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Does The Minimum Wage Affect Welfare Caseloads?

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Abstract

Although minimum wages are advocated as a policy that will help the poor, few studies have examined their effect on poor families. This paper uses variation in minimum wages across states and over time to estimate the impact of minimum wage legislation on AFDC caseloads, thus directly assessing whether minimum wages benefit a group they are intended to help. We find that the elasticity of the welfare caseload with respect to the minimum wage is about 0.15. This suggests that minimum wages are not an efficient policy for facilitating the transition from welfare to work.

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The degree to which minimum wages affect employment has been of interest to economists and policy makers for many years. This interest stems largely from an inconsistency between the intent of minimum wage laws and their theoretical effects: the goal of minimum wages is to improve individuals' abilities to support their families and avoid welfare (see, for example, Ellwood, 1988), but the textbook model of supply and demand predicts that such wage gains come at the expense of lower employment levels (Brown, 1988). In order for minimum wages to improve the well-being of families overall, the demand curve for low-skilled workers must be relatively inelastic.

Although many studies have estimated the relationship between minimum wage increases and employment rates,[1] the bulk of the literature has focused on understanding how teenagers are affected. Less attention has been paid to the impact of minimum wage increases on poor adults, and even fewer studies have considered the potential relationship between minimum wages and welfare recipiency. A significant number of potential recipients could be affected by minimum wage policies, however. In 1996, for example, about 14% of unmarried female household heads with children had wages that were between the 1996 federal minimum wage and the 1997 federal minimum wage.[2]

If higher minimum wages raise the earnings of low-skilled single mothers, then they may help to reduce the number of women who require welfare benefits in order to support their families. The minimum wage also may reduce welfare caseloads by increasing the return to work and thus encouraging welfare recipients to enter the labor market. In 1992, the federal minimum wage was \$4.25/hour, the median AFDC worker earned \$4.50/hour and the 25th percentile AFDC worker earned \$3.45/hour, which suggests that a significant fraction of working welfare recipients are willing to accept jobs for wages near the minimum.[3] However, if minimum wages reduce the demand for workers with limited skills, then they may unintentionally lead to increases in the number of women who participate in welfare programs.

Recent passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) intensifies the need to understand the relationship between work and welfare. PRWORA limits the maximum number of years an adult can receive welfare benefits to five years (although states may opt to impose a stricter time limit), and penalizes welfare recipients who do not find work (or an approved substitute for work) within two years of initiating welfare benefits. At nearly the same time as Congress passed PRWORA, the federal government increased the national minimum wage and several states raised their minimum wages above the new federal level. The ostensible reason for these increases was to reduce individuals' need for welfare benefits by increasing the return to work. In fact, we have no idea whether higher minimum wages will help welfare recipients comply with the new welfare law.

In this study, we use variation in minimum wages across states and over time to identify their effect on the size of the state welfare caseload. Our empirical results indicate that, once state trends and a variety of other factors are accounted for, the elasticity of the welfare caseload with respect to the minimum wage is between 0.1 and 0.2. In other words, a 35 percent increase in the minimum wage like that recently implemented in California could lead to a 3 to 7 percent increase in the size of the welfare caseload, holding all else equal. These results suggest that minimum wages are not an efficient means of improving the financial independence of low-skilled single mothers, since the wage gains experienced by those who keep their jobs are counteracted by an increase in the welfare rolls. Policies like the Earned

Income Tax Credit, which increases wages through the tax code without depressing the demand for low-skill labor, are likely to be more effective in facilitating the transition from welfare to work.

In the next section we briefly describe the various mechanisms by which the minimum wage might be expected to affect the probability of becoming a welfare recipient and review the related empirical literature. In Section II we lay out our empirical framework and discuss the data that we use to estimate the relationship between minimum wage levels and welfare caseloads. Section III presents our results. Section IV examines the robustness of our estimates to alternative specifications and omitted variables, and Section V concludes with policy implications.

I. Background

Conventional neoclassical theory predicts a decline in the demand for low-skilled labor in response to an increase in the minimum wage. Since welfare spells are frequently precipitated by employment loss, a fall in job opportunities should lead to a rise in the number of welfare recipients.[4] Many recent studies of welfare dynamics (e.g. Blank, 1997; Council of Economic Advisors, 1997, 1999; Figlio and Ziliak, 1999; Grogger, 2000; Hoynes, 2000; Moffitt, 1999; Schoeni and Blank, 2000; Wallace and Blank, 1999; Ziliak, Figlio, Davis and Connolly, 2000) confirm that welfare participation increases when labor demand is low.

There are a number of reasons, however, that welfare participation rates might be unaffected when the minimum wage increases. First, although the competitive model predicts a fall in employment when the minimum wage rises, it has nothing to say about the magnitude of this decline. Empirical studies of the relationship between minimum wages and employment generally produce elasticity estimates that range from 0 to -0.3. A few studies have even produced positive elasticity estimates.[5] If the employment effects of minimum wage policies are negligible, then the elasticity of the welfare caseload with respect to the minimum wage also may be negligible.

A second reason that caseloads might be unresponsive to minimum wage increases is that many welfare recipients work in jobs that are "off the books" (see Edin and Lein (1997)). We find that more than 30% of working AFDC recipients in the 1993 CPS reported hourly wages that were more than 10% below the minimum. The employment of women who work in such jobs is unlikely to be jeopardized by minimum wage increases, even if such policies reduce employment among other workers.

If the job opportunities facing potential welfare recipients are not substantially reduced when minimum wages rise, then such policies could lead to reductions in welfare caseloads. Those individuals previously working at or near the minimum wage almost certainly will experience an increase in earnings when the minimum wage is increased, and Bane and Ellwood (1994) find that up to 25% of AFDC exits are related to earnings increases. An increase in potential earnings also may induce some welfare recipients who would not otherwise look for work to become active members of the labor force. Studies of the Earned Income Tax Credit (e.g., Dickert, Houser and Scholz,1995; Eissa and Liebman 1996; Meyer and Rosenbaum, 2001) indicate that exogenous wage increases positively affect single mothers' probabilities of working. The earnings generated from full-time work at the minimum wage typically hover close to the median state's AFDC guarantee for a family of three. As a result, fluctuations in the minimum wage could substantively affect the trade-off between work and welfare. Finally, if the labor market for low-skilled workers is not competitive but is instead dominated by monopsonists, then increases in the minimum wage may lead to an increase in the demand for labor, which would subsequently reduce the welfare caseload.

Although the potential effect of minimum wages on welfare participation has been largely unexplored by empirical researchers, a few recent studies have estimated the relationship between minimum wages and other measures of well-being among the poor. Connolly and Segal (1997) use changes in the federal minimum wage in 1990 and 1991 to estimate its impact on the earnings of poor working families. They compare outcomes across states with different fractions of affected workers and find that, in states where the increase should have had the biggest impact, poor and near-poor families experienced significantly higher growth in household earnings. In contrast, Addison and Blackburn (1996) do not find any evidence of a relationship between minimum wages and poverty. In a similar but more detailed study, Neumark and Wascher (1997) use CPS surveys matched over consecutive years to examine whether changes in family income are associated with state-level changes in minimum wages. They conclude that minimum wages effectively increase the incomes of some poor families, but that employment-reducing effects of minimum wages hurt other poor and near-poor families. A related paper with similar findings (Neumark, Schweitzer and Wascher, 1998) uses matched CPS surveys to estimate the effects of minimum wages on the distribution of income to needs.

Two papers use simulations to measure the effect of minimum wages on poor families. Horrigan and Mincy (1993) simulate the effects of the minimum wage on income inequality. Controlling for disemployment effects, incomplete coverage, and compliance, they find that family income inequality would have been no different if the minimum wage had kept pace with inflation during the 1980's. MaCurdy and O'Brien-Strain (1997) simulate the benefits and costs of a recent change in California's minimum wage, noting that the costs of minimum wages must either reduce the profits of employers or be passed on to consumers in the form of higher prices for goods and services. When they assume that the minimum wage has no effect on employment, they find that low-income households pay a disproportionate share of these higher costs.

The purpose of this study is to more narrowly focus on estimating the impact of minimum wage legislation on welfare caseloads. To our knowledge only two other papers have addressed this question. Brandon (1995) and Turner (1999) both use the Survey of Income and Program Participation to estimate the effect of minimum wage increases on the probability of exiting AFDC, and they reach opposite conclusions. Their studies are based on only a few years of survey data (1986-1988 (Brandon) and 1990-1991 (Turner)), however, and Baker, Benjamin and Stanger (1999) have convincingly argued that minimum wage effects may be hidden in short panels. Three additional studies incorporate the minimum wage into analyses of welfare caseloads (CEA, 1999; Grogger, 2000, 2001), although understanding the effects of minimum wage policy is not their focus. These studies also yield mixed findings, with the CEA study producing negative, statistically significant coefficient estimates and the Grogger papers finding a positive relationship.[6] Since neither study is driven by an interest in minimum wage policy, however, the reason for their different findings has not been explored.

