RENT-SHARING IN THE LABOR MARKET

Andrew Oswald

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RENT-SHARING IN THE LABOR MARKET*

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Abstract

Is the labor market well-approximated by a competitive model, or is wage determination instead a kind of non-competitive rent-sharing? This unsettled question lies at the heart of labor economics and macroeconomics. The paper argues that new research -- drawing upon data of a kind not available to previous generations of researchers -- appears to provide persuasive evidence for the existence of rent-sharing in the labor market.

Rent-Sharing in the Labor Market

Andrew J. Oswald

1. Introduction

Can the market for labor be represented satisfactorily by a standard competitive model? Many years ago, the Harvard economist Sumner Slichter (1950) argued that it could not. A competitive model, he said, fails to explain the fact that similar people are paid significantly differently in different industries. Slichter's data were crude and highly aggregated by today's standards, but appeared to reveal a positive correlation between wages and the employer's so-called ability-to-pay. He argued that this correlation provided *prima facie* evidence against the conventional competitive model in which firms are wage-takers.

In the ensuing decades, these ideas were neglected. The North Holland Handbook of Labor Economics, edited by Orley Ashenfelter and Richard Layard (1986), is 1300 pages long and was in the mid-1980s the encyclopedic statement of its field. Yet the two volumes include no reference to Slichter's work on non-competitive differentials[1], and little discussion of the possibility that non-union workers might earn rents in equilibrium (though, of course, they discuss how union workers could do so). The Handbook is dominated by the intellectual framework offered by the competitive model; a central concern of the book is to examine the role of human capital in the determination of people's remuneration. Reasonably enough, The Handbook reflects the orientations and beliefs of labor economics as it was taught at universities from the 1960s to the 1980s.

This paper describes new empirical work that takes seriously the possible existence of imperfections in labor markets. There are two generations of modern research. The first is fairly well-known and is not the focus here. It replicates Slichter on today's data. The central piece of

[1] I shall follow convention in using the term "rents" to denote supernormal returns, that is, earnings greater than some alternative or competitive-market return. I take the term "quasi-rents" to mean rents that are available ex post to an agent but not necessarily ex ante. In normal usage, the quasi-rent of a firm is the difference between sales revenue and variable costs, but this literature's usage is slightly different.

evidence is the statistical fact -- demonstrated by Dickens and Katz (1987) and Krueger and Summers (1987, 1988) -- that in a country such as the United States there continue to be large unexplained inter-industry wage differentials. In other words, in a modern cross-section wage regression in which many independent variables are included, it is found that dummy variables for industries enter with economically and statistically significant coefficients. The papers also show, in line with rent-sharing theories discussed later, that estimated US wage differentials are positively correlated in cross-section with US industry profitability. Sanfey (1992, 1993) establishes the same for Britain. These papers are useful additions to what we know. Nevertheless, it might be argued that they do not constitute a major step beyond Sumner Slichter's contribution.

In the late 1990s, a second generation of research is visible. It represents evidence of an arguably more decisive sort. The work has emerged as a response to three damaging criticisms of the Slichter-Dickens-Katz-Krueger-Summers evidence for rent-sharing theory.

(i) *The cross-section correlations that these investigators find may be caused by unobservable fixed effects*. For example, a special kind of technology might, for competitive reasons, lead to both high profits and high pay. A special kind of market structure could do the same[2]. In these cases, it would be wrong to view a profit-pay correlation as something to do with the existence of rents. (ii). *In a cross-section industry correlation, it is difficult to distinguish cause and effect*. Thus Slichter's correlation is not proof of rent-sharing effects. (iii) *Correlations at the industry-level rest on a variety of extreme and untested aggregation assumptions*. This, some argue, renders suspect any empirical claims.[3] What theory suggests is that longitudinal microeconomic information, especially about firms and establishments[4], is the desirable basis for testing. Slichter did not have such data.

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[2] It could also be that omitted person-specific effects explain industry wage differentials. But the evidence in sources such as Gibbons and Katz (1993) suggests not.

[3] Readable surveys on the economics of industry differentials include Groshen (1991) and Thaler (1989). Further results using industry data are given in Beckerman and Jenkinson (1991), Holmlund and Zetterberg (1991), and Sanfey (1992, 1993). Time-series results on the role of lagged profits in aggregate real wage equations, and a summary of earlier types of evidence, are contained in the book by Carruth and Oswald (1989).

[4] The case for microeconomic empirical research, using primary data on firms and establishments, has been made many times in the published literature.

2. Thinking about the issues

Imagine a competitive labor market operating within an industry, J. Consider the consequences of an exogenous rise in demand for the product of that industry. In the labor-market model of most textbooks -- frictionless competition with a perfectly elastic supply of workers at the going rate of pay -- this demand shock cannot have either a short-run or long-run impact on the wage. The supply of labor to the industry is horizontal; demand affects only the intersection on the quantity axis; wages are fixed by the law of one price; the level of pay is independent of profitability.

Such a framework might be considered too extreme. An alternative is a form of model that probably aligns with most labor economists' thinking. This is a framework that is essentially competitive but has been augmented to allow for some frictions. In such a world, competitive pressures win out eventually, but they do so gradually. After a positive demand shock, profits π^j and wages w^j at first rise together. The reason for the correlation is straightforward. It is induced by the outward shift in the demand curve for labor in industry J. This traces along a labor supply curve that is upward-sloping because it takes firms time and money to fill jobs. However, workers of a given skill are then being rewarded more highly in industry J than in other sectors of the economy. In long-run equilibrium, this is not sustainable. There will be inward migration of workers into the high-paying industry. By competitive logic, this migration continues until it has eventually eliminated the wage gain in the industry. Increased profits, therefore, may raise pay levels in the short-run but will have no long-run effect on pay. My university does not have to offer me more money when it becomes richer. It is other universities' wage offers alone that are relevant, because those are what might attract me away.

This prediction of canonical wage theory -- one that might be described as the long-run irrelevance of profits -- can be tested if appropriate data are available. In an idealized world, the hypothesis would not be investigated in an econometric way (just as researchers into the effects of sleeping pills would not choose to rely on secondary data). Consider the following thought-experiment. The government gives $20 million dollars to fund a social science experiment. A sample of firms is chosen. They are divided into two groups. The first is the experimental group. These firms have their profits abruptly increased by a large donation from the experimenters' research fund. The profits of the second (control) group are left unchanged. The experimenters then wait. After some years, they return to the firms. To see how the different firms have behaved, the experimenters measure whether wages have risen more in the experimental group, perform a statistical check on the outcomes in the two groups, and write up their work for publication.

This is not meant as a proposed research strategy. Economics in the 1990s cannot mimic such methods. But the thought-experiment makes the issues clear. The research question is whether long-run pay responds to exogenous changes in profitability, and the practical difficulty is how to find in reality some form of data that might allow causality to be delineated in the sharp way that is possible in the idealized field experiment.

A new generation of research attempts this. It estimates wage equations and tests for the statistical significance of an independent variable, π/n, measuring profitability or quasi-rents per worker. In the last few years, data sets of many kinds have been used. These cover establishments, firms, labor contracts, and individuals linked to industries. Later pages will argue that they lead to a common conclusion.

In this field, as in most parts of social science, it is easier to document a correlation than to establish in a compelling way a causal connection between variables. It is useful to be clear about why researchers in this area believe they have found causal links from profitability to pay. They have three reasons. First, there is evidence that profit changes precede wage changes. This would not satisfy David Hume, but counts as one type of argument for causality. Second, many researchers in this field think they have successfully identified the wage equation by finding an 'instrument' for profits, namely, a variable that shifts one (and only one) curve and thus causally traces out another curve. Third, survey evidence shows that real-life pay-setters say that profitability affects their decisions.

Section 3 discusses interactions between wages and profits. It describes four theoretical models. Section 4 looks at the prototype paper in recent microeconometric work. Section 5 examines the approach of those papers that estimate wage equations in which current or lagged profitability variables (of one form or another) are included as independent variables. New work that addresses simultaneity is covered in Section 6. Its substantive claims are startling. Conceptual objections are tackled in Section 7. Section 8 discusses matters of interpretation and other evidence. Section 9 summarizes.

