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The Effects of an Employer Subsidy on Employment Outcomes: A Study of the Work Opportunity and Welfare-to-Work Tax Credits

Sarah Hamersma
University of Florida
E-mail: sarah.hamersma@cba.ufl.edu

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Abstract

Recent changes in American public assistance programs have emphasized the role of work. Employer subsidies such as the Work Opportunity Tax Credit (WOTC) and the Welfare-to-Work Tax Credit (WtW) are designed to encourage employment by reimbursing employers for a portion of wages paid to certain welfare and food stamp recipients, among other groups. In this paper I develop a simple dynamic search model of employment subsidies and then test the model's implications for the employment outcomes of WOTC- and WtW-subsidized workers. My model predicts that subsidized workers will have higher rates of employment and higher wages than equally productive unsubsidized workers, and it highlights some possible effects of the subsidy on job tenure. I test these predictions using a unique administrative data set from the state of Wisconsin. These data provide information on demographic characteristics, employment histories, and WOTC and WtW participation for all welfare and food stamp recipients in the state for the years 1998–2001. My ability to precisely identify the workers who are subsidized allows me to distinguish the effects of program participation from those of eligibility. I estimate the employment, wage, and job tenure effects of the WOTC and WtW using propensity score matching estimation, which allows me to control for selection into the programs while maintaining fewer functional form assumptions than typical methods. I find that the WOTC and WtW have limited effects on the labor market outcomes of the disadvantaged population. While the programs may modestly increase employment and wages in the short run, these gains do not persist over time.

The Effects of an Employer Subsidy on Employment Outcomes: A Study of the Work Opportunity and Welfare-to-Work Tax Credits

I. INTRODUCTION

In this paper I estimate the effects of the Work Opportunity Tax Credit (WOTC) and the Welfare-to-Work Tax Credit (WtW) on employment outcomes of disadvantaged workers. These credits offer subsidies to firms that hire individuals who may otherwise have difficulty finding jobs, such as certain welfare recipients, disadvantaged youth, and disabled individuals. Past work on previous employer-based credits found weak or even nonexistent employment effects, which resulted in the elimination of these subsidies. The WOTC has been reauthorized four times since its implementation in 1996, and the WtW three times since its implementation in 1998, yet no study has carefully examined their effectiveness.

I first develop an analytical model of the WOTC and WtW that allows workers from the same population to be paid different wages based on their value to the particular firms in which they are employed. I also incorporate a binding minimum wage, which results in some long-term unemployment. Finally, I allow wages and employment status to change over time as employers learn about workers' productivity in their firm. This dynamic element is essential to the model, since predictions about wage trajectories and job tenure cannot be made based on a static model. For example, concerns that disadvantaged workers will end up in short-term, low-paying jobs cannot be addressed analytically without a model that allows changes in employment status over time.

My model provides predictions regarding the effects of the WOTC and WtW on employment and wages, as well as insights into their potential effects on job tenure. First, it predicts that subsidized groups will have higher employment rates than otherwise identical unsubsidized workers in both the short run (under uncertainty) and long run (when productivity is known). Second, it predicts that wages will be higher for subsidized workers than for their unsubsidized counterparts. Finally, it suggests two competing effects of the subsidy on job tenure, making the net expected effect of the subsidy on job tenure unclear.

Most past evaluations of employer tax credits similar to the WOTC and WtW have been limited by their use of survey data that do not include subsidy-receipt details necessary to study the employment outcomes of program participants. Most of these studies used data (either firm- or individual-level) that include information on eligibility without identifying which workers are actually WOTC- or WtW-certified. This led researchers to study the effects of subsidy eligibility rather than participation.[1] However, the fraction of eligible workers who participate in tax credit programs is low. Hamersma (2003) finds that no more than 17 percent of eligible food stamp recipients were certified for the WOTC/WtW in 1999, and no more than 33 percent of eligible welfare recipients were certified. These upper-bound estimates indicate that a large majority of qualified workers are not being claimed by their employers for the tax credits. Low participation means that data that do not contain information on subsidy receipt have a severe limitation, since any estimated labor market effects are averaged over the whole eligible population even when few are participating in the program.

This is the first empirical analysis of the employment, wage, and job tenure effects of employer subsidies that also accounts for the distinction between eligibility and certification, which allows me to identify the effects of the program on those who actually utilize it. My study uses unique administrative data on earnings, WOTC/WtW eligibility, and WOTC/WtW certification for welfare recipients in the state of Wisconsin. These data allow me to avoid some of the biases common to treatment-effect studies. In particular, these data have two elements that Heckman, Ichimura, Smith, and Todd (1998) document as keys to minimizing bias: the data for both treatment and comparison groups are from the same sources, and individuals from both groups operate in the same local labor markets. I use demographic characteristics and detailed welfare and food stamp records to control for additional determinants of labor

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[1]Hollenbeck, Willke, and Ershadi (1986) are an exception; in their study of the Targeted Jobs Tax Credit, they examine both eligibility and certification (participation) effects. They do not, however, examine job tenure at the certified job.

market outcomes, and also use employer data to address selection into the WOTC/WtW (as employers must submit applications for the credits). Although my analysis is limited to one state, Wisconsin's particularly aggressive program for promoting work among welfare recipients and its national leadership in welfare reform efforts make it an excellent setting for my study.

I use the data in two specific ways to examine the effects of the WOTC and WtW on employment outcomes. First, I estimate the employment effects of the subsidies by comparing employment levels of WOTC/WtW-eligible workers to employment levels of similar but ineligible workers. Second, I estimate the wage and tenure effects of the subsidies by comparing WOTC/WtW participants to other WOTC/WtW-eligible workers who did not participate, thus isolating the effect of program participation.[2] I estimate these effects using a semiparametric matching estimation strategy that is more flexible than the linear regression methods often used to estimate subsidy effects. Matching allows me to account for selection into the subsidy program and to avoid strong assumptions about functional form. By using the panel version of this estimator, I control for any time-invariant unobserved factors that may affect employment outcomes.

My results indicate that the WOTC and WtW have limited effects on the labor market outcomes of the disadvantaged population. Eligibility for the subsidy does not appear to affect long-run employment probabilities, though this may be partly explained by limited participation among firms that hire eligible workers. When I examine the specific effects of being subsidized through the WOTC/WtW (apart from merely being eligible), the implications of my model find a bit more support. Workers are predicted to have about a 10 percent increase in earnings as a result of the WOTC/WtW, but they do not experience longer job tenure. My estimates indicate that about 40 percent of the value of tax credits is

[2]I do not, however, assume that workers select randomly into participation and nonparticipation, since companies that claim the WOTC/WtW may differ from those that do not. I address this potential heterogeneity by allowing for selection into participation based on employer traits. Note that this comparison between participants and eligible nonparticipants cannot be used to assess employment effects, since (by construction) all WOTC/WtW participants are employed.

passed on to the worker in the form of increased wages, suggesting substantial market power on the part of employers in the low-wage labor market. However, there do not appear to be any persistent wage gains following the WOTC job. These results suggest the WOTC and WtW programs may have slightly improved the labor market experience of the disadvantaged, but even the gains for those who are subsidized are modest, and low participation limits the number of workers affected.

I begin my discussion by noting some key elements of the WOTC and WtW programs and discussing the existing studies of the programs. I follow with a description of my search model of employment with subsidies. After presenting the implications of the model, I describe the data I use to test them. I then present the econometric methodology and results. I conclude with policy implications of these results and suggestions for future research.

II. THE WOTC AND WTW PROGRAMS

The WOTC is a subsidy to employers that hire new workers from certain disadvantaged groups, such as welfare recipients, young food stamp recipients (ages 18–24), poor veterans, youth from disadvantaged geographic areas, Supplemental Security Income (SSI) recipients, and low-income ex-felons.[3] While the employer must apply for the subsidy at the time of hire, the amount of the subsidy varies according to the number of hours worked at that firm by the employee in the first year of employment (after which the subsidy expires). The subsidy level is 40 percent of wages if the worker spends more than 400 hours at the firm, 25 percent of wages for 120–399 hours, and zero percent of

[3] There are specific requirements to qualify for each target group. For example, a worker must be from a family that has received welfare for at least 9 of the last 18 months (assessed at the time of hire) in order to qualify as part of the targeted welfare group. To qualify for the food stamp group, a worker must be between the ages of 18 and 24 (inclusive) and must be from a family that has received food stamps for the last 6 months (or at least 3 of the last 5 months if the family is no longer eligible). Definitions of the other target groups can be found at the U.S. Department of Labor Web site: http://www.doleta.gov.

wages for less than 120 working hours. The WOTC applies to up to $6,000 in earnings, resulting in a maximum potential subsidy of $2,400 per qualified worker.

The WtW applies specifically to long-term welfare recipients (with at least 18 months of continuous welfare receipt at the time of hire).[4] This subsidy is 35 percent of wages in the first year of employment and 50 percent in the second year, but it only applies to those who work at least 400 hours with the firm each year. The WtW subsidizes up to the first $10,000 in wages each year, resulting in a maximum subsidy of $8,500 over two years.

To receive the credits, participating employers typically ask applicants or new hires to fill out a simple one-page form indicating their membership in any of the listed target groups. If this form indicates WOTC or WtW group membership, the employer and new hire each fill out sections of an additional short form and both forms are submitted, along with basic documentation, to the State Employment Security Agency (SESA).[5] The SESA then assesses the worker's eligibility and sends the employer a written certification if the worker is qualified for one or both of the subsidies. The firm then claims the total subsidy (aggregated across all qualified workers) on its federal tax return.[6]

Government agencies have examined a few aspects of the WOTC and WtW programs. The U.S. General Accounting Office (2001) documented that, despite concerns to the contrary, employers do not appear to fire WOTC workers when their subsidies run out in order to replace them with new WOTC workers. The GAO (2002) also found that most of the (relatively few) firms that claim the WOTC are large companies that hire many low-skilled workers and qualify for total WOTC and WtW tax credits of

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[4]The WtW definition is technically divided into three groups: (a) members of families that received AFDC/TANF for at least 18 consecutive months ending on the hiring date, (b) members of families that stopped being eligible for AFDC/TANF payments after August 5, 1997, because federal or state law limited the maximum time those payments would be made (individual must be hired within two years from the date the assistance ended), and (c) members of families that received AFDC/TANF for any 18 months after August 5, 1997, and within two years of the hiring date. Most WtW certifications are based on membership in the first group.

[5]For example, a worker in the welfare group would submit her case number.

[6]A more detailed discussion of the program's administration can be found in Hamersma (2003).

$100,000 or more. The U.S. Department of Labor (2001) conducted a set of 16 in-depth case studies and concluded that employers have a positive assessment of the WOTC and WtW and that they view the subsidies as reimbursement for the added expenses of training and mentoring disadvantaged employees. However, employers report very few changes in recruiting, hiring, or wage policy based on the subsidy.

DeVaro (2001) presents the only previous theoretical model of the WOTC and WtW programs. After estimating a structural model of employment, he simulates the effect of a wage subsidy (defined as a one-time reduction in the fixed cost of hiring an unskilled worker). DeVaro's results indicate that tax credits should improve placement rates for low skilled workers and increase their starting wage distribution. These results depend upon the assumption that employers respond to the subsidies by adjusting recruitment strategies. DeVaro does not provide an empirical investigation of his model's implications.

The research so far on the WOTC and WtW programs has done little to assess whether these tax credits have met the goal of improving employment outcomes among the disadvantaged. Examinations of the labor market effects of past programs have had mixed results, and most have not taken subsidy participation issues into account when assessing these effects.[7] The remainder of this paper provides

[7]A number of studies of the New Jobs Tax Credit (NJTC) and Targeted Jobs Tax Credit (TJTC) focus primarily on the employer side of the market, using employer surveys to assess potential employment, displacement, and/or job tenure effects of a subsidy (Perloff and Wachter, 1979; Bishop and Haveman, 1979; O'Neill, 1982; Bishop and Montgomery, 1986; Bishop and Kang, 1991; Bishop and Montgomery, 1993). Other papers use individual-level survey data (such as the Current Population Survey) to assess participation rates and possible employment effects from the worker side (Christensen, 1984; Katz, 1998). All of these studies are limited because they can only examine the effects of eligibility for the credit, as the data do not indicate who is actually subsidized. A summary of their findings can be found in Bartik (2001). Hollenbeck, Willke, and Ershadi (1986) provide the only study of subsidy effects that addresses selection into certification, using data that indicate whether or not a person is certified for a subsidy. Their study suggests that TJTC eligibility improved employment only for nonwhite males and had a negative effect on other groups. Certification for the subsidy, on the other hand, appears to have increased wages, but was also associated with fewer quarters of employment for certified workers. Though the authors control for selection into certification, they do not address selection into eligibility. They also focus on the subsidy's effects on total earnings and total quarters worked after being subsidized, without examining the wages and tenure at the worker's subsidized job specifically.

theoretical predictions and empirical estimates of the effects of the WOTC and WtW on employment, wages, and job tenure.

