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Minimum Wage Effects in the Post-welfare Reform Era

By David Neumark
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Overview

Minimum wage laws remain a subject of considerable debate at all levels of government despite years of research on their costs and benefits. At the national level, there have been frequent proposals in recent years to increase the federal minimum wage. Many states have followed suit, attempting (and sometimes succeeding) to raise their minimum wages above the federal level. At the present time, 21 states and the District of Columbia have minimum wages that exceed the federal wage floor, while 6 others recently passed ballot initiatives to raise theirs as well. Additionally, city-wide minimum wages have been enacted in a handful of cities, and living wages which typically set a higher minimum wage for a subset of workers in an area have spread to scores of other cities.

A major drawback of much of the existing minimum wage research is that it was performed using data that extends through the mid-1990s at the latest. Since then, the low-wage labor market has undergone substantial changes. Welfare reform, expansions of the federal Earned Income Tax Credit (EITC), and the growth of state EITCs have changed work incentives faced by the poor and thus the types of individuals competing for entry-level jobs. These reforms may have altered the effects of minimum wages as well. Therefore, evidence from earlier research is likely less applicable to the evaluation of recent or future increases in state and federal minimum wages.

In this study, Dr. David Neumark of the University of California at Irvine focuses on the effects of state-level minimum wages in the post-welfare reform era. Specifically, he estimates the effect of the minimum wage on employment levels, wages, and earnings of teenagers and young adults (aged 16-24) for a wide variety of demographic and skill groups over the 1997-2005 period. Additionally, he estimates the effects of other policy changes and investigates potential interaction effects. The author finds, consistent with earlier research, that the most negative minimum wage employment effects are felt by at-risk groups, such as the less-skilled and young minority males. He also finds that there may be positive minimum wage effects on the employment of young minority women aged 20-24 when combined with EITC policies. However, this benefit comes at a substantial cost to other groups. Among those who pay the highest costs are minority males and female high school dropouts. Minority males and high school dropouts often serve as "poster children" for increases in the minimum wage, yet experience the strongest disemployment effects as well as decreased earnings which are magnified by higher state EITC levels.

Welfare Reform and the EITC

In his analysis of the post-welfare reform era, the author considers EITC expansions and the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PROWRA) legislation that replaced the Aid to Families with Dependent Children (AFDC) program with Temporary Assistance to Needy Families (TANF). TANF made welfare funds available to states under the condition that they introduce policies designed to move recipients off welfare by encouraging self-sufficiency. Such policies have included specific legislation requiring welfare recipients to work, as well as limits on the number of months that families can receive welfare payments. Although the author specifically included these time limits and work requirements, he was unable to find any interaction between them and the minimum wage regarding their effect on employment.

Therefore, the analysis focuses primarily on the EITC. At the federal level, the EITC increased sharply during the 1990s, rising to a 40 percent earned income tax credit (with two children) in 1996, where it has remained since. Additionally, a number of states introduced their own EITC

programs, which typically specify a percentage supplement to the federal EITC. Currently, 20 states plus the District of Columbia have their own EITC programs. In most cases, state-level EITCs are refundable, generally fully so, but since the results did not differ based on refundability, this distinction is ignored.

Employment

Minimum wages have strongly negative effects on the employment of teenagers and minorities (African American, Hispanic, or both). The author finds that a 10% increase in the minimum wage will decrease minority employment by 3.9%, with the majority of the burden falling on minority teenagers (6.6%). Although the size of the disemployment effects for African Americans is quite large -2.8% (and even larger for African American teenagers, -8.4%), it is the statistically significant effect for Hispanics (-4.9%) that is driving these results. This supports earlier research which found that minimum wages have the largest negative effects on low-skilled employees, such as teens and minority teens.

Most of these negative results appear to be due to the impact that minimum wages have had on male employment in the post-welfare era. The author finds uniformly negative effects on males, particularly minorities. For every 10% increase in the minimum wage, African American or Hispanic males aged 16-24 and 20-24 experienced decreased employment of 6.3% and 5.5% respectively during this period. While minimum wages appear to have had less of a negative impact on women's employment, there is still very strong evidence of disemployment effects among the least-skilled (*i.e.,* high school dropouts). For these vulnerable individuals, a 10% increase in the minimum wage led to an 8% decrease in employment. Moreover, the relatively favorable results for females are tempered by evidence that for some women a high minimum wage coupled with an EITC reduces their employment prospects.

Wages and Earnings

For the most part, the author finds positive effects on wages from minimum wage hikes. This includes the wages of the least skilled, both male and female. The minimum wage has a particularly positive impact on the earnings of 20-24 year-old African American or Hispanic women, increasing their earnings by 8% for each 10% increase in the minimum wage. This is not surprising, since the wage effect for this group was one of the highest and the employment effect was small but positive. However, at the same time, the EITC reduces wages for 16-24 and 20-24 year-old minority men and women.

The evidence reveals policy effects on earnings that differ substantially across different groups. The EITC boosts minority women's earnings, and coupling the EITC with a higher minimum wage appears to enhance this positive effect. In contrast, higher minimum wages reduce the earnings of minority men, particularly when the EITC is high (for those aged 16-24, a 10% increase in the minimum wage coupled with a 25% state EITC supplement is associated with a 19.8% decrease in earnings). This policy combination also hurts female teenagers and 20-24 year-old high school dropouts.

Conclusion

In considering the post-welfare reform era, the author finds that the disemployment effects of minimum wages are concentrated on young minority men; for young white men, the estimated effects are negative but smaller and not statistically significant. For young women, in contrast, there is little evidence of minimum wage effects on employment, with the exception of high school dropouts.

The effect of mixing minimum wage policies with EITCs varies quite sharply between men and women. Higher minimum wages reduce the earnings of minority men, more so when the EITC is high. In contrast, the EITC boosts minority women's employment and earnings. With the negative effects of coupling minimum wage hikes with EITC policies concentrated on already vulnerable groups (particularly young minority men and the least-skilled), governments should exhibit extreme caution in trying to enhance the EITC with higher minimum wages, which have been shown to affect many individuals who are not in low-income families.

—Jill Jenkins
Chief Economist

Minimum Wage Effects in the Post-welfare Reform Era

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I. Introduction

Despite the abundance of research on the costs and benefits of minimum wage laws, they remain a subject of considerable debate at all levels of government. At the national level, there have been frequent proposals in recent years to increase the federal minimum wage from its current level of $5.15 per hour, which was set in 1997.[1] As of the end of the sample period, seventeen states and the District of Columbia had minimum wages that exceeded the federal wage floor; moreover, state minimum wages have recently been raised above the federal level in some large states (such as Wisconsin, Florida, Illinois, and New York), whereas with the exception of California the states with high minimum wages had previously been relatively small.[2] As a result, the share of the population aged 16-64 residing in states with a minimum wage higher than the federal level rose from 15.6 percent in 1998 to 38.4 percent in 2005, based on the Current Population Survey (CPS) data we use in this paper. In addition, living wages, which typically set a higher minimum wage for a subset of workers in an area, have spread to scores of cities, while city-wide minimum wages have recently been enacted in San Francisco and Santa Fe.[3]

This ongoing interest in mandated wage floors points to the continued importance of understanding their effects. And, the increasing prevalence of state and local minimum wage laws and the growing share of the population covered by these wage floors makes a direct focus on their impact particularly significant currently. Because much of the policy debate over minimum wage increases concerns their potentially adverse effects on employment opportunities for low-skilled individuals, we focus on employment outcomes in this paper.[4]

There is, of course, a large existing body of research that has studied the employment effects of minimum wages. The earlier literature initially used time-series data and variation in the national minimum wage, and the results tended to show that increases in the minimum wage led to employment losses among lesser-skilled groups of workers, with the elasticities of employment of the low-skilled with respect to the minimum wage ranging from –0.1 to –0.2 (Brown et al., 1983). However, in the early 1990s researchers began to exploit the variation in state minimum wages that was associated with an increasing tendency by states to set minimum wages above the federal level (e.g., Neumark and Wascher, 1992; Card, 1992a). This latter body of research proved more controversial, with some of the research tending to replicate the disemployment effects found in earlier studies (e.g., Neumark and Wascher, 1992 and 2000), other research finding no effect (e.g., Card, 1992a and 1992b; Card and Krueger, 2000) or even significant positive effects (Card and Krueger, 1994), and some research pointing to even stronger negative effects of minimum wages than found in the earlier research (e.g., Burkhauser et al., 2000). We will not fully dissect this literature here. However, our general view is that the studies finding no effects or positive effects are problematic because of problems with data or econometric specifications (Neumark and Wascher, 1992 and 2000). At the same time, we are somewhat skeptical of the large negative elasticities reported by Burkhauser et al. (2000) for teenagers as a whole,[5] although other work has found sizable effects for particular subgroups near the minimum wage (Abowd et al., 2000; Currie and Fallick, 1996).

The original motivation for using variation in state minimum wages was to obtain more reliable identification of the effects of minimum wages. In particular, because changes in the national minimum wage were relatively infrequent and because such changes were potentially correlated with other economic and

*This paper is based on joint work with William Wascher. The author is grateful to anonymous reviewers for helpful comments and to Stephen Ciccarella for outstanding research assistance.

policy changes, it was difficult to isolate the effects of minimum wages using only national data. With the federal minimum wage now unchanged for nearly a decade and with the number of states implementing their own higher minimum wages even greater, the case for focusing on the effects of state minimum wages is even more compelling today. Moreover, the spread of state minimum wages to larger states makes the issue more pressing as more workers are covered by state minimum wages, and provides a good opportunity for more reliably estimating the effects of the minimum wage.

At the same time, however, other labor market policies have radically changed the environment of the low-wage labor market from that prevailing in the 1980s and early 1990s. One implication of these changes is that the evidence from the earlier literature on minimum wage effects may not be directly applicable to an evaluation of recent or future increases in state and federal minimum wages. In particular, two policy changes are likely to have changed work incentives faced by the poor and thus the types of individuals competing for low-wage jobs. The first was the 1996 legislation that replaced the Aid to Families with Dependent Children (AFDC) program with Temporary Assistance to Needy Families (TANF). TANF made welfare funds available to states under the condition that they introduce policies designed to move recipients off welfare by encouraging self-sufficiency. Such policies have included specific legislation requiring welfare recipients to work, as well as limits on the number of months that families can receive welfare payments.[6]

The second important policy development relevant to low-wage labor markets was the expansion of the Earned Income Tax Credit (EITC). This expansion occurred along two dimensions. At the federal level, the credit rate increased sharply over the 1990s, rising from a rate of 14 percent in 1990 to 40 percent (with two children) in 1996, where it has remained since. In addition, a number of states introduced their own EITC programs, which typically specify a percentage supplement to the federal EITC that is provided to families by the state. The number of states with an EITC rose from seven states in 1996 to 15 states and the District of Columbia in 2005, boosting the percentage of the 16-64 year-old population residing in states supplementing the federal EITC from 14.3 to 29.4 percent, based on CPS data.[7]

Because of these changes, our best estimates of the effects of minimum wages in the current labor market environment is likely to come from data restricted to the period in which these most recent changes had occurred, rather than from studies that are based on information predating the period of welfare reform and EITC expansion, or that include samples extending back into the 1970s. Thus, in this paper, our dataset begins in 1997, subsequent to the most recent federal increases in the minimum wage, the introduction of TANF, and the increase in the federal EITC.

