Abstract

Becker's theory of human capital predicts that minimum wages should reduce training investments for affected workers because they prevent these workers from taking wage cuts necessary to finance training. In contrast, in noncompetitive labor markets, minimum wages tend to increase training of affected workers because they induce firms to train their unskilled employees. We provide new estimates on the impact of the state and federal increases in the minimum wage in the US between 1987 and 1992 on the training of low-wage workers. We find no evidence that minimum wages reduce training, and little evidence that they tend to increase training. We therefore develop a hybrid model where minimum wages reduce the training investments of workers who were taking wage cuts to finance their training, while increasing the training of other workers. Finally, we provide some evidence consistent with this hybrid model.

Keywords: Imperfect Labor Markets, Low Wage Workers, General Human Capital, Firm Sponsored Training
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Daron Acemoglu is a member of MIT. J`rn-Steffen Pischke is a member of the Centre for Economic Performance, London School of Economics; s.pischke@lse.ac.uk

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Minimum Wages and On-the-Job Training

Daron Acemoglu and Jörn-Steffen Pischke

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1 Introduction

Much of the recent debate on the minimum wage has focused on its employment implications. The theory of human capital suggests that minimum wages should also have important adverse effects on human capital accumulation. In the standard human capital theory, as developed by Becker (1964), Ben-Porath (1967), and Mincer (1974), a large part of human capital is accumulated on the job, and workers often finance these investments through lower wages. A binding minimum wage will therefore reduce workplace training, as it prevents low wage workers from accepting the necessary wage cuts (Rosen, 1972). The early empirical literature has confirmed this prediction. The negative impact on human capital formation has been an important argument against minimum wages in the minds of many economists and policy-makers, and an important piece of evidence in support of the standard theory of human capital.

In this paper, we revisit the impact of minimum wages on training. We build on our previous work, Acemoglu and Pischke (1999b), which showed that a compression in the structure of wages can induce firm-sponsored training. We show that in noncompetitive labor markets minimum wages can increase—rather than decrease—training investments because they compress the wage structure.

The intuition for this result is that minimum wages make it less profitable to employ unskilled workers. When there are no rents in the employment relationship, as in a competitive labor market, the firm has no option but to lay off workers who were previously paid below the new minimum wage. In contrast, in the presence of labor market rents, it may be more profitable to increase the productivity of workers, who are already receiving high wages, rather than laying them off. Figure 1 illustrates this intuition diagrammatically. It draws the relation between worker skills, τ, productivity, $f(\tau)$, and wages $w(\tau)$. The gap between productivity and wages, Δ, is the rent that the firm obtains. A binding minimum wage, in the absence of such rents, forces the firm to lay off the worker. However, with Δ sufficiently high, the firm would like to retain the worker despite the higher wages dictated by the minimum wage. In this case, the firm would also like to increase the productivity of the worker. Without the minimum wage, the gap between $f(\tau)$ and $w(\tau)$ was constant, so there was no point in incurring costs of training. However, with a minimum wage, profits are less at $\tau = 0$ than at $\tau = 1$. So if the firm can increase its employee's skills to $\tau = 1$ at a moderate cost, it will prefer to do so. In essence, the minimum wage has made the firm the *de facto* residual claimant of the increase in the worker's productivity, whereas without the minimum wage, the worker was the residual claimant.

This reasoning suggests that a binding minimum wage may induce firms to invest

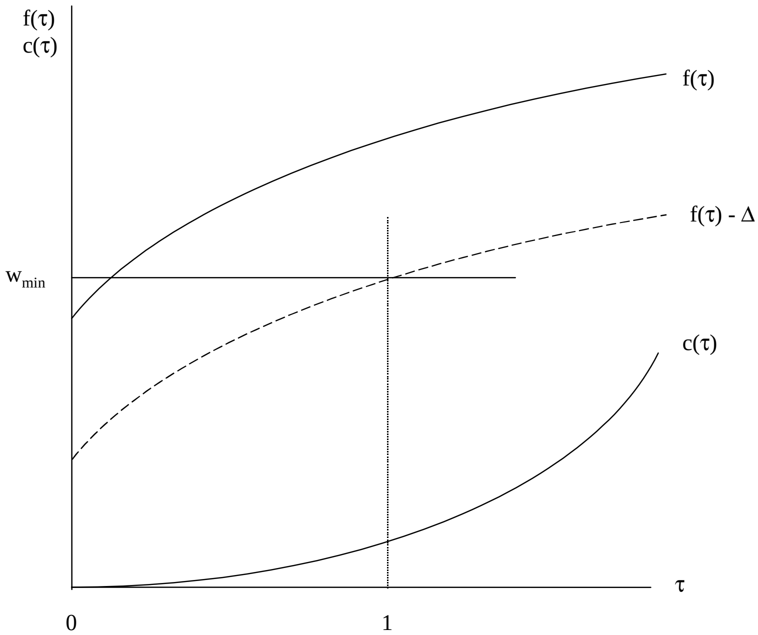

Figure 1: Training with a Minimum Wage and Employment Rents

more in the skills of their employees. Since this implication differs sharply from the prediction of the standard theory, empirical evidence on this point can shed light on whether non-competitive features affecting training decisions are important. Although existing evidence points to a negative effect of minimum wages on training, we argue that this evidence suffers from a number of problems. We therefore adopt a different approach and provide new empirical estimates that are quite different from those in the literature. We use the National Longitudinal Survey of Youth (NLSY) for the period 1987 to 1992. This period encompasses a number of state minimum wage increases as well as two federal increases in the minimum in 1990 and 1991. Our data therefore contain a large amount of within state variation in minimum wages. Furthermore, the NLSY is a panel of youths and oversamples those from disadvantaged backgrounds, so it contains a relatively high number of low wage workers directly affected by minimum wage increases.

Our empirical results show almost no evidence of a reduction in training in response to minimum wages. But, they also do not provide strong support for our alternative model. Overall, the evidence suggests that minimum wages appear to have little effect on training investments for low-wage workers. Although we cannot rule out modest positive or negative effects, our two standard error confidence bands exclude large negative effects of minimum wage increases on training—in fact, most of our most reliable estimates are positive.

An appealing way to explain the empirical findings is a hybrid model in which minimum wages increase training for some workers while reducing it for others. In this model, as in the standard theory, the wages of some workers are low because they are compensating their employers for investments in general skills. The minimum wage laws prevent this. This approach therefore suggests that minimum wages reduce the training of workers taking wage cuts to finance their training, while inducing further training for those who were constrained in their human capital investments. This hybrid model predicts that the impact of minimum wages should depend on the presence and size of labor market rents. We end the paper with some evidence pointing in this direction.

The rest of the paper is organized as follows. The following section discusses the previous empirical literature. Section 3 presents a simple theoretical setup where, contrary to the predictions of the standard theory, minimum wages increase training investments. In Section 4, we describe our data set, and discuss the empirical strategy to estimate the effects of minimum wages on training. Our results are presented in Section 5. Since the empirical evidence supports neither the standard Becker theory nor our model, in Section 6, we consider a hybrid model where minimum wages increase training for some workers, while reducing it for others. In Section 7, we provide some additional empirical evidence on this hybrid model. Section 8 concludes.

2 A Critique of the Previous Empirical Literature

There is a small empirical literature investigating the impact of minimum wages on training. Part of this literature focuses on whether minimum wage laws lead to slower observed wage growth in micro data. Both Leighton and Mincer (1981) and Hashimoto (1982) have found this to be the case and concluded that minimum wage laws lead to less training. But, since a minimum wage increases the wages of low paid workers, it can reduce wage growth without affecting training. Therefore, it is unsatisfactory to interpret the decline in age-earnings profiles as evidence of reduced investment in general training. Consistent with this view, Grossberg and Sicilian (1999) find no effect of minimum wages on training, but still find lower wage growth for minimum wage workers. Furthermore, Card and Krueger (1995) compared cross sectional wage profiles in California before and after the 1988 minimum wage increase with a number of comparison states. They also found flatter profiles in California after the minimum wage increase. However, they point out that the Californian profile also shifts up and does not cross the previous age-wage profile. This pattern contradicts the standard theory, but is consistent with the predictions of our model.

Given the difficulty of interpreting changes in the slope of wage profiles, we find it more compelling to look at the impact of minimum wages on training directly, but we are only aware of four previous studies doing this for the US. Leighton and Mincer (1981) use worker reported data on the receipt of training from the Panel Study of Income Dynamic (PSID) and the National Longitudinal Survey and find that workers in states with lower wages and therefore a more binding federal minimum wage receive significantly less training. Cross state comparisons may be confounded by the presence of other state effects, however. For example, industrial and occupational composition of employment varies substantially across states, and different industries and occupations have different skill requirements. These considerations suggest that across state comparisons are hard to interpret.

Schiller (1994) reports a similar finding using later data from the NLSY by comparing the training incidence of minimum wage workers with those earning higher wages. The evidence from this study is even harder to interpret because worker traits which lead to higher pay are typically also associated with more training. Grossberg and Sicilian (1999) use data from the Employment Opportunity Pilot Project (EOPP) and compare minimum wage workers both to workers earning slightly less and slightly more, ameliorating the problem of worker heterogeneity somewhat. They find insignificant negative effects on training for male minimum wage workers and insignificant positive effects for women. Leighton and Mincer only analyzed men, although women make up the majority of minimum wage workers.

Some of these problems are overcome in a more recent study by Neumark and Wascher (1998), who use Current Population Survey (CPS) supplements to compare the impact of minimum wages on training within states using comparisons of young workers in 1991 with older workers (who are less likely to be affected by the minimum wage) and with young workers in 1983. These comparisons assume that state differences in training levels are the same for younger and older workers and remain so over long time periods, which are stringent requirements. They also find negative effects of minimum wages on training, but these effects seem to be too large to be sensible.

To see why the effects implied Neumark and Washer's paper are implausibly large, note that their treatment group consists of all young workers. Not all of these workers are affected by the minimum wage, however. Let us assume, quite generously, that all workers earning less than 160 percent of the minimum are "affected" by the minimum wage. The 160 percent of the average federal minimum over the period they study is \$5.60, and 40 percent of workers aged 20-24 are paid below this wage in 1991. Neumark and Washer's estimates imply that formal training among workers aged 20-24 in California (a high minimum wage state) was 3.2 percentage points lower than in states which were subject to the lower federal minimum. This point estimate, then, implies that among affected workers, training will be lower by approximately 8 percentage points (i.e. 3.2 percentage points divided by 0.40). The average incidence of training among affected workers in low minimum wage states is 3.0 percent (much lower than among all workers aged 20-24 for whom the incidence is 10 percent). So this estimate implies that introducing California's minimum wage to low minimum wage states should have wiped out all training *two and a half times* among affected workers in these states! Clearly, an implausibly large effect.[1]

3 Minimum Wages and Training In Noncompetitive Labor Markets

In this section, we use a two-period model to analyze the impact of minimum wages on training. The main result of this analysis is that plausible deviations from perfectly competitive labor markets, which introduce firm-specific rents and prevent workers from financing their own training, change the conclusions of Becker's theory. Namely, we find that minimum wages can *increase* investments in general training.

3.1 Environment

The world lasts for two periods, 1 and 2. There is no discounting, and all agents are risk-neutral. There is a continuum of workers with mass 1, who supply labor inelastically. These workers differ by ability. More specifically, there is a distribution of abilities across workers denoted by $G(\eta)$ with support $\left[\underline{\eta}, \overline{\eta}\right]$. We introduce heterogeneity in abilities to capture the disemployment effects of the minimum wage. This feature will also be useful later in Section 6 when we discuss the possibility of workers paying for their own training.