III. A DYNAMIC THEORY OF EMPLOYMENT SUBSIDIES

My search model is based on key elements of models proposed by Jovanovic (1979) and Flinn (2003) to describe the labor market. Both prior models are based on the premise that worker and employer decisions (including whether to accept jobs, whether to offer jobs, and how to determine wages) are made over time, and can be subject to uncertainty (Jovanovic) or institutional constraints (Flinn). In Jovanovic's (1979) seminal model, workers choose whether to accept wage offers made by firms that view some noisy signal of each worker's productivity at that particular firm. Workers' wages are equal to their expected marginal product at the firm, which firms estimate by observing output each period with some error. This gradual learning treats job matches as "experience goods" whose value cannot be determined *ex ante*.[8]

Flinn (2003) introduces a minimum wage and investigates its effects on labor market outcomes and welfare in a search framework. Flinn incorporates the possibility of wage bargaining, and analyzes the effects of the minimum wage under different levels of worker bargaining power. Adding bargaining power to the model allows him to relax Jovanovic's assumption that workers are always paid their (expected) marginal products; this is an important consideration if firms in certain markets are able to extract some rents from workers and pay wages closer to the reservation wage.[9] However, Flinn's model assumes that there is no uncertainty about productivity, even at the time of hire. In the context of the low-wage labor market, in which employers might perceive some risks of hiring inexperienced workers, this

[8] Also see Ljungqvist and Sargent (2000), who present a simplified version of this model in which firms and workers operate in discrete time and face only one period of uncertainty about productivity.

[9] For example, Katz and Summers (1989) argue that workers and their employers divide rents, and that workers in industries with low capital-to-labor ratios (such as the service industry) are less able to extract rents from their employers than workers in other industries. See Murphy and Topel (1990) for an alternative explanation of these wage differentials across industries.

assumption is restrictive. I therefore develop a model that maintains the bargaining and minimum wage aspects of Flinn's model but incorporates a simple form of uncertainty based on Jovanovic (1979), allowing job matches to be characterized as experience goods. This hybrid model is extended to include wage subsidies for a particular subset of workers.[10]

A. Setup

I adopt a simple characterization of the labor market. Firms are homogeneous and engage in production and employment decisions in (infinitely many) discrete periods. Workers and firms are risk-neutral with discount rate β. There exists a distribution of worker-firm productivity levels given by the cumulative distribution function $F(\theta) \sim N(\mu,\sigma^2)$. Any match of a firm and worker is associated with an idiosyncratic match value θ that indicates the worker's productivity at that firm, where each θ is an independent draw from $F(\theta)$. The only factor of production is labor. In order to address the possibility of unemployment in the long run, each worker-firm match faces exogenous job destruction with probability η each period.

There are two government employment policies in my model. First, the economy has a binding minimum wage of m.[11] Second, the government provides wage subsidies for workers with certain characteristics (such as particular levels of welfare receipt), and the labor force can be divided into those who are eligible to be subsidized (*E*) and those who are not (*N*).[12] These types are fully observed by all parties.

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[10]Mortensen and Pissarides (2003) examine the effects of subsidies on labor market outcomes using a matching (rather than search) framework without a minimum wage. Their theory predicts that employment subsidies will increase employment and wages; this result also follows from my model.

[11]Wisconsin firms were subject to a federal minimum wage of $5.15 per hour throughout my sample period.

[12]Note that this model necessarily applies only to labor markets in which workers of both types exist. Also, I do not allow individuals to change their behavior in order to change their subsidy eligibility. While it is possible

Subsidized workers qualify their employers for a positive subsidy *S* in each period they are employed, and employers are assumed to claim the subsidy for all eligible workers.[13] The subsidy does not have a time limit.[14] I constrain *S* to be less than the minimum wage so that employers retain some positive monetary cost of hiring any worker. For purposes of model exposition, I assume that both types of workers draw from the same job match distribution *F(θ)*. This is a strong assumption; however, the implications of the model remain the same even if subsidized workers are assumed to draw from a distribution that is shifted to the left relative to the distribution for unsubsidized workers, provided the leftward shift is not too large relative to *S*.[15]

The job matching process occurs according to a specific sequence of events. An unemployed worker interviews with a randomly chosen firm once per period.[16] The worker's productivity at that firm is drawn from *F(θ)*. The worker and firm observe only a noisy measure of the match value, $y = \theta + \varepsilon$, where $\varepsilon \sim N(0, \sigma_e^2)$. For subsidized workers, the match value is increased by *S*. Firms will offer employment to any worker who provides an expected marginal benefit to the firm that is greater than or equal to the minimum wage, *m*.[17] Upon acceptance of an offer, a division of the expected match value is

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that this could occur in the WOTC and WtW, I assume for now that the potential gains to qualifying one's employer for a subsidy are not large enough to cause people to stay on welfare longer than they otherwise would.

[13]As noted in the introduction, current levels of participation in the WOTC and WtW programs are quite low. I will take this into account when I test the implications of this model with WOTC/WtW administrative data.

[14]Although the WOTC and WtW have time limits of 1 year and 2 years respectively, these limits are very seldom binding among the disadvantaged population due to the high turnover in low-wage industries (see U.S. GAO, 2001).

[15]In other words, the subsidy must at least compensate for the less-productive nature of the targeted group. A sufficient condition for this is given in the discussion that follows.

[16]For simplicity, I assume there is no unemployment compensation. Since unemployment compensation in the United States requires job histories that many welfare recipients do not have, this is a reasonable assumption in my context (see Gustafson and Levine, 1998). I also assume that only the unemployed can interview for jobs.

[17]This assumption abstracts away from concern about subsidized workers displacing other workers. The assumption is that even if subsidies make subsidized workers more desirable, the firms simply hire more subsidized workers without hiring fewer of those who are unsubsidized.

proposed (in the form of a first-period wage offer) based on Nash bargaining. All workers have the same bargaining power, $\alpha \in [0,1]$.[18] An exception to the Nash bargaining wage occurs when the bargained wage would be less than m, in which case the firm will offer m. If a firm is unwilling to make an offer of at least m, it does not make an offer and its "disagreement" outcome is zero. The applicant's disagreement value is the value of continued search next period, denoted Q, discounted by β.

If a worker accepts a job offer in the first period, both the worker and the employer observe the true match value θ before a second-period wage offer is made. The worker can then accept or reject the second-period wage offer, which is based on Nash bargaining over the (now determinate) match value, again constrained by the minimum wage.

B. Description of Equilibrium

In order to assess the labor market dynamics of this model, I work backward from the second period to determine the employment and wage offers in each period.[19]

The wage offers for those eligible for a subsidy (E) and those not eligible for a subsidy (N) will differ in the second period (and thus for all later periods). First consider the case of those who are not subsidized. In order to calculate the match surplus and eventually the equilibrium wage offer, I must first describe the worker's and firm's decision problems. The worker's maximized value of a job offer at wage w can be described by:

$$J(w) = \max\left\{w+\beta[(1-\eta)J(w)+\eta Q],\ \beta Q\right\}.$$

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[18]This level of bargaining power is unrelated to subsidy eligibility. This assumption may not be appropriate if there are unobserved differences between subsidized and unsubsidized types that contribute to differential bargaining power.

[19]Note that any job offer that is accepted in the second period will be accepted for every period following, so my analysis of the second period is the same as it would be if I were to start at any later period.

The value of accepting the job reflects the possibilities of either keeping the job in the subsequent period ($(1-\eta)J(w)$) or of losing the job and searching again (ηQ). The value function can be simplified to:

$$J(w) = \max\left\{\frac{w + \eta\beta Q}{1\text{-}\beta(1\text{-}\eta)}, \beta Q\right\}.$$

The worker will accept a job only if the wage offer makes the job's value higher than βQ. The appropriate reservation wage is:

$$w^* = (1-\beta)(1-\eta)\beta Q.$$

By assumption, the minimum wage is binding, therefore $m > w^*$. This means that all legal wage offers are accepted by the worker. The worker's surplus from an accepted job with wage w is:

$$\text{Worker's Surplus} = \frac{w + \eta\beta Q}{1\text{-}\beta(1\text{-}\eta)} - \beta Q.$$

The firm's problem is even simpler, since productivity is perfectly observable starting in the second period. The firm will make wage offers to any worker whose wage is less than or equal to the marginal benefit of hiring the worker. The firm's surplus from doing so is:

$$\text{Firm's Surplus} = \frac{\theta - w}{1\text{-}\beta(1\text{-}\eta)}.$$

The wage offer is determined by bargaining over the total surplus generated to both the firm and worker. Nash bargaining allows the worker to claim a portion α of this surplus. The resulting wage offer is a weighted average of the worker's productivity and reservation wage:

$$w = \alpha\theta + (1-\alpha)w^*.$$

Note that this general formula includes the special cases in which either wages are equal to the reservation wage (no bargaining power for workers, i.e., $\alpha = 0$) or wages are equal to the worker's productivity (no bargaining power for firms, i.e., $\alpha = 1$, as in Jovanovic's model).

The Nash bargaining wage will be offered by the firm as long as it is higher than the minimum wage m. This will be the case as long as θ is above some threshold level:

$$\hat{\theta} = \frac{m-(1-\alpha)w^*}{\alpha}.$$

Using the fact that $m > w^*$, I can show that $m < \hat{\theta}$. This means that some workers with productivity above the minimum wage (those with θ between m and $\hat{\theta}$) will not be paid the wage resulting from Nash bargaining, because this wage would be below m. However, the firm can always benefit from hiring a worker at the minimum wage m if that worker's productivity is above m. The minimum wage constraint simply forces the firm to divide the surplus in a way that meets the minimum wage requirement, resulting in a smaller (but still positive) surplus to the firm relative to the Nash bargaining surplus. This results in the constrained wage offer:

$$w = \begin{cases} \alpha\theta + (1-\alpha)w^* & \text{if } \theta \geq \hat{\theta} \\ m & \text{if } \theta \in [m, \hat{\theta}) \\ 0 & \text{if } \theta < m. \end{cases}$$

The parallel result for those who are subsidized follows directly from the discussion above. The primary difference is that the marginal benefit to the firm provided by the worker is the sum of his or her productivity and the subsidy ($\theta + S$). I subscript the reservation wage with an "*E*" for those in the subsidy-eligible population, since their value of unemployment will differ from that of the unsubsidized. The wage offer structure for subsidized workers is:

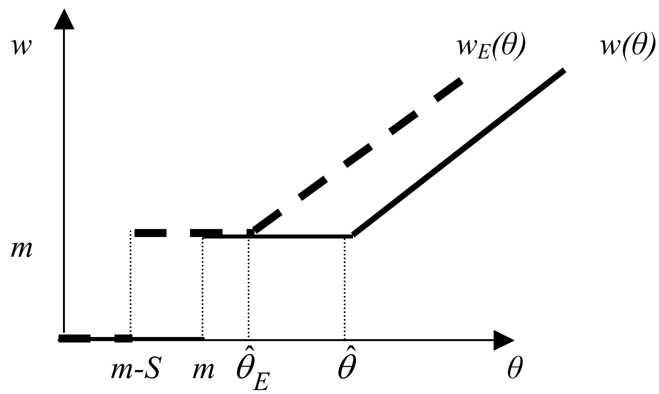
$$w_E = \begin{cases} \alpha(\theta + S) + (1-\alpha)w_E^* & \text{if } \theta \geq \hat{\theta}_E \\ m & \text{if } \theta \in [\text{m-S}, \hat{\theta}_E) \\ 0 & \text{if } \theta < \text{m-S} \end{cases}$$

where $\hat{\theta}_E$ is defined as: $$\hat{\theta}_E = \frac{m - (1-\alpha)w_E^*}{\alpha} - S .$$

In comparing these results to those of the unsubsidized population, I can show that $\hat{\theta}_E < \hat{\theta}$.[20] This results in the wage offer graphs shown in Figure 1:

[20]This follows from the argument that Q_E must be at least as high as Q, which must be true since the availability of a subsidy can only increase the value of a worker to the firm in this model.

