Measures of Alternative Wages ($\bar{\mathbf{w}}$)

Data for the following measures were taken from *Employment and Earnings, United States,* 1909–75, and from *Employment and Earnings, States and Areas,* 1939–78: (i) national average for hourly earnings of production workers in all manufacturing industries; (ii) national average for hourly earnings of nonsupervisory workers in retail trade; (iii) national average for hourly earnings of production workers in all nondurable goods manufacturing industries; (iv) national average for hourly earnings of production workers in SIC 27 (printing and publishing); (v) state average for hourly earnings of production workers in all manufacturing industries; and (vi) state average for hourly earnings of production workers in SIC 27 (printing and publishing).

Data for the following measures were taken from annual issues of the *Annual Survey of Manufactures* and of the *Census of Manufactures:* (vii) regional

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statistical significance of this result. It is our impression that the relevant coefficient is statistically significant in a linear specification of the right-hand side of our eq. (11) and is not statistically significant in a specification based on the Stone-Geary utility function, (17) above. However, MaCurdy and Pencavel do not require statistically significant evidence that the alternative wage influences employment to conclude that the data favor the weak efficiency hypothesis.

TABLE A1

Summary Statistics for Selected Variables from ITU Micro Data, 1948–65

Variable	Mean	Standard Deviation	Minimum	Maximum	Number of Observations
Contract wage/CPI	3.24	.44	2.14	4.09	200
"Active" membership	303.87	266.30	34.00	902.00	200
Journeyman membership	319.19	283.27	34.00	938.00	200
Imputed hours/2,000	279.93	247.80	23.98	860.15	200
Assessments/CPI (× 1,000)	59.03	59.97	2.64	347.44	200
Advertising linage (× 1,000)	29.49	16.14	5.40	63.70	197
Advertising rate per line/CPI	.48	.30	.13	1.19	200
Contract wage divided by:					
National average for real hourly earnings of production workers in all U.S. manufacturing industries	1.42	.14	1.15	1.75	200
Regional average for real hourly earnings of production workers in all manufacturing industries	1.46	.24	1.06	2.03	156
State average for real hourly earnings of production workers in all manufacturing industries	1.50	.26	1.08	2.10	166
National average for real hourly earnings of production workers in all U.S. printing and publishing	1.19	.12	.97	1.53	180
Regional average for hourly earnings of production workers in all printing and publishing	1.22	.15	.96	1.80	156
State average for real hourly earnings of production workers in all printing and publishing	1.25	.14	1.01	1.80	130
National average for weekly real contract wage of ITU journeyman printers	1.03	.08	.79	1.20	200
State average for weekly real contract wage of ITU journeyman printers	1.048	.08	.79	1.32	200
Real hourly contract wage of ITU journeyman printers in an adjacent local	1.00	.07	.81	1.23	200
National average for real hourly earnings of nonsupervisory workers in retail trade	.93	.004	.92	.94	190
National average for real hourly earnings of production workers in all U.S. durable goods manufacturing industries	1.33	.13	1.08	1.66	200

average for hourly earnings of production workers in all manufacturing industries (this variable was calculated as the ratio of total wage payments to production workers divided by total hours of production workers for the relevant census region); and (viii) regional average for hourly earnings of production workers in SIC 27 (printing and publishing) (this variable was calculated as the ratio of total wage payments to production workers divided by total hours of production workers for the relevant census region).

Data for the following measures were taken from annual statistical supplements to the *Typographical Journal:* (ix) national average for ITU contract minimum weekly wage scales; (x) state average for ITU contract minimum weekly wage scales; and (xi) hourly ITU contract minimum-wage scale for an "adjacent" local. Pairings were as follows: (Cincinnati, Columbus); (Dubuque, Fond du Lac); (Memphis, Louisville); (Augusta, Columbia); (Elmira, Albany).

All nominal values were deflated by the consumer price index (all items).

For the equations in which $\log(1 - U)$ was included as a regressor, the variable U was measured as the state-insured unemployment rate, taken from the March 1964 and March 1966 issues of the *Manpower Report of the President.*

Data for the following locals were used in our empirical analysis: Cincinnati, Ohio; Augusta, Georgia; Columbia, South Carolina; Dubuque, Iowa; Memphis, Tennessee; Fond du Lac, Wisconsin; Louisville, Kentucky; Elmira, New York; Columbus, Ohio; Albany, New York.

