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MINIMUM WAGES AND TRAINING REVISITED

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ABSTRACT

Theory predicts that minimum wages will reduce employer-provided on-the-job training designed to improve workers' skills on the current job, but may increase the amount of training that workers obtain to qualify for a job. We estimate the effects of minimum wages on the amount of both types of training received by young workers by exploiting cross-state variation in minimum wage increases. The evidence provides considerable support for the hypothesis that higher minimum wages reduce training (especially formal training) aimed at improving skills on the current job. At the same time, there is little or no evidence that minimum wages increase training undertaken to qualify for or obtain jobs. Consequently, it appears that, overall, minimum wages substantially reduce training received by young workers.

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I. Introduction

The existing research on the economic consequences of minimum wage laws has focused disproportionately on the disemployment effects of such laws on younger and lesser-skilled workers. One drawback of this focus is that it provides too narrow a basis for policy evaluation; that is, there may be other channels through which minimum wages influence the well-being of the population.[1] In this paper, we address what we see to be one shortcoming along these lines--the near absence of evidence on the effects of minimum wages on skill formation associated with on-the-job training. Of course, the lost opportunities for on-the-job training are a cost of the disemployment effects of minimum wages. But this is only part of the story. If there are also reductions in on-the-job training for individuals who remain employed, the overall effect of minimum wages on skill formation could be considerably larger.

The possibility that minimum wages reduce on-the-job training was initially raised by Rosen (1972), Feldstein (1973), and Welch (1978). In the simplest case, training is financed out of workers' wages. Because the Fair Labor Standards Act (FLSA) specifies that the minimum wage applies to the wage net of any deducted training costs, however, a higher minimum wage raises the floor below which the net wage cannot fall, and hence may deter training. Alternatively, the arrangement could be structured such that the worker receives a wage above the minimum, but pays the employer for training. In this case, however, the employer must still pay the worker for time spent in training required for the job, which raises the cost of training to the employer without raising its value to the employee. Thus, regardless of the arrangement, the FLSA is likely to reduce on-the-job training paid for by the worker. Of course, to the extent that training is firm-specific

[1]For example, this research ignores the effects of minimum wages on family incomes (e.g., Neumark and Wascher, 1997).

rather than general, the employer bears more of the cost. In this case, a higher minimum wage makes it less likely that employers will find it profitable to hire a worker and pay for on-the-job training.

The best-known empirical test of the prediction that minimum wages reduce on-the-job training is Hashimoto (1982), who finds evidence consistent with this hypothesis for white men.[2,3] However, there are several reasons to question this evidence. First, Hashimoto uses an indirect test based on empirical observations on wage growth taken from panel data. This potentially is a problem because factors such as relative demand shifts induced by minimum wages could also affect wage profiles. In this sense, direct evidence on on-the-job training would be more convincing. Second, Hashimoto uses only time-series variation in minimum wages, stemming from the 1967 amendments to the FLSA. In contrast, the standard in the "new" minimum wage research is to exploit cross-state variation in minimum wages to avoid attributing to minimum wages influences from unmeasured variables common to all observations in particular years. In this paper, we attempt to remedy both of these problems by utilizing cross-state variation in minimum wage increases coupled with direct information on on-the-job training available in the 1983 and 1991 Current Population Survey (CPS) supplements.

Hashimoto's work, and the discussion so far, focuses on the effects of minimum wages on on-the-job training. As emphasized by Leighton and Mincer (1981), however, minimum wages

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[2]Hashimoto also reports evidence suggesting that the higher wages attributable to minimum wage increases are unlikely to offset the higher wage growth (and eventual higher wages) that training would have produced in the absence of the minimum wage hike.

[3]In fact, in Hashimoto's model the theoretical prediction regarding the effects of minimum wages on training is unambiguous, while the prediction regarding disemployment effects is ambiguous. The ambiguity surrounding the direction of the employment effect stems from the possibility of decreasing returns to scale in the provision of training by the firm, so that hiring an additional worker can raise the cost of all labor. As in the standard monopsony model (Stigler, 1946), this can lead to positive employment effects of minimum wages.

may encourage low-skilled individuals to obtain more schooling if the additional education raises their marginal product above the minimum wage floor. Whether or not minimum wages provide an incentive for schooling depends on the extent to which education increases an individual's market wage above the minimum and on the opportunity cost of the additional schooling; the latter, in turn, depends on the minimum wage and on the probability of finding employment if one searches for work instead of going to school.[4]

This raises the possibility that even if higher minimum wages reduce on-the-job training, they need not reduce skill formation if they provide an incentive for individuals to obtain additional schooling. This point is perhaps most relevant to the argument, made by some advocates of minimum wages in policy circles and the media, that higher minimum wages lead to a "high-wage" economy by increasing training. To a large extent, this argument is based on flawed reasoning regarding the effects of minimum wages on training. For example, Levin-Waldman (1996) argues that "if raising the minimum wage might increase the demand for skilled labor, employers might consequently be induced to provide the type of on-the-job training necessary to make so-called low-skilled workers more productive workers" (p. 27). As we just illustrated, however, in terms of standard human capital theory, coupled with the constraints imposed by the FLSA, this argument is clearly incorrect. Nonetheless, insofar as minimum wages could increase schooling or other training acquired by workers in order to qualify for jobs, this so-called "high-wage" strategy cannot be dismissed.

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[4]See also Welch (1974), Ehrenberg and Marcus (1979), Gustman and Steinmeier (1982), Lang (1987), and Agell and Lommerud (1997) for additional discussion of the effects of minimum wages on enrollment or education decisions. There is a sizable empirical literature on the effects of minimum wages on schooling; in addition to Leighton and Mincer, Mattila (1978), Cunningham (1981), Ehrenberg and Marcus (1982), and Neumark and Wascher (1996a and 1996b) have studied this issue. No clear consensus is evident from these papers, although our own previous work suggests that minimum wages reduce school enrollments.

In the present paper, we do not revisit the entire issue of schooling decisions and minimum wages. We do, however, consider evidence concerning the effects of minimum wages on the proportion of workers who undertook training to qualify for their present jobs, including training received in school. We use this evidence to assess whether, consistent with the arguments made about schooling, such training provides an offset to the effects of minimum wages on on-the-job training. We find, as predicted by the basic human capital model, that minimum wages reduce on-the-job training. Moreover, we find no evidence that minimum wages induce additional training intended to help workers qualify for their current job.

Of course, because the skills acquired in school that helped a worker qualify for his or her present job may not reflect the total investment content of schooling, the evidence presented in this paper is not sufficient to draw firm conclusions regarding the net effect of minimum wages on skill formation. If minimum wages also reduce schooling generally (as we have reported elsewhere), then the results presented here would suggest that minimum wages reduce skill accumulation among young and relatively unskilled workers. But even if minimum wages increase schooling levels, reductions in on-the-job training can still lead to a net decline in skill formation.

II. Previous Work on the Effects of Minimum Wages on Training

Despite the potential importance of minimum wage effects on training, and the voluminous literature on other issues relating to training, there is remarkably little empirical work on this topic beyond Hashimoto's original research. The only detailed published study of which we are aware is by Leighton and Mincer (1981), who examined the relationship between a minimum wage variable based on state coverage and a "standardized state wage." The latter is the estimated state dummy variable coefficient from a regression of wages on personal and job characteristics and is presumed to be inversely related to minimum wage effects, in the sense that the federal minimum wage has

more "bite" in states with lower wages. Specifically, Leighton and Mincer use a minimum wage variable defined as the state coverage ratio divided by one plus this standardized wage; the predicted effect of this variable on measures of on-the-job training is therefore negative.

