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Temporary Help Service Firms' Use of Employer Tax Credits: Implications for Disadvantaged Workers' Labor Market Outcomes

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Abstract

Temporary help services (THS) firms are increasing their hiring of disadvantaged individuals and claiming more subsidies for doing so. Do these subsidies—the Work Opportunity Tax Credit (WOTC) and Welfare-to-Work Tax Credit (WtW)—create incentives that improve employment outcomes for THS workers? We examine the distinct effects of THS employment and WOTC/WtW subsidies using administrative and survey data. Results indicate that WOTC/WtW-certified THS workers have higher earnings than WOTC-eligible but uncertified THS workers. However, these workers have shorter job tenure and lower earnings than WOTC/WtW-certified workers in non-THS industries. Panel estimates suggest that these effects do not persist over time.

***JEL codes:* H2, I3, J3, J4**

Temporary Help Service Firms' Use of Employer Tax Credits: Implications for Disadvantaged Workers' Labor Market Outcomes

INTRODUCTION

In recent years, important labor market changes have influenced the employment experiences of low-skilled and disadvantaged workers. One strong trend has been an increase in these workers' participation in the temporary help services employment sector. As temporary help service (THS) employment has increased, THS firms have become increasingly prominent labor market intermediaries for disadvantaged groups (Blank 1998; Lane et al. 2003).¹ Furthermore, as the industry more than doubled in size between 1992 and 2000 (Pressler 2002), workers with recent participation in public welfare programs came to make up a disproportionate share of THS employees, a share that has continued to increase over time (Autor and Houseman 2005; Barker and Christensen 1998; Heinrich, Mueser, and Troske 2005).

Another important trend has been an increase in employers' participation in hiring subsidy programs. The Work Opportunity Tax Credit (WOTC) and Welfare-to-Work Tax Credit (WtW) reimburse employers (in the form of federal income tax credits) for up to 50 percent of the wages of workers with certain employment barriers, with the objective of helping low-income and disadvantaged individuals gain labor market experience, improve their work skills, and keep their jobs longer.² In fact, a U.S. Department of Labor (USDOL) publication used to market the program attempts to attract employers with the bold headline: "Hiring Welfare Recipients Can Reduce Employer Taxes by \$8,500 Per New Hire" (<http://www.doleta.gov/wtw/>

¹ Temporary help services employment increased at a pace five times faster than overall employment between 1979 and 1995 (Segal 1996).

² The U.S. Department of Labor Web site describes the WOTC as "one tool in a diverse toolbox of flexible strategies designed to help move people from welfare to work and gain on-the-job experience" (USDOL 2007). Both the WOTC and WtW have been reauthorized approximately every two years; the most recent law expired at the end of 2005 but was retroactively reauthorized in December 2006.

resources/fact-taxcredits.pdf). Unsurprisingly, certifications of newly hired employees have increased since their inception in 1996 (WOTC) and 1997 (WtW), totaling more than 500,000 in fiscal year (FY) 2001.

Little attention has been paid to the fact that an increasing fraction of workers being certified for the WOTC or WtW are actually being certified through work at THS firms. In contrast to the legislative intent to subsidize long-term positions, most THS firms are qualified to claim the WOTC and WtW subsidies despite their role as labor market intermediaries rather than end-user employers. We investigate how worker outcomes in subsidy-certified THS jobs are affected by both the subsidies and the temporary nature of employment. An empirical investigation is particularly important in this case, since economic theory does not offer any clear prescriptions about whether THS employment should help these workers; THS firms drive a wedge between the worker and his or her employer and have limited legal obligations to their workers (such as protection from unjust dismissal) relative to traditional employers. More specifically, we focus on two key questions. First, do THS firms' claims of these subsidies affect the labor market outcomes of their workers? Second, among disadvantaged workers in subsidized firms, how do the labor market outcomes of THS workers compare to those of workers in traditional jobs? The answers to these questions may help policymakers determine whether continuing to subsidize temporary jobs is an effective strategy for improving employment outcomes of the disadvantaged.

There are several interesting parallels between the WOTC/WtW programs and the emergence of THS firms as a growing source of employment for public assistance recipients. For example, proponents of both would argue that they make employers more willing to hire workers that they might not otherwise consider by reducing costs. In this way, both subsidies

and temporary work may allow workers to get a foothold in the labor market, an effect that may be magnified when THS firms themselves are subsidized. However, critics have suggested that both employer subsidy programs and THS firms could contribute to worker “churning,” i.e., the dismissal of workers whose subsidies run out or the perpetuation of unstable, short-term work assignments. Thus far, there is no evidence of churning as a result of the WOTC and WtW programs (U.S. General Accounting Office 2001).³ And although a number of studies of temporary workers have shown that they are more likely than other workers to change employers, to work fewer and more variable hours, and to have less attachment to the labor force, findings on the longer-term implications of temporary employment for disadvantaged workers’ labor market outcomes are more mixed.⁴ In light of the disparate findings, concerns persist that these workers may not be obtaining jobs that promote labor market attachment and retention, while THS firms are claiming federal tax credits designed for that purpose.

This is the first study, to our knowledge, to examine the distinct effects of THS employment and WOTC/WtW participation on the labor market outcomes of WOTC/WtW-certified THS workers.⁵ We use uniquely available administrative data on workers affected by the WOTC/WtW and THS employment to investigate these issues, along with a new survey of THS firms’ WOTC/WtW awareness and utilization. Because national statistics do not distinguish WOTC/WtW approvals (or “certifications”) by job type, we compiled administrative microdata available to us from Wisconsin for our empirical analysis.⁶ We found that among employers in Wisconsin, three of the six firms with the largest number of applications were THS

³ Such effects are difficult to examine because very few workers remain employed long enough to reach the subsidy cap or time limit.

⁴ See, for example, Autor and Houseman (2005); Blank (1998); Heinrich, Mueser, and Troske (2005); Houseman and Polivka (1999); Nollen (1996); Pavetti and Acs (2001); and Segal and Sullivan (1997).

⁵ This is known as the effect of “treatment on the treated.” In this study, we separately estimate the effects of THS and WOTC/WtW treatment on workers who experience both treatments.