We are indebted to John Pencavel for providing us with some of the data on which our work is based. For earlier work based on data for these locals, see Pencavel (1984) and Dertouzos and Pencavel (1981).

Descriptive statistics for the variables used in our analysis are presented in Table A1.

Appendix B

Problems of Empirical Implementation

Measuring Employment

A major shortcoming of our data is that they provide no direct measure of local employment. Instead, we are provided only with measures of local membership.[20] If we are to interpret our membership data as employment data, we must first assume that the disemployed members of each local, denoted by $(\bar{L} - L)$ in the context of equation (20), have already left the local, so that observed membership reflects L and not $\bar{L}$. Clearly, such an assumption is problematic, especially since the differences between local membership and local employment may not be independent of the contract wage.

In an attempt to deal with this problem we have also made use of data on local dues paid to the national union to construct an alternative, imputed measure of actual employment at the local level. By definition,

$$A_{it} = \theta_{it} w_{it} h_{it}, \tag{B1}$$

[20] Local membership data are further broken down into two groups, journeymen and apprentices. The ITU also reports for each local the number of members on pension. Consequently, it is possible to measure the number of "active" journeymen by taking the difference between total journeyman members and members on pension.

where A_{it} denotes assessments (dues) for the ith local in year t, θ_{it} denotes the assessment rate for local i in year t, w_{it} denotes the contract wage for local i in year t, and h_{it} denotes total man-hours worked for local i in year t.[21] We have no direct measures for θ_{it} or h_{it}, but we can observe θ_t, the overall assessment rate for the ITU as a whole, and M_{it}, "active" membership for local i in year t. Therefore, if we assume that θ_{it} can be written as $\lambda_i\theta_t$ and that h_{it} can be written as $\tau_i M_{it} v_{it}$, where λ_i and τ_i are local-specific constants and v_{it} reflects the intertemporal variation of hours per active member in local i, equation (B1) can be rewritten as

$$A_{it} = \lambda_i \tau_i \theta_t w_{it} M_{it} v_{it}. \tag{B2}$$

Taking logarithms, we can restate equation (B2) as

$$\log(A_{it}) = \log(\lambda_i \tau_i) + \log(\theta_t) + \log(w_{it}) + \log(M_{it}) + \log(v_{it}). \tag{B3}$$

Treating (B3) as a regression equation, with $\log(v_{it})$ as the unobserved error term, we found the following estimates for the 10 locals comprising our data set:[22]

$$\log(A_{it}) = \text{local-specific intercepts} + \underset{(.021)}{.982} \log(\theta_t) + \underset{(.065)}{1.011} \log(w_{it})$$

$$+ \underset{(.070)}{.951} \log(M_{it}) + e_{it},$$

$$R^2 = .997,\ \text{D-W} = 1.860,\ \text{df} = 176.$$

These estimated coefficients seem sufficiently close to those implied by (B3) to warrant treating the logarithm of $(A_{it}/\theta_t)w_{it}$ as a measure (up to a constant term) of the logarithm of total man-hours for local i in year t. Consequently, we have used as a dependent variable in our empirical analysis the imputed measure $\hat{h}_{it}$, defined as

$$\hat{h}_{it} = \log(A_{it}) - \log(\theta_t) - \log(w_{it}). \tag{B4}$$

Finally, as an alternative and less restrictive procedure, we have also used local-specific assessments as the dependent variable in equations such as (19) and (22), with w_{it}, θ_t, and local-specific dummies included as regressors. In this case, the absence of any contract wage effect on employment would translate into a coefficient of unity for the contract wage, while a negative employment effect of alternative wages would translate into an equivalent negative effect on (log) assessments.

Measuring Alternative Wages

We also face problems with regard to the measurement of alternative wages. On the one hand, we run the risk of choosing an irrelevant measure for the alternative wage, in which case the data would seem to reflect an absence of

[21] Assessment rates may vary over time and across locals because of differences in the proportions of apprentices, journeymen, and pensioned and unemployed members across locals.