II. Data and Empirical Model

In the previous section we discussed a number of reasons that we are unable to predict from theoretical models whether minimum wages will increase or decrease welfare caseloads. The answer to this question must be obtained empirically. We begin our analysis by estimating a reduced form model that is motivated by the related literature on minimum wages and teenage employment:

$$C_{st} = bMW_{st} + X_{st}d + \sum_{s=1}^{s=50} a_s + \sum_{t=1}^{t=T} g_t + \sum_{s=1}^{s=50} w_s t + e_{st} \tag{1}$$

where C_{st} is the logarithm of the average monthly per capita AFDC-Basic caseload[7] in state s and year t (caseload divided by the number of women aged 15-55), MW_{st} is the logarithm of the minimum wage in state s and year t, X_{st} is a set of control variables that vary across states and over time, a_s is a time-invariant state-specific effect, g_t is a year-specific effect (we also present specifications that include a quartic time trend), and $w_s t$ is a state-specific linear time trend. The sign of $\hat{b}$ will tell us whether supply or demand effects dominate the response to minimum wage increases.

We use the AFDC caseload as our dependent variable instead of the number of AFDC recipients for two reasons. First, many recipients are children and we wish to distinguish between the number of households that are affected (and thus the number of adult recipients whose labor market opportunities are altered) from differences in household fertility across states. Another reason for choosing the caseload variable is that in the wake of welfare reform, this statistic has received more attention from policymakers.

The vector X_{st} includes a number of state and time-varying variables that may be correlated with both minimum wage and caseload levels. We include these variables in order to reduce the potential for omitted variables bias. For example, because a state's economic conditions may influence its minimum wage policies as well as the number of women who require AFDC for support, we include the logarithm of the real value of average production wage, the log of real gross state product, and the state unemployment rate. X_{st} also includes two lagged values of the unemployment rate because a number of recent studies have found that caseload fluctuations are a function of employment opportunities in previous periods.

Blanchard and Katz (1992) present evidence that individuals migrate to states experiencing economic booms; those who migrate to take advantage of improved labor market conditions are unlikely to be welfare participants. In light of this, we increase the flexibility of our model by moving the denominator of C_{st} to the right side of the equation, so that log (population) is treated as an explanatory variable. Note, however, that this specification does not change our interpretation of b, which is the elasticity of the per capita caseload with respect to the minimum wage.

Our vector of control variables includes average production wages because the demand for minimum wage workers is likely to depend on their wage relative to the wages of other workers in the state. The production wage is not an ideal measure of the market-clearing wage for adults who are at risk of becoming welfare recipients because the majority of these adults are single women, and women are more likely to be employed in the service sector. This is the only average wage series that is available both over time and across states, however, and it should be highly correlated with wages in more relevant occupations. We shall examine the sensitivity of our results to alternative wage measures that we calculated ourselves using the CPS.

X_{st} also includes several variables that partially account for differences in welfare take-up rates across states and over time. The probability of participating in a welfare program is affected by the level of benefits available (Moffitt, 1992). Moreover, residents' preferences for benefit levels may be correlated with their tastes for minimum wages. Thus, we add to our equation measures of the generosity of each state's welfare benefits. We include the total value of AFDC benefits plus Food Stamp benefits for a family of three with no income and a dummy variable that indicates whether the state maintains an AFDC-Unemployed Parent program. While individuals could not simultaneously be eligible for both AFDC-UP and AFDC-Basic, the presence of an AFDC-UP program might affect participation in the Basic program by increasing AFDC's visibility. Our inclusion of this dummy variable may also pick up differences in take-up rates related to state residents' attitudes regarding welfare. For example, in states where voters support an AFDC-UP program, the stigma associated with welfare participation rates may be relatively low. Our equation also controls for the fraction of households that are headed by a single mother,[8] and the fraction of women between the ages of 15 and 55 who are white.

The model is estimated using state-level panel data that span the years from 1983 to 1996.[9] Panel data allow us to control for both national time trends and static differences across states that might be correlated with both minimum wage levels and the size of the welfare caseload. We base our analysis on state-level caseload data instead of an individual-level survey dataset because this enables us to increase the efficiency of our minimum wage estimates.

Since the policy of interest varies across states, estimates based on individual-level data will be equivalent to estimates generated by state-level regressions where the dependent variable is an *estimate* of the state caseload. Even with large datasets such as the Current Population Survey, estimates of state caseloads are likely to be very imprecise for small states because there are so few observations on which to base the estimate.[10] [11] [12] We are able to eliminate measurement error in the dependent variable by using the *actual* caseload instead of an estimate.

There are several reasons that we chose 1983-1996 as the years for our analysis. First, as shown in Table 1, minimum wages display virtually no variation across states prior to 1985, whereas after 1987 there is substantial variation in minimum wages both across states and within states over time. If data from earlier years were added to the sample, then the identification of minimum wage effects would increasingly rest on time-series variation in the federal minimum wage. Most of this variation is eliminated when we include time dummies (or time-trends) in our equation.[13] A second reason we chose 1983-1996 is that 1996 marks the beginning of PRWORA. Although caseloads began to decline prior to passage of PRWORA, in the years that followed the caseload decline was particularly dramatic. It is generally believed that trends since 1994 have been driven by some combination of welfare reform and a robust economy, but a full explanation remains elusive. We were concerned that, by including data after 1996, the identification of minimum wage effects would become muddled with other factors driving the caseload decline. Our final reason for choosing 1983-1996 is that several of our control variables are not available prior to the 1980s or after 1996. Nevertheless, we have estimated models that include a subset of our control variables through the year 1999 and discuss the results in Section IV.

Appendix A provides details on the sources of our data and Appendix Table A.1 presents their means and standard deviations. We use the CPI to adjust all monetary variables to 1994 dollars. The data set contains 700 observations.

III. Results

We began our analysis by imposing as few restrictions on the data as possible, and then systematically placed more structure on the model until we arrived at equation (1). For details on the alternative specifications that we considered see Appendix B. Because equation (1) includes both state and time fixed effects and a state specific time trend, the identification of our estimates is coming from variation in minimum wages around a state-specific trend. Given that the real minimum wage typically trends downward over time and then increases abruptly with new legislation, the source of our identification seems appropriate.

Table 2 presents four different sets of results. All standard error estimates are corrected for heteroskedasticity and serial correlation using a procedure suggested by Newey and West (1987).[14] [15] In the first two columns we report estimates that are based on equation (1), and in the second two columns we present estimates based on a model that replaces the year-specific dummy variables with a quartic time trend. The advantage of the first specification is that it allows us to implicitly control for all unobserved time-specific factors that are common across states. The disadvantage of this specification is that it wipes out our ability to use changes in the federal minimum wage as a source of identification. When we include both federal minimum wage and the state minimum wage variables together in the model with a quartic time trend, the coefficient estimate on the state minimum wage variable is reduced by about one third, which suggests that variation in the federal minimum wage is providing some of the identification. As shown in Figure 1, predicted caseloads based on the models that include year-specific effects and the quartic trend are very similar. The quartic trend does a good job of tracing out the caseload pattern over time, but still allows changes in federal minimum wage legislation to contribute to the identifying variation. We remain agnostic about whether the gains from this additional flexibility outweigh the potential omitted variables problems, and present both sets of results. They are very similar.

For each specification, we present both unweighted OLS estimates and weighted least squares estimates, where the observations for each state are weighted by the population of women whom are between the ages of 15 and 55. Card, Katz and Krueger (1994) and Krueger (1995) have shown that weighting can have a significant impact on estimated employment elasticities. If one wants to estimate the degree to which minimum wage legislation will contribute to an overall rise or fall in U.S. welfare participation probabilities, then weighting is appropriate. California and New York, for example, together comprise almost 30% of the U.S. caseload, so caseload changes in those states will contribute a great deal to changes in the national caseload. On the other hand, big states and small states are contributing equal amounts of information to our analysis, so our WLS regressions may place too much emphasis on large states.

The estimates in Table 2 indicate that increasing the minimum wage by 10% will lead to a one- to two-percent increase in the size of the welfare caseload. To put this estimate into perspective, these results suggest that in California, where minimum wages were recently increased from $4.25 to $5.75, we would expect welfare caseloads to rise by between 3 and

7%, holding everything else constant. Of course, PRWORA went into effect at approximately the same time that California's minimum wage rose, and the new law places time limits on welfare recipiency and eliminates welfare as an entitlement. These restrictions make it harder for low income families to rely on welfare when employment opportunities are scarce, and so TANF caseloads may be less affected than AFDC caseloads by changes in demand resulting from changes in the minimum wage.[16]

Our point estimates imply that the disemployment effects of the minimum wage are significantly larger for single mothers than for other demographic groups. In 1995, there were approximately 9 million single parent families in the United States;[17] Blank (1997) estimates that approximately one quarter of these families contained a working adult, but were still poor. If we assume that 2% of working single mothers lose their jobs as a result of a 10% increase in the minimum wage (empirical studies of the relationship between minimum wages and teenage employment have produced elasticity estimates that range from 0 to –0.3) and that about half of these women then become AFDC recipients (take-up rates are estimated to be about 50%), then AFDC caseloads will rise by about 23,000. This would constitute a caseload increase of about 0.5%. Since our estimates run between one and two percent, they imply larger employment elasticities than have been estimated for teenagers. The confidence intervals around our point estimates are quite large, however, and include magnitudes that are consistent with previously estimated effects for teenagers.