We view period 1 as the early career of workers. A worker with ability η_i produces $\theta\eta_i$ in the first period, where $\theta < 1$. In the second period, he produces η_i. In addition, during the first period, he can be trained. To simplify the discussion we assume that training is indivisible, so only $\tau = 0$ (no training) and $\tau = 1$ (training) are possible. A worker who is trained produces $\eta_i + \phi$ in the second period where $\phi > 0$. Additive returns to training simplify the expressions, without affecting the results; moreover, they highlight that firm-sponsored training does not arise because of a complementarity between training

[1] In response to this criticism, Neumark and Washer now argue that workers earning above 160 percent of the minimum are also affected by the minimum wage.

and ability (a possibility demonstrated in Acemoglu and Pischke, 1999b).

We assume that training is general, so this increase in productivity applies equally in all firms. The cost of training, which is independent of ability, is incurred in terms of lower output in the first period and is equal to $c > 0$. In order to draw a stark contrast to the Becker model, we assume that all training investments have to be financed by the firm. This could be because training is non-contractible, so the firm can renege on its training promise even if the worker takes a wage cut to finance his training (see Acemoglu and Pischke, 1999a, for a discussion).

We assume:

Assumption 1: $\phi > c$,

which implies that training is productive.

There is free-entry at zero cost and all firms have access to the same technology. We also assume that the firm obtains an additional revenue δ, if the worker was employed in the first period with this firm. Therefore, δ is a firm-specific productivity increase, and it is the only deviation from competitive labor markets we introduce.

The presence of the term δ creates a match-specific surplus to be divided between the firm and the worker. We assume Bertrand competition among firms (or equivalently, firms make take it or leave it offers to workers). Since the second-best opportunity of workers does not include δ, initial employers capture the whole amount δ, only paying the "market wage", even though workers' productivity is higher than this. As will become apparent shortly, the presence of rents from the employment relationship is crucial, but the results would be unaffected if the worker captures a fraction $\beta < 1$ of this surplus. Observe also that this mobility cost implies that all workers will stay with their initial employer.[2]

3.2 Equilibrium Without Minimum Wages

Equilibrium can now be characterized by backward induction. In the second period, each firm is willing to pay up to η for an untrained worker of ability η employed by another firm, and is willing to pay $\eta + \phi$ for a trained worker. Bertrand competition then ensures that

$$w_2(\eta_i, NT) \quad = \quad \eta_i, \tag{1}$$

[2] Alternatively, we could have that a worker who changes jobs in the second period incurs a mobility cost δ, because he has to acquire some costly skills, specific to the new employer, before becoming productive. All our results continue to hold with this alternative interpretation. Also, in practice, workers earning close to the minimum wage have high mobility. Our results continue to hold if a fraction $s < 1$ of workers change jobs.

$$w_2(\eta_i, T) = \eta_i + \phi.$$

Using the fact that in equilibrium all workers stay with their first period employer,[3] the profits of a representative firm from a worker of ability η_i, as a function of its training strategy, NT or T, can be written as:

$$\begin{aligned} \Pi(\eta_i, NT) &= (\theta\eta_i - w_1(\eta_i)) + (\eta_i + \delta - w_2(\eta_i, NT)) \\ \Pi(\eta_i, T) &= (\theta\eta_i - c - w_1(\eta_i)) + (\eta_i + \delta + \phi - w_2(\eta_i, T)), \end{aligned} \quad (2)$$

where $w_1(\eta_i)$ is the first period wage of a worker with ability η_i. The first bracket in each expression therefore gives first period profits, while the second is profits in period 2. Substituting (1) into (2), we immediately see that $\Pi(\eta, T) - \Pi(\eta, NT) = -c$, irrespective of the value of η. That is, if it trains its employees, the firm simply loses the training cost. The reason is simple: a trained worker receives ϕ more in the second period, and this is exactly the increase in his contribution to the firm's output. So the worker is the full residual claimant of the increase in productivity due to training, and the second period profit of the firm is equal to δ, independent of whether the worker is trained. Since the training cost c has to be paid by the firm, it is not optimal for any firm to invest in its employee's skills. Although there are firm-specific rents, because they do not interact with training—in particular, they do not induce compression in the wage structure (see Acemoglu and Pischke, 1999b)—, firms have no incentive to train, even though training is socially desirable.

To complete the characterization of the equilibrium, we have to determine first period wages. Firms have to make zero profits from all workers, and there is no training, so $\Pi(\eta_i, NT) = 0$ for all η_i. This gives:

$$w_1(\eta_i) = \theta\eta_i + \delta \quad (3)$$

A noteworthy feature is that compensation can be front-loaded or back-loaded. Front-loading arises because firms anticipate δ, the rent they will receive in the second period, and are willing to bid higher than the worker's current productivity.[4]

We can summarize this analysis (proof in the text):

[3] Even though workers do not receive any of the firm-specific rents, there is no labor mobility in equilibrium. Suppose this were not so, then the firm could offer ε more than the market wage and convince the worker to stay, so mobility cannot be part of an equilibrium.

[4] In the case where the firm-specific rent δ is shared between the firm and the worker, there will be a further force towards an upward sloping wage profile. For example, if the worker receives a fraction β of this rent, the second period wage of an untrained worker will be $\eta_i + \beta\delta$, while his first period wage will be $\theta\eta_i + (1 - \beta)\delta$. As β increases, the age-earnings profile becomes steeper.

Proposition 1 There is a unique equilibrium in which there is no training, all workers are employed, and receive the first period wage given by (3) and the second period wage $w_2(NT)$ given by (1).

So despite Assumption 1, which ensures that training is socially beneficial, there is no training in equilibrium. This is in line with Becker's standard theory; firms are unwilling to invest in the general skill of their employees as they do not receive any of the benefits of training, and here workers are assumed unable to "buy" training. However, there is full employment in this decentralized economy, in particular, the level of employment is equal to the labor force, 1.

3.3 The Impact of Minimum Wages on Training

Now consider the imposition of a minimum wage $w_M > \underline{\eta}$ that is binding for some (untrained) workers in the second period. We start by writing wages in the presence of minimum wage laws. With a similar reasoning to above, (1) changes to:

$$\begin{aligned} w_2(\eta_i, NT) &= \max\{w_M, \eta_i\}, \\ w_2(\eta_i T) &= \max\{w_M, \eta_i + \phi\}. \end{aligned} \tag{4}$$

These expressions feature the "max" operator because the minimum wage may be less than or greater than worker productivity. Profits with and without training are still:

$$\begin{aligned} \Pi(\eta_i, NT) &= (\theta\eta_i - w_1(\eta_i)) + (\eta_i + \delta - w_2(\eta_i, NT)) \\ \Pi(\eta_i, T) &= (\theta\eta_i - c - w_1(\eta_i)) + (\eta_i + \delta + \phi - w_2(\eta_i, T)) \end{aligned}$$

where now $w_1(\eta_i) \geq w_M$.

First, consider a worker for whom

$$\eta_i \leq w_M - \phi. \tag{5}$$

In this case, the second period wage is equal to the minimum even if this worker is trained. Then,

$$\Pi(\eta_i, T) - \Pi_j(\eta_i, NT) = \phi - c. \tag{6}$$

Assumption 1 implies that (6) is strictly positive for all η_i. So in stark contrast to the economy without minimum wages, firms now prefer to train all their employees whose ability is low enough to satisfy (5). The firm has to pay the minimum wage irrespective of whether the worker is skilled or not, so the full return to training is captured by the employer—*the firm is now the full residual claimant.*

Next, consider a worker for whom (5) does not hold, so that the second period wage for this worker, if trained, exceeds the minimum. So he will be paid $w_2(\eta_i, T) = \eta_i + \phi$ if trained. Then,

$$\Pi_j(\eta_i, T) - \Pi_j(\eta_i, NT) = w_M - \eta_i - c, \tag{7}$$

which can be positive or negative. If it is positive, i.e. if

$$\eta_i \leq w_M - c \tag{8}$$

the firm makes higher profits from trained—rather than untrained—workers.

Because condition (8) is more restrictive than (5) (see Assumption 1), we will have $\Pi_j(\eta_i, T) \geq \Pi_j(\eta_i, NT)$ if and only if (8) is satisfied. Nevertheless, condition (8) is not sufficient for firm-sponsored training. Firms also need to make nonnegative profits; i.e.,

$$\Pi_j(\eta_i, T) \geq 0. \tag{9}$$

The minimum that a worker can be paid in the first period is w_M, although the first period wage may be higher than this. Hence, (9) requires

$$(1+\theta)\,\eta_i + \delta + \phi - c - w_M - \max\{\eta_i + \phi, w_M\} \geq 0. \tag{10}$$

All workers for whom (8) and (10) are satisfied will be trained.

We are now in a position to determine the equilibrium level of employment and training. First, consider workers with ability $\eta_i > w_M - c$. As indicated above, these workers will not obtain training, and will be employed as long as firms make zero profits. This requires

$$(1+\theta)\,\eta_i + \delta - w_M - \max\{\eta_i, w_M\} \geq 0. \tag{11}$$

Intuitively, in the second period, a worker with ability $\eta_i > w_M$ will receive the wage $w_2(\eta_i, NT) = \eta_i$. Therefore, the firm will employ this worker only if the loss that it makes in the first period, $\theta\eta_i - w_M$, is less than the profit it expects to make in the second period, δ. The calculation is somewhat different for a worker with $w_M - c < \eta_i < w_M$, since the minimum wage will be binding for this worker in the second period, and so the second period profit is $\eta_i + \delta - w_M$. Combining these conditions, the number of workers who will be employed, but not receive any training, is

$$N(w_M) = 1 - G\left(\max\left\{w_M - c; \frac{w_M - \delta}{\theta}, \frac{2w_M - \delta}{1+\theta}\right\}\right), \tag{12}$$

where the "max" operator takes care of various cutoffs involved.

If, in contrast, $\eta_i \leq w_M - c$, then it is more profitable for a firm to train the worker rather than employ him as an untrained employee. Such a worker will be employed—and trained— as long as (10) holds. Therefore, the number of workers employed and trained is[5]

$$T(w_M) = \max\left\{\left[G(w_M - c) - G\left(\frac{w_M - \delta + c}{\theta}\right)\right]; 0\right\} \tag{13}$$

Here, the max operator takes care of the fact that the expression in the square bracket could be negative, in which case there would be no firm-sponsored training because the firm would not be able to make zero profits by training its employees. In fact, in this case, a necessary and sufficient condition for firm-sponsored training and for the introduction of a minimum wage to increase training is

$$\delta > \delta^* \equiv (1 - \theta) w_M + (1 + \theta) c. \tag{14}$$

This highlights the importance of firm-specific rents. If $\delta = 0$, then (14) cannot be satisfied and the firm will therefore not find it profitable to train. Therefore, the deviation from perfectly competitive labor markets is essential for minimum wages to increase training. It is only with sufficiently large rents—as implied by equation (14)— that there will also be any firm-sponsored training.

We can now state the main result of this section (proof in the text):

Proposition 2 Suppose that a minimum wage satisfying $w_M > \underline{\eta}$ is imposed. Then the level of employment is $N(w_M) + T(w_M)$ and $T(w_M)$ workers receive firm-sponsored trained where $N(w_M)$ is given by (12) and $T(w_M)$ is given by (13). $T(w_M)$ is strictly positive whenever (14) holds.

As in the standard neoclassical model, minimum wages reduce employment as now $N + T < 1$ (whenever the lower support of the ability distribution, $\underline{\eta}$, is less than $(w_M - \delta + c)/\theta$). But moderate minimum wages also induce firms to offer training to some of the affected workers. To see this, consider the introduction of a minimum wage w_M satisfying condition (14) above in an economy without minimum wage. Before the imposition of the minimum wage, there is no firm-sponsored training, and since workers are unable to "buy" training from their employers, there is no training at all. After the introduction of the minimum wage, training increases to $T(w_M)$. Therefore, minimum wages can increase training in this economy.