We first focus on results from the basic empirical framework developed and used in the existing research on the employment effects of minimum wages, which leads to a relatively standard pooled time-series cross-section data analysis. Paralleling much of the existing research, we first focus on teenagers and young adults (aged 16-19 and 20-24), relatively low-skilled workers for whom minimum wage effects are likely to be most pronounced. However, we also extend our analysis to study minimum wage effects at a more disaggregated level, focusing attention on those subgroups (minorities, high-school dropouts, etc.) for whom minimum wages might be most binding or who might be more affected by other policy changes.

We then extend this framework to incorporate information on the effects of welfare reform and the EITC into our analysis. We view this extension as important for three reasons. First, because changes in welfare rules or the EITC vary across states and can directly affect employment rates for the groups we study, controlling for these changes should improve our ability to isolate the direct effects of minimum wages from the effects of other labor market policies. Second, extending the standard specification to include changes in welfare rules and the EITC along with the minimum wage will highlight differences in how these policies affect various demographic or skill groups. For example, some

researchers have found that the EITC increases employment of young, unskilled women (Eissa and Leibman, 1996), whereas much of the minimum wage literature has found disemployment effects for a range of low-skilled workers.

Third, the inclusion of welfare reform and the EITC in our analysis helps to provide evidence on potential interactions between these policies. Most importantly, some minimum wage advocates argue that a higher minimum wage is complementary with an EITC. Obviously if there are no disemployment effects from a higher minimum wage, then a higher minimum wage will amplify the anti-poverty effects of the EITC because minimum wage workers, while scattered throughout the family income distribution, are disproportionately situated in poor families. Moreover, when the minimum wage declines in real value, a family's EITC payment can decline because the income level at which the maximum credit is received is indexed to inflation. Because of this, with a fixed nominal minimum wage, inflation can drop the family below this income level, if the family is dependent on income from minimum wage work.[8] Furthermore, as noted by Blank and Schmidt (2000), a higher minimum wage enables a family to achieve the same level of income (earnings plus EITC) at the maximum EITC credit with a smaller EITC payment. This in turn implies a lower marginal tax rate as the credit is phased out, which could reduce some labor supply disincentives.

However, these arguments do not fully consider the potential labor market responses to the two policies, nor how they might affect different groups. For example, a higher minimum wage would reduce the positive employment effect of the EITC if it reduces job opportunities for women who might be induced to enter the labor market because of the EITC. On the other hand, entry of lower-skilled women into the labor market in response to the EITC may be enhanced because of a higher minimum wage, which makes combined income from labor earnings and the EITC higher.[9] These scenarios, however, focus on those more likely to be eligible for the EITC, and to increase labor supply in response to it, by entering the labor market. Among groups likely to be ineligible, such as female teenagers (and low-skilled males), a high minimum wage coupled with an EITC could represent a "double whammy," with the minimum wage reducing their employment prospects via the higher wage imposed on employers, and the EITC reducing their employment prospects via the increased supply of women entering the labor market. Thus, the effects of interactions between policies, and how these interactive effects vary across different groups, are potentially quite complex, and obtaining evidence on them is important, particularly in light of the number of states implementing both higher minimum wages and state EITCs, and the combination of these policies at the federal level.

II. Data

We construct a database that combines information on wages, employment, and hours of work of individuals with information on minimum wage levels, and earned income tax credit and welfare policies by state, over the period 1997 to 2005.[10] The minimum wage data are compiled from annual summaries of federal state labor legislation reported each year in the Department of Labor's Monthly Labor Review. Most state minimum wages are specified as equal to or exceeding the federal minimum wage, although there are some states with a minimum wage that is below the federal minimum, often applying to small groups of workers not covered by the federal law. Because we do not have detailed information on who is covered by state law and because coverage of the federal minimum wage is extensive, we simply use the higher of the state or federal minimum as the effective state minimum.

The information on state EITCs comes from a series of reports published by the Center on Budget and Policy Priorities. State EITCs specify a percentage of the federal EITC that is paid to state taxpayers via the state income tax system, as a "supplement" to the federal EITC. Our state EITC variable is this percentage. In two states, this percentage varies with the level of income and/or with number of children. For Wisconsin, the supplement varies with number of children; we use the supplement for families with two children (14 percent). Minnesota

has its own phase-out, but the average supplement is reported as 33 percent.[11] In addition, the state credit is refundable in most states and nonrefundable (or partially refundable) in a few, and in some cases the recipient has a choice. In the latter, we use the refundable rate, on the presumption that most eligible families would prefer that rate. (A refundable EITC gives money back to the family even if there is no tax liability, whereas a non-refundable EITC only reduces any existing tax liability.) In our sample period, the federal EITC is unchanged with a phase-in tax credit of 40 percent for families with two or more children. As a result, identification of EITC effects comes solely from the state variation in the credit.

Characterizing state welfare policy is more difficult. The Urban Institute's Welfare Rules Database provides a detailed characterization of variation in state welfare policies (such as benefit amounts, asset tests, work requirements, length of time benefits can be received, etc.).[12] This database currently goes only through 2003, but we have extended the data through 2005 using information available on a state-by-state basis from other sources. There are a potentially huge number of possible policy variables to use. Because this paper is not a full-blown analysis of the effects of welfare reform, we build on the findings from Keane and Fang (2004) to choose which variables to include in our specification. In particular, they find that the most important variables influencing the welfare participation of single mothers are time limits and work requirements. We therefore focus on these two variables in our analysis.[13] Variation in the welfare reform variables stems from differences in policies chosen, as well as in the timing of implementation of welfare reform.[14]

We merge these state level policy variables with data from CPS Outgoing Rotation Groups (ORG). The ORG files are used to construct individual-level measures of wages, employment, and hours, as well as demographic and human capital indicators (sex, race, ethnicity, education, etc.). Finally, we append to each record the state unemployment rate in each month, and the proportion of the population in each demographic and skill group we study. The latter variable is exogenous (aside from migration). The unemployment rate is potentially endogenous, but by using the state-wide unemployment rate rather than a rate for groups more strongly affected by the minimum wage, we hope to capture more of an aggregate demand indicator. These state-level controls are the standard demand and supply controls used in previous minimum wage studies.

III. Methods

We estimate models for wages, employment, and earnings for a wide variety of demographic and skill groups, although we focus on the employment effects of minimum wages. The earnings estimates are unconditional rather than conditional on employment, so that the estimates reflect variation on both the extensive (employment) and intensive (hours of work if employed) margins of work, as well as variation in wages. All specifications are estimated at the individual level, with standard errors adjusted to account for non-independence among observations within the same state and over time.[15]

Denoting the dependent variable generically as Y, the control variables as X, and the minimum wage as MW, we begin with models for wages and employment of the form

$$Y_{ist} = \alpha + \log(MW_{ist})\beta + X_{ist}\lambda + G_s\mu + M_t\nu + G_s \cdot t \cdot \pi + \varepsilon_{ist}$$

The 'i,' 's,' and 't' subscripts denote individuals, states, and months, respectively. All specifications include fixed effects by state (G) and month (M). The state and time effects control for overall differences across these states that might be correlated with policy differences (such as the tendency to have higher state minimums in higher wage states), and for general changes over time (such as those generated by other policy changes) that might be correlated with minimum wages.[16] Finally, the model also includes state-specific time trends. Many of the results were similar with and without the state-specific trends, but in some cases the point estimates were quite different even if statistical tests for their exclusion were not decisive, which may reflect low power; in other cases statistical tests unambiguously called for the inclusion of state-specific trends.[17]

Two comments on the minimum wage variable

are in order. First, for all of the specifications we estimate, we also examined results that included the minimum wage variable with a one-year lag, reflecting earlier findings indicating that the policy effects evolve over a year in ways that can look quite different from the contemporaneous effects (Baker et al., 1999; Neumark and Wascher, 1992; Neumark et al., 2004). Second, earlier research on the employment effects of wage floors often used the minimum wage divided by a measure of the average wage, capturing the idea that it is the effect of the minimum wage on the relative price of unskilled labor that is most relevant for the employment of such labor. With a logarithmic specification, the log of the minimum wage and the log of the average wage can be included separately; if the coefficients are equal in absolute value and opposite-signed, then this is equivalent to including the log of the ratio. When we included the log of the minimum wage and the log of the average wage (for males aged 35-54) separately in the employment and hours equations, the null hypothesis that these two variables had coefficients equal in absolute value but opposite in sign was often rejected. Moreover, the estimated coefficient on the minimum wage was not sensitive to including the average wage control. Thus, we report results including only the log of the minimum wage.

In order to capture the influences of other policy changes, we augment equation (1) by adding measures of state EITCs and welfare reform. We are interested in both the estimated effects of these variables, as well as in how their inclusion influences the estimated minimum wage effects. In addition, we estimate models with policy interactions. As it turns out, the welfare reform variables have no discernible effects on the dependent variables, so we focus on minimum wage-EITC interactions. Finally, for some of these specifications we also estimate models for earnings, to gauge the effects of the alternative policies (and their combinations) on a measure that summarizes the combined effect of wage changes and employment or hours changes. To simplify the specification, we specify the minimum wage variable in these models as the average of the current and lagged (one year) minimum wage (AMW):[18]

$$Y_{ist} = \alpha + \log(AMW_{st'})\beta + EITC_{st'}\gamma + [EITC_{st'} \cdot \{\log(AMW_{st'}) - \overline{\log(AMW)}\}]\delta + X_{ist}\lambda + G_s\mu + M_t\nu + G_s \cdot t \cdot \pi + \varepsilon_{ist}.$$

IV. Recent Studies

Although we are not aware of any research that has examined the interactions between the minimum wage and welfare or EITC policies, it seems useful to summarize studies that have focused on the effects of minimum wage changes in the post-welfare reform era. As was the case for previous research on the minimum wage, these studies do not come to a consensus about the influence of increases in the minimum wage on the employment of low-skilled workers.

Bernstein and Schmitt (2000) estimate regressions of changes in employment rates (for two groups—teenagers, and adults with less than a high school education), defined over 1995-1996, 1995-1997, 1995-1998, on the fraction affected by the two federal minimum wage increases in this period.[19] Although not specified, we assume this fraction is computed from the 1995 wage distribution, as simply the fraction below the new minimum wage. The increasingly longer differences capture more of the effects of the two federal increases, which took place in 1996 and 1997. (They also report results beginning in 1994, arguing that these estimates should be more free of any effects of future anticipated minimum wage increases.)