The other coefficient estimates presented in Table 2 are either of the expected sign or are indistinguishable from zero. Unsurprisingly, we find that good economic conditions are associated with lower caseloads, and that more generous AFDC benefits are associated with higher caseloads. Like previous studies of caseload dynamics we find that employment demand in previous periods affects the current caseload. Our estimated coefficients on these variables are similar in magnitude to Blank (1997). We also note that periods of high population growth (relative to trend) are associated with reductions in welfare caseloads. This is consistent with Blanchard and Katz (1992), who find evidence of state-to-state migration in response to changes in labor market conditions.

IV. Robustness of the Estimates

Alternative Model Specifications

The remainder of this paper is devoted to investigating the robustness of our estimates. We consider alternative model specifications and possible omitted variables biases, and we reconcile our estimates with the minimum wage estimates produced by other studies. In order to simplify the presentation we carry forward our analysis using the model in column 2 of Table 2, but the minimum wage estimates are robust across all of the specifications except that estimates produced by the unweighted model with year-specific effects lose some of their magnitude and their statistical significance in some of the models that include state-specific business cycle effects.

Table 3 shows how our estimates of minimum wage effects change when we change the way that the model is specified. First, we consider the possibility that our linear state-specific time trends are too restrictive. When we add quadratic state-specific time trends to our basic equation estimates of the minimum wage's effect are somewhat reduced, but they continue to be statistically significant. Next, we estimate the effect of the minimum wage *level* on welfare caseloads. Although it is convenient to think about minimum wage effects in terms of elasticities, one might argue that a logarithmic specification incorrectly implies that an increase in the minimum wage from $1 to $2 would have the same percentage effect on welfare caseloads as a wage increase from $3 to $6. Nonetheless, the estimate in column 2, which indicates that raising the minimum wage by $1 would increase welfare caseloads by 4%, is similar to our elasticity estimate.

Klerman and Haider (2001) convincingly argue that standard fixed effects models applied to welfare stock data (such as the model used in this paper) are likely to be misspecified, and that this misspecification may lead to misleading results when evaluating a policy change. The misspecification occurs because the flows into and out of welfare follow different processes, and because welfare receipt in one period is dependent on welfare receipt in previous periods, even conditional on covariates. This means that even if the flows are functions only of contemporaneous variables, the caseload stock will be a non-linear function of both current and lagged variables.

Ideally, one should independently estimate the effect of the policy on the flow into welfare and the flow out of welfare and then use the estimates to obtain estimates of the policy's effect on the caseload. Unfortunately, flow data are not available at the national level and so such an approach is not feasible. Klerman and Haider's next best alternative is to include lagged values of the policy variable as well as interactions between it and all of its lagged and contemporaneous values. The results from this exercise are presented in the next two columns of Table 3. We present the coefficient estimates on lagged and interacted minimum wage variables, and use these estimates to calculate estimates of the long-run minimum wage elasticity.[18] In column 3, the long-run elasticity is calculated by summing the minimum wage coefficients. The estimate is 0.24. Calculation of a long-run elasticity estimate when there are interactions requires more assumptions and so in column 4, we calculate the percentage increase in the caseload that would occur if the minimum wage were raised 1% above its mean value of $4.40. The resulting estimate is 0.20. Taken together, columns 3 and 4 provide no evidence that our inability to model the flows into and out of welfare have led to upward biased minimum wage estimates.

Omitted Variables

One concern that has been elucidated in the minimum wage literature is that the timing of minimum wage increases corresponds to periods of economic growth, when employment levels would be expected to rise. If this hypothesis is true, then the coefficients presented in Table 2 will be biased downward and can be thought of as a lower bound of the true minimum wage effect. Conversely, if minimum wages are legislated during downturns in the economy, then the estimates in Table 2 will be too high. In columns 1, 2 and 3 of Table 4, we allow for the possibility that state heterogeneity in business cycle responses is correlated with state minimum wage changes by adding state interactions with the unemployment rate, production wage and gross state product variables to our equation. The magnitude of the estimated minimum wage coefficient continues to lie between 0.1 and 0.2. Note, however, that the estimates are generally smaller than those in produced by the basic equation in Table 2. This raises the possibility that, even though we include a much richer set of economic controls than do most studies of the minimum wage, our results (and the results of most employment studies) may still be biased upward by our inability to *completely* control for economic factors specific to the state in time period t. Unfortunately, we know of no minimum wage studies that have used a convincing instrument to get around this problem. Neumark and Wascher (1992) instrument the minimum wage with the average minimum wage in surrounding states, but in our model this instrument is either uncorrelated with the minimum wage or correlated with the dependent variable, depending on the specification we use. The potential endogeniety of the minimum wage deserves additional investigation.

With the exception of the demographic variables included in equation (1), our empirical equation follows models that have been used to estimate the relationship between minimum wages and teenage employment rates. Like those studies, we control for a large number of economic variables because we want to be as confident as possible that we are distinguishing between caseload changes that result from the impact of minimum wages on the demand for low-skilled workers and changes that arise from other economic conditions that also affect the caseload. We next consider the possibility that other factors that are correlated with minimum wage changes are driving our results. The inclusion of these additional variables is motivated by recent studies of caseload dynamics, particularly those by Blank (1997) and Wallace and Blank (1999).

To date, most caseload studies have estimated the effect of welfare reform on caseload size using information on state welfare waivers. Many states obtained waivers from federal rules governing the AFDC program prior to the passage of PRWORA in 1996, which allowed them to incorporate work incentives such as "making work pay," work requirements, limits on the length of time one could receive welfare, and enforcement of child support. With the exception of work incentives,[19] these waivers were expected to reduce welfare caseloads, and all three of the studies mentioned above find evidence that some of these waivers were positively correlated with caseload reductions. Since many waivers were initiated at the same time as an increasing number of states were establishing minimum wage levels above the federal minimum, it is important to consider whether their implementation is in any way responsible for the magnitude of our estimated minimum wage coefficient. In order to test the hypothesis that the imposition of welfare waivers is driving our results, we include four dummy variables that indicate the presence of four categories of statewide waivers:[20] 1) those that require work; 2) those that provide incentives to work; 3) those that impose limits on the length of time an individual can receive welfare benefits; and 4) those that enforce child support rules.[21]

In addition to the welfare waivers, we also add two variables intended to capture changes in state Medicaid eligibility that occurred during the period we study. Prior to 1988, eligibility for Medicaid was explicitly tied to AFDC eligibility and there is evidence that this link encouraged AFDC participation and reduced single mothers' probabilities of working (Yelowitz, 1995). Federal legislation passed in 1986 and 1987 severed this link by eliminating Medicaid eligibility criteria related to AFDC eligibility and increasing income thresholds for some children. We control for these expansions because severing the relationship between AFDC and Medicaid may have changed the labor supply of low-income individuals who value health insurance (Yelowitz, 1995). In addition, states that implemented the expansions more rapidly and instituted broader eligibility criteria also might have provided more generous minimum wages during this period. The first variable, HIGHAGE, indicates the highest age for which children in low-income families are eligible for Medicaid benefits. It is equal to zero in years prior to the expansions and varies by state and year starting in 1988. The second variable, MAXINC, is the threshold level of income (as a percent of the poverty line) below which at least some children are eligible.[22] This variable also is equal to zero prior to 1988 and varies by state and year after that. Our expectation is that higher values of HIGHAGE and MAXINC will be associated with lower caseloads. Blank includes similar variables in her 1997 paper.

The next column of Table 4 presents estimates of equation (1) with the addition of the welfare waiver and Medicaid expansion variables described above. As in other studies, we find that the presence of welfare waivers is correlated with the size of the caseload. In particular, we find that child support programs reduce caseloads by about 6.4% and that time limits are associated with larger caseloads. The sign of this estimate is not intuitive, but it is consistent with Blank (1997), who includes a much richer set of control variables and concludes that the coefficients on her welfare waiver variables are correlated with other changes occurring at the same time.[23] [24] Of most relevance for this study, however, is that the inclusion of these variables does not alter the minimum wage estimate. If anything, the estimates of the effect of the minimum wage on caseloads become larger.

