NBER WORKING PAPER SERIES

THE EFFECT OF THE PAYROLL TAX ON EARNINGS: A TEST OF COMPETING MODELS OF WAGE DETERMINATION

Kevin Lang

Working Paper 9537
http://www.nber.org/papers/w9537

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2003

I am grateful to Eli Berman, Kerwin Charles, Stephen Donald, Chris Foote, Don Fullerton, Caroline Hoxby, Larry Katz, Larry Kotlikoff, Steve Levitt, Michael Manove, Andy Weiss and participants in seminars at Boston College, Boston University, Columbia University, Duke University, Harvard University, MIT, Northwestern and the NBER for helpful comments. The usual caveat applies. The views expressed herein are those of the authors and not necessarily those of the National Bureau of Economic Research.

The Effect of the Payroll Tax on Earnings: A Test of Competing Models of Wage Determination
Kevin Lang
NBER Working Paper No. 9537
February 2003
JEL No. J0, J3, H2

ABSTRACT

Under the standard competitive model, a tax change affecting workers with highly inelastic labor supply, will lower earnings by the entire nominal employer share of the tax increase. If wages play a motivational role but the market still clears, the range of possible outcomes is broader but wages should still not rise if the tax is nominally divided 50/50. In contrast, because there is excess labor (involuntary unemployment) in equilibrium, efficiency wage models resemble models in which labor supply is perfectly elastic, and thus earnings rise by more than the worker's nominal share. The 1968, 1974 and 1979 increases in the taxable earnings base for FICA provide good opportunities to test the models. This tax increase affected only those workers earning significantly more than the median earnings for male full-time/year-round workers. Such workers' labor force participation is likely to be highly inelastic. The results support models in which the motivational effects of wages are important but cannot clearly distinguish between the efficiency wage and market-clearing versions of those models.

Kevin Lang
Department of Economics
Boston University
270 Bay State Road
Boston, MA 02215
and NBER
lang@bu.edu

1 Introduction

Labor market models can be loosely divided into those in which wages serve only to allocate workers' time to different activities including jobs and those in which wages play a wider range of roles such as motivating workers or attracting workers with greater unobservable skills. In the former models (see for example, Rosen, 1986), markets for all jobs clear. Any wage differentials reflect productivity differences associated with the types of skills workers bring to the job or the conditions under which they work. In contrast, when wages play a wider role, the market may fail to clear if the effect of wages on productivity is sufficiently large. If so, wage differentials may reflect luck or exceed those needed to compensate workers for bad working conditions. In general, the market is efficient in models in which the labor market clears but inefficient in those in which it does not.

While it is natural to focus on the extreme versions of the models – the standard competitive model in which wages do not affect productivity and efficiency wage models in which the productivity effects are sufficiently large to preclude market-clearing – there is actually a continuum of models in which wages affect productivity but the labor market clears. The focus in this paper will be on models in which the wage can be viewed as entering the production function directly. Such models include theories in which higher wages induce more effort by deterring shirking (Shapiro/Stiglitz, 1984) or increasing worker morale (Solow, 1979; Akerlof, 1984). When the productive effects of wages are sufficiently large, I will call such models efficiency wage models but note that this formulation excludes some models commonly viewed as efficiency wage models such as those in which higher wages reduce vacancies (Weitzman, 1989, Lang, 1991, Montgomery, 1991).

Previous papers ask whether exogenous differences in wages generate greater productivity (Capelli and Chauvin, 1991) or exogenous increases in the cost of monitoring generate higher wages (Krueger, 1991) but do not addresses whether wage differentials pay for themselves. Levine (1992) does find that wage differentials pay for themselves, but it is not clear that his wage differentials can be viewed as exogenous. Finally, some papers examine the relation between monitoring and pay, positing a negative relation under the efficiency wage model. However, these papers never address the question of what exogenous factor causes firms to choose different combinations of wages and supervision. There is also a large literature on interindustry wage differentials that asks whether the pattern of differentials is consistent with market-clearing (Dickens and Katz, 1987; Krueger and Summers, 1988; Katz and Summers, 1989; Murphy and Topel, 1987; Gibbons and Katz, 1992).

The approach in this paper is quite different. I rely on the comparative statics of a change in tax law to distinguish between the two models. The argument is quite simple. Under the standard competitive model, if a tax change affects a group of workers with highly inelastic labor supply, their earnings will fall by essentially the entire nominal employer share of the tax increase. Allowing the wage to play a motivational role but maintaining the market-clearing assumption broadens the range of possible outcomes. Given a reasonable estimate of the elasticity of demand, earnings could *fall* from anywhere between 0 and more than 100% of the employer's nominal share but would not rise. In contrast, because there is excess labor (involuntary unemployment) in equilibrium, efficiency wage models function very much like models in which the supply of labor is perfectly elastic, and thus earnings *rise* by more than the worker's nominal share.

I argue that the 1968, 1974 and 1979 increases in the taxable earnings base for FICA provide good opportunities to test the models. This tax increase affected only those workers earning significantly more than the median earnings for male full-time/year-round workers. Such workers' labor force participation is likely to have been highly inelastic. In addition, low earnings workers did not experience this tax increase.

The results are supportive of models in which the motivational effects of wages are important but cannot clearly distinguish between the efficiency wage and market-clearing versions of those models.

2 Theory

Until 1991, FICA taxes could be summarized by two parameters, the tax rate (formally split between employer and employee) and the earnings base. Taxes were levied on all earnings up to the earnings base. Thereafter, no taxes were collected.[1] The history of FICA taxes between 1967 and 1980 is given in Table 1.

As can be seen from the table, between 1973 and 1977, the FICA tax rate was constant at 5.85% or a little more than 11% if one takes account of both the employer and employee shares. During this same period, the earnings base was roughly constant relative to earnings in the economy,

[1] There is a slight complexity in that the worker's share of the tax is applied to earnings up to the earnings base in that year regardless of where the income was earned. The earnings base is applied separately to each employer so that a if a worker had more than one employer in the course of the year, the employers' share could be applied collectively to more than the earnings base.

fluctuating around 1.13 times the median earnings for male full-time/year-round workers. In 1978, the tax rate was raised to 6.05% or 11.36% of full earnings including the employer share and, in 1979, to 11.55% including the employer share.

More significantly, in 1979 the earnings base jumped to 1.35 times median earnings for male full-time/year-round workers. For a worker earning $22,900 in 1979, this represents an increase in payroll taxes equal to approximately 2.6% of earnings including the employer's share. This dwarfs the 0.19 percentage point increase in the tax rate.

Note that an increase in the earnings base has no direct effect on workers who would have earned less than the old earnings base. Workers who would have earned more than the new earnings base, experience an increase in a pure employment tax and thus only an income effect unless the shift is sufficient for them to jump to the lower segment on the budget line. Finally, for workers who would have earned less than the new base but more than the old base, there is both an income and a substitution effect as well as the possibility that they will jump to the lower segment of the budget line.

In what follows, we will concentrate on workers who would have earned more than the new earnings base. We begin by treating increasing the earnings base as an increase in a pure employment tax, as it is for high-earnings workers, and investigate its effect on wages under different models. Since the markets for different types of workers are not independent, this will require a fuller justification which we provide later in this section.