FIGURE 1



I draw two main conclusions about the second period (or long run) from this figure. First, employment levels are higher among the subsidized; some lower-productivity workers gain employment through the firm's receipt of a subsidy that compensates for their less-than-minimum-wage productivity. Second, wages for the subsidized begin to increase (moving above the minimum wage) at a lower level of productivity. These conclusions imply that in the long run, subsidized workers have higher wages than unsubsidized workers of the same ability.

A similar strategy can be used to generate results for the first period. Details are provided in Appendix A. The combination of the first and second period results creates a series of implications discussed below.

C. Implications

This model generates unambiguous predictions about relative wages and employment rates, both over time and across types (subsidized vs. unsubsidized). It also generates useful insights about expected job tenure. All proofs are reported in Appendix A.

The following result compares the employment rates of the subsidized and unsubsidized groups in both the short run and the long run.

Result 1: Subsidy-eligible workers have higher employment rates than ineligible workers in both the short run and the long run**.**

This implication is a natural outcome of the model, since eligibility for a subsidy (within a population of relatively homogeneous workers) can only make a worker more attractive to firms. The result would remain even if subsidized workers drew from a leftward-shifted productivity distribution (i.e., $N(\theta\text{-}d,\sigma^2)$); a sufficient condition for this to hold is that the "net effect" of the subsidy is still positive (i.e., S - $d>0$).[21]

The second main result of the model relates to wage levels across the two groups over time:

Result 2: In both the short run and the long run, workers who are employed but not subsidized obtain lower wages on average than subsidized workers. This is also true for unconditional wages, since employment rates are higher among the subsidized than among the unsubsidized.

This result reflects the fact that workers obtain a share of the subsidy benefits provided to their employers, allowing them to have higher average wages than their unsubsidized counterparts.[22] The size of this share, or incidence of the subsidy, depends upon the amount of bargaining power workers have and on whether the firm's wage offer (which depends on the worker's productivity) is constrained by the

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[21]The necessary condition is (of course) less restrictive, but is also less intuitive and requires some extra notation (it is available upon request). Burtless (1985) and Hollenbeck, Willke, and Ershadi (1986), among others, suggest that employers may discriminate against subsidy-eligible workers in such a way that the value of the subsidies does not make up for the perceived negative attributes of the qualified workers. If subsidized workers are not actually less productive, however, firms that discriminate will receive lower profits than those that do not, which presumably will cause discrimination to be phased out in the long run. However, suppose subsidy-eligible workers are indeed less productive than other workers. If subsidized workers are still less successful in the job market, this is not necessarily an argument against the effectiveness of subsidy programs *per se*. It may simply be an example of a program in which the level of the subsidy has not been set sufficiently high to cause subsidized workers to have a net advantage in the labor market relative to nonsubsidized workers. This highlights the importance of assembling an appropriate comparison group when studying effects of subsidies.

[22]This strict inequality occurs as long as $\alpha > 0$; for $\alpha = 0$, all employed workers are paid minimum wage.

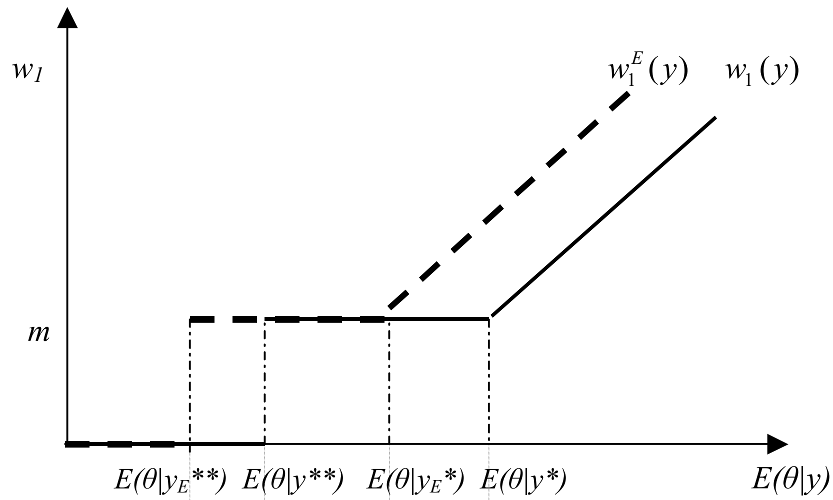
minimum wage. Subsidized workers who are paid the minimum wage (i.e., have relatively low expected or actual match values) receive a larger portion of their subsidy than those with better matches. This occurs because the firm is allowed to keep only that portion of total surplus (including the subsidy) that exceeds the minimum wage, rather than the portion that would be arranged under unconstrained Nash bargaining.

The combination of the implications discussed above allows for the clear graphical illustration of the equilibrium employment and wage outcomes in Figure 2. This figure shows the second-period wage pattern (already discussed in Figure 1) along with its relationship to first-period wages (conditional on expected productivity). The first-period graph uses the notation y^{**} to refer to the initial value of y needed for hiring to occur, and y^* to refer to the threshold above which workers receive the Nash wage rather than m. Notice that employment standards in the first period are lower than in the second period for both subsidized and unsubsidized workers. This means firms will hire some workers even with the expectation that these workers will fail to meet the standards for continued employment when their productivity is revealed in the second period (graphically, $E(\theta|y^{**}) < m$ and $E(\theta|y_E^{**}) < m\text{-}S$).[23] This feature reflects the modeling of labor as an experience good. When comparing the subsidized and unsubsidized workers, note that those who are subsidized have higher wages and employment rates in both the short run and the long run due to the increased marginal benefits they provide to the firm relative to equally productive unsubsidized workers.

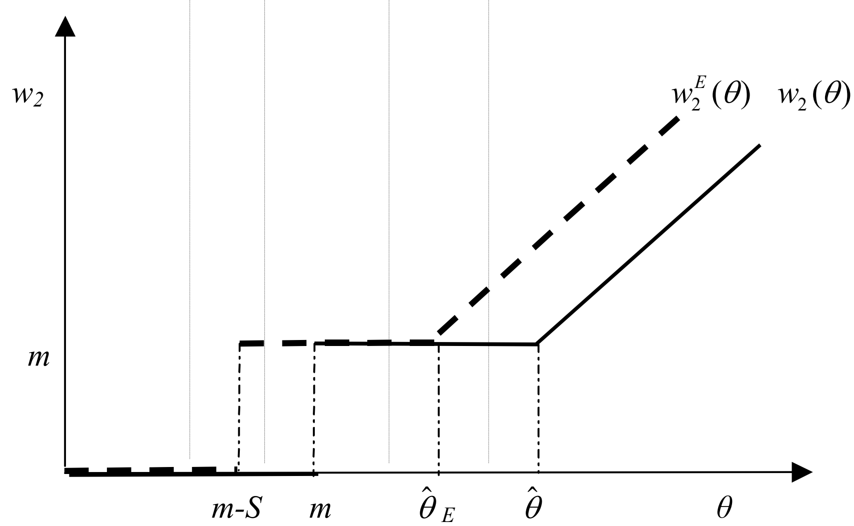
[23]The intuition for this is that the firm can fire a worker in the second period if that worker is not profitable to employ (for a disagreement outcome of zero) while the firm will obtain strictly positive surplus if the worker turns out to be productive enough to remain at the firm. These asymmetric possible outcomes lead the firm to hire some workers whose expected outcome is below that needed for continued employment.

FIGURE 2

Period 1:



Period 2:



Notes: The Period 1 graph uses $E(\theta|y)$ rather than y itself to enhance the comparability between the two periods. The implicit expressions for the threshold levels of $E(\theta|y)$ are provided in Appendix A. Result 1 can be seen graphically by noting that $E(\theta|y_E{**}) < E(\theta|y{**})$; since $E(\theta|y)$ is strictly increasing in y, this means $y_E{**} < y{**}$. Result 2 can be seen by noting that the wage distribution of the subsidized weakly dominates that of the unsubsidized.

Result 3: While subsidies may affect job tenure, the relationship between the tenure of subsidized and unsubsidized workers cannot be predicted because of competing forces in the model.

Given expressions for the relative employment rates of the two types of workers in each period, it is theoretically possible to compare expected job tenure or (equivalently) retention rates. Unfortunately, while the retention rate (i.e., the probability of continuing employment conditional on being hired) can be expressed for each group, the implicit nature of the relevant expressions makes an analytical comparison infeasible (see Appendix A for these expressions). However, even without an explicit solution, the model does suggest two competing forces that affect comparative job tenure. Being subsidized implies facing a lower productivity standard to maintain employment in the second period (relative to an unsubsidized person). It follows that *conditional on equal productivity*, a subsidized person is more likely to be retained, implying that the subsidized will have longer average tenure. However, the workers being considered for retention do not have equal productivity on average; the subsidized workers hired in the first period are of lower productivity on average (due to facing a lower hiring standard). This lower productivity suggests that the average subsidized person may be *less* likely to be retained, all else equal.

In addition to these competing forces within the model, there are two specific institutional features of the WOTC/WtW that could potentially affect tenure.[24] First, workers must meet an hours requirement (120 hours on the job) to qualify their firms for any subsidy; this should (at least near the margin of that requirement) cause firms to increase tenure of the subsidized. Second, the one-year time limit of the WOTC suggests that subsidized workers will eventually lose the advantage of facing lower retention standards, resulting in increased turnover (i.e., decreased tenure relative to the unsubsidized) near the one-year margin. Since both the model and the institutional features of the WOTC suggest competing influences of the subsidy on job tenure, I do not make a hypothesis on the sign of the job

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[24]Both features are left out of the model to maintain the stationarity that keeps the model relatively tractable.

tenure effect but treat this as an empirical question. The sign and significance of my estimate could help identify the dominant forces in the determination of tenure.

D. Model Summary

This dynamic search model suggests a number of benefits that may accrue to workers and firms as a result of wage subsidies. A subsidy can benefit workers by providing employers with a reason to hire them even if their productivity at a particular firm is not as high as the wage they are paid. The effect of the subsidy on wages and long-term employment of subsidized workers is also positive. Firms benefit from the subsidy program because they can capture part of the additional (subsidy) surplus obtained when they hire a subsidized worker. While the improvement in wages and employment rates is consistent with the predictions of a static model, this model provides additional insight into dynamic employment patterns.[25] The model suggests that a subsidy-eligible worker who is relatively productive at a particular firm (high θ) may benefit from a subsidy because it makes her more likely to be hired initially (even with "bad draws" of ε). In this sense the subsidy encourages experimentation with workers who may initially appear to be less productive than they actually are.[26] Finally, the model highlights some potential (competing) influences of subsidies on job retention. These findings suggest that a dynamic model of employment subsidies provides the same (intuitive) results as a static model, but also generates additional insights into expected employment patterns and durations.

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[25]See Dickert-Conlin and Holtz-Eakin (2000) for a static model of employment subsidies and for an extension of this model to cases in which subsidized workers experience stigma.

[26]To be clear, firms also experiment to some extent with unsubsidized workers, as this is a result of the uncertainty in the model rather than the subsidy *per se*. However, the subsidy expands such experimentation for the subset of workers who are subsidized.

IV. DATA

To test the results and explore the insights generated by the model, I use a rich administrative data set that includes detailed demographic and employment information for welfare recipients in the state of Wisconsin. The data are collected from three sources: the Wisconsin Client Assistance for Re-employment and Economic Support (CARES) database, the Wisconsin WOTC/WtW database, and the Wisconsin Unemployment Insurance (UI) database. The CARES data, provided by the Institute for Research on Poverty, contain demographic and monthly welfare and food stamp data for every person with any record of welfare receipt in 1998–2001. It is important for me to have detailed data on welfare receipt, since I need to identify WOTC/WtW-eligible and nearly eligible individuals in order to estimate the effect of eligibility on employment (Result 1).

I merge both the WOTC/WtW database and the UI records to the individuals in the CARES data. The WOTC/WtW database, provided by the Wisconsin Department of Workforce Development, includes records for all WOTC/WtW applications from mid-1998 to early 2003. These data allow me to identify which eligible individuals were actually certified for the WOTC/WtW. Since participation in the subsidy program is low, I investigate wage and job tenure effects of the WOTC/WtW programs (Results 2 and 3) by comparing workers who are certified to similar workers who are eligible but not certified. UI records from 1996 to 2002 provide the key measures of employment outcomes for each worker. These records include quarterly earnings and industry for each job held. If an individual is certified for the WOTC/WtW, the certified job can be identified by matching employer numbers (available in both the WOTC/WtW database and the UI records) so that I can examine outcomes at this specific job. This unique combination of administrative data from three sources is the richest that has been used for evaluating employer hiring subsidies.