⁶ There is a small sector of the temporary industry that is nonprofit (Seavey 1998) and as such does not qualify for the WOTC/WtW, but this is the exception in the industry.

employers. WOTC/WtW applications (and certifications) from THS firms have increased steadily over time, expanding from fewer than 16 percent of all applications in 1999 to 26 percent in 2002.⁷

We approach the empirical estimation by comparing labor market outcomes for a sample of WOTC/WtW-certified THS workers (the “treatment group”) to two comparison groups: workers in the THS industry who are not WOTC/WtW-certified (but are WOTC-eligible), and workers who are WOTC/WtW-certified but do not work in the THS industry. These groups are assembled using three sources of administrative data on the population of public assistance recipients in Wisconsin over several years. Our analysis of treatment effects uses econometric matching estimation to isolate the effects of WOTC/WtW and THS employment on labor market outcomes by controlling for observable factors and also addressing individuals’ selection into THS or subsidized employment. We first examine the effects of the subsidy programs on workers in the THS sector. We hypothesize that if, in fact, the WOTC/WtW programs are working as intended, earnings and labor market attachment will be greater among those THS workers for whom tax credits are claimed than for those whose employers are not participating, all else equal. Second, we examine the effects of THS employment versus traditional employment among WOTC/WtW-certified workers. We hypothesize that earnings will be lower and/or tenure will be shorter for workers in THS jobs. However, we describe how this relationship may be different in the context of subsidized employment, and thus, this investigation represents an important new contribution to both the temporary employment and tax credit literatures.

We find large differences in labor earnings and job tenure across the three groups, some of which reflect significant (and previously undocumented) differences in worker and employer

⁷ Based on WI WOTC/WtW administrative data from 1999–2002.

characteristics. Among THS workers, those who are WOTC/WtW-certified have quite similar job tenure to eligible unsubsidized workers, but the WOTC/WtW-certified workers have much higher earnings per quarter employed; this holds even when we control for observable characteristics. This suggests that some of the WOTC/WtW funds pass through to the THS worker in the form of increased earnings per quarter, just as they do (though to a far lesser extent) for workers in other industries (Hamersma 2006a). Using panel estimation to examine longer-term outcomes, however, the earnings difference between these workers is no longer statistically significant. Including all WOTC/WtW-certified workers, we find that THS workers have much lower total earnings than non-THS workers due to much shorter average job tenure, and again, this relationship holds when we control for observable characteristics. Fortunately for these workers, panel estimates show no evidence of continued lower earnings over time.

In light of the limited information available to us to explain firms' selection into the WOTC/WtW, we added a second component to our study—a telephone survey of Wisconsin THS firms—to further investigate their awareness and use of the WOTC and WtW tax credits. Among the 101 firms that completed surveys, more than 70 percent were aware of the tax credits, and of these, 60 percent reported that they currently claim the tax credits. Contrary to legislative intent, however, only one firm reported that prospective employees' eligibility for the tax credits might affect their hiring decisions. Thus, our findings suggest that concerns about the use of WOTC/WtW funds to subsidize THS employment are likely warranted, since subsidies do not appear to improve the job outcomes of disadvantaged workers in these jobs nor encourage the hiring of additional disadvantaged workers as intended.

THE CHARACTERISTICS OF THE WOTC/WTW AND THS EMPLOYMENT

The WOTC and WtW subsidy programs are the most recent in a long line of wage subsidy programs designed to improve work outcomes among the disadvantaged. They are most similar to the Targeted Jobs Tax Credit (TJTC), which operated from 1978 until 1994. The TJTC allowed employers to claim subsidies for workers in several target groups, including certain welfare recipients, food stamp recipients, disabled individuals, and disadvantaged youth. The WOTC program, like the TJTC, is a one-year subsidy involving several target groups; we focus on welfare recipients and food stamp youth (ages 18–24).⁸ Under the WOTC, the tax credit is 40 percent of wages, up to a maximum credit of \$2,400, as long as the employee works for at least 400 total hours at the firm. The credit is 25 percent of wages if the employee works at least 120 but fewer than 400 hours and is zero otherwise. The WtW program applies specifically to workers with at least 18 months of welfare receipt who work 400 hours per year or more. It is administered exactly like the WOTC but involves a subsidy of 35 percent in the first year of employment (maximum credit \$3,500) and 50 percent in the second year (maximum credit \$5,000).

Employers can participate in these programs by applying for subsidy certification for any new worker they hire. Participating employers typically assess eligibility by including a short WOTC prescreening form in workers' application or new-hire materials. This pre-screening form, along with a one-page subsidy application, must be completed and signed on or before the date of hire. The employer must then submit them to the state (along with fairly minimal

⁸ Eligibility for the WOTC through welfare receipt requires that workers have been welfare recipients for at least 9 of the last 18 months. The food-stamp youth category includes workers 18–24 years old who have received food stamps for the past 6 months *or* have received food stamps for 3 of the last 5 months (if the family has since become ineligible). Other target groups are low-income ex-felons, vocational rehabilitation clients, poor veterans, high-risk youth (who live in economically distressed areas), Supplemental Security Income recipients, and high-risk youth with summer jobs (who qualify for a different subsidy level due to having shorter-term jobs). Some target group definitions were modified for the program as of January 2007, but these do not affect the time period we study here.

documentation) within 21 days of hire. Upon approval from the state, the employer is authorized to claim the WOTC or WtW tax credits on its federal tax return.⁹ Note that in the case of THS employment, the legal employer is the THS firm; it is subject to the same time limits and subsidy rates as other firms regardless of whether the worker has multiple job assignments during an employment spell with the firm.

The limited research on the WOTC and WtW suggests a low level of participation among firms that is not well understood. Hamersma (2003) estimates that fewer than one-third of eligible welfare recipients and fewer than 18 percent of food-stamp youth were claimed by their employers for the credits. The General Accounting Office reported that the employers most likely to claim the subsidies are large firms that qualify for over \$100,000 per year in WOTC/WtW tax credits (U.S. GAO 2002).

In a study of the Wisconsin welfare population, Hamersma (2006a) used matching estimation to assess the effects of the WOTC/WtW on employment, wages, and job tenure. The results indicate that employment probabilities do not measurably improve when workers become eligible for the programs.¹⁰ However, within the WOTC/WtW-eligible population, those whose employers participate have about 10 percent higher earnings per quarter than similar unsubsidized workers (though average job tenure is unaffected). These small to nonexistent effects are consistent with the previous literature on targeted employer tax credits.¹¹

Research investigating the influence of the THS industry on the employment patterns of the disadvantaged is also relatively new, reflecting the recent growth of employment in the

⁹ Additional details about WOTC/WtW administration are available in Hamersma (2003).

¹⁰ This is likely related to the low WOTC/WtW participation rate among prospective employers.