We begin by examining the effect on wages of an employment tax when hours and other job characteristics are fixed, workers are homogeneous and coverage is universal. Moreover, we treat labor force participation as perfectly inelastic since we are modeling the behavior of high-earnings workers who typically are strongly attached to the labor market.

Subsequently we address a number of issues – 1) endogenous hours/job quality; 2) substitutability among different types of workers when coverage is not universal; 3) the endogeneity of the tax to the labor supply decision.

2.1 Wage Effects of an Employment Tax with Exogenous Hours

We begin with a simple model that includes the generic Solow (1979) efficiency wage model as a special case.

For the representative firm, we have

$$\pi = f(e(w)L) - (w + T)L \tag{1}$$

where e is efficiency units of labor and T is the employment tax. Maximizing with respect to L and w gives the familiar first-order-conditions

$$f'e = w + T \tag{2}$$
$$f'e' \leq 1. \tag{3}$$

When (3) holds with equality, we have the canonical efficiency wage model. When it does not, we have a market clearing model but in which wages increase productivity if $e' > 0$. We begin with the market-clearing model.

2.1.1 Wage Effects with Market-Clearing

We can solve (2) for L to get

$$L = \frac{g(\frac{w+T}{e})}{e} \tag{4}$$

where $g = f'^{-1}$ and is the demand for effective labor units. L is fixed since labor supply is perfectly inelastic, we can fully differentiate the right-hand-side of (4) to get $\frac{dw}{dT}$

$$\frac{dw}{dT} = -\frac{g'}{g'(1 - \frac{(w+T)e'}{e}) - ge'}. \tag{5}$$

or

$$\frac{dw}{dT} = -\frac{\varepsilon_d}{\varepsilon_d(1 - \varepsilon_s) - \varepsilon_s} \tag{6}$$

where ε_d and ε_s are the elasticities of demand for and supply of effective labor units. Recall that hours and participation are fixed. It is the possibility of a change in effort that permits supply to be elastic.

In the special case of the standard competitive model, $e' = 0$. Then

$$\frac{dw}{dT} = -1$$

and we have the standard result that when labor supply is perfectly inelastic, labor bears the entire burden of the payroll tax.

At the other extreme, where the incentive effects of higher wages are almost sufficient to induce the payment of an efficiency wage, $\varepsilon_s \to 1$ so that $\frac{dw}{dT} \to \varepsilon_d$. Given that the payroll tax is nominally levied equally on worker and employer, if $\varepsilon_d = -.5$, we would not observe any shifting of the tax.

2.1.2 Wage Effects in the Efficiency Wage Model

So far we have assumed that the efficiency effect of a wage increase is dominated by its direct cost so that inequality (3) is strict. There is a discontinuity in the wage effect when we shift to an efficiency wage world in which both first-order conditions hold with equality. In this case we have

$$\frac{e'(w+T)}{e} = \varepsilon_s = 1, \tag{7}$$

or in other words, the wage is set at the point at which the elasticity of effort with respect to the wage equals 1.

Applying the implicit function theorem yields

$$\frac{dw}{dT} = -\frac{e'}{e\frac{d(.)}{dw}} > 0 \tag{8}$$

where $d(.)/dw$ is the derivative of the left-hand-side of (7) with respect to w. This derivative must be negative by the second-order condition.

In other words, the after-tax wage received by workers rises. The intuition is that when the per worker employment tax rises, firms will want to reduce the number of workers and require more intensive work from each remaining worker. In the competitive model, because labor force participation is perfectly inelastic, the equilibrium impact is on earnings not on the number of workers employed. However, in the efficiency wage model, the excess supply of labor means that the model operates as if the supply of labor were perfectly elastic. If there were no change in the level of effort required of workers, the wage would not change. However, because effort increases, so does the wage.

It must be recognized that the Solow model is not applicable to all efficiency wage models. In particular, recruiting models, in many ways variants of search models, do not fall within this framework.

Perhaps more significantly, the Solow model has the rather odd characteristic that the wage is independent of productivity in the range that the efficiency wage condition is binding. As productivity increases, the wage remains constant, the quantity of labor demanded increases and unemployment falls. The natural way to extend the model is to allow productivity to depend on some reference wage which depends on the wage, the prevailing wage and the level of unemployment (Shapiro and Stiglitz, 1984). For models in which effort depends on the unemployment rate as well as the wage ($e = e(w, u)$), the analysis can be more complicated. In the simplest case, $d^2e/dwdu = 0$, and the analysis is unchanged. Pisauro (1994) analyzes an efficiency wage model in which u affects effort and obtains similar results. In the Shapiro-Stiglitz model, wages could fall somewhat in response to a tax levied entirely on the employer.[2]

2.2 Extensions and Issues

2.2.1 Wage Effects with Endogenous Job Quality/Hours

We now consider the case where the tax may alter characteristics of the job such as hours or working conditions. In the discussion, we refer to h, the job characteristic as hours, but, in fact, the analysis applies to any characteristic. In this section we limit ourselves to the market-clearing model since adding job characteristics does not substantively alter the efficiency-wage results.

We rewrite the production function so that the number of efficiency units provided by a worker depends on hours worked as well as the wage. The market wage depends on hours required by the employer. So that (1) now becomes

$$\pi = f(e(w,h)L) - (w(h)+T)L. \tag{9}$$

For simplicity assume that utility is separable in leisure and wages so that

$$U(w,h) = u(w) - v(h), \qquad u' > 0, v' > 0, u'' < 0, v'' > 0.$$

By the usual argument, efficient contracting ensures that the marginal wage equals the monetized marginal value of leisure or

$$w' = \frac{v_h}{u_w} \tag{10}$$

where u_h and u_w are the marginal disutility of labor and the marginal utility of consumption.

[2] It seems natural to write $e = e(\frac{w}{g(u)w^*})$ where $g' < 0$ and $w^* = w$ in equilibrium. This effort function would arise if for example the worker compared the value of employment with the value of unemployment with some expectation of reemployment after becoming unemployed. Then the firm's condition for profit maximization is

$$\frac{e'(w+T)}{e} - g(u)w^* = 0$$

where in equilibrium $w = w^*$. Note that in the absence of taxes, this model implies that exogenous increases in productivity raise wages and that the unemployment rate is constant. Stability of the equilibrium requires that the derivative of the l.h.s. of the equation with respect to w be negative. Applying the implicit-function theorem therefore establishes that $\frac{dw}{dT}$ has the same sign as $\frac{e'}{e} - g'w^*\frac{du}{dT}$. Without fully modelling unemployment, we cannot make strong statements about $\frac{du}{dT}$ but in most plausible models, it will be positive, implying that $\frac{dw}{dT} > 0$ as in the canonical model.

Maximizing (9) with respect to L, h, and w gives

$$
\begin{aligned}
f'e &= w(h) + T && (11)\\
f'e_h &= w' && (12)\\
f'e_w &\leq 1 && (13)
\end{aligned}
$$

with the inequality being strict in the market-clearing case.