[5]The full expression for the number of workers who receive training is $\max\left\{\left[G(w_M - c) - G\left(\max\left\langle\frac{w_M-\delta+c}{\theta}, \frac{2w_M-\delta-\phi+c}{1+\theta}\right\rangle\right)\right], 0\right\}$. However, $w_M - c \geq \frac{w_M-\delta+c}{\theta}$ implies that $\frac{w_M-\delta+c}{\theta} \geq \frac{2w_M-\delta-\phi+c}{1+\theta}$ (since $\phi > c$ by Assumption 1).

Intuitively, the firm has to pay minimum wages even for unskilled workers and a binding minimum wage reduces the rents the firm receives from the employment relationship. Training raises the worker's productivity, and therefore restores some of these rents. Firm-specific rents (labor market imperfections) are crucial for this result. As noted above, when $\delta = 0$, there will be no training, since in this case condition (14) can never be satisfied.

Figure 1 in the introduction gives the basic intuition. The minimum wage determines the wage both at $\tau = 0$ and $\tau = 1$, so all productivity increases from training accrue to the employer, as in the case when $\eta_i \leq w_M - \phi$. The most interesting case might be the one where $w_M - \phi < \eta_i \leq w_M - c$. If the minimum wage is low enough, it may induce the firm to sponsor training but the worker receives some of the proceeds from the training, because the productivity of the trained worker exceeds the minimum wage.[6]

Notice that the results continue to apply if w_M is a wage above the legal minimum that the firm has to pay to the worker, due to other imperfections such as bargaining. Therefore, if an increase in the minimum creates spillover effects to wages above the minimum, our analysis predicts that firms may also be induced to train the workers affected by these spillovers. In practice, minimum wages appear to create spillover effects (e.g. DiNardo, Fortin and Lemieux, 1996; Lee, 1999), so we expect them to also influence the training of low wage workers earning above the minimum.

Since the effect of minimum wages on training analyzed here differs sharply from the prediction of the standard Becker theory, empirical evidence on minimum wages and training can shed light on whether non-competitive features and restrictions on workers' ability to finance their own training are important. In the next part of the paper, we investigate whether minimum wages increase or reduce training investments for low-wage workers.

4 Empirical Strategy and Data

The federal minimum wage was unchanged between 1981 and 1990, but various states imposed their own minima above the federal level during the late 1980s. While minimum wages were rather uniform across states before 1987 and after 1991, there was substantial dispersion between these dates. We will exploit this variation. Table 1 displays the statutory minimum wages in the US states over this period.

We use two complementary approaches to identifying the impact of minimum wages

[6] This is special to the case where the firm has to choose a discrete level of training. If the firm could choose training continuously, it would never train the worker beyond the point where it has to pay above the minimum wage in the second period (see Figure 1).

on training. The first one, which we find most compelling, looks at the training of workers who are directly affected by an increase in the state or federal minimum wage. The second approach looks at the relation between training and a measure of how binding the minimum wage is across regions. This latter approach is most closely related to the empirical work in the previous literature, and therefore serves as a check on the robustness of our results.

4.1 Empirical Specification

The most direct way to estimate the impact of the minimum wage on training outcomes is to look at workers who are actually affected by changes in the minimum wage. In order to illustrate the approach, consider the following regression equation

$$\tau_{irt} = \alpha mw_{irt} + \beta' x_{it} + d_t + v_r + \mu_i + \varepsilon_{irt} \qquad (15)$$

where τ_{rit} is a measure of training for individual i in region r at time t, d_t, v_r and μ_i are time, region, and individual effects, and x_{it} are other individual characteristics like education, age, gender, and information about the job an individual holds. mw_{irt} is a measure of whether the minimum wage binds for individual i in region r at time t. One measure for mw_{irt} would be whether the actual wage of an individual is close to the minimum wage for the region and time period. This has been the approach of Schiller (1994) and Grossberg and Sicilian (1999) using cross-sectional variation. But this strategy has the problem that other, possibly unobserved, person characteristics which are correlated with the individual's wage will also tend to be correlated with training receipt.

We therefore difference eq. (15) to obtain

$$\Delta\tau_{irt} = \alpha \Delta mw_{irt} + \beta' \Delta x_{it} + \Delta d_t + \Delta\varepsilon_{irt}. \qquad (16)$$

Changes in training should now be related to changes in whether a worker is affected by the minimum wage. As a measure of Δmw_{irt} we use a dummy variable which indicates that the minimum wage increased from one year to the next, and the worker earned below the new minimum wage in the base year. This measure captures workers who are directly affected by a change in the minimum wage, similar to Card's (1992) analysis of employment effects. The measure relies purely on the variation of the minimum wage and base period wages, but not changes in individual wages, which may be correlated with the timing of the training received by a worker. Our analysis will focus on individuals who do not move between states because moving would also confound Δmw_{irt} with behavioral effects. Therefore, the region effect does not appear in eq. (16).

Although we feel that this analysis of eq. (16) exploits variation of the minimum wage most directly, hence gives the most reliable results, previous studies have relied on eq. (15) to study the impact of the level of the minimum wage on training. To make our results more comparable to these previous studies, we also undertake analysis of the levels equation. Rather than indicating affected workers directly, the variable mw_{irt} will in this case measure how binding the minimum wage is in a particular state or region. We start by using simply the actual minimum wage in region r at time t, w_{rt}^m. However, the same federal minimum affects more workers in low wage regions than in high wage regions, and this source of variation would not be captured by w_{rt}^m. Therefore, we prefer specifications in which a given level of the minimum wage is allowed to have different effects in different regions depending on their average wages. For this purpose, we consider relative minimum wage measures $w_{rt}^m/\overline{w}_r$, where $\overline{w}_r$ is a measure of the location of the wage distribution in region r (the median wage of older workers) over the whole sample period. This measure $\overline{w}_r$ should not be affected by the minimum wage itself, and therefore just parameterizes the wage distribution, and $w_{rt}^m/\overline{w}_r$ measures how high the minimum wage is relative to the region's wage distribution. Notice that we are not using $\overline{w}_{rt}$ which would move with the business cycle at the regional level, and might create a spurious correlation if training incidence were also cyclical.

An obvious choice for the regions are states, since minimum wages also vary at the state level. However, the wage distribution also varies within states, so that the relative minimum wage measure can be defined for smaller regions. Apart from states, we use two other measures. One partitions states into SMSAs and non-SMSA parts as our region definition. This lets us exploit the often substantial variation in wage levels between large SMSAs (like New York City) and rural areas in a state (like upstate New York) in the analysis. In total, we distinguish 136 regions. Details on the construction of these are available in Appendix 1. When we use states or these smaller regions, our measure for $\overline{w}_r$ is the average of the median wages of male workers age 35-54 in each year between 1987 and 1992. The second "region" definition distinguishes between the male and female wage distribution within states, i.e. $\overline{w}_r$ uses the average of the median wages of male workers age 35-54 in the state if the respondent is male, and of the median wages of female workers age 35-54 if the respondent is female. This measure exploits the fact that women should be more affected by a given minimum wage than men because women tend to earn less than men.

In the right hand panel of Table 1 we show the relative minimum wages using the male medians by state and year over the sample period. It is apparent that there is substantially more dispersion in this measure (which is still coarser than the other regional measures we use below) than in the minimum wage itself. For example, the federal minimum wage increases between 1989 and 1991 raised the relative minimum wage by only 0.055 in New Jersey but by 0.085 in Arkansas, both states without a state minimum wage above the federal level in 1989. This illustrates how the scaling of the minimum wage measure leverages the increase in the federal minimum wage across states with different wage distributions, even once we control for state effects.

There are a number of practical problems in implementing the estimation of equations (15) and (16). For example, training is not easily defined at a point in time. Because most training spells only last for a short period of time, we will define τ_{rit} as referring to all incidents of training within a single year. Thus, τ_{rit} will be 1 if the individual received any training during the year, and 0 otherwise. In the differenced equation (16), the dependent variable takes on the values -1, 0, and 1. We will estimate the models as linear probability models, facilitating differencing and the inclusion of fixed effects in the levels version.

Looking at a time period as long as a year has its drawbacks. The minimum wage may change within a year. In order to minimize the impact of this, we look at periods of 12 months starting in April and ending in March. Both federal minimum wage increases in 1990 and 1991 went into effect April 1. Some state minimum wage increases also took effect on April 1, but many did not. Whenever the minimum wage changed during the year, we use an employment weighted average of the minimum wage in effect during the year.

The covariance matrix of the error term in eq. (16) will have a first order moving average structure at the individual level. We therefore estimate standard errors with the Huber estimator, which is robust to arbitrary cluster effects at the individual level. This covariance estimator is consistent in this case but not efficient. In (15), the key regressor, the minimum wage variable, only varies at the region and year level while we use individual level data. Conventional standard errors may therefore overstate the precision of the estimates (Moulton, 1986). Suppose the error term has the form $\varepsilon_{rit} = \lambda_i + \upsilon_{rt} + \xi_{rit}$, i.e. the error is composed of an individual level component λ_i, a region*time component υ_{rt}, and a component ξ_{rit}, which is uncorrelated across individuals, regions, and time periods. Notice that the error ε_{rit} will be heteroskedastic, since we are estimating a linear probability model. There is no straightforward way to calculate consistent standard errors for this error structure. We extend the standard Huber estimator to allow for both an individual level component and a region*time component in the error term. This estimator seems to perform well in practice in samples of our size. We report formulas and the results of some small Monte Carlo experiments in Appendix 2.

4.2 The Data

Our data on training come from the National Longitudinal Survey of Youth (NLSY). The NLSY is a panel of youths aged 14 to 21 in 1979. This dataset is particularly suitable for this project because it samples young workers, and it oversamples those from disadvantaged backgrounds, who are more likely to work in jobs at or slightly above the minimum wage (see Card and Krueger, 1995). We will follow the cohorts interviewed in the NLSY from 1987 to 1992, years of significant changes in state and federal minimum wages. During the 1988 to 1992 surveys, the NLSY asked a consistent set of questions about on-the-job training during the previous year. The information about the training includes length and type of the program, site, and whether the explicit costs of the training were paid for by the employer or someone else. The first set of training questions in 1988 refer to a longer time frame than the questions in subsequent years, because no similar data were collected in 1987. In 1993, the module on training in the survey was expanded substantially and the survey switched from paper and pencil to computer assisted interviewing. We use some data from the 1993 survey to complement information on training during the April 1992 to March 1993 year. There were some other minor additions to the training questions before 1993 as well.

The sequence of questions on training begins with a lead-in stating

> "I would now like to ask you about other types of schooling and training you may have had, excluding regular schooling we have already talked about. Some sources of occupational training programs include government training programs, business schools, apprenticeship programs, vocational or technical institutes, correspondence courses, company or military training, seminars, and adult education courses."

This suggests that respondents will mostly report relatively formal training programs and neglect other sources of informal on-the-job training, a suspicion which has been substantiated by Loewenstein and Spletzer (1999b) using the more detailed training data in the NLSY starting in 1993. While this may be a drawback, this limitation of the data is pervasive in this literature.

We are only interested in training programs which take place in firms, or are sponsored directly by the employer, not in courses taken by individuals outside work on their own initiative or government sponsored training programs. We therefore classify the following forms of training as "employer related training": any training for which the respondent gives as venue an apprenticeship program; formal company training; or seminars or training programs at work run by someone other than the employer; or if the respondent classifies the training as on-the-job training or work experience; or if the employer paid for the training (even though we do not interpret the answer to this question as the employer necessarily bearing the investment costs). We do not classify a training programs as employer related if the training was partly paid for by a government program.[7] For each training program we record the start date as reported on the 1988 to 1993 surveys. If this date falls within an April to March period between 1987 and 1993, we assign the training to this particular year. We treat observations with a missing start date as missing.[8]

We do not use any information on the job or employer in the estimation directly. However, training often takes place when individuals start new jobs, and minimum wage increases may affect turnover and hiring. Hence, it seems important to control for turnover in some fashion. We include a dummy variable for whether a respondent started any new job within a particular year from information in the work history module of the data.