¹¹ Hollenbeck and Willke (1991), for example, evaluate the longest-running program, the Targeted Jobs Tax Credit (TJTC). They do not find statistically significant impacts of TJTC certification on employment or job retention but report positive wage impacts for most worker groups. The inspector general of the USDOL similarly concludes that the TJTC was not “an effective means of helping target group members find employment” and recommends that the program be discontinued (USDOL 1994). Katz (1998), alternatively, finds a modest positive employment effect for disadvantaged young adults; he does not examine wages or tenure.

industry. A primary concern for this study is the overrepresentation of public assistance recipients among THS employees. In recent work, Heinrich, Mueser, and Troske (2005) study the populations of welfare recipients in Missouri and North Carolina and find that the proportion with jobs in the temporary help services sector more than doubled between 1993 and 1997.¹² Among Autor and Houseman's (2005) sample of welfare-to-work clients in Michigan, 21 percent who found jobs were working for temporary help agencies. In fact, THS firms are registered providers of welfare-to-work services in a number of states.¹³

There is considerable evidence that the increasing participation of the disadvantaged in THS employment may be a cause for concern. Temporary workers are paid lower wages than permanent employees in the same positions, and they are more likely to be underemployed and to work fewer hours (Blank 1998; Houseman and Polivka 1999; Nollen 1996; Segal and Sullivan 1997). Temporary help service jobs seem to lack some of the attributes that other research suggests are critical to welfare recipients' successful transition from welfare to stable employment—secure income, training opportunities, health insurance benefits, and paid time off (Blank 1998; Cohany 1998; Jorgenson and Riemer 2000; Morris and Vekker 2001). Some have also criticized THS firm arrangements specifically because they sever the traditional employer-employee relationship between workers and client firms on whose premises they work. Studies by Miles (2000) and Autor (2003), for example, link the growth of temporary jobs to THS firms' exemption from legal protections against unjust dismissal of workers.¹⁴ Temporary help service

¹² Of those welfare recipients employed in the first quarter of 1999, over 13 percent in Missouri and over 17 percent in North Carolina were working in temporary help jobs. These numbers are substantial, since just over 2 percent of the general working population works in the THS sector.

¹³ Note that Wisconsin is not one of these states.

¹⁴ Another difference is reflected in benefit provision; for example, Cohany (1998) finds that whereas 61 percent of permanent workers have health insurance, only 7 percent of temporary workers receive this benefit.

firms can end job placements without firing workers, and they have no obligation to provide a job assignment at any time.

Of course, as we also noted earlier, there is an alternative understanding of THS employment that suggests that disadvantaged workers are more likely to take these jobs because they best match their preferences or skills. Segal and Sullivan (1997) and Carre (1992) show that a large portion of the temporary/permanent wage gap can be explained by worker characteristics and by the higher concentration of low-skilled workers in occupations mediated by THS firms (i.e., relatively low-wage administrative, clerical, and laborer jobs). Heinrich, Mueser, and Troske (2005) suggest that for these workers with less desirable characteristics, the ability to enter into a contract where the employer has no long-term obligation may facilitate their access to the labor market and possibly lead to permanent jobs if workers demonstrate high productivity. If this is, in fact, a beneficial service that THS firms provide, any additional encouragement that WOTC/WtW subsidies might give to THS firms could produce net benefits for participants and society in the form of reduced public assistance obligations.

Economic theory provides only limited help in modeling the interaction of THS employment with the WOTC subsidies and predicting the anticipated effects of each of them. For example, a typical model of subsidies predicts increased earnings (due to subsidy pass-through) and perhaps longer job tenure (in cases where the subsidy increases with time on the job).¹⁵ Both effects are likely to improve labor force attachment in general. However, in the case of THS workers these predictions are less clear. There is a wedge between the worker and his or her employer (the THS firm), both in terms of pay—since a portion of the wages paid by the client firm go to the THS firm rather than the worker—and in terms of productivity, which

¹⁵ The tenure effect is not completely clear in the case of the WOTC if less-skilled workers are more likely to get hired, since these workers may be more likely to be fired than the average worker. See Hamersma (2006a) for a search model illustrating this point.

may be hard for the THS firm to directly observe. This muddies the usual prediction of increased earnings for the worker. The tenure incentives are also more unwieldy in the context of THS work. There is substantial uncertainty as to whether workers will reach the minimum hours requirement to qualify for a subsidy, since workers may decline assignments and client firms may cancel contracts.

The typical expected effects of a THS job are likewise potentially different in the context of subsidized employment. While we would still expect the usual shorter tenure (due to the nature of the work), firms with very short average tenure may be less likely to participate in subsidy programs; this means the effect may be smaller in our sample of WOTC-participating firms. Predicting an earnings effect is difficult (even in the absence of a subsidy) because the compensating differential for a THS job may be positive or negative, depending on whether the worker suffers or benefits from the temporary arrangement. In effect, this becomes an empirical question that we address for the population of subsidized workers.

Moreover, this population of subsidized workers provides a convenient, more homogeneous sample than a typical study comparing THS and non-THS workers. The firms that participate in the WOTC tend to be large, national firms that provide support for processing the WOTC paperwork, and the workers qualified for the WOTC must have particular public assistance records, so both firms and workers are relatively homogeneous. This lessens econometric problems associated with controlling for differences among workers selecting into THS employment.

DATA

Research on the effects of federal employer tax credit programs, including the WOTC and WtW tax credits, has been limited by data access problems. The state labor agencies that are

responsible for certifying workers' eligibility do not keep records of hours or earnings for these workers, since the responsibility for auditing the amount of subsidy claimed belongs to the Internal Revenue Service.¹⁶ Thus there is no single source of data that includes both certification information and labor market outcomes.

In Wisconsin, we have the opportunity to take advantage of a unique combination of data sets that provides us with the information needed to evaluate subsidy eligibility, identify certified workers, and examine labor market outcomes over time. We combine information from three key databases: the Wisconsin Client Assistance for Re-employment and Economic Support (CARES) database with demographic and monthly public assistance receipt data, Wisconsin's WOTC/WtW program database, and matching Unemployment Insurance (UI) data on quarterly earnings and industry. Together, these merged data contain records documenting TANF and food stamp receipt, demographic characteristics, and earnings for the universe of individuals who received any Temporary Assistance for Needy Families (TANF) or food stamps during the period 1998–2001, along with WOTC/WtW information for the subset of these recipients whose employers applied for the tax credits. The WOTC/WtW database provides worker-level information about all WOTC/WtW applications submitted by firms, including workers' target groups and whether a certification was granted.¹⁷ The UI earnings data include North American Industry Classification System (NAICS) industry codes by which we can distinguish THS employers from typical firms. Because determination of WOTC/WtW eligibility via TANF receipt requires 18 months of prior data, much of our analysis is focused on workers who began a

¹⁶ Incidentally, even if company tax forms were available, the IRS requires only an aggregate tax credit claim from each employer, so the data would not identify the particular workers claimed or even the number of workers claimed by a given firm.

