Effort and Wages: Evidence from the Payroll Tax

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Abstract. I show that under a canonical efficiency wage model, a per capita employment tax levied on the employer raises the wage. In contrast, under market-clearing, wages fall regardless of whether effort is contractible. I examine the effect of increases in the earnings base for the payroll tax in the United States on wages of high-wage workers for whom the change represents an increase in a per capita tax. In most specifications, the results suggest that wages rose, consistent with the efficiency wage model, but they are generally too imprecise to rule out large effects of wages on noncontractible productivity that are insufficient to prevent market-clearing. Provided labour demand is inelastic, the results are inconsistent with a model of contractible effort.

Résumé. If you do not provide a French abstract, the English abstract will be translated into French and inserted here.

JEL classification: H24, J30, J32

1. Introduction
Do higher wages increase productivity or is there merely an equilibrium relation between wages and productivity in which workers are compensated for higher effort? The workhorse hedonic model (see for example, Rosen 1986), in which the relation between wages and job characteristics adjusts

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to clear the market for heterogeneous jobs and workers, assumes the latter.
In contrast, many efficiency wage models assume the former. For example,
higher wages may increase worker morale (Solow 1979, Akerlof 1984) or deter
shirking (Shapiro and Stiglitz 1984). Importantly, in efficiency wage models,
the causal effect of higher wages on productivity is sufficiently large that even
in the presence of excess labour supply, firms choose not to lower wages. This
contrasts sharply with the hedonic model in which effort is contractible.

Of course, even if effort is not contractible, wages could have a positive
causal effect on productivity without this effect being sufficient to make firms
want to offer a wage above the minimum required to attract workers. In this
latter case, the labour market would clear despite the presence of forces that
motivate the efficiency wage models.

In this paper, I rely on the comparative statics of a change in a per capital
payroll tax applied to a group of workers with highly inelastic labour supply
to distinguish among the models. I contrast the results of the theory with
changes in the earnings of high-earnings workers following an increase in the
base to which the payroll tax was applied in the United States. For workers
with earnings sufficiently above the payroll tax earnings base, increases in this
base are essentially increases in a per capita payroll tax.

Under the standard competitive model with exogenous effort, if a tax
change affects a group of workers with highly inelastic labour 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.

If effort is contractible as in the model of compensating differentials, we
get a similar result. Worker utility will fall. Wages will fall and effort will rise.
If labour demand is elastic, the increased effort raises demand for workers.
Consequently the decline in earnings is less than the full amount of the tax
increase. More plausibly, the demand for labour is inelastic in which case the
increase productivity of labour reduces the demand for workers so that the
wage decline exceeds the firm’s share of the payroll tax.

In contrast, because there is excess labour (involuntary unemployment) in
equilibrium, efficiency wage models function very much like models in which
the supply of labour is perfectly elastic. Firms will prefer to hire fewer but
more productive workers. Therefore 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’ labour
force participation is likely to have been highly inelastic. In addition, low
earnings workers did not experience this tax increase.

The evidence points to wage increases that exceed workers' share of the
payroll tax although standard errors are sufficiently high that for most
specifications we cannot exclude the possibility that labour demand is
relatively inelastic, that effort is not contractible and effort responds positively
to wages but not sufficiently to interfere with market-clearing. Unless, labour
demand is quite elastic, the results are inconsistent with contractible effort.

These results are consistent with previous papers that asked whether
exogenous differences in wages generate greater productivity (Capelli and
Chauvin 1991, but see Parsons 2013 for the opposite result) or exogenous
increases in the cost of monitoring generate higher wages (Krueger 1991) but
which did not address 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 (Leonard, 1987; Neal, 1993). However, these
papers never address the question of what exogenous factor causes firms to
choose different combinations of wages and supervision. Ichino and Riphahn
(2005) consider exogenous changes in the ability of firms to dismiss workers
and finds that increased employment protection increases absenteeism.

I note that the approach taken here is also quite different from the large
literature on interindustry wage differentials, which asked whether the pattern
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).1

2. Background

The Federal Insurance Contributions Act (FICA) in the United States levies
taxes that are intended to cover the cost of providing social security and
Medicare (the national health plan for the elderly). Until 1991, FICA taxes
could be summarized by two parameters, the tax rate (nominally split equally
between employer and employee) and the earnings base. Taxes were levied on
all earnings up to the earnings base. Thereafter, no taxes were collected.2 The
history of FICA taxes between 1967 and 1980 is given in Table 1.

1 See also the large experimental literature on gift exchange of which the seminal paper is
Fehr et al (1998), Chen and Edin (2002) who use differences between piece-rate and
time-rate workers across industry and a meta-analysis by Krassoi Peach and Stanley
unemployment and higher wages, relations they predict from an efficiency wage model.