Using data from the PSID for 1973-1975, and from the NLS for Young Men for 1967-1969, they first report evidence that wage growth was lower in states with higher values of the minimum wage variable. This finding holds for both white men and black men, although the evidence is generally significant only for white men.[5] Second, they examine the relationship between the minimum wage variable and direct training measures in the two data sets.[6] In general, the estimated minimum wage effects on on-the-job training are negative and significant, at least for those with a high school education or less. Finally, the authors report some evidence from the NLS using a measure of off-the-job training (excluding schooling), which might increase as a result of a higher minimum wage. The evidence is in this direction, but generally not statistically significant. Overall, the authors conclude that "The hypothesis that minimum wages tend to discourage on-the-job training is largely supported by our empirical analysis" (p. 171).

We attempt to improve on this analysis in two ways. First, as was the case with Hashimoto's analysis, Leighton and Mincer do not use any information on variation in minimum wages across states, of which there was very little in the 1967-1975 period they examine. The identifying information they use comes mainly from variation in state wages, which may itself be

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[5]They also report results for turnover, arguing that if training has a firm-specific component, more training should be associated with lower turnover. They find evidence in the PSID data that higher coverage is associated with shorter job tenure, but in the NLS find this only for black males.

[6]In the PSID, one question asks whether the respondent is learning things on his job that could lead to a better job or a promotion, while a second administered to those with at most a high school education asks whether the respondent had received any training other than schooling. The NLS has a question on training on the current job, which, according to the authors, appears to refer to more formal training.

related to training (which is not included as a regressor in constructing the standardized state wage). In particular, because training is likely to be positively related to wages at the state level, and because the wage variable appears in the denominator of the dependent variable, there is the potential for negative bias in the estimated coefficient. This bias would increase the likelihood of finding evidence consistent with the theoretical prediction. This is particularly troubling because Leighton and Mincer report separate estimates of the coefficients of coverage and the standardized wage in a set of appendix tables; these estimates show that much of the effect of the minimum wage variable comes from this standardized wage, rather than coverage. Second (and related), although the data come from multiple years, there is no attempt to control for state-specific differences by including state dummies, or data on some other group whose training is unlikely to be affected by minimum wages. Again, the empirical analysis in this paper aims to remedy these deficiencies.

Finally, Grossberg and Sicilian (1995) report that in the Employment Opportunities Pilot Project (EOPP) data set, men and women in minimum wage jobs experienced lower wage growth than workers in other low-wage jobs (in all cases this refers to the starting wage). Using the direct measures of training available in the EOPP, however, they find no evidence that workers in minimum wage jobs receive significantly less training than these comparison groups (workers earning either less than the minimum or earning just above the minimum). Thus, the evidence is consistent with Hashimoto's finding that minimum wages lower wage growth, but as there is no evidence of a relationship between the minimum wage and direct measures of training, they argue that we should be reluctant to attribute the wage growth effect to negative influence of minimum

wages on training.[7] Rather, they suggest that the observed differences in wage growth could be due to other sources, such as changes in factors affecting long-term incentive contracts (Lazear, 1979) or adverse selection (Salop and Salop, 1976).

However, we view their evidence and interpretation as suspect for a number of reasons. First, those workers in jobs paying less than the minimum are presumably in the uncovered sector, where training may be less frequent; if so, there will be a systematic upward bias in a comparison of training of minimum wage workers to training of workers below the minimum wage. While this issue does not address the comparison between minimum wage workers and those earning just above the minimum, we note that for men the point estimates indicate substantially less training for the former, although the difference is not statistically significant; of course, any shortfall could reflect unobserved differences between workers at different wage levels. Second, the authors effectively define their treatment groups based on a worker's starting wage; however, this wage is presumably jointly endogenous with training, making it difficult, at best, to interpret these estimates.[8] Third, the authors include as a control variable a measure of job complexity, which refers to the number of weeks it takes a new employee in the position surveyed to become fully trained and qualified; clearly this variable may pick up much of the variation in training. Finally, the authors never offer any explanation as to why other factors potentially affecting wage growth (such as long-term incentive contracts) might have changed in a manner that is associated with minimum wages.

Thus, there is very little evidence on the effects of minimum wages on training, and the

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[7]Grossberg and Sicilian assert that neither Hashimoto nor Leighton and Mincer had direct evidence on training, so that all the existing evidence regards wage growth. However, as our discussion of the latter paper indicates, Leighton and Mincer did use measures of training from two data sets.

[8]The authors attempt to address this endogeneity in the wage growth estimates, but inexplicably not in the training estimates.

evidence that exists has some potentially serious limitations. Consequently, we think that an empirical analysis revisiting the question of the effects of minimum wages on training is of interest.

III. The Data

The data we use to measure training are taken from supplements to the January 1983 and January 1991 CPS surveys. The 1983 survey included an Occupational Mobility, Training, and Job Tenure Supplement, and the 1991 survey included a Training Supplement.[9] The measures of training available in these supplements dovetail nicely with the two types of training for which the effects of minimum wages may differ: training to improve skills on the current job, and training to obtain (qualify for) the current job. The two supplements are very similar, with nearly-identical questions on many aspects of training.[10] As explained below, some of our statistical experiments rely only on the 1991 data, in which case comparability of the questions over time is not an issue. But others require both surveys, making this relevant.

The first set of questions we use concerns training to improve skills on the current job, which we interpret as measuring on-the-job training, and which theory predicts will decline in response to a higher minimum wage. The relevant questions are identical in 1983 and 1991, and are as follows:

(1) Since you obtained your present job, did you take any training to improve your skills?

Did you take the training in: A formal company training program? Informal on-the-job training?

In general, we expect that minimum wages are more likely to affect formal than informal training,

[9]These surveys are used by Constantine and Neumark (1996) and Bowers and Swaim (1994) to study changes in training over time, the contribution of these changes to the growth in wage inequality, and changes in returns to training.

[10]There are more differences between the surveys in detailed questions regarding financing of training, length of training, etc.

as informal training may entail considerably lower costs to employers.

The second set of questions concerns training for skills needed to obtain the current job.[11] We interpret these questions as indicators of training to qualify for the job, which theory suggests may increase in response to a minimum wage increase. There are some slight differences in these questions in the two years, as follows:

(2) Did you need specific skills or training to obtain your current (last) job?

> 1983: Did you obtain those skills or training through one or more of the following: A training program in high school or post-secondary school? A formal company training program such as apprenticeship training or other type of training having an instructor and a planned program? Informal on-the-job training or experience in previously held job or jobs?
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> 1991: Did you obtain those skills or training through one or more of the following: A training program in high school or post-secondary school, including colleges and universities? A formal company training program, including apprenticeships? Informal on-the-job training?

These training variables are undoubtedly imperfect measures of training. However, as documented in Appendix Table A1, they are strongly (and significantly) associated with higher wages, when added to standard human capital earnings equations.[12]

We supplement these data on training with information from the CPS on race, sex, schooling, age, marital status, industry, and state of residence. We also retain the CPS individual weights to construct weighted estimates, since the estimation of means is important in our

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[11]The question actually refers to the current or last job, but as the first training question only pertains to those currently with a job, we restrict the analysis to those currently with jobs in order to keep the samples the same.