We focus on the results for teens, paralleling much of the minimum wage literature. For teenagers, the only time a negative and significant disemployment effect emerges is for the 1995-1996 period—i.e., only capturing the contemporaneous effects of the first increase. When the sample period is extended to 1997 or 1998, or when it is extended back to 1994 (or both), the estimates are almost always negative, but not significant. On the other hand, for the specifications beginning in 1995, some of the estimated elasticities are quite large. For the one case of negative and significant teen employment effects just noted, the elasticity is –1, and for the others it ranges from –.1 to –.4; these fall by about half when

the sample begins in 1994. Thus, it is perhaps not unreasonable to conclude that the small sample afforded by looking only at this limited period is part of the reason for the failure to detect stronger disemployment effects. This is perfectly consistent with the general lack of robustness of these estimates.

Bernstein and Schmitt (1998) report results from a difference-in-differences analysis that computes the change in employment rates for various groups of teens and young adult high school dropouts, relative to changes from the period prior to the most recent federal minimum wage increase, as well as adjusting for aggregate employment changes. This analysis does not account for state-level variation in minimum wages; in this case, the federal increases induced different minimum wage changes in different states, because many states had higher minimums prior to the 1996 and 1997 federal increases (see Neumark, 2001). The estimates across many groups are centered on zero. Focusing on the estimates for the full increase, there are rather large negative effects estimated for black men (a 4.8 percentage point drop in the employment rate), which is not statistically significant (t=1.47), and a large and significant positive effect estimated for Hispanic women (a 7.8 percentage point increase in the employment rate). Although not reported, the implied elasticities from some of these significant estimates are huge. For example, for Hispanic female teens the 7.8 percentage point increase in the employment rate is probably an increase of 20% or more (all the study reports is the employment rate for all teen females, which averages about 43%), which coupled with the 21% increase in the minimum implies an elasticity near 1. We do not have an explanation for some of the inordinately large estimates that result, although they make us somewhat reluctant to put a great deal of store in these estimates, especially given the problem of omitted variation in state minimum wages.

Neumark (2001) also studied the recent federal increases, using a "pre-specified research design." This study resulted from an effort by David Levine, as editor of Industrial Relations, to get various researchers who had studied minimum wages to pre-specify a research design for studying this federal minimum wage increase. The journal would review the design and accept it (with revisions) or not, after which the authors, when the data were released, would simply follow their "recipe" and report the results. The motivation for this project was to try to cut through the apparent relationship between who authored minimum wage studies and what answers they found. As documented in Table 1 of the paper, perhaps the most pronounced tendency was for research by us to find negative employment effects (although not always), and for research by David Card and Alan Krueger to find zero (or positive) effects—a pattern not inconsistent with prior biases affecting the reported results.[20] While the intended outcome from this effort was a symposium with a number of papers by previous researchers who had studied the minimum wage, only one pre-specified research design was submitted and published.

In this analysis, standard panel data models are estimated with two different minimum wage variables—the minimum wage relative to the average wage in the state, and the fraction below variable described above. These variables are entered contemporaneously as well as lagged, and the models include state and year fixed effects, and sometimes adult employment rate controls (for those with more than a high-school education). The data cover the months of October-December, 1995-1998. These months are used because the federal minimum wage increases occurred in October 1996 and September 1997. Variation in state minimum wages is used, so that the identifying information is the change in the minimum wage variable, by state, introduced by the federal increases. However, there is not a great deal of variation in this period, making it far from ideal for studying the effects of minimum wages, although the pre-specified research design approach has other merits.

The results for teenagers indicate positive but insignificant or zero effects of the federal minimum wage increases on employment. The estimates are generally imprecise, but near zero. For example, for the first specification with current and lagged relative minimum wages, the estimated elasticity is .06. For young adults (aged 16-24) the estimates have a greater tendency to be negative, although

they are insignificant. The estimates are larger in absolute value, generating elasticities of approximately –.15 in some cases, but not others. For example, the sign changes when the data are restricted to observations from the ORG files. The non-robustness of the results parallels to some extent what Bernstein and Schmitt found, possibly reflecting the small sample size. Finally, when the sample is restricted to the less-skilled workers among these age groups, there is stronger evidence of disemployment effects. For non-enrolled 16-24 year-olds with no more than a high school education, the estimated elasticities are around –.30, and for non-enrolled 20-24 year-olds with no more than a high school education the elasticities are around –.15. Thus, there is stronger evidence of adverse employment effects for young individuals with lower amounts of education, although again there is some variation in the estimates, and they are not always significant. Finally, for the fraction below specifications, somewhat precise estimates are also obtained for non-enrolled 16-24 and 20-24 year-olds with less than a high school education and these point to estimates that are somewhat variable, but always negative and often significant. For the main specifications, the elasticities range from –.11 to –.21. We read the evidence in this paper as pointing to disemployment effects of the minimum wage for young, unskilled workers, with the exception of teenagers; the absence of disemployment effects for teenagers parallels some of the earlier work by Neumark and Wascher discussed earlier, which suggests that this lack of a net effect may mask compositional shifts.

Looking beyond the federal increases to recent state increases, another Economic Policy Institute study, by Chapman (2004), estimates a cross-section regression of state-level employment growth between 2000 and 2003 on the share of each state's workforce earning between 100 percent and 120 percent of the state minimum wage in 2003, and finds no relationship.[21] This result is robust to including a control for the share of manufacturing employment in 2000. It is unclear from the study why the proportion of the workforce earning near the minimum wage in 2003 would be expected to affect employment growth from an earlier year to 2003. For example, if the overall gains in employment disproportionately reflected growth in low-wage employment, or if the rise in employment was due in part to an increase in labor supply among less-skilled workers, we would expect a positive relationship between the low-wage share in 2003 and employment growth from 2000 to 2003. In addition, we think most economists on either side of the debate would agree that minimum wages do not drive overall employment, but instead, if anything, influence employment among low-skilled workers who are most affected by minimum wages, although organizations opposing minimum wages have made stronger claims.[22]

Focusing on state variation in minimum wages, the Fiscal Policy Institute (2004) shows that employment rose faster between 1998 and 2001 in states with a minimum wage higher than the federal level than in states where the federal minimum was binding. This study examines overall employment growth, and growth in employment in retail, at small businesses (fewer than 50 employees), and at small retail businesses. Most of the states with higher minimum wages after 1998 raised their minimum in 1999 or later to a level above the federal minimum wage, so the minimum wage variable for the most part captures increases in the minimum wage, although the authors do not exploit variation in the timing of these increases. For overall employment, the authors report that the states that increased their minimums (11 states plus the District of Columbia) had higher employment growth from 1998 to 2001 than the other states, and about the same employment growth from 2001 to 2004. For retail employment, growth was higher in the high minimum wage states mainly in the latter period. The employer size analysis can only be done with County Business Patterns data through 2001, and shows greater growth of small business employment in the high minimum wage states, and among small retail businesses.

The principal problem with this research, however, is that it makes no effort to control for other factors that might have influenced employment growth over that period. (In addition, it pays no attention to the level or timing of state minimum wages.) It might have made more sense to compare, say, the difference between retail employment

growth and overall employment growth in the two sets of states, a simple difference-in-difference analysis. For the 1998-2004 period, overall employment growth was about 2 percentage points faster in the high minimum wage states, while retail employment growth was a little over 4 percentage points faster, consistent with the study's contention that a higher minimum wage is associated with faster retail employment growth; that is, a simple difference-in-difference based on retail versus total employment suggests that minimum wages boosted retail employment by 2 percentage points. But for the 1998-2001 period, based on numbers provided by the author, retail employment grew 1.3 percentage points less than total employment in the high minimum wage states, versus .4 percentage points less in the low minimum wage states, so in this case the difference-in-difference estimate is –.9 percentage point, which would be interpreted as a negative effect of minimum wages on retail employment.[23] It is not clear which is the better period to compare, as the economy reached a low point in early 2001, and then grew subsequently. Finally, for small businesses and small retail businesses, 1998-2001 changes are reported, and the 2001 data are the latest used. For small businesses, the same calculation indicates a difference-in-difference estimate of .2, and for small retail businesses 0.[24] Thus, this simple (but more meaningful) analysis suggests that it is difficult to detect much relative difference in employment growth in retail, small businesses, and small retail businesses, in contrast to the study's claim of relatively large positive effects.[25] A recent study by Sabia (2006) provides a more thorough analysis of the effects of minimum wages on employment in retail and in small businesses, and finds negative effects, and much stronger negative effects for teens in these sectors. However, that study is based on a much longer sample period, leaving unanswered the question of what the more thorough analysis for the period studied by the Fiscal Policy Institute would have concluded.

V. Results

Descriptive Statistics

Table 1 reports descriptive statistics at the individual and state level, including the outcomes we study and the policy variation. The table covers the period 1997-2005, and the individual-level data are for 16-24 year-olds, except where otherwise noted. The sample is about one-half female, as expected, and there are more individuals in the 20-24 year-old age group, which encompasses one additional year. About 15 percent of the sample is black and 15 percent is Hispanic. These two groups are not mutually exclusive, although the overlap is very small; about 1.3 percent of those either black or Hispanic are reported as both black and Hispanic. When we disaggregate by schooling level, we focus on 20-24 year-olds, since their current schooling is more likely to be indicative of their completed schooling. Among 20-24 year-olds, 44.5 percent have completed at most a high school education, and 13.6 percent have not completed high school (and are labeled high school dropouts, although they of course may complete high school later).[26] The average state unemployment rate faced by sample members in this period was 5 percent. Our regression models also include the proportion of the age-skill-demographic group in the population, but because there are many such proportions calculated for the different age, skill, and demographic group combinations we consider, we do not report the descriptive statistics, except for the proportions of the overall population that are in the 16-19, 20-24, and 16-24 year-old age groups; these are, respectively, .079, .093, and .172.

The second panel of the table reports on the labor market outcomes we study. As expected, average wages are higher for 20-24 year-olds than for 16-19 year-olds, as are employment rates and earnings (unconditionally, which is what we study in the regression models, as well as conditional on positive earnings).[27] The policy variables shown in the last panel indicate that across all observations, the minimum wage averaged $5.37, 22 cents higher than the federal minimum. Of course as indicated earlier, state minimum wages and the number of states with a minimum wage above the federal level rose over the sample period. For individuals in states with a higher minimum, the average minimum wage was $6.31, 22.5 percent above the federal minimum. On average, sample members faced state EITC supplements of 4.1 percent, with the

figure four times as high for observations with state EITCs. Over 80 percent of the observations on individuals in states that supplement the EITC are from states with a refundable EITC, and in almost all cases the EITC is fully refundable.

Effects of Minimum Wages on Employment

We begin our empirical analysis of minimum wages with basic regression estimates of their effects on employment. Table 2 reports the coefficient estimates from regressions for the three age groups, for 20-24 year-olds with a high school education or less, and for 20-24 year-olds who dropped out of high school. The following two tables report results disaggregated by race and ethnicity, and then disaggregated further by sex.

The first specification includes the contemporaneous minimum wage, the control variables, and state and time fixed effects, but excludes the state-specific trends. The second specification adds the state-specific trends. In the third specification, we substitute the lagged minimum wage for the contemporaneous minimum wage, while in the fourth we include both. We show results for all of these specifications, but the discussion tends to emphasize the summed contemporaneous and lagged effects from the fourth specification.