2 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 if a worker had more than
TABLE 1  
FICA tax provisions (1967-1980)

<table>
<thead>
<tr>
<th>Year</th>
<th>Earnings Base</th>
<th>Median Earnings Men (Year-Round/Full-Time)</th>
<th>Base/ Median</th>
<th>FICA Tax Rate (employee share)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1967</td>
<td>6,600</td>
<td>7,182</td>
<td>0.92</td>
<td>4.40</td>
</tr>
<tr>
<td>1968</td>
<td>7,800</td>
<td>7,664</td>
<td>1.02</td>
<td>4.40</td>
</tr>
<tr>
<td>1969</td>
<td>7,800</td>
<td>8,455</td>
<td>0.92</td>
<td>4.80</td>
</tr>
<tr>
<td>1970</td>
<td>7,800</td>
<td>8,966</td>
<td>0.87</td>
<td>4.80</td>
</tr>
<tr>
<td>1971*</td>
<td>7,800</td>
<td>9,399</td>
<td>0.83</td>
<td>5.20</td>
</tr>
<tr>
<td>1972*</td>
<td>9,000</td>
<td>10,202</td>
<td>0.88</td>
<td>5.20</td>
</tr>
<tr>
<td>1973</td>
<td>10,800</td>
<td>11,186</td>
<td>0.97</td>
<td>5.85</td>
</tr>
<tr>
<td>1974</td>
<td>13,200</td>
<td>11,863</td>
<td>1.11</td>
<td>5.85</td>
</tr>
<tr>
<td>1975</td>
<td>14,100</td>
<td>12,758</td>
<td>1.11</td>
<td>5.85</td>
</tr>
<tr>
<td>1976*</td>
<td>15,300</td>
<td>13,455</td>
<td>1.14</td>
<td>5.85</td>
</tr>
<tr>
<td>1977</td>
<td>16,500</td>
<td>14,626</td>
<td>1.13</td>
<td>5.85</td>
</tr>
<tr>
<td>1978</td>
<td>17,700</td>
<td>15,730</td>
<td>1.13</td>
<td>6.05</td>
</tr>
<tr>
<td>1979</td>
<td>22,900</td>
<td>17,014</td>
<td>1.35</td>
<td>6.13</td>
</tr>
<tr>
<td>1980</td>
<td>25,900</td>
<td>18,612</td>
<td>1.39</td>
<td>6.13</td>
</tr>
</tbody>
</table>

NOTES: * Denotes cannot be matched with previous year.

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. From 1974 to 1977, the earnings base was roughly constant relative to earnings in the economy, at around 1.12 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.

one employer in the course of the year, the employers’ share could be applied collectively to more than the earnings base.

Canadian Journal of Economics / Revue canadienne d’économique 20XX 00(0)
In what follows, I will concentrate on workers who would have earned more than the new earnings base. I treat 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 I provide following the main theory section.

3. Theory

Workers are homogeneous and coverage is universal. Labour force participation is perfectly inelastic since I am modelling the behaviour of high-earnings workers who typically are strongly attached to the labour market.\(^3\)

I begin with non-contractible effort. In this case, job characteristics are fixed. Wages may affect effort by motivating workers, but the firm cannot offer a contract that sets both wages and effort. I then consider what happens when effort is contractible; this model can be interpreted as allowing firms to alter worker conditions including hours, effort or the unpleasantness of the job. As extensions, I address substitutability among different types of workers when coverage is not universal and the endogeneity of the tax to the labour supply decision.

Note that since the empirical section examines year-to-year changes, the model assumes no firm entry or exit. The tax increase will therefore generally lower profit. In the extensions section I briefly discuss longer-term effects when profits must return to zero.

3.1. Equilibrium Effort and Wages

Consider a simple model based on the generic Solow (1979) efficiency wage model. There is a single competitive firm with profit function

\[ \pi = f(eL) - (w + T) \times L \]  

where \( e \) is efficiency units of labour and \( T \) is the employment tax. Note that output depends on the number of efficiency units, \( eL \), the price of output is normalized to equal 1, and the tax is (nominally) levied only on the firm. In the empirical part of the paper, it is important to remember that half of the FICA tax is nominally levied on the worker.\(^4\)

\(^3\) The possibility that high-wage workers are paid efficiency wages is consistent with Rebitzer and Taylor (1991) and Bulow and Summers (1986). In these models the low-wage sector is competitive.

\(^4\) Implicitly this specification assumes that the worker cares about her wage net of taxes. Nothing in the mathematics changes when effort is not contractible if she responds to her nominal wage but not to taxes that are nominally levied on the firm. In that case, \( T \) should be interpreted as the employer’s nominal tax rather than the full amount of the tax. It is, however, essential that workers do not value the taxes nominally levied on the firm as discussed in Kerschbamer and Kirchsteiger (2000).
Labour supply, measured in bodies, is fixed at \( L = 1 \).

Worker utility is given by
\[
    u = u(w, e)
\]  
with
\[
    u_e(w, 0) \geq 0, \\
    u_{ee} < 0,
\]
and
\[
    u_{ew}(w, e) \geq 0.
\]

Note that when
\[
    u_{ew}(w, 0) = 0, \quad \forall w,
\]
we are in the standard environment in which wages have no incentive effects.

### 3.2. Effort not contractible

When effort is not contractible, the worker chooses effort to maximize utility given the wage or
\[
    u_e(w, e) = 0,
\]
from which we have
\[
    \frac{de}{dw} = -\frac{u_{ew}}{u_{ee}} =: e'(w) \geq 0.
\]
The interesting case is when \( u_{ew} > 0 \), in which case the solution to the differential equation yields
\[
    e = e(w).
\]
Further, assume directly that \( e''(w) < 0 \).