[22] We are indebted to David Card for several useful discussions regarding this approach. Equation (B2) was also estimated in a manner that allowed the coefficient for $\log(\theta_t)$ to vary across locals. The resulting estimates were essentially identical to those reported here.

even weak efficiency in employment determination, with the contract wage reflecting both its own effect on employment and some portion of the employment effect of the unobserved alternative wage. On the other hand, even if we have chosen the correct measure for the alternative wage, this alternative wage may be so highly correlated with the contract wage that estimation of the two variables' separate effects may be impossible. Realizing the potential for these problems, but having no clear basis for choice of an alternative wage measure, we have chosen to use several alternative measures that we believe may be relevant. Each measure corresponds to a different assumption regarding the distribution of alternative wage offers confronting disemployed printers, and while we have no way of knowing which, if any, of these assumptions is valid, it seems likely that at least one of them may provide some useful information.

Measuring Demand-related Variables

ITU printers may typically be employed either by newspapers or "job" establishments. In our empirical work we require data that reflect firms' demands for printers' services (e.g., output, product price, and advertising rates). Such data are reasonably available only for newspapers, however, and so we have no measures of demand-related variables directly relevant to those printers employed in job establishments. Moreover, the data do not allow us to identify which members of a local are employed by news (or job) establishments. Therefore, to whatever extent the printers in our sample are employed by job establishments and to whatever extent the determinants of job establishments' demand for printers' services differ from those of news establishments, our empirical work will be subject to error.

Now that the most obvious limitations of our data have been discussed, it remains for us to discuss four additional sources of potential problems in our interpretation of the data, all of which fall under the general heading of misspecification. The first of these concerns our ability to control for other factors that might enter firms' production functions. The second concerns the presence of additional constraints on firms' behavior and, in particular, the possibility that a fixed-output framework might be more appropriate for our analysis. The third concerns the question whether our interpretation of the data would remain valid when placed in an explicitly dynamic framework. Finally, the fourth source of potential problems concerns the issue of identification.

Controlling for Other Factors Entering Firms' Production Functions

To this point we have implicitly assumed that firms are price takers in the market for all other factors of production and that the effects of variations in the rental prices of these other factors can be taken into account in our statistical work. Obviously, this is a difficult task made even more difficult by the fact that we have no direct measures for local employment of other factors of production and only very limited measures of rental prices for other factors of production. The only factor price data we have are annual observations on (1) the price of newsprint and (2) the BLS wholesale price index for machinery and equipment. Both of these price indexes are aggregate measures that are common across locals. Thus we have no real ability to allow for cross-local variation in the employment or rental prices of other factors and only limited ability to control directly for intertemporal variation in these variables. Because of these data limitations we have assumed that firms' production functions are of the constant elasticity of substitution form so that printers' (log) marginal products can be expressed in terms of employment and output alone. To the extent this assumption is invalid, our inferences will be subject to error.

Allowing for the Possibility of Quantity Constraints

In Section III of the text we treated output as endogenous to the firm. There may, however, be some cases in which it is more appropriate to treat output as exogenous for the firm. This would surely be the case, for example, if one were studying a regulated industry that was constrained to offer some minimum level of service. Alternatively, in an industry such as newspaper production, which is characterized by continuously increasing returns to scale, output may not be determined by the usual marginal conditions.[23] It may therefore be worthwhile to describe briefly how our analysis would be changed in the case of exogenous output constraints.

To deal with the fixed-output case, consider the "other inputs" requirement function, $K = K(L, Q)$, which gives the other inputs necessary to produce Q units of output when L units of labor input are used.

Given that Q units of output are to be produced, the usual minimization of costs subject to a utility constraint leads to an efficiency condition analogous to (11) of the form:

$$\frac{w - rK_L}{L} = \frac{\partial u(\cdot)/\partial L}{\partial u(\cdot)/\partial w}, \tag{B5}$$

where r denotes the rental rate for capital.

The empirical analysis can now proceed exactly as before, with the function $rK_L(L, Q)$ replacing the marginal revenue function $R_L(L, K)$ throughout. The main difference between this case and the case in which output is chosen freely by the firm is that in this case the rental prices for other factors of production should enter into any estimated employment equation. In addition, any empirical specification must obviously allow for interactions between rental prices and the marginal rate of substitution function K_L. The advantage of this model is that it is consistent with a situation in which union and management bargain over capital/labor or man/machine ratios but output is not determined in this way.