Most caseload studies include few control variables, but Blank (1997) considers a number of other factors that might affect caseload fluctuations. She finds that in models that include state-specific time trends caseload dynamics are partly explained by the fraction of the population that consists of new immigrants. Borjas and Hilton (1996) have shown that in the 1990s, immigrant households were 7 percentage points more likely than native households to participate in welfare programs; thus, large influxes of immigrants might increase welfare caseloads. Immigrants may also crowd potential welfare recipients out of jobs, thus increasing the likelihood of welfare participation of non-immigrants. We examine these possibilities by rerunning our basic regression with the addition of data on the fraction of the population in each state that are newly admitted immigrants and a one year lag of this variable.[25] The results are presented in column 5 of Table 4. The estimated minimum wage coefficient in this specification is virtually unchanged.

Next, we consider the possibility that our minimum wage estimates are biased upward because we have not sufficiently controlled for voter tastes. Suppose, for example, that state residents' preferences are such that they favor policies to help the poor. While our inclusion of state fixed effects and state-specific time trends should go a long way toward controlling for these unmeasurable tastes, there remains the possibility that fluctuations in preferences around the trend can explain our findings. We test this possibility in column 6 by adding to our regression a set of variables that measure the political atmosphere within the state. These include a dummy variable that is equal to 1 if there is Democratic control of both legislative houses *and* the governorship, a dummy variable equal to 1 if control of the legislative houses and/or governorship is divided between Democrats and Republicans (DIVIDED CONTROL), the ratio of per-capita state debt to per-capita income (DEBT/INCOME), a dummy variable equal to 1 if the governor does not have the power to use a line-item veto (NO LINE ITEM VETO), the percent of the upper legislative house who are Democrats (% UPPER DEMOCRATS), and the percent of the lower legislative house who are Democrats (% LOWER DEMOCRATS). These variables have been gathered for all states except Alaska, Hawaii and Nebraska, and are available from 1983-1994. Again, the inclusion of these variables has little effect on the minimum wage estimate.

Next, we check the robustness of our minimum wage estimates to the inclusion of alternative wage controls. Blank includes the log of wages at various percentiles of the wage distribution in her caseload regressions. We replace production wages with the log of the 10th percentile and the log of the 25th percentile wage in the CPS Monthly Outgoing Rotation Group files. Since these measures (particularly the 10th percentile wage) are more likely to be directly affected by minimum wage legislation than the average production wage, this is not our preferred specification, but we include it for purposes of comparison. The estimated minimum wage coefficient is not affected by the inclusion of these alternative wage measures, but, unlike Blank, we find little evidence that higher "alternative" wages are correlated with lower caseloads. The difference appears to result from the fact that we include additional controls for economic conditions.

Finally, we approximately replicate Blank's specifications by including all of the variables added in columns 1-4 into a single regression. The estimates produced by the analysis of this more inclusive model are shown in the 8th column of Table 3. Even when all of these variables are included in the regression model, the estimated minimum wage effect remains similar to the estimates in Table 2.

Reconciliation with the CEA's Minimum Wage Estimates

Although our estimates appear to be robust to changes in model specification and the inclusion of additional variables, a remaining concern is that they differ in both sign and magnitude from estimated minimum wage effects produced by the Council of Economic Advisors Report (1999). The CEA included the minimum wage as a control variable in their caseload analysis, and found that its estimated coefficient was approximately twice as large as our estimate, statistically significant and *negative.* There are three potentially important differences between our study and the CEA model: first, we include several additional control variables, second, we include the population variable on the right side of the regression equation instead of on the left, and third, we use fewer years of data. The CEA data span the years 1976-1998.

The authors of the CEA report have graciously shared their data with us, and we have used it to replicate their results in the first column of Table 5. Using their model and data we estimate a minimum wage elasticity of –0.37 (the comparable estimate in the CEA report is –0.39---the difference is apparently due to the fact that we used different software packages). In the second column of Table 5 we replicate our analysis using the CEA data and control variables, but restricting the time period to 1983-96 and putting the population variable on the right hand side of the regression equation. This generates an estimate of 0.26. Since this estimate is larger than our estimate in the second column of Table 2 the fact that the CEA analysis includes fewer control variables is unlikely to explain the difference between the two studies' results.

In the third column of Table 5 we show what happens to the minimum wage estimate when we use the CEA's time period, but move the population variable to the right side of the equation. Moving the population variable increases the estimated minimum wage effect to –0.14, which is almost two thirds smaller in absolute magnitude than the estimate in column 1. In addition, the estimate is no longer statistically different from zero. This finding strengthens our earlier perception that the population variable may be capturing the effect of other omitted variables, especially when combined with the magnitude and direction of the estimated population coefficients in Tables 2-4.

Moving the population variable reduces the difference between the CEA's estimate and our own, but it does not eliminate it. The remaining difference is driven by our reliance on a shorter time period. Columns 4 and 5 show what happens to the minimum wage estimates when the analysis is based on alternative time periods. Eliminating the years prior to 1983 results in a positive coefficient on the minimum wage, though it is still statistically insignificant. Including the years prior to 1983, but eliminating 1997-98 also makes the point estimate more positive. One might conclude, therefore, that our estimates differ from the CEA's because by relying on a shorter time period we are missing important identifying information. What is puzzling about that conclusion is that there is virtually no state level variation in the minimum wage prior to the mid 1980's—so why does the inclusion of data for the years prior to 1983 make a difference?

The answer appears to be that adding additional years of data changes the estimated slope of the state time trends. Recall that both models are identified off of variation in the minimum wage around a state time trend. Figure 2 shows that throughout the 1980's the national caseload trend was fairly flat, but that beginning around 1989 caseload growth took off, peaked around 1994, and then began to decline even more rapidly. Prior to 1980 the caseload exhibits a slight downward trend. When the time period for the analysis is restricted to 1983-1996, the imposition of linear trends fits the general caseload pattern reasonably well, but as more years of data are added to the analysis the linear restriction may be less and less accurate. While the inclusion of year dummies will capture these trends at the national level, it may be incorrect to assume that relative to national fluctuations a particular state's caseload would be constantly increasing or decreasing throughout the period. More likely, the state caseload increased at a different rate from the national caseload during the period during which the national caseload was growing and then decreased at a different rate from the national caseload during the period when the national caseload was declining. Since the minimum wage effect is identified off of variation around a state trend, this may be very important.

Columns 6 and 7 confirm our hypothesis. The minimum wage estimate in column 6 is based on data from 1983-1996, but here the coefficients on the state time trends are restricted to be equal to the state time trend estimates generated by the model used in column 3. As expected, the minimum wage estimate is no longer positive and statistically significant (compared to column 2). In column 7, the opposite exercise is conducted—here we use all of the years of data that are available, but restrict the state trends to be equal to those estimated in column 2. This increases the minimum wage estimate to 0.135 (although it is not statistically significant at conventional levels). Also as expected from Figure 2, the estimated state trend coefficients are almost uniformly larger when the shorter time period is used.

Taken together, columns 6 and 7 suggest that our estimates differ from those of the CEA largely because we are identifying off of different state trends. Since we believe that linear state trends describe the world less accurately as more years of data are used, we next use the data from 1976-1998 to estimate a model that allows the state trends to change between 1989-94 and 1995-98. The results from this exercise are presented in column 8. The minimum wage estimate is now positive, statistically significant at the 15% level, and of similar magnitude to the estimate in column 2 (as well as in Tables 2-4). In column 9 we re-estimate our basic model using our original data, adding data from 1997-1999, and allowing the state trend to differ between 1983-1994 and 1995-1999. The minimum wage estimate of 0.174 is similar to the estimates presented elsewhere in the paper.

In sum, the differences between the two studies' findings appear to result from two factors: differences in the treatment of the population variable and differences in the estimated state trends off of which the minimum wage effects are identified.[26] We cannot think of a reason that it would be preferable to put population on the left side of the equation, especially since its estimated coefficient strongly suggests that the caseload and the population do not move in tandem. On the other hand, additional years of data may provide useful identifying information, provided that caseload changes that would have occurred in the absence of minimum wage increases are sufficiently controlled (this is obviously important no matter how many years of data are used!). Researchers include state time trends in order to more fully control for omitted variables within states that might be trending up or down over time, but linear trends may not sufficiently control for changes in omitted variables that contributed to the dramatic caseload fluctuations of the 1990's. Since observable variables do a poor job of explaining these fluctuations (e.g. Wallace and Blank, 1999) it may be preferable to allow the model to more fully account for unobservables that may have affected the caseload. We find that when we increase the flexibility of the CEA model by allowing the state caseload trends to vary over time, and by moving the population variable to the right hand side of the equation, the estimated minimum wage elasticity is similar to the estimate produced by our model and data.

V. Conclusion

This paper has investigated the effect of minimum wages on welfare caseloads using state level panel data. Our results suggest that minimum wage levels have a substantial positive effect on the size of the AFDC caseload, and that, therefore, minimum wages will not help many low-income families achieve self-sufficiency. A ten-percent increase in the minimum wage is estimated to increase welfare caseloads between 1 and 2 percent. We find that the inclusion of other variables that influence the evolution of caseloads over time, such as state-specific welfare reforms and changing political preferences, do not weaken our results.