Maximizing profit with respect to \( L \) and \( w \) gives the first-order conditions
\[
    f'e = w + T \\
    f'e' \leq 1
\]
When (11) holds with equality, we have the canonical efficiency wage model except that
\[
    \frac{e'}{e}(w + T) = 1
\]
rather than the more standard \( e'w/e = 1 \). With some abuse of terminology, I will refer to \( e'(w + T)/e \) as the elasticity of effort, denoted \( \varepsilon_s \). When (11) is a strict inequality, we have a market clearing model but in which wages increase productivity if \( e' > 0 \).
3.2.1. Wage Effects of an Employment Tax: Market-Clearing

Let us begin with the more standard market-clearing case. We can solve (10) for \( L \) to get

\[
L = \frac{g\left(\frac{w+T}{e}\right)}{e}
\]

(13)

where \( g = f'^{-1} \) and is the demand for effective labour units. Using the assumption that \( L \) is fixed, fully differentiate the right-hand-side of (13) to get

\[
\frac{dw}{dT} = -\frac{g'\left(1 - \frac{(w+T)e'}{e}\right)}{g'\left(1 - \frac{(w+T)e'}{e}\right) - ge'}
\]

(14)

or

\[
\frac{dw}{dT} = -\frac{\varepsilon_d}{\varepsilon_d(1 - \varepsilon_s) - \varepsilon_s}
\]

(15)

where \( \varepsilon_d \) is the elasticity of demand for effective labour units with respect to their price, \( (w + T)/e \). Recall that participation is fixed. It is the possibility of a change in effort that permits effective supply to be elastic.

In the special case of the standard competitive model, \( e' = 0 \). Then

\[
\frac{dw}{dT} = -1,
\]

the standard result that when labour supply is perfectly inelastic, labour 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 \( dw/dT \approx \varepsilon_d \). 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.

3.2.2. Wage Effects in the Efficiency Wage Model

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

\[
e'(w + T) / e = \varepsilon_s = 1,
\]

(16)

or in other words, the wage is set at the point at which the elasticity of effort with respect to the wage equals 1.\(^5\)

Fully differentiating yields

\[5\] The assumptions that \( f'' < 0 \) and \( e'' < 0 \) ensure that the second-order conditions are satisfied.
\[ \left( \frac{e'}{e} + \frac{e''(w + T)}{e} - \frac{(e')^2(w + T)}{e^2} \right) dw = -\frac{e'}{e} dT. \] (17)

But applying (16) to the third term on the left-hand-side and rearranging terms gives

\[ \frac{dw}{dT} = -\frac{e'}{e'(w + T)} > 0. \] (18)

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 labour 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 labour means that the model operates as if the supply of labour 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.\(^6\)

The Solow model used here 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 labour 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, \(d^2e/dwdv = 0\), and the analysis is unchanged. Pisauro (1991 and 1994) analyses 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. It seems natural to write \(e = e[w/(g(u)w^*)]\) 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

\[ \frac{e'(w + T)}{e} - g(u)w^* = 0 \] (19)

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\(^6\)The maintained assumption has been that workers care about their net wage. If workers care about their nominal gross wage, then the model predicts that this will rise. Note that in both cases there is a discrete difference between the upper limit of the market-clearing model and the prediction of the efficiency wage model. When effort depends on wages net of taxes, wages rise by at least the workers share under the efficiency wage model and should not rise at all under market-clearing. When they care about their nominal gross wage, this should rise under the efficiency wage model but fall by at least half the employer’s nominal share under market-clearing.
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.

Finally, I note that I have ignored other taxes, in particular the income tax, which is applied to the worker’s share of the payroll tax. It is easily proved that the after-(income) tax wage must rise in response to an increase in the payroll tax.\(^7\) During the period I will be examining, marginal tax rates hovered around 20% for a family of four with one earner and the median income (Tax Policy Center, 2002). Therefore, under the efficiency wage model, earnings should rise by at least about 1.25 times the worker’s share of the payroll tax increase.

3.3. Contractible effort

If effort is contractible, the firm maximizes profits subject to providing the competitive level of utility. In this case, we have

\[
L = f(eL) - (w + T) L + \lambda (u(w, e) - u^*) \tag{20}
\]

and the corresponding first-order conditions

\[
f' e - (w + T) = 0 \tag{21}
\]

\[
f' L + \lambda u_e = 0 \tag{22}
\]

\[-L + \lambda u_w = 0 \tag{23}
\]

\[u(w, e) = u^*. \tag{24}
\]

It can readily be verified that if in this setting, the labour supply elasticity were infinite at \( u^* \), the result would be analogous to the efficiency wage setting. Firms would want to hire fewer workers but demand more effort. However, since labour supply is inelastic, instead \( u^* \) falls. As a result \( e \) increases and \( w \) declines.

I prove in the appendix:

**Proposition 1.** **If the profit function is given by (20) and labour supply is perfectly inelastic, then \( \frac{dw}{dT} < 0 \). Moreover, if \( f'' e + f' < 0 \), \( \frac{dw}{dT} < -1 \).**

The condition that \( f'' e + f' < 0 \) is essentially a statement about the elasticity of demand. If workers become more productive, firms reduce their

\[^7\] This assumes that effort depends on the after-tax wage so that the efficiency wage condition becomes \( e (1 - t) (w + T) / e = 1 \).
demand for bodies. Thus the condition corresponds to $\varepsilon_d > -1$ in a more standard model. Thus with contractible effort we would expect wages to fall by more than the increase in the per capita self-employment tax.