Substituting (10) into (12) and fully differentiating establishes that w and h move in opposite directions.[3] This is not surprising. Recall that w is total, not hourly, wage compensation. If both consumption and leisure are normal goods, then when compensation rises, workers take part of the increase in the form of greater consumption and part in the form of leisure.

To find $\frac{dw}{dT}$, we use (4) but recognize that e now depends on h as well as w. Again we use the fact that $dL = 0$ and fully differentiate the right-hand-side to get

$$\frac{dw}{dT} = -\frac{g'}{g'(1 - \frac{(w+T)e'}{e}) - ge' - (\frac{g'(w+T)e_h}{e} + \frac{ge_h}{e})\frac{dw}{dh}} \tag{14}$$

or

$$\frac{dw}{dT} = -\frac{\varepsilon_d}{\varepsilon_d(1 - \varepsilon_s) - \varepsilon_s - (1 + \varepsilon_d)\frac{(w+T)e_h}{e}\frac{dh}{dw}}.$$

The additional term in the denominator shows that if labor demand is inelastic, the hours increase in response to reduced earnings lowers earnings even further. By definition, inelastic labor demand means that increasing supply lowers total payments to labor.

In short, given consensus elasticities of demand above -1, allowing for job characteristics such as hours or safety increases our expectation that under the market-clearing hypothesis much or all of the burden of an increase in the earnings base will be borne by the workers.[4]

2.2.2 The Payroll Tax is an Impure Employment Tax

We have modeled the FICA tax as a pure employment tax. In fact, while a small change in labor supply does not affect the tax, it is possible for a worker to reduce the tax by a sufficient reduction in labor supply. The effect of recognizing this complexity is most easily analyzed if we model the

[3] The derivative with respect to h is negative by the second-order conditions. $\frac{dh}{dw} = -\frac{\frac{v'u''}{(u')^2}}{d(.)/dh}$.

[4] Note that the wage change is not even an approximate measure of the distribution of burden between the firm and worker since workers also adjust effort.

tax as being collected from the worker so that labor supply depends on the after-tax hourly wage.

If $wh_s^i < Y^*$ where Y^* is maximum taxable earnings, worker i's labor supply is determined by

$$h_i^s = h_i^s((1-t)w_i, \theta_i, Y_i^a) \tag{15}$$

where t is the tax rate and θ is a vector of factors that determine tastes for leisure and Y_i^a is nonlabor income.

If $wh_i^s > Y^*$, worker i's labor supply is determined by

$$h_i^s = h_i^s(w_i, \theta_i, Y_i^a - tY^*). \tag{16}$$

Of course, this kinked budget line implies a nonconvexity. The solution to (15) may give $wh_i^s < Y^*$ while at the same time the solution to (16) gives $wh_i^s > Y^*$. In this case, we need to check the utility level associated with the two solutions and choose the solution with the higher utility. In the extreme case, the individual may be indifferent between the two in which case a small change in the tax structure can dramatically alter labor supply.

It is therefore critical to the analysis so far that increasing the earnings base not lead to large reductions in labor supply by a substantial number of workers. In the empirical work below, we examine the importance of such shifts.

2.2.3 Benefit Increases

The increase in the earnings base also raises the base on which retirement and disability benefits are earned. This benefit increase partially offsets the tax increase so that the full income from working falls by less than the tax increase. Of course, the health insurance component of FICA is not affected by the earnings base increase. Because of the complexity of the social security benefit formula, the value of the increased OASDI benefit varies across individuals. Still, because of the very nonlinear relation between payments and benefits, most of those earning above the new cutoff will not recoup the added taxes. Feldstein and Samwick (1992) estimate very high marginal social security tax rates for high earnings workers, and their estimates do not include the health and disability components of FICA. Their results show that for high-earnings workers, the marginal benefits of increased social security contributions are small. Given the complexity of the social security system, it is not obvious that workers perceive any direct relation between their payments and their benefits.

Of course, benefit increases should be offset 1 for 1 by wage decreases. Therefore, to the extent that the tax increase is partially offset by increased benefits, this reinforces our expectation under the competitive model that high-earnings workers will see their after-tax earnings fall by the full amount of the tax increase but could provide an alternative explanation for the failure of the prediction of the efficiency wage model.

2.2.4 Substitution Between Low- and High-Earnings Workers

So far we have treated the tax increase as affecting all workers. If the market for workers above the earnings base were separate from the market for other workers, we would be fully justified in this choice. However, these markets are surely interrelated, and we must consider how this affects our predictions, particularly for the market-clearing model.

If wage differentials reflect productivity differences among workers, labor demand is likely to be highly elastic. Workers with one set of skills are likely to be close substitutes for workers with a similar set of skills. In the extreme case when worker skill can be measured in effective labor units, the demand for a particular skill level will be perfectly elastic. In this case, wages of high-earnings workers would have to fall, relative to other workers, by at least the amount of the tax, and more if the wage decline made them less productive. More generally, the substituting of low earners for high-earners will raise the earnings of the former and further reduce the earnings of the latter.

If wage differentials were primarily compensating differentials for job characteristics, reducing wages in high wage/unpleasant jobs would cause an outflow of workers from the high wage sector. Wage differentials would have to adjust to leave workers indifferent among the different types of jobs offered in equilibrium. However, there is little evidence to support the view that most wage differentials are compensating differentials for bad job characteristics (Brown, 1980). So the case that must concern us is where most wage differentials reflect productivity differences but some small part of the difference reflects compensating differentials which is identical to the case of endogenous hours discussed above. The sole caveat is that, as in the case of labor supply, because of the nonconvexity of the budget set, there is a theoretical possibility that such a worker will jump from a high-earnings job with undesirable characteristics to a much lower-earnings job with better characteristics.

2.3 Summary of Theoretical Predictions

If we allow wages to have efficiency effects, the market-clearing model permits a broad range of predictions regarding the effect of an increase in the FICA earnings base on the earnings of high-earnings workers. Even when the labor force participation of high-earners is perfectly inelastic, high earners may experience earnings declines that exceed the tax increase if labor demand is very elastic and incentive effects are important. On the other hand, if incentive effects are important and demand is relatively inelastic, the earnings of high-earners might fall by only about half of the tax. Given that the FICA tax is nominally divided equally between worker and employer, this implies no change in nominal wages.

In contrast, when the incentive effects of wages are sufficiently large to generate an efficiency wage equilibrium, the tax increase should have a positive effect on the pre-tax wage if there is no associated benefit increase from the tax increase.

3 Data and Methods

If we could randomly assign workers to separate markets with different earnings bases, measuring the earnings effect of in increase in the earnings base would be easy. Unfortunately such an experiment is not available.

Our approach is to examine earnings increases for a group of high-earnings workers.[5] If workers' earnings fall due to a tax increase, such workers should experience unusually small increases in years in which the earnings base increases. Conversely, if firms must pay higher wages in response to the tax increase, such workers should get unusually large increases. (Recall that the tax is nominally split equally between worker and firm.)