3. Models of the wage-profit correlation

This section considers four ways to think about the problem. These touch upon concepts as diverse as bargaining power, competitive frictions, the sharing of risk, and concerns for fairness.

The textbook model of a competitive labor market maintains that firms are wage-takers. It implies that a highly profitable baker in Chicago will pay the same for a given class of labor as does a relatively unprofitable Chicago bakery. This canonical model could, however, be a useful one for the study of the grain market while simultaneously be a misleading one for the study of the market for people's labor. At least four other approaches are possible. The first is a bargaining framework in which rents are divided between the firm and its employees; the second is a competitive model in which the short-run supply curve of labor slopes upwards; the third is a contract model in which risk-sharing occurs; the fourth models a profit-maximizing concern for fairness. Under each of these, there may be a link from profitability to pay.

Model 1: Bargaining

In a bargaining framework, it is straightforward to demonstrate that there is a positive partial correlation between wages and profit-per-employee, and a negative partial correlation between wages and unemployment. These are long-run correlations in the sense that they exist in equilibrium. Workers share some of the firm's surplus.

Consider a model in which wages are determined as if by a Nash problem in which ϕ is the bargaining power of employees. Write this maximization problem as

$$\text{(1)} \qquad \text{Maximize } \phi \log\left\{\left[u(w)-u(\overline{w})\right]n\right\}+(1-\phi)\log\pi$$

where u(w) is the worker's utility from wage w, $\overline{w}$ is the wage available from temporary work in the event of a breakdown in bargaining, n is employment, and π is profits[5]. This formulation relies on the assumption that in the event of bargaining delay the firm earns zero profit and the worker wage $\overline{w}$, and by the choice of units the variable n is also the probability of employment. Define profits as $\mu f(n)$-wn, where f is a concave production function and μ is a product price. The maximization's solution must be such that each side earn at least what is available as an outside option. It will be useful for later to think of there as being a demand shock, μ, that affects profits, π. Thus μ is a parameter in the profit function.

At an interior optimum, the following first-order conditions hold:

$$\text{(2)} \qquad w: \; \frac{\phi u'(w)}{\left[u(w)-u(\overline{w})\right]n}-\frac{1-\phi}{\pi}=0$$

$$\text{(3)} \qquad n: \; \frac{\phi}{n}+\frac{(1-\phi)[\mu f'(n)-w]}{\pi}=0$$

Rewriting the first of these as

$$\text{(4)} \qquad \frac{u(w)-u(\overline{w})}{u'(w)}=\left(\frac{\phi}{1-\phi}\right)\frac{\pi}{n}$$

by using the approximation

$$\text{(5)} \qquad u(\overline{w})\cong u(w)+(\overline{w}-w)u'(w)$$

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[5] This makes the assumption of a so-called utilitarian union (from Oswald 1982), but with a fixed union membership that is the same as the expected utility form in McDonald and Solow (1981). Similar equations stem from Pencavel's (1991) constant-elasticity objective functions.

produces

$$(6) \qquad w \cong \overline{w} + \left(\frac{\phi}{1-\phi}\right)\frac{\pi}{n}.$$

This equation is useful. It shows that, to a first-order approximation, the equilibrium wage is determined by the outside wage available in the event of a temporary dispute in bargaining, the relative bargaining strength of the two sides, and the level of profit-per-employee[6].

Simple though it is, equation (6) is the fulcrum of this paper and of the underlying empirical literature (though of course it appears there in various guises). The equation is a succinct way to encapsulate the hypothesis of rent-sharing in a form that might be implemented empirically.

Equation (6) is also more general than might at first be apparent. Because it stems only from the first of the two first-order conditions, equation (6) is true independently of the exact nature of the employment function. In particular, given efficiency, it does not depend on whether employment is fixed along a labor demand curve (which would result from efficient bargaining under locally horizontal indifference curves as in Oswald 1993) or an upward-sloping contract curve as in McDonald and Solow (1981)[7]. Equation (6) is, of course, what emerges from a union model. Many economists would expect it to fit the data poorly in non-union sectors -- unless they believe, as Pencavel (1991) has suggested, that union models might be more general than thought. The paper returns to this issue when discussing the evidence.

A conventional assumption about the underlying determinants of $\overline{w}$, the outside temporary income, is that it can be described by a function $c(w^0, b, U)$, where w^0 is the going wage in other sectors of the economy, b is the level of income when unemployed, and U is the unemployment rate among workers of the type employed by the firm. A natural interpretation of the algebra is

[6] One of the few 1980s papers to contain an equation like (6) is Moene (1988). The equation is also implicit in some of the work on trade unions done in the first part of the 1980s, but in sources such as McDonald and Solow (1981) it is not made explicit. Carruth and Oswald (1989) is an early attempt to use equation (6) to motivate an empirical test for profit-effects in wage determination.

[7] Equation (6) would not, however, be generated by the Nickell and Wadhwani (1990) version of a labor demand model. That model relies upon an unexplained inefficiency and would include one extra term for the elasticity of labor demand. As far as I know, the only way to make this kind of right-to-manage model logically consistent is to assume locally horizontal workers' indifference curves. Almost all the papers that use the right-to-manage model fail to make that assumption.

that $\overline{w}$ is expected jobless income and U determines the probability of receiving b rather than w^0. Some evidence for unemployment effects is given in Blanchflower and Oswald (1994). Written in full, therefore, equation (6) can be represented as

$$(7) \qquad w \cong c(w^0, b, U) + \left(\frac{\phi}{1-\phi}\right)\frac{\pi}{n}.$$

In a regression version of (7), estimated on longitudinal data, year dummies might be used to capture alternative wage w^0 and benefits b, leaving industry unemployment U and employer profit-per-employee π/n as the key explanatory variables[8].

Model 2: Competitive frictions

At the other extreme from a bargaining model lies competitive theory. It is of interest to examine whether this, too, can imply a positive co-movement of wages and profitability. It is not sufficient, for later purposes, to appeal in a loose way to a conventional demand/supply picture, because profits do not appear on such a diagram. This subsection shows that in a competitive model, given demand shocks and a positively-sloped labor supply function, there may be a positive short-run correlation between wages and profitability. Intuitively, this is the result of temporary frictions taking the firm up a labor supply curve.

Imagine demand shocks hitting the economy. Because the focus is the relationship between wages and profits, it is convenient to define a maximum profit function

$$(8) \qquad \pi(\mu, w) = \max[\mu f(n) - wn]$$

where employment, n, is chosen to maximize the difference between revenue and labor costs, and f(n) is a concave production function, μ is a demand shock (or output price) variable, and w is the wage. The function $\pi(\mu,w)$ is convex and homogenous of degree one in the prices μ and w. The later analysis assumes that the function is twice differentiable. Assume that $\pi(\mu,w)$ represents the profit of the representative firm within an industry. By an appropriate choice of units, the long-run

[8] Jonathan Haskell has pointed out to me that as π/n enters multiplied by the phi's, it would be natural to interact profitability and unionism in a wage regression. Some studies have effectively done this, by estimating separate union and non-union equations.

equilibrium level of profits can be set as $\pi(\mu,w) = 0$. This is the usual convention that profits be written net of some required return to the entrepreneur who runs the firm. In this framework there is a labor demand curve defined by the derivative of the maximum profit function with respect to wages. Assume that there is also a labor supply function $l(w)$, which may be upward-sloping in the short-run, but which is horizontal in the long-run.

Equilibrium in this market is given by the equation

(9) $-\pi_w(\mu, w) = l(w)$

where the function on the left is the demand curve for labor, and the function on the right is the supply curve of labor. The differential of equation (9) is

(10) $-\pi_{ww}dw - \pi_{w\mu}d\mu = l'(w)dw$

so that the relationship between demand shocks and wages is

(11) $$\frac{dw}{d\mu} = -\frac{\pi_{w\mu}}{l'(w) + \pi_{ww}} \geq 0$$

showing that wages rise, of course, in a boom.

Because the profit function is homogeneous of degree one,

(12) $\pi = \mu\pi_\mu + w\pi_w$.

Differentiating:

(13) $\pi_w = \mu\pi_{\mu w} + w\pi_{ww} + \pi_w$.