### 3.4. Extensions and Issues

#### 3.4.1. The Payroll Tax is an Impure Employment Tax

I have modeled the FICA tax as a pure employment tax. In fact, while a small change in labour supply does not affect the tax, it is possible for a worker to reduce the tax by a sufficient reduction in labour supply. The effect of recognizing this complexity is most easily analysed if we model the tax as being collected from the worker so that labour supply depends on the after-tax hourly wage.

If $wh_i^* < Y^*$ where $Y^*$ is maximum taxable earnings, worker $i$’s labour supply is determined by

$$h_i^* = h_i^*(1 - t)w_i, \theta_i, Y_i^a)$$

(25)

where $t$ is the tax rate and $\theta$ is a vector of factors that determine tastes for leisure and $Y_i^a$ is nonlabour income.

If $wh_i^* > Y^*$, worker $i$’s labour supply is determined by

$$h_i^* = h_i^*(w_i, \theta_i, Y_i^a - tY^*)$$

(26)

Of course, this kinked budget line implies a nonconvexity. The solution to (25) may give $wh_i^* < Y^*$ while at the same time the solution to (26) gives $wh_i^* > Y^*$. 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 labour supply.

Note, however, that if this were the case, we would find large reductions in earnings among affected workers leading us to conclude that the tax lowered earnings. The only difficulty would arise if the overall reduction in labour supply were sufficiently large to increase the earnings of workers who do not alter their labour supply. In the empirical work below, I examine the importance of workers shifting from above the kink to below the kink in the budget line.

#### 3.4.2. 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 retirement 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

Canadian Journal of Economics / Revue canadienne d’économique 20XX 00(0)
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 one for one 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.

3.4.3. Substitution Between Low- and High-Earnings Workers
So far I 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, this choice would be fully justified. 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, labour
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 labour 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, substituting low earners for high earners will raise
the earnings of the former and further reduce the earnings of the latter. Thus
substitutability among workers increases our expectation that the earnings of
high-earnings workers will fall under the market-clearing model.

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).

3.4.4. Long-Run Effects
The assumption that \( f'' < 0 \), implicitly means that there must be some
fixed cost of operating. If not, firm size would shrink to zero. It is relatively
straightforward to examine the long-run effects of the tax increase in either of
the competitive models by solving the dual of the firm’s problem. If we assume
that the market maximizes worker utility subject to constant firm profit, we derive the effect of a tax increase in the long run after the number of firms in the market has adjusted.

In the appendix, I show that we continue to find that under the market-clearing model, \( \frac{dw}{dT} < 0 \). However, the wage decrease never exceeds the employer's share when effort is contractible while it always does so when the market clears and effort is not contractible, the opposite of what we believe is plausible in the short-run.

The easiest way to extend the efficiency wage model to allow for entry and exit is to assume that the firm's cost of finding workers declines as the employment rate falls but that the relation between effort and wages is independent of the employment rate. In this case, the wage predictions of the model are unchanged.

### 3.4.5. Other Models of Wage Determination

It is impossible within an article of reasonable length to address the wide variety of models of wage determination. I have generally referred to \( e \) as "effort" although in some settings it might just represent effective labour units. Thus higher wages might attract higher quality workers where that higher quality is unobserved by the econometrician but may or may not be observed by firms. In the latter case, we have the potential for an adverse selection model (e.g. Weiss 1980) of efficiency wages. The arguments in this paper would go through with a single competitive firm, but the efficiency-wage equilibrium with multiple firms is model dependent.

There is ample evidence that higher wages do induce more applicants and reduce turnover. Since this might lead firms to pay above market-clearing wages, certain search models (e.g. Montgomery 1991) have been described as recruiting or turnover efficiency wage models although since these models have since been developed separately from the efficiency wage literature, it is probably better to view them as a different class of models. Again it is impossible to do justice to the full variety of such models. Using a version of Montgomery's model with a large number of workers and firms and a fixed cost of opening a vacancy, it is possible, although somewhat tedious, to show that the wage falls but not by the full amount of the tax.\(^8\)

Finally, there is also evidence that firms sometimes use outsourcing to shed low-wage workers in order to reduce their wages (e.g. Goldschmidt and Schmieder 2017). Firms' inability to lower the wages directly of such workers might reflect the sorts of fairness concerns emphasized in this paper but also might reflect workers' ability to capture rents. Certain rent-sharing models (e.g. Dickens 1986) in which firms share rents in order to forestall

\(^8\) For recent evidence see Schmieder (2013).

\(^9\) Proof available upon request.
collective action by their workers have also been described as efficiency wage models although, again, these also have now developed separately and should probably be viewed as a distinct class of models. Since increases in the tax reduce the rents to be shared, we would expect the wage to fall albeit by less than the tax increase. But again, this may depend on the specifics of the models such as how the outside wage is determined.