¹⁷ Note that some of the workers who qualified for WOTC via welfare or food stamps may also have qualified under other target groups. We assume that the wage or job duration effects of a certification (for someone eligible via welfare/food stamps) are the same regardless of which target group was listed on the WOTC application. However, we do allow selection into a THS job (among WOTC-certified workers from any target group) to be related to the target group listed.

WOTC or THS job during July 1999–December 2001.¹⁸ Unemployment Insurance data for this group extends from 1995 through the second quarter of 2004.

In addition to these administrative data, we administered a short telephone survey to Wisconsin THS firms over a period of three weeks (September 16–October 6, 2005) to collect information about their awareness and use of the WOTC and WtW tax credits. A total of 224 THS firms were randomly selected from a sample frame of approximately 300 THS firms in 36 Wisconsin cities.¹⁹ Of these 224 firms to which calls were attempted, we did not get an answer or were unable to complete a call back for 66, and 18 were disconnected/wrong numbers or the firm was no longer in business. Among the other 140 firms, 31 declined to participate and 8 referred us to an out-of-state corporate office, yielding 101 completed surveys and a 72 percent response rate for those firms with which we were able to make voice contact. The survey questions and additional information are in an appendix available from the authors upon request.

DESCRIPTIVE AND CROSS-SECTIONAL ANALYSIS

Our administrative data provide a unique opportunity to explore the characteristics of public assistance recipients by subgroup according to their WOTC/WtW certification status and their THS or non-THS employment status. This descriptive analysis, which examines worker demographics, firm characteristics, and labor market outcomes, allows us to see which groups of workers are most likely to be affected by WOTC/WtW policy and its interaction with THS employment. It also underscores the importance of controlling for these factors when we attempt to identify the effects of THS employment and WOTC/WtW certification.

¹⁸ Eligibility via welfare receipt requires welfare receipt in at least 9 of the last 18 months.

¹⁹ In some cases, multiple firms in our sample are branch offices of the same larger company. We interviewed them separately, since they may have different WOTC/WtW practices.

We present descriptive statistics in three tables. In Table 1, we focus exclusively on workers who obtained a job at a THS firm during July 1999–December 2001 and who were WOTC/WtW-eligible *or* WOTC/WtW-certified via welfare or food stamp receipt.²⁰ In Tables 2A and 2B, we present descriptive statistics for WOTC/WtW-*certified* workers only, dividing the sample according to THS status to better understand the differences between THS and non-THS workers among the WOTC/WtW-certified. Table 2A contains data on a large number of WOTC/WtW recipients (from all target groups) whose WOTC/WtW jobs began in 1999–2002, providing some information about the firm at which they were hired, their target group, and their wages and occupation. Table 2B provides demographic characteristics for the subgroup of the sample on which we focus our analysis: certified workers during July 1999–December 2001 who had welfare/food stamp records sometime during 1998–2001. Since WOTC and WtW are administered via the same process, with WtW targeting a more narrow subgroup of workers, we treat WtW as an additional WOTC target group in our analysis and will refer to the programs simply as WOTC.²¹

Beginning with Table 1, we provide separate statistics for WOTC-eligible THS workers who were certified for the WOTC and those who were not certified. We also provide some limited characteristics of the THS firm at which the worker is employed. The sample means indicate several patterns in the characteristics of WOTC-certified workers in THS firms relative to their uncertified counterparts. First, WOTC-certified workers are more often white (28.6 percent) than eligible uncertified workers (12.3 percent). They also tend to be more likely to

²⁰ We assess eligibility according to the WOTC guidelines for the welfare and food stamp target groups. Welfare recipients must have at least 9 months of receipt in the last 18 months and food stamp youth must (a) have received food stamps for the last 6 months and (b) be between the ages of 18 and 24 at the time of hire.

²¹ Any worker who qualifies for WtW also qualifies for WOTC (because welfare receipt requirements are stricter). Firms are allowed to apply for both credits but may claim only one; thus, a worker who qualified for WtW may be ultimately claimed under WOTC if it is advantageous. This will often be the case when job duration is short; see Hamersma (2006b) for details on the distribution of job duration among WOTC/WtW workers.

have finished high school; only 42.2 percent are dropouts versus 59.6 percent of uncertified workers. Milwaukee workers and those with larger numbers of children are less likely to be certified. The last two characteristics listed in Table 1 reflect differences in firm characteristics of certified and uncertified workers. WOTC-certified workers are more likely to be found in THS firms that are headquartered out of state, perhaps because firms that operate at the national or international level tend to be larger and therefore have more to gain from participating in the WOTC. However, when we measure the size of the THS firm's "disadvantaged workforce" in Wisconsin over the time period of interest, we find that WOTC-certified workers tend to come from slightly smaller firms than their uncertified counterparts.²²

Tables 2A and 2B show that there are several statistically significant differences between WOTC workers at end-user firms and WOTC workers employed in the temporary help industry, a few of which are particularly notable. First, Table 2A reveals that THS firms certify a disproportionately high percentage of low-income ex-felons among their WOTC workers (about 25 percent) relative to the average among non-THS firms (less than 7 percent). There are also other smaller differences across the two groups in the size of each target group, suggesting that the target group may be a strong predictor of the type of employment (THS or non-THS) that a worker obtains.²³

²² We estimate "disadvantaged workforce size" by counting, for each firm, the number of person-quarter UI records (in 1999:1–2004:2) for workers who received welfare or food stamps in Wisconsin at some point in 1998–2001 (our CARES sample) or for whom the employer applied for WOTC certification. It is possible that some firms that appear "small" are actually large national firms that happen to have relatively few disadvantaged workers located in Wisconsin.

²³ The extremely large difference in the ex-felon category also indicates that there may be more work to be done on understanding the particular effects of the WOTC and of THS employment on this subgroup of the population, given the growing numbers of ex-felons among the low-skilled population. The difference is even more stunning here because the low-income ex-felon target group is known as one of the hardest to successfully certify due to some issues regarding proof of "low-income" (recently addressed in the December 2006 WOTC legislation). This means that if someone qualified for both the ex-felon target group and another, the employer would be likely to claim them under the other group, so our data may understate the number of ex-felons being certified for the WOTC. Unfortunately, our available sample sizes preclude a separate analysis of ex-felons.

Second, the starting wages of THS workers are generally higher, as seen in the relatively larger proportions of THS workers in the higher wage categories; almost 40 percent of THS workers are paid \$8.00 or more per hour, while only about 17 percent of non-THS workers are in this wage range. Some of this disparity may be explained by differences in occupation, although the broad categories available in Table 2A provide limited insight into whether this may be the case (particularly because about 32 percent of THS workers and 18 percent of non-THS workers have occupations coded as simply “other”). A more likely explanation of the wage differential may be related to education level, as provided in Table 2B. THS workers who are certified for the WOTC tend to be more educated than other WOTC workers, with almost 59 percent having at least a high school degree compared to only 49 percent among non-THS workers.