[12]See also Constantine and Neumark (1996), who also review evidence suggesting that these regressions reflect, at least in part, causal effects of training. In the log wage regressions reported in the appendix table, the estimated returns to schooling are low (around .02). However, this stems from including the training variables. When they are excluded, the estimate return rises to .042, and rises still further (to about .06) if older individuals are added to the sample.

analysis.[13] To be included in our extract, respondents must be currently working (in the labor force status recode) and not self-employed. Finally, we appended to the CPS records information on the state and federal minimum wage in January of each year from 1983 to 1991.[14]

IV. Empirical Analysis

Minimum Wages

The exogenous variation that we exploit to infer the effects of minimum wages on training comes from increases in state and federal minimum wages. Thus, we initially focus on the evidence from the January 1991 CPS, a period that followed numerous increases in state minimum wages in the late 1980s and an increase in the federal minimum wage in 1990. Table 1 presents the legislated minimum wages by state (the higher of the state or federal minimum) as of January of each year from 1983 through 1991, along with the federal minimum.[15] The second-to-last column indicates that 12 states had minimum wages above the federal minimum in 1991, while the column for 1983 shows only two states above the federal minimum in that year.

We could, in principle, use the state-level increase in the minimum wage between 1983 and 1991 as our exogenous source of variation. However, there is additional variation associated with differences in the timing of state-level minimum wage increases that may also be relevant. That is, because the questions in the CPS are not limited to training received in the current year, the

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[13]This weight adjusts for overall non-response, using information on location and race. We do not correct for non-response to these supplements, as discussed in Diebold, et al. (1997). In that paper, this latter non-response was critical because the estimation involved ratios of counts from different supplement years.

[14]Observations from Washington, D.C. are dropped because minimum wages there were set on an occupational basis in the sample period, making it more difficult to measure annual increases in the minimum wage (see Neumark and Wascher, 1992).

[15]For workers covered under state and federal laws, the higher minimum wage prevails. Because the differences in coverage are minor, we simply take the higher of the two minimum wages as the prevailing one.

minimum wage effect on training may be related to how binding the minimum was over a worker's early years in the labor market, rather than just in 1991. For example, although the legislated minimum wage was $4.25 in 1991 in both California and Iowa, California's increased to $4.25 in 1989, while Iowa's increased to $3.85 in 1990 and $4.25 in 1991. In this case, we might expect to find a bigger impact of the minimum wage on training in California than in Iowa. To capture these differences in timing, in most of our specifications we use as the explanatory variable the percent by which the state minimum exceeded the federal minimum over the previous three years. The last column of Table 1 reports this gap for each state; it ranges from 0 to a high of 21.86 for California and Connecticut.[16,17]

Training to Improve Skills on the Current Job

Table 2 reports descriptive statistics from the 1991 CPS for the variables measuring training obtained to improve skills on the current job. We show results both for the overall age group of 16-24 year-olds, as well as for the 16-19 (teenager) and 20-24 (young adult) subgroups. Although a higher proportion of 16-19 year-olds are paid the minimum wage, and that group has been the focus of most work on disemployment effects of minimum wages, the influence of minimum wages on training may be greater for 20-24 year-olds because of the generally higher incidence of training for workers in this age group. That is, the effects of minimum wages on training need not be strongest for those with a wage right at the minimum. Rather, the effects will be most evident among workers having a combination of wages that are sufficiently low and training costs that are

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[16]These gaps obviously would change if we were to use a different "window," such as two or four years instead of three. Below, we report results using alternative windows to explore the robustness of our results, and find that they are insensitive to such changes.

[17]As Table 1 shows, state minimum wages were above the federal minimum wage in 1983 only in Connecticut and Alaska. As explained below, when we use data from the 1983 survey as well as the 1991 survey, we define a similar minimum wage gap variable that exceeds zero for these two states.

sufficiently high to cause the minimum wage to be a binding constraint. The workers for whom this holds are not necessarily the lowest-wage workers, as the training costs for these workers may be minimal. The table also reports estimates for 35-54 year-olds, who, as explained below, are used as a control sample in some of the estimations.

The proportions for the whole sample shown in the top panel of Table 2 indicate that the reported incidence of any training to improve skills on the current job is .27 among 16-24 year-olds, quite a bit lower (.18) for teenagers, and correspondingly higher (.30) for young adults. The reported incidence of formal training to improve skills is .08 for 16-24 year-olds, while the reported incidence of informal training is .14.[18] In both cases, the estimates are higher for the older workers and lower for the younger workers. In particular, the incidence of formal training among teenagers is extremely low (.025, vs. .100 for 20-24 year-olds), suggesting that minimum wages (or anything else) are likely to have relatively little detectable impact on formal training among teenagers.[19]

To provide a rough sense of the relationship between training and minimum wage increases, the bottom panels of the table report means for these training variables disaggregated by whether the minimum wage in the state was above or equal to the federal minimum wage over the three-year window we use--that is, whether the state minimum wage gap was positive or zero. The estimates indicate that the incidence of any training among 16-24 year-olds and among 20-24 year-olds was lower in the subset of states in which minimum wages were higher. The same is true of

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[18]Formal and informal training are not exhaustive. The survey also asks about training via correspondence courses, armed forces, and friends or relatives. These are included in the "any training" measure.

[19]The low incidence of reported formal training among teenagers in the 1991 data is not attributable solely to higher minimum wages in some states in 1991. In the 1983 data, the proportions are similar, with 7.6 percent of 20-24 year-olds and 1.9 percent of 16-19 year-olds reporting formal training of this type. Of course, it is also possible that the federal minimum wage in both years was sufficiently high to deter training among teenagers.

formal and informal training.

Of course, this simple comparison of means masks a number of other possible sources of variation in training that may generate a spurious negative relationship with minimum wages. For example, in principle, at least, the states with a relatively high minimum wage may have had persistently high minimum wages in the past and (for some unrelated reason) a persistently lower incidence of training. In this case, we would not want to draw any causal inference from the relationship between training reported in 1991 and minimum wages over the last few years. As we showed in Table 1, however, virtually no states had minimum wages exceeding the federal level between 1983 and 1987, so that our minimum wage gap variable essentially does capture state-level changes in the three years prior to the data we have on the incidence of training.

In addition, a simple comparison between states that did and did not increase their minimum wages takes no account of differences in training across states that may arise as a result of technological change, economic conditions, government policy, etc., and which may also be correlated with minimum wage increases. For example, if firms in states in which market wages are relatively higher, perhaps due to a higher incidence of training, are less constrained by an increase in the minimum wage, then a simple comparison of means would generate a bias against finding that minimum wages reduce training. Alternatively, if those states with relatively less training in every year were those in which minimum wages rose relatively more, the bias would be in the other direction. To address this problem, we need to identify a control sample for which the minimum wage in 1991 (and the immediately preceding years) should not have an influence on the incidence of training. We could then use the differences between the minimum wage-training relationship in our treatment and control samples to identify the effects of minimum wages.

In particular, we recast the simple comparisons shown in Table 2 as a regression of the

form:

(1) $T_{ij} = \alpha + \beta I_j + \epsilon_{ij}$,

where T_{ij} is the training measure (a dummy variable) for individual i in state j in 1991, and I_j is the dummy variable for states with increases in the minimum wage exceeding the federal increase. Estimated as a linear probability model, this regression model would give us precisely the same estimate as the comparison between the two subsamples in Table 2. Of course, once we get beyond a simple comparison of means, we can also substitute our minimum wage variable (denoted MW_j) for I_j.