The estimates for all individuals combined, distinguished only by age, are reported in the first three columns of Table 2. Regardless of specification, the point estimates are consistent with the strongest negative effects for 16-19 year-olds, followed by 16-24 year-olds combined, and then 20-24 year-olds, as we would expect if the youngest individuals are the least skilled. For the two older groups none of the estimates are statistically significant, and for the 20-24 year-olds the estimates are generally slightly positive. For teenagers, we find a marginally significant negative estimate for the specification excluding state-specific trends, but not when the trends are included, although the estimates do not change by that much. In the specification combining contemporaneous and lagged effects, the contemporaneous effect is marginally significant, but neither the lagged nor the summed effect is. For the two low schooling groups, the estimates are again insignificant, although for high school dropouts the point estimates, especially when including lagged effects, are quite large. For the last specification, including contemporaneous and lagged effects, we report the implied elasticities of employment for the relevant group with respect to changes in the minimum wage; given that the coefficients measure the effect of the log minimum wage on the probability of employment, we divide by the probability of employment for the corresponding group to compute the elasticity. For teens the estimated elasticity is –.16. This is in the typical "consensus" range of minimum wage elasticities (Brown, 1983; Fuchs et al., 1998), although as noted above for this sample the estimate is not statistically significant (which does not mean that the estimate is zero). The next three estimated elasticities are small, while for high school dropouts aged 20-24 the estimate is much larger. Nonetheless, for estimates that do not disaggregate by race, sex, or ethnicity, we fail to find statistically significant effects of minimum wages.

The estimates disaggregating by race and ethnicity are reported in Table 3. The differences between the results for the different race and ethnic groups are rather striking. Estimates are reported for non-black, non-Hispanics, for blacks and Hispanics combined, and then for the two groups separately. Given that non-black, non-Hispanics make up a large share of the overall sample, the results for this group are very similar to those for the full sample. But for blacks and Hispanics combined, there is strong evidence of disemployment effects of minimum wages as long as teenagers are included. For both teenagers and 16-24 year-olds, we find a negative and statistically significant effect of minimum wages on employment in specifications including the lagged effect. For teens, the implied elasticity is –.66, and for 16-24 year-olds it is –.39. When we break up blacks and Hispanics, we find that although there are some large negative point estimates for blacks, the statistically significant evidence of negative employment effects emerges for Hispanics, in particular for 16-24 and 20-24 year-olds (with the evidence for the latter group only marginally significant). The estimated elasticity for Hispanic teens is similar (–.43), but not significant.

Then, in Table 4, we also disaggregate by sex. Because of small samples, we do not separately disaggregate blacks and Hispanics by sex. Here, we find perhaps an even more striking pattern of heterogeneity in the estimated effects of minimum wages. In particular, for women (see the lower panel) there is virtually no evidence of significant effects of minimum wages on employment. The one exception is for 20-24 year-old high school dropouts, for whom there is strong evidence of a lagged disemployment effect. The point estimate for minority teenagers is large and negative (–.55), but insignificant. But other than that, the estimates are generally quite small. The estimates for men, however, contrast sharply. For all race and ethnic groups combined, there is no significant evidence of disemployment effects, once the state-specific trends are included, and the same is true for non-black, non-Hispanic men. But for black or Hispanic men, there is strong evidence that minimum wages reduce employment, with statistically significant negative effects for 16-24 and 20-24 year-olds (and a larger, but insignificant, implied elasticity for teenagers). For the low-schooling groups, the point estimates are negative but insignificant, and contrary to expectations larger negative for those with a high school education or less versus dropouts.

To display the employment results more conveniently, the first column of Table 5 reports all of the estimated employment elasticities from Tables 2-4. As explained in the notes to the table, we generally report the summed contemporaneous and lagged effects from specification 4, except in cases where the data indicate that the specification with only a contemporaneous or only a lagged effect is preferred.

For the post-welfare reform period, the message is quite clear. There is virtually no evidence that higher minimum wages reduced women's employment, except perhaps for the very least-skilled high school dropouts. And the estimates are often positive rather than negative, indicating that the issue is not one of negative but of insignificant point estimates. However, there is quite strong evidence that higher minimum wages in this period led to disemployment among young minority men.[28]

Effects of Minimum Wages on Wages

We also estimated similar models for wages, for those who are employed, with the log wage as the dependent variable. These results are summarized in the second column of Table 5. As for the employment specifications, we generally report summed contemporaneous and lagged effects, except in cases where the data indicated that the specification with only a contemporaneous or only a lagged effect is preferred.[29] For the wage (and later the earnings) estimates, the dependent variable is already in logs, so the estimated coefficients (or their sums) are directly interpretable as elasticities.

When we disaggregate only by age, the estimated effect on wages is positive in all cases. It is larger for 16-19 year-olds than for 16-24 or 20-24 year-olds, and the estimate is smallest for the latter group. It is significant for 16-19 year-olds but not the other two groups. The estimated wage effect is also positive and significant for 20-24 year-olds with less schooling, with the effect largest among high school dropouts. Taken as a whole, these estimates suggest that the size of the effect is inversely related to skill level. Note, however, that the estimated elasticities are well below one, most likely because even in these low-skill groups, many workers earn above the minimum wage.

Disaggregating by race and ethnicity, we find a positive and significant effect for non-black, non-Hispanic teens only. For the minority groups, there are no significant effects, and for Hispanics (and black and Hispanic teens combined) the point estimates are negative.[30] Disaggregating by sex as well, the overall results for the three age groups are quite similar, with positive and significant effects for teenage males as well as for females, and smaller and insignificant effects for the 16-24 year-old females and for 20-24 year-olds. The results are also similar for the non-black, non-Hispanic sub-samples. For black or Hispanic females, the estimated wage effects are insignificant except for 20-24 year-olds. For minority teen males, there is statistically significant evidence of a negative effect. For the two groups of less-educated 20-24 year-olds, for men we find a stronger positive wage effect for dropouts, as we would expect, whereas for women the estimates are

similar for dropouts and those with a high school education or less.

For the most part, then, the estimates point to positive effects of minimum wages on the wages of least-skilled workers, although there is a handful of cases of negative effects, which may be explained by the underlying economics or may reflect data issues (including smaller samples).

Minimum Wages, the EITC, and Welfare Reform

Perhaps the most obvious question that arises with respect to changes in other policies is whether the pattern of these changes can help explain the apparent absence of negative effects of minimum wages on employment for females. For example, if the EITC encourages female employment—which we expect for women with children—and states increasing minimum wages were also raising their EITCs, this could explain the absence of disemployment effects of minimum wages for females; the same argument applies to welfare reform, of course.

To explore the effects of the EITC on women's employment, specification 1 in Table 6 takes the final minimum wage specification in the tables just discussed and adds the state EITC and indicators for time limits and work requirements adopted as part of welfare reform. The estimates are reported for the same subgroups of women included in Table 4.

Looking first at the EITC and welfare reform effects, we find no evidence that welfare reform affected female employment. The estimates are never statistically significant, either individually or jointly. The estimated effects of time limits are either negative or centered on zero, although the estimated effects of work requirements are positive, as might be expected.

On the other hand, with the exception of teenagers (of any race or ethnic group), and high school dropouts, there is always a strongly significant positive effect of the state EITC on women's employment, paralleling existing work cited earlier; recall that our identification of EITC effects comes from state variation in EITC supplements.[31] The effects are sizable. For example, the estimate for 20-24 year-olds as a whole (.302) implies that imposing a 10 percent EITC supplement boosts the probability of employment by three percentage points. It is not surprising that we find no effect for teenagers, since few of them have children.[32] Conversely, the estimated effects are larger for minority women, who have higher fertility rates at ages 20-24.[33]

Of course it would be ideal to accurately identify who is eligible for the EITC and to determine whether the expected effects appear for the right groups (i.e., employment increases for the eligible). Eligibility can depend on whether one has children, as well as on age, enrollment status, financial dependency, and of course family income. Unfortunately, with the CPS ORG files used for this analysis, there is no way to identify the eligibles along the most straightforward lines of who has children. In the monthly CPS files, respondents are not asked about number of children (under 18) until 1999. Furthermore, this is only asked of householders or their spouses. This is an endogenously selected sub-sample in terms of family structure and income, with inclusion in it potentially related to labor market outcomes and EITC receipt. Thus, although the CPS ORG files restrict us to the type of analysis presented here, there is clearly scope for further refinement and analysis of the EITC effects based on identification of eligibility status, and it would be wise to interpret the results for the effects of the EITC cautiously until such work has been completed.

Another issue is the refundability of the EITC, and whether this affects the strength of its impact. In general, a refundable EITC should have more impact, because it is valuable even if the family owes no income taxes, and is more valuable if the value of the credit exceeds the tax liability. Similar models were estimated including both the EITC variable and this variable interacted with the proportion of the EITC that is refundable (this value is either one or zero in almost all cases, with the only exception being Rhode Island which has a partially refundable EITC). The models were estimated for 20-24 year-old women, and the various subgroups thereof. There was never a significantly higher effect of the EITC when it was refundable, although in almost all instances the point estimates indicat-

ed stronger effects in these cases. Given the weakness of the results, however, in the ensuing analysis we simply focus on the EITC without regard to refundability.

Returning to the evidence on minimum wage employment effects, although the results point to sharp employment-inducing effects of the EITC, adding the EITC (and welfare reform) variable(s) to the specification does little to alter the estimated minimum wage effects. Thus, the absence of disemployment effects of minimum wages for women in this period appears to be a real result, and not attributable to the exclusion of other policy changes correlated with minimum wage increases.

The next issue to which we turn is that of policy interactions. Given the absence of any effect of the welfare reform variables, and the strong main effects of the EITC, we focus on minimum wage-EITC interactions. To eliminate the extra complication of having interactions between the EITC and both contemporaneous and lagged minimum wages, we instead use a specification with an average of the current and lagged minimum wage variables (the average of the logs). We first report estimates for this model (specification 2) to verify that nothing changes qualitatively. In particular, the minimum wage employment elasticities for specification 2 are very similar to those in the earlier tables. Then, specification 3 includes this minimum wage variable and the EITC, along with their interaction. As noted earlier, a higher minimum wage could reduce the positive employment effect of the EITC for those who might be eligible for the EITC. Alternatively, the interaction for these women could be positive, because the higher minimum wage makes entering the labor market even more lucrative than does the EITC in isolation, for this particular subgroup. But among groups likely to be ineligible, such as female teenagers (and males), a high minimum coupled with an EITC could be a particularly bad combination, with the minimum wage reducing their employment prospects via the higher wage floor imposed on employers, and the EITC further reducing their employment prospects via the increased supply of eligible women entering the labor market.