Increases in minimum wages might lead to rising welfare caseloads for several reasons. First, as suggested by the classical economic model, the imposition of minimum wages can result in a reduction in the number of job vacancies. As jobs become less available, low-skilled workers are less able to find work and are thus more likely to apply for AFDC benefits. Second, increasing minimum wages might cause more workers to enter the labor market. If the new labor market entrants possess more human capital than do likely welfare recipients, then potential welfare recipients may be "crowded out" of jobs. Lang and Kahn (1998) find that minimum wage increases shift food-service jobs from adults to teenagers and students, which provides support for this possibility. Either job loss or crowding-out could also lead to hours reductions rather than total job loss, and it may be that the earnings decline resulting from a reduction in hours of work is large enough to increase welfare participation. Our data do not enable us to distinguish between these possibilities, but we hope to explore them in future research.

Our results suggest that increases in the minimum wage may impose a real cost on government – the cost associated with larger welfare caseloads. Because the higher production costs that result from minimum wages are at least partly borne by consumers, the cost to society of minimum wage policies will be higher than the cost resulting from the increase in welfare payments alone. This additional cost might be considered acceptable, however, if minimum wage policies improved outcomes among the poor. Our findings, however, suggest that the cost to society is not balanced by reductions in the number of families that depend on welfare. These indirect costs should be considered when comparing the cost of the minimum wage to the cost of policies with similar goals, such as the Earned Income Tax Credit (see Burkhauser, Couch, and Glenn, 1996).

Our policy conclusions come with a number of caveats. First of all, it is important to remember that low-skilled adults who maintain their employment levels are likely to benefit from the higher wages mandated by minimum wage legislation. On balance, however, our finding that welfare caseloads increase when minimum wages rise suggests that the number of families whose earnings rise enough to help them move off of welfare is exceeded by the number whose employment-based income declines. Second, if minimum wages are increased in response to rising caseloads or poor economic conditions, then our policy variable will be endogenous and our estimates may be biased upward. We have tried to eliminate potential omitted variables bias by including as many control variables as possible, but if these controls are not complete then our study will be subject to the same criticism as the teenage employment studies. We know of no minimum wage studies that have used a convincing instrument to get around this problem. Finally, it is important to remember that research on the impact of minimum wages on teenage employment has produced elasticity estimates that are all over the map, and that these estimates vary according to the data years analyzed, the source of the identifying variation, and the empirical strategies used to eliminate fixed effects. We have considered a number of alternative specifications in our analysis, but, as in the employment literature, an alternative approach may yield different results.

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Figure 1: Predicted Values of Log(Caseload/Population) from Regressions using Time Dummy Variables and using a Quartic Time Trend

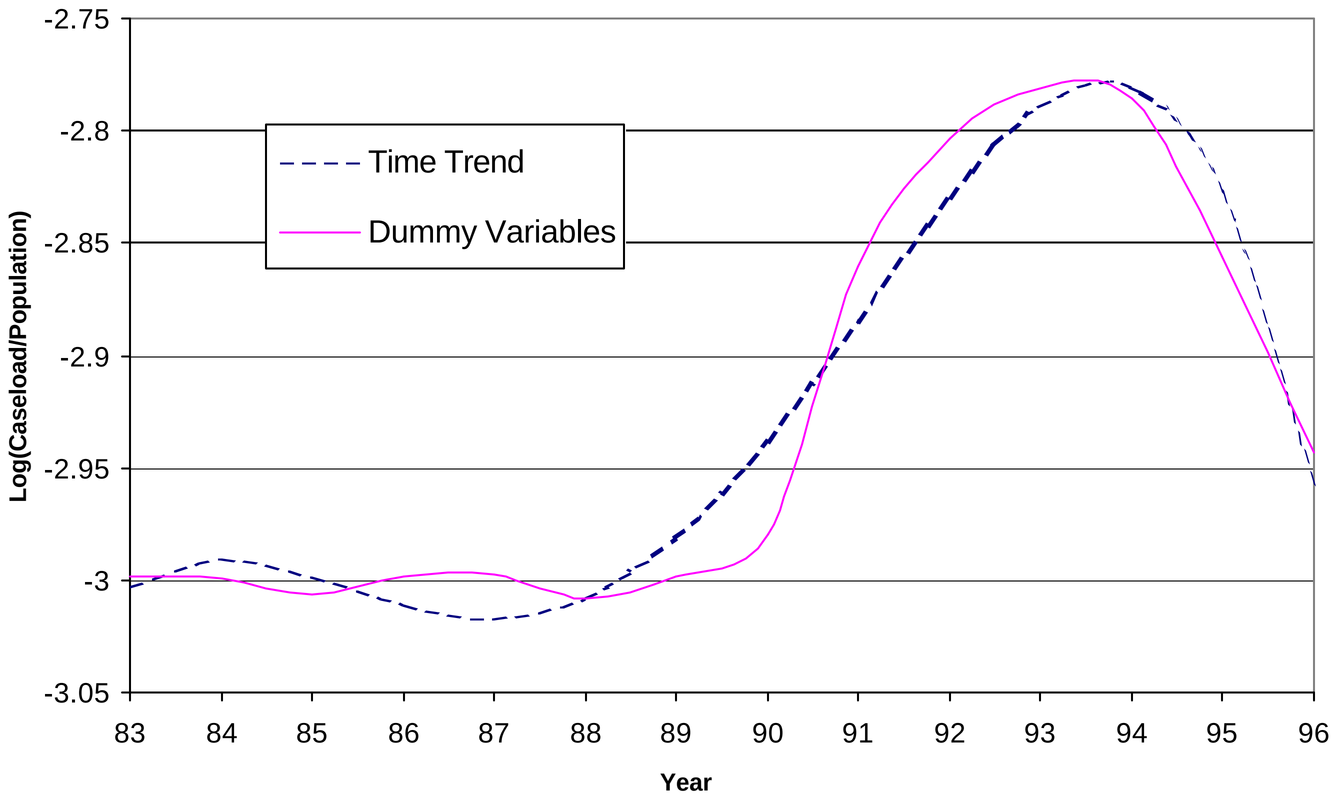

Figure 2: Caseload Changes Over Time

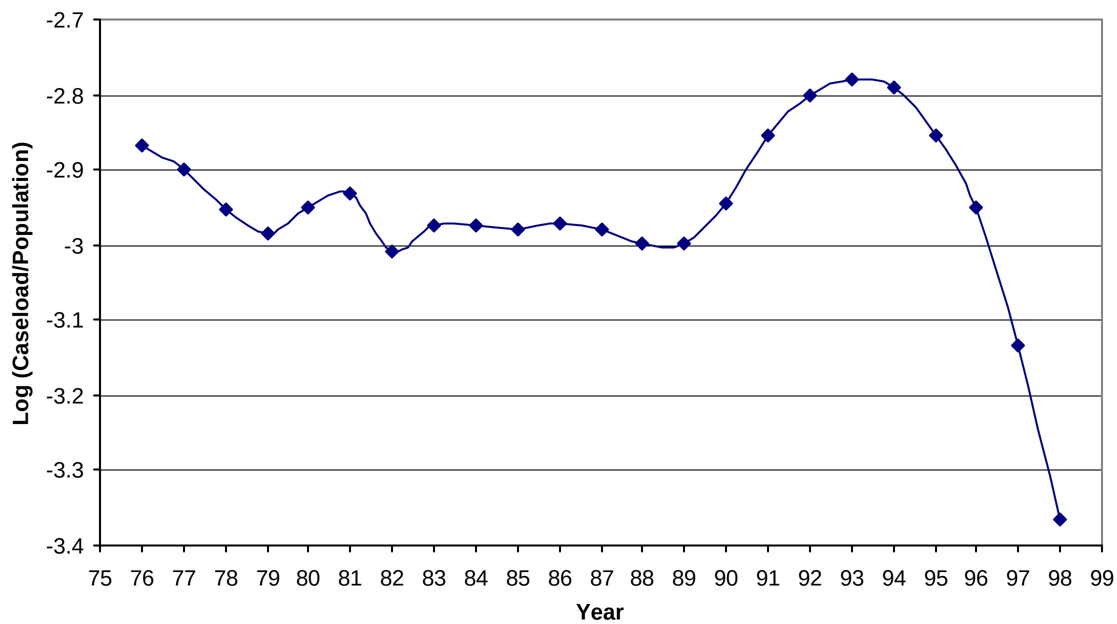

Table 1
State Minimum Wage Levels: 1980-1996[1]