Cancelling terms and re-arranging:

(14) $$\frac{\pi_{ww}}{\pi_{w\mu}} = -\frac{\mu}{w} < 0$$

To establish the reduced-form relationship between wages and profits, differentiate throughout the profit function $\pi(\mu,w)$ to give, laboring the point slightly,

(15) $$\frac{d\pi}{dw} = \pi_\mu \frac{d\mu}{dw} + \pi_w$$

(16) $$= -\pi_\mu[l'(w) + \pi_{ww}] / \pi_{w\mu} + \pi_w$$

$$(17) \qquad = -\frac{\pi_{\mu} l'(w)}{\pi_{w\mu}} + \frac{\mu \pi_{\mu}}{w} + \pi_{w}$$

where equation (11) and (14) have been used to substitute terms. Note that there is nothing illicit about $d\mu/dw$; it is simply the inverse of the derivative of wages with respect to the exogenous demand shock. The right hand side of equation (17) is non-negative. It is strictly positive if either supernormal profits are being made ($\pi > 0$), or the labor supply curve is strictly increasing. To check the former, note that, by homogeneity, $\pi > 0$ implies and is implied by

$$(18) \qquad \frac{\mu \pi_{\mu}}{w} + \pi_{w} > 0$$

The latter follows from $l'(w) > 0$, and the fact that $\pi(\mu,w)$ is increasing in the demand shock μ and has a negative cross-partial derivative. Equation (17) then shows that, even in a competitive market, movements in wages and movements in profitability can be positively correlated.

There is one special case to be borne in mind. This is where it could be argued that a perfectly competitive labor market is compatible with a long-run positive correlation between pay and profits. If workers have different tastes for jobs, might it be that the labor supply curve is upward-sloping even in the long run? Economists have traditionally resisted such an opinion. Labor markets are not usually viewed as sufficiently thin to allow such arguments to be persuasive. Perhaps a single firm in an area of low population density might face an upward-sloping labor supply function in the long-run, but later sections will show that there is a pay-profit link even for highly aggregated industries such as 'printing and publishing' and 'financial services'. Although logically possible, it seems far-fetched to suggest that <u>every</u> employee (not merely some) in a publishing office would find it so intrinsically more or less pleasant to work there than in the office of a bank that to increase employment by one worker in the publishing and printing industry that whole industry would have to raise its pay slightly. Direct evidence consistent with these ideas comes from Dickens and Katz (1987). They point out that secretaries and janitors are paid more in highly profitable industries despite the fact that these individuals presumably do the same kind of work everywhere.

Model 3: Sharing risk

This subsection takes a different approach. It shows that in a labor contract model (with symmetric information) in which both workers and the firm are risk-averse, profits and wages are positively correlated. Intuitively, the two parties share the good times and bad times. The elasticity of wages with respect to profits equals the ratio of the parties' relative risk-aversion parameters.

The third model is a generalization of the Baily (1974) and Azariadis (1975) optimal contract framework. In this, the firm and workers are assumed to reach an implicit contract in which wages are set to provide efficient 'insurance' against random demand shocks. Although the original articles assumed that firms are risk-neutral, and thus obtained the result that wages should be rigid, that assumption can be generalized to allow the firm to be averse to risk. The model then predicts a positive correlation between pay and profitability.

A labor contract model can be represented as the following maximization problem:

$$(19) \quad \text{Maximise} \int v(\pi)g(\mu)d\mu$$

subject to

$$(20) \quad \int [nu(w) + (1-n)u(b)]g(\mu)d\mu \geq \bar{u}$$

$$(21) \quad \pi \equiv \mu f(n) - wn$$

The solution is a wage function $w(\mu)$ defined on demand shocks. Implicit in the above formulation are the following assumptions. First, the firm's utility depends upon profits and can be represented by a concave function $v(\pi)$. Second, the worker receives utility $u(w)$ when employed and $u(b)$ when unemployed. Normalizing the size of the labor pool to unity, the probability of employment is n and of unemployment 1-n. Assume that there is no private unemployment insurance and that b is exogenously given[9]. Demand shocks here follow a probability density function $g(\mu)$. Firms must offer their employees the market level of expected utility.

The key first-order conditions are

[9] This is in line with the US data reported in Oswald (1986).

$$(22) \quad w(\mu):\ -v'(\pi) + \lambda u'(w) = 0$$

$$(23) \quad n(\mu):\ v'(\pi)[\mu f'(n) - w] + \lambda[u(w) - u(b)] = 0$$

where λ is a multiplier on the integral constraint (20) and is thus independent of μ. Equation (22) defines an implicit function linking profits and wages. Differentiating:

$$(24) \quad \frac{dw}{d\pi} = \frac{v''(\pi)}{\lambda u''(w)}$$

which is strictly positive if both parties are strictly risk-averse, is undefined if workers are risk-neutral, and is zero if firms are risk-neutral. The latter is the well-known case studied by Baily (1974) and Azariadis (1975).

Assume that workers' relative risk-aversion is r and the firm's relative risk-aversion is Ω. Then, combining (22) and (24),

$$(25) \quad \frac{dw}{d\pi}\frac{\pi}{w} = \frac{\Omega}{r}.$$

In other words, the elasticity of wages with respect to profits is equal to the ratio of the firm's relative risk-aversion to the workers' relative risk-aversion. Here the firm and its employees choose to share the risk of demand fluctuations, so that wages and profits will rise and fall together. Again this will have to be considered as an alternative to models with noncompetitive rent-sharing.

Model 4: Concerns for fairness

The last possibility is a 'fairness' model. Here the idea is that workers are rationally given a split of the profits because firms know that to do otherwise would be seen as unfair. It rests on the assumption -- unconventional in economics -- that the employer's profitability enters the worker's utility function.

There is little formal exploration of this possibility in the published literature on rent-sharing. But to get a feel for how a proof would go, think of two players playing the famous ultimatum game. One proposes a split of a dollar -- say, 90 cents for him/herself, and 10 cents for the other player. The other either agrees to that split (the players then get the split) or rejects it (in which case neither person gets anything). In the laboratory, people routinely offer close to fifty-fifty even though standard theory says they should not. The proposer should take approximately all of the money and the responder ought not to mind. Thaler (1988) is a lucid introduction to the puzzle.

Let the proposer, such as an employer, get x in the unit interval [0,1]. The opponent, who in this case could be seen as an employee, gets 1 - x. Assume that players are potentially jealous or concerned with fairness. The proposer knows that if the other player is left with a tiny sum of money, then the whole deal may be rejected. He or she realises that to keep the worker it is necessary to make an offer that will satisfy his or her notions of fairness, but does not know how much this particular worker is concerned about fairness.

Assume that some people care more about fairness than others. Let there be a known distribution of jealousy or rejection levels, z, in the population. Thus the player responding to the proposer accepts splits giving more than implied by z, and rejects others as unfair. Denote its density function f(z) with cumulative distribution function F(z). Assume F(1) = 1, which implies that if the proposer attempts to take all of the money the deal will definitely be rejected. Assume that F(0) = 0, which implies that if the proposer offers all of the money to the other player then acceptance is guaranteed. By definition, F(x) is the integral of f(z) over [0, x]. The more the proposer tries to take, the lower the chance that he or she will get away with it. The proposer's problem is: Choose x to maximize EU = x.[1 - chance of offer being rejected] + zero = x[1 - F(x)] . The first-order condition for an interior optimum is 1 - F(x) - xf(x) = 0,with the second-order condition requirement that -2f(x) - xf'(x) < 0.

It is straightforward to show that it pays the proposer to be generous. For a uniform distribution of rejection (or "jealousy") levels, the algebra comes out especially neatly. It is optimal for the proposer to suggest a 50-50 split. The proof is as follows. Take a uniform distribution, f = 1/(b - a) and F = (x - a)/(b - a). Here b can be thought of as the length of the unit interval. Plugging this into the first-order condition gives an optimal demand of x* = (1 - F(x))/f(x) = [1 - (x - a)/(b - a)]/[1/(b - a) = 0.5b, or in other words half of the sum to be split.