The evidence on employment, as it turns out, is consistent mainly with the latter interactive effect. As shown for specification 3 in Table 6, the significant minimum wage-EITC interactions arise for all teenagers and for minority teenagers, and are negative. To help in interpreting the coefficient estimates, the bottom rows of the table report the implied minimum wage elasticity for states with no EITC, with a relatively low 10 percent EITC, and with a quite high 25 percent EITC. In all cases for teenagers (and indeed for 16-24 year-olds and the two lower schooling groups), the estimates become more negative with a higher EITC, reflecting the negative estimate of the interactive coefficient. Furthermore, for all teenagers and minority teenagers, the implied minimum wage elasticity is significant only at the higher EITC (marginally so for all teenagers). In addition, for the high school dropouts, although the interaction coefficient itself is not statistically significant, the implied elasticity with either the 10 or the 25 percent EITC is significant and negative (marginally for the 10 percent EITC). The one exception is for 20-24 year-old minority women, for whom a higher minimum wage coupled with a generous EITC boosts employment significantly.

Thus, for almost all groups of women aged 20-24, the EITC increases employment. On the other hand, a high minimum wage combined with a high EITC reduces employment of female teenagers. Finally, looking at employment, there is one potentially important group for which a higher minimum wage combined with a higher EITC appears to generate higher employment--minority women aged 20-24. The boost to employment of 20-24 year-old minority women, even at the cost of lowered employment among female teenagers, might be viewed as a good policy outcome on distributional grounds, as the 20-24 year-old minority women are more likely to have children to whom we would like to direct some of the benefits of the EITC. Thus, although some groups appear to gain and others to lose by combining a higher minimum wage with a more generous EITC, there may be benefits for groups we are trying to help more. However, the evidence of positive minimum wage-EITC interactions for minority women aged 20-24 is weaker, but for those with a high school education or less becomes positive and significant for a high EITC.[34]

However, to this point we have only examined employment effects of the minimum wage and the EITC. These policies may also affect hours as well as wages. Thus, we next estimate similar specifications for wages and for earnings (wages times weekly hours, with earnings defined as zero for the non-employed), to get a better sense of how these policies —and their interaction—affect the economic well-being of women. The results are reported in Table 7. For wages, there was no evidence of minimum wage-EITC interactions, so we simply report the specification with the minimum wage and EITC variables added separately. The interesting result revealed by these estimates is that the EITC reduces wages for 16-24 and 20-24 year-old minority women. However, the evidence of positive minimum wage-EITC interactions for minority women aged 20-24 is weaker, but for those with a high school education or less becomes positive and significant for a high EITC [35] These negative wage effects work in the opposite direction from the positive employment effects we just saw. The regressions for earnings, therefore, are needed to better assess the overall impact of the EITC.

For earnings, the first specification includes the minimum wage and EITC variables, and the second adds their interaction. For the first specification, the estimated effects of the minimum wage are sometimes positive and sometimes negative. The largest effect, and the only one that is significant (marginally), is for 20-24 year-old black or Hispanic women, for whom the elasticity of earnings with respect to the minimum wage is .8. Looking back at Table 5, we see that the wage effect for this group was one of the highest, and the employment effect was small but positive, which explains the positive earnings effect. The estimated effects of the EITC on earnings are positive across the board. Not surprisingly given the employment effects reported in Table 6, the estimated earnings effects are largest for 20-24 year-olds, although the estimates are statistically significant for 16-24 year-old women (all, and non-black, non-Hispanic). The estimates are by far the largest, and also marginally significant, for the two low-schooling groups of 20-24 year-olds, which makes sense since these groups are likely to reap the most from the EITC since their earnings are low; these groups are less likely to be in the phase-out range where the EITC can generate incentives to work less rather than more. In addition, less-educated women are more likely to have children at these ages. Curiously, the estimated effect is also positive and marginally significant for all teenagers, although for minority teenagers the estimate is near zero. Overall, then, for women the positive employment effects of the EITC generally outweigh the negative wage effects; this indicates that, in general, the gains from the EITC are not completely dissipated by wage reductions stemming from outward labor supply shifts.[36]

For the specifications adding the minimum wage-EITC interaction, reported in the bottom part of the table, there are two findings of note. First, for the same groups for which the employment estimates in Table 6 indicated adverse effects of a high minimum wage coupled with a high EITC (all teenage women, minority teenagers, and 20-24 year-old high school dropouts), the same evidence emerges with respect to earnings. For these three groups the estimated coefficient of the interaction is negative and at least marginally significant, and especially for the dropouts the effect of a higher minimum wage is particularly strong when the state EITC is high. The other interesting result, however, which cuts in the other direction, is that for 20-24 year-old minority women there is a positive and significant effect of the minimum wage-EITC interaction. This is in line with the earlier argument that for some subgroups a higher minimum could enhance the positive impact of the EITC by encouraging work (even if, overall, a higher minimum reduces employment). Thus, for younger, non-teenage minority women there does appear to be a potentially beneficial impact from combining a high EITC with a higher minimum wage.

Finally, we turn to the effects of the EITC (and the minimum wage) on men. A higher EITC could reduce earnings of men. First, focusing on the EITC, we do not expect a positive employment for men because fewer of them live with children, and if they do they are more likely to be employed regardless of the EITC, in which case it is considerably more

likely that they are in the plateau or phase-out region of the EITC where there are incentives, if anything, to reduce labor supply. Second, if the EITC induces increased labor supply of women (via labor market entry), some men may face more competition and hence lower wages (Leigh, 2004). A higher minimum wage coupled with an EITC could cut in different ways. To the extent that it is nonbinding on a large portion of the workforce, the negative wage effects could be exacerbated by a high minimum wage that leads to more labor market entry among women. On the other hand, a higher minimum wage could create a floor below which wages cannot fall despite the increased labor supply of women, in which case a higher minimum wage coupled with a higher EITC might reduce men's employment.

To explore these questions, we first estimated models for men's employment incorporating the EITC and minimum wage-EITC interactions, but detected no substantive or significant employment effects of the EITC; hence, we do not report these results. However, as reported in Table 8, we do find evidence of negative effects of the EITC on some men's earnings, and of effects of the interaction between the minimum wage and the EITC on men's wages and on men's earnings. The top panel reports the wage effects. For minority men aged 16-24 and 20-24, the estimated main effect of the EITC is significantly negative, and the estimated interactive effect of the minimum wage and the EITC is negative and marginally significant. As before, we also report the implied minimum wage effect for different levels of the state EITC, and at high levels of the EITC the estimated minimum wage elasticity is negative and marginally significant. More generally, for other groups (such as all teenagers and 20-24 year-old high school dropouts), the higher the EITC, the more the positive effect of the minimum wage on wages is reduced or eliminated, reflecting the negative point estimates of the minimum wage-EITC interaction. But the main effect of the EITC measures the effect of the EITC evaluated at the mean of the minimum wage variable, and hence provides a good sense of how the EITC affects wages for a representative sample member.

The bottom panel reports the estimates for earnings. Given the absence of strong employment effects, these to a large extent mirror the wage effects. In particular, as shown in specification 1, without the interactions, for 20-24 year-old minority men the EITC reduces earnings. Moreover, as shown in specification 2, in conjunction with a high minimum wage the EITC reduces earnings even more, as indicated by the significant negative estimate of the interactive effect. The same is true for 16-24 year-old minority men more broadly, at higher levels of the EITC. And a comparison of the estimated elasticities in Tables 7 and 8 suggests that at high EITC rates the negative effects on men's earnings are somewhat larger.[37] Finally, note that in no case is there evidence that a higher minimum wage increases men's earnings, whereas there is some evidence of adverse earnings effects for minority men.[38]

VI. Conclusions

The low-wage labor market has undergone substantial changes since the first wave of the "new minimum wage" research, most of which studied data through the mid-1990s at the latest. Welfare reform and the growth of state EITCs have changed incentives to work, and in doing so have potentially changed the effects of minimum wages. Plus, the recent spread of minimum wages higher than the federal minimum to many states, including larger states, has increased interest in the effects of these state minimums.

In this paper, therefore, we study the effects of minimum wages in the post-welfare reform era. We estimate relatively standard models of minimum wage effects on employment of young men and women, as in the preceding research, but we also estimate models that introduce effects of other policy changes, and that allow for interactions between minimum wages and the EITC.

The evidence on employment effects finds that the disemployment effects of minimum wages are concentrated on young minority men. For these men, we find significant negative employment effects, with elasticities in the range of –.5 or –.6. For young white men the estimated effects are negative, with somewhat smaller elasticities, but not

statistically significant. For young women, in contrast, there is essentially no evidence of much effect of minimum wages on employment, with the exception of high school dropouts. Furthermore, this absence of disemployment effects for women is not the result of estimating models omitting other policy changes that could influence employment. When we incorporate information on state EITCs and state variations in welfare reform, we continue to find little evidence that minimum wages affect employment. At the same time, there is quite strong evidence that the EITC boosts employment of young women (although not teenagers).

We also explore the question of how minimum wages and the EITC interact in affecting both women and men. There are possible explanations as to why a higher minimum wage could enhance the effect of the EITC for women, by inducing particular subgroups to increase their employment rates to a greater extent than would be caused by the EITC alone. But it is also possible for a high EITC coupled with a high minimum wage to have adverse effects, especially for men who may have to compete with the women induced to enter employment by a higher EITC.

The evidence reveals policy effects that vary quite sharply across different groups. Higher minimum wages reduce earnings of minority men, and more so when the EITC is high. In contrast, the EITC boosts minority women's employment and earnings, and coupling the EITC with a higher minimum wage appears to enhance the positive effect of the EITC for minority women, although it hurts female teenagers and 20-24 year-old high school dropouts. Whether or not the policy combination of a high EITC and high minimum wages is viewed as favorable or unfavorable therefore depends in part on whose incomes policymakers are trying to increase. There is a potential argument for more concern with the incomes of younger minority women, who may be more likely to have and be caring for children. On the other hand, the estimates suggest that at high EITC rates the negative effects on men's earnings are somewhat larger, and the apparent adverse effects for female teenagers and dropouts also have to enter into the equation. Given the variation in effects, there is no clear policy prescription. We hope, though, that we have helped to identify some of the important distributional effects that need to be weighed by policymakers.

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Endnotes

1. Most recently, the Senate considered legislation to raise the federal minimum wage to $7.25 per hour over two years.

2. As of September 1997, when the federal minimum wage was last raised, the following states had minimum wages above the federal level: Alaska ($5.65); Connecticut ($5.18); Washington, DC ($6.15); Hawaii ($5.25); Massachusetts ($5.25); and Oregon ($5.50). As of the end of the sample period, in December 2005, state minimum wages above the federal were as follows: Alaska ($7.15); California ($6.75); Connecticut ($7.10); Delaware ($6.15); Washington, DC ($6.60); Florida ($6.15); Hawaii ($6.25); Illinois ($6.50); Maine ($6.35); Massachusetts ($6.75); Minnesota ($6.15); New Jersey ($6.15); New York ($6.00); Oregon ($7.25); Rhode Island ($6.75); Vermont ($7.00); Washington ($7.35); and Wisconsin ($5.70). Maryland implemented a minimum wage of $6.15 in February 2006. For current information on state minimum wages, see the Department of Labor web site http://www.dol.gov/esa/minwage/america.htm (viewed May 24, 2006).