We limit our sample to workers who have 12 years of education or less, a group most likely to be affected by the minimum wage. We use the oversamples of blacks, Hispanics and poor whites in the analysis, but we drop the military subsample. The results are weighted by the NLSY sampling weights throughout. Individuals living in the District of Columbia are excluded, because DC had a plethora of different minimum wage rules, making it hard to define a sensible overall measure of the minimum wage. Our basic sample includes all workers who report a wage at the interview for the current year and for the past year,[9] and who were employed for at least one month during the year according to the information in the work history module. We also restrict the sample to those with valid wage information. For the analysis using eq. (16) we also include individuals who move between states.

In addition, we use the 1987 to 1993 outgoing rotation group files of the CPS. We calculate the median wage for workers age 35 to 54 from this data source to construct the relative minimum wage measure. The CPS outgoing rotation groups are large enough to do this even for smaller areas within states. For example, each region-year cell has at least 27 observations on older male workers, but few cells are that small, and the median number of observations is 274.

[7]The most common government program, JTPA, involves wage subsidies up to 50 percent if trainees are placed with private sector employers. Hence, the incidence of training under this program is unlikely to be affected by minimum wage legislation.

[8]The NLSY also provides information on the length of training. We do not use the duration of training directly, because of the frequency of missing values but we checked that our results are robust to excluding very short training spells.

[9]The NLSY refers to this wage measure as the CPS wage because of the CPS style question. The CPS wage may refer to a prior job if the respondent is not working at the time of the interview.

5 Empirical Results

Table 2 reports means of some demographic indicators for the three samples from the NLSY. All samples include respondents with 12 years of schooling or less working in at least one month during the year. Since young adults still obtain additional schooling in this age range, the samples changes over time. The non-mover sample differs little from the unrestricted sample. About 20 to 25 percent of each sample report having started a new job during the calendar year. While this includes secondary and temporary jobs, the number matches relatively closely the fraction of low education workers in the same age group who report tenure of 12 month or less on their current job in the January 1991 CPS. This relatively high rate of job starts reflects the large turnover among young, low skilled workers.

The average nominal hourly wage in the basic sample rises from about $7.50 in 1987 to almost $10 in 1992. While a number of sample members earn the minimum wage or less, as shown in the last row, the majority of respondents earn far above the minimum wage. These samples therefore include many workers whose wage is not directly affected by the minimum. These higher wage workers effectively form our control group in the differenced analysis. When we look at the impact of the level of the minimum wage, on the other hand, most of the sample members are not directly affected by the minimum wage, a problem which has also affected previous studies. In order to address this issue we also use a low wage sample, defined as workers employed at a wage which is 150 percent of the federal minimum or less in the *previous* year. The last two columns give the basic descriptive statistics for this sample, which is about a third the size of the basic sample. These low wage workers include more women and blacks. The number of high school dropouts is only slightly higher. Average wages in this sample are much lower and do not grow substantially over the sample period, due to the sample selection. We think of this group as much more likely to be actually subject to the minimum wage or spillovers resulting from it. Since a larger fraction of these workers are directly affected by the minimum wage, the standard theory predicts that we should find larger negative effects in this sample.

Table 3 reports sample means for some of the key variables in our regressions. The first row of the table reports the incidence of training, which is around 10 percent. The only exception is 1987, where the measured incidence is much lower. This is presumably due to the fact that the training questions were first asked on the 1988 survey so that the questions referring to 1987 had a longer recall period than for later years. There is also a small drop in 1991, possibly due to the recession. If we exclude very short training spells of 1 day or less, the incidence drops to about 7 to 8 percent.

Our measure of the minimum wage in a region is the higher of the state and the federal minimum wage. In Table 3, we report the average minimum wage in a year across all regions. These averages are weighted by the residence of our sample population. In 1987, the average minimum wage was 3.36, when only Alaska, and the New England states had state minima above the federal minimum wage of 3.35. It rises to 3.44 in 1988, mainly reflecting the increase in the California state minimum, and increases further to 3.51 in 1989, due to new minimum wage laws in other states, among them Oregon, Washington, and Pennsylvania. The standard deviation across respondents reaches a high of 29 cents in this year, indicating a substantial amount of variation in state minimum wage levels. This variation drops substantially in 1990 and 1991, when the two federal increases raise the minima in those states which had not taken action before. The averages of 3.87 and 4.26 are now only slightly above the federal minimum wages of 3.80 and 4.25. In 1992, New Jersey raises its minimum wage to 5.05, increasing the spread again.

For our analysis of equation (16), we use four different measures for workers affected by the minimum wage. The first one includes all workers who earned less than the new minimum wage in a year prior to a minimum wage increase. This includes workers who report a wage below the initial minimum. The top panel of Table 3 shows that about 1.4 percent of the sample were affected by minimum wage increases in 1988, mostly due to the increase in the California minimum wage. The state increases in 1989 affect slightly more workers, but a large fraction of workers (7 and 9 percent, respectively) is only affected by the federal increases in 1990 and 1991. The second measure excludes workers below the minimum wage in the base year. It is not completely clear whether these workers should be considered affected or not. Minimum wage coverage was fairly universal during the time period we consider, so that these reports presumably reflect mostly measurement error.[10] Excluding workers with wages below the base period minimum cuts the fraction of affected workers about in half in each year.

An increase in the minimum may also affect the wages of higher wage workers via spillover effects, and our model then predicts that their training should be affected. Lee (1999), for example, finds large spillover effects from the minimum wage changes during our sample period, and we report similar results below in Table 5. In order to investigate whether spillovers affect our results, we also report specifications that include workers who initially earned above the new minimum in the affected group. We choose alternatively 150 and 130 percent of the new minimum wage. This yields about two to four times as many affected workers as our original measure. The bottom panel of Table 3 reports the relative minimum wage measures, i.e. the minimum wage divided by the

[10] It is also possible that respondents receiving tips do not include these in the wage measure.

state, regional, or state/gender median for older workers. The changes in these measures over time reflect again the increases in various state minimum wages and the federal increases in 1990 and 1991.

5.1 Results Using Affected Workers

Regression results for the first differenced version of the model are displayed in Table 4. Apart from the minimum wage, the regressions include a constant (capturing any linear effects of age), a full set of time dummies and variables for the change in high school graduation status and for whether the worker took a new job. High school graduates are between 4 and 9 percentage points more likely to receive training. This effect is imprecisely estimated because there are few workers who acquire a high school degree or equivalent during the sample period. Workers starting a new job are 3 to 4 percentage points more likely to receive training and this effect is estimated quite precisely. These estimates are sensible and demonstrate that the training variable is able to pick up the expected variation in the data.

The coefficients on the variable for affected workers are directly interpretable as the effect of raising the minimum wage on the incidence of training. The table displays four different specifications corresponding to four different definitions of the "affected" variable and employs three different samples, and hence implicitly three different control groups. In column (1), we define all workers whose wage in the previous year is below the current minimum as affected. All other workers in the sample serve as the control group. The point estimate indicates that being affected by a minimum wage increase raises the probability of receiving training by 1 percentage point. The effect is not statistically significant, however.

The control group in column (1) comprises both higher workers in affected and unaffected states as well as low wage workers in states and periods which had no minimum changes. Even though our sample was chosen to represent a relatively homogeneous set of workers, there is a good deal of heterogeneity. It is therefore sensible to limit the control group to different subgroups. Columns (2) uses the same definition of the affected variable but limits the control group to higher wage workers in the states which were affected by minimum wage changes. The point estimate of the impact of minimum wages on training is almost the same. Column (3) performs the opposite exercise and limits the control group to other low workers rather than higher wage workers. We define this group as those workers whose wage in the previous period is less than 150 percent of the current minimum wage. Hence, it also includes workers in affected states and periods with wages slightly above those of affected workers. The effect of the minimum wage on training is again very similar but slightly lower now. Our conclusion is that the exact choice of control group plays very little role in the estimates.

The specifications in columns (1) to (3) counts workers who were reporting wages below the previous minimum in the base year as affected. It is possible that some of the wages below the initial minimum were due to measurement error, these workers really earn much higher wages and were not truly affected by the minimum. Therefore we exclude these workers from the affected group in the columns (4) to (6). The results are not very different. Changing the control group again makes little difference.

Neither of the previous specifications allows spillovers of the minimum wage on workers with slightly higher wages. In fact, these workers make up the bulk of the control group in columns (3) and (6). If we redefine instead the affected group as workers whose previous wage was within 150 or 130 percent of the new minimum wage in columns (7) and (8), we find basically zero effects of minimum wages on training. It is quite possible that the effects drop slightly because these specifications count too many workers as "affected".[11]

Overall, these results provide neither strong support for the standard theory nor for our model. Average training incidence for workers affected by a minimum wage increase is 5.2 percent for the measure of affected workers used in column (1). The 95 percent confidence interval is consistent with declines in training as large as 1.8 percentage points or increases up to 2.8 percentage points. This means we can reject that the minimum wage eliminates more than a third of the training in this group. Similar conclusions are obtained for the other specifications.

5.2 Results Using Minimum Wage Changes

While the results in Table 4 indicate no adverse effects of minimum wages on training, our methodology differs somewhat from the previous literature. In this section we present alternative results based on the regression equation (15). The sample we use in this section also includes workers who move from state to state between interviews, but this sample differs little from the non-mover sample.

Recall that this sample contains many workers not directly affected by the minimum wage, so it is natural to worry about whether minimum wages will have a significant effect on the earnings of workers in this sample, since in the absence of such a finding,

[11] The training question that we use includes very short training programs, and it is important to ensure that our results are not sensitive to eliminating these short spells. When we repeat these regressions excluding training spells lasting a single day or less, we obtain slightly smaller coefficients for affected workers, e.g. 0.004 for the measure in column (4) and -0.013 for the measure in column (8), while the standard errors are basically unchanged.

we may expect no effect on training incidence either. Before turning to the impact of minimum wages on training, we therefore look at the effect of the minimum wage on actual wages. Table 5 displays quantile regression estimates of the real wage of workers in this sample on the real value of the minimum wage and a full set of year and state dummies. The first feature of the results is that low quantiles of the wage distribution are affected by changes in the minimum wage, which shows that minimum wages do have an effect on the wages of low paid workers. So according to the standard theory, there should be a negative effect on training.

A one dollar increase in the minimum wage raises wages of the 10^{th} percentile worker in the NLSY by 37 cents, which may seem small. The second column in the table replicates these results with a comparable sample using the CPS outgoing rotation groups, with similar results, showing that these results are not particular to the NLSY. There are various reasons to expect why the coefficients even at the low quantiles should be less than one. There is certainly much measurement error in the wage reports of workers, biasing these coefficients down. Furthermore, Table 2 revealed that even workers at the 10^{th} percentile will typically earn above the old minimum wage already, and therefore they may not receive the full increase when the minimum wage goes up. Nevertheless, the results in Table 5 show that workers as high as the 30^{th} percentile of the wage distribution may be affected by minimum wage changes, and therefore their training may also be affected.

Table 5 also shows that there are many workers in this sample who are not affected by minimum wages. This means that our estimates of training effects will tend to be biased towards zero. This motivates our strategy to compare our basic results to those in the lower wage sample using only workers earning less than 150 percent of the federal minimum wage in the previous year, corresponding roughly to the workers up to the 30^{th} percentile of the original sample. This sample more closely approximates the workers actually affected by the minimum wage or by spillovers resulting from the minimum. If the results in the larger sample are biased towards zero, then the more restrictive sample should lead to more extreme estimates.