As shown in Table 1, the FICA tax rate was constant within each of the following sets of years: 1967-1968, 1969-1970, 1971-1972, 1973-77, and 1979-80. The March 1972 and 1973 and the March 1976 and 1977 files cannot be

[5] While it might appear that it is possible to look simply at how wage levels are affected by changes in the earnings base, this is not the case. For example, we might look at whether the wages of workers who earn at least \$30,000 are higher or lower when the earnings base is \$30,000 than when it is \$20,000. However, the increase in the base affects the set of workers who earn at least \$30,000. The models make no predictions about wages conditional on being above some cut-off. For example, suppose that raising the earnings base increases the earnings of all workers earning above \$30,000 by \$1,000 but some workers who would have earned less than \$30,000 earn between \$30,000 and \$31,000. The average wage conditional on earnings \$30,000 could go up or down and would certainly go up by less than the true increase of \$1,000 for high-earning workers.

matched so that we cannot examine the 1971-72 and 1975-76 periods. The largest increase in the earnings base occurred between 1978 and 1979 when it rose from 113% to 135% of median earnings for male full-time/year-round workers. While the tax *rate* did increase over that period, the increase was quite small, from 6.05% to 6.13%. We treat this as an additional year with no change in the tax rate.

We therefore have the following potential "experimental years"

1. 1967-68 when the base rose by 10 percentage points relative to median earnings.

2. 1973-74 when the base rose by 14 percentage points.

3. 1978-79 when the base rose by 32 percentage points.

These experimental years can be compared with the four control years:

1. 1969-70 when the base fell by 5 percentage points.
2. 1974-75 when the base was constant.
3. 1976-77 when the base fell by one percentage point.
4. 1979-80 when the base rose by 4 percentage points[6]

Hours data and therefore hourly wages are available only starting in 1976. Thus the last experiment is the only one for which we can use wage comparisons rather than annual earnings comparisons.

The theory as developed requires that the earnings base increase not be offset by a benefit increase. In each year that the earnings base was raised, there was an automatic benefit increase associated with that increase. After 1974, primary insurance amounts (PIA) were indexed. However, in the earlier years there were benefit increases that could be problematic. The PIA increased by about 13% in February 1968 and 11% in June 1974. Given the high rate of inflation in 1974 and 1975, the latter increase generates close to a zero real increase in both years. The earlier years are somewhat more problematic since there is a real increase in 1968 and a smaller but real decrease in 1970. Since high-earners with high PIAs would benefit more from the increase, this will bias our results towards finding a bigger earnings decrease in response to the 1967-68 base increase.

The theories apply to workers who would have earned more than the new earnings bases even in the absence of a change and who continue to do so after the increase. Our inability to identify such workers directly is

[6] Table 1 compares the earnings base to median earnings. We could compare changes in the earnings base to changes in the consumer price index. For most of the years we consider, this would not generate any substantive change. The real change in the base from 1979 to 1980 is reduced to approximately 0 and the real change in the base from 1973 to 1974 is reduced to about 9 percentage points. The remaining changes are minor. The changes are even more modest if we index using the implicit GDP deflator.

a significant problem. We employ a number of strategies to address this problem.

Consider two pairs of years, one (denoted years 1 and 2) in which the real earnings base remains constant at $20,000 across the two years and one (denoted years 3 and 4) in which it rises from $20,000 to $30,000. Given our strategy of examining earnings growth, we would like to identify workers who would have earned more than $30,000 in year 2 and in year 4, regardless of whether the earnings base were $20,000 or $30,000. In fact, in year 2 we observe their earnings only at the lower earnings base while in year 4, we observe them only at the higher earnings base. If the earnings base had no effect on who earns at least $30,000, this would not be a problem. Unfortunately, theory suggests that the earnings base should affect who is in this class. The severity of this problem is an empirical issue which is addressed below. If the earnings base has only a small effect on the composition of the set of people earning at least $30,000, then relying on earnings in years 2 and 4 to identify the workers in whom we are interested will be a good strategy.

If the earnings base change has a large effect on who earns more than the new earnings base, relying on earnings in years 2 and 4 entails a significant endogeneity problem. We can avoid this problem by using year 1 and year 3 earnings to identify workers. Thus we can examine earnings growth for workers who earned at least $30,000 in those years. This approach avoids the endogeneity issue since workers in both years 1 and 3 were subject to the lower earnings base but is likely to generate much more misclassification. Workers whose earnings are unusually low in one year will have earnings that are predictably higher the next – a beginning assistant professor drawing salary for only four or six months may know that she will earn more than the earnings base the following year. Similarly, workers with unusually high earnings expect to fall below the earnings base the following year.

Reactions to early versions of this paper suggest that most economists are more concerned by the endogeneity issue than by the misclassification issue. We use both approaches but rely more heavily on classifications based on first-year earnings.

In each case, we measure the earnings effect of the earnings base increase by comparing earnings increases for high-earnings workers in the year the base changed with the earnings increase in control years. We classify workers as being high-wage on the basis of their earnings relative to earnings for the median year-round/full-time worker. When we examine the effect of the 1968 earnings base increase, we look at earnings increases for all workers earning at least 102% of median earnings for male year-round/full time workers in 1967, 1969, 1974, 1976 and 1979. We ask whether real earnings increases for these workers are higher or lower from 1967 to 1968 than they are in each of the other four years. Similarly, when we examine the effect of the 1974 earnings base increase, we examine workers who earned at least 111% of median earnings for male year-round/full time workers in 1973, 1969, 1974, 1976 and 1979 and for the effect of the 1979 increase, we look at real earnings increases for workers earning at least 135% of this median in 1978 and each of the four control years.

Note that whether we classify high-earnings workers on the basis of first or second-period earnings, the earnings increase will include a transitory component. The earnings increase is given by

$$\Delta w_i = \overline{w}_{it} + \epsilon_{it} - \overline{w}_{it-1} - \epsilon_{it-1}$$

where $\overline{w}$ are the permanent components of earnings and the ϵ are transitory (one year) components of earnings or measurement error. When the sample is drawn based on reported first-year earnings, we have

$$E(\Delta w_i) = \overline{w}_{it} - \overline{w}_{it-1} - E(\epsilon_{it-1}|\overline{w}_{it-1} + \epsilon_{it-1} > Y^*)$$

where Y^* is the new earnings base. We expect our estimates Δw_i to be biased downwards relative to the change in the permanent component of earnings. The sample will overrepresent people with high transitory first-year earnings. Conversely, when we draw the sample on the basis of second-year earnings, the sample will overrepresent people with high transitory second-year earnings, and our estimates Δw_i will be biased upwards.

The critical identifying assumption is that the bias depends only on the earnings level used for the sample cut-off and not on whether the earnings base changed that year. If so,

$$\begin{array}{rcl} & & E(\Delta w|base\_change) - E(\Delta w|no\_change) \\ & = & \Delta\overline{w}|base\_change - \Delta\overline{w}|no\_change \\ +E(\epsilon_{it-1}|\overline{w}_{it-1} + \epsilon_{it-1} & > & Y^*\&base\_change) \\ -E(\epsilon_{it-1}|\overline{w}_{it-1} + \epsilon_{it-1} & > & Y^*\&no\_change) \\ & = & (\Delta\overline{w}|base\_change) - (\Delta\overline{w}|no\_change). \end{array}$$

While our sample is quite large since it draws on microdata from seven March CPS surveys, as emphasized in Donald and Lang (2000), as the number of observations per year gets large, the problem effectively reduces to one in which there are seven data points corresponding to the seven years we study. In each of these years, there is a wide variety of forces affecting wages – the Vietnam war, oil shocks, monetary and fiscal policy, etc. Our experimental and control years both contain relatively high and relatively low inflation years, boom years and recession years, years of war and years of relative peace. There is no obvious reason that wage increases would tend to be higher or lower for high-earnings workers, especially relative to the median, in years in which the earnings base changed.