This is a way of formalizing something[10] that may pass through the minds of the managers of highly profitable enterprises: "I am making so much money, I had better share some of it. Otherwise my workers will give me trouble for behaving unfairly." Such models are in their infancy.

Each of these four approaches can produce a form of positive correlation between profit changes and wage changes. This paper will argue that model 1 seems to fit the facts. Remarkably, it apparently does so even in predominantly non-unionized markets. Model 2 is ultimately less persuasive. While this model needs to be borne in mind (and in many cases apparently has not been) by those writing in the new literature, a slow-adjusting competitive framework predicts no long-run effect from profit shocks on to pay. As the next section explains, recent empirical work seems to be discovering long-run effects. Model 3 represents an important possible account of why pay and profit movements occur in the same direction, but cannot also explain the industry cross-section correlation between the level of pay and the level of profitability. In contrast, model 1 can do so. The approach of model 4, stressing fairness, have not yet been taken seriously in the empirical literature. At this point it is difficult to evaluate.

It might be wondered where so-called insider-outsider models (Lindbeck and Snower, 1989) and matching models (Pissarides, 1990) fit into all this. They typically use the assumption of a Nash bargain. But they do not do so in a distinctive way, and may therefore be viewed simply as further examples of the bargaining model equation (6) that is set out earlier. For this reason, researchers in these branches of the economics literature may find the later empirical estimates relevant for their own models.

4. The prototype that researchers have followed

[10] This section gives a proof I use in graduate teaching. Model 4 is included here because my hunch is that fairness models will eventually make inroads in this debate.

This section is an attempt to describe the prototypical paper's approach. The aim is to set out ideas in a way intelligible to economists who have not worked in this area. Hence, the paper's account must eschew some of the technical details that would justifiably concern an expert. They are more appropriately discussed in a survey paper for specialists.

In a nutshell, the prototypical paper estimates a version of equation (6). It regresses workers' or plants' wages on a combination of the outside wage and the employer's profit-per-employee. It uses microeconomic data. It differences out fixed-effects. It pays some attention to problems of simultaneity. It finds, even in non-union settings, that there is evidence of strong profit-effects on pay. It therefore concludes that the wage-taking competitive model gives the wrong prediction. Table 1 gives an early glimpse of the literature's results.

The exact prodedures and estimates take longer to describe. There are, it might be argued, two channels within the new literature. One estimates wage equation (6) without paying a great deal of 'economic' concern -- this is my term and its meaning will be discussed later -- to the possible endogeneity of profits. The other's raison d'etre is to claim that correct instrumenting is vital. Both avenues are described here. An example empirical paper is also discussed from each of the two sub-literatures. The papers chosen are Abowd and Lemieux (1993) and Blanchflower, Oswald and Sanfey (1996). Both are reasonably representative. The first is chosen here because it will likely eventually be viewed as having made a seminal methodological contribution. Whether the paper's substantive conclusions will prove correct is less certain. The second is chosen here because it was the first to be able to difference out fixed effects while controlling for workers' characteristics as well as profitability. Both papers suffer from weaknesses, however, that are described below.

To estimate equations (6) and (7), what is needed is a statistical procedure for testing the null hypothesis that there are no effects of employers' prosperity upon pay. A test must measure the twin roles of internal pressure from the firm's product market (π/n) and external pressure from the labor market in which the firm operates ($\overline{w}$)[11].

In model 1's equations, for example, the conceptual framework is a bargaining analysis in which rents are divided between the employer and the workers. Prosperity in the product market leads to a large surplus to be divided. As firms get richer, the level of pay increases. By contrast, high unemployment in the firm's local labor market will reduce workers' relative bargaining strength. As employees become weaker, the level of pay falls.

The literature typically begins with a linearization of equation (7). Assume that the underlying wage equation of interest takes the form

$$(26) \quad w_{it} = k_i + \alpha w_{it-1} + \beta U_{jt} + (\gamma\pi/n)_{kt} + \delta Z_t + \varepsilon_{it}$$

where w_{it} is the real wage for a worker or labor 'contract' beginning at time t for the i' th pair of firm and employee (or perhaps union), w_{it-1} is the real wage in the previous contract of pair i, U_{jt} is the regional unemployment rate at time t relevant to the pair in region j, π/n_{kt} is a measure of profitability at time t in the kth product market, Z_t is a vector of other relevant variables (which also may be indexed by i, j or k) such as the education level of the workers, and epsilon is a pair-specific error term. In many settings the Z variables are likely to have to include other personal characteristics of the firm's workers. To eliminate the pair-specific fixed effects k_i, most authors difference over contract years to get

$$(27) \quad \Delta w_{it} = \alpha \Delta w_{it-1} + \beta \Delta U_{jt} + \gamma \Delta(\pi/n)_{kt} + \delta \Delta Z_t + v_{it}.$$

This equation, with the original variables in logarithmic form where appropriate, and allowing for a moving average error term v_{it} and the possible correlation between Δw_{it-1} and v_{it}, is the structure used in the majority of modern work in this field.

Most authors are aware of the potential pitfalls in estimating this kind of equation. There are, in particular, a number of reasons to instrument the profitability variable. First, it may be wrong to believe that the quasi-rent-splitting parameter, given by the ratio of phi's in equation (6),

[11] Card (1990) is somewhat similar, but he does not look at profits, and is ultimately concerned with a different set of issues.

is the same across firms. Second, quasi-rents may be measured with error. Third, there may be straightforward endogeneity bias, because the employer's quasi-rent per employee may depend upon wages. In one particular case, there is no such third bias. That is where there are so-called strongly efficient bargains (which, in fact, Abowd's (1989) evidence supports). These have the characteristic that employment is independent of the wage. In such a contract, the level of quasi-rents is a sufficient statistic for the effect of product market conditions on the firm's ability-to-pay (as in equation 6). [12]

Some researchers ignore the simultaneity problem. They estimate the equation by, for example, least squares dummy variables or some other OLS-based panel estimator. This approach can never be entirely satisfactory. However, its proponents claim that it has down-to-earth merits. One is transparency: we know what we are getting. This is probably a difficult argument for most young applied economists to understand. It tends to be favored by experienced researchers (who perhaps believe they can make instinctive adjustments in their heads). Another is that, given the likely direction of simultaneity bias, any positive coefficient on profits is likely to be an underestimate of the true coefficient. Thus even an OLS estimate of differenced equation (27) may be informative.

Other researchers argue that it is sensible to estimate the wage equation using lagged measures of profitability. These, it can be claimed, are less open to the charge that estimation is invalidated by the endogeneity of profits. If profits-per-worker in 1994 can be treated as predetermined, then they can be used as an explanatory variable in an equation for 1996 wages. A variant on this -- it could be viewed as the search for a 'statistical' instrument -- is to estimate the equation using lagged values of profitability to instrument the current π/n variable. This approach is popular in Great Britain. It tends to be viewed more sceptically in the USA -- on the grounds that a lagged variable is at best an ad hoc instrument, and that the instrument-validity tests commonly used to support it often have low power to reject the null.

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[12] Abowd and Lemieux define quasi-rents as 'profits'-per-employee where the wage is set to the alternative rather than actual level.

Finally, simultaneity can be taken seriously. For that, an 'economic' instrument has to be found. In other words, the researcher has to discover a sensible economic variable -- not a statistical variable like lagged profits or something equally mechanical -- that is a shifter in the profit equation but does not enter the wage equation directly. The earlier mathematics makes it clear that this instrument probably has to capture the demand shock μ. In principle, to identify this kind of equation, data must be available to the econometrician on the exogenous variables in the firm's product demand curve. In practice, however, that can prove problematic.

5. Estimation without an 'economic' instrument

Consider estimating equation (27) by using not the current level of profit-per-employee but lagged levels of π/n (such as annual lags from t-1 to t-3). With profit-per-worker as the independent variable, an estimated equation might be interpreted as a dynamic version of the theoretical equation (6) with a pre-determined level of profitability.