Since my model applies to a group of subsidized and unsubsidized workers who are in the same labor markets, I choose the relevant population from these data by including only WOTC-eligible or nearly WOTC-eligible individuals in my analysis. In order to qualify for the WOTC as a member of the welfare target group, one must have at least 9 months of welfare receipt within an 18-month period. Eligibility for the WtW requires 18 months of continuous welfare receipt. For my study, I assemble records for all welfare recipients who received at least 6 months of welfare within any 18-month period in 1998–2001. Those with 6 to 8 months of receipt serve as a nearly eligible comparison group to the WOTC/WtW eligible population. I do not have information to determine the eligibility of others in the data who may qualify for the WOTC as part of some other target group (poor veterans, ex-felons, high-risk youth, etc.). However, I am able to examine the food stamp receipt of each person in the data, and I drop any observations who are nearly eligible via welfare and *already* eligible via food stamps, since they could confound the estimation of the effects of eligibility.[27]

There are 10,422 people in my subpopulation of interest, of which 9,123 were WOTC- or WtW-eligible via welfare receipt at some point between July 1999 and December 2000.[28] A total of 1,028 WOTC and WtW certifications were issued for members of my sample who started work in this period. Descriptive statistics for this population can be found in Table 1. This table indicates that the population has some features common to disadvantaged populations in general: a large fraction are racial minorities and over half the population has less than a high school degree.

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[27]I do not separately estimate the effects of becoming eligible via food stamps. I do this partly because assessment of eligibility in terms of food stamp receipt is trickier, but primarily because the age group that qualifies (18–24) may have complicated labor market patterns due to schooling decisions.

[28]I assign WOTC/WtW eligibility based on monthly welfare records. Note that although I have welfare participation data from 1998, I do not assess WOTC eligibility during that time period. In order to definitively assess eligibility at any point in time, I must be able to examine each person's welfare records for the past 18 months. I therefore start assessing monthly eligibility in the 19th month of my sample, which is July 1999.

TABLE 1
Demographics for W-2 Sample

Variable	Category	% of Sample	Variable	Category	% of Sample
Race	White	16	Gender	Female	87
	Black	60		Male	13
	Other	12	Education	Less than high dchool	56
	Unknown	12		High dchool	31
Ever Eligible	Yes	88		Some vollege	7
for WOTC?	No	12		College fegree +	< 1
Ever Certified	Yes	10		Unknown	6
for WOTC?	No	90			

Notes: N=10,422 (4,227 are in the employment effect estimation sample; 7,647 are in the wage/tenure estimation sample; there are 1,452 individuals that are in both groups).

Educational distribution changes over time; this table reflects education level in the year in which WOTC certification or eligibility "treatment" occurs (or the year in which it almost occurred, for the comparison group). This quarter is defined in more detail as "t0" later in the paper. The reported distribution includes some logical and reasonably uncontroversial imputations, but leaves as unknown those values for which there is no obvious reasonable imputation (details available upon request).

"Ever eligible" and "ever certified" refer to being eligible/certified in the period of interest: July 1999–December 2000.

Race is unknown in some cases because the data rely on a caseworker's assessment. In the data analysis I combine those in the "unknown" group with the "other" race category.

Further details on the selection of this sample are given in the Estimation and Discussion section of the paper.

As noted in my description of the model, I do not distinguish theoretically between eligible and subsidized individuals, since employers are assumed to claim any subsidies for which they are qualified. However, low participation rates in the WOTC/WtW indicate that the distinction between eligibility and subsidy certification must be accounted for empirically. To do this, I examine the wage and tenure effects of the WOTC/WtW using a method that allows me to consider employers' willingness to have an eligible worker certified through the programs. In this estimation, I take into account employer characteristics that may influence the likelihood that an eligible worker is certified (for example, the employer's industry and propensity to hire disadvantaged workers). The implicit assumption in this selection correction is that controlling for these characteristics is sufficient to explain any systematic factors that cause selection into certification (i.e., the “selection on observables” assumption). Given this assumption, I can isolate the effects of certification on employment outcomes. In parts of the analysis, I am able to relax this assumption and instead allow time-invariant differences between groups over time.

The state of Wisconsin is particularly interesting for this study not only because of the detailed data available, but also because of its recent welfare reforms and their potential contribution to WOTC effectiveness. The Wisconsin Works (W-2) program was implemented as part of a state welfare reform in the fourth quarter of 1997, a year after the WOTC was introduced. W-2 was arguably the most work-oriented welfare reform in the nation, characterized by an insistence on immediate job-seeking that reflected a continuing commitment to the strong, employment-based welfare policies that Wisconsin started implementing in the late 1980s (Kaplan, 2000). The program has been seen as a national model for work-first approaches to welfare in which the role of job training or education is deemphasized in favor of work experience.

V. MATCHING METHODOLOGY

I test the employment and wage implications of my model, and also investigate the job tenure issue, by applying the econometric method of matching to the relevant sample for each question. The matching estimator for treatment effects was introduced to the economics literature in a series of papers published in the late 1990s by Heckman, Ichimura, Smith, and Todd (Heckman, Ichimura, and Todd, 1997; Heckman, Ichimura, Smith, and Todd, 1998; Heckman, Ichimura, and Todd, 1998). This approach allows estimation of treatment effects in contexts where there is nonrandom selection into treatment. Since it is semiparametric, it relies on fewer functional-form assumptions than typical parametric methods that rely on the same underlying assumptions on the selection process. Matching also deals explicitly with the so-called “common support” problem, in which some treated individuals have no observationally similar counterparts among the untreated and thus cannot be reasonably used in estimation of treatment effects.[29] Finally, a panel version of matching allows me to utilize the longitudinal nature of my data by examining outcomes both before and after the treatment. These features of the matching method suggest that it may be more effective and flexible than, for instance, the linear fixed-effects estimation used by Hollenbeck, Willke, and Ershadi (1986) to analyze the Targeted Jobs Tax Credit, a past tax credit program similar to the WOTC/WtW.

Matching generates a consistent estimate of the average effect of treatment on the treated, which can be expressed as

$$E(Y_1 - Y_0 \mid D = 1),$$

where Y_1 is the labor market outcome in the presence of treatment (either WOTC eligibility or WOTC certification), Y_0 is the outcome in its absence (which is unobserved), and $D = 1$ indicates treatment. The matching estimator takes the form

$$\hat{M}(S) = \sum_{i \in I_1} \frac{1}{N_1} [(Y_{1i} - \hat{E}(Y_{0i} \mid X_i, D_i = 1)] ,$$

where I_1 indicates the treatment group, N_1 is the size of the treatment group, and X_i is a vector of characteristics that affect selection into treatment and/or outcomes. The expectation is estimated for each treated individual using semiparametric methods that utilize those in the comparison group who are observationally similar to that individual (in terms of X_i).

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[29]Typical regression analyses seldom address this issue, and therefore include these observations in their estimation.

In this study I use a version of matching with two modifications.[30] First, I do not match on all X_i variables but instead match on the estimated probability of being treated, $P_i(X_i)$ (called the propensity score). This limits the dimensionality of the estimation problem.[31] I discuss the specific choice of X_i in the next section when I estimate $P_i(X_i)$ for each type of treatment (eligibility and certification). Second, when possible I use information both before and after the treatment to account for unobservable individual heterogeneity that may not be captured in the propensity scores. Let t' indicate a period before anyone is treated and t a period after those in the treatment group are treated. Then the panel version of the propensity score matching estimator is:

$$\hat{M}(S) = \sum_{i \in I_1} \frac{1}{N_1}[(Y_{1it} - Y_{0it'}) - \sum_{j \in I_0} W_{N_0,N_1}(i,j)(Y_{0jt} - Y_{0jt'})] \qquad \text{for } P_i \in S,$$

where I_0 indicates the comparison group and S is the set of P_i s that are in the common support of the treatment and comparison groups.[32] $W_{N_0,N_1}(i,j)$ is a local linear weight that allows each treated person to be compared to a weighted average of the comparison group such that the largest weights are placed on those observations most similar to herself (in terms of P_i).

This panel version of the propensity-score matching estimator will be consistent for the effect of treatment on the treated if the following mean-independence condition holds:

$$E\{Y_{0t} - Y_{0t'} \mid P(Z), D=1\} = E\{Y_{0t} - Y_{0t'} \mid P(Z), D=0\}.$$

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[30]This method is formally referred to as "semiparametric conditional difference-in-differences matching" and is developed in detail in Heckman, Ichimura, and Todd (1997).

[31]Matching directly on X_i requires data on both treated and untreated individuals with every possible combination of all variables in X. My sample, like those used in most applications of matching, is not large enough to meet this strong requirement. Rosenbaum and Rubin (1983) discuss the use of the propensity score to address this issue.

[32]The common support is the set of P_is for which there are both treatment and comparison group observations, i.e., $S = \text{Supp}(P \mid D=1) \cap \text{Supp}(P \mid D=0)$. There may be some treatment group observations with very high probabilities of treatment for whom there are no corresponding comparison group members; these observations are removed prior to estimation. I use the 2 percent trimming method described in Todd (1999).

This condition states that if there had been no treatment, those in the treatment group would have had the same *change* in outcomes over time as those in the comparison group, conditional on their probability of being treated. Essentially, people with the same probability of treatment are assumed to have the same outcome "trajectory" in the absence of treatment, regardless of whether they are ultimately treated.[33] This assumption is most appropriate when the treatment and comparison groups are drawn from the same population and can reasonably be assumed to differ only in their treatment status after P_i is accounted for. My data include a fairly homogeneous group of individuals (all on welfare in Wisconsin for at least 6 months), so the assumption seems reasonable in this application.

VI. ESTIMATION AND DISCUSSION

A. Result 1: Employment Effects

Result 1 states that subsidized workers will have higher employment levels in both the short run and the long run relative to unsubsidized workers. I test this implication of the model using a sample of individuals who either became eligible or nearly became eligible for the WOTC/WtW credits in July 1999–December 2000.[34] The data are quarterly (since all outcome variables from the UI data are quarterly) and I identify the quarter in which eligibility ("treatment") begins by examining welfare histories. A worker is classified as becoming eligible for the WOTC/WtW in a given quarter if he or she reached the threshold of having at least 9 months of welfare receipt in the last 18 months. The quarter in which eligibility begins, which varies across the treated workers in the sample, is labeled t0 (for "time zero"). I form the comparison group by identifying workers who become "almost eligible," i.e., who have 6–8 months of welfare receipt in the last 15 months as of a given quarter and are therefore not yet (quite)

[33]The conditional mean outcome levels between treatment and comparison group are free to differ by some (constant) level over time without treatment.

[34]The CARES data also contain data for the year 2001. I chose this time frame so that I could examine outcomes for several quarters following treatment.

WOTC eligible. If such a worker becomes eligible in the following quarter, she is assigned to the treatment group, so (by construction) the comparison group contains only workers who do not become eligible for the WOTC (i.e., welfare leavers). I again use t0 to label this “almost eligible” quarter for the comparison group. All employment outcomes for both treated and untreated workers are examined relative to quarter t0, which differs across individuals, rather than according to a specific calendar quarter (see Heckman, Ichimura, Smith, and Todd (1998) for an example of a similar strategy).[35]

A first-order concern with any difference-in-differences estimation strategy is that the treatment and comparison groups may vary in unobservable characteristics in a way that is not time invariant, thus violating the identification assumption. For instance, one may be concerned that the comparison group of “welfare leavers” could potentially be systematically different from my treatment group of those who become WOTC eligible, since some of them stay on welfare only a bit longer than the comparison group while others remain on assistance for years. For this reason, I also report a separate set of estimates that drop particularly long-term welfare recipients from my sample, since they may have an unintended negative effect on average outcomes for the treatment group. In Appendix B, I provide evidence that the treatment and comparison groups appear to differ in a fairly constant way in the 3.5 years prior to t0.

For the first stage of my analysis, I estimate the probability of becoming eligible for the WOTC using a logistic regression.[36] The independent variables that I use to predict eligibility (i.e., additional welfare takeup) are age, age squared, age cubed, gender, educational level, race, number of children under

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[35]In addition to the main requirements given for sample inclusion and treatment/comparison assignment, I also require that: (a) sample members must be at least 16 years old; (b) sample members cannot have been WOTC certified in the recent past (1998:1–1999:2); (c) those in the comparison group must not become eligible or certified for WOTC between 1999:2 and 2001:4 (since this would contaminate the treatment/comparison distinction). This leaves the sample size at 4,227 (as noted in Table 1).

[36]Black and Smith (2004) note that using a nonparametric estimator for P(X) reintroduces the curse of dimensionality, and that the literature has found it better to impose parametric assumptions on the first stage of estimation (P(X))and maintain nonparametric matching of outcomes rather than the alternative (nonparametric estimation of P(X) with parametric estimation of the outcome equation).