However, as in any case in which there are effectively only seven observations the probability of a random correlation between unmeasured factors and the variable of interest cannot be treated as zero (as it would be asymptotically). Our degree of confidence must reflect the limitations of our sample. In order to make this explicit, our estimation approach follows that suggested in Donald and Lang. For each pair of years, we calculate the median earnings increase (c_t) for workers with earnings above the earnings base. As noted above, the median increase should not be given a structural interpretation. The c coefficients will tend to be positive when we base the sample on second-period earnings and negative when we base it on first-period earnings since we expect individuals with high wages to have experienced high wage growth. We use the median rather than the mean, because it is important to avoid spurious sources of differences in the $\widehat{c}'$s. In particular, annual earnings are top-coded at \$50,000 (far greater than the earnings base), a problem that becomes more serious over time and there is undoubtedly considerable measurement error in wage growth which can diminish the robustness of the mean.

In the second stage, the estimated medians, the $\widehat{c}$'s, are used as dependent variables in a regression of the following form:

$$\widehat{c_t} = a_0 + a_1 year\ base\ changed_t + u_t. \tag{17}$$

The regression compares the earnings growth of high-earners in the "experimental" year with the average of the earnings growth of high-earners in the control years. It recognizes that for each of the experiments there are only five data points and three degrees of freedom.

Because differences in wage growth across years might reflect changes in the composition of the labor force, we also estimate the following equation

$$wage_{it} - wage_{it-1} = X_i B_t + c_t + e_{it} \tag{18}$$

for each pair of years. Again the sample is restricted to the high-earnings group. We regress the estimated c's using (17).

As before, equation (18) has no structural interpretation. Since we rely on quantile regression, it simply provides the best linear prediction of median period t wage growth conditional on period t-1 (or period t) wages being high and other individual characteristics.

However, there is no reason to expect these conditional means to differ across years unless changes in the FICA tax or other factors change the wage structure. If FICA tax changes lower the relative wages of high wage workers,we would expect this to be reflected in the coefficients of equation (18).

Eissa (1995) tests for effects of the 1986 income tax reform by comparing labor supply changes for women with very high family incomes (who should be affected by the tax reform) with those with somewhat lower family incomes who should not be. As a further test of the effect of the FICA tax change, we mimic this strategy. We estimate the equivalent of (18) for workers with earnings below the old earnings base for whom there should be no effect of the increased earnings base. In the second stage, we use the difference between $\widehat{c}_t$ for high-earnings and low-earnings workers as our dependent variable in (17). This "differences-in-differences" strategy will be superior to the simple differences approach if there are common shocks to earnings increases for both high and low-earnings workers and which are not captured by trends in median earnings for which we do not otherwise control. This approach also allows us to free the B's to vary across pairs of years.

The data are drawn from matched March annual demographic supplements to the Current Population Survey. The March supplement provides information on earnings the previous year. The data used here are from the Unicon CPS files. For each pair of years, we match the first four rotation groups from the first year with the last four rotation groups from the second year using the identifiers for those years that allow matching of interview location.[7] The samples are then matched on the basis of exact agreement on race, sex and age (adjusted by one year). Approximately 70% of the potential sample is matched in each year. The sample is limited to workers with positive earnings in both years. Additional restrictions

[7] The Current Population Survey samples residences for four month, skips these residences for eight months and then samples them again for four more months before dropping them from the sample. Thus each location is included in eight "rotation groups" with the last four occurring one year after the first four. Since the survey relies on a sample of residences rather than households, the respondents in the two years may differ. By matching on exact age, sex and race, we avoid most mismatches. Undoubtedly, response and coding error results in some real matches being missed.

on earnings are described in the presentation of the results. For example, when we study the effect of the 1968 tax increase on high-earnings workers and define high-earnings by first-period earnings, the sample is limited to workers who earned at least 102% of median earnings in the first year of each two-year pair.

To calculate real wages we index wages by median earnings for male full-time/year-round workers rather than the more conventional consumer price index. This strategy sidesteps a number of issues about the appropriateness of different measures of real wages. In particular, we do not have to address issues of anticipated versus unanticipated inflation. In addition, by indexing to a measure of median earnings our approach is, in many ways, analogous to the popular differences-in-differences approach. By indexing wages to median earnings, we are, in effect, asking whether *relative to the median worker* wages for high earnings workers rise more or less in years in which the earnings base increases. It is worth noting that while the instability of inflation in this period is a disadvantage, one advantage of the period is that, because of the relatively high inflation, nominal wage rigidity is much less of a concern than it might be in other periods. In periods of low-inflation, even if the full cost of the tax were shifted to workers in the long run, we might find little shifting in the short run, because firms are reluctant to cut wages. However, during this period, full-shifting is consistent with nominal wage increases.

Equations (18) and (17) can be estimated separately for each of the three "experiments." However, the three "experiments" are not really independent. The first-stage results for each of the four control years are presumably correlated across experiments even though each first-stage is estimated separately using different definitions of high and low earners. Therefore, we cannot treat the three estimates of a_1 as independent.

We combine the three experiments in the following way which again follows the procedure proposed by Donald and Lang. We take the fifteen median wage changes (five for each of three experiments) and regress these on year and experiment dummies to produce seven year effects. We then calculate the value of the tax change in that year. The one-side tax change for high-earnings workers is approximately $76 (1979 dollars) in 1968, $155 in 1974 and $323 in 1979 and is zero in each of the control years. We then regress the seven year effects on the one-sided tax change. If wages fall to fully offset the nominal tax increase on employers, the coefficient from this regression should be about -1 and be about 1 if wages rise to fully offset the nominal tax on workers.

4 Results

We begin by asking whether the tax change has a measurable effect on the relation between being a high-earnings worker in the first period and being a high-earnings worker in the second period. The results are shown in Table 2. As can be seen relatively few workers who earned less than the real earnings base in their first year earned more than the earnings base in their second year. The highest proportion of such workers is the 7.5% of workers who earned less than the 1968 earnings base in 1967 but more than this base in 1968. At the other extreme, only 2.2% of workers who earned less than the 1979 earnings base in 1974 earned more than this amount in 1975.

Movement from being measured as above the earnings base in one year to below the earnings base in the following year is more common. Fully 26.5% of workers who reported earning more than the 1979 earnings base in 1979 reported earning less than that base in 1980. In contrast, only 8.7% of those earning less than the 1968 base in 1967 earned less than that base in 1968.

Perhaps the most important result is that there is no evidence that increasing the earnings base causes significant numbers of workers to reduce their earnings so that they no longer earn more than the new earnings base. If anything, workers seem somewhat more likely to shift into the high-earnings region and less likely to leave the high-earnings region in years when the base is raised. Although not the central focus of our approach, this is consistent with firms adjusting wages to offset much of the worker share of the tax increase.