The modern papers that do this find well-determined effects. In other words, the π/n variable usually enters positively in a wage equation, with a standard error that is less than half its coefficient. Blanchflower, Oswald and Sanfey (1996) is representative[13]. For their US study, no suitable 'matched' microeconomic data exist, that is, there are no statistical sources reporting information both about U.S. employees and their employers. Yet most economists would wish personal characteristics to be entered as independent variables in a wage equation. For this reason, the paper splices together microeconomic and industry-level data, drawing upon random samples of workers in U.S. manufacturing industry. Table 2 reports equations from Blanchflower et al. Here the dependent variable is the logarithm of workers' hourly earnings.

Columns 1 and 2 of Table 2 can be thought of as a natural test for the existence of rent-sharing in the labor market of the United States. The estimated equations include a dummy variable for each year, to control for economy-wide movements from 1964 to 1985. The equations

[13] Sanfey's thesis does a fixed-effects analysis on industry data.

also include a conventional set of demographic and educational variables for the individuals in the sample. This is, of course, to guard against the possibility that highly profitable sectors pay well merely because they employ people with unusually high levels of human capital. To control for industry fixed effects, the estimation includes a dummy variable for each industry. Because of occasional negative values, the profitability variable is not entered as a logarithm. The bottom line of the analysis is that there are strong effects from lagged π/n. Taken at face value, Table 2 is evidence for rent-sharing in the United States.

It is of particular intellectual interest to discover whether there is rent-sharing in labor markets with little or no unionism. Hence Blanchflower et al also look at a sub-sample of industries that pay poorly. The motivation for this is that the results for the full sample contain some unionized sectors and it might be that the rent-sharing finding is being driven simply by that portion of the data. Although a variable for unionism cannot be defined from the CPS for use in the regressions, simple exclusion of high-wage manufacturing industries is feasible. It can be thought of as throwing out a disproportionate share of unionized employees. The remaining sub-sample is then very largely non-union. Doing this, Blanchflower et al find the quantitative impact of profits becomes even larger than in the full sample. A representative estimate of the long-run elasticity of wages with respect to profit-per-employee in the US manufacturing low-wage sector is 0.08. In this sector, therefore, a doubling of profit-per-person would imply a rise of 8 per cent in the wage.

Table 2's coefficients looks like evidence for the existence of rent-sharing. Is it large? A natural calculation is to examine the consequences of a movement in profit-per-employee from one standard deviation below the mean to one standard deviation above. In the Blanchflower et al data, this is an enormous change in π/n from 1.9 to 19.3, or, in other words, over a nine-fold movement in profitability. Using the central parameter estimate, the elasticity of w with respect to π/n is just under 0.02. A change from one standard deviation below the mean of profitability to one standard deviation above the mean level of profitability is therefore associated approximately with a 7 per cent increase in pay. Given that the paper's methods control for fixed effects, it is appropriate to think of this as the wage difference for identical US workers between two sectors with different profitability. If the width of a distribution can be thought of as four standard deviations, the "range" of wages due to rent-sharing in U.S. manufacturing is then close to 24 per cent of the mean wage. Modern econometrics thus apparently confirms the intuition of Lester (1952).

It would be reasonable to inquire whether many other studies have obtained these kinds of results. The answer is that they have (though most other investigators have been unable to control for personal characteristics). Christofides and Oswald (1992) examine contracts reached between four hundred establishments and seventy unions across a variety of Canadian industries. Currie and McConnell (1992) do the same for over a thousand United States contracts negotiated in the 1970s. Blanchflower, Oswald and Garrett (1990) have a randomly chosen cross-section of twelve hundred British establishments covering the public and private sectors. Nickell and Wadhwani (1990) tackle these same issues with data drawn from the published accounts of two hundred British publicly-quoted manufacturing companies over the years 1972-1982. Denny and Machin (1991) use an unbalanced sample of four hundred manufacturing British companies from the mid-70s to the mid-80s. Panel data three hundred UK companies and establishments are used by Hildreth and Oswald (1994). As Table 1 shows, there are strong similarities in these papers' findings.

There is some instrumenting in this part of the literature, but it is largely of a mechanical sort that relies on lags. The next section discusses alternatives.

6. The remarkable consequences of having an economic instrument for the profitability variable

In the last few years, researchers have attempted to estimate equation (27) in a different way. They have looked for economic variables to measure μ, the demand-shock variable, and used those to instrument profitability. The most surprising conclusion from such studies is one that is not always mentioned by authors: rent-sharing effects turn out to be enormous. According to some of the studies, almost all the variation in wages observed in the data is due to rent-sharing.

If the studies had been badly designed, or used poor data, it would be easy to dismiss such claims. But they have not. While rent-sharing effects this large are theoretically possible, their size would be viewed as remarkable, and perhaps unbelievable, by the large numbers of labor economists who stress the central role of human-capital differences in the determination of pay.

As an example, it is revealing to begin with the work of John Abowd and Thomas Lemieux (1993). It is not uncommon for economists who start literatures to exaggerate earlier research's weaknesses, and, perhaps contentiously, Abowd and Lemieux (1993) comes close to asserting that previous authors were unaware of the need to instrument profitability, and that in the prior literature the estimated amount of rent-sharing is trivial.

Neither of these points seems to be true. Even the earliest of the modern literature, such as the book by Carruth and Oswald (1989) and the papers by Blanchflower et al (1990) and Nickell and Wadhwani (1990), discuss simultaneity bias. The first and third of these use instruments for just the reasons promulgated by Abowd and Lemieux; the second cannot, but suggests that the direction of bias is known. Moreover, it appears to be misleading to describe the literature's estimated effects as tiny. The literature suggests that someone working in a profitable establishment would as a result of his or her employer's prosperity earn a premium of around 5%-20% of pay. While some economists might call this small, few would view it as inconsequential[14].

Instrumental variables for quasi-rents have to represent the exogenous demand shocks -- the μ variables from section 3 of this paper -- that affect profitability.[15] Candidate instrumental variables also have to be uncorrelated with the error term in the wage equation. What is needed is an external "shock, or a natural experiment, that hits the industry independently of behavior",

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[14] Abowd and Lemieux assert that Christofides and Oswald (1992) find rent-sharing effects of less than one cent per hour. This assertion is unfounded. Abowd and Lemieux may accept (private communication) that the correct size is many times this. As Christofides and Oswald is estimated in differences, it is not strictly possible to make any statement about the proportion of hourly pay that can be attributed to rents. What can be done is to ask how much of wage changes can be due to rent-sharing effects. As the previous section showed, the spread of wages induced by variations in profits is estimated by Christofides and Oswald to be a little over 5% of average pay. At the mean, therefore, it might loosely be claimed that rents are more than 2% of the wage. With a mean wage in the data set of $9 per hour, this implies rents of roughly 20 cents an hour.

[15] In one special case this method will fail: some constant elasticity production functions lead to quasi-rent/employee being independent of demand shocks.

p.991, Abowd and Lemeiux (1993). It might be thought that the industry price of output would be a suitable instrument. In most practical circumstances, however, prices will be a function of wages. If a negotiating union is large enough, the wage in the labor contract may influence the industry price.

There are four important papers in this new genre, and each is based on the use of a different instrument. The papers are, where the instrument for profitability is in italics, as follows:

Estevao and Tevlin (1995): *output movements in the sector to which an industry sells*. [Intuition: Consider the industry for manufactured ice. A natural instrument is the output level of the fishing industry. When fishing boats bring back more fish it is necessary for fishermen to buy more ice. This is a positive demand shock in the ice industry, so profits in the ice industry should rise, but this should not affect ice workers' wages through any direct channel. Hence profits in the manufactured ice industry can be instrumented with fishing boats' total production.]

Abowd and Lemieux (1993): *the prices of exports and imports*. [Intuition: When there is a rise in the world price of lumber, for example, that raises profits in Canadian lumber firms, who are approximately price takers. It may thereby lead indirectly to a different level of pay among those in the wood-using industry. It does not do so directly, because the price of lumber should not enter the wage equation for wood-products workers.]

Van Reenen (1996): *the number of major innovations made by that company* in previous periods. [Intuition: The author's idea is that innovations create upward shocks to profits and, by that channel, feed through into higher pay for workers. The attraction of Van Reenen's method is that it is hard to see a reason why past innovations would enter directly into a wage equation for today.]