6, county unemployment rate, and months of benefit receipt (welfare and food stamps) in the previous quarter. I also include quarterly and regional dummies.[37] This specification performs reasonably well in the regression-based balancing test presented in Smith and Todd (2005).[38] The results of the estimation are in Table 2.

Most of the results of this logistic regression are not surprising. Individuals with higher levels of education, relative to the omitted category of "less than high school," are less likely to become eligible for the WOTC/WtW. Age is also strongly related to the probability that a person is eligible, but the relationship is quite nonlinear. More extensive welfare use in the previous quarter predicts a lower likelihood of staying on welfare long enough to become WOTC-eligible; this seems a bit counterintuitive, but may be a result of using a sample that was not *previously* eligible for the WOTC (many long-term recipients are thus not in the sample at all, and these recipients may drive the usual expected negative duration dependence).[39]

I use these results to generate propensity score estimates for each person to represent their predicted probability of WOTC/WtW eligibility in month t0. I then use the propensity score estimates to generate individual-level estimates of what the employment outcome for each eligible person *would have*

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[37]I assign regions based on county of residence using a study of appropriate clustering of Wisconsin counties for analytical comparisons (Shields and Deller, 1996). The regions are defined based on a set of economic and demographic characteristics (not necessarily on geography). I collapse some of these suggested regions (due to small sample sizes in low-population regions) to create 4 regions: Milwaukee, Dane County (Madison), Urban/Manufacturing areas, and Tourism/Agriculture/Other areas. When workers do not have a reported county (and thus region) in quarter t0, I impute the county using their own records for up to a year before or after.

[38]Almost all of the 21 variables balance by this measure; just one is statistically significant at the 1 percent level, while three others are significant at the 10 percent level, and the other 17 are not statistically significant. I tried several other specifications with different higher-order and interaction terms and did not find one that performed better. See Smith and Todd (2005) for details about this specification test.

[39]The welfare and food stamp variables are clearly endogenous to participation. This is not problematic, however, since the estimation of P(Z) has a specific statistical purpose: P(Z) acts as a one-dimensional substitute for the vector Z in order to represent the effects of Z on the participation decision while avoiding the curse of dimensionality introduced by matching on every combination of Z values. The only econometric restriction on the choice of Z is that for each Z, any correlation between Z and the error term in the outcome equation for the untreated state (U_0) is the same both for those who ended up being treated and those who did not. This correlation does not need to be zero (i.e., variables do not need to be exogenous) as in a typical regression model.

TABLE 2
Propensity Score Estimation for Selection into Eligibility

Variable	Odds Ratio	Std. Error	z-Statistic
Age	0.453	0.064	-5.63
Age squared	1.021	0.004	4.98
Age cubed	0.9998	0.00004	-4.36
Female	1.048	0.149	0.33
High school diploma	0.753	0.063	-3.4
Some college	0.577	0.072	-4.43
College degree	0.846	0.393	-0.36
Black	0.840	0.093	-1.57
Other race	0.896	0.109	-0.9
Number of children under 6	1.030	0.040	0.76
Months of welfare in previous quarter	0.886	0.042	-2.56
Months of food stamps in previous quarter	1.058	0.044	1.37
County unemployment rate	0.989	0.068	-0.16

Notes: Sample size: 3,556 ($N_{treatment}$ = 2,485, $N_{comparison}$ = 1,071). A little less than 5 percent of the initial sample of 4,227 individuals was lost due to missing data on education, county, or number of children under 6. The values reported in this table are from the sample that also drops long-term recipients (543 individuals); the values with the unrestricted sample are similar.

Pseudo R^2 = .042.

Four region indicators, six quarter indicators, and an intercept are also included in the estimation.

Omitted indicators: Education = less than High School, Race = Caucasian .

County unemployment rates are annual rates, as reported by the Bureau of Labor Statistics, for the year in which t0 occurs (1999 or 2000).

been if she had not become eligible. I do this using local linear regression weights applied to the comparison group, which allow comparison group members with propensity scores most similar to a particular treated individual to be given the most weight in that individual's counterfactual estimate. After transforming the dependent variable into its change over time to eliminate any time-invariant unobserved heterogeneity between treatments and controls, I can then estimate the effect of eligibility on the change in employment status of each eligible person. I average these effects to obtain the matching estimate of the effect of eligibility on employment.

I use three different measures of employment to test the hypothesis that employment levels are higher for subsidy-eligible workers in both the short run and over the course of the year following initial eligibility. First, I use an indicator for employment in the second quarter following t0, roughly 6 months after eligibility has been established (treatment) or nearly established (comparison). I expect this to represent the short-run effect of the subsidy.[40] Second, I look at employment in the fourth quarter, when long-term effects may have begun to set in. Finally, I test effects of eligibility over the longest period of time available in my data using the probability of *ever* being employed in the year and a half following t0. All of the estimates reflect the use of differencing, in which I measure the change in the outcome between the period of interest and the appropriate past quarters (symmetrically about t0). The longer-term estimates are likely more reliable due to stronger support for the difference-in-differences assumption (see Appendix B). Table 3 contains the estimates.

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[40]I do not report results for the first quarter after eligibility has been established, because I am concerned that the structure of the treatment and comparison groups likely has a large influence on those results. My concern is that by construction all of the comparison group members are "welfare leavers," and in this first quarter just following their welfare spell they are more likely to be employed than perhaps they would be in the longer term, since some of them likely left welfare for a job; at the same time, few of the treatment group members have had time to respond to the subsidy program upon becoming eligible so recently. The results for the first quarter suggest negative employment effects of the subsidy that are likely an artifact of this sample construction. I want to avoid this endogeneity problem and look at longer-term trends, when many of the treatment group members also become "leavers" and have had time to respond to the existence of the subsidy.

TABLE 3
The Employment Effects of WOTC/WtW Eligibility

	Probability of Employment in the 2nd Quarter after t0	Probability of Employment in the 4th Quarter after t0	Probability of Any Employment in the 6 Quarters following t0
Estimate of the effect of eligibility on employment	.021 (.025)	-.030 (.029)	-.037* (.019)
Estimate of the effect of eligibility on employment (without long-term welfare recipients)	.056** (.027)	.002 (.027)	-.008 (.021)

Notes: t0 is the quarter in which eligibility is first established (treatment group) or is nearly established (comparison group). Matching is done on log(P(X)/(1-P(X)) so that estimates are robust to choice-based sampling (see Smith and Todd, 2005). I use cross-validated bandwidths based on the full sample for each estimate (BW = 3 and 0.05, respectively). I also trim away the 2 percent of treated observations for which the comparison group has the lowest kernel density, as in Heckman, Ichimura, Smith, and Todd (1998).

Row 2 drops those with more than 24 months of welfare receipt in 1998–2001 from the treatment group (there are no long-term recipients in the comparison group).

Standard errors are estimated via 200 bootstrap replications.

Sample Sizes: Row 1 – $N_{treatment}$ = 2,947, $N_{comparison}$ = 1,071
Row 2 – $N_{treatment}$ = 2,436, $N_{comparison}$ = 1,071.

Table 3 does not offer strong support for positive WOTC employment effects. As expected, the second row of estimates (without long-term welfare recipients) is a bit more encouraging than those in the first row. The difference between the two rows suggests that long-term recipients do in fact influence estimates negatively; I focus on the second row of estimates since they are likely more accurate. The first entry in the second row suggests that eligibility for the WOTC/WtW is associated with a 5.6 percentage-point higher likelihood of being employed in the second quarter after eligibility is established in comparison to the employment rate expected without the program. This effect is statistically significant at the 5 percent level and seems to suggest a meaningful and perhaps economically large effect of the program. However, a longer-term examination of the data reveals that any short-term effects do not last long. Both of the longer-term employment indicators suggest no measurable effect of the program after a year or more.[41] These estimates suggest that the WOTC/WtW may (at best) have a positive effect on employment near the time eligibility occurs, but is unlikely to have any effect in the long run.

Low rates of participation in the WOTC/WtW programs are one likely reason for such small, imprecisely estimated long-term employment effects. In the sample of 2,947 eligible individuals used in this estimation, 2,000 obtained jobs at some point in the year following their initial eligibility, but only 175 were certified for the WOTC/WtW.[42] This suggests a participation rate of less than 10 percent, which is on the low end of previous estimates for the welfare target group (Hamersma, 2003).[43] Even if the subsidy had large effects on these participants, it is unlikely to affect the average employment rate of the whole eligible group substantially. For instance, suppose that the subsidies' effects were so large that fully half of these certified workers would have been unemployed without the program. While this would be a 50 percent marginal employment effect among the certified (substantially larger than the 13–30 percent estimated by Bishop and Montgomery, 1993, in their study of the Targeted Jobs Tax Credit), it would generate only a 3-percentage-point improvement in employment rates among the eligible group as a whole.[44] If the actual marginal effect is smaller, precise estimation would require much larger samples.[45]

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[41]Similar results are obtained with alternative measures of employment, including the probability of employment in the third quarter after t0, probability of any employment in 4 quarters following t0, and total quarters employed in 4 quarters and 6 quarters following t0. Details are available upon request.

[42]I look at one year instead of 6 quarters due to limited WOTC data for 2002. I was able to use 6 quarters in the estimation because it utilized UI data for 2002 employment outcomes but only required WOTC data through 2001.

[43]One reason for this low estimate may be that I am only counting certification among those whom I have determined to be eligible for the program. The data suggest that there are likely some certified workers who are *in*eligible (who would have been counted in Hamersma, 2003) who are not counted here.

[44]I calculate this by comparing the rate of any employment among the eligible group in the first year after eligibility was established (67.9 percent) to the employment rate that would have occurred if half of the certified workers had been unemployed (64.9 percent).

[45]A basic power calculation suggests that with the current sample sizes, I would have only a 54 percent probability of properly rejecting the null hypothesis of "no effect" at the 10 percent level *even if* there were actually a 3-percentage-point effect (50 percent marginal effect). This calculation is based on a treatment group employment rate of 67.9 percent (the rate in my sample) and a hypothetical comparison group average of 64.9 percent. (Even if my sample size for the comparison group were as large as that of my treatment group, I would still only have a 78

This means that we cannot necessarily rule out the possibility that workers who actually participate in the WOTC/WtW experience meaningful gains. However, the evidence provided in this analysis does not suggest a large effect.

B. Result 2: Wage Effects

My second theoretical result states that subsidized workers will have higher wages than their unsubsidized counterparts in both the short run and the long run. Since many workers who are eligible for the WOTC via welfare receipt do not have jobs in the relevant time frame, I cannot use the same sample as I used in the employment-effect estimation. To address wage effects, I assemble a sample of all Wisconsin workers who started a new job sometime in the period July 1999–December 2000 and who were eligible for the WOTC or WtW (via welfare receipt) in the quarter that the job began.[46] This provides me with a relatively homogeneous population of workers who vary on the dimension of whether or not they were actually WOTC/WtW certified by their employers. Focusing on this sample and controlling for employer differences that are predictive of certification allows me to isolate the effect of subsidy certification.

There are some sample construction issues that arise in estimating wages. First, some workers have more than one WOTC/WtW-certified job in their records (1998–2002), and others have WOTC/WtW applications submitted on their behalf but are never certified. To avoid confusion in the definition of treatment, I retain a treatment group consisting only of people with a single WOTC/WtW

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percent probability of properly rejecting the null). If the marginal effect of WOTC eligibility on employment is less than 50 percent, the detection of an employment effect is even less likely.