We then construct a difference-in-difference estimator using two alternative control samples. The first comprises workers aged 35-54. Because this group has higher average wage levels and because any training they did receive was likely in the more distant past, the incidence of training they report is likely to be associated with longer-run state-specific differences in training levels rather than with recent cross-state variation in minimum wage increases. Specifically, we estimate the regression:

(2) $T_{ij} = \alpha + S_j\beta + \gamma Y_{ij} + \delta MW_j \cdot Y_{ij} + \epsilon_{ij}$,

where 35-54 year-olds are now added to the sample, Y_{ij} is a dummy variable indicating that the individual is in the younger age group (either 16-24, 16-19, or 20-24, depending on the specification), and S_j is a vector of state dummy variables.[20] In this specification, the vector of coefficients β captures the cross-state variation in training common to the workers in all age groups, while γ picks up the average difference in training between the age groups. Finally, δ picks up the differences in the incidence of training between younger and older workers associated

[20]Because the data for this specification are for a single year, the state dummy variables (S_j) pick up the state-level variation in the minimum wage variable (MW_j).

with variation in the state minimum wage gap; this is interpreted as the causal effect of the minimum wage.[21]

As a second control sample, we use respondents to the 1983 CPS training supplement who are in the same younger age groups as our 1991 treatment sample. Unlike in equation (2), once we introduce the data from 1983, the minimum wage variable is not the same for all observations in a state (i.e., it is not the same for 1983 and 1991). Thus, we can simply introduce this variable along with state dummy variables to identify the minimum wage effects.[22] In this case, we estimate the following regression using the data for 1983 and 1991:

$$(3) \qquad T_{ijt} = \alpha + S_j\beta + \gamma Z_t + \delta MW_{jt} + \epsilon_{ijt} \ ,$$

where Z_t is a dummy variable indicating that the observation comes from 1991. In this regression, β again captures the cross-state variation in training, while γ now picks up the average difference in training between 1983 and 1991. Finally, δ captures the extent to which training has changed more (or less) in states with larger minimum wage increases, which again is interpreted as the causal effect of the minimum wage.

These alternative control samples each have their own advantages and disadvantages. The advantage of using the 35-54 year-olds in 1991 is that, because the control sample comes from the same year as the treatment sample, we capture state effects even if they vary over time. The

[21]Note that in contrast to specifications for the employment effects of minimum wages (e.g., Neumark and Wascher, 1992), we do not use a minimum wage variable defined relative to an average wage. In employment studies, the relevant factor is the relative price of unskilled to more-skilled labor, leading naturally to specifying the minimum wage variable relative to an average for all workers. In studying training, however, we are most interested in the minimum wage relative to what the market wage for young, unskilled workers would be in the absence of the minimum. This is unobserved, and the observed average for these workers clearly will be affected by the minimum wage. Thus, we specify our models in terms of the difference between the state minimum wage and the federal minimum wage, and allow the state and year dummy variables to capture variation in market wages.

[22]Because there was essentially no variation in minimum wage increases across states in 1983, the earlier data control for state differences in training due to other sources.

disadvantage is that if the incidence of training among 16-24 year-olds is affected by minimum wages, then the incidence of training among 35-54 year-olds may be affected indirectly. For example, employers training fewer 16-24 year-olds because of higher minimum wages may substitute towards training 35-54 year-olds if they need to increase workforce skills. Alternatively, if higher skills among older and younger workers are complementary in production, training could fall for the older group. Although the direction of bias is therefore ambiguous, the point is that in neither of these scenarios is the older group a valid control sample. Using the 16-24 year-olds from 1983 essentially reverses these advantages and disadvantages. On the one hand, because the data are from eight years earlier, the control sample is much more plausibly unaffected by the treatment. On the other hand, using the earlier sample may be inadequate for controlling for state variation in training, since the unmeasured state effects may not be invariant over this time period. As a consequence, we report results using both control samples to explore the robustness of our estimates. Our confidence in the results is bolstered by the fact that both control samples yield similar conclusions.

In addition to the problems addressed by the alternative control samples, training may also vary with individual characteristics. We thus include in the regression models a vector of individual-level controls for race, sex, schooling, age, and marital status.[23] We estimate each model as a linear probability model, with heteroscedasticity-consistent standard errors.[24]

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[23]Because the industry and occupation in which young individuals find work may be related to the ease with which they can be trained, we do not include them as control variables here; however, the results were little changed when these variables were added.

[24]The results were very similar using probit models. There were, however, two exceptions, for formal and in-school training for teenagers using older workers as a control group, in which we obtained large positive estimates with much higher standard errors (and hence insignificant coefficient estimates). Because this was true even for specifications without control variables, and because the estimates from these latter specifications were much different from what was implied by the means, we attribute these exceptions to problems with the distributional assumption. We therefore chose to present the results

In both specifications, there is also the potential for bias associated with a positive correlation between training and unobserved ability or productivity. In the literature, this correlation is apparent in comparisons of cross-sectional and longitudinal estimates of the returns to training (Lynch, 1992; Bartel, 1992), and in comparisons of cross-sectional estimates of the returns to training with and without controls for ability (Gardecki and Neumark, 1998). By pricing the lowest-ability workers out of the labor market, minimum wage increases may lead employers to hire relatively more higher-ability workers. If higher-ability workers are more likely to get trained, then variation in the average unobserved productivity or quality of employed young workers across states may generate a positive bias in estimates of the effects of minimum wage increases on training. However, this bias would tend to weaken any negative effect of minimum wages on training to improve skills on the current job, and would strengthen any positive effect of minimum wages on training to qualify for the current job. Given that we find evidence of negative effects of minimum wages on training to improve skills and no evidence of positive effects on training to qualify, the existence of this bias only strengthens our conclusions.

The results from the two alternative difference-in-difference regressions are reported in Table 3. Focusing first on the estimates using older workers as a control sample (column (1) in the top panel), we find that minimum wages reduce the incidence of on-the-job training among 16-24 year-olds, with the estimated effect significant at the ten-percent level. The point estimate for 16-19 year-olds is also negative, but not significant, while the point estimate for 20-24 year-olds is negative (-.199) and significant at the ten-percent level. This coefficient estimate indicates that a

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from the linear probability model, which provides consistent estimates of the conditional mean of the dependent variable without strong distributional assumptions. The linear probability and probit estimates were very similar for all other types of training, and for all of the specifications using younger workers in 1983 as the control sample.

ten-percent higher minimum wage reduces the proportion receiving training by two percentage points. Since 30.4 percent of this age group reports receiving any training (Table 2), this estimate implies an elasticity of about -.65, indicative of a large deterrent effect from minimum wages. However, when the individual-level control variables are included (column (1')), the estimated effect becomes insignificant for all age groups, with t-statistics around one for 16-24 and 20-24 year-olds.

The results for separate estimates of the effects of minimum wages on formal and informal training are reported in columns (2)-(3'). For informal training, we find small and insignificant effects of minimum wages for all age groups. For formal training, however, the effects on the 16-24 year-old age group are negative and significant or nearly so at the ten-percent level, and the negative effects on 20-24 year-olds are negative and significant at the five-percent level; the estimated magnitudes of the effect for 20-24 year-olds imply that a ten-percent rise in the minimum wage reduces the incidence of formal training by 1.5 to 1.8 percentage points. Given that about 10 percent of workers in this age group report formal training, the implied elasticities are as high as -1.8.

The bottom panel reports results using young workers from 1983 as the control sample. These estimates provide even stronger evidence that minimum wages deter on-the-job training. In column (1), where we look at overall training, the effects on training for 16-24 and 20-24 year-olds are negative and significant at the five- to ten-percent level, whether or not we include the individual-level controls. In this case, there is still no evidence that minimum wages deter informal training, or that minimum wages have significant adverse effects for teenagers. On the other hand, the evidence that minimum wages deter formal training among 16-24 or 20-24 year-olds is stronger, with the estimated effects negative and significant at the five-percent level whether or not

the individual-level controls are included.[25]

Taken as a whole, the evidence supports the hypothesis that minimum wages reduce the incidence of training aimed at improving skills on the current job, as theory suggests. The effects are strongest for formal training, which would be expected if formal training entails higher direct and indirect costs, while informal training is a joint product with output. Because the descriptive statistics in Table 2 indicate that formal training is less prevalent for young workers than is informal training, these results also indicate that an important component of training does not appear to be reduced by minimum wages. However, the wage regression estimates reported in Appendix Table A1 suggest that the returns to the incidence of formal training are much higher (possibly because of greater intensity or duration), so the consequences of reduced formal training may be much more severe.