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
Federal Min.Wage	3.10	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.80	4.25	4.25	4.25	4.25	4.25
AL	3.60	3.85	3.85	3.85	3.85	3.85	3.85	3.85	3.85	3.85	3.85	4.75	4.75	4.75	4.75	4.75	4.75
AK																	
AZ																	
AR																	
CA										4.25	4.25						
CO																	
CT	3.12	3.37	3.37	3.37	3.37	3.37	3.37	3.37	3.75	4.25	4.25	4.25	4.27	4.27	4.27	4.27	4.27
DE	3.14	3.48	3.62	3.82	3.85	3.86	4.16	4.33	4.33	4.33	4.85	4.85	5.45	5.45		5.25	5.25
DC																	
FL																	
GA																	
HI									3.85	3.85	3.85	3.85		5.25	5.25	5.25	5.25
ID											3.85	4.25	4.65	4.65	4.65	4.65	4.65
IL																	
IN																	
IA																	
KS																	
KY																	
LA																	
ME								3.55	3.65	3.75	3.75						4.75
MD																	
MA						3.45	3.55	3.65	3.65	3.75	3.85	3.85					
MI																	
MN								3.55	3.85	3.95	4.25						
MS																	
MO																	
MT																	
NE																	
NV										3.40							
NH																	
NJ								3.45	3.55	3.65	3.75	3.85					
NM														5.05	5.05	5.05	5.05
NY																	
NC																	
ND																	
OH																	
OK																	
OR											4.25	4.75	4.75	4.75	4.75	4.75	4.75
PA										3.70	3.70	4.25	4.45	4.45	4.45		
RI								3.55	3.65	4.00	4.25	4.25				4.45	4.45
SC																	
SD																	
TN																	
TX																	
UT																	
VT																	
VA								3.45	3.55	3.65	3.75	3.85				4.75	4.75
WA										3.85	4.25	4.25			4.90	4.90	4.90
WV																	
WI																	
WY																	
# States Exceeding Fed. Min.[2]	3	3	3	3	3	4	4	8	10	13	16	13	6	8	8	10	11

[1]As of January 1, in the given year.
[2]Alaska's minimum wage is always fifty cents above the federal minimum.

Table 2

Basic Regression Estimates of the Effect of the Minimum Wage on Welfare Caseloads 1983-1996

	Time Dummies		Quartic Time Trend	
	Unweighted	Weighted	Unweighted	Weighted
	(1)	(2)	(3)	(4)
Minimum wage	0.124	0.184	0.184	0.236
	(0.070)	(0.053)	(0.065)	(0.056)
Log (Average Production Wage)	-0.362	0.087	-0.284	0.286
	(0.252)	(0.249)	(0.248)	(0.235)
Unemployment Rate	0.006	0.004	0.008	0.003
	(0.004)	(0.004)	(0.003)	(0.004)
Unemployment Rate	0.011	0.004	0.012	0.008
t-1	(0.004)	(0.003)	(0.003)	(0.003)
Unemployment Rate	0.012	0.018	0.011	0.013
t-2	(0.004)	(0.004)	(0.003)	(0.003)
Log(Gross State Product)	-0.483	-0.601	-0.507	-0.629
	(0.107)	(0.110)	(0.102)	(0.099)
Log(Population)	-1.358	-1.661	-1.457	-1.846
	(0.541)	(0.352)	(0.482)	(0.292)
Fraction of Population White	-2.973	0.625	-2.570	1.051
	(1.998)	(2.624)	(1.938)	(2.655)
Fraction of Households Female Headed	0.416	0.591	0.190	0.148
	(0.219)	(0.199)	(0.157)	(0.139)
Maximum Benefit For AFDC+FS Family of 3	0.001	0.001	0.001	0.001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
State Provides AFDC-UP	0.053	0.089	0.061	0.095
	(0.019)	(0.024)	(0.017)	(0.021)
State Fixed Effects	yes	yes	yes	yes
State*Time Trends	yes	yes	yes	yes
Number of Observations	700	700	700	700

Table 3

Additional Estimates of Minimum Wage Effects on Welfare Caseloads Weighted and with Time Indicator Variables 1983-1996

	Quadratic State Trends	Use MWage Level	Add Lagged Minimum Wage	
	(1)	(2)	(3)	(4)
Minimum Wage	0.118	0.041	0.107	2.83
	(0.055)	(0.012)	(0.056)	(1.49)
Lagged Minimum Wage			0.132	2.97
			(0.065)	(1.57)
Interaction MW*LagMW				-1.88
				(1.03)
Log (Average	0.119	0.086	0.062	0.061
Production Wage)	(0.185)	(0.249)	(0.024)	(0.237)
Unemployment Rate	0.006	0.004	0.004	0.003
	(0.004)	(0.004)	(0.004)	(0.096)
Unemployment Rate	0.010	0.004	0.005	0.005
t-1	(0.003)	(0.003)	(0.003)	(0.004)
Unemployment Rate	0.012	0.018	0.018	0.019
t-2	(0.004)	(0.004)	(0.004)	(0.005)
Log(Gross State Product)	-0.169	-0.601	-0.577	-0.589
	(0.114)	(0.110)	(0.120)	(0.120)
Log(Population)	-2.454	-1.656	-1.624	-1.625
	(0.451)	(0.355)	(0.109)	(0.108)
Fraction of Population	10.143	0.602	0.777	0.928
White	(2.995)	(2.622)	(2.613)	(2.572)
Fraction of Households	0.322	0.591	0.639	0.631
Female Headed	(0.197)	(0.200)	(0.207)	(0.205)
Maximum Benefit For	0.001	0.001	0.001	0.001
AFDC+FS Family of 3	(0.0002)	(0.0002)	(0.0002)	(0.0002)
State Provides AFDC-UP	0.099	0.089	0.095	0.096
	(0.025)	(0.024)	(0.025)	(0.025)
Long-Run Elasticity Estimate			0.239	0.201
Year Fixed Effects	yes	yes	yes	yes
State Fixed Effects	yes	yes	yes	yes
State*Time Trends	yes	yes	yes	yes
Number of Observations	700	700	700	700

Table 4
Additional Estimates of Minimum Wage Effects on Welfare Caseloads
Weighted and with Time Indicator Variables 1983-1996

	State-Specific Business Cycles?				Add	Add	Alternative	
	State* Urate	State* Prod Wg	State* GSP	Add Waivers	Percent Immigrants	Political Variables	Wage Measures	Blank Model*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Minimum Wage	0.145	0.247	0.083	0.230	0.151	0.153	0.154	0.172
	(0.059)	(0.065)	(0.051)	(0.045)	(0.074)	(0.054)	(0.047)	(0.053)
Log (Average Production Wage)	0.218	2.353	0.160	0.109	0.032	-0.194		
	(0.264)	(1.815)	(0.235)	(0.257)	(0.246)	(0.204)		
Log(10th Wage percentile)							0.179	0.069
							(0.089)	(0.081)
Log(25th Wage percentile)							-0.136	-0.194
							(0.071)	(0.076)
Unemployment Rate	-0.011	0.004	0.005	0.004	0.004	0.005	0.004	0.007
	(0.006)	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Unemployment Rate t-1	0.006	0.002	0.007	0.005	0.005	0.007	0.005	0.011
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Unemployment Rate t-2	0.012	0.017	0.014	0.017	0.017	0.014	0.018	0.013
	(0.004)	(0.008)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Log(Gross State Product)	-0.586	-0.741	-0.206	-0.584	-0.611	-0.347	-0.587	
	(0.113)	(0.128)	(0.181)	(0.113)	(0.112)	(0.126)	(0.105)	
Log(Population)	-1.837	-1.720	-1.574	-1.819	-1.778	-1.569	-1.672	-1.754
	(0.348)	(0.333)	(0.352)	(0.357)	(0.369)	(0.406)	(0.344)	(0.419)
Fraction of Population White	1.634	0.325	3.116	0.326	0.492	-12.059	0.612	-14.591
	(2.366)	(2.360)	(2.857)	(2.347)	(2.572)	(3.264)	(2.435)	(3.234)
Fraction of Households Female Headed	0.321	0.473	0.201	0.656	0.579	0.572	0.549	0.528
	(0.171)	(0.284)	(0.183)	(0.198)	(0.203)	(0.208)	(0.207)	(0.220)
Maximum Benefit For AFDC+FS Family of 3	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.681
	(0.0002)	(0.0003)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.117)
State Provides AFDC-UP	0.049	0.059	0.064	0.090	0.094	0.055	0.086	0.057
	(0.013)	(0.030)	(0.017)	(0.022)	(0.024)	(0.017)	(0.024)	(0.017)
High Age				0.001				0.002
				(0.002)				(0.001)
Max Inc				-0.0001				-0.0001
				(0.0001)				(0.0001)
% Immig					0.202			-0.104
					(0.492)			(0.384)
% Immig at t-1					2.775			2.866
					(1.294)			(1.255)
State has Work Requirement				0.013				-0.029
				(0.014)				(0.017)
State has Work Incentive				0.001				0.029
				(0.017)				(0.025)
State has Time Limits				0.033				0.065
				(0.024)				(0.025)
State has Child Support				-0.066				-0.048
				(0.017)				(0.019)
Democratic Control of Both Houses & Gvshp*100						-0.319		-0.196
						(1.078)		(1.084)
Divided Control						-0.021		-0.021
						(0.010)		(0.009)
(Debt/Income)*100						0.142		0.132
						(0.032)		(0.030)
No Line Item Veto*100						0.0467		0.519
						(2.346)		(2.661)
% Upper Democrats*100						0.0097		0.0054
						(0.0488)		(0.0459)
% Lower Democrats*100						0.002		0.001
						(0.001)		(0.001)
State Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
State*Time Trends	yes	yes	yes	yes	yes	yes	yes	yes
Number of Observations	700	700	700	700	700	564	700	564

*In order to more closely approximate Blank's model, we have replaced the maximum benefit level with its log.