Table 6 presents our regression results for the incidence of training on the minimum wage and relative minimum wage measures. We present four sets of regressions with and without time and region fixed effects, as well as one specification which includes individual instead of region fixed effects.[12] Other covariates in the regressions are dummies for blacks, Hispanics, females, high school graduates, and whether the individual

[12] If training this year makes it less likely that the individual will be trained in the future, the individual effects model may be problematic. However, in our sample, training is positively correlated over time.

started a new job during the year, and a linear variable for age.[13] The coefficients on the demographic covariates are again sensible. Blacks receive about 3 percentage points less training and Hispanics 1.5 percentage points less. Contrary to the typical finding in the literature, the effect for women is very small and insignificant in this low wage group. High school dropouts receive about 5 percentage points less training. Workers starting new jobs are slightly more likely to receive more training but the effect is only sizeable at 2 percentage points and significant once individual effects are controlled for.

If no region effects are included, as in columns (1) and (2), the effect of the minimum wage measure is positive and significant. This reflects that states with higher minimum wages tend to have more training. Because these states tend to have higher wages in general, this may simply mean that high and low wage states differ, for example in terms of their industrial and occupational structure. In fact, the positive result in column (2) vanishes when we look at the relative minimum wage measures below or when we control for state and time effects in column (4). This latter specification, which we prefer, suggests that higher minimum wages have a small negative effect on training. The federal minimum wage increased by 81 cents in real terms from 1989 to 1991. This increase led to about 0.6 percentage points less of training. We find similar results including individual fixed effects instead of state effects in column (5).[14]

The estimates are more negative if we look at the low wage sample in row 2. Unfortunately, these results are very imprecisely estimated and are consistent with both substantial negative and positive effects. One reason for this is that the minimum wage variable we are using in the first two rows does not exploit a relevant part of the information, which is that a given level of the minimum wage will have very different effects depending on how high wages are in a region. To exploit this information, we next turn to the relative minimum wage measures.

Rows 3 and 4 of the table report results where we divide the minimum wage by the median wage of older males in the state. The specification which exploits primarily the cross region variation in the minimum wage measure in column (2) now results in a moderate negative coefficient. In row 5 below, where we use more detailed regions and obtain more precise results, this coefficient is even significant. This specification compares most closely to the approach of Leighton and Mincer (1981). But the results in columns (4) or (5), which are much closer to zero, indicate again that the minimum

[13]Curvature of the age profile is empirically unimporant for the small age range in the sample, therefore we do not include higher order terms.

[14]In the specification with individual fixed effects, only the region*time clusters remain in the error term. The standard Huber estimator of the covariance matrix allowing for these clusters is consistent. Our standard errors in column (5) are therefore the most reliable. Interestingly, the standard errors on the minimum wage variable do not differ substantially from those in column (4).

wage measure was picking up across region differences in training incidence. We feel that the estimates including time and region or individual effects are most reliable. Some of the time variation of our training measure is likely due to the change in the survey questions and cyclical variations. Differences in training incidence across regions may reflect differences in industrial and occupational compositions. The negative effect of the minimum wage in column (2) is therefore likely to reflect the fact that higher wage regions tend to have more training. The results including time and region effects, which are the most reliable, do not indicate any negative effects of minimum wages on training.[15]

In order to interpret the magnitudes of the estimates and confidence intervals using this relative measure of the minimum wage, return to the bottom panel of Table 3. The relative minimum wages increased by about 5 to 6 percentage points from 1989 to 1991. To gauge the impact of the federal increases, it is more useful to calculate the means only for the states that were subject to the federal minimum in 1989. For these states, the federal increases raised the relative minimum wage measures by 0.07 using the male state or region medians, and by 0.08 using the state/gender medians. This means that a 95 percent confidence interval for the estimates in column (4) using state medians for the unrestricted sample excludes negative changes in training of 2.9 percentage points or larger in response to the federal increases. It is instructive to compare this result to the findings by Neumark and Wascher (1998), for example. Their point estimates imply that the California state minimum wage (which was similar in magnitude to the federal increase between 1989 and 1991) led to a decline in formal training of 3.2 percentage points among young workers age 20-24, a group with a similar average training incidence as our sample. We can reject a decrease of this size for our sample.

Looking at the low wage sample, we obtain a virtually identical point estimate when we include time and state effects. When we include individual instead of state effects, the point estimate is actually positive now. Thus, the results with the low wage sample do not indicate that the estimates for the basic sample were attenuated. These conclusions are unchanged when we look at the minimum wage measure scaled by alternative values of the median wage. In rows 5 and 6, we use the median wage for 136 regions smaller than the states. We find small positive point estimates for the basic sample, and slightly larger estimates in the low wage sample. To the degree that these results indicate attenuation in the basic sample, they suggest that the actual effect of minimum wages on training is positive, rather than negative. The within estimates using these

[15]Our theory predicts that minimum wages reduce employment, but our samples focus on employed workers. This might bias our estimates up. When we use a sample of respondents including non-workers, we actually find slightly more postive results, however.

smaller regions turn out not to be any more precise than the state results, however.

The last two rows in Table 7 present results scaling the minimum wage by the median wage for older workers in the same state and of the same gender as the respondent. The results for the basic sample are again rather similar to the previous measures. The results are now somewhat more precise, because there is more variation in the measure exploiting gender differences. For the low wage sample, the results are a bit different. Controlling for time and state effects, we now find a negative effect for minimum wages. However, this result is not replicated when we control for individual effects instead. In addition, if we tighten the low wage sample further to include only workers who earned less than 130 percent of the federal minimum wage in the previous year, we find a coefficient of 0.097 (with a standard error of 0.294). We conclude that the negative estimate for the low wage sample is much more likely an indication of the sampling variation of these estimates rather than evidence that the estimates for the basic sample are attenuated.

However, the results do not provide strong support for our model either. Although some of the estimates are positive, and our baseline estimate is consistent with a 3.8 percentage point increase in the training probability of affected workers, none of the positive effects are statistically significant.

5.3 Discussion of the Results

Overall, the results using either affected workers, which is our preferred methodology, or using minimum wage levels provide strong support neither for the standard theory nor for our alternative model based on labor market imperfections. In fact, most estimates show no—or little—effect of minimum wages on training. This may be due to a variety of reasons:

1. Training incidence is relatively small in this sample of low wage workers, and with our sample size we may simply be unable to detect the effects of minimum wages. Mismeasurement of training in the data, of which there undoubtedly will be some, will make this worse. However, we have demonstrated that we are able to detect numerically small but significant effects on other covariates. Our results also differ substantially from the previous literature and they are precise enough, for example, to reject the point estimates obtained by Neumark and Wascher (1998). Furthermore, our estimates are much more precise than theirs.

2. Measurement error may bias our estimates towards zero. In particular, the regressions in Section 5.2 may include too many unaffected workers in the sample,

therefore attenuating the effect. While not definite, we have probed this explanation by comparing the results from the base sample with a low wage sample. The pattern of results does not suggest a major role for attenuation bias.

3. Our training measure does not distinguish general and specific training and most training in our sample may be specific. If this training is already being financed by firms, which could be likely for low wage workers, there is no reason to expect that minimum wages lead to a reduction in training. But the literature on this topic suggests that the bulk of reported training is very general. Loewenstein and Spletzer (1999a) find in later waves of the NLSY that 63 percent of all training is general, and in another 14 percent of training programs, most of the skills are reported to be general. They find similar results with other datasets as well.

4. If most training is rather informal, the reduction in training may not show up in our data. While possible, we believe that it is unlikely that formal and informal training will behave very differently. Loewenstein and Spletzer (1999b) analyze questions on informal training in later waves of the NLSY, and find formal and informal training to be highly correlated. Furthermore, it would be inconsistent with Becker's theory or our alternative model if minimum wages had no effect on formal training.

5. The implications of both Becker's or our theory can be seen most starkly if training is a zero-one choice. When training is a continuous choice, in Becker's theory, a higher minimum wage may eliminate some but not necessarily all training. This will happen because some workers earn low wages because they are financing their own training. A binding minimum wage will prevent them from financing all the previous training but it may still be consistent with some worker financed training. A similar argument could be made with respect to our model. Workers may have received firm financed training even in the absence of minimum wages (see Acemoglu and Pischke, 1999b). A minimum wage may have raised the intensity of training without a large effect on the number of trained workers. Given our results, we cannot rule these possibilities. For reasons stated above, we do not pursue the analysis of training durations.

6. Finally, our finding of small overall effects on training could result from a combination of the effects in Becker's model and our theory, where minimum wages reduce training for some workers and induce training for others.

We do not believe that any single one of the statistical or theoretical arguments 1 – 5 above on its own is a sufficient explanation for our findings. Obviously, it is possible that a combination of these factors could account for our results. It is important to point out, however, that all of the points 1 – 4 also affect previous empirical studies on this topic, since they have analyzed similar data with analogous limitations. Since all the previous studies also have similar—and some other— problems, we find our results at least as reliable as other studies on the topic. We conclude that small effects of the minimum wage on training are therefore a distinct possibility. This makes the last explanation for the results appealing. In the next section, we develop a model which combines elements of Becker's theory with our model and derive some further empirical implications.

6 A Hybrid Model: The Effect of Minimum Wages When Workers Can Pay For Training

Our baseline model in Section 3 introduced two deviations from the standard Becker model. Labor markets were noncompetitive, and workers were unable to pay for training. Under these assumptions, we derived the opposite of the prediction of the standard Becker model: minimum wages were found to increase—rather than decrease—training. Since the empirical evidence seems to support neither the standard Becker model nor our model, we now generalize the model of Section 3 by allowing some of the workers to invest in their general training.

We maintain all the assumptions from Section 3, and in addition, assume that some of the workers can "buy" training from their employers, so the contractual problems discussed in Section 3 are absent, at least for a certain fraction of the workers. In particular, a fraction λ of the workers can pay for their training, as long as the minimum wages do not rule out wage cuts. The remaining $1-\lambda$ fraction of the workers cannot. For example, these workers have a consumption commitment and cannot borrow to meet it if they take a wage cut to finance training.[16] For brevity, we will refer to this latter group of workers as "credit constrained."

6.1 Equilibrium Without Minimum Wages

Equilibrium wages without the minimum wage continue to be given by equation (1) in Section 3. Therefore, once again $\Pi_j(\eta_i, T) - \Pi_j(\eta_i, NT) = -c$, and firms have no incentive to invest in their employees' skills. But now, there can be worker-financed training. More specifically, Assumption 1 ensures that all workers who can afford it

[16] For simplicity, we assume that the probability of being able to finance training is independent of the worker's ability. All the results go through in a more realisitc model, where workers smooth consumption over the two periods and some workers are credit constrained.

will prefer to get training by taking a wage cut. Therefore, a worker with access to credit will always receive training, whereas constrained workers will be unable to obtain training. All workers will be employed, and competition among firms will ensure that first-period wages are given by

$$w_1(\eta_i, T) = \theta\eta_i + \delta - c \tag{17}$$

for workers financing their training, and

$$w_1(\eta_i, NT) = \theta\eta_i + \delta \tag{18}$$

for workers not paying for their training.

In this competitive equilibrium, all training is therefore of the Becker-type, financed by workers taking a wage cut in the first period. The total number of workers obtaining training is simply

$$T^c = \lambda.$$

The only reason why the equilibrium is not first-best is because some of the workers are credit constrained and cannot invest in training even though doing so would increase total output.

6.2 The Impact of Minimum Wages on Training

Now consider a binding minimum wage, i.e. w_M such that $w_M > \underline{\eta}$. Similar reasoning to above immediately implies that second period wages are given by equation (4).