[46]This sample initially contains 1,019 certified and 6,763 uncertified workers. Due to missing employer identification numbers, I lose about 13 percent of the treated sample when I match the WOTC job data to UI records. This results in the sample of 7,647 observations used to calculate the descriptive statistics in Table 1. Prior to the propensity score estimation, I also drop uncertified workers if there are no certified people in their county or in their industry (since they would be unhelpful in constructing counterfactuals for certified workers). These losses reduce the sample to 884 certified and 6,317 uncertified workers.

certification and a comparison group consisting only of people with no WOTC/WtW records of any kind and thus no possible interaction with the program.[47] Second, I need to identify a single job (for which the worker was hired while WOTC/WtW eligible) for each person in the sample. Clearly, for the treatment group I choose their WOTC-certified job. For the comparison group, some sample members have more than one new job for which they could have been certified in the time frame. I use a random selection mechanism to choose from among the possible jobs for each comparison group member, and assign the randomly chosen job as the relevant one for comparison with the employment outcomes of the treatment group.[48]

The first step in using the data is to again estimate propensity scores. Since all workers in the sample are eligible, I need to identify particular traits of workers or jobs that predict certification for the subsidy. In their 2002 report, the General Accounting Office found that certain industries (such as service and hospitality) are more likely to certify workers than others. The report also indicated that those employers who participate typically have a large number of qualified employees. I therefore include indicators for industry and estimates of the size of the qualified population at the firm.[49] I also include an

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[47]A significant fraction (about 30 percent) of the certified workers in my welfare target group sample do not appear to be eligible for the credit in the quarter in which they were certified, based on my assessment of eligibility via their CARES records. There are several possible explanations. First, I am only able to observe cash welfare receipt for each worker, and cannot observe their other possible means of participating in W-2 (for instance, child care benefits) that also count as months on welfare for the purposes of WOTC/WtW eligibility. Second, the Department of Workforce Development has a policy of verifying the accuracy of documentation for 10 percent of applications, which means that some ineligible workers may be certified if their applications are not among those audited. Finally, there may be a few workers who qualify for the WtW based on receipt prior to 1998 (when data become available to me) due to a special provision in the WtW target group definition. Since my goal here is to estimate the effects of certification, I retain the observations who do not appear eligible but were certified under the welfare target group.

[48]As a check on the appropriateness of this method, I compared the distribution of the job-start quarters of the relevant jobs (between July 1999 and December 2000) for the treatment and comparison groups. I found that the treatment and comparison groups had very similar distributions of job-start dates, suggesting that the randomization chose "comparable" jobs by at least one measure.

[49]I estimate this using the current sample to count the number of WOTC/WtW-eligible welfare workers the firm has hired over the time period covered by my data. I assign industries based on the first two digits of the

indicator for whether the offices from which a firm's payroll is processed are located in Wisconsin, since those outside the state would need to submit their forms to the appropriate Wisconsin state agency separately from any submitted to ther own state. I include the county unemployment rate to control for any effects it may have on wage offers being made by firms. On the worker side, I include an indicator for whether the worker is one of the parents on the welfare record (vs. a working teenager or older child living at home). I expect that this would positively influence participation, since this person would have more accurate information about past and current family welfare receipt. Other worker demographic variables are included for their possible effects on wages. Finally, I include indicators for region and quarter, as I did in the employment logit, along with higher-order terms and interactions between some of the variables to improve the results of the balancing test for the specification.[50] The results are shown in Table 4.

The logit results reveal most of the expected effects. Two industries, the "Retail Trade" and "Transportation and Warehousing" industries, are statistically significantly more likely to certify eligible workers than the "Accommodation and Food Service" industry (the omitted category). In contrast, industries like "Admin/Support/Waste Mgmt/ Remediation Services," "Health Care and Social Assistance," and "Professional, Scientific, or Technical Services" appear less likely to certify workers. This confirms that the WOTC is more likely to be claimed in industries that depend on a large low-skilled work force. The effects of the other industries relative to the omitted category are not precisely estimated.

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employers NAICS industry code, and I combine some categories due to small certified samples in some industries. The final list of industries included is provided in Table 4.

[50]The added higher-order terms and interactions are noted in Table 4. There are 36 variables in the logit, and these variables balance quite well when I restrict the sample a bit to eliminate the observations for which the comparison group observations are in the right tail of their distribution (only some of the industry codes fail the regression balancing test; the other 32 variables pass). This type of restriction is employed by Black and Smith (2004) in their reporting of the balancing test, as it can be difficult to balance the observations for which there is a low-density comparison group. If I do not restrict the sample, I have 5 variables that are significant at the 1 percent level, one at the 5 percent level, and 3 at the 10 percent level (the other 26 pass the test).

TABLE 4
Propensity Score Estimation for Selection into Certification

Variable	Odds Ratio	Std. Error	z-statistic
Industry Indicators			
Admin/Support/Waste Mgmt/ Remediation Services	0.380	0.0541	-6.8
Finance, Insurance, Information, Real Estate/Rental/Leasing	1.540	0.4220	1.57
Health Care and Social Assistance	0.758	0.1145	-1.83
Manufacturing	1.231	0.3889	0.66
Other Services (except Public Admin.) or Wholesale Trade	1.172	0.3700	0.5
Professional, Scientific, or Technical Services	0.076	0.0781	-2.51
Retail Trade	1.379	0.2066	2.14
Transportation and Warehousing	2.013	0.5691	2.47
Other Variables (firm/geographic)			
Firm headquarters in WI (empWI)	0.107	0.0289	-8.24
No. of WOTC -eligible workers hired by firm (#workers)	1.414	0.0433	11.31
empWI * #workers	1.109	0.0231	4.99
empWI * #workers^2	0.998	0.0003	-5.31
No. of workers^2	0.986	0.0013	0.99
No. of workers^3	1.0002	0.00002	9.5
No. of workers^4	0.9999	1.1E-07	-8.86
County unemployment rate	0.704	0.1299	-1.9
Other Variables (individual)			
Age	1.004	0.0463	0.08
Age squared	0.9998	0.0007	-0.29
Female	1.295	0.2488	1.35
High school diploma	1.129	0.1120	1.22
Some college	0.795	0.1702	-1.07
College degree	2.616	2.9777	0.85
Black	1.228	0.2088	1.21
Other race	1.198	0.2253	0.96
Parent on welfare record	2.407	0.4609	4.59

Notes: Sample size: 6,413 ($N_{treatment}$ = 761, $N_{comparison}$ = 5,652) .

Pseudo R^2 = .28.

When workers do not have a reported region in quarter t0, I impute the county using their own records for up to a year before or after. Logical imputations are done for some missing education data (details available upon request). Remaining missing values of covariates reduce the sample size from 7,201 to 6,520; the loss of 107 comparison group members (described below) brings the sample to its final size of 6,413.

These estimates use the whole available sample; the difference-in-difference estimates are slightly different because they use a slightly smaller sample due to the more rigorous data requirements.

Quarterly indicators, regional indicators, interactions between region and empWI, and a constant term are also included in the estimation. The interaction of the “employer’s payroll office in WI” and “region” resulted in one subgroup (107 comparison observations) being dropped from the sample, since there is no WOTC participation among employers in the sample that are in region 4 and have WI-based payroll offices. In addition, I include a dummy for “large firm” (firms with more than 100 eligible workers). This is included because the data yield an unusual pattern in which the number of workers claimed increases in the number eligible EXCEPT in the three firms with the largest number of eligible employees, which hardly participate in the program (just one firm has one certification). The dummy variable allows the pattern to differ for these firms, since even the quartic firm size variable could not capture these outliers very well. The coefficient on the dummy is large and highly statistically significant, and it allows the logit to perfectly predict the nonparticipation of workers in the two nonparticipating firms. These workers (all in the comparison group by construction) are thus dropped from the matching estimation. All results are essentially the same if I drop these firms prior to the logit.

Omitted indicator: Industry = Accommodation and Food Services; Race = White; Education = less than High School.

Firms with more qualified workers tend to engage in more certification.[51] Firms with headquarters in Wisconsin are, however, less likely to participate than firms with headquarters out of state. This may be a function of total firm size, since many of the largest firms likely have out-of-state headquarters and large firms are more likely to participate in the WOTC/WtW than smaller firms (U.S. GAO, 2002). At the individual level, parents are more likely to be certified than qualified children, as expected.

One complication in testing my wage prediction is that UI records report quarterly earnings but not hours worked. This means no direct measure of wages is available. Instead, I use average earnings per quarter to approximate a wage measure.[52] I examine the short-run effects of WOTC/WtW certification on wages by examining average earnings per quarter at the WOTC-certified (or potentially certified) job only. I then assess long-run effects in two ways: first, I consider average quarterly earnings across *all* jobs over *all* quarters in the year following the starting quarter of the WOTC job (including zeros for quarters with no earnings). Second, I consider average quarterly earnings across *all* jobs over all *employed* quarters in the year following the starting quarter of the WOTC job. The first measure of "wages" is more closely linked to the theory, while the second and third allow me to use the methodological advantage of differencing out past earnings outcomes. I report the results in Table 5.

My first point estimate suggests that quarterly earnings rise by about $121 in response to certification for the WOTC/WtW. Quarterly earnings at the relevant job averaged about $1,150 per quarter, making this a meaningful, though not enormous, improvement (10 percent). This result supports the prediction of higher short-term wages for the subsidized. The estimate is statistically significant at conventional levels. Taking the estimate at face value, I can provide preliminary evidence on the

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[51]This conclusion excludes the three firms with the largest number of eligible workers, as described in the table notes.

[52]To interpret this as a wage effect, it is necessary to assume that the subsidy program does not meaningfully affect hours of work. My model does not make any predictions on hours of work, as employment is treated as a binary decision (work or not work).

TABLE 5
The Earnings Effects of WOTC/WtW Participation

	Earnings/Quarter while at *Relevant Job* That Started in t0	Avgerage Earnings per Quarter *in All Jobs* for the Year after t0	Avgerage Earnings per *Employed* Quarter *in All Jobs* in the Year after t0
Estimated earnings effect of WOTC/WtW certification	$ 120.90** (53.91)	$ 38.60 (70.35)	$ - 75.03 (85.19)

Notes: t0 is the quarter in which a WOTC/WtW-certified job begins (treatment group) or in which a job begins for which a worker could have been certified but was not (comparison group).

Matching is done on log(P(X)/(1-P(X)) so that estimates are robust to choice-based sampling. I use cross-validated bandwidths for each estimate (BW = 0.6, 2.8, and 7.5, respectively). I also trim away the 2 percent of treated observations for which the comparison group has the lowest kernel density, as described in Todd (1999).

Standard errors are estimated via 200 bootstrap replications.

If I use regression-adjusted matching (as described in Heckman, Ichimura, Smith, and Todd, 1998) using the variables age, age squared, gender, race, education, region indicators, industry indicators, and quarter indicators, the results are similar.

The second column uses a slightly smaller sample due to the difference-in-differences data needs. The third column uses a sample of people who have employment in at least one quarter in the year prior to certification and in at least one quarter in the year after certification.

Sample sizes: Column 1 – $N_{treatment}$ = 745, $N_{comparison}$ = 5,308
Column 2 – $N_{treatment}$ = 714, $N_{comparison}$ = 4,920
Column 3 – $N_{treatment}$ = 523, $N_{comparison}$ = 3,011.

incidence of the subsidy by comparing the average gains to a subsidized worker to the size of the tax credit received by her employer. I first calculate the average gains per worker over the course of the subsidized job, based on the average certified job length in the sample of 3.0 quarters; this is $363. I then compare this to an estimate of the average tax credit for which these workers' employers qualified; this is about \$855 per worker.[53] This implies that about 40 percent of the tax credit is passed through to workers in the form of a wage premium.

The panel estimates, which look more broadly at labor market earnings before and after a worker is exposed to the WOTC, are very small and have very large standard errors. This provides the interesting insight that if the WOTC does improve wages, the increase in wages comes only through the WOTC job itself and not through changes in the worker's broader work experience following the subsidized job.

C. Result 3: Job-Tenure Effects

My model also suggests potentially interesting effects of the WOTC on job tenure, although it does not generate a clear sign prediction. Since I am again dealing with selection into certification, I use the same propensity score estimation as that used in the wage discussion to predict certification. I then test this proposition using the same sample as that used to study wage effects, since job tenure is also likely to be a function of the job characteristics and demographics used previously.

I create two types of estimates for the job tenure effect of the WOTC/WtW. First, I measure tenure by the total number of quarters worked at the particular job being examined (which was either WOTC-certified or could have been). This is the natural definition of job tenure, and most appropriately corresponds to the measure being addressed in the model. However, this measure does not lend itself to a panel approach, since there is no clear "past tenure" variable with which to difference. In order to consider the possible unobservable individual heterogeneity that could drive cross-sectional results (in

[53] I determine this value by taking the total estimated cost of the program and dividing it by the number of workers who were certified. To estimate the cost of the program, I first must estimate hours worked by each of the certified workers in order to apply the appropriate subsidy rate. I do this by dividing each worker's total earnings by their starting wage as reported on the WOTC application (note that I do not have wages for uncertified workers, which is why I do not use hours in most of my analysis). My estimates of hours worked suggest that about 36 percent of the certified workers worked less than 120 hours, qualifying for no subsidy; only about 37 percent qualify for the 40 percent subsidy. For purposes of this simple calculation, I assume that employers claim the WOTC and not the WtW; if some claim the WtW, the subsidy level will be 35 percent instead of 40 percent but can apply to up to \$10,000 in wages for up to two years instead of \$6,000 in the first year only.

which I can only control for observables), I also provide a differenced estimate of job tenure effects, where tenure is more loosely defined as the number of quarters worked *at any job* in the year following the start of the relevant job. Since this can also be measured in the year prior to the relevant job, I can examine whether subsidies cause changes in labor force "attachment." The estimates of the effect of WOTC certification on job tenure (either at the relevant job or, more generally, in the relevant year) are provided in Table 6.