3. For an up-to-date review of living wages and research on their effects, see Adams and Neumark (2005).

4. However, Neumark et al. (2004, 2005) argue that the distributional consequences of minimum wage are more important from a policy perspective.

5. These larger elasticities stem from the exclusion of year effects in the standard minimum wage specifications used in much of this literature. Instead, the authors attempt to directly control for business cycle effects with a "recession dummy." In general, however, labor economists studying minimum wages have professed skepticism that changes over time in the economic environment relevant to low-skilled workers can be captured with a few control variables, and thus have opted to include year effects in their specifications. The inclusion of year effects implies that identification comes primarily from variation in state minimum wages (or solely from the state variation if the minimum wage variable does not have an average wage measure in its denominator, but instead is just the level or log of the minimum wage).

6. For details as well as some recent analyses of welfare reform, see Blank (2002) and Keane and Fang (2004).

7. The 15 states with EITC supplements in 2005 are Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Massachusetts, Minnesota, New Jersey, New York, Oklahoma, Oregon, Rhode Island, Vermont, and Wisconsin, and the supplemental EITC in those states ranges from 4.92 to 35 percent.

8. See, for example, Economic Policy Institute (2004).

9. Nothing in the conventional theory implies that employment of particular subgroups will decrease in response to a higher minimum wage; conventional theory only predicts that overall labor demand for less-skilled workers will fall. A particular subgroup for which the market wage was previously below the reservation wage can, after an increase, find the reverse and be drawn into the labor force. For example, Neumark and Wascher (1996) find that an increase in the minimum wage induces some higher-skilled teenagers to leave school and enter the labor market.

10. We want to focus on the period after which welfare reform began and, as explained below, we enter lagged values of the minimum wage

in some specifications. Thus, we start the sample period in 1997.

11. See http://www.stateeitc.com.

12. See http://www.urban.org/toolkit/databases/index.cfm.

13. In our exploratory work we looked at numerous dimensions of time limits, including their length, how long until they are first binding on at least part of the population, etc. Similarly, we examined the impact of work requirements with and without full sanctions in terms of reduced payments, etc. However, this exploration yielded little variation in effects, so here we simply report results for whether and when a state implemented time limits and whether the state imposed work requirements.

14. The coding of time limits is not completely unambiguous. In general, when a state has not implemented its TANF policy, and is still under AFDC, the time limit is coded as 0 (unless there is a lifetime limit under a waiver), because the benefits received do not count towards the federal or state TANF lifetime limit. When a state has implemented its TANF policy and has a "periodic limit," but not a more (or less) restrictive state lifetime limit, it is coded as 60 (the federal lifetime limit). When a state has both a periodic limit and a lifetime limit, it is coded as the lifetime limit. For example, Arizona is coded as 0 for 1996 because it had not yet implemented TANF and had a periodic limit, but did not have a lifetime limit under a waiver. It is coded as 60 (the federal lifetime limit) beginning in January 1997 because it only had a periodic limit, and not a state lifetime limit. New York is a special case. Following Keane and Fang (2004), it is coded as 0 beginning in August 1997 because, as indicated in the Urban Institute database, "Once individuals have reached the 60-month time limit, they are eligible to receive non-cash assistance through the Safety Net Assistance program beginning 8/97." Ohio is coded as 60 because it has a lifetime limit of 60 months. The Welfare Rules Database also indicates that there is a "benefit waiting period" such that individuals can receive benefits for 36 months, but must wait 24 months before they can receive additional benefits. Several other sources, including the website of the U.S. Department of Health and Human Services (HHS) (http://www.acf.hhs.gov/programs/ofa/TIME2.HTM, viewed May 15, 2006), indicate that for Ohio the state lifetime limit is 36 months, effective October 1997, though HHS also states that "24 months after reaching time limit, family may receive an additional 24 months of assistance if good cause exists." Virginia is coded as 60 because it has a lifetime limit of 60 months. It also has a benefit waiting period such that individuals can receive benefits for 24 months, but must wait 24 months to receive additional benefits, although this does not affect the lifetime limit.

15. Specifically, each observation comes from a particular state, month, and year. However, we cluster the data at the state level to compute standard errors robust to heteroscedasticity and arbitrary correlations across individuals in the same state either contemporaneously or over time (Bertrand et al., 2004).

16. For the time effects, we include a unique dummy variable for every month in the sample. In all cases, omitting the time effects led to much stronger adverse effects of minimum wages on employment and hours. However, the time effects were always jointly significant, and Hausman tests for excluding the time effects (based on changes in the estimated minimum wage effects) nearly always indicated that the time effects should be included, although the test statistics are not quite correct when the standard errors of the regression model deviate from the i.i.d. assumption, which is allowed for in computing robust standard errors.

17. Hausman tests for bias in the estimated minimum wage effects when the trends were

excluded tended to reject the exclusion of the state-specific trends, or at least to yield a low p-value, when the estimated minimum wage effects were sensitive to including the trends. The estimated coefficients of the state-specific trends were statistically significant, based on Wald tests. The need for state-specific trends is an indication that we have been unable to identify some important state-specific influences on employment and wage trends with our standard specification. One possibility is that our measure of welfare reform is inadequate. In particular, although the rules governing the TANF programs were set by 1998, implementation of those policies may have evolved over time. Each state is required to report annually on their success in meeting federally-specified targets, and the evidence suggests that states have moved toward those targets at different speeds. Results for the key specifications are reported without state-specific trends in Appendix Tables A1 and A2, which can be compared with estimates in Tables 5, 6, and 8 to see where the findings are sensitive to the inclusion of these trends; these results are discussed further below.

18. Note that we demean the average minimum wage variable in the interaction. This enables us to interpret the estimated coefficient on the EITC variable as the effect of the state EITC at the average minimum wage in the sample, and has no impact on the estimated coefficient of the interaction.

19. The analysis parallels their earlier (1998) paper.

20. The related issue of publication bias in estimated effects of minimum wages on employment is discussed in Card and Krueger (1995) and Neumark and Wascher (1998).

21. This paper also discusses trends in specific states; we focus only on the regression analysis.

22. This study also makes what we regard as misleading claims about what economists know about minimum wages, in particular "[T]here is no valid, research-based rationale for believing that state minimum wages cause measurable job losses" (p. 2). This claim, in our view, is based on a highly selective reading of the minimum wage literature based mainly on the New Jersey-Pennsylvania fast-food study (Card and Krueger, 1994). Although Chapman offers some critiques of our research on this study, and the overall conclusions, which the reader may want to consider, the literature on minimum wage effects in the United States is far broader than this one study, and clearly much of it does point to disemployment effects. (see Neumark and Wascher, 2006)

23. These calculations are based on January data, which the authors use for the payroll data.

24. These calculations are based on March data, for which the CBP data are reported.

25. An updated version of this study was released in 2006, using data through January 2006 (for the analysis of total and retail employment). But for some reason even though five additional large states increased their minimum wages by 2004 (Florida, Minnesota, New Jersey, New York, and Wisconsin), the "treatment group" of states with minimum wage increases is still treated as the 11 states that had raised their minimum by 2003. We therefore focus on the earlier study.

26. The education classifications are based on education attained, and whether the person reports a high school diploma or GED. We do not distinguish between the latter two cases, although there is evidence suggesting that this distinction is important for employment outcomes (e.g., Cameron and Heckman, 1993). Separate information on diploma and GED holders is first available in the CPS in 1998.

27. All wage, earnings, and minimum wage figures are nominal. The time effects in the regression models will account for aggregate nominal changes.

28. Appendix Table A1 reports the same set of employment elasticities from specifications omitting the state-specific trends. The results differ mainly for the smaller groups (minorities, those with less education, etc.). In general, though, there is still evidence of negative employment effects for minority males, and in this case also for all 16-19 and 20-24 year-olds, as well as non-black, non-Hispanics in these age groups. There is no evidence of disemployment effects for females.

29. Although past work has tended to emphasize the importance of lagged effects of minimum wages on employment, they can also be relevant to wage effects. There may be some implicit "give-back" in periods subsequent to minimum wage increases when other workers receive raises but those directly affected by a recent minimum wage increase do not (see the evidence in Neumark et al., 2004, for workers very near the minimum). Alternatively, there could be lagged positive effects, either because of a lag in compliance or because part of the response occurs as employers substitute towards higher-skilled workers, which bids up their wages. The estimates from specifications that include both contemporary and lagged effects tended to be somewhat larger than just the contemporaneous effects, but they were qualitatively similar.

30. Note that just as we should not necessarily expect employment declines for all groups in response to a higher minimum wage, we should not necessarily expect wage increases for all groups. A higher minimum wage could cause specific subgroups of the population to increase their employment rates, with the outward labor supply shift reducing wages of other groups (as well as their employment).

31. However, this contrasts with recent evidence on EITC effects based on Wisconsin's higher EITC supplement for families with three children (Cancian and Levinson, 2005).

32. In June 2004, only 6.7 percent of 15-19 year-olds had children; figures are not reported for 16-19 year-olds. (See www.census.gov/population/www/socdemo/fertility.html, viewed September 11, 2006).

33. The same source cited in the previous footnote indicates that the share of white women aged 20-24 with children was 29.2 percent, versus 43 percent for blacks and 47.2 percent for Hispanics.

34. Results for this last specification excluding the state-specific trends are reported in the top panel of Appendix Table A2. The positive effects of the EITC on employment persist for minority women and those with a high school education or less, although in this case the effect is also apparent for high school dropouts. However, the evidence of minimum wage-EITC interactions is weaker, and for those with a high school education or less, opposite in sign. And there is still virtually no evidence of gains from combining a high minimum wage and a high EITC.

35. These adverse effects of the EITC on wages of low-skill groups, which we also find for men below, parallel findings reported in Leigh (2004).

36. The middle panel of Appendix Table A2 reports the earnings results for women, excluding the state-specific trends. The evidence of positive EITC effects is weaker statistically than in Table 7, although qualitatively similar. The same is true of the evidence on minimum wage-EITC interactions, discussed below.

37. Again, Appendix Table A2 reports results for men's earnings excluding the state-specific trends. These results are much weaker, and in particular the negative effects on minority males are not apparent.