Table 3 presents the results of conducting each of the "experiments" independently. The years in which the earnings base changed are marked in bold. The top half of the table shows the median earnings change in each year for workers who earn more than the higher real earnings base in the first year. Not surprisingly, the median changes are all negative. Workers who in one year report high earnings, frequently have lower earnings the next year because their first-year earnings are inflated by a combination of measurement error and transitory earnings.

It is immediately apparent that the coefficients are highest (lowest in absolute value) in the years in which the earnings base increased. In each of the three cases, earnings declines for this group are substantially lower in the experimental year than in the four control years. Because the changes for control years are correlated across experiments, there is no simple way to combine the results from the three experiments. We can, however, consider two extremes – 0 correlation of control years across experiments and perfect correlation. In the former case, the probability of the relative wage increase being highest in the experimental year is .2 for each experiment and .008 for all three experiments or .016 for a two-tailed test. When the correlation is perfect, we can ask what the probability is that all three experiments would show higher relative wage increases than all four control years. This probability is $\frac{3}{7} * \frac{2}{6} * \frac{1}{5}$ or $\frac{1}{35}$, again significant at the .05 level for a one-tailed test and at the .06 level for a two-tailed test. Thus, the results strongly suggest that the effect of the earnings base increase is to raise the earnings of high-earnings workers.

The bottom half of the table addresses this comparison more formally. Each row is based on an analysis of five "first-stage" estimates. Following Donald and Lang (2000), we treat the five first-stage estimates as observations of the dependent variable and regress the median on a constant term and a dummy variable for the experimental year. The resulting t-statistic is distributed as t with 3 degrees of freedom. Thus the critical values for a two-tailed test are 2.353 (.1 level) and 3.182 (.05 level).

The first row tests whether the difference between the median earnings change in the experimental year and the control years is statistically significant. The difference ranges from 313 for the 1979 change to 434 for the 1974 change, but each difference is measured with a large standard error. For the 1968 and 1974 changes we can reject at the .05 level the hypothesis that the tax has no effect on workers' earnings while we can reject this hypothesis at the .1 level for the 1979 change. Recall that the FICA tax is nominally applied equally to worker and firm. Thus (if we ignore changes in hours and other working conditions) a finding of no effect on earnings would essentially imply equal sharing of the tax. The results are consistent with firms increasing wages by more than the worker share of the tax, strongly suggest that wages rise in response to the tax and very strongly reject the hypothesis that wages fall due to the tax increase.

The second row of the lower panel of Table 3 is similar to the row above it but selects the sample on the basis of earnings in the second period. As discussed above, this reduces errors due to misclassifying workers on the basis of transitory first-period earnings but may be subject to endogeneity problems if the increase affects who becomes a high-earnings worker. The results are similar, in terms of both magnitude and statistical significance, to those obtained when the sample is determined on the basis of first-period earnings.

To avoid some of the problems associated with dividing the sample on the basis of first- or second-period earnings, we use a probit equation to predict whether workers will earn more than the higher earnings base in the second period. The explanatory variables are real first-period earnings, a dummy variable for being above the higher earnings base in the first period and a set of demographic variables (age, race and ethnicity, education, lives in an MSA, marital status, sex, census state, number of children under 18). The sample was limited to the control years. We then obtain a predicted probability of earnings above the higher earnings base in the second year and use this probability to weight the earnings change. The first-stage estimates are the median weighted earnings change. The third row shows the results of regressing this estimate on a dummy variable for the experimental year. Again the precise sample selection method has little effect on either magnitude or statistical significance.

Over the period 1968 - 1980, there were significant shifts in the age, education and sex composition of the labor force. While the experimental years are spread throughout this period, it is possible that some of the experimental effect is actually capturing the change in labor force composition. To control for this, we used median regression in which the real wage change was regressed on year dummies and the controls listed in the previous paragraph. As shown in the fourth line, adding the controls has a very modest effect on the magnitude of the estimated effects but increases the standard errors noticeably. While it is still possible to reject the hypothesis that earnings fell by the entire amount of the tax increases, it is no longer as evident that earnings rose.

We might be concerned if the results were being driven by very high-earnings workers. Therefore in the fifth row, we restrict the sample to workers with real first-period earnings between the higher earnings base and that base plus $5000. Again the results do no change substantively.

Perhaps the greatest concern is that somehow the results so far capture something else that is happening in the economy. While we have attempted to mimic a differences-in-differences estimator by indexing wages to the wage of the median male year-round/full-time worker, it is possible that this normalization is imperfect. One way of addressing this concern is by comparing outcomes for high-earnings workers with outcomes for low-earnings workers. The last two rows of table 3 provide differences-in-differences estimates of earnings growth. In the penultimate row, the high-earnings sample is restricted as in the previous row and the low-earnings sample is restricted to those earning between $5000 below the lower earnings base and that earnings base. In the last row the high-earnings sample is unrestricted and the comparison sample consists of those earning less than the lower earnings base.

While all of the estimates remain positive, the differences-in-differences estimates are lower in magnitude and fall short of significance at conventional levels. The reductions in magnitude and significance are particularly large when the samples are restricted. Nevertheless, we can reject full shifting to workers in all three years and cannot reject full shifting to firms when we use the restricted sample. Thus while the differences-in-differences estimates indicate a need for caution, they support the interpretation derived when we use the simple differences.

Moreover, it should be remembered that we would not expect the "control" group in the differences-in-differences to be unaffected by the policy change. Some workers who earn less than the lower cutoff in the year before the policy change, will nevertheless earn more than the new cutoff after the policy change. Thus some workers who are classified as unaffected by the policy are misclassified. As a consequence, the difference-in-differences estimate *should* be less the other estimates.

In essence, with misclassification, the slope of the regression of real income in the second year on real income in the first year should be steeper in years in which the earnings base increased. In practice, it is impossible to distinguish the tax explanation from other explanations for a steeper slope.[8] However, there is no obvious reason other than the tax effect, that the slope should be steeper in precisely the years that the earnings base was raised.

We do not report the results using hourly wages. Recall that there are only three years for which hourly wages are available. The coefficients are uniformly positive but, not surprisingly given only one degree of freedom, are very imprecise.

As discussed above, it is possible to combine the three experiments by first deriving seven "year effects" from the three experiments and then regressing these year effects on a measure of the magnitude of the tax change for those with earnings above the earnings base. The first line of table 4 reports the results of this estimation for the base model in table 3. The coefficient from this regression is 1.20 which implies that wages rise by more than the tax increase on workers as predicted by the efficiency wage model. However, given that there are only five degrees of freedom (seven observations minus two parameters), the confidence interval is quite large so that we cannot reject models in which the market clears but the wage has important incentive effects. The extreme model in which there are no incentive

[8] In principle, we could estimate an equation in which the second-period wage depended on first-period income and the probability that the worker would be a high-earnings worker in the second period (which would itself by a nonlinear function of first period earnings. By imposing restrictions on the functional form, we could distinguish the two effects. In practice, the resulting standard errors are so large as to make the exercise uninformative.

effects and workers earnings rise by the full amount of the tax lies outside the confidence interval.[9]

The second and third lines repeat the exercise using the differences-in-differences approach in the last two lines of table 3. As discussed above, differences-in-differences reduces any bias due to incorrect indexation but increases bias due to misclassification of workers. Using the unrestricted sample, the point estimate indicates that about six-tenths of the worker share of the tax is shifted to employers. The confidence interval again includes full shifting to employers and excludes full shifting of the firm share to workers. Thus differences-in-differences and simple differences tell similar stories although the range of the confidence interval is somewhat lower with differences-in-differences.