The evidence also indicates that the reductions in training associated with higher minimum wages are most severe among 20-24 year-olds; indeed, we find little evidence that minimum wages reduce the incidence of training among 16-19 year-olds. This presumably reflects the near absence of formal training among the younger group in the first place, so that there is little scope for minimum wages to have much impact, despite the lower wages earned, on average, by teens. In addition, the training that teenagers do receive appears to be low-cost, and thus a higher minimum wage may not be much of a constraint. For example, among teenagers who reported receiving formal training in the 1991 CPS, 58 percent reported receiving one week of training or less, 28.7 percent reported two to 12 weeks, and 13.5 percent reported 13 or more weeks. In contrast, the

[25]Given that theory makes an unambiguous prediction about training to improve skills on the current job, one could argue that our hypothesis tests should be one-sided. In that case, all of the estimates reported in the above paragraphs as significant at the ten-percent level would instead be significant at the five-percent level.

corresponding percentages for 20-24 year-olds were 41.8, 42.3, and 16.0. Thus, training for 20-24 year-olds is both more prevalent and more lengthy. As we suggested earlier, if the impact of minimum wages depends on the amount of training these workers would otherwise receive and its cost, it will not necessarily be the lowest-wage workers whose training is most adversely affected by minimum wage increases, and thus it should not be surprising that the negative effects of minimum wages on training are strongest among 20-24 year-olds.[26]

Training to Obtain the Current Job

While theory predicts that minimum wages will reduce training to improve skills on the current job, the possibility exists that this effect will be offset by an increase in training to qualify for a job. Tables 4 and 5 lay out exactly the same analyses of training to obtain the current job as were done for training to improve skills in Tables 2 and 3. The only difference is that in-school training is also considered. Given the similarities of the analyses, the results can be summarized briefly.

The descriptive statistics in Table 4 indicate that the incidence of this type of training is somewhat higher than the incidence of training on the current job. Although the incidence of formal training is similar here to that in Table 2, there is both more informal training and more in-school training (which was very rare for training to improve skills and hence was not reported in that table). Simple difference estimates based on the lower panels of Table 4 are suggestive of

[26]We estimated versions of equations (2) and (3) allowing the effects of minimum wages on training to vary with the current wage relative to the minimum wage. In general, the estimated interactions in specifications corresponding to those reported in Table 3 (and Table 5 below) were small and insignificant, and not consistently of one sign. However, consistent with the point that minimum wages need not necessarily reduce training the most among the lowest-wage workers, we did find some statistically significant evidence indicating that minimum wages reduce the incidence of informal training among higher-wage teenagers. In contrast, the effects on formal training were small and insignificant regardless of the wage. Of course, wages and training of young individuals are jointly endogenous, so interpreting such regressions is problematic.

positive effects of minimum wages on this type of training, as the incidence of training is higher in states with a positive minimum wage gap for any training (all age groups), formal training (16-24 and 16-19 year-olds) and informal and in-school training (all age groups). However, in the difference-in-difference estimates reported in Table 5, the evidence for positive effects evaporates. The only consistently positive coefficient estimates on the minimum wage variable are for 16-19 year-olds, and none of these are even marginally significant.[27] Moreover, for 16-24 year-olds as a whole and 20-24 year-olds separately, the coefficient estimates are nearly always negative, and they are significant at the five- or ten-percent level for some types of training when older workers in 1991 are used as the control sample.

Thus, there is no evidence that minimum wages increase the incidence of training to qualify for the current job, and, if anything, minimum wages may reduce the incidence of such training. Consequently, it appears that the principal effect of minimum wages on training is to reduce training received by young workers.

Robustness and Sensitivity Analysis

In this subsection, we report on some additional analyses that we conducted to explore the robustness and sensitivity of the estimated relationships between minimum wages and training. First, as we noted earlier, we have used a three-year window to define the percentage gap between the state minimum wage and the federal minimum wage. This is a somewhat arbitrary choice, and there are arguments for using both shorter and longer windows. For example, because mean current job tenure is closer to one year than to three years (especially for teenagers), a shorter window might be desirable for the analysis of training to improve skills on the current job.[28]

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[27]Because the theoretical prediction is ambiguous, two-sided tests are appropriate in this case.

[28]In the 1991 CPS supplement, mean tenure is 1.34 years for 16-24 year-olds, .67 for 16-19 year-olds, 1.62 for 20-24 year-olds, and 8.84 for 35-54 year-olds.

Alternatively, for the analysis of training to obtain skills, a longer window spanning multiple jobs may be more relevant, especially for the 20-24 year-olds. In addition, because employers may not make year-to-year changes in the provision of training, a longer-run view of the level of minimum wages in a state may be most pertinent to how much training young workers receive.

To indicate the robustness of the results to using different windows, Table 6 presents difference-in-difference estimates corresponding to those in the earlier tables for windows of five, four, two, and one years.[29] Only specifications including the demographic controls are reported, so these estimates are most comparable to those in the columns with a prime superscript in Tables 3 and 5. Focusing first on the results for training to improve skills on the current job, the estimates of the minimum wage effect are very robust to using different windows to define the minimum wage variable. Regardless of the control sample used, the estimated effects are always negative for any training, formal training, and informal training among 16-24 and 20-24 year-olds, and for any training and informal training among teenagers. The estimated coefficients for informal training are not significant, whereas the estimated coefficients for any training and formal training are often significant at the five- or ten-percent level for 16-24 and 20-24 year-olds. In general, the negative effects of the minimum wage are strongest and most significant for 20-24 year-olds, for formal training, and when using workers of the same age in 1983 as the control sample. The findings for training to obtain the current job are similarly robust. Thus, the results in Table 6 show that our conclusions are not sensitive to the precise window used to define the level of the state minimum wage relative to the federal minimum wage, and confirm our finding that the principal effect of minimum wages on training is to reduce training to improve skills on the current job, with no

[29]When we use a window of one year, we are simply using the percentage gap between the minimum wage prevailing in January 1991 and the federal minimum wage.

offsetting benefits in the form of additional training to qualify for the current job.[30]

In Table 7, we examine whether our estimated minimum wage effects might reflect spurious correlations between minimum wages and the incidence of training. In particular, although we have used young workers in 1983 and older workers in 1991 as control samples to compare with the treatment sample of young workers in 1991, it is possible that reported training fell relatively more for both young and older workers in states in which minimum wages rose. Given our stated reasons for choosing older workers as a control sample in the first place, we would tend to interpret such evidence as indicative of a spurious relationship between minimum wages and training, because minimum wages are expected to have little effect on training reported by older workers.[31] To address this concern, Table 7 reports estimates of equation (3) for workers aged 35-54. In this estimation, we use 35-54 year-olds in 1991 as the treatment sample, and 35-54 year-olds in 1983 as the control sample. We report results with windows of one through five years for defining the minimum wage variable.

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[30]We also attempted to use information on the duration of training to verify whether the evidence of negative effects on the incidence of training to improve skills carried over to the data on length of training. The 1991 survey includes some very broad measures of the duration of training, although they do not measure its intensity (i.e., hours per day). Specifically, data are available for formal training to improve skills on the current job, and for formal and school training to obtain the job, in intervals of: no training; one week or less; two-12 weeks; 13-25 weeks; or 26+ weeks. For 1983 there are no data on the duration of formal training to improve skills on the current job, but there are data on duration of formal and school training to obtain the current job. Because we were most interested in exploring the robustness of our findings on training to improve skills on the current job, we restricted attention to the 1991 data, using the older workers as a control sample. We estimated models similar to those described above, but exploiting the information on duration (using multinomial logit and ordered logit models).