Let us first discuss the adverse effect of the minimum wage on investment in skills, which were absent in Section 3. Some of the workers previously financing their own training will now be unable to do so because paying for their own training would involve receiving a wage below the minimum in the first period. In particular, all workers with access to credit and productivity such that

$$\theta\eta_i + \delta - c < w_M. \tag{19}$$

will be unable to take the necessary wage cut to finance training.[17] Therefore, the number workers obtaining training by taking a wage cut is now

$$\lambda \left[1 - G\left(\frac{w_M - \delta + c}{\theta}\right)\right], \tag{20}$$

[17]This is without a training subminimum. In this model, as in the standard Becker model, introducing training subminima increases training.

which is clearly a decreasing function of w_M. So a binding minimum wage reduces training for some workers. In a competitive labor market this is the only impact of the minimum wage on training. Since all training is general in this economy, the minimum wage eliminates all training among *affected* workers (i.e. those workers for whom equation (19) holds).

There is still the second mechanism at work which we described in Section 3 above. The introduction of a minimum wage induces firms to train some of their low skilled workers. Consider a worker who does not have access to credit, and hence could not afford to buy training when there was no minimum wage. From the results in Section 3, it is more profitable for a firm to pay for the training of some workers than to use them as unskilled employees. The analysis is identical to that in Section 3, except that the firm may finance the training of workers, who can pay for this training themselves, but are constrained by w_M. (19) has to be satisfied for these workers, but this implies immediately that (10) has to be violated, i.e. it is not profitable for firms to sponsor training of these workers. Therefore, total training in equilibrium with a minimum wage is

$$\begin{aligned} T^{nc} &= \lambda \left[1 - G\left(\frac{w_M - \delta + c}{\theta} \right) \right] + \\ &\quad (1-\lambda) \max \left\{ \left[G\left(w_M - c \right) - G\left(\frac{w_M - \delta + c}{\theta} \right) \right] ; 0 \right\} \end{aligned} \tag{21}$$

where the second term is the amount of firm-sponsored training given by $T(w_M)$ in equation (13) above.

Notice that if $\delta = 0$—i.e., if there are no employment rents—, then the term in the second line in (21) will be equal to zero as discussed in Section 3. In this case, $T^{nc} = \lambda [1 - G((w_M + c)/\theta)]$, and therefore, the implications of a minimum wage will be identical to the standard Becker model. In contrast, when $\delta > 0$, minimum wages can increase training.

Now consider the introduction of a minimum wage w_M, and compare two sectors with different values of δ, i.e. different amounts of rents. A prediction of the hybrid model here is that in sectors where $\delta < \delta^*$ given by (14) in Section 3, the introduction of the minimum wage reduces training, whereas in sectors with $\delta \geq \delta^*$, the minimum wage may increase training. This observation implies a useful empirical implication, which we will investigate empirically in the next section. In particular, we will look at the effect of minimum wages on sectors that differ by the extent of rents and competitiveness.

7 Further Evidence

In this section, we make a first attempt at providing some evidence that industry structure matters for the effects of minimum wages on training. We use the industry wage differential as a proxy for the rents present in an industry (see Katz and Summers, 1989). We estimated industry wage differentials for 47 two digit industries for all workers age 18 to 65 in the 1986 CPS Merged Outgoing Rotation Groups.[18] We then split these industries into two groups depending on the estimated wage differential, each group containing half of total employment in the CPS sample. The group with a lower industry wage differential is presumed to comprise more competitive industries, the group with higher industry wage differentials the less competitive ones. These groups are then assigned to each observation in the NLSY based on the industry affiliation in the previous year (since current industry affiliation may potentially be affected by the minimum wage).

In Table 7, we repeat the regressions for affected workers as in Table 4 using the whole sample, and add a dummy variable for less competitive industries and an interaction of this variable with the affected indicator. We also repeat the same regressions using the industry wage differential directly, rather than splitting industries into two distinct groups. In this case, we scaled the industry wage differential such that the industry with the lowest differential (private household services) is set to zero. The range of the industry wage differentials goes up to 0.85 (petroleum), the mean in the NLSY sample is 0.52, and the mean for affected workers is around 0.40. The model in the previous section predicts that the interaction term should have a positive coefficient—which would correspond to a more positive effect of minimum wages in less competitive industries.

The results in Table 7 are largely supportive of the model. When we use the interaction with the industry split we find zero or small negative coefficients on the variable for affected workers. This now captures the effect on workers in more competitive industries. The effects on the interaction with the indicator for a less competitive (higher industry wage differential) industry are throughout positive. The magnitude of the difference is quite substantial, though typically not statistically significant. In columns (2) and (3) the interaction term has a magnitude of about 0.03, indicating that training may go up by as much as 3 percentage points more for affected minimum wage workers in less competitive industries. The interaction effect is not very precisely estimated, however, because there are relatively few minimum wage workers in these high wage industries.

The results are similar when we use the industry wage differential directly, rather than

[18] The regression also includes years of schooling, a quartic in experience, dummies for female, married, a female-married interaction, black, Hispanic, SMSA, and three region dummies. The sample size is 169,921.

a discrete industry split. The main effect on the indicator for affected workers is now to be interpreted as the effect on workers in the private household sector. This effect is large and negative. This is consistent with the notion that some workers in the sectors with the lowest industry wage differentials were financing some training themselves, and this training is being eliminated by the increase in the minimum wage. The interaction term with the industry wage differential is again positive and of the order of 0.1 in columns (6) and (7). This means that each 10 percentage point increase in the wage differential is associated with a 1 percentage point lower reduction in the amount of training due to the minimum wage. For example, the results in column (6) imply that the minimum wage has no effect on training in social services (with an industry wage differential of 0.32 above the private household sector), while raising training by 3 percentage points for a worker in the wholesale industry (with an industry wage differential of 0.56 above private household). When we allow for spillovers from the minimum wage, as in columns (7) and (8), the interaction term is significant. While these results are far from definite, they are encouraging for the hybrid model.

8 Concluding Remarks

This paper makes some theoretical as well as empirical contributions to the literature on minimum wages. The perceived wisdom is that minimum wages ought to lower training for affected workers. This conclusion is based on competitive labor markets and the ability of workers to "buy" training from firms. We show that it is possible to obtain the opposite theoretical prediction in a model where there are employment rents due to labor market fictions and workers cannot finance their own training.

In addition, we provide a new set of estimates based on minimum wage changes at the state and federal level during the late 1980s. This evidence indicates no or only small effects of minimum wages on training, a result which does not clearly support either model.

To reconcile these results, we integrate elements of the standard Becker model, where workers pay for general training, with a model in which firms pay for the training. The overall effect of minimum wages on training is ambiguous in this hybrid model, but the model also has some new predictions. The effect of minimum wages on training should depend on the labor market structure. When employment rents are more important, a positive effect of minimum wages on training is more likely. We provided evidence consistent with this implication using industry wage differentials as a measure of industry rents.

Future research should establish whether the relationship between the effect of minimum wages and the competitive conditions of the industry holds up in other dimensions as well. In addition, the model also implies that minimum wages should increase training more for workers who are more likely to be credit constrained. It would be interesting to investigate this issue. Unfortunately, the NLSY does not contain good measures of the extent of credit constraint for individuals, so this issue is also left for future research.

9 Appendix 1: Construction of Regions

Some of the minimum wage measures used in this paper scale the state minimum wage level by a regional reference wage. An NLSY respondent's reference wage is the median wage in his/her geographic region for 35-54 year old male workers calculated from the CPS Outgoing Rotation Groups. This appendix explains how the regions were constructed.

To account for the fact that there is significant variation in average wages within many states, and to allow the minimum wage measure to have greater within-state variation, we used information on an NLSY respondent's CMSA/MSA of residence, ("msa_code"). Because of sample sizes, we used msa_code only for individuals residing in the large MSAs. For all other areas (i.e., smaller MSAs and non-MSAs), we used data on whether an individual resided in an MSA ("msa_status").

The following list contains the 35 MSAs we classified as large. We included the 20 CMSAs defined by the 1990 FIPS Guidelines (plus the St. Louis, MO CMSA, while we do not distinguish the Providence, RI CMSA from other metropolitan parts of RI) and added 15 MSAs with large populations.

Atlanta, GA
Baltimore, MD
Boston-Lawrence-Salem, MA-NH CMSA
Buffalo-Niagara Falls, NY CMSA
Chicago-Gary Lake County, IL-IN CMSA
Cincinnati-Hamilton, OH-KY CMSA
Cleveland-Akron-Lorraine, OH CMSA
Columbus, OH
Dallas-Fort Worth, TX CMSA
Denver-Boulder, CO CMSA
Detroit-Ann Arbor, MI CMSA
Hartford-New Britain-Middletown, CT CMSA
Houston, TX CMSA
Indianapolis, IN
Kansas City, KS
Los Angeles-Anaheim-Riverside, CA CMSA
Miami-Ft. Lauderdale, FL CMSA
Milwaukee-Racine, WI CMSA
Minneapolis-St. Paul, MN
New Orleans, LA

New York-New Jersey-Long Island, NY-NJ-CT CMSA
Norfolk-Virginia Beach-Newport News, VA
Orlando, FL
Philadelphia-Wilmington-Trenton, PA-DE-NJ CMSA
Phoenix , AZ
Pittsburgh-Beaver Valley, PA CMSA
Portland-Vancouver, OR CMSA
Sacramento, CA
St. Louis, MO CMSA
San Antonio, TX
San Diego, CA
San Francisco-Oakland-San Jose, CA CMSA
Seattle-Tacoma, WA CMSA
Tampa-St. Petersburg-Clearwater, FL
Washington, DC, DC-MD-VA

The full set of regions consists of state-msa_code level regions for the above 35 MSAs and state-msa_status level regions for all other areas. The example of an individual living in Massachusetts illustrates how the reference wage is chosen. If the individual lived in the Boston CMSA, the reference wage would be the median wage for 35-54 year old male workers living in the Massachusetts part of the Boston CMSA (the "MSA median"). If the individual lived in a metropolitan area other than the Boston CMSA, the reference wage would be the median wage for 35-54 year old male workers living in Massachusetts metropolitan areas other than the Boston CMSA. If the individual lived in a non-metropolitan area, the reference wage would be the median wage for 35-54 year old male workers living in Massachusetts non-metropolitan areas ("non-MSA median").

For confidentiality reasons, both the NLSY and CPS sometimes suppress the specific MSA of residence for individuals in smaller MSAs and/or smaller states. In the CPS data, there were five states in which msa_status was sometimes suppressed: Maryland, Nevada, Rhode Island, Utah and Wyoming. We classified these suppressed observations as non-MSA, with the following rationale. In Maryland, the suppressed observations are from the Maryland part of the Philadelphia CMSA. In Nevada, they are from the non-central city part of the Reno MSA. In Rhode Island, they are from the Rhode Island part of the New London MSA. In Utah, they are from the Utah part of the Flagstaff MSA. Finally, no MSAs in Wyoming are identified. For all five states, an individual was either classified as living in an MSA or the information was suppressed, i.e. observations from fringes of MSAs were grouped with the non-MSA observations to ensure confidentiality. Thus, classifying all the individuals living in areas with suppressed codes as non-MSA seems the most sensible choice.

In addition, there are MSAs which straddle different states and sometimes part of an MSA in a particular state is not identified if the part is too small. This was the case for: the Indiana part of the Cincinnati MSA, the Maryland part of the Philadelphia CMSA, the North Carolina part of the Norfolk MSA and the Wisconsin part of the Chicago CMSA and Minneapolis MSA. The CPS groups these areas with other metropolitan areas, and thus it is appropriate to use the state-MSA median for these observations in the NLSY. Finally, in Maryland, the Cumberland, MD-WV MSA and the Hagerstown, MD MSA are not separately identified. Here we assign the non-MSA median since Maryland's non-MSA median is really a median excluding the Baltimore and Washington, D.C. MSAs.