3.5. 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-earners workers even when the labour force participation of high-earners is perfectly inelastic. Although I present results that are general, I find those in which labour demand is relatively inelastic to be most plausible. For a demand elasticity of $-0.5$, in the short run earnings will fall by between half and all of the tax increase if effort is not contractible. The upper limit is reached when the elasticity of effort with respect to the wage is just less than 1. Since the tax is nominally divided evenly between the worker and firm, earnings could fall by anywhere between roughly zero and the firm’s share of the tax. For the same labour demand elasticity, if effort is contractible, in the short run earnings should fall by more than the firm’s share.

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 after-tax earnings if there is no associated benefit increase from the tax increase. Thus we would expect earnings to rise by more than the worker’s share of the payroll tax increase assuming that workers do not perceive any significant increase in benefits from the tax increase. If instead, workers respond to their nominal gross wage, their nominal gross wage will rise.

Therefore if we can reject that the nominal gross wage fell or stayed constant, we must be in an efficiency wage world. If we can reject that it rose or stayed constant, the efficiency wage model in which workers are not motivated by the firm’s total payments is rejected. Finally, if we can reject that the nominal gross wage fell by the employer’s share or more, we can conclude that increased wages have incentive effects and that effort is not contractible. In sum, the critical questions are whether we can reject that $\frac{dw}{dT} = -1$ and whether we can reject $\frac{dw}{dT} = 0$ and in the latter case whether the rejection is against an alternative hypothesis that $\frac{dw}{dT} > 0$ or $\frac{dw}{dT} < 0$.

4. Data and Methods

If we could randomly assign workers to separate markets with different earnings bases, measuring the earnings effect of an 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.\textsuperscript{10} 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 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\%. I 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 1 percentage point.
4. 1979-80 when the base rose by 4 percentage points\textsuperscript{11}

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.

\textsuperscript{10} While it might appear that it is possible to simply look 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 earning $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.

\textsuperscript{11} 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.
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. 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 base even in the absence of a change and who continue to do so after the increase. Our inability to identify such workers directly is a significant problem. I employ a number of strategies to address it.

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. I
use both approaches but rely more heavily on classifications based on first-year earnings.

In each case, I 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. I 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. I 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

\[ \Delta w_i = \bar{w}_{it} + \epsilon_{it} - \bar{w}_{it-1} - \epsilon_{it-1} \]

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

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

where \( Y^* \) is the new earnings base. We expect our estimates \( \Delta w_i \) 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 I draw the sample on the basis of second-year earnings, the sample will overrepresent people with high transitory second-year earnings, and our estimates of \( \Delta w_i \) 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,

\[
E(\Delta w|base\_change) - E(\Delta w|no\_change) = \Delta \bar{w}|base\_change - \Delta \bar{w}|no\_change
+ E(\epsilon_{it-1}|\bar{w}_{it-1} + \epsilon_{it-1} > Y^* & base\_change)
- E(\epsilon_{it-1}|\bar{w}_{it-1} + \epsilon_{it-1} > Y^* & no\_change)
= (\Delta \bar{w}|base\_change) - (\Delta \bar{w}|no\_change).
\]
While our sample is quite large since it draws on microdata from seven March CPS surveys, as emphasized in Donald and Lang (2007), 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 I 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, I 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 I base the sample on second-period earnings and negative when I base it on first-period earnings since we expect individuals with high wages to have experienced high wage growth. I use the median rather than the mean, because it is important to avoid spurious sources of differences in the \( \hat{c} \)'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 \( \hat{c} \)'s, are used as dependent variables in a regression of the following form:

\[
\hat{c}_t = a_0 + a_1 \text{year base changed}_t + u_t. \tag{27}
\]

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 labour force, I also estimate the following equation

\[
\text{wage}_{it} - \text{wage}_{it-1} = X_i B_t + c_t + e_{it} \tag{28}
\]

for each pair of years. Again the sample is restricted to the high-earnings group. I regress the estimated \( c \)'s using (27).

As before, equation (28) has no structural interpretation. Since we rely on quantile regression, it simply provides the best 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 (28).

Eissa (1995) tests for effects of the 1986 income tax reform by comparing
labour 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, I
mimic this strategy. I estimate the equivalent of (28) 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, I use the difference between $\hat{c}_t$ for high-
earnings and low-earnings workers as the dependent variable in (27). 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 that are not captured by trends in median earnings
for which I 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 informa-
tion on earnings the previous year. The data used here are from the Unicon
CPS files. For each pair of years, I 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.\(^{12}\) 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 on earnings are described in the presentation of
the results. For example, when I 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, I 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, I do not have to address

\(^{12}\) 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.
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, I am, 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 (27) and (28) 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.

I combine the three experiments in the following way, which again follows
the procedure proposed by Donald and Lang. I 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. I 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. I 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.
An efficiency wage model with no offsetting benefit increases would suggest a
coefficient substantially above 1 since after-income tax wages must rise.