Third, Table 2B indicates that THS workers are more likely to be male and tend to be slightly older and have fewer children than their non-THS counterparts. This may reflect the flexibility required by THS jobs. Although some studies have suggested that the flexibility associated with THS jobs might also be appealing to women with families (e.g., the ability to decline inconvenient assignments), other research (discussed earlier) documents the challenges mothers face in arranging child care and accommodating changing work schedules.

Since our focus in this analysis is on employment outcomes, we provide statistics on workers’ employment outcomes at the job of interest (by THS and WOTC subgroup) in Table 3. The table reveals two strong trends. First, WOTC certification has limited association with average tenure among THS workers, but it is strongly associated with much higher quarterly earnings (averaging \$2,868 among the certified compared to \$1,656 among the uncertified). Second, work at a THS firm is associated with lower tenure among WOTC workers (2.03

quarters versus 3.07 quarters), but average earnings per quarter worked are very similar for THS and non-THS WOTC workers.²⁴

Is it possible that these differences in labor outcomes are completely driven by differences in the observable characteristics of these groups? Our analysis of these data suggests that this is not the case. When we control for key characteristics using cross-sectional matching estimation, the same patterns remain.²⁵ We find that WOTC-certified THS workers have earnings of about \$255 more than their eligible but uncertified THS counterparts. We also continue to see shorter tenure among the THS than among non-THS workers within the WOTC-certified sample. The negative estimated difference in tenure is a statistically significant 1.21 quarters, which is quite large given that the mean tenure in the sample is about 3 quarters and is the main reason that quarterly earnings are also lower for this group.²⁶ These differences may reflect the effects of WOTC certification and THS employment “treatments,” but it is also possible that firms and workers select into each of these treatments based on unobserved characteristics. If these unobserved characteristics are related to earnings and labor force attachment, the estimated differences may not be direct effects of the treatment. The following section provides an econometric evaluation of the interaction of WOTC with THS employment that takes into account these selection issues.

²⁴ All differences across cells in Table 3 are statistically significant with at least 95 percent confidence.

²⁵ Matching estimation is described in more detail in the next section. The control variables used here are the same as those used for the panel regressions described in that section. The cross-sectional results referenced here are available in more detail upon request.

²⁶ For WOTC-certified workers, the data provide workers’ starting hourly wages (in one-dollar-wide categories). Assigning each worker the midpoint of their reported category, and controlling for observables using matching, we find that hourly wages are about \$1 per hour higher for THS workers. This is not enough to compensate for the shorter tenure.

PANEL MATCHING ESTIMATION OF THE EFFECTS OF WOTC/THS INTERACTION

We use a panel version of the econometric method of matching to address the two key issues of this paper: the labor market effects of the WOTC on workers in the THS sector, and the labor market effects of THS employment (relative to traditional employment) on workers who are certified for the WOTC. The matching estimator for treatment effects was introduced to the economics literature in a series of papers published in the late 1990s by economists Heckman, Ichimura, Smith, and Todd (Heckman, Ichimura, and Todd 1997, 1998; Heckman et al. 1998). This methodology, which originated in the statistics literature (see, for example, Rosenbaum and Rubin 1983), allows estimation of treatment effects in contexts where there is nonrandom selection into treatment. This is a key feature for our purposes, since workers select into THS employment and firms select into WOTC participation. Matching also generates estimates with more limited functional-form assumptions than many other estimators, since part of the estimation is nonparametric. In addition, matching 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.²⁷ And finally, Heckman et al. (1998) emphasize that matching works best when both treatment and comparison groups are from the same local labor markets, which is also a feature of our data.

The matching estimator is consistent for the effect of interest, called the treatment on the treated, which can be expressed as

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

²⁷ Typical regression analyses seldom address this issue and instead include these observations.

where Y_1 is the labor market outcome in the presence of treatment, Y_0 is the outcome in its absence, and $D = 1$ indicates treatment (WOTC participation or THS employment).²⁸ The general approach of matching is to estimate counterfactuals for each treated person so that his or her treated outcome (which is observed) can be compared to what would have happened in the absence of treatment (which we estimate using the comparison group).

Although more flexible than linear regression, matching still requires a maintained assumption about the relationship between the treated and untreated. Cross-sectional matching is valid only if, after controlling for observable characteristics X , a person's ultimate treatment status is not related to what their outcome would have been in the absence of treatment. In other words, people select into treatment based on observables only. In order to relax this assumption, we use a panel version of the matching estimator that allows a time-invariant (unobserved) difference in outcomes between the treated and untreated. These estimates use dependent variables defined as the difference between pre- and posttreatment outcomes for each individual. The following mean-independence assumption must hold for the periods t (before treatment) and t' (after treatment):

$$E(Y_{0t'} - Y_{0t} \mid D = 1, X) = E(Y_{0t'} - Y_{0t} \mid D = 0, X) \text{ for } X \in S,$$

where S is defined as the overlapping support among the treatment and comparison groups.²⁹

This assumption implies that the outcome trajectories of the treatment and comparison groups

²⁸ The intuition behind the simplest version of matching is straightforward. The method divides a sample into cells within which there is little variation in observable characteristics that may influence selection into treatment and/or outcomes. The outcomes of the treated and untreated are compared within each cell. Cells in which either all individuals are treated (or all untreated) are discarded, as they cannot provide information on the effects of treatment (Angrist and Krueger 1999). The final estimator is a weighted average of the estimated within-cell WOTC effects, where the greatest weight is put on cells with high proportions of treated individuals.

²⁹ The observable characteristics X are presumed to be drawn from a distribution with some minimum and maximum draws defining the bounds on each variable's support. These distributions are allowed to differ between the treatment and comparison groups, so that the support for a given variable in X may differ between the two groups. The region of overlapping support is the intersection of these supports, i.e., it excludes any range of X values that could only be drawn by one group and not the other.

must match, but their outcome levels are allowed to differ by an unobserved constant. For example, this assumption can hold even if the people who select into THS employment have lower wage levels in all periods due to something unobservable about them. This panel version of the matching estimator often performs better than its cross-sectional counterpart, perhaps because it allows for this unobserved heterogeneity (see, for example, Heckman et al. 1998).