In the NLSY data, there were two complications. First, as in the CPS, msa_status was suppressed for certain individuals. We dropped the suppressed observations (62 out of 21,680 observations from the Basic Sample). Second, the NLSY uses NECMAs for the New England states whereas the CPS uses CMSA/MSAs. The NLSY switched to NECMA codes in 1988; we constructed the NECMA codes for 1987 by using information on state and county of residence. We then mapped NECMA codes into MSA codes. Specifically, individuals living in the Boston NECMA or Hartford NECMA were classified as living in the Boston CMSA and Hartford CMSA, respectively. Also, individuals living in any NECMA were classified as living in an MSA.

All together, the unrestricted NLSY sample contains 21,618 observations from 136 regions and 753 region-year categories. Of the 136 regions, 44 correspond to state-msa_code level regions, 45 to state-MSA level regions and 47 to state-non-MSA regions. These regions form a partition of the country. The median number of observations in each region-year cell in the CPS is 274 and the range is 27 (for the Indiana part of the Chicago CMSA in 1991) to 2261 (the Los Angeles CMSA in 1990). Less than 10 percent of all cells have fewer than a 100 observations and less than 5 percent of the NLSY observations fall into those cells.

10 Appendix 2: Estimation of the Standard Errors

We assume that the error term has the form $\varepsilon_{ij} = \lambda_i + \upsilon_j + \xi_{ij}$ where i denotes individual and j denotes region*time. Let x_{ij} be a vector of right-hand side variables,

$$X_i = \begin{bmatrix} x'_{i1} \\ x'_{i2} \\ \cdots \\ x'_{iJ} \end{bmatrix}$$

the matrix of right-hand side variables for individual i, and X the stacked matrix of all the X_i's. To simultaneously adjust for individual and region*time effects, we use the covariance matrix

$$\widehat{V} = (X'X)^{-1}X'\Omega X(X'X)^{-1}$$

where

$$X'\Omega X = \sum_i X_i'\widehat{\varepsilon}_i\widehat{\varepsilon}_i'X_i + \sum_{i\neq k}\sum_j \widehat{\varepsilon}_{ij}\widehat{\varepsilon}_{kj}x_{ij}x_{kj}'.$$

In order to get an idea how this covariance estimator performs in our sample, we conducted a small Monte Carlo experiment. For this experiment we generated samples with the same number of observations, individuals, regions, and time periods as in our unrestricted sample according to the design

$$\begin{aligned} y_{ij}^* &= 0.4 + 0.1x_{1ij} + 0.1x_{2j} + 0.1x_{3i} + \lambda_i + \upsilon_j + \xi_{ij} \\ y_{ij} &= \begin{cases} 1 & \text{if } p \leq y_{ij}^* \\ 0 & \text{if } p > y_{ij}^* \end{cases} \end{aligned}$$

where p and each of the x's are drawn from a uniform (0,1) distribution and λ_i, υ_j, ξ_{ij} $\sim N(0, 0.1)$. In regression 1, we computed the OLS estimate of y on the three x's and constructed the covariance matrix above. In regression 2, we estimated the regression of y on the x's also including region and time fixed effects. We replicated each regression 10000 times with the following results:

	Regression 1			Regression 2		
regressor	x_{1ij}	x_{2j}	x_{3i}	x_{1ij}	x_{2j}	x_{3i}
standard deviation of $\widehat{\beta}$	0.0116	0.0320	0.0126	0.0116	0.0355	0.0124
mean of estimated standard error	0.0116	0.0314	0.0124	0.0115	0.0304	0.0122
Year Effects		No			Yes	
Region Effects		No			Yes	

Regression 1 indicates that the covariance estimator does a good job in estimating the sampling variation for all regressors. However, in regression 2, the sampling variation for x_{2j}, which only varies at the region*time level, i.e. the analogue to our minimum wage measure, is understated by about 15 percent. Of course, these results are only suggestive. In our design, υ_j contributes a third to the total variance of the error term. The results would change as we change the relative variances of this error component.

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Table 1
Minimum Wage and Relative Minimum by State and Year

	Statutory Minimum Wage						*Minimum Wage/Avg. Median Wage Age 35-54*					
State	*1987*	*1988*	*1989*	*1990*	*1991*	*1992*	*1987*	*1988*	*1989*	*1990*	*1991*	*1992*
Alabama	3.35	3.35	3.35	3.80	4.25	4.25	0.308	0.306	0.305	0.344	0.383	0.381
Alaska	3.85	3.85	3.85	4.30	4.75	4.75	0.215	0.214	0.213	0.237	0.260	0.259
Arizona	3.35	3.35	3.35	3.80	4.25	4.25	0.262	0.261	0.260	0.293	0.326	0.325
Arkansas	3.35	3.35	3.35	3.80	4.25	4.25	0.342	0.341	0.339	0.382	0.426	0.424
California	3.35	3.35/4.25	4.25	4.25	4.25	4.25	0.234	0.279	0.294	0.292	0.291	0.290
Colorado	3.35	3.35	3.35	3.80	4.25	4.25	0.245	0.244	0.243	0.274	0.305	0.304
Connecticut	3.37/3.75	3.75/4.25	4.25	4.25	4.27	4.27	0.236	0.265	0.279	0.277	0.277	0.276
Delaware	3.35	3.35	3.35	3.80	4.25	4.25	0.242	0.241	0.240	0.270	0.301	0.300
Florida	3.35	3.35	3.35	3.80	4.25	4.25	0.304	0.303	0.302	0.340	0.378	0.377
Georgia	3.35	3.35	3.35	3.80	4.25	4.25	0.290	0.288	0.287	0.324	0.360	0.359
Hawaii	3.35/3.85	3.85	3.85	3.85	4.25	4.75/5.25	0.251	0.276	0.275	0.273	0.300	0.343
Idaho	3.35	3.35	3.35	3.80	4.25	4.25	0.296	0.294	0.293	0.330	0.368	0.366
Illinois	3.35	3.35	3.35	3.80	4.25	4.25	0.239	0.238	0.237	0.268	0.298	0.297
Indiana	3.35	3.35	3.35	3.80	4.25	4.25	0.281	0.280	0.279	0.314	0.350	0.348
Iowa	3.35	3.35	3.35/3.85	3.85/4.25	4.25/4.65	4.65	0.285	0.283	0.293	0.330	0.363	0.386
Kansas	3.35	3.35	3.35	3.80	4.25	4.25	0.270	0.269	0.267	0.301	0.335	0.334
Kentucky	3.35	3.35	3.35	3.80	4.25	4.25	0.291	0.290	0.288	0.325	0.362	0.361
Louisiana	3.35	3.35	3.35	3.80	4.25	4.25	0.289	0.288	0.286	0.323	0.359	0.358
Maryland	3.35	3.35	3.35	3.80	4.25	4.25	0.235	0.234	0.233	0.262	0.292	0.291
Massachusetts	3.55/3.65	3.65/3.75	3.75	3.80	4.25	4.25	0.250	0.256	0.256	0.258	0.287	0.286
Michigan	3.35	3.35	3.35	3.80	4.25	4.25	0.231	0.230	0.229	0.258	0.287	0.286
Minnesota	3.35/3.55	3.55/3.85	3.85/3.95	3.95/4.25[1]	4.25	4.25	0.256	0.272	0.288	0.292	0.312	0.311
Mississippi	3.35	3.35	3.35	3.80	4.25	4.25	0.340	0.339	0.337	0.380	0.423	0.422
Missouri	3.35	3.35	3.35	3.80	4.25	4.25	0.279	0.278	0.277	0.312	0.348	0.346
Montana	3.35	3.35	3.35	3.80	4.25	4.25	0.292	0.290	0.289	0.326	0.363	0.361
Nebraska	3.35	3.35	3.35	3.80	4.25	4.25	0.303	0.302	0.300	0.338	0.377	0.375
Nevada	3.35	3.35	3.35	3.80	4.25	4.25	0.266	0.265	0.264	0.297	0.331	0.330
New Hampshire	3.45/3.55	3.55/3.65	3.65/3.75	3.75/3.85	4.25	4.25	0.257	0.263	0.269	0.277	0.308	0.307
New Jersey	3.35	3.35	3.35	3.80	4.25	5.05	0.216	0.216	0.214	0.242	0.269	0.319
New Mexico	3.35	3.35	3.35	3.80	4.25	4.25	0.284	0.283	0.281	0.317	0.353	0.352
New York	3.35	3.35	3.35	3.80	4.25	4.25	0.238	0.237	0.236	0.266	0.296	0.295
North Carolina	3.35	3.35	3.35	3.80	4.25	4.25	0.315	0.313	0.312	0.352	0.391	0.390
North Dakota	3.35	3.35	3.35	3.80	4.25	4.25	0.291	0.290	0.288	0.325	0.361	0.360
Ohio	3.35	3.35	3.35	3.80	4.25	4.25	0.260	0.259	0.258	0.291	0.324	0.322
Oklahoma	3.35	3.35	3.35	3.80	4.25	4.25	0.282	0.281	0.279	0.315	0.350	0.349
Oregon	3.35	3.35	3.35/4.25[2]	4.25/4.75	4.75	4.75	0.262	0.261	0.287	0.336	0.364	0.363
Pennsylvania	3.35	3.35/3.70	3.70	3.80	4.25	4.25	0.269	0.273	0.294	0.300	0.334	0.333
Rhode Island	3.55/3.65	3.65/4.00	4.00/4.25	4.25	4.45	4.45	0.281	0.302	0.321	0.324	0.338	0.337
South Carolina	3.35	3.35	3.35	3.80	4.25	4.25	0.305	0.304	0.302	0.341	0.379	0.378
South Dakota	3.35	3.35	3.35	3.80	4.25	4.25	0.337	0.336	0.334	0.377	0.420	0.418
Tennessee	3.35	3.35	3.35	3.80	4.25	4.25	0.331	0.329	0.328	0.370	0.411	0.410
Texas	3.35	3.35	3.35	3.80	4.25	4.25	0.280	0.279	0.278	0.313	0.348	0.347
Utah	3.35	3.35	3.35	3.80	4.25	4.25	0.259	0.258	0.256	0.289	0.322	0.320
Vermont	3.45/3.55	3.55/3.65	3.65/3.75	3.85	4.25	4.25	0.306	0.313	0.320	0.324	0.361	0.360
Virginia	3.35	3.35	3.35	3.80	4.25	4.25	0.252	0.251	0.249	0.281	0.313	0.312
Washington	3.35	3.35/3.85	3.85/4.25	4.25	4.25	4.25	0.236	0.244	0.277	0.294	0.293	0.292
West Virginia	3.35	3.35	3.35	3.80	4.25	4.25	0.294	0.293	0.292	0.329	0.366	0.365
Wisconsin	3.35	3.35	3.35/3.65	3.80	4.25	4.25	0.270	0.269	0.286	0.302	0.336	0.335
Wyoming	3.35	3.35	3.35	3.80	4.25	4.25	0.263	0.262	0.261	0.294	0.327	0.326

[1] Minnesota: Only large employers covered by the new 1991 minimum wage.
[2] Oregon: Minimum wage changed from 3.35 to 3.85 and then 4.25 during the year.
Notes: Left hand panel of the table shows the higher of the state or federal minimum wages in each state and year. Years begin in April of the year shown until March of the following year. Multiple minima are shown if the state minimum changed during the April to March period. The right hand panel of the table shows the minimum wage divided by the average median wage for male workers 35-54 years old in the state during the 1987 to 1992 period.