5. Results

I 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 7.1% 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%
TABLE 2
Relation between ex post and ex ante measures of high earnings

<table>
<thead>
<tr>
<th>Above Cut-Off</th>
<th>1979 Experiment</th>
<th>1974 Experiment</th>
<th>1968 Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>in 2nd Year</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>1979-80</td>
<td>97.6</td>
<td>26.5</td>
<td>95.6</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>73.5</td>
<td>4.4</td>
</tr>
<tr>
<td>1978-79</td>
<td>96.4</td>
<td>17.3</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>3.6</td>
<td>82.7</td>
</tr>
<tr>
<td>1976-77</td>
<td>97.0</td>
<td>18.9</td>
<td>96.4</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>81.1</td>
<td>3.6</td>
</tr>
<tr>
<td>1974-75</td>
<td>97.8</td>
<td>24.0</td>
<td>95.8</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>76.0</td>
<td>4.2</td>
</tr>
<tr>
<td>1973-74</td>
<td>95.6</td>
<td>18</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>4.4</td>
<td>82</td>
</tr>
<tr>
<td>1969-70</td>
<td>97.5</td>
<td>23.2</td>
<td>95</td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>76.8</td>
<td>5.0</td>
</tr>
<tr>
<td>1967-68</td>
<td>92.9</td>
<td>13.9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td>7.1</td>
<td>86.1</td>
</tr>
</tbody>
</table>

NOTES: * Denotes cannot be matched with prior year.
NOTES: Each cell is a percentage. Pairs in columns add to 100 percent.

of workers who reported earning more than the 1979 earnings base in 1979 reported earning less than that base in 1980. In contrast, only 13.9% 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
TABLE 3
Differences in earnings increases

First-Stage Estimates (high-earnings workers 1st year)

<table>
<thead>
<tr>
<th>Year</th>
<th>1968</th>
<th>1974</th>
<th>1979</th>
</tr>
</thead>
<tbody>
<tr>
<td>1968</td>
<td>-234</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>1970</td>
<td>-416</td>
<td>-487</td>
<td>-787</td>
</tr>
<tr>
<td>1974</td>
<td>-259</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>1975</td>
<td>-661</td>
<td>-742</td>
<td>-1081</td>
</tr>
<tr>
<td>1977</td>
<td>-585</td>
<td>-659</td>
<td>-1034</td>
</tr>
<tr>
<td>1979</td>
<td>-</td>
<td>-</td>
<td>-735</td>
</tr>
<tr>
<td>1980</td>
<td>-683</td>
<td>-803</td>
<td>-1190</td>
</tr>
</tbody>
</table>

Second-Stage Coefficients

<table>
<thead>
<tr>
<th>High based on 1st year</th>
<th>352*</th>
<th>414*</th>
<th>288</th>
</tr>
</thead>
<tbody>
<tr>
<td>(136)</td>
<td>(153)</td>
<td>(191)</td>
<td></td>
</tr>
<tr>
<td>High based on 2nd year</td>
<td>346*</td>
<td>346*</td>
<td>437</td>
</tr>
<tr>
<td>(143)</td>
<td>(117)</td>
<td>(249)</td>
<td></td>
</tr>
<tr>
<td>Weighted by P(high 2nd yr.)</td>
<td>281*</td>
<td>450*</td>
<td>313</td>
</tr>
<tr>
<td>(114)</td>
<td>(146)</td>
<td>(189)</td>
<td></td>
</tr>
<tr>
<td>High based on 1st year (controls)</td>
<td>344</td>
<td>376</td>
<td>311</td>
</tr>
<tr>
<td>(177)</td>
<td>(193)</td>
<td>(188)</td>
<td></td>
</tr>
<tr>
<td>Men only</td>
<td>383*</td>
<td>363*</td>
<td>301</td>
</tr>
<tr>
<td>(124)</td>
<td>(127)</td>
<td>(177)</td>
<td></td>
</tr>
<tr>
<td>Restricted sample</td>
<td>314**</td>
<td>378**</td>
<td>288*</td>
</tr>
<tr>
<td>(86)</td>
<td>(98)</td>
<td>(106)</td>
<td></td>
</tr>
<tr>
<td>Differences-in-differences (restricted sample)</td>
<td>203</td>
<td>227</td>
<td>121</td>
</tr>
<tr>
<td>(96)</td>
<td>(146)</td>
<td>(79)</td>
<td></td>
</tr>
<tr>
<td>Differences-in-differences (unrestricted sample)</td>
<td>242</td>
<td>139</td>
<td>121</td>
</tr>
<tr>
<td>(139)</td>
<td>(157)</td>
<td>(198)</td>
<td></td>
</tr>
</tbody>
</table>

NOTES: Controls are age, sex, race, education, census “state”, married spouse present, lives in an MSA and number of children under age 18.
* Denotes significance at .1 level; ** Denotes significance at .05 level.

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 \( \frac{2}{3} \times \frac{2}{3} \times \frac{2}{3} \) or \( \frac{8}{27} \), 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 (2007), I 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 288 for the 1979 change to 414 for the 1974 change, but each difference is measured with a large standard error. For the 1968 and 1974 changes I can reject at the .1 level the hypothesis that the tax has no effect on workers’ earnings. 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 by the firm’s share due to the tax increase.\(^{13}\)

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,

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\(^{13}\) At the .05 level in 1974 and 1979 and just shy of the .05 critical value in 1968.
to those obtained when the sample is determined on the basis of first-period earnings although the significance levels at which we can reject full shifting to the worker decline somewhat.

To avoid some of the problems associated with dividing the sample on the basis of first- or second-period earnings, I 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. I 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 labour 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 labour force composition. To control for this, I 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.

While I have assumed that the labour force participation of all high-earnings workers is inelastic, it is possible that high-earnings women are more likely to withdraw from the labour force in response to a tax increase. The fifth row therefore restricts the sample to men. The results are similar to those obtained in the first row.