When we restrict the sample range, the confidence interval using differences-in-differences becomes much sharper. The coefficient (.45) is similar to that obtained using the full sample but the estimate is much more precise. The lower end of the confidence interval is at the upper end of the range consistent with market-clearing. The upper end is below the full shifting predicted by the efficiency wage model unless workers put positive value on the increased benefits. The results strongly rule out the standard model without incentive effects.

Finally, the last two lines of the upper section of table 4 shift to measuring real earnings changes using the GDP deflator although the groups, high and low-earnings workers, continue to be defined as before.[10] With the unrestricted sample, the results are uninformative. The confidence interval ranges between -3 and 3. With the restricted sample, the results are broadly similar to those obtained with simple differences. We can reject full shifting to workers but not to firms.

[9] The confidence interval assumes that the error terms are homoskedastic and normally distributed. Despite the fact that three observations are used to determine the year effects for control years and only one is used for the experimental years, the estimated standard errors of the year effects do not vary greatly across years so that the homoskedasticity assumption is reasonable. Whether the normality assumption is reasonable is a matter of taste.

[10] Unfortunately the period we are studying includes those rare years in which the GDP deflator and the CPI differ significantly. The efficiency wage model suggests that the wage should depend on the CPI while the market-clearing model suggests using the GDP deflator and the hybrid model some combination of the two. In practice, using the CPI to index income results in confidence intervals that are so large as to be completely uninformative.

5 Replication Using Panel Study of Income Dynamics

Several seminar participants suggested that it would be preferable to use the Panel Study of Income Dynamics instead of the March CPS. The PSID has three potential advantages. First, we can match the 1975 and 1976 earnings of workers and thus have an additional control year. Second, since the survey is designed to follow families over time, with only a modest amount of care, we can be sure that errors in matching do not introduce measurement error into year-to-year earnings changes. Third, we can examine hourly earnings as well as annual earnings. On the other hand, because earnings data were initially recorded only in bracketed form in the PSID, we cannot use the 1968 experiment. Moreover, the smaller sample size may introduce more imprecision in the measurement of median earnings changes.

I replicated many of the approaches in table 3 and consistently found that while the point estimates were consistently positive – increases in the earnings base raised wages - the confidence intervals were so large as to render the exercise uninformative. Replicating the results in the first part of table 4 proved slightly more informative. The results are presented in the lower panel of the table. Note that in contrast with the upper part of the table where there were three experimental years and four control years, in the lower panel, there are two experimental years and five control years.

Despite the differences in samples, the results in the lower panel are similar to those in the upper panel albeit less precise. Using the base specification, the point estimate indicates that earnings rises by almost the entire amount of the tax increase but the confidence interval includes all economically interesting values. When we rely on differences-in-differences, the point estimate falls, but we are able to rule out the conclusion that earnings fall by the full amount of the tax increase. Restricted the sample to those with $5000 of the cutoffs, gives even more precise differences-in-differences estimates which again rule out the earnings falling by the full tax increase.

Finally, using the hourly wage instead of annual earnings generates even more imprecise results. Point estimates were generally positive but occasionally negative. In only one case did the confidence interval exclude at least some points between wages rising by the full amount of the increase and falling by the full amount. This is the case of the differences-in-differences estimate using the restricted sample. Again we reject the hypothesis that wages fall by the full amount of the increase.[11]

[11] The scaling using hourly wages is somewhat arbitrary. This scaling presumes that

6 Discussion and Conclusion

Perhaps the most important contribution of this paper is to demonstrate that it is possible to use the comparative statics of the models to distinguish between competing models of wage determination. Wages and employment undoubtedly respond differently to a variety of policy interventions under standard competitive and efficiency-wage setting.

In fact, earlier studies of the incidence of payroll taxes could be construed as tests of the competing models if they were able to distinguish between the incidence of taxes and the incidence of benefits. Thus Gruber's (1997) finding that the incidence of the Chilean payroll tax falls on workers tends to support the competitive model. However, he emphasizes that he cannot distinguish between the effects of taxes and benefits. His results would be consistent with an effect of the wage on productivity or even the efficiency wage model if workers valued the change in benefits sufficiently to offset the change in taxes.

As with most differences-in-differences studies, the results here are ultimately based on a small number of observations. Yet the results are surprisingly consistent across approaches. The earnings of high earners consistently rise faster in years when the earnings base increases than in the comparisons years. Moreover, in most specifications, this relative change is statistically significant implying that earnings rise in response to the tax increase. This provides support for the efficiency wage model. and tends to contradict the standard competitive model.

However, as noted in the theory section, there is really a range of models between the standard competitive model and the efficiency wage model. If wage increases raise worker productivity, then even though labor force participation is inelastic, the supply of effective labor units may be elastic. If the supply of effective labor units is sufficiently elastic, the nominal 50/50 split of the tax between workers and firms may not require wages to adjust. In general, the estimates in this paper are not sufficiently precise to reject this possibility. Thus we cannot reject the hypothesis that productivity is very responsive to wage increases but must be cautious in concluding that this effect is sufficient to prevent the labor market from clearing.

individuals work 2000 hours per year. If they worked fewer hours, the absolute values of the coefficient and endpoints of the confidence interval would be higher. However, for any reasonable scaling, the result excludes the full burden falling on workers.

7 REFERENCES

Akerlof, George A., "Gift Exchange and Efficiency Wage Theory: Four Views," *American Economic Review*, 74 (March 1984): 79-93.

Brown, Charles. "Equalizing Differences in the Labor Market." *Quarterly Journal of Economics* 94 (1980): 113-34.

Capelli, Peter and Chauvin, Keith, "An Interplant Test of the Efficiency Wage Hypothesis," *Quarterly Journal of Economics*, 106 (February 1991): 75-101.

Dickens, William T. and Katz, Lawrence F., "Interindustry Wage Differences and Industry Characteristics," in Kevin Lang and Jonathan S. Leonard, eds., *Unemployment and the Structure of Labor Markets*, Oxford:Basil Blackwell, 1987.

Donald, Stephen G. and Lang, Kevin, "Inference with Difference in Differences and Other Panel Data," Boston University, unpublished, 2000.

Feldstein, Martin and Samwick, Andrew, "Social Security Rules and Marginal Tax Rates," *National Tax Journal,* 45 (March 1992): 1-22.

Gibbons, Robert and Katz, Lawrence F., "Does Unmeasured Ability Explain Inter-industry Wage Differentials?," *Review of Economic Studies,* 59 (July 1992): 515-35.

Gruber, Jonathan, "The Incidence of Payroll Taxation: Evidence from Chile," *Journal of Labor Economics*, 15 (July 1997): S72-101.