TABLE 6
The Job Tenure Effects of WOTC/WtW Participation

	Quarters Employed at *Relevant Job* Starting in t0	Total Quarters Employed in A*ll Jobs* during Year after Relevant Job Start
Estimate of the effect of WOTC/ WtW certification on job tenure	-.059 (.129)	.077 (.092)

Notes: t0 is the quarter in which a WOTC/WtW-certified job begins (treatment group) or in which a job begins for which a worker could have been certified but was not (comparison group).

Matching is done on log(P(X)/(1-P(X)) so that estimates are robust to choice-based sampling. I use cross-validated bandwidths for each estimate (BW = 1.4 and 2.3, respectively). I also trim away the 2 percent of treated observations for which the comparison group has the lowest kernel density, as described in Todd (1999).

Standard errors are estimated via 200 bootstrap replications.

If I use regression-adjusted matching (as described in Heckman, Ichimura, Smith, and Todd, 1998) using the variables age, age squared, gender, race, education, region indicators, industry indicators, and quarter indicators, the results are similar.

The second column uses a slightly smaller sample due to the difference-in-differences data needs.

Sample sizes: Column 1 – $N_{treatment}$ = 745, $N_{comparison}$ = 5,308
Column 2 – $N_{treatment}$ = 714, $N_{comparison}$ = 4,920.

These estimates provide two conclusions about the effects of the WOTC/WtW on job tenure. The first estimate, based on tenure at the actual WOTC/WtW job (or WOTC/WtW qualified job for the comparison group), is near zero and is statistically insignificant. There is no evidence that subsidized workers have longer job tenure than the unsubsidized.

The second estimate, which assesses the subsidy's effects on the level of labor market attachment, suggests that there is also no evidence of an effect on the number of quarters worked each year. This estimate of .077 quarters (or about 1 additional week worked) is neither statistically nor economically significant. The combination of these two estimates suggests that workers who become certified for the WOTC do not experience meaningful gains in job tenure or labor force attachment as a result of the subsidy. In terms of the economic model and institutional features presented earlier, it does not appear that any of the potential influences on job tenure dominates; it may be that all effects are very small, or that they are larger but directly counteract one another.

VII. CONCLUSION

This study of the WOTC and WtW provides an examination of a new employer subsidy program using data and methods that overcome some of the limitations in past work. I develop a theoretical model with testable implications for the effects of wage subsidies on employment and wages, as well as insight into their effects on job tenure. I then test these implications using a unique administrative data set and a matching methodology that allows me to isolate the effects of subsidy certification. I find evidence of some short-run improvements in employment levels as a result of the WOTC, but no improvements in longer-run employment levels. When I limit my sample to those who are eligible and examine the effects of being certified, I am able to more carefully examine the effects of the WOTC/WtW on those who participate. I find no evidence of a meaningful improvement in job tenure as a result of the subsidy. However, there is evidence that WOTC/WtW-certified workers may experience short-term wage improvements. Based on my point estimate, workers on average receive a wage premium of about 10 percent at their subsidized job, which is equal to about 40 percent of the subsidy on average.

My evaluation of the WOTC/WtW provides very limited support for the effectiveness of the program, which is consistent with evaluations of past subsidy programs. Researchers found that the Targeted Jobs Tax Credit often failed to be beneficial, and in some respects was actually detrimental to workers.[54] I find no negative effects of the WOTC/WtW programs, but I also find that it has no (measurable) positive effects on long-term employment or job tenure, and generates limited wage improvements. Workers appear to experience a wage premium at the job in which they are subsidized, but aside from this particular job they do not appear to make substantial wage improvements. Similarly, workers do not respond to the subsidy program by becoming more active participants in the broader work force (as measured by number of quarters employed). These results are consistent with the limited incentives that can be provided by narrowly targeted programs that provide temporary, job-specific subsidies to individual employers.

Even if one concludes that the apparent wage gains from the WOTC/WtW are encouraging, this study suggests that low participation rates among eligible workers limit the extent of the benefits of the program. While my estimation of the probability of certification provides some evidence on factors that affect firms' decisions to apply for the WOTC/WtW, much work remains to be done before the barriers to participation are fully identified and can be addressed. As long as participation remains low, the WOTC/WtW will continue to be a small program that may have some positive effects on a particular subgroup of disadvantaged workers but fails to generate strong improvements in the low-skilled labor market as a whole.

There are some other key issues that still need to be addressed in research on the WOTC and WtW. The discontinuities in subsidy levels caused by hours requirements create incentives for firms to seek retention near the 120-hour mark (which induces a positive subsidy) and the 400-hour mark (which increases the subsidy to the 40 percent level). We could better understand employer behavior, and in

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[54]For example, Hollenbeck, Willke, and Ershadi (1986) found that eligibility for the Targeted Jobs Tax Credit improved labor market prospects only for nonwhite males and had a negative effect on other demographic groups, perhaps due to stigma. Certification for the subsidy, on the other hand, appears to have increased wages, but was also associated with higher turnover and fewer quarters of employment for the treatment groups. The combination of these results with other evidence contributed to the decision to stop reauthorizing the subsidy, which expired at the end of 1994.

particular firms' tendency to respond to these incentives (or not), if we investigated the patterns in hours worked by WOTC workers. This investigation is possible with the Wisconsin data, because I can access both starting wages and quarterly earnings (and therefore hours) for WOTC participants. In addition, these data allow me to examine the characteristics of employers choosing *not* to claim the WOTC when they have hired eligible workers. This could help disentangle different plausible explanations for low participation. Finally, the interaction of the WOTC and WtW with other trends and policies could usefully be explored. For example, about 16 percent of WOTC/WtW recipients in Wisconsin were subsidized through a job in the temporary work industry. I would like to examine whether those with temporary work arrangements experience the same subsidy effects as other WOTC/WtW participants.

Appendix A

FIRST PERIOD EQUILIBRIUM

In this discussion I address the first-period decisions of firms and workers who face uncertainty about workers' productivities. Consider a worker's value, in the first period, of accepting a job with first period wage offer w_1. The worker will receive wage w_1 in the first period, but the second period outcome is unknown. The worker may lose the job (either due to exogenous job loss or low revealed productivity) or the worker may have sufficient productivity to keep the job, at either a wage of m or the Nash wage. All of these possibilities are illustrated in the following expression for the value of a first-period wage offer w_1 for an unsubsidized worker:

$$V(w_1)=\begin{cases} w_1+\beta\Bigg\{\eta Q+(1-\eta)\Bigg[\int_{-\infty}^{m}\beta Q\,dG(\theta'|\theta+\varepsilon)+\int_{m}^{\hat{\theta}}\left(\frac{m+\eta\beta Q}{1-\beta(1-\eta)}\right)dG(\theta'|\theta+\varepsilon) \\ \qquad\qquad +\int_{\hat{\theta}}^{\infty}\left(\frac{\alpha\theta'+(1-\alpha)w^*+\eta\beta Q}{1-\beta(1-\eta)}\right)dG(\theta'|\theta+\varepsilon)\Bigg]\Bigg\} & \text{if } w_1\geq m \\ \beta Q & \text{if } w_1<m \end{cases}.$$

For a subsidized worker, the value of wage offer w_1 is:

$$V_E(w_1) = \begin{cases} w_1 + \beta\left\{\eta Q_E + (1-\eta)\left[\int_{-\infty}^{m-S} \beta Q_E dG(\theta'|\theta+\varepsilon) + \int_{m-S}^{\hat{\theta}_E}\left(\frac{m+\eta\beta Q_E}{1-\beta(1-\eta)}\right)dG(\theta'|\theta+\varepsilon)\right.\right. \\ \left.\left. \qquad + \int_{\hat{\theta}_E}^{\infty}\left(\frac{\alpha(\theta'+S)+(1-\alpha)w_E^* + \eta\beta Q_E}{1-\beta(1-\eta)}\right)dG(\theta'|\theta+\varepsilon)\right]\right\} & \text{if } w_1 \geq m \\ \beta Q_E & \text{if } w_1 < m \end{cases}.$$

In order to find the value of the first-period wage offer, given the noisy signal (θ+ε), I need to define the surplus over which the firm and worker bargain. Although neither party knows the actual surplus generated by the match, the expected value of the surplus can be calculated. When the worker and firm Nash bargain over this expected surplus, the equilibrium wage offer is:

$$w_1^N = \alpha \mathrm{E}(\theta|\theta+\varepsilon) + (1-\alpha)w^* + \beta(1-\eta)\left\{\int_m^{\hat{\theta}}\left(\frac{\alpha(\theta'-w^*)-(m-w^*)}{1-\beta(1-\eta)}\right)dG(\theta'|\theta+\varepsilon)\right\}.$$

The first two terms of the wage expression give the wage equation for an economy with uncertainty but *without* a minimum wage.[55] The last term of the wage expression above is negative.[56] It can be interpreted as a negative wage effect of the minimum wage, because it represents the possibility

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[55]Note that the normality assumptions on θ and ε allow me to solve explicitly for $E(\theta|\theta+\varepsilon)$ in terms of the value of (θ+ ε) and the means and standard deviations of the two distributions:

$$E(\theta \mid \theta+\varepsilon) = \mu + \frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_\varepsilon^2}(\theta+\varepsilon-\mu).$$

However, to keep the notation simple and to maintain the intuition of the results, I leave the expression in the form $E(\theta|\theta+\varepsilon)$ in the rest of the discussion.

[56]A short proof is available upon request.

that the firm may not get its full (Nash) share of the total surplus for certain workers. Essentially, the risk of this occurrence is priced out in the first period. This result is a consequence of my combined implementation of a minimum wage, bargaining, and uncertainty, and is not found in either the Jovanovic (1979) or Flinn (2003) models.

A similar wages structure applies to the subsidy-eligible in the first period:

$$w_1^{\mathrm{E}} = \alpha(\mathrm{E}(\theta|\theta+\varepsilon)+\mathrm{S}) + (1-\alpha)w_E^* + \beta(1-\eta)\left\{\int_{m-S}^{\hat{\theta}_E}\left(\frac{\alpha((\theta'+S)-w_E^*)-(m-w_E^*)}{1-\beta(1-\eta)}\right)dG(\theta'|\theta+\varepsilon)\right\} \quad .$$

As with the unsubsidized group, the last term in this wage expression is negative.[57] The size of this minimum-wage-induced wage reduction compared to the corresponding wage reduction of the unsubsidized group depends upon the mean of the distribution of *θ*.

[57]The proof has the same form as the proof of the parallel result for the unsubsidized group and is available upon request.

THRESHOLD EXPRESSIONS (USED IN FIGURE 2):

Expressions for wage thresholds of the unsubsidized group:

$$\mathrm{E}(\theta| y^{**}) = (1-\beta)(1-\eta)\beta\mathrm{Q} - \beta(1-\eta)\left\{\int_m^\infty \frac{\theta' - (1-\beta)(1-\eta)\beta Q}{1-\beta(1-\eta)} dG(\theta' | y^{**})\right\}.$$

$$\mathrm{E}(\theta|y^*) = \frac{\mathrm{m}\text{-}(1\text{-}\alpha)(1\text{-}\beta)(1\text{-}\eta)\beta\mathrm{Q}}{\alpha} - \frac{\beta(1-\eta)}{\alpha}\left\{\int_m^{\hat{\theta}} \frac{\alpha(\theta' - (1-\beta)(1-\eta)\beta Q) - (m\text{-}(1\text{-}\eta)(1\text{-}\beta)\beta\mathrm{Q}}{1-\beta(1-\eta)} dG(\theta' | y^*)\right\}.$$

Similar equations for the thresholds for the subsidy-eligible group:

$$\mathrm{E}(\theta| y_{\mathrm{E}}^{**}) = (1-\beta)(1-\eta)\beta\mathrm{Q}_{\mathrm{E}} - \beta(1-\eta)\left\{\int_{m-S}^\infty \frac{(\theta' + S) - (1-\beta)(1-\eta)\beta Q_E}{1-\beta(1-\eta)} dG(\theta' | y_E^{**})\right\} - S.$$

$$\mathrm{E}(\theta|y_{\mathrm{E}}^*) = \frac{m\text{-}(1\text{-}\alpha)(1\text{-}\beta)(1\text{-}\eta)\beta\mathrm{Q}_{\mathrm{E}}}{\alpha} - \frac{\beta(1-\eta)}{\alpha}\left\{\int_{m-S}^{\hat{\theta}_E} \frac{\alpha((\theta' + S) - (1-\beta)(1-\eta)\beta Q_E) - (m\text{-}(1\text{-}\eta)(1\text{-}\beta)\beta\mathrm{Q}_{\mathrm{E}}}{1-\beta(1-\eta)} dG(\theta' | y_E^*)\right\} - S.$$

PROOF OF RESULT 1:

Result: $y^{**} > y_E^{**}$, i.e., the standard for employment in the first period is higher for the subsidized group than the unsubsidized group.