38. The same is true of specifications excluding the minimum wage-EITC interaction.

Table 1

Descriptive Statistics, 16-24 Year-Olds, 1997-2005

	Mean	Standard deviation	N
I*ndividual and state characteristics*			
Aged 16-19	.466	…	390,617
Aged 20-24	.534	…	390,617
Female	.504	…	390,617
Black	.149	…	390,617
Hispanic	.153	…	390,617
High school or less, 20-24	.445	…	200,116
High school dropout, 20-24	.136	…	200,116
State unemployment rate	.050	.011	390,617
Proportion of population aged 16 and over age:			
16-19	.079	.015	390,617
16-24	.172	.024	390,617
20-24	.093	.018	390,617
Labor market outcomes			
Wages, 16-19	6.74	2.86	59,346
Wages, 16-24	8.60	5.16	158,470
Wages, 20-24	9.61	5.81	99,124
Employed, 16-19	.397	…	190,501
Employed, 16-24	.550	…	390,617
Employed, 20-24	.684	…	200,116
Earnings, 16-19	55.95	110.69	171,934
Earnings, 16-24	135.04	212.60	332,637
Earnings, 20-24	213.13	255.84	160,703
Earnings, 16-19, earnings > 0	169.34	133.73	59,106
Earnings, 16-24, earnings > 0	288.85	228.59	158,152
Earnings, 20-24, earnings > 0	353.51	242.78	99,046
Policy variables			
Minimum wage	5.37	.55	390,617
Minimum wage, states above federal	6.31	.53	86,559
State EITC	.041	.085	390,617
State EITC, states with EITC	.164	.095	104,135
State EITC refundable, states with EITC	.8184	…	104,135
State EITC fully refundable, states with EITC	.8180	…	104,135

Standard deviations are reported for continuous variables. Earnings are weekly, and are computed as wages multiplied by hours, set to zero for those not working. Estimates are weighted. Observations using allocated data to construct wages, employment, or earnings are deleted. Individual and state statistics are shown for the full employment sample, which drops the fewest observations owing to allocated data. The unemployment rates are not seasonally adjusted figures from local area unemployment statistics (http://data.bls.gov/cgi-bin/dsrv, viewed April 5, 2006).

Table 2

Estimated Effects of Log Minimum Wages on Employment, 1997-2005

Sample	All			High school or less	High school dropout
Ages	16-19	16-24	20-24	20-24	20-24
Specification 1					
Log minimum wage	-.054*	-.019	.041	.092	.122
Specification 2: add state-specific linear time trends					
Log minimum wage	-.070	-.025	.030	.016	-.126
Specification 3					
Log minimum wage, lagged 1 year	-.023	-.014	.012	-.080	-.194
Specification 4					
Log minimum wage	-.073*	-.024	.031	.041	-.091
Log minimum wage, lagged 1 year	.010	-.003	-.003	-.098	-.152
Sum	-.063	-.027	.028	-.057	-.243
Employment elasticity	-.158	-.049	.041	-.088	-.455
N	190,501	390,617	200,116	87,915	25,887

Statistical significance is indicated by * (10-percent significance level), ** (five percent), and *** (one percent). Standard errors are clustered by state. All specifications include controls for the share of population in the group studied, the statewide unemployment rate, education (16 categories), black, Hispanic, marital status (7 CPS categories), state, and calendar year and month. Specifications 2-4 add state-specific trends. For the employment elasticity, the calculation is based on the summed effect, except in cases where either the contemporaneous or lagged effect was significant when included in isolation, but the summed effect was not, or when the contemporaneous or lagged effect alone was more strongly significant than the summed effect (based on the number of asterisks). In these cases, the calculation is based on the significant contemporaneous or lagged effect. Statistical significance of the elasticity reported corresponds to that of the coefficient estimate or sum of estimates from which it is calculated.

Table 3

Estimated Effects of Log Minimum Wages on Employment, Disaggregated by Race/Ethnicity

	Non-black, non-Hispanic			Black or Hispanic			Black			Hispanic		
Ages	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24
Specification 1												
Log minimum wage	-.081***	-.023	.034	.010	-.002	.065	.066	.127	.214**	-.066	-.090**	-.005
Specification 2: add state-specific linear time trends												
Log minimum wage	-.071	-.022	.017	-.067	-.022	.066	-.115	.043	.203**	-.049	-.094	-.075
Specification 3												
Log minimum wage, lagged 1 year	.029	.031	.049	-.171*	-.185**	-.194	-.155	-.148	-.175	-.127	-.249***	-.275*
Specification 4												
Log minimum wage	-.088	-.034	.005	-.035	.020	.122	-.093	.080	.264***	-.024	-.043	-.014
Log minimum wage, lagged 1 year	.067	.046	.047	-.153	-.196**	-.260**	-.107	-.191	-.322*	-.114	-.226**	-.268**
Sum	-.021	.012	.052	-.188**	-.176**	-.138	-.201	-.110	-.056	-.138	-.269**	-.282
Employment elasticity	-.048	.020	.072	-.664**	-.385**	-.226	-.836	-.275	-.104	-.426	-.491***	-.417*
N	143,215	292,959	149,744	47,286	97,658	50,372	23,677	46,367	22,690	24,284	52,574	28,290

See notes to Table 2.

Table 4

Estimated Effects of Log Minimum Wages on Employment, Disaggregated by Sex

	All			Non-black, non-Hispanic			Black or Hispanic			All, high school or less	All, high school dropout
Ages	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24	20-24	20-24
Males											
Specification 1											
Log minimum wage	-.070**	-.079***	-.034	-.078*	-.070**	-.034	-.075	-.097**	-.015	-.034	-.042
Specification 2: add state-specific linear time trends											
Log minimum wage	-.066	-.067	-.021	-.052	-.051	-.010	-.108	-.095	-.021	-.048	-.129
Specification 3											
Log minimum wage, lagged 1 year	-.124	-.103	-.063	-.087	-.051	.015	-.215	-.290***	-.370**	-.195	.016
Specification 4											
Log minimum wage	-.041	-.047	-.006	-.033	-.043	-.015	-.069	-.035	.068	.001	-.149
Log minimum wage, lagged 1 year	-.106	-.081	-.060	-.073	-.032	.022	-.178	-.271***	-.407***	-.195	.086
Sum	-.146	-.129	-.066	-.106	-.074	.007	-.247	-.306***	-.339*	-.194	-.063
Employment elasticity	-.376	-.228	-.092	-.246	-.124	.009	-.855	-.627***	-.552**	-.264	-.095
N	95,605	191,141	95,536	71,752	144,099	72,347	23,853	47,042	23,189	45,449	13,504
Females											
Specification 1											
Log minimum wage	-.040	.023	.086	-.088**	.009	.083	.082	.065	.103	.171**	.170
Specification 2: add state-specific linear time trends											
Log minimum wage	-.075	.002	.055	-.099	-.012	.031	-.012	.035	.113	.039	-.243
Specification 3											
Log minimum wage, lagged 1 year	.058	.059	.060	.124	.096	.064	-.164	-.100	-.028	-.006	-.522**
Specification 4											
Log minimum wage	-.100	-.014	.046	-.148*	-.041	.016	.025	.063	.134	.046	-.142
Log minimum wage, lagged 1 year	.102	.065	.039	.188*	.114	.057	-.177	-.132	-.099	-.027	-.460
Sum	.002	.051	.085	.040	.073	.072	-.152	-.069	.035	.019	-.601*
Employment elasticity	.006	.095	.131	.087	.124	.105	-.551	-.162	.064	.034	-.798**
N	94,986	199,476	104,580	71,463	148,860	77,397	23,433	50,616	27,183	42,466	12,383

See notes to Table 2.

Table 5		
Summary of Wage and Employment Results		
	Employment elasticity	Wage elasticity
Males and females combined		
16-19, all	-.158	.254***
16-24, all	-.049	.095
20-24, all	.041	.052
High school or less, 20-24	-.088	.258***
High school dropout, 20-24	-.455	.389***
16-19, non-black, non-Hispanic	-.048	.304***
16-24, non-black, non-Hispanic	.020	.103
20-24, non-black, non-Hispanic	.072	.012
16-19, black or Hispanic	-.664**	-.093
16-24, black or Hispanic	-.385**	.044
20-24, black or Hispanic	-.226	.174
16-19, black	-.836	.114
16-24, black	-.275	.138
20-24, black	-.104	.179
16-19, Hispanic	-.426	-.133
16-24, Hispanic	-.491***	-.042
20-24, Hispanic	-.417*	.166
Males		
16-19, all	-.376	.224***
16-24, all	-.228	.128**
20-24, all	-.092	.098
16-19, non-black, non-Hispanic	-.246	.386***
16-24, non-black, non-Hispanic	-.124	.164**
20-24, non-black, non-Hispanic	.009	.086
16-19, black or Hispanic	-.855	-.369**
16-24, black or Hispanic	-.627***	-.124
20-24, black or Hispanic	-.552**	.043
High school or less, 20-24	-.264	.233**
High school dropout, 20-24	-.095	.438**
Females		
16-19, all	.006	.286***
16-24, all	.095	.064
20-24, all	.131	-.032
16-19, non-black, non-Hispanic	.087	.250**
16-24, non-black, non-Hispanic	.124	.020
20-24, non-black, non-Hispanic	.105	-.143**
16-19, black or Hispanic	-.551	.207
16-24, black or Hispanic	-.162	.233
20-24, black or Hispanic	.064	.289*
High school or less, 20-24	.034	.247**
High school dropout, 20-24	-.798**	.434***

Results in first column are from Tables 2-4. Results in second column are from wage equations paralleling the employment specifications in Tables 2-4; see notes to Table 2. Observations with nominal wage below $1 are dropped. The specification for computing the wage elasticities was chosen in the same way as for the employment elasticities in Tables 2-4.

Table 6

Estimated Effects of Minimum Wages, EITC, and Welfare Reforms on Female Employment

	All			Non-black, non-Hispanic			Black or Hispanic			All, high school or less	All, high school dropout
Ages	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24	20-24	20-24
Baseline minimum wage elasticity (Table 4)	.006	.095	.131	.087	.124	.105	-.551	-.162	.064	.034	-.798**
Specification 1											
Log minimum wage	-.106	-.003	.070	-.153*	-.034	.034	.015	.086	.182	.073	-.095
Log minimum wage, lagged 1 year	.105	.059	.026	.191*	.110	.046	-.173	-.143	-.122	-.040	-.486*
State EITC (percentage supplement)	.003	.182***	.302***	.035	.170***	.276***	-.096	.224***	.397***	.446***	.397
Welfare time limits imposed	.012	-.003	-.015	.014	.001	-.009	007	-.013	-.032	-.007	-.031
Work requirements	.026	.016	.008	.023	.001	-.017	.027	.057	.093	.010	.127
Joint significance of welfare variables, p-value	.189	.693	.579	.131	.986	.665	.788	.506	.329	.956	.485
Minimum wage elasticity	-.002	.104	.148	.083	.130	.116	-.572	-.135	.108	.058	-1.503
Specification 2											
Average of log minimum wage and log minimum wage lagged 1 year	-.042	.033	.086	-.022	.044	.065	-.088	-.006	.111	.035	-.520
Minimum wage elasticity	-.104	.062	.133	-.048	.075	.094	-.317	-.015	.200	.062	-1.345
Specification 3											
Average of log minimum wage and log minimum wage lagged 1 year	.030	.069	.085	.014	.075	.088	.046	.031	.060	.076	-.363
State EITC (percentage supplement)	-.087	.137**	.310***	.018	.140*	.243***	-.367*	.160*	.533***	.394***	.119
Log minimum wage variable x EITC	-1.127**	-.451	.213	-.514	-.388	-.216	-2.411**	-.441	1.314	-.414	-2.367
Minimum wage elasticity, no state EITC	.074	.129	.131	.031	.129	.127	.166	.072	.109	.135	-.941
Minimum wage elasticity, 10% state EITC supplement	-.205	.045	.164	-.081	.062	.096	-.706	-.032	.345	.062	-1.553*
Minimum wage elasticity, 25% state EITC supplement	-.623*	-.081	.213	-.249	-.038	.049	-2.015**	-.187	.700**	-.048	-2.471***
N	94,896	199,476	104,580	71,463	148,860	77,397	23,433	50,616	27,183	42,466	12,383

See notes to Table 2. Specifications include the same control variables as specification 4 in tables 2-4. In the interactive specifications, the interaction is between the minimum wage variable minus its mean and the EITC. This way, the non-interacted EITC coefficients measure the effects of the EITC evaluated at the mean minimum wage; the transformation has no effect on the interactive coefficients.