Katz, Lawrence F. and Summers, Lawrence H., "Industry Rents: Evidence and Implications," *Brookings Papers on Economic Activity*, (1989): 209-75.

Krueger, Alan B., "Ownership, Agency, and Wages: An Examination of Franchising in the Fast Food Industry," *Quarterly Journal of Economics*, 106 (February 1991): 75-101.

Krueger, Alan and Summers, Lawrence, "Efficiency Wages and the Inter-Industry Wage Structure," *Econometrica*, 56 (January 1988): 259-93.

Lang, Kevin. "Persistent Wage Dispersion and Involuntary Unemployment." *Quarterly Journal of Economics* 106 (1991): 181-202.

Levine, David I, "Can Wage Increases Pay for Themselves? Test with a Production Function," *Economic Journal*, 102 (September 1992): 1102-15.

Montgomery, James D., "Equilibrium Wage Dispersion and Interindustry Wage Differentials," *Quarterly Journal of Economics*, 106(1), February 1991, 163-79.

Murphy, Kevin M. and Topel, Robert, "Unemployment, Risk and Earnings: Testing for Equalizing Differences in the Labor Market," in Kevin

Lang and Jonathan S. Leonard, eds., *Unemployment and the Structure of Labor Markets*, Oxford:Basil Blackwell, 1987.

Pisauro, Giuseppe, "The Effect of Taxes on Labour in Efficiency Wage Models," *Journal of Public Economics*, 46 (December 1991): 329-45.

Pisauro, Giuseppe, "Social Security Contributions and Efficiency Wage Theory: Incidence and Effects on Employment," in Baldassarri, Mario and Roberti, Paolo, eds., *Fiscal problems in the single-market Europe*, New York: St. Martin's Press, 1994: 127-51.

Rosen, Sherwin. "The Theory of Equalizing Differences," in *Handbook of Labor Economics,* edited by O. Ashenfelter and R. Layard. Amsterdam: North-Holland, 1986.

Shapiro, Carl and Stiglitz, Joseph E., "Equilibrium Unemployment as a Worker Discipline Device," *American Economic Review*, 75 (June 1984): 433-44.

Solow, Robert, "Another Possible Source of Wage Stickiness," *Journal of Macroeconomics*, 1 (1979): 79-82.

Weitzman, Martin, "A Theory of Wage Dispersion and Job Market Segmentation," *Quarterly Journal of Economics*, 104 (February 1989): 121-38.

TABLE 1 FICA TAX PROVISIONS (1967-1980)				
Year	**Earnings Base**	**Median Earnings Men (Year-Round/Full-Time)**	**Base/ Median**	**FICA Tax Rate** (employee share)
1967	6,600	7,182	0.92	4.4
1968	7,800	7,664	1.02	4.4
1969	7,800	8,455	0.92	4.8
1970	7,800	8,966	0.87	4.8
1971*	7,800	9,399	0.83	5.2
1972*	9,000	10,202	0.88	5.2
1973	10,800	11,186	0.97	5.85
1974	13,200	11,863	1.11	5.85
1975	14,100	12,758	1.11	5.85
1976*	15,300	13,455	1.14	5.85
1977	16,500	14,626	1.13	5.85
1978	17,700	15,730	1.13	6.05
1979	22,900	17,014	1.35	6.13
1980	25,900	18,612	1.39	6.13

*Annual demographic supplement cannot be matched with prior year.
Sources: Office of the Chief Actuary, Social Security Administration, History of the Provisions of Old Age Survivors and Disability and Health Insurance, http://www.ssa.gov/OACT/HOP/hoptoc.htm, Oct. 26, 1999 and U.S. Census Bureau, "Full-Time, Year-Round Workers (All Races) by Median Earnings and Sex: 1960 to 1998," http://www.census.gov/hhes/income/histinc/p38.html, Nov. 10, 1999.

TABLE 2 RELATION BETWEEN EX POST AND EX ANTE MEASURES OF HIGH EARNINGS						
	1979 Experiment		**1974 Experiment**		**1968 Experiment**	
Above Cut-Off in 2nd Year	**Above Cut-Off In 1st Year**					
	No	Yes	No	Yes	No	Yes
1979-80						
No	97.6	26.5	95.6	19.8	94.3	15.5
Yes	2.4	73.5	4.4	80.2	5.7	84.5
1978-79						
No	**96.4**	**17.3**				
Yes	**3.6**	**82.7**				
1976-77						
No	97.0	18.9	96.4	20.0	94.3	14.8
Yes	3.0	81.1	3.6	80.0	5.7	85.2
1974-75						
No	97.8	24.0	95.8	18.6	93.9	16.5
Yes	2.2	76.0	4.2	81.4	6.1	83.5
1973-74						
No			**95.6**	**18.0**		
Yes			**4.4**	**82.0**		
1969-70						
No	97.5	23.2	95.0	15.2	94.5	16.4
Yes	2.5	76.8	5.0	84.8	5.5	83.6
1967-68						
No					**92.5**	**8.7**
Yes					**7.5**	**91.3**

TABLE 3 **Differences in Earnings Increases**			
First-Stage Estimates (high-earnings workers 1st year)			
Year	1968	1974	1979
1968	**-234**	**-**	**-**
1970	-505	-569	-888
1974	-	**-259**	-
1975	-661	-742	-1081
1977	-585	-659	-1034
1979	-	-	**-735**
1980	-683	-803	-1190
Second-Stage Coefficients			
High based on 1st year	374 (90)	434 (114)	313 (140)
High based on 2nd year	364 (112)	365 (94)	465 (193)
Weighted by P(high 2nd yr.)	306 (73)	491 (102)	350 (128)
High based on 1st year (controls)	371 (161)	396 (189)	277 (200)
Restricted sample	285 (48)	445 (108)	285 (65)
Differences-in-differences (restricted sample)	66 (41)	124 (75)	130 (77)
Differences-in-differences (unrestricted sample)	261 (107)	242 (134)	140 (167)

Standard errors in parentheses. Controls: Age, sex, race, education, census "state", married spouse present, lives in an MSA and number of children under age 18.

TABLE 4
Results from Combining Experiments

	Estimated Fraction of Worker Tax Paid by Employers	95% Confidence Interval
Base Specification	1.20 (0.57)	-0.28, 2.67
Differences-in-Differences (Unrestricted sample)	0.57 (0.47)	-0.64, 1.79
Differences-in-Differences (Restricted sample)	0.45 (0.12)	0.15, 0.75
Differences-in-Differences (Unrestricted sample) - GDP deflator	-0.14 (1.19)	-3.20, 2.92
Differences-in-Differences (Restricted sample) - GDP deflator	0.17 (0.39)	-0.83, 1.18
PSID REPLICATION		
Base Specification	0.85 (1.00)	-1.72, 3.41
Differences-in-Differences (Unrestricted sample)	0.38 (0.50)	-0.90, 1.67
Differences-in-Differences (Restricted sample)	0.53 (0.35)	-0.36, 1.41
Differences-in-Differences (Restricted sample) - Hourly Wage	1.13 (0.56)	-0.32, 2.58