Table 2
Sample Means of Demographics
(Standard Deviations in Parentheses)

	Non-mover Sample		*Unrestricted Sample*		*Low Wage Sample*	
Variable	*1988*	*1992*	*1987*	*1992*	*1987*	*1992*
Female	0.434	0.428	0.432	0.427	0.527	0.602
Black	0.134	0.132	0.128	0.133	0.169	0.204
Hispanic	0.067	0.067	0.064	0.067	0.061	0.073
Age	27.2	31.2	26.2	31.2	25.8	31.1
Less than High School	0.187	0.171	0.192	0.172	0.262	0.240
New Job	0.273	0.216	0.268	0.220	0.338	0.323
Nominal Hourly Wage	8.16 (5.36)	9.71 (6.54)	7.58 (5.51)	9.70 (6.51)	5.13 (2.57)	5.91 (2.82)
Real Hourly Wage (1982-84 $)	7.30 (4.79)	8.52 (5.73)	6.81 (4.95)	8.51 (5.71)	4.60 (2.30)	5.18 (2.47)
Fraction Earning Minimum or Less	0.042	0.058	0.068	0.059	0.155	0.162
Number of Observations	3872	3094	3979	3143	1673	1049

Notes: Unbalanced panel from the NLSY. Unrestricted sample consists of individual–year observations that have high school education or less, work in at least one month of the year and in both the prior and current year have non-missing wage data. Non-mover sample excludes from the unrestricted sample individuals who have moved to a new state since the previous year. Low wage sample imposes on the unrestricted sample the restriction that the “CPS wage” in the previous year is less than or equal to 150% of the federal minimum wage. Year refers to April to March (of following calendar year). New job refers to the start of any new job during the year. Hourly wage is the “CPS wage” for workers employed at a “CPS job” only. NLSY data include black, Hispanic and poor white oversamples. The poor white oversamples were discontinued in the 1991 survey year, accounting for the lower number of observations in 1992. Statistics are weighted by the NLSY sampling weights.

Table 3
Sample Means of Key Variables
(Standard Deviations in Parentheses)

Variable	*1987*	*1988*	*1989*	*1990*	*1991*	*1992*
			Non-mover Sample			
Training Incidence		0.099	0.099	0.106	0.094	0.103
Nominal Minimum Wage		3.44	3.51	3.87	4.26	4.28
		(0.21)	(0.29)	(0.16)	(0.05)	(0.15)
Minimum wage increased and wage in prior year is below the current minimum wage		0.014	0.020	0.069	0.091	0.001
Minimum increased and wage in prior year is below the current minimum and above prior year minimum		0.007	0.008	0.034	0.050	0.000
Minimum wage increased and wage in prior year is below 150 % of the current minimum wage		0.062	0.085	0.264	0.293	0.011
Minimum wage increased and wage in prior year is below 130 % of the current minimum wage		0.041	0.058	0.166	0.204	0.007
Number of Observations		3872	3793	3187	3128	3094
			Unrestricted Sample			
Training Incidence	0.064	0.099	0.100	0.108	0.094	0.103
Nominal Minimum Wage	3.36	3.44	3.51	3.87	4.26	4.28
	(0.06)	(0.21)	(0.29)	(0.16)	(0.05)	(0.15)
Real Minimum Wage	3.02	3.08	3.12	3.42	3.75	3.76
(1982-84 $)	(0.05)	(0.19)	(0.26)	(0.14)	(0.05)	(0.13)
Minimum Wage/Median Wage (men	0.267	0.271	0.276	0.301	0.331	0.332
35-54, states)	(0.030)	(0.027)	(0.028)	(0.031)	(0.038)	(0.037)
Minimum Wage/Median Wage (men	0.272	0.277	0.282	0.307	0.338	0.340
35-54, regions)	(0.043)	(0.041)	(0.042)	(0.045)	(0.053)	(0.051)
Minimum Wage/Median Wage (35-	0.324	0.330	0.336	0.366	0.402	0.404
54, state and gender)	(0.077)	(0.076)	(0.078)	(0.086)	(0.097)	(0.097)
Number of Observations	3979	4046	3971	3303	3210	3143

Notes: See Table 2 for a description of the sample composition. Training incidence refers to employment related training. Minimum wage refers to the higher of the state or federal minimum applicable to an NLSY respondent. Median wage for 35-54 year old workers are calculated from the CPS Outgoing Rotation Groups. The poor white oversamples were discontinued in the 1991 survey year, accounting for the lower number of observations in 1990 to 1992. Statistics are weighted by the NLSY sampling weights.

Table 4
The Effect of Minimum Wage Increases on Affected Workers

Comparison Group	*All*	*Affected States*	*Low Wage Workers*	*All*	*Affected States*	*Low Wage Workers*	*All*	*All*
Independent Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Minimum wage increased and wage in prior year is below the current minimum wage	0.009 (0.014)	0.011 (0.015)	0.003 (0.016)	--	--	--	--	--
Minimum increased and wage in prior year is below the current minimum and above prior year minimum	--	--	--	0.016 (0.019)	0.018 (0.020)	0.011 (0.021)	--	--
Minimum wage increased and wage in prior year is below 150 % of the current minimum wage	--	--	--	--	--	--	0.005 (0.008)	--
Minimum wage increased and wage in prior year is below 130 % of the current minimum wage	--	--	--	--	--	--	--	-0.003 (0.010)
Change in high school graduation status	0.070 (0.054)	0.091 (0.083)	0.040 (0.041)	0.070 (0.054)	0.090 (0.083)	0.040 (0.041)	0.070 (0.054)	0.071 (0.054)
Change in new job status	0.032 (0.008)	0.039 (0.012)	0.039 (0.011)	0.032 (0.008)	0.039 (0.012)	0.039 (0.011)	0.032 (0.008)	0.032 (0.008)
Number of Observations	17074	7552	5873	17074	7552	5873	17074	17074

Notes: Non-mover sample, consisting of all workers with a high school education or less, who do not move between states from one year to the next. The low wage comparison sample consists of all workers with wages in the prior year below 150 % of the current minimum wage. Dependent variable is the change in training incidence between two consecutive years. All regressions also include a constant and year dummies. Regressions are weighted by NSLY sampling weights. Standard errors are adjusted for the presence of individual effects in the error term, and therefore robust to the MA structure of the error.

Table 5
Quantile Regressions for Real Wage on Real Minimum Wage
Unrestricted Sample

Quantile	*NLSY*	*CPS*
0.10	0.376	0.247
	(0.087)	(0.024)
		[0.084]
0.20	0.156	0.137
	(0.105)	(0.011)
		[0.109]
0.30	0.146	0.052
	(0.163)	(0.016)
		[0.121]
0.40	0.049	0.032
	(0.158)	(0.045)
		[0.102]
0.50	0.137	0.000
	(0.178)	(0.025)
		[0.111]
0.60	0.029	0.130
	(0.173)	(0.053)
		[0.113]
0.70	-0.120	0.030
	(0.247)	(0.043)
		[0.135]
0.80	0.296	0.011
	(0.446)	(0.044)
		[0.198]
0.90	0.312	0.157
	(0.593)	(0.143)
		[0.282]
Year Effects	Yes	Yes
State Effects	Yes	Yes

Notes: Samples include respondents with a high school degree or less. Dependent variable is the real hourly wage. NLSY regressions are weighted by the NLSY sampling weights. Standard errors in parentheses are not adjusted for clusters in the error term. Bootstrapped standard errors using state*year blocks are in brackets (100 replications). Number of observations is 21618 in the NLSY, and 119464 in the CPS.

Table 6
Panel Regressions for Training Incidence

Sample	*Independent Variable*	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Unrestricted	Real Minimum Wage	0.038	0.053	0.026	-0.007	-0.012
		(0.012)	(0.019)	(0.010)	(0.017)	(0.021)
Low Wage	Real Minimum Wage	-0.002	-0.004	-0.001	-0.021	-0.034
		(0.015)	(0.031)	(0.013)	(0.032)	(0.054)
Unrestricted	Real Minimum Wage/Avg. Median	-0.003	-0.186	0.305	-0.028	-0.099
	Wage (men 35-54, states)	(0.081)	(0.100)	(0.103)	(0.211)	(0.224)
Low Wage	Real Minimum Wage/Avg. Median	-0.020	-0.018	0.016	-0.169	0.198
	Wage (men 35-54, states)	(0.120)	(0.142)	(0.153)	(0.406)	(0.416)
Unrestricted	Real Minimum Wage/Avg. Median	-0.070	-0.177	0.307	0.038	0.050
	Wage (men 35-54, regions)	(0.060)	(0.067)	(0.100)	(0.193)	(0.172)
Low Wage	Real Minimum Wage/Avg. Median	-0.052	-0.063	0.050	0.117	0.270
	Wage (men 35-54, regions)	(0.079)	(0.078)	(0.146)	(0.371)	(0.328)
Unrestricted	Real Minimum Wage/Avg. Median	-0.024	-0.169	0.237	0.047	-0.017
	Wage (35-54, state and gender)	(0.063)	(0.081)	(0.083)	(0.148)	(0.167)
Low Wage	Real Minimum Wage/Avg. Median	-0.037	-0.049	-0.013	-0.140	0.127
	Wage (35-54, state and gender)	(0.081)	(0.096)	(0.109)	(0.248)	(0.313)
	Year Effects	No	Yes	No	Yes	Yes
	State or Region Effects	No	No	Yes	Yes	No
	Individual Effects	No	No	No	No	Yes

Notes: Sample includes respondents with a high school degree or less. Dependent variable is training incidence. All regressions also include a linear term in age, dummy variables for blacks, Hispanics, females, less than high school, whether the respondent started a new job within the year, and a constant where applicable. Regressions are weighted by the NLSY sampling weights. Standard errors in columns (1) to (4) are adjusted for the presence of state*time or region*time and individual effects in the error term (see Appendix 2 for details). Standard errors in columns (5) are adjusted for the presence of region*time effects in the error term. Number of observations is 21652 in the unrestricted sample and 7032 in the low wage sample.

Table 7
The Effect of Minimum Wage Increases on Affected Workers by Industry

Independent Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Industry in prior year is less competitive	-0.007 (0.005)	-0.007 (0.005)	-0.010 (0.006)	-0.008 (0.006)	--	--	--	--
Industry wage differential in prior year	--	--	--	--	-0.024 (0.015)	-0.025 (0.015)	-0.038 (0.017)	-0.030 (0.016)
Minimum wage increased and wage in prior year is below the current minimum wage	0.005 (0.016)	--	--	--	-0.008 (0.027)	--	--	--
Minimum increased and wage in prior year is below the current minimum and above prior year minimum	--	0.008 (0.023)	--	--	--	-0.032 (0.039)	--	--
Minimum wage increased and wage in prior year is below 150 % of the current minimum wage	--	--	-0.007 (0.011)	--	--	--	-0.044 (0.022)	--
Minimum wage increased and wage in prior year is below 130 % of the current minimum wage	--	--	--	-0.006 (0.013)	--	--	--	-0.021 (0.035)
Minimum increased and wage in prior year is below the current minimum wage*less competitive industry	0.011 (0.039)	--	--	--	--	--	--	--
Minimum increased and prior wage is between current and prior minimum*less competitive industry	--	0.036 (0.039)	--	--	--	--	--	--
Minimum increased and prior wage is below 150 % of the current minimum wage*less competitive industry	--	--	0.032 (0.020)	--	--	--	--	--
Minimum increased and prior wage is below 130 % of the current minimum wage*less competitive industry	--	--	--	0.004 (0.024)	--	--	--	--
Minimum increased and wage in prior year is below the current minimum wage*industry wage differential	--	--	--	--	0.036 (0.073)	--	--	--
Minimum increased and prior wage is between current and prior minimum*industry wage differential	--	--	--	--	--	0.115 (0.095)	--	--
Minimum increased and prior wage is below 150 % of the current minimum wage*industry wage differential	--	--	--	--	--	--	0.102 (0.045)	--
Minimum increased and prior wage is below 130 % of the current minimum wage*industry wage differential	--	--	--	--	--	--	--	0.036 (0.052)

Notes: Non-mover sample, consisting of all workers with a high school education or less, who do not move between states from one year to the next. Dependent variable is the change in training incidence between two consecutive years. All regressions also include a constant, the change in high school graduation status and new job status, and year dummies. Regressions are weighted by NSLY sampling weights. Standard errors are adjusted for the presence of individual effects in the error term, and therefore robust to the MA structure of the error. Sample size is 17052.

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