Table 5
Reconciliation of Minimum Wage Estimates with the Council of Economic Advisors Estimates

	CEA Model 1976-98 (1)	Our Model* 1983-96 (2)	Move Log Population to Right Side of Equation 1976-98 (3)	1983-98 (4)	1976-96 (5)	Use State Trends from Column 3 1983-96 (6)	Use State Trends from Column 2 1976-98 (7)	Allow State Trends to Vary 1976-98 (8)	Allow State Trend to Vary: Our Model 1983-1999 (9)
Minimum Wage	-0.371	0.261	-0.139	0.047	-0.013	-0.040	0.135	0.163	0.174
	(0.105)	(0.067)	(0.131)	(0.134)	(0.142)	(0.091)	(0.184)	(0.114)	(0.082)
Unemployment Rate	-0.004	0.007	0.007	0.0001	0.010	-0.008	0.017	0.010	-0.002
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
Unemployment Rate t-1	0.015	0.012	0.013	0.012	0.010	0.016	0.011	0.008	0.005
	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)
Unemployment Rate t-2	0.043	0.021	0.027	0.025	0.022	0.028	0.018	0.012	0.019
	(0.005)	(0.003)	(0.005)	(0.007)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)
Log(Population)		-2.060	-1.335	-2.394	-1.471	-0.778	-2.523	-1.727	-0.049
		(0.051)	(0.345)	(0.591)	(0.296)	(0.472)	(0.515)	(0.320)	(0.057)
Log (Average Production Wage)									-0.113
									(0.279)
Fraction of Population White									-0.094
									(0.494)
Fraction of Households Female Headed									0.408
									(0.242)
State Provides AFDC-UP									0.074
									(0.254)
Log (Gross State Product)									-0.609
									(0.123)
Maximum Benefit For AFDC+FS Family of 3	0.148	0.167	0.287	0.194	0.237	0.409	0.235	0.200	0.0005
	(0.090)	(0.074)	(0.080)	(0.106)	(0.076)	(0.117)	(0.115)	(0.068)	(0.0002)
State has Welfare Waiver	-0.093	-0.023	-0.083	-0.089	-0.030	-0.061	-0.015	-0.022	-0.019
	(0.036)	(0.025)	(0.032)	(0.033)	(0.023)	(0.026)	(0.038)	(0.019)	(0.020)
State has Implemented TANF	-0.189	-0.498	-0.160	-0.154	-0.353	-0.325	-0.271	-0.015	-0.025
	(0.040)	(0.115)	(0.039)	(0.050)	(0.110)	(0.118)	(0.068)	(0.034)	(0.042)
State Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
State*Time Trends	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of Observations	1150	700	1150	800	1050	700	1150	1150	850

*The estimates in column 2 are based on our model, but use the sparser set of control variables available from the CEA. In order to replicate the CEA model we have use the log of maximum AFDC benefits for a family of three instead of the level of AFDC + Foodstamp benefits available to a family of three. Like the CEA, we also include dummy variables for whether the state has a welfare waiver or has implemented the TANF program.

Appendix A

The following list documents the sources for the independent variables included in our regression analyses.

AFDC Caseloads	Social Security Bulletin, *Annual Statistical Supplement*, various years.
Minimum Wage	U.S. Department of Labor, Division of State Standards Programs, Wage and Hour Division, Employment Standards Administration.
Production Wages	U.S. Department of Labor, Bureau of Labor Statistics: *State and Area Current Statistics-Most Requested Series* website.
Unemployment Rate	U.S. Department of Labor, Bureau of Labor Statistics: *State and Area Current Statistics-Most Requested Series* website.
State Population	U.S. Census Bureau: *State Population Estimates* website.
Gross State Product	U.S. Department of Commerce: *Bureau of Economic Analysis* website.
AFDC+Foodstamp Benefit Levels	U.S. House of Representatives: *Background Materials and Data on Programs Within the Jurisdiction of the Committee on Ways and Means*, Washington, D.C., various years.
Existence of AFDC-UP program	Congressional Research Service, The Library of Congress, 1987. *State Use of Aid to Families with Dependent Children-Unemployed Parent (AFDC-UP) Program: An Overview*, 87-969 EPW & U.S. Department of Health and Human Services, Social Security Administration, 1992. *Characteristics and State Plans for Aid to Families with Dependent Children.*

HighAge MaxInc	National Governors' Association, "State Coverage Of Pregnant Women and Children," NGS Center for Policy Research, various years, and Intergovernmental Health Policy Project, "Medicaid And Indigent Care: An Overview of State Legislative Activity," 1992 and 1994 (George Washington University).
Welfare Waivers	Ziliak, James P., David N. Figlio, Elizabeth E. Davis, and Laura S. Connolly, "Accounting for the Decline in AFDC Caseloads: Welfare Reform or Economic Growth?" University of Oregon, unpublished mimeo.
Immigration	Immigration and Naturalization Service, *Statistical Yearbooks*, 1988 and 1996. Washington, D.C.: NTIS.
Percent of households headed by single women with children	Current Population Survey Monthly Outgoing Rotation Groups, 1980-1996.

Appendix B

When the minimum wage effect is identified using only time-series variation (including only state fixed effects) or only cross-sectional variation (including only year dummies or a quartic time trend), estimates of the minimum wage's effect are negative and are sometimes statistically significant. In other words, states that tend to have high minimum wages also tend to have low caseloads, and years in which real minimum wages are relatively high correspond to years in which caseloads are relatively low.

We then placed more restrictions on the data by including both state and year fixed effects, as in Neumark and Wascher's (1992) employment study. The resulting minimum wage effect is about –0.16 and is statistically different from zero. On the other hand, if we account for unobservable state-specific factors by taking first differences (rather than by including indicator variables for each state), our estimate of the minimum wage effect is close to zero and not statistically significant. When we replace our year-specific effects with a quartic time trend the two estimation approaches continue to yield different results.

The state-fixed effect and first-differences estimation strategies both identify the impact of the minimum wage using variation in its real value within states over time, but the first-differences approach uses within-state variation across two years, whereas the inclusion of state-indicator variables, which is equivalent to mean-differencing, uses within-state variation across the entire length of the panel. See Baker, Benjamin and Stanger (1999) for a thorough discussion of the importance of this distinction. The difference in the estimates produced by these approaches is consistent with the existence of unobserved state factors that are not fixed over time. We were able to reconcile the results of the two estimation approaches by including state-specific linear time trends in our regression model. Of the four specifications we present in Table 2, the only pair of point estimates that are not particularly close are those produced by the unweighted specification that includes year-effects. Using the first differences estimation strategy produces a coefficient estimate of 0.032, whereas using the strategy that includes state-specific dummy variables produces an estimate of 0.124. The 90% confidence intervals around each estimate include the other estimate. The estimates presented in Table 2 are, therefore, based on a specification that includes state dummies, year dummies (or quartic time trends), and linear state-specific trends. This is the same model described in equation (1).