Strategy: I consider a generic subsidy-ineligible (N) worker and demonstrate that if this worker is hired, an E worker with the same signal would also be hired. I then use the strict inequality in their expected match surplus to argue that there are signals for which the E worker would be hired while the N would not, generating the strict inequality in my result.

Proof: Consider a subsidy-ineligible (N) worker and a subsidy-eligible (E) worker who draw the same signal y. Suppose the N worker is hired (i.e., $y > y^{**}$). This N worker is more likely to be fired in the second period (thus providing zero employer surplus) than the E type worker since m > (m-S). If the

N worker does remain employed (θ>m), she will (in expectation) provide strictly less surplus than the E worker because, while both workers draw from the same match distribution, the E worker qualifies the employer for an additional subsidy S. It follows that if this N type worker is hired, the E type with the same signal will be hired as well since she generates a strictly larger expected surplus. In fact, given this strictly larger expected surplus, there must be some y below y** such that an E worker would be hired even though an N type worker would not. The result is that $y^{**} > y_E^{**}$.

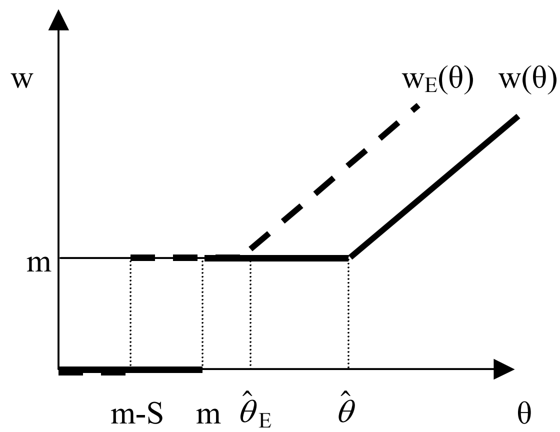
PROOF OF RESULT 2:

(Note: I provide separate proofs for the second and first periods.)

Result for 2nd Period: $E(w_2 \mid \theta > m) < E(w^E_2 \mid \theta > m-s)$, i.e. workers who are not eligible for subsidies and are employed get lower 2nd-period wages on average. This is also true for unconditional wages, i.e., $E(w_2) < E(w^E_2)$.

Strategy: This result follows from the wage distribution in the second period, which is graphed in the text.

Proof: See the graph below that was developed based on 2nd period results:



Since the E wage distribution dominates (sometimes strictly) at every θ, it is clear that $E(w_2 \mid \theta > m) < E(w^E_2 \mid \theta > m\text{-}s)$. This also holds unconditionally, since E types have a higher employment rate.

Result for 1st Period: $E(w_1 \mid y > y^{**}) < E(w^E_1 \mid y > y_E^{**})$, i.e., workers who are not eligible for subsidies and are employed get lower wages on average. This is also true for unconditional wages, i.e., $E(w_1) < E(w^E_1)$.

Strategy: I use a proof by contradiction using the basic structure of the model and focusing on the second period outcomes resulting from hiring a worker.

Proof: Suppose both a subsidy-eligible (E) worker and a subsidy-ineligible (N) worker have the same first-period signal $y < y_E^*$. The E type will certainly be paid no more than the minimum wage m (by definition of y_E^*) if she is hired at all. Suppose the N type is paid more than m. We know that the N type is more likely to be fired in the second period (thus providing zero employer surplus) than the E type worker since m > (m-S). If the N worker does remain employed (θ>m), she will provide strictly less surplus than an E worker of the same ability because the E worker qualifies the employer for a subsidy S. Since both types of workers have the same bargaining power α, higher levels of surplus for E types will be reflected in higher wages, except in cases where both groups are paid the minimum wage. It follows, then, that when an N type worker is paid more than m, an E type of the same expected ability should be paid a wage strictly greater than that paid to the N type worker. This is a contradiction, since the E type is paid no more than m when $y < y_E*$. It follows that the N type worker will *not* be paid more than m when $y < y_E*$. In fact, given the strictly larger expected surplus derived from a subsidized worker, there must be at least some y above y_E* such that an N type worker would still receive a wage of only m even when the E type has a higher wage. The result is that $E(w_1 \mid y > y*) < E(w^E_1 \mid y > y_E*)$. This means that average wages *among those receiving a wage above the minimum* are lower for the unsubsidized group.

Note that those who are employed but receive a wage exactly equal to m have an average wage of m regardless of their type. Formally, $E(w_1 \mid y** < y < y*) = m$ and $E(w^E_1 \mid y_E** < y < y_E*) = m$. This is lower than both $E(w_1 \mid y > y*)$ and $E(w^E_1 \mid y > y_E*)$.

It follows that $E(w_1 \mid y > y**) < E(w^E_1 \mid y > y_E**)$, i.e., average first-period wages among those employed are lower for the unsubsidized group. Since we know that $y**>y_E**$, we can also see that there is more unemployment among the unsubsidized, so the unconditional statement $E(w_1) < E(w^E_1)$ is also true.

EXPECTED JOB TENURE:

The following provide expected probabilities of being fired, given that one is hired initially. I first show the unsubsidized case, followed by the subsidized.

For unsubsidized: $E_y[\Pr(\theta<m \mid y) \mid y > y**]$

Rewrite this as: $\int_{y=y**}^{\infty} \Pr(\theta < m \mid y) dG(y \mid y > y**)$

Note that $\theta \sim N(\mu,\sigma_\theta^2)$ and $y = \theta + \varepsilon$ is just a noisy signal of θ, where the noise is N(0,1).

This means $y \sim N(\mu,\sigma_\theta^2 + 1)$

The conditional distribution of θ given y is:

$$\theta \mid y \sim N(\mu + \frac{\sigma_\theta^2}{\sigma_\theta^2 + 1}(y - \mu), \frac{\sigma_\theta^2}{\sigma_\theta^2 + 1}).$$

Call the mean here f(y) and call the standard deviation σ_1^2. Rewrite the probability:

$$\Pr(\theta < \mathrm{m}|\theta + \varepsilon) \;=\; \Pr\left[\frac{\theta - \mathrm{f}(\theta+\varepsilon)}{\sigma_1^2} < \frac{m - \mathrm{f}(\theta+\varepsilon)}{\sigma_1^2} \mid \; \theta + \varepsilon\right] = \Phi\left[\frac{m - \mathrm{f}(\theta+\varepsilon)}{\sigma_1^2}\right].$$

Note also that the $dG(\, y \mid y > y^{**})$ is a truncated normal, with pdf:

$$dG = \begin{cases} \dfrac{h(y)}{1 - H(y^{**})} & \textit{if } y \; > \; y^{**} \\ 0 & \textit{otherwise} \end{cases}$$

where H is a normal pdf with mean μ and standard deviation σ_θ^2+1. Standardizing gives:

$$dG = \begin{cases} \dfrac{\phi\left(\dfrac{y - \mu}{\sigma_\theta^2 + 1}\right)}{1 - \Phi\left(\dfrac{y^{**} - \mu}{\sigma_\theta^2 + 1}\right)} & \textit{if } y \; > \; y^{**} \\ 0 & \textit{otherwise} \end{cases}$$

So the expression for the fraction of those hired initially who we expect to be laid off is:

$$\int_{y=y^{**}}^{\infty} \Phi\left[\frac{m-[\mu+\sigma_1^2(y-\mu)]}{\sigma_1^2}\right]*\frac{\phi\left(\frac{y-\mu}{\sigma_\theta^2+1}\right)}{1-\Phi\left(\frac{y^{**}-\mu}{\sigma_\theta^2+1}\right)}dy.$$

Among the subsidized, the fraction is:

$$\int_{y=y_E^{**}}^{\infty} \Phi\left[\frac{(m-S)-[\mu+\sigma_1^2(y-\mu)]}{\sigma_1^2}\right]*\frac{\phi\left(\frac{y-\mu}{\sigma_\theta^2+1}\right)}{1-\Phi\left(\frac{y_E^{**}-\mu}{\sigma_\theta^2+1}\right)}dy.$$

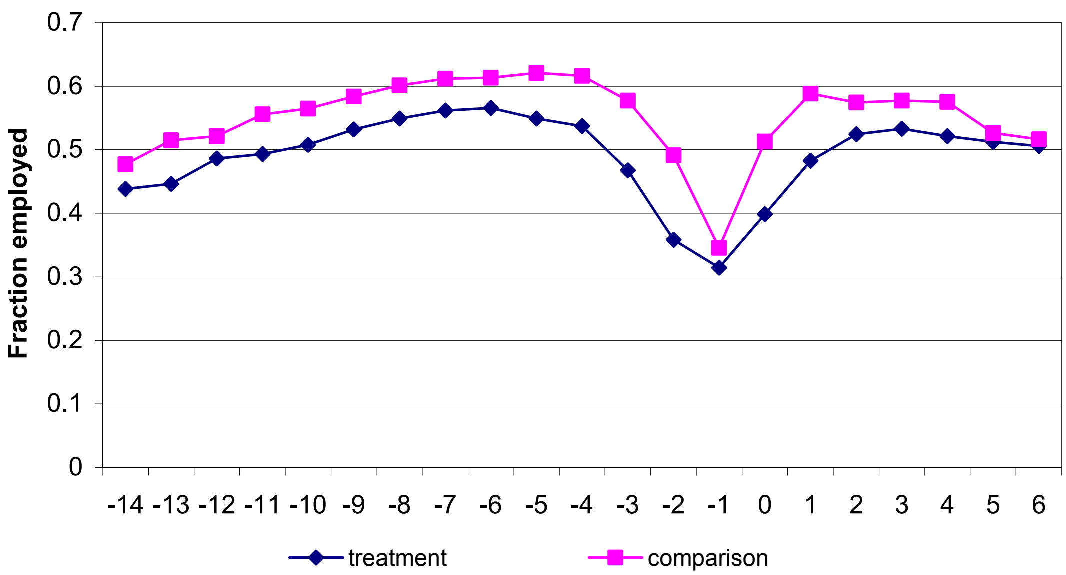
These two expressions cannot be compared directly analytically, since the limits of integration are defined only implicitly. While the integrand for the subsidized workers is smaller, the range of the integration is larger because $y^{**} > y_E^{**}$.

Appendix B

The identifying assumption behind difference-in-differences estimation is that any systematic differences between the groups being compared are constant over time. I want to see if the data prior to the period of treatment appear consistent with this assumption for the WOTC-eligible and nearly-eligible groups I use in my analysis of employment effects of the WOTC.

The following graph displays the pattern of quarterly employment rates for my sample starting 14 quarters before the quarter of "treatment" (t0). This allows me to see whether any differences between the groups appear to be constant before treatment. In the graph below I use the restricted sample (which excludes those with more than 24 months of welfare receipt in the sample period). The parallel graph for the unrestricted sample is similar, but a bit noisier.

Table B1
Treatment and Comparison Employment Rates (quarterly, relative to t0)



Note: This graph reports the employment rate in the treatment and comparison groups in each quarter for 3.5 years before treatment and for 1.5 years after treatment. Those who were ages 16 and under in a given quarter were dropped from the employment rate estimate that quarter.

The graph suggests a fairly constant difference in employment rates until near the time of treatment. For instance, from the earliest quarter until (and including) the fourth quarter prior to treatment, the average gap between the rates is 5.6 percentage points, with a range of quarterly gaps from 3.8 to 7.9. Just prior to treatment, the gap increases, suggesting that periods just prior to the time of treatment may not be as appropriate for estimation as periods that are further from the date of treatment. This implies that my longer-term estimates are likely the more reliable of the estimates of employment effects. The large fall in employment rates just before t0 (in both groups) is unsurprising, since this is a period during which both groups are on welfare.

Technically, my difference-in-differences assumption is made conditional on propensity scores, not unconditionally as described in the above graph. A set of graphs similar to the one above for various propensity score ranges is available upon request; the graphs are qualitatively similar to the one above, though the data are noisier because they are more sparse.

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