Table 7

Estimated Effects of Minimum Wages and the EITC on Female Wages and Earnings

	All			Non-black, non-Hispanic			Black or Hispanic			All, high school or less	All, high school dropout
Ages	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24	20-24	20-24
Female Wages											
Log minimum wage	.136*	.109	.059	.157	.128	.070	-.046	.053	.031	.110	.508***
Log minimum wage, lagged 1 year	.152*	-.046	-.095	.094	-.107*	-.172**	.251*	.175	.271*	.136	-.273
State EITC (percentage supplement)	.096	-.037	-.133	.085	.048	-.004	.125	-.250**	-.408**	-.039	.245
Minimum wage elasticity	.287***	.063	-.035	.252**	.021	-.103	.206	.228	.301	.247*	.235
N	30,574	80,386	50,262	25,774	65,505	39,761	4,830	15,331	10,501	17,943	3,668
Female Earnings											
Specification 1											
Average of log minimum wage and log minimum wage lagged 1 year	7.34	-10.10	-28.44	10.85	-31.66	-82.20	-7.90	39.09	111.12*	29.72	-90.55
State EITC (percentage supplement)	24.44*	44.55***	47.11	31.09	48.67***	53.83	3.27	38.43	39.77	110.15*	141.55*
Minimum wage elasticity	.141	-.086	-.158	.185	-.242	-.416	-.212	.437	.797	.213	-1.167
Specification 2											
Average of log minimum wage and log minimum wage lagged 1 year	19.61	-11.55	-40.94	21.98	-28.43	-83.64	5.26	28.54	79.45	34.83	-48.87
State EITC (percentage supplement)	4.19	47.02**	68.92*	14.23	43.73	56.05	-22.98	61.00**	111.28**	100.63	66.71
Log minimum wage variable x EITC	-198.25**	23.97	210.48	-173.46	-51.47	23.51	-231.48*	188.76	577.11**	-95.49	-744.64*
Minimum wage elasticity, no state EITC	.378	-.098	-.227	.376	-.218	-.423	.141	.319	.570	.249	-.630
Minimum wage elasticity, 10% state EITC supplement	-.004	-.078	-.111	.079	-.257	-.411	-.479	.531	.984*	.181	-1.589*
Minimum wage elasticity, 25% state EITC supplement	-.577	-.047	.065	-.366	-.316	-.393	-1.409*	.847*	1.606**	.078	-3.028***
N	85,698	171,741	86,043	63,883	127,254	63,371	21,815	44,487	22,672	36,040	11,214

See notes to Tables 2 and 6. Earnings are the product of wages and hours, and are set to zero if hours are zero.

Table 8

Estimated Effects of Minimum Wages and the EITC on Male Wages and Earnings

	All			Non-black, non-Hispanic			Black or Hispanic			All, high school or less	All, high school dropout
Ages	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24	20-24	20-24
Male Wages											
Average of log minimum wage and log minimum wage lagged 1 year	.215***	.115	.106	.395***	.145	.036	-.523**	.019	.247	.268**	.603
State EITC (percentage supplement)	.180	.025	-.088	.153	.129	.084	.267	-.546**	-.898***	.233	-.052
Log minimum wage variable x EITC	-.099	-.505	-.694	-.148	.200	.478	.029	-3.823*	-5.315**	-.170	-1.697
Minimum wage elasticity, no state EITC	.215***	.115	.106	.395***	.145	.036	-.523	.019	.247	.268**	.520**
Minimum wage elasticity, 10% state EITC supplement	.205**	.065	.037	.380***	.165	.083	-.520*	-.363	-.284	.251**	.434**
Minimum wage elasticity, 25% state EITC supplement	.190	-.011	-.067	.358	.195	.155	-.516	-.937*	-1.081*	.226	.179
N	28,772	77,634	48,862	23,738	61,836	38,098	5,034	15,798	10,764	23,703	6,537
Male Earnings											
Specification 1											
Average of log minimum wage and log minimum wage lagged 1 year	-9.87	-22.61	-5.06	21.88	20.31	48.96	-89.02***	-123.77*	-122.77	1.51	-20.32
State EITC (percentage supplement)	-3.92	38.77	87.71	-12.06	84.58	173.86	18.20	-58.13	-128.52**	139.81	60.98
Minimum wage elasticity	-.165	-.148	-.020	.330	.122	.182	-1.964	-1.023	-.607	.006	-.102
Specification 2											
Average of log minimum wage and log minimum wage lagged 1 year	-19.69	-30.63	-4.60	12.77	-1.68	17.82	-97.98***	-91.12	-39.05	-22.96	-36.75
State EITC (percentage supplement)	12.95	52.22	86.98	1.02	116.17	218.56*	39.85	-134.38	-311.07***	183.81	86.56
Log minimum wage variable x EITC	155.94	129.21	-7.38	138.24	337.06	481.77	158.64	-594.58	-1575.24**	425.58	282.43
Minimum wage elasticity, no state EITC	-.329	-.200	-.018	.193	-.010	.066	-2.161***	-.753	-.193	-.091	-.185
Minimum wage elasticity, 10% state EITC supplement	-.068	-.116	-.021	.401	.192	.245	-1.811**	-1.245**	-.971	.078	-.043
Minimum wage elasticity, 25% state EITC supplement	.322	.011	-.026	.714	.496	.514	-1.286	-1.983**	-2.139**	.331	.170
N	86,236	160,896	74,460	64,257	120,884	56,627	21,979	40,012	18,033	35,313	10,971

See notes to Tables 2 and 6.

Appendix Table A1

Estimates of Employment Effects Omitting State-Specific Trends

	Employment elasticity without trends
Males and females combined	
16-19, all	-.095
16-24, all	-.023
20-24, all	.059
High school or less, 20-24	.090***
High school dropout, 20-24	.137
16-19, non-black, non-Hispanic	-.150**
16-24, non-black, non-Hispanic	-.025
20-24, non-black, non-Hispanic	.057
16-19, black or Hispanic	.090
16-24, black or Hispanic	-.009
20-24, black or Hispanic	.079
16-19, black	.524
16-24, black	.353*
20-24, black	.349*
16-19, Hispanic	-.179
16-24, Hispanic	-.175*
20-24, Hispanic	-.007
Males	
16-19, all	-.174*
16-24, all	-.085**
20-24, all	-.049
16-19, non-black, non-Hispanic	-.189*
16-24, non-black, non-Hispanic	-.128**
20-24, non-black, non-Hispanic	-.058
16-19, black or Hispanic	-.243
16-24, black or Hispanic	-.230***
20-24, black or Hispanic	-.072
High school or less, 20-24	-.085
High school dropout, 20-24	-.020
Females	
16-19, all	-.029
16-24, all	.074
20-24, all	.149
16-19, non-black, non-Hispanic	-.055
16-24, non-black, non-Hispanic	.053
20-24, non-black, non-Hispanic	.147
16-19, black or Hispanic	.361*
16-24, black or Hispanic	.160*
20-24, black or Hispanic	.176
High school or less, 20-24	.322**
High school dropout, 20-24	.382

All elasticities are from specification 4 in Tables 2-4, with the exception of excluding the state-specific trends.

Appendix Table A2

Estimates of Welfare Reform and EITC Effects Omitting State-Specific Trends

	All			Non-black, non-Hispanic			Black or Hispanic			All, high school or less	All, high school dropout
Ages	16-19	16-24	20-24	16-19	16-24	20-24	16-19	16-24	20-24	20-24	20-24
Female Employment											
Average of log minimum wage and log minimum wage lagged 1 year	-.005	.050	.106*	-.058	.036	.107	.106*	.087**	.118**	.198***	.193
State EITC (percentage supplement)	.009	.109***	.179***	.022	.067	.106	-.008	.187**	.297***	.372***	.713***
Log minimum wage variable x EITC	-.281	-.141	.075	-.044	-.086	-.023	-.473	-.199	.270	.171	-.081
Minimum wage elasticity, no state EITC	-.011	.093	.163*	-.125	.061	.155	.383*	.205**	.212**	.349***	.498
Minimum wage elasticity, 10% state EITC supplement	-.081	.067	.174*	-.135	.046	.152	.211	.158	.261*	.379***	.478
Minimum wage elasticity, 25% state EITC supplement	-.185	.028	.124	-.150	.024	.147	-.045	.088	.334	.425**	.446
Female Earnings											
Average of log minimum wage and log minimum wage lagged 1 year	-1.71	4.95	18.36	-11.03	2.77	17.19	19.03	15.99	34.44	55.48***	53.90
State EITC (percentage supplement)	6.48	31.15	44.39	8.42	32.33	50.38	.04	21.22	19.62	96.33**	137.37*
Log minimum wage variable x EITC	-43.75	100.56	265.10	-4.85	81.82	193.35	-84.46	115.90	355.47	195.63*	-232.70
Minimum wage elasticity, no state EITC	-.033	.042	.102	-.188	.021	.087	.509	.179	.247	.397***	.694
Minimum wage elasticity, 10% state EITC supplement	-.117	.127	.249	-.197	.084	.185	.283	.309	.502*	.537***	.395
Minimum wage elasticity, 25% state EITC supplement	-.243	.255	.470	-.209	.178	.331	-.056	.503	.885	.747***	-.055
Male Earnings											
Average of log minimum wage and log minimum wage lagged 1 year	4.67	20.45	68.00***	8.31	29.33	71.13**	-7.96	.98	68.54**	61.10	68.86
State EITC (percentage supplement)	17.69	-11.05	22.75	-22.06	6.67	54.93	8.13	-34.36	-45.91	66.39	-61.25
Log minimum wage variable x EITC	45.15	-77.82	-237.13	95.46	-20.52	-140.44	-1.51	-192.37	-568.58	-281.86	-268.64
Minimum wage elasticity, no state EITC	.078	.134	.273***	.125	.176	.264	-.175	.008	.339	.242	.346
Minimum wage elasticity, 10% state EITC supplement	.153	.083	.178	.269	.164	.212	-.179	-.151	.058	.130	.211
Minimum wage elasticity, 25% state EITC supplement	.266	.007	.035	.485*	.145	.134	-.184	-.390	-.363	-.037	.009

See notes to Tables 6 and 8.

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