Teal (1996): *the employer's overdraft, the amount borrowed from banks, etc* [Intuition: The author's seemingly reasonable assumption is that these things are highly correlated with the financial performance of an establishment but that they affect pay only through their impact on profit-per-worker.]

It is useful to return to Table 1. In this table, I calculate the estimated size of the rent-sharing effects that different papers estimate. In a few cases this requires some mild guesswork, because authors do not always report in a consistent way. It can be seen that the four papers that use an economic instrument (starred in the table) produce large estimates of the rent-sharing parameter. Indeed, the estimates are sufficiently big that they imply that nearly all the variation in the raw wage data are explained by differences in employers' profitability. My judgment is that most of the authors were unaware of this when first writing up their papers.

When, for example, John Abowd and Thomas Lemieux regress wages on quasi-rents and the other variables discussed above, the OLS estimate of the coefficient on quasi-rents is positive but small. It varies from 0.003 to 0.008 (their Table III, p. 1001). The t-statistics vary from unity to two. This seems to indicate a relatively minor and not especially well-determined amount of rent-sharing. However, switching to two-stage least squares (instrumenting quasi-rents with import and export prices, as explained) transforms the numbers. The estimated coefficient on quasi-rent per worker increases up to fifty-fold. The instrumented estimates of the rent-splitting parameter then vary from 0.23 to 0.39. They are fairly well-defined. The lowest of the coefficient estimates occurs when year dummies are included, which might be thought of as the most general and therefore most convincing specification. However, the t-statistic on quasi-rents becomes less than 1.5.

Abowd and Lemieux thus find a high degree of rent-sharing. The authors' favored estimate of the rent-division parameter, akin to the ratio of the phi's in equation (6), is approximately 0.2. This emerges from a specification (their column 5 of Table IV, p. 1002) in which the previous contract wage enters as an independent variable and there are dummy variables for each year. Abowd and Lemieux take the view that there is an extra reason to take seriously the estimate of a rent-sharing coefficient of 0.2. The mean of quasi-rents per worker in their sample is $8.7 per hour. Twenty per cent of $8.7 is approximately $1.7, which is the implied premium that the average worker earns. This premium can be thought of as the excess of the negotiated wage over the alternative competitive wage. The mean wage in the data is $7.9 per hour. Hence, from their calculations, the implied competitive wage is $6.2 (namely, $7.9 - $1.7). The average wage premium created by rents is thus, as a proportion, 1.7/6.2, or in other words 28%. This is close, they say, to existing cross-section estimates of the union/non-union wage differential in Canada, which are of the order of 20% to 25%. Such a match is viewed by the authors as corroboration.

As Table 1 reveals, the other three main papers of this sort come to remarkably similar conclusions. Teal (1996) draws upon a new data source from Ghana that is close to being a matched-panel. The consequences of intrumenting per-head profits are striking. Its coefficient rises more than five-fold from 0.05 to 0.27. Similar findings occur in British data. Van Reenen (1996) draws upon a panel of 600 large British firms from 1976-82. When Van Reenen switches from OLS to estimation with instrumental variables, the coefficient on quasi-rents increases ten-fold from 0.03 to 0.3. New evidence from Estevao and Tevlin (1994) are reproduced in Table 3. First, the general conclusion of authors from Slichter (1950) to Blanchflower et al (1996) is confirmed. The competitive model's null hypothesis, that $\beta_0 = 0$, is rejected. Second, using ordinary least squares, the coefficient on profit-per-employee is positive and well-determined. At approximately 0.05, the OLS profit-elasticity of pay is larger than that in much of the modern literature described earlier in this paper. Third, once again, instrumenting the profit variable dramatically increases the elasticity of pay with respect to profits. Following columns 3 and 4 of Table 3, the elasticity is close to 0.35.

In a sense, these kinds of results might be viewed as highly encouraging. As theory predicts, instrumenting profits or quasi-rents noticeably raises the coefficient in a wage equation. Moreover, the coefficients are similar, and well-determined, in the four studies.

There are technical difficulties. In their discussion of the size of effects, Abowd and Lemieux appear to have neglected to allow for the lagged dependent variable in their wage equation. Therefore, contrary to the spirit of the cross-section union differential literature against which they wish to be compared, their estimate is not long-run. It is straightforward, however, to calculate a long-run figure. The lagged dependent variable in their favored specification is 0.31. Consider the steady-state equilibrium when all lagged-wage effects have fed through. In that case, the (long-run) coefficient on quasi-rents is equal to the authors' favored 0.2 multiplied by the inverse of 0.69. Then the authors' conclusions are awry by a factor of roughly one half. Working this through, the authors' methods indicate that we should expect the Canadian union wage-differential literature to have produced estimates as large as 45%. It has not done so.

A second objection to Abowd and Lemieux is that the authors' chosen coefficient of 0.2 is close to the bottom of their own estimates. This would not matter if there were some powerful reason to believe that their high estimates are flawed. That, indeed, is what they implicitly argue. Nevertheless, others might read their results and conclude that long-run coefficients approaching 0.4 or more are equally representative of these data. That would imply that the cross-section union differential should have been discovered to be dramatically greater than has been produced in the literature. If these objections are to be taken seriously, Abowd and Lemieux's chosen coefficient under-states the magnitude of rent-sharing.

To understand this, a calculation has to be done. The mean and standard deviation of pay in the authors' data are, respectively, approximately \$8 and \$1.7. The mean and standard deviation of quasi-rents are, respectively, approximately \$8 and \$8. Beginning from the mean level of quasi-rents, therefore, a one-standard deviation rise is a doubling of quasi-rents. With the authors' coefficient of 0.2, this translates into an implied hike in pay of \$1.6. Yet the raw data reported in Abowd and Lemieux (1993) imply that a one-standard deviation move up the wage distribution is a rise of \$1.7. Thus the dispersion in quasi-rents accounts for almost all pay dispersion. Moreover, as explained, there are reasons to read Abowd and Lemeiux as finding a much greater coefficient than 0.2. This quickly leads to a point where rent-sharing purports to explain more wage variation than exists in the data set.[16]

In private correspondence, John Abowd and Thomas Lemieux have put forward a good counter-argument, or, more precisely, an interesting interpretation that could make sense of the facts. They believe that there is a great deal of measurement error in the quasi-rent data. This, they suggest, means that the observed variation in quasi-rents is larger than the true variation and thus that it is misleading to multiply standard deviations as just done. There may be something to this.

[16] This is logically possible. It requires high-education levels (for example) to occur disproportionately in very unprofitable firms.

It seems unobjectionable to suggest that there is likely to be measurement error. Unfortunately, even if the dispersion of the true quasi-rent distribution were, say, only half that in the observed quasi-rent distribution, the estimated size of rent-sharing effects would still be remarkably large. Moreover, any measurement error in the wage distribution would presumably work against the authors' rebuttal, by raising their estimate.

It is not easy to know what to make of the large estimates that have emerged from this part of the literature. The intellectual case for instrumenting profits or quasi-rents is a strong one. Some increase in the coefficient would be expected when moving from OLS methods (indeed, that is the reason why instrumenting is desirable). Yet, for reasons that are difficult to understand, the estimates are too large to be easily swallowed.

Part of the problem may stem from most studies' lack of controls for personal characteristics. It is conceivable, as Francis Kramarz has suggested to me, that these characteristics are highly correlated with the profitability of the employer.

7. Objections to bear in mind

A sceptic might think of a number of objections to this whole line of empirical work. Here are some -- alongside their counter-arguments.

One is that a positive relation between wages and profit-per-employee might arise if the analysis is unconsciously estimating a form of labor demand curve (in the Cobb-Douglas production case, for example, the existence of a positive wage/profit-per-employee correlation is easy to demonstrate)[17]. Four arguments can be offered against this view. First, long estimated lags on the profit-per-employee variables make it difficult to interpret causality as running from w to π/n. Second, instrumenting should help to solve the implied simultaneity. Third, reversing and re-estimating these equations with π/n on the left-hand side can be shown in studies like Hildreth

[17] Alan Manning has suggested to me that wage equations are generically unidentified.

and Oswald's to produce negative rather than positive coefficients on the wage variables (this may be related to a negative wage/ profit-margin association found in Allen (1992)). Fourth, the labor demand interpretation does not square with the fact that total profit can also be shown to enter positively on the right-hand side of the pay equations (although ideally some normalization of profit is required, fixed effects here will play that approximate role). Along a labor demand curve, by contrast, total profits are a declining convex function of the wage.