The common application of matching we use, called propensity score matching, is a two-step process in which we first estimate the probability of treatment based on the conditioning variables. By generating predicted probabilities of treatment (i.e., propensity scores), we reduce the matching process to the one-dimensional problem of comparing treated and untreated workers with similar propensity scores (rather than requiring matches on all of the X variables).³⁰ Upon estimation of the propensity score, we perform the matching estimation by comparing the outcome trajectory of each treated person to a weighted average of the outcome trajectories of untreated people with similar propensity scores. To make this weighted average an appropriate counterfactual for a given treated person, the highest weight is placed on those untreated people with propensity scores most similar to the treated individual. We use nonparametric local-linear regression (similar to a kernel estimator) to calculate and apply these weights, which produces an estimated treatment effect for each treated individual without imposing functional-form assumptions on the relationship between the propensity scores and the outcomes.³¹ These individual effects are averaged to form the overall treatment-effect estimate.

³⁰ Rosenbaum and Rubin (1983) show that as long as the mean independence assumption holds for matching on X , it will also be valid for matching on the propensity score $P(X)$.

³¹ Smith and Todd (2005) provide a detailed explanation of this method, which takes each treated observation and estimates a full set of weights for the comparison group in order to construct a person-specific estimate of what their untreated outcome would have been.

The Effects of the WOTC on THS Workers

We use the sample of THS workers who were WOTC-eligible or WOTC-certified (from Table 1) to investigate the effects of the WOTC within the THS industry.³² As we discovered earlier, there are a number of worker and firm characteristics that are associated with WOTC certification. Many of these factors, such as education, are also likely to influence wages and job tenure. We include these variables in the propensity score estimation reported in Table 4.

The results of the propensity score estimation reflect some of the patterns we observed in our earlier tabulations. Workers who are older, white, or more highly educated are significantly more likely to be WOTC certified than younger, minority, or less educated workers. The effect of the number of children is negative, which is consistent with Table 1. In addition, Milwaukee residents and workers at firms headquartered in Wisconsin are less likely to become WOTC certified. Finally, higher typical earnings at the firm for all of their disadvantaged workers (in the past year) are predictive of higher earnings for the THS worker being observed.

Using these propensity scores in the matching estimation, we examine the effects of WOTC certification on the earnings and job tenure of THS workers who are WOTC-eligible. Since the panel estimation requires each worker's outcomes to be measured both before and after treatment in order to control for time-invariant unobservables, we must choose broad measures of earnings and tenure that are not specific to the THS job of interest (which, by definition, isn't observed in prior periods). We choose to examine total earnings and total quarters employed in all jobs for periods up to two years before and after the start date at the job of interest.³³ This incorporates the outcomes at the job of interest into a broader measure of earnings growth and

³² The sample used here is slightly smaller than that used in Table 1; see the Table 4 notes.

³³ There is, for example, no clear prior outcome such as "previous period's wage at a THS job" to subtract from the wage at the current THS job.

labor force attachment extending (in many cases) to time periods subsequent to the end of that particular job.

For our application of panel matching, we use local linear regression to create a counterfactual change in earnings and tenure for each WOTC-certified worker, predicted by a weighted average of workers with similar propensity scores who were WOTC-eligible but not certified. The difference between this counterfactual and the worker's actual change in outcomes is attributed to the effects of the WOTC program. These differences are averaged across workers to generate an estimate of the average effect of treatment on the treated. Table 5 contains these results, which are estimated separately for the first year and the second year after the THS job begins.

We do not find much evidence that being certified for the WOTC brings about improvements in workers' job outcomes during these two years. While the bottom panel of Table 5 shows positive point estimates of the effect of the WOTC on earnings, these are imprecisely estimated so we cannot reject the hypothesis of no effect. Similarly, we do not find evidence of any WOTC effects on labor force attachment as measured by quarters worked per year. In fact, while the sign of any effect cannot be determined, the small coefficients (0.071 and 0.141) combined with their standard errors (0.099 and 0.114, respectively) suggest that any large effect in either direction is quite unlikely.³⁴ We therefore find no support for the hypothesis that the WOTC provides a "foot in the door" for THS workers to improve future outcomes beyond the WOTC job. This is consistent with the findings in Hamersma (2006a) that overall earnings and labor force attachment do not appear to be affected by WOTC certification. The results could reflect the limited scope of the subsidy or a broader general equilibrium effect that equalizes wages over time.

³⁴ Note that the mean annual quarters worked in the sample is roughly 2.5.

The Effects of THS Employment on WOTC/WtW Workers

We use the sample of WOTC workers (from Table 2B) to investigate the effects of THS employment among WOTC recipients.³⁵ Since particular demographic characteristics and target group information proved to be associated with selection into THS employment, we include those characteristics in the propensity score estimation shown in Table 6.

Selection into THS employment among WOTC-certified workers appears to depend heavily on worker demographic characteristics. Similarly to selection into the WOTC, workers who are older, male, or more highly educated are more likely to work in THS jobs than workers who are younger, female, or less-educated. The effects of race are not precisely estimated. Target group is a strong predictor of THS employment among WOTC workers, as expected; the effect of ex-felon status is particularly large. Relative to the reference occupation (professional/technical/managerial), workers in all other occupations are more likely to be in THS jobs, particularly if they are in the service or other (nonclerical/sales) sectors. The signs on the other coefficients are generally consistent with the patterns from Table 2B.

As before, we use these propensity scores to nonparametrically estimate the effects of treatment, which in this case is THS employment. We continue to look at total earnings and quarters employed for the first year after job start and the second year after job start. Of course, many workers had additional jobs during this period that were not with THS firms. This means we are simply examining the effect of one particular THS job on a worker's future earnings and labor force attachment, wherever they may work. This corresponds to our policy interest in understanding whether THS employment might place WOTC workers on a different labor market trajectory than typical employment.

³⁵ The sample used here is slightly smaller than that used in Table 2B; a discussion of the dropped observations is included in the Table 6 notes.

Table 7 reports the panel estimates of the effects of THS employment on WOTC workers. While none of the individual estimates are statistically significant, there is an interesting pattern: in measures of both annual earnings and quarters worked, the estimate for the second year after the WOTC job begins is (statistically) significantly larger than the estimate for the first year. This may reflect “recovery” from the downward shock of the (usually) low-earnings THS job. Since the estimates cannot individually be distinguished from zero, we cannot make any inference about whether this “recovery” results in long-term improvement of labor market outcomes, whether workers merely “break even” after the low earnings at the THS job, or whether workers continue to fall short of their pre-THS job level of earnings and employment.

Check of Assumptions in Matching Estimation

The validity of the panel matching results rests on the assumption that, conditional on observable characteristics, the expected change in one’s outcome in the absence of treatment does not depend on one’s treatment status. As Lee (2006) elaborates, the primary function of the propensity score is to serve as a balancing score, and thus, the purpose of the balancing test is to confirm that the propensity score adequately performs this role. Equivalently, there should be no other variable that could be added to the conditioning set of the propensity score models that would improve our estimation, and after the application of matching, there should be no statistically significant differences between covariate means of the treatment and comparison groups. This latter point is important, as only “after-matching” tests compare differences in time-invariant covariates (that are unaffected by treatment) for the resulting matched sample.