Qualitatively, the evidence on duration pointed in the same direction as that for the incidence of training, with the probability of longer training spells to improve skills on the current job lower in states that had raised their minimum relatively more; however, the estimates were generally not statistically significant, probably due to the crude measurement of training with these data. We also found no evidence of positive effects of minimum wages on the duration of training to qualify for the job. Thus, although the duration data are noisier, they are consistent with the main findings we report.

[31]Alternatively, as suggested earlier, minimum wages could deter training among younger workers but increase training among older workers, which would lead to an overly strong estimate of the effect of minimum wages on training received by younger workers.

A failure to find any evidence of minimum wage effects in the 35-54 year-old treatment sample would bolster our confidence that our findings for young workers reflect causal effects of minimum wages, and this is exactly what Table 7 shows. For training to obtain the current job, we found little evidence of minimum wage effects for young workers, and we find little evidence of any effect here. Of course, the more important analysis is for training to improve skills on the current job, for which we found negative and significant effects of minimum wages for young workers. As can be seen in the first three columns of Table 7, however, all of the point estimates are close to zero and insignificant for the older workers. Thus, there is no indication of a negative relationship between minimum wages and training for older workers that would call into question a causal interpretation of the negative relationship we found for younger workers.

V. Conclusion

Theory predicts that minimum wages will reduce on-the-job training intended to improve skills on the current job, but may increase training to qualify for a job. If the former effect is larger, the influence of minimum wages on training may represent an additional cost of minimum wage increases that is not captured by traditional estimates of the disemployment effects, because reductions in on-the-job training potentially affect a greater number of persons. If the latter effect dominates, then part of the costs of minimum wages may be offset by an increase in human capital accumulation associated with individuals raising their skills sufficiently to compete for minimum wage jobs. Either way, understanding the effects of minimum wages on on-the-job training is essential in evaluating the wisdom of minimum wage increases. Surprisingly, there is very little evidence on this question, except with respect to the effects of minimum wages on schooling.

We estimate the effects of minimum wages on the incidence of training among young workers, focusing both on on-the-job training used to improve skills on the current job, and on

training that helped workers obtain or qualify for their current job. We exploit cross-state variation in minimum wage increases to assess the effects of minimum wages on the training received by young workers, using either older workers contemporaneously, or young workers from an earlier period, as a control sample.

The evidence provides considerable support for the hypothesis that minimum wages reduce training aimed at improving skills on the current job, especially formal training. For young workers in their early 20's, the estimated effects indicate elasticities of the incidence of formal training with respect to the minimum wage ranging from about -1 to -2, implying sizable deleterious effects of minimum wages. Moreover, there is no evidence that minimum wages raise the amount of training obtained by workers to qualify for their current job, and, indeed, there is some evidence that minimum wages reduce this type of training as well. Consequently, it appears that the principal effect of minimum wages on training is to substantially reduce formal training to improve skills on the current job. Among other implications, this evidence undermines the case for using minimum wages to encourage a "high-wage" path for the economy.

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Table 1 - Effective Minimum Wage by State and the Average Percentage Difference Between State and Federal Minimums Over the Past Three Years

	1983	1984	1985	1986	1987	1988	1989	1990	1991	Gap (%)
AK	3.85	3.85	3.85	3.85	3.85	3.85	3.85	3.85	4.20	13.46
AL	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
AR	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
AZ	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
CA	3.35	3.35	3.35	3.35	3.35	3.35	4.25	4.25	4.25	21.86
CO	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
CT	3.37	3.37	3.37	3.37	3.37	3.75	4.25	4.25	4.25	21.86
DE	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
FL	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
GA	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
HI	3.35	3.35	3.35	3.35	3.35	3.85	3.85	3.85	3.85	10.39
IA	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.85	4.25	8.92
ID	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
IL	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
IN	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
KS	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
KY	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
LA	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
MA	3.35	3.35	3.35	3.35	3.55	3.65	3.75	3.75	3.80	7.96
MD	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
ME	3.35	3.35	3.45	3.55	3.65	3.65	3.75	3.85	3.85	9.39
MI	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
MN	3.35	3.35	3.35	3.35	3.35	3.55	3.85	3.95	4.25	14.89
MO	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
MS	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
MT	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
NC	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
ND	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
NE	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
NH	3.35	3.35	3.35	3.35	3.45	3.55	3.65	3.75	3.85	7.40
NJ	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
NM	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
NV	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
NY	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
OH	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
OK	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
OR	3.35	3.35	3.35	3.35	3.35	3.35	3.35	4.25	4.75	17.29
PA	3.35	3.35	3.35	3.35	3.35	3.35	3.70	3.70	3.80	6.97
RI	3.35	3.35	3.35	3.35	3.55	3.65	4.00	4.25	4.25	19.37
SC	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
SD	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
TN	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
TX	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
UT	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
VA	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
VT	3.35	3.35	3.35	3.35	3.45	3.55	3.65	3.75	3.95	8.28
WA	3.35	3.35	3.35	3.35	3.35	3.35	3.85	4.25	4.25	17.88
WI	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.65	3.80	2.99
WV	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
WY	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	0
Federal	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	

Note: The effective minimum wage is the state's own legislated minimum unless the federal minimum is greater, in which case the federal minimum wage becomes the state's effective minimum wage. The gap column indicates the average percentage by which a state's effective minimum wage exceeds the federal minimum wage. For the 1979-1983 period, the federal minimum was $2.90 in 1979, $3.10 in 1980, and then fixed at $3.35 beginning in 1981. (All of these minimum wages were effective as of January 1 of each year.) Of the fifty states, only Alaska and Connecticut had higher minimum wages. Alaska's was $.50 higher than the federal minimum in each year, while Connecticut's was one to two cents higher in each year.

Table 2 - Descriptive Statistics for Training to Improve Skills on Current Job, 1991 CPS

	Any training to improve skills on current job (1)	Formal (2)	Informal (3)	N (4)
Overall proportions:				
Ages 16-24	0.2687	0.0780	0.1448	6745
Ages 16-19	0.1829	0.0253	0.1315	2057
Ages 20-24	0.3044	0.0999	0.1504	4688
Ages 35-54	0.4768	0.1941	0.1675	22941
Proportions by average minimum wage levels in last three years:				
state minimum = federal minimum (35 states)				
Ages 16-24	0.2746	0.0811	0.1468	4757
Ages 16-19	0.1815	0.0212	0.1315	1457
Ages 20-24	0.3131	0.1058	0.1531	3300
state minimum > federal minimum (15 states)				
Ages 16-24	0.2557	0.0712	0.1404	1988
Ages 16-19	0.1860	0.0342	0.1313	600
Ages 20-24	0.2853	0.0868	0.1442	1388

Table 3 – Difference-in-Difference Estimates of Minimum Wage Effects on Training to Improve Skills on Current Job, 1983 and 1991 CPS

	Any training to improve skills on current job		Formal		Informal	
	(1)	(1')	(2)	(2')	(3)	(3')
Older workers in 1991 as control sample:						
Ages 16-24	-0.1653	-0.0879	-0.1193	-0.0960	-0.0398	-0.0295
	(0.0957)	(0.0941)	(0.0630)	(0.0625)	(0.0769)	(0.0768)
Ages 16-19	-0.1073	-0.0608	-0.0174	0.0221	-0.0479	-0.0435
	(0.1446)	(0.1461)	(0.0774)	(0.0782)	(0.1262)	(0.1263)
Ages 20-24	-0.1988	-0.0999	-0.1785	-0.1470	-0.0397	-0.0266
	(0.1112)	(0.1098)	(0.0736)	(0.0730)	(0.0884)	(0.0884)
Workers of same age in 1983 as control sample:						
Ages 16-24	-0.2269	-0.2076	-0.1276	-0.1189	-0.0473	-0.0477
	(0.1022)	(0.1006)	(0.0587)	(0.0580)	(0.0833)	(0.0833)
Ages 16-19	-0.1338	-0.1239	0.0502	0.0530	-0.1067	-0.1067
	(0.1675)	(0.1675)	(0.0750)	(0.0754)	(0.1483)	(0.1485)
Ages 20-24	-0.2700	-0.2392	-0.1986	-0.1820	-0.0270	-0.0278
	(0.1254)	(0.1237)	(0.0761)	(0.0752)	(0.1001)	(0.1002)
Demographic controls	No	Yes	No	Yes	No	Yes

Note: Estimated coefficients from linear probability models are reported. Heteroscedasticity-consistent standard errors are reported in parentheses. The demographic control variables include race, gender, schooling, age (within-group), and marital status. The first panel reports estimates of equation (2) and the second estimates of equation (3).