We might be concerned if the results were being driven by very high-earnings workers. Therefore in the sixth 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 although their statistical significance increases. 

14 These are the best results in the sense that there is no built-in bias as there is in the differences-in-differences estimates discussed next, and the standard errors are the lowest of any specification. This conclusion comes ex post. Honesty requires that they be presented as a robustness check rather than as the central results.
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 than the other estimates.

I 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 1.11, 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 very important incentive effects. The extreme model in which there are no incentive effects and workers
TABLE 4  
Results from combining experiments

<table>
<thead>
<tr>
<th></th>
<th>Estimated Fraction of Worker Tax Paid by Employers</th>
<th>90% Confidence Interval</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Base Specification</strong></td>
<td>1.11</td>
<td>-0.12, 2.34</td>
</tr>
<tr>
<td></td>
<td>(0.61)</td>
<td></td>
</tr>
<tr>
<td><strong>Restricted sample</strong></td>
<td>1.19</td>
<td>0.03, 2.35</td>
</tr>
<tr>
<td></td>
<td>(0.58)</td>
<td></td>
</tr>
<tr>
<td><strong>Differences-in-Differences (Unrestricted sample)</strong></td>
<td>0.44</td>
<td>-0.52, 1.39</td>
</tr>
<tr>
<td></td>
<td>(0.47)</td>
<td></td>
</tr>
<tr>
<td><strong>Differences-in-Differences (Restricted sample)</strong></td>
<td>0.51</td>
<td>-0.22, 1.24</td>
</tr>
<tr>
<td></td>
<td>(0.36)</td>
<td></td>
</tr>
<tr>
<td><strong>PSID REPLICATION</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Base Specification</strong></td>
<td>0.85</td>
<td>-1.16, 2.86</td>
</tr>
<tr>
<td></td>
<td>(1.00)</td>
<td></td>
</tr>
<tr>
<td><strong>Restricted sample</strong></td>
<td>1.08</td>
<td>-0.77, 2.94</td>
</tr>
<tr>
<td></td>
<td>(0.92)</td>
<td></td>
</tr>
<tr>
<td><strong>Differences-in-Differences (Unrestricted sample)</strong></td>
<td>0.49</td>
<td>-0.67, 1.66</td>
</tr>
<tr>
<td></td>
<td>(0.58)</td>
<td></td>
</tr>
<tr>
<td><strong>Differences-in-Differences (Restricted sample)</strong></td>
<td>0.68</td>
<td>-0.05, 1.42</td>
</tr>
<tr>
<td></td>
<td>(0.37)</td>
<td></td>
</tr>
<tr>
<td><strong>Differences-in-Differences (Restricted sample) - Hourly Wage</strong></td>
<td>1.19</td>
<td>-0.01, 2.38</td>
</tr>
<tr>
<td></td>
<td>(0.59)</td>
<td></td>
</tr>
</tbody>
</table>

Earnings fall by the full amount of the tax lies well outside the 90% confidence interval.\(^{15}\)

An alternative approach to Donald and Lang, which does not require the assumption that the error term is normally distributed is to consider the 209 permutations of the data in addition to the actual combination.\(^{16}\) Of these, 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.

\(^{15}\) This idea is due to Andreas Hagemann (forthcoming).

\(^{16}\) This idea is due to Andreas Hagemann (forthcoming).
10 have a coefficient that is greater than the one obtained from the data, suggesting that the coefficient is significantly different from 0 at the .1 level. However, a total of 24 have coefficients that are greater in absolute value than the true coefficient, suggesting that the coefficient falls somewhat short of statistical significance at that level. Since both approaches to determining statistical significance are common in the literature, it is difficult to draw a strong conclusion.

The second line restricts the sample to individuals who exceed the cutoff by no more than $5,000. The results are quite similar to those in the first line but somewhat more precise. We can now reject a coefficient of 0 at the .1 level. The results of the randomization inference are, however, the opposite of those for the full sample. Only 17 of the 209 permutations of the data yield coefficients that exceed in absolute value the coefficient using the true data, suggesting that we can reject the null of 0 at the .1 level. However, 13 of the permutations yield coefficients yield more positive coefficients, suggesting the opposite.

The third and fourth 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 a little less than half of the worker share of the tax is shifted to employers. The 90% 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 (.51) is similar to that obtained using the full sample but the estimate is somewhat more precise. The results strongly rule out the standard model without incentive effects.

6. 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 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 rise by almost the entire amount of the tax increase but the confidence interval includes all economically interesting values. With the restricted sample, the coefficient exceeds 1 and the 90% confidence interval excludes full shifting. When we rely on differences-in-differences, the point estimate falls, but we are still able to rule out the conclusion that earnings fall by the full amount of the tax increase. Restricting 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 and come close to ruling out no increase in earnings. Finally, using the hourly wage instead of annual earnings generates even more imprecise results. Point estimates were generally positive but occasionally negative. Only in the case of the differences-in-differences can we reject the hypothesis that wages fall by the full amount of the increase.\footnote{The scaling using hourly wages is somewhat arbitrary. This scaling presumes that 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. 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 in standard competitive and efficiency-wage settings.

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 labour force participation is inelastic, the supply of effective labour units may be elastic. If the supply of effective labour 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 labour market from clearing. We can, however, reject contractible effort unless we believe firm entry and exit happens very quickly.