The results of balancing tests for the estimation of selection into THS employment (Table A.1) suggest that the propensity score estimation performed very well in balancing the covariates

between treatments (those who take temporary jobs) and comparison group members.³⁶ Bias was substantially reduced for all covariates, and there were no statistically significant differences between treatment and comparison group members after matching. The balancing tests results for selection into the WOTC performed equally well (Table A.2); again, there were no statistically significant differences between treatment and comparison group members after matching. The success of the propensity scores in balancing differences between treatment and control group members in both samples implies that we should interpret our panel matching results with confidence; that is, the selection mechanisms in these cases have been appropriately modeled and any biases associated with selective differences have been effectively reduced.³⁷

FINDINGS FROM A SURVEY OF WISCONSIN THS FIRMS

Given that relatively little is understood about why some firms choose (or choose not) to claim tax credits for disadvantaged workers, and moreover, since even less is understood about whether such incentives might differ for THS firms, we decided to supplement our empirical analysis with a survey of THS firms to inquire about their awareness and use of the WOTC and WtW tax credits. Our survey of 101 THS firms in Wisconsin showed that more than 70 percent were aware that they could claim the WOTC and WtW tax credits, and 60 percent of these were currently claiming tax credits. When asked if they knew what qualifications or characteristics an

³⁶ We used the Stata *pstest* command after matching to perform the balancing tests. Although *pstest* is not useful for default local linear regression matching, we alter the kernel (to biweight), and thus, local linear regression loops over the treatment observations to calculate the smoothed outcome and takes into account which observations were used in calculating the counterfactual. (We thank Edwin Leuven for his technical assistance with this point.) The detailed results of the balancing tests are available from the authors upon request.

³⁷ Of course, the matching procedure and balancing tests do not allow us to rule out lingering *unobserved* differences between the treatment and comparison groups. A set of procedures recently developed by Altonji, Elder, and Taber (2005) facilitate a test of the extent to which unobserved variables might explain away any estimated effects (after matching), under strict assumptions on the relationship between observed and unobserved factors. This test is not particularly useful in our case, as once we apply panel matching methods to our data (which control for individual heterogeneity), we do not find any statistically significant effects of being WOTC-certified or in temporary help employment.

individual had to possess for their firm to claim the tax credits, many indicated that they used a form (e.g., with a registration or new hire packet) to screen for those who might qualify.³⁸ About one-half of the WOTC-participating THS firms offered additional information about the processes they used to claim the credits, and of these, most (24) firms reported that they submitted forms to a corporate office and/or a third-party firm to process the applications for tax credits. Only a few firms had any idea about the quantity or value of claims processed by their firm in a year. One exception was a local office of a large, nationwide THS corporation that reported claiming about 3 percent of all workers and also described a corporate contract in which a large number of workers were claimed, yielding more than \$1 million in tax credits for the corporate office.

At the same time, the respondent who had detailed information on the value of the tax credits to his firm also reported that information on *who* qualified was kept confidential. In fact, although three-quarters of the firms claiming tax credits indicated that a list of qualifying employees was kept at the firm, most respondents emphasized that it was an intentional practice that they did *not* know who qualified at the time of the hiring decision. Only one respondent suggested that an individual's eligibility might (positively) affect their hiring decision, and this person was careful to add that other information was more important. Among the 26 firms that did not claim the credits, the most common reason offered (by 15) was that they did not have any eligible workers or enough eligible workers to make the paperwork worthwhile.

In summary, a key, overarching finding from the survey component of this study is that THS firms do not appear to be using the opportunity to claim tax credits for disadvantaged workers to deliberately increase the hiring of disadvantaged workers. In general, they do not

³⁸ The characteristics or qualifications most frequently mentioned (in order of highest frequency) were: ex-felon and welfare recipient (44 percent), food stamp recipient (41 percent) and poor veteran and Supplemental Security Income recipient (23 percent).

have information about individuals' status as qualifying (or not) for the tax credits at the time of hire, and even after hiring decisions are made, information about employees who are claimed is kept confidential. As one firm related in the telephone survey: "The information is sent to our corporate office, a third party processes the forms, and the tax credits come back to us like a bonus." In effect, the firms get "bonuses" for simply putting a form in their hiring packets and sending them off to be processed. These findings are consistent with our empirical findings discussed earlier, suggesting that the WOTC does not improve the future labor market outcomes of disadvantaged workers.

CONCLUSION

The objective of this paper is to identify the distinct effects of temporary employment and employer subsidies on the labor market outcomes of workers who are certified for these tax credit programs through their work for THS firms. First, we find that simply comparing average employment outcomes of these workers to others who are either uncertified or nontemporary is not sufficient to understand the effects of the subsidy or of temporary work. This is because characteristics like gender, age, race, and education level vary substantially between these groups. We also find that employer characteristics, such as firm size, are important predictors of participation in the WOTC subsidy program, and therefore must be taken into account when assessing the effects of subsidies.

When these worker and firm characteristics are used as conditioning variables in an analysis of the effects of WOTC certification on THS workers, we conclude that WOTC certification is associated with much higher quarterly earnings at the THS job but has little effect on job tenure. We find no evidence of longer-term effects of having been WOTC-certified. When we restrict our analysis to WOTC-certified workers and examine the outcomes of THS

and non-THS workers, we find that those in THS employment have much shorter job tenure than those in traditional jobs, resulting in substantially lower earnings at the WOTC job. It is not clear whether some improvements in labor market outcomes during the two years following the start of the THS job are enough to compensate for these lower earnings.

Our findings lead to several conclusions. Based on the raw data and cross-sectional estimates related to earnings and tenure at the relevant (THS or WOTC) job, we conclude that workers in the temporary help industry have higher earnings when subsidized (relative to unsubsidized THS workers), but given these subsidies, they still experience lower total earnings than those in traditional jobs due to the shorter job tenure in the THS industry.³⁹ In our panel data analysis of labor market outcomes in *all* jobs during the two years following the relevant job start, we find no evidence of meaningful broader effects (positive or negative) of the WOTC subsidy or THS employment on labor force attachment or earnings at other jobs.⁴⁰

These findings bear some similarities to those of other recent studies, although there is still no consensus among this research regarding the patterns of outcomes over time associated with THS employment. Autor and Houseman (2005) employ a quasi-experimental research design that uses random assignment of welfare recipients to labor market intermediaries and compare welfare-to-work clients who entered temporary help service jobs with those in direct-hire placements. Although they found initial gains in both earnings and employment for those entering temporary employment, their subsequent total earnings (over a two-year period) are substantially lower than those of direct-hire workers, primarily because they worked less (or had

³⁹ Our analysis implicitly assumes that it would be possible for THS workers to hold traditional jobs; we compare THS and traditional workers rather than comparing them to the unemployed.