To use company panels, a number of special steps have to be taken. One of the most worrying is that it is necessary to assume that a meaningful average wage can be created by dividing the firm's wage bill by the size of its total workforce. This mixes together the price of apples and the price of pears: a figure for average-remuneration necessarily aggregates across the firm's cleaners, engineers, secretaries, and chief executive officer. Most authors do not discuss this difficulty in any detail. Their view may be -- perhaps reasonably -- that some of the potential difficulty is reduced by the ability in such a panel to include in the wage equation a set of controls for firm-specific fixed effects. As long as the structure of, and relative remuneration within, a company stays constant over time, the lack of disaggregated information on pay may not matter. Average pay is thus a sufficient statistic. In a sense, changes in the firm's average level of pay will then be uncontaminated by the lack of data on the nature of the workers who make up the average, so that a regression run in, for example, differences need not lead to biased estimates. Nevertheless, especially for large conglomerates engaged in acquisitions, the methodological approach is open to the criticism that there may be systematic changes over time in the composition of firms' workforces. A second difficulty in, for example, Nickell and Wadhwani's work is that a key independent variable, sales revenue-per-employee, is defined by dividing by employment. Thus, if there is any measurement error in the firm's level of employment, a spurious positive correlation will tend to arise between the average wage and the level of sales per person. That is an especial weakness here, because the paper wishes to the argue that there is an economic case for just such a positive correlation. However, it is likely that the authors would wish to argue that their instrumenting acts to emasculate the effects of the measurement error.

It might be argued that the significance of a profit-per-employee term simply reflects the existence of a particular form of omitted-variable bias. It is known that size-of-workplace enters positively in cross-section earnings equations, so it could be that employment, n, should be included (John Pencavel, for example, raised this question in an earlier draft). However, the estimated positive π/n terms mean that n enters with the wrong sign for this to be an explanation of π/n's significance. As an additional check, in the spirit of Reder (1962), employment can be entered as an extra independent variable in specifications. In Hildreth and Oswald (1997), it is always insignificant and negative without affecting the other coefficients.

A further possibility is that the correlation between profits and pay may be the consequence of omitted variables for worker quality. Potentially this is a serious possibility. In the long-run, economists will rely on matched panels to address it. Two counter-arguments, however, are that fixed effects should take out much of the potential bias and that Teal (1996) and Blanchflower et al (1996) find that entering personal and educational controls does not influence the effect from π/n. Another response is that there is no reason why, in a competitive market, high-skill workers will work in high-profit firms (their higher wages and greater productivity should cancel out). Thus, although bias of this general type is always a possibility without matched worker/firm data sets, it is hard to see how it could explain the findings.

Although most data sets do not record hours of work, variation in intensity of work also seems to be incapable of accounting for the authors' long-run results. A competitive framework might require employees to raise their effort or hours in a boom, but that should, by the usual arguments, be a temporary phenomenon. There could be difficulties with measurement error. The instrumented and lagged specifications used earlier, however, should obviate the potential measurement error problem caused by the fact that the wage variable is created using employment as a divisor. An even better reason to doubt that this is at the heart of the paper's key result is that the equations with total profit, π, also find well-determined positive profitability effects on pay. Finally, one study, Blanchflower et al (1996), does control for hours.

Finally, it is not unionism that is doing it all. Authors such as Hildreth and Oswald re-estimate their equations and interact the profit terms with dummy variables for the existence of unionism throughout the time period. The aim is to investigate whether rent-sharing effects are found only in the union sector. Profitability effects appear to be larger in less-unionized firms and establishments. It is unclear whether this reflects the known tendency of union pay levels to be less flexible to all kinds of shocks, but it does seem to suggest that these rent-sharing results are not being driven by the highly unionized workplaces in the data.

A famous objection to the kind of rent-sharing analysis proposed here is that the true model is a competitive one with slow adjustment (see model 2). According to such an argument, profit movements and wage movements may occur together, but this is merely because in the short-run a boom takes the industry up an upward-sloping labor supply curve. One counter-argument is that the Blanchflower et al tables suggest a long-run relationship, as demonstrated by solving the implied difference equations for their steady-states, between workers' remuneration and their industries' prosperity. A different test can be done by incorporating into the regression equations a set of current and lagged employment variables. If profitability is acting -- as in a competitive framework with gradual adjustment -- simply as a proxy for movements along a labor supply curve, the inclusion of employment and its rate of change should destroy the statistical significance of a variable like π/n. This is Reder's (1962) desired test. Blanchflower et al (1996) perform it. A set of employment terms (from n(t) to n(t-3)) is incorporated in the earnings equation. Despite the significance of some of the employment variables, the coefficients on the profitability terms are barely affected. This is not what the competitive model predicts.

Different authors have used different measures of profits and of quasi-rents. The right way to define profits according to the theoretical models is the amount of true surplus to be divided between the firm and its employees. Implicitly, this paper requires that the authors' differing treatments of profits are not sufficient to explain the substantive differences in their economic claims.

The right way to define the short-run versus the long-run might also be viewed as debatable. This issue matters, because the augmented competitive model with frictions is consistent with temporary profit-effects. Throughout this paper the assumption is that "the long-run" corresponds to the steady-state solution of the difference equation estimated by regression methods. If this is the wrong thing to do, then the paper's judgments may be faulty.

8. Interpretation and other kinds of evidence

What binds these papers together is the common test they employ, namely, to estimate a version of equation (6) and to show that the data imply a long-run positive correlation between pay and profitability. Unlike research prior to the 1990s, the new work has a natural kind of microeconomic data (the full optimum would be to exploit a matched worker-firm panel).

Each of the papers[18] suggests that a range of wages is produced by the fact that different employers have different degrees of profitability. The estimated size of this range varies markedly -- see Table 1 -- from one study to another. In one sense, this variety is unfortunate. In another, it is probably unsurprising. First, the majority of the papers described here are so new that they are only just appearing in print. The field has barely begun. We have little feel for how to compare the elasticities produced with alternative forms of micro data. Second, the most radical differences occur between the papers that use special instruments and those that do not. There is a good case for instrumenting a profit or quasi-rent variable in a wage equation, and four recent papers show imagination in hitting upon instruments for which a strong intutive argument can be made. Nevertheless, parts of economic research have discovered that the use of certain instruments can generate parameter estimates that are eventually viewed by the profession as implausibly large. It is too early to say whether that will happen here, but it is possible.

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[18] Related work that cannot be discussed here includes Abowd et al (1994), Dowrick and Mumford (forthcoming), Ghosal and Loungani (1996), Hartog et al (1994), Hussain and Zhuang (1994), and Wulfsberg (1993).

Those sceptical of microeconometric research may be reassured by the existence of complementary forms of evidence. There are other sources of information on a wage-profit correlation, and some industrial relations researchers are likely to see these results as establishing statistically a relationship that they have observed many times in actual wage-setting. Blanchflower and Oswald (1988) documents direct questionnaire evidence of this type. It shows that, when making decisions about how high to fix their workers' pay, British managers say they give a larger weight to profits than to any other consideration. That is true in the non-union sector as well as the union sector. Similar findings have emerged recently in innovative field-work in Sweden by Agell and Lundborg (1993). Experiments like those in Bazerman (1985), which indicate that real-life pay-setters are swayed by notions of fairness in the division of a pot of potential rents, point in the same direction. Carruth and Oswald (1987, 1989) summarizes further sorts of evidence for profit-effects on pay, including estimates from time-series equations. Pencavel's (1990) criticisms of such work are slowly being answered by new microeconometric research.

Reder's (1962) attack on the work of Slichter and his contemporaries helped to turn an earlier literature away from non-competitive views and back toward the competitive framework. His main point was that empirical work had failed to control for employment fluctuations, so that the tests ostensibly favoring rent-sharing might merely be revealing a temporary move up the labor supply curve in booming industries. However, this objection no longer stands, because the new generation of work has controlled for employment.