Table 4 - Descriptive Statistics for Training to Obtain Current Job, 1991 CPS

	Any training to obtain current job (1)	Formal (2)	Informal (3)	School (4)	N (5)
Overall proportions:					
Ages 16-24	0.3945	0.0681	0.2081	0.1973	6745
Ages 16-19	0.2515	0.0359	0.1552	0.0818	2057
Ages 20-24	0.4540	0.0815	0.2302	0.2454	4688
Ages 35-54	0.6248	0.1354	0.2959	0.3714	22941
Proportions by average minimum wage levels in last three years:					
state minimum = federal minimum (35 states)					
Ages 16-24	0.3875	0.0673	0.2032	0.1961	4757
Ages 16-19	0.2377	0.0273	0.1443	0.0778	1457
Ages 20-24	0.4493	0.0838	0.2275	0.2450	3300
state minimum > federal minimum (15 states)					
Ages 16-24	0.4099	0.0698	0.2190	0.1999	1988
Ages 16-19	0.2816	0.0545	0.1787	0.0905	600
Ages 20-24	0.4642	0.0762	0.2361	0.2463	1388

Table 5 – Difference-in-Difference Estimates of Minimum Wage Effects on Training to Obtain Current Job, 1983 and 1991 CPS

	Any training to obtain current job		Formal		Informal		School	
	(1)	(1')	(2)	(2')	(3)	(3')	(4)	(4')
Older workers in 1991 as control sample:								
Ages 16-24	-0.1008	-0.0074	-0.0795	-0.0760	-0.1586	-0.1621	-0.1398	0.0036
	(0.1014)	(0.0982)	(0.0600)	(0.0598)	(0.0886)	(0.0885)	(0.0852)	(0.0819)
Ages 16-19	-0.0039	0.0442	0.0483	0.0370	-0.1049	-0.1184	0.0619	0.1557
	(0.1604)	(0.1585)	(0.0833)	(0.0848)	(0.1359)	(0.1375)	(0.1161)	(0.1172)
Ages 20-24	-0.1571	-0.0388	-0.1344	-0.1267	-0.1881	-0.1931	-0.2343	-0.0547
	(0.1171)	(0.1148)	(0.0698)	(0.0693)	(0.1027)	(0.1025)	(0.0999)	(0.0974)
Workers of same age in 1983 as control sample:								
Ages 16-24	-0.0879	0.0166	-0.0520	-0.0448	-0.0768	-0.0715	-0.0326	0.0379
	(0.0978)	(0.1080)	(0.0588)	(0.0586)	(0.0950)	(0.0946)	(0.0884)	(0.0842)
Ages 16-19	0.0906	0.1371	-0.0056	0.0048	0.0852	0.1036	0.1343	0.1681
	(0.1909)	(0.1875)	(0.0929)	(0.0925)	(0.1576)	(0.1571)	(0.1258)	(0.1238)
Ages 20-24	-0.0867	-0.0114	-0.0720	-0.0619	-0.1482	-0.1424	-0.0952	0.0049
	(0.1356)	(0.1326)	(0.0735)	(0.0731)	(0.1167)	(0.1167)	(0.1117)	(0.1067)
Demographic controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: Estimated coefficients from linear probability models are reported. Heteroscedasticity-consistent standard errors are reported in parentheses. The demographic control variables include race, gender, schooling, age (within-group), and marital status. The first panel reports estimates of equation (2) and the second estimates of equation (3).

Table 6 – Alternative Difference-in-Difference Estimates of Minimum Wage Effects, 1983 and 1991 CPS, Based on Cumulative Minimum Wage Changes Over the Past Five Years, Four Years, Two Years, and One Year

	Training to improve skills on current job			Training received to obtain current job			
	Any	Formal	Informal	Any	Formal	Informal	In-school
For minimum wage changes over the past five years:							
Older workers in 1991 as control sample:							
Ages 16-24	-0.1465	-0.1602	-0.0393	-0.0064	-0.1197	-0.2283	0.0191
	(0.1477)	(0.0979)	(0.1208)	(0.1542)	(0.0940)	(0.1386)	(0.1289)
Ages 16-19	-0.0760	0.0246	-0.0257	0.0719	0.0618	-0.1691	0.2512
	(0.2299)	(0.1210)	(0.2007)	(0.2469)	(0.1333)	(0.2132)	(0.1853)
Ages 20-24	-0.1762	-0.2407	-0.0491	-0.0536	-0.2019	-0.2729	-0.0712
	(0.1728)	(0.1150)	(0.1388)	(0.1810)	(0.1097)	(0.1614)	(0.1537)
Workers of same age in 1983 as control sample:							
Ages 16-24	-0.3093	-0.1931	-0.0578	0.0399	-0.0591	-0.0713	0.0724
	(0.1587)	(0.0913)	(0.1314)	(0.1715)	(0.0924)	(0.1490)	(0.1339)
Ages 16-19	-0.1608	0.0846	-0.1316	0.2663	0.0219	0.1977	0.3007
	(0.2632)	(0.1160)	(0.2347)	(0.2933)	(0.1449)	(0.2448)	(0.1961)
Ages 20-24	-0.3654	-0.2958	-0.0340	-0.0231	-0.0879	-0.1837	0.0076
	(0.1956)	(0.1191)	(0.1580)	(0.2102)	(0.1158)	(0.1846)	(0.1703)
For minimum wage changes over the past four years:							
Older workers in 1991 as control sample:							
Ages 16-24	-0.1155	-0.1305	-0.0306	-0.0074	-0.0970	-0.1931	-0.0154
	(0.1204)	(0.0798)	(0.0984)	(0.1257)	(0.0766)	(0.1129)	(0.1050)
Ages 16-19	-0.0621	0.0210	-0.0276	0.0648	0.0530	-0.1380	0.2134
	(0.1875)	(0.0990)	(0.1636)	(0.2016)	(0.1090)	(0.1741)	(0.1514)
Ages 20-24	-0.1381	-0.1964	-0.0351	-0.0493	-0.1647	-0.2320	-0.0616
	(0.1407)	(0.0935)	(0.1130)	(0.1474)	(0.0892)	(0.1313)	(0.1251)
Workers of same age in 1983 as control sample:							
Ages 16-24	-0.2578	-0.1553	-0.0501	0.0235	-0.0499	-0.0715	0.0526
	(0.1289)	(0.0741)	(0.1068)	(0.1393)	(0.0750)	(0.1210)	(0.1087)
Ages 16-19	-0.1361	0.0709	-0.1141	0.2116	0.0176	0.1531	0.2437
	(0.2142)	(0.0947)	(0.1909)	(0.2387)	(0.1182)	(0.1992)	(0.1598)
Ages 20-24	-0.3041	-0.2392	-0.0291	-0.0284	-0.0741	-0.1647	-0.0011
	(0.1588)	(0.0966)	(0.1283)	(0.1706)	(0.0939)	(0.1497)	(0.1381)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Table continues on next page.