Amendments Suggested by a Dynamic Framework

Our discussion in Section III made no distinction between short-run and long-run employment determination. Our implicit assumption has been that the data reflect a sequence of equilibria generated from a static model. This assumption should be questioned, especially since there is so much other evidence of serial correlation in employment equations and since the whole context of our discussion is one in which there is some fixity in employment relations. It is beyond the scope of this paper to deal exhaustively with the question of employment efficiency in a fully dynamic context.[24] We have

[23] For a discussion of returns to scale in the printing industry, see Rosse (1970).

[24] David Card has pointed out that alternative wages may exert a negative contemporaneous influence on employment even in the absence of Pareto efficiency if firms

made some attempt, however, to allow for a lagged response of employment to wages by placing our employment equations with a partial adjustment model that includes lagged values of the endogenous variable on the right-hand side. With this amendment, we find no serious evidence of remaining serial correlation in the residuals from our estimated equations. Obviously, however, our model for the time-series properties of the data can be questioned. Although our major results do not appear to be sensitive to our treatment of the dynamic nature of the data, the reader should be aware that we think there is some ambiguity regarding the appropriate way to deal with this issue.

The Issue of Identification

Our discussion in Section III focused on the implications of Pareto efficiency for employment determination and confined its attention to equations such as (19) and (22), in which employment is treated as a function of the contract wage. In the bargaining context that we have stressed, however, it is almost certainly true that contract wages and employment are jointly endogenous, and so the question naturally arises, Are equations such as (19) and (22) identified? As usual, the answer to this question depends on the assumptions one is willing to maintain.

To pose the problem in its simplest form, suppose that bargaining between the firm and the union leads to a contract wage that is some linear function of workers' marginal revenue product and their alternative wage, for example,

$$w = d_0 + d_1 R_L + d_2 \bar{w}. \tag{B6}$$

Suppose also that efficiency condition (11) leads to an employment equation of the general form

$$R_L = g_0 + g_1 w + g_2 \bar{w}. \tag{B7}$$

If equations (B6) and (B7) describe the determination of contract wages and employment, it is apparent that neither equation can be identified without further restrictions.

In the absence of any other information, in order to identify equation (B7), we seek some variable(s) that will influence the wage bargain (B6) without at the same time influencing the efficient level of employment (B7). Unfortunately, none of the standard variables typically used to identify labor demand and supply functions will work in this case, since equation (B7) incorporates all variables influencing both supply and demand. Our choice must be limited to only those variables that affect unions' wages independently of the optimal employment of union members.

After considering a number of possibilities, we have concluded that our best hope for identification lies in the presence of contractual arrangements that are designed to stabilize workers' earnings over time or to compensate workers, ex post, for price-induced changes in the purchasing power of their earnings. With income-smoothing arrangements, one might expect equation (B6) to include lagged wages as well as current values for R_L and $\bar{w}$. If so and if there were no corresponding lags in the effects of contract wages on efficient employment, lagged wages could serve as instruments in the estima-

face nonzero costs of employment adjustments and if future contract wages depend positively on current alternative wages. For a careful treatment of employment efficiency in a dynamic context, see Card (1984).

tion of equations such as (B7). A similar argument might be made for arrangements that index current wages to some average of current and past consumer prices. In this case, current and past consumer prices would enter equation (B6) but would enter equation (B7) only insofar as they directly influence optimal employment levels. For the printers in our sample it seems reasonable to assume that the direct employment effect of consumer prices is negligible relative to the direct effect on wages, and so we have also used current and lagged consumer prices, in addition to lagged contract wages, as instruments for current contract wages in the estimation of our employment equations.[25]

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[25] We should note one potential problem with the use of current and lagged consumer prices as instruments in the estimation of equations such as (B7). The use of these variables will not be appropriate if firms fully compensate their employees ex post for unforeseen earnings losses caused by variations in consumer prices. In the presence of full compensation, current real wage effects of consumer prices are fully offset by expected future compensation, and so consumer price–induced wage variation would have no employment effect. In this case, the data would appear to be consistent with strong efficiency, even if a monopoly model of union-firm interaction were valid.

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