If the empirical message of this paper -- that a new generation of work seems to have established the existence of rent-sharing -- becomes accepted, that will raise a major puzzle for economic theory. The reason is that, unexpectedly, equations like (6) and (7) apparently fit the data for non-union sectors. Somehow the findings discussed here are going to have to be linked back to theoretical work on how rents can persist without unions[19]. Our knowledge here remains rudimentary, but the data offer a set of facts upon which theoretical work can build.

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[19] Shapiro and Stiglitz (1984) is one example, McLeod and Malcomson (1993) another. Malcomson (1994) offers an overview.

9. Conclusions

This paper describes evidence for rent-sharing in the labor market. It discusses a set of recently-minted research papers.[20] The papers draw upon a style of information denied to earlier generations of researchers, namely, microeconomic and longitudinal data on contracts, establishments, firms, and people. They estimate versions of the simple wage equation:

$$(6') \qquad w \cong \overline{w} + \left(\frac{\phi}{1-\phi}\right)\frac{\pi}{n}$$

where, in natural notation, the employer's equilibrium wage is shaped by a mixture of outside wage opportunities and profit-per-employee. Contrary to the prediction of the competitive wage-taking model, these studies find profit-per-employee to matter in wage determination[21]. The studies have largely been produced in unknowing isolation from one another; they are only now beginning to appear in journals; they cover various countries and settings. Yet, intriguingly, the studies paint a consistent picture.

At the literature's heart is the following testable hypothesis. When an employer enjoys a sustained exogenous burst of profits, does that feed through into higher long-run pay for its employees? The new literature summarized in Table 1 suggests that, contrary to the wage-taking prediction of the competitive model, the answer is yes. This appears to be true in non-unionized as well as unionized settings. Hence there may be empirical grounds to apply bargaining models in more widespread circumstances than has been presumed.

It is sometimes claimed that the labor market is 'obviously' non-competitive. This is more easily said than proved. Perhaps for that reason, such claims have left comparatively little impression[22]. What seems to make the new literature more persuasive is the quality of its

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[20] As explained in the text, this is a second generation that is emerging post Dickens-Katz-Krueger-Summers.

[21] Specialist labor economists will be reassured that this is not merely because of an omitted variable for firm size.

[22] Those who doubt this might look at the recent wage-determination special issue of the Quarterly Journal of Economics (February 1992). No paper there adopts a noncompetitive model.

underlying data and methods. While no single study is free of flaws, weaknesses in one appear to be covered by strengths in another. Especially dramatic findings are produced when researchers correct for endogeneity with a sensible economic variable: John Abowd and Thomas Lemieux use import and export prices to instrument profitability; John Van Reenen uses firms' earlier technical innovations; Francis Teal uses firms' financial and banking characteristics; Marcello Estevao and Stacey Tevlin use output movements in sectors from which the employers' goods are purchased. On the face of it, this kind of work is an important methodological and substantive contribution to applied economics. However, at the time of writing, there is a difficulty with this class of papers. Their estimated rent-sharing effects are perhaps too large to be believable.

If the impact of profits upon pay were statistically significant but small, the material examined in this paper would be of intellectual interest but not of great practical import. The competitive model would be close enough to the truth to be sufficient. However, this is not what emerges from Table 1. Some authors' results, indeed, imply that the majority of the wage dispersion in the real world is due to rent-sharing. Even if such remarkable conclusions do not survive future scrutiny (my instinct is that they will not), they hint at the importance of this area, and at likely controversy ahead.

Table 1 Recent Microeconometric Tests for Profits (or Quasi-Rents) in a Wage Equation

Study	Dependent Variable	Data on	Country	4SD $\Delta\pi$ raises pay	Fixed effects	Sample size approx
Blanchflower et al (1990)	Weekly earnings	Establishments	Britain	15%	No	1100
Denny & Machin (1991)	Average pay	Firms	Britain	4%	Yes	2000
Christofides & Oswald (1992)	Contract wage	Union contracts	Canada	6%	Yes	600
Currie & McC -onell (1992)	Average pay	Union contracts	USA	not given	Yes	1300
Abowd & Lemieux (1993)	Contract wage	Union contracts	Canada	90%*	Yes	1100
Hildreth & Oswald (1994)	Average pay	Firms and establ -ishments	Britain	16%	Yes	3300 and 400
Teal (1996)	Weekly earnings	Establishments	Ghana	100%*	No	700
Van Reenen (1996)	Average pay	Firms	Britain	120%*	Yes	2600
Blanchflower et al (1996)	Hourly earnings	Workers/indus. π	USA	24%	Yes	400
Estevao & Tevlin (1994)	Annual earnings	4-digit industries	USA	70%*	Yes	1700
Nickell & Nicolitsas (1994)	Average pay	Firms	Britain	25%	Yes	200

Note: These report coefficients on profit or quasi-rent variables in microeconometric wage equations. The equations are estimated using the numbers of observations stated in the final column.

The heading "4SD $\Delta\pi$ raises pay" gives my best estimate or guess of the approximate percentage rise in wages induced by a move up the profit or quasi-rent distribution of four standard deviations. It can be thought of as the estimated spread of pay that is produced by rent-sharing.

A paper by Nickell and Wadhwani (1990) mentions in a footnote that it finds no effect from profits. It does, however, obtain a positive sales-per-employee variable.

* denotes π variable is instrumented by something other than lagged values of independent variables

Table 2 Hourly Log Earnings Equations for U.S. Manufacturing, 1964-1985

Dependent variable: log w_t

	(1)	(2)	(3)	(4)
Log unemployment U_t	-0.012	-0.0091	-0.0284	-0.0291
	(0.84)	(0.68)	(2.91)	(3.16)
Profit per head $(\pi/n)_t$	-0.0021			
	(0.80)			
Profit per head $(\pi/n)_{t-1}$	0.0023	0.0054	0.0025	
	(0.75)	(2.77)	(1.82)	
Profit per head $(\pi/n)_{t-2}$	0.0064	0.0071		0.0030
	(2.53)	(3.56)		(2.26)
Log w_{t-1}	0.2393	0.2402	0.4264	0.3572
	(3.83)	(3.72)	(7.68)	(6.66)
Year dummies	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
R^2	0.99	0.99	0.91	0.92
DF	225	257	237161	237147

Source: Blanchflower et al (1996), Tables II and III, using data on 200,000 workers from the US Current Population Surveys.

There is no instrumenting in this Table. Columns 1 and 2 estimate regressions using industry/year cell means. Columns 3 and 4 estimate regressions using as the dependent variable the unexplained component from a first-stage micro regression. Profit-per-employee is in levels not logs. The unemployment rate is for the industry.

t-statistics are in parentheses.

Table 3 Industry Wage Equations for U.S. 4-Digit Manufacturing, 1964-1986 : Estevao and Tevlin 1994 (from their Table 2, p.27)

	(1)	(2)	(3)	(4)
Log profit-per-head $(\pi/n)_t$	0.048	0.054	0.551	0.354
	(0.006)	(0.006)	(0.120)	(0.063)
Log US unemployment	-0.007		0.075	
	(0.007)		(0.026)	
Log US average wage	1.179		0.434	
	(0.55)		(0.222)	
Annual time trend	Yes	No	Yes	No
Year dummies	No	Yes	No	Yes
Personal controls	No	No	No	No
Industry fixed effects	Yes	Yes	Yes	Yes
Estimation	OLS	OLS	IV	IV
R^2	0.26	0.28		
N	1703	1703	1703	1703

Source Estevao and Tevlin (1994)

Data source Wayne Gray's Productivity Database compiled using the Annual Survey of Ma nufactures

Notes The dependent variable is the log of the annual average earnings per-employee in the relevant industry, deflated by the CPI. Unemployment is the unemployment rate in the economy, and the average US wage is the average wage in manufacturing deflated by the CPI.

The sample consists of all workers. There is no instrumenting in columns 1 and 2. In columns 3 and 4, the profit-per-employee variable is instrumented with a vector of demand shift variables and the other variables in the relevant column equation.

The equations are estimated in differences to control for industry fixed-effects.

Standard errors are in parentheses.

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