(Table 6 continued)

	Training to improve skills on current job			Training received to obtain current job			
	Any	Formal	Informal	Any	Formal	Informal	In-school
For minimum wage changes over the past two years:							
Older workers in 1991 as control sample:							
Ages 16-24	-0.1342	-0.0943	-0.0477	-0.0058	-0.0771	-0.1630	-0.0022
	(0.0975)	(0.0650)	(0.0792)	(0.1022)	(0.0616)	(0.0921)	(0.0851)
Ages 16-19	-0.0932	0.0174	-0.0423	0.0720	0.0450	-0.0992	0.1344
	(0.1505)	(0.0791)	(0.1303)	(0.1646)	(0.0879)	(0.1432)	(0.1181)
Ages 20-24	-0.1500	-0.1420	-0.0518	-0.0437	-0.1304	-0.1998	-0.0535
	(0.1142)	(0.0765)	(0.0910)	(0.1196)	(0.0714)	(0.1068)	(0.1021)
Workers of same age in 1983 as control sample:							
Ages 16-24	-0.2378	-0.1269	-0.0669	0.0441	-0.0353	-0.0487	0.0367
	(0.1046)	(0.0610)	(0.0863)	(0.1130)	(0.0600)	(0.0984)	(0.0879)
Ages 16-19	-0.1409	0.0445	-0.1087	0.1874	0.0096	0.1566	0.1549
	(0.1727)	(0.0767)	(0.1533)	(0.1942)	(0.0951)	(0.1628)	(0.1255)
Ages 20-24	-0.2753	-0.1897	-0.0543	0.0037	-0.0503	-0.1314	-0.0089
	(0.1291)	(0.0795)	(0.1040)	(0.1381)	(0.0749)	(0.1216)	(0.1121)
For minimum wage changes over the past year:							
Older workers in 1991 as control sample:							
Ages 16-24	-0.2241	-0.1221	-0.0762	-0.0435	-0.1228	-0.2569	-0.0317
	(0.1364)	(0.0918)	(0.1093)	(0.1438)	(0.0844)	(0.1287)	(0.0819)
Ages 16-19	-0.1802	-0.0119	-0.0445	0.0963	0.0348	-0.1655	0.1951
	(0.2095)	(0.1064)	(0.1825)	(0.2311)	(0.1251)	(0.2000)	(0.1597)
Ages 20-24	-0.2404	-0.1713	-0.0908	-0.1081	-0.1920	-0.3088	-0.1174
	(0.1600)	(0.1094)	(0.1248)	(0.1688)	(0.0970)	(0.1494)	(0.1458)
Workers of same age in 1983 as control sample:							
Ages 16-24	-0.3468	-0.1431	-0.0979	0.0546	-0.0311	-0.0665	-0.0068
	(0.1479)	(0.0877)	(0.1207)	(0.1599)	(0.0819)	(0.1382)	(0.1254)
Ages 16-19	-0.2059	0.0695	-0.1058	0.2505	-0.0102	0.2370	0.1641
	(0.2415)	(0.1024)	(0.2153)	(0.2727)	(0.1351)	(0.2263)	(0.1717)
Ages 20-24	-0.4029	-0.2239	-0.0983	-0.0012	-0.0363	-0.1883	-0.0575
	(0.1828)	(0.1155)	(0.1454)	(0.1957)	(0.1010)	(0.1710)	(0.1612)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimated coefficients from linear probability models are reported. Heteroscedasticity-consistent standard errors are reported in parentheses. The demographic control variables include race, gender, schooling, age (within-group), and marital status.

Table 7 - Difference-in-Difference Estimates of Minimum Wage Effects on Training for Older Workers (Ages 35-54), Using Workers of the Same Age in 1983 as the Control Sample

	Training to improve skills on current job			Training received to obtain current job			
For minimum wage changes over the past:	Any	Formal	Informal	Any	Formal	Informal	In-school
One year	0.0102	0.0096	0.1052	-0.0187	0.0881	-0.0347	-0.0763
	(0.0970)	(0.0776)	(0.0747)	(0.0914)	(0.0701)	(0.0964)	(0.0853)
Two years	-0.0136	-0.0020	0.0499	-0.0446	0.0652	-0.0408	-0.0625
	(0.0864)	(0.0547)	(0.0533)	(0.0640)	(0.0493)	(0.0679)	(0.0602)
Three years	-0.0205	0.0130	-0.0321	-0.0485	0.0725	-0.0431	-0.0598
	(0.0659)	(0.0524)	(0.0517)	(0.0861)	(0.0474)	(0.0654)	(0.0580)
Four years	-0.0181	0.0239	-0.0444	-0.0595	0.1016	0.0477	-0.0827
	(0.0841)	(0.0670)	(0.0657)	(0.0784)	(0.0603)	(0.0832)	(0.0740)
Five years	-0.0086	0.0316	0.0614	-0.0612	0.1321	-0.0466	-0.0990
	(0.1034)	(0.0823)	(0.0808)	(0.0966)	(0.0741)	(0.1023)	(0.0910)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimated coefficients from linear probability models are reported. Heteroscedasticity-consistent standard errors are reported in parentheses. The demographic control variables include race, gender, schooling, age (within-group), and marital status.

Appendix Table A1 – Log Wage Regressions for 16-24 Year Olds, 1991 CPS

	Any training to improve skills on the current job		Formal or informal training to improve skills on the current job		Any training that was needed to obtain the current job		Formal or informal training that was needed to obtain the current job	
	(1)	(1')	(2)	(2')	(3)	(3')	(4)	(4')
Any training to improve skills on the current job	0.1669	0.1234	...	...	...	...	...	...
	(0.0285)	(0.0249)						
Formal training to improve skills on the current job	...	...	0.2461	0.1760	...	...	...	...
			(0.0481)	(0.0436)				
Informal training to improve skills on the current job	...	...	0.0465	0.0435	...	...	...	...
			(0.0354)	(0.0302)				
Any training received to obtain current job	...	...	...	...	0.2028	0.1407	...	...
					(0.0252)	(0.0224)		
Formal training received to obtain current job	...	...	...	...	...	...	0.1686	0.1192
							(0.0538)	(0.0491)
Informal training received to obtain current job	...	...	...	...	...	...	0.0900	0.0609
							(0.0337)	(0.0303)
Age	...	0.0691	...	0.0845	...	0.0977	...	0.0726
		(0.0729)		(0.0728)		(0.0722)		(0.0781)
Age-squared	...	-0.0003	...	-0.0006	...	-0.0010	...	-0.0003
		(0.0018)		(0.0018)		(0.0018)		(0.0018)
Male	...	0.0945	...	0.0937	...	0.0906	...	0.0874
		(0.0200)		(0.0200)		(0.0198)		(0.0201)
White	...	0.0607	...	0.0541	...	0.0550	...	0.0533
		(0.0298)		(0.0299)		(0.0280)		(0.0293)
Married	...	0.0242	...	0.0210	...	0.0218	...	0.0234
		(0.0298)		(0.0301)		(0.0296)		(0.0309)
Highest grade completed	...	0.0200	...	0.0201	...	0.0159	...	0.0208
		(0.0071)		(0.0071)		(0.0071)		(0.0073)

Note: Estimated coefficients are reported for each independent variable included in a specification, with standard errors in parentheses.