Appendix Table A.1

Summary Statistics

	Mean (Standard Deviation)		
Log (Per Capita Caseload)	10.584 (1.184)	% Immigrant	0.002 (0.002)
Log (Minimum Wage)	1.480 (0.075)	% Immigrant$_{t-1}$	0.002 (0.002)
Log (Average Production Wage)	2.506 (0.126)	State has any Welfare Waiver	0.121 (0.327)
Unemployment Rate	6.320 (2.052)	State has Work Incentive	0.080 (0.271)
Unemployment Rate$_{t-1}$	6.612 (2.052)	State has Time Limits	0.047 (0.212)
Unemployment Rate$_{t-2}$	6.762 (2.052)	State has Child Support	0.090 (0.286)
Log (Gross State Product)	-3.733 (0.198)	State has Work Requirement Requirement	0.080 (0.241)
Employment Growth	0.005 (0.008)	Democratic Control of Both Houses & Gvshp	0.319 (0.467)
Log (Population)	14.939 (1.005)	Divided Control	0.365 (0.482)
Fraction of the Population White	0.805 (0.136)	(Debt/Income)*10	82.76 (51.83)
Fraction of Households Female Headed	0.117 (0.031)	No Line Item Veto	0.135 (0.342)
Maximum Benefit: AFDC+FS Family of Three	699.2 (151.4)	% Upper Democrats*100	60.05 (18.21)
State Provides AFDC-UP	0.720 (0.449)	% Lower Democrats*100	58.83 (17.58)
HighAge	5.954 (6.173)		
MaxInc	102.3 (81.00)		
Sample Size	700		

Appendix Table A.2

Additional Estimates of Minimum Wage Effects on Welfare Caseloads
Unweighted and with Quartic Time Trend

	Quadratic State Trends	Use MWage Level	Eliminate Urate	State-Specific Business Cycles? State* Urate	State* Prod Wg	State* GSP	Sample= 1987-96
	(1)	(2)	(3)	(5)	(6)	(7)	(8)
Minimum Wage	0.120	0.040	0.199	0.125	0.197	0.113	0.123
	(0.060)	(0.015)	(0.065)	(0.053)	(0.058)	(0.059)	(0.049)

	Add Waivers	Add % Immigrants	Add Political Variables	Alternative Wage Measures	Blank Model & Log(MAXBEN)	Blank Model Prod. Wages & Log(MAXBEN)
	(9)	(10)	(11)	(12)	(13)	(14)
Minimum Wage	0.218	0.178	0.239	0.132	0.266	0.255
	(0.061)	(0.067)	(0.066)	(0.063)	(0.062)	(0.070)

[1] Examples from the literature include Baker, Benjamin and Stanger (1997), Brown, Gilroy and Kohen (1983), Card (1992a, 1992b), Card and Krueger (1994,1995), Card, Katz and Krueger (1994), Currie and Fallick (1996), Deere, Murphy and Welch (1995), Katz and Krueger (1992), Kim and Taylor (1995), Neumark (1999), Neumark, Schweitzer and Wascher (2000), Neumark and Wascher (1992, 1994), and Wellington (1991). See Brown, Gilroy and Kohen (1982) for a review of earlier papers.

[2] Based on the 1996 CPS Monthly Outgoing Rotation Files. 13.7% of unmarried female household heads with children earned between $4.25 and $5.15 per hour. An additional 3.1% reported wages lower than the 1996 federal minimum wage of $4.25.

[3] Calculated from the March 1993 CPS. Both AFDC participation and earnings relate to the year prior to the survey (1992).

[4] Blank (1989) finds that one third of AFDC spells begin when a female head experiences a decline in earnings and about one fifth of spells end because of earnings increases. Bane and Ellwood (1983), find that 12% of spell beginnings can be attributed to earnings declines and about one third of exits can be attributed towards earnings increases. In more recent work (Bane and Ellwood, 1994) these numbers are revised downward to 7% and 25%.

[5] See Card and Krueger (1995).

[6] Grogger finds that the positive relationship between minimum wages and welfare use declines with the age of the youngest child. He finds no evidence that minimum wages increase welfare use among those with older children.

[7] The AFDC program had two components, the “Basic” program, which comprised over 90% of the caseload, and the “UP” (unemployed parents) program, which was much more limited, applied to married-couple families, and was not available in all states during the time period we analyze. Blank (1997) shows that a very different set of factors affect AFDC-UP and AFDC-Basic caseloads.

[8] We use the CPS Outgoing Rotation Group files to estimate the fraction of households headed by a single mother. Prior 1990, the ORG data do not provide enough demographic information to allow us to distinguish between children and other related persons in the household. For these years, we follow Blank (1997), and use the fraction of households headed by a single woman and including other related persons under age 18 in the household as a proxy for single mother households.

[9] We were unable to include the District of Columbia in our analysis because we were unable to obtain a measure of production wages for the district.

[10] In the March 1997 CPS, for example, 14 states have fewer than 40 single female household heads between the ages of 15 and 55, with children. In addition, many of these women will be working for wages that are much higher than the minimum wage and/or will not be eligible for welfare.

[11] Another drawback to using CPS data for the analysis is that estimates of welfare participation based on the March CPS have historically been lower than estimates from administrative data. Furthermore, there is some evidence that underreporting in the CPS may have increased beginning around 1993 (Bavier, 1999).

[12] A disadvantage of using state-level aggregate data, however, is that they do not allow us to consider the possibility that there are differences inminimum wage effects across demographic groups. We attempted to look for variation in minimum wage effects across race and education groups by supplementing our caseload analysis with an analysis based on the March 1984-1997 CPS files. Our CPS sample was restricted to unmarried female households heads between the ages of 16 and 54, with children, and it contained 53,442 observations. We estimated the effect of minimum wages on the probability of being an AFDC recipient by estimating a probit model in which the dependent variable was equal to 1 if the individual received AFDC benefits sometime during the year prior to the survey and 0 otherwise. We included all of the control variables included in equation (1) with the exception of the state level demographic variables, which were replaced with individual level controls for age, and dummy variables indicating whether the individual was white, non-Hispanic black or Hispanic, whether her highest level of education was a high school diploma or less and whether she had a child under the age of 6. None of the minimum wage effects that we estimated using the CPS were statistically significant, and the standard error estimates were often bigger than the coefficient estimates. For example, evaluated at the sample means, our estimated minimum wage coefficient indicated that a 10% increase in the minimum wage would increase the probability that a woman becomes a welfare recipient by 27%! But the standard error estimate on this coefficient was nearly three times its size. Likewise, when we added interactions between the minimum wage and a dummy variable indicating that the individual's highest level of educational attainment was a high school diploma (or less) we estimated marginal effects of -.015 (standard error estimate 0.090) for the minimum wage and 0.058 (standard error estimate 0.067) for the interaction term. Inclusion of an interaction term between the minimum wage and a dummy variable indicating whether the individual was white produced similar but somewhat more precise results. These results are not at all surprising given that only a handful of states have sufficient numbers of observations to provide accurate information about the state's welfare population. The benefits of using individual level data for this study, therefore, seem to be outweighed by the efficiency loss.

[13] David Ellwood has pointed out to us that raw correlations between the federal minimum wage and the size of the national welfare caseload are much larger between 1980 and 1994 than in previous years. If our estimates were primarily identified from time series variation in the federal

minimum wage, then our choice of years would surely be suspect. Since we include time dummies (or quartic time trends) in our analysis, however, virtually all of our identification is coming from cross-sectional variation in state minimum wage changes over time.

[14] The Newey-West method produces consistent standard errors as N and T approach infinity; our sample, by comparison, is relatively small. However, OLS and White standard error estimates are very similar.

[15] Our model does not provide intuition regarding the appropriate number of autocovariances to be used in the autocorrelation weighting matrix, so we computed the Newey-West standard error estimates using autocovariances ranging from 4 to 8. Our results were not affected by values chosen. The results reported in the paper are based on values equal to 6.

[16] We expect that post-PRWORA restrictions will increase the supply of labor among low income single mothers. Minimum wage increases may further increase this supply. But unless these additional job applicants are successful at finding work, the minimum wage is unlikely to reduce caseloads through a supply response. Our reduced form estimates indicate that the minimum wage probably reduces the demand for these workers.
It is possible that TANF caseloads will increase less than AFDC caseloads when minimum wages rise because of the TANF work requirement. If the minimum wage reduces employment opportunities for TANF candidates, so that some of them cannot meet the work requirement, then the TANF caseload may not rise in response to the minimum wage by as much as the AFDC caseload because those unable to find jobs will not qualify for TANF benefits.

[17] From the Statistical Abstract of the United States.

[18] Similar results are produced by models that include additional lags and interactions, but some of the minimum wage coefficient estimates become statistically insignificant.

[19] Work incentives might increase welfare caseloads if they include provisions such as raising the amount of income that AFDC recipients can earn before they begin to lose benefits.

[20] Some states implemented waivers that only affected small portions of the state. We identify the presence of a waiver only if it was implemented throughout the state.

[21] These variables were obtained from the U.S. Department of Health and Human Services and are included in Ziliak, Figlio, Davis and Connolly (2000).

[22] This threshold varies within a state/year cell depending on the age of the child. We have chosen to use the maximum threshold for *any* child in the state/year cell.

[23] Martini and Wiseman (1997) also note that the apparent effect of welfare waivers may not be causal.

[24] In regressions not included in the table, we pooled all of the waivers together to include a single indicator variable equal to 1 if the state had been granted any waiver. Our estimated coefficients on this variable are about –0.026 (standard error estimate of 0.01) in each specification, which is very similar Blank's estimated waiver coefficient. This specification has no substantive effect on our minimum wage estimate.

[25] These data are reported in the INS's Statistical Yearbooks. The number of immigrants to each state is measured as the number of legally admitted immigrants who intend to reside in that state. These figures omit undocumented immigrants. Measurement error will arise from differences in the intended residence of an immigrant and the actual residence of that immigrant.

[26] These differences may also help explain why Grogger's estimates tend to be smaller than ours.