Experience presenting this paper in seminars suggests that table 4 is some sort of Rorschach test for economists. Those who are disinclined to believe the efficiency wage hypothesis read the table one way while those inclined to believe the hypothesis read the table the opposite way. The most important point is that the table strongly suggests that incentive effects of wages are important.
8. REFERENCES


Hagemann, A. (forthcoming) “Placebo Inference on Treatment Effects When the Number of Clusters Is Small” *Journal of Econometrics*


Appendix A1: Proof of Proposition 1

A1. Preliminaries:
Combining (22) and (23) gives
\[ f' = -\frac{u_e}{u_w}. \] (A1)

Because \( e \) is a bad not a good, the marginal rate of substitution between \( e \) and \( w \) denoted by \( u_e/u_w \) becomes more negative when either \( e \) or \( w \) increases. Thus
\[ \frac{u_{ee}}{u_w} - \frac{u_{we}u_e}{u_w^2} = \frac{u_{ee} + f'u_{we}}{u_w} < 0 \] (A2)
\[ \frac{u_{we}}{u_w} - \frac{u_{ww}u_e}{u_w^2} = \frac{u_{we} + f'u_{ww}}{u_w} < 0. \] (A3)

Since \( u_w > 0 \), both numerators are negative.

The set of points satisfying (A1) has slope
\[ \frac{dw}{de} = -\frac{u_{we}f'' + u_{ee} + f'u_{ew}}{u_{ew} + f'u_{ww}} \] (A4)
which is negative, where the derivation uses the fact that \( u_e = -u_wf' \).

Moreover, the combinations of \( w \) and \( e \) satisfying (21) have slope
\[ \frac{dw}{de} = f''e + f' \] (A5)
which may be positively or negatively sloped. For stability, we require the difference between the two slopes to satisfy
\[ f''e + f' + \frac{u_{we}f'' + u_{ee} + f'u_{ew}}{u_{ew} + f'u_{ww}} > 0 \] (A6)
or
\[ (f''e + f')(u_{ew} + f'u_{ww}) + u_{we}f'' + u_{ee} + f'u_{ew} < 0. \] (A7)

A1. Body of proof
Now fully differentiate (21) and (A1) with respect to \( w, e \) and \( T \) to get
\[ (f''e + f')de - dw = dT \] (A8)
\[ \left( f'' + \frac{u_{ee} + f'u_{we}}{u_w} \right)de + \frac{u_{we} + f'u_{ww}}{u_w}dw = 0 \] (A9)

Solving gives
\[ \frac{dw}{dT} = -\frac{f''u_w + u_{ee} + f'u_{we}}{(f''e + f')(u_{we} + f'u_{ww}) + f''u_w + u_{ee} + f'u_{we}} \] (A10)
\[ \frac{dw}{dT} = \frac{u_{we} + f'u_{ww}}{(f''e + f')(u_{we} + f'u_{ww}) + f''u_w + u_{ee} + f'u_{we}}. \] (A11)
Since both numerators are negative by (A2), (A3) and $f'' < 0$, $dw/dT$ and $de/dT$ have opposite signs. Moreover by (A7), the denominator must be negative and thus $dw/dT < 0$.

Appendix A2: The Effect of the Tax Increase with Firms

Entry/Exit

A2. Contractible Effort

Maximize

$$L = u(w, e) + \mu (f(eL) - (w + T)L)$$

(A12)

with respect to $L, e, w$ and $\mu$ to get the first-order conditions.

$$u_w - \mu L = 0$$

(A13)

$$u_e + \mu f'eL = 0$$

(A14)

$$\mu (f'e - (w + T)) = 0$$

(A15)

$$f(eL) - (w + T)L = 0$$

(A16)

Fully differentiating and solving for $dw/dT$ and $de/dT$ and setting $L = 1$ gives

$$\frac{dw}{dT} = -\frac{u_{ew}f' + u_{ee}}{u_{ew}(f')^2 + 2u_{ew}f' + u_{ee}} < 0$$

(A18)
\[ \frac{de}{dT} = \begin{vmatrix} u_{ww} & 0 & -u_w & -1 \\ u_{ew} & 0 & u_w(f''e + f') & f' \\ -u_w & u_w & u_w f'' e^2 & 0 \\ -1 & 1 & 0 & 0 \end{vmatrix} \]

(A19)

\[ \frac{de}{dT} = \frac{u_{ww} f' + u_{ew} f'' e^2}{u_{ww} (f')^2 + 2 u_{ew} f' + u_{ee}} > 0 \]  

(A20)

**A2. Noncontractible Effort**

Maximize

\[ L = u(w, e(w)) + \mu (f(e(w))L - (w + T)L) \]  

(A21)

with respect to \( w, \) \( L \) and \( \mu \) gives the first order conditions.

\[ u_w + u_e e' + \mu (f'e'L - L) = 0 \]  

(A22)

\[ \mu (f'e' - (w + T)) = 0 \]  

(A23)

\[ f(e(w))L - (w + T)L = 0. \]  

(A24)

Fully differentiating and solving for \( dw/dT \) and setting \( L = 1 \) gives

\[ dw/dT = \frac{1}{f'e' - 1} < -1. \]  

(A26)