⁴⁰ It is possible to assess the joint effect of both WOTC certification and THS employment on a given employment outcome (relative to a worker with neither) by simply adding the coefficients together for the two estimates, though this estimate is also statistically insignificant for all outcomes. For example, the estimated coefficient on total wages at all jobs in the first year following the start of a WOTC-certified THS job would be based on the first rows in Tables 5 and 7: $\$310 + (-\$247) = \$63$. The standard error of this sum is \$595, which indicates very low precision.

shorter tenure in their THS jobs). Their point estimates, however, like ours, are not statistically significant for estimates over a longer time period. Heinrich, Mueser, and Troske (2005) find that welfare recipients entering temporary help jobs received lower earnings and had less promising prospects for movement from welfare than those who had jobs in other industries, but that these differences were small after controlling for individual characteristics and became negligible over time as these workers' earnings in subsequent years increased faster, possibly due to transitions to employment in other industries. One point of tentative agreement appears to be that there is no strong evidence of substantial differences in earnings when labor market outcomes of these workers are examined over a longer period of time.

The potentially positive effects of the WOTC on earnings combined with concerns about job tenure among THS workers suggest an interesting policy question: Would the goals of the WOTC program and the incentives it generates for employers be better aligned if the tax credits were made available to the client firm of the THS intermediary (rather than, or in addition to, the THS firm itself) upon the hire of the THS worker into a permanent position? Under the current program, a client firm would need to submit a new WOTC application if it hired a temporary worker into a permanent position, and this application would only be accepted if the worker were still a member of a WOTC target group at the time of the transition. Workers who left public assistance when they initially obtained the THS job may no longer have the recent welfare or food stamp receipt needed to qualify for the subsidy. Because of this possibility, the client firm may lack an incentive to transition a worker to permanent employment.

If we believed that THS firms were strategic in their employment of disadvantaged workers to claim these tax credits, we might also be concerned that THS firms would have an incentive to maintain workers' temporary arrangements until they had worked the minimum number of hours for the WOTC to be claimed (i.e., delaying the transition to a permanent position).⁴¹ However,

⁴¹ An additional incentive is created by the subsidy cap: if the THS firm reaches the maximum credit it could claim for a worker in a given year (when a worker's earnings reach \$6,000), it may be more likely to

our survey of Wisconsin THS firms suggests that THS employers pay little attention to *whom* they are claiming or for how long. Thus, the bigger public policy concern that our research raises is that it does not appear that the primary objective of these tax credits—to encourage employers to hire more disadvantaged workers—is being served by subsidizing temporary help services firms, which are among some of the largest claimants and beneficiaries of these tax credits.

substitute other workers for whom it could claim additional credits. However, this cap is seldom reached. In our WOTC-eligible sample of THS workers, average earnings at the THS job are only about \$1,700, and over 90 percent of workers earn less than \$6,000.

Table 1 Demographic Characteristics of WOTC-Eligible THS Workers

Sample: All WOTC-eligible and WOTC-certified workers (via welfare or food stamps) who had THS jobs starting in 7/99–12/01 and who had some welfare or food stamp receipt in 1998–2001 (sample means).

Characteristic	Full sample	WOTC-eligible but uncertified workers	WOTC-certified workers	Difference in means
Gender	<i>N</i> = 11,529	<i>N</i> = 11,052	<i>N</i> = 477	
Female	0.838	0.838	0.849	−0.015
Race	<i>N</i> = 11,529	<i>N</i> = 11,052	<i>N</i> = 477	
White	0.131	0.123	0.286	−0.163***
Black	0.588	0.594	0.444	0.150***
Hispanic	0.096	0.098	0.067	0.031***
Other race (non-white)	0.185	0.185	0.203	−0.018
Education	<i>N</i> = 11,289	<i>N</i> = 10,846	<i>N</i> = 443	
Less than high school	0.590	0.596	0.422	0.174***
High school or equivalent	0.357	0.352	0.497	−0.145***
Some college	0.052	0.051	0.079	−0.029**
College degree or higher	0.002	0.002	0.002	0.000
Children	<i>N</i> = 11,480	<i>N</i> = 11,033	<i>N</i> = 447	
Number of children < 18 in household	2.71	2.73	2.24	0.491***
Number of children < 6 in household	1.49	1.49	1.26	0.237***
Age	<i>N</i> = 11,522	<i>N</i> = 11,052	<i>N</i> = 470	
Age in quarter of job start	24.9	24.9	25.2	−0.287
Residence	<i>N</i> = 11,458	<i>N</i> = 11,005	<i>N</i> = 453	
Lives in Milwaukee	0.788	0.795	0.627	0.168***
Firm headquarters	<i>N</i> = 11,529	<i>N</i> = 11,052	<i>N</i> = 477	
Wisconsin	0.657	0.661	0.566	0.095***
Size of firm's disadvantaged workforce ^a	<i>N</i> = 11,529	<i>N</i> = 11,052	<i>N</i> = 477	
Number of person-quarters worked at firm in 2000:1–2003:2 by CARES sample	18,487	18,597	15,923	2,674***

NOTES: The set of WOTC-certified THS workers includes two groups: those who were certified under the welfare or food stamp target groups (even if we cannot confirm eligibility) OR who were certified under another group but were eligible for the welfare or food stamp group. We limit the sample to those who received welfare or food stamps sometime during 1998–2001 since we have demographic data available for this subsample. Our comparison group of WOTC-eligible but uncertified THS workers includes those who were eligible for the WOTC or WtW as members of the food stamp youth or welfare target groups. (This reflects our inability to assess eligibility for the other target groups given the available data). Demographic variables are available for this whole comparison group. Unemployment Insurance data are available for the entire sample. Our final sample contains 477 WOTC-certified THS records and 11,052 WOTC-eligible but uncertified THS records. Mean differences are labeled: * if significant at 10%, ** if significant at 5%, and *** if significant at 1%.

^a We estimate “disadvantaged workforce size” by counting, for each firm, the number of person-quarter UI records (in 1999:1–2004:2) for workers who received welfare or food stamps in Wisconsin at some point in 1998–2001 (our CARES sample) or for whom the employer applied for WOTC certification.

Table 2A Characteristics of WOTC-Certified Workers and their Jobs

Sample: All WOTC certifications in Wisconsin for jobs starting in 1999–2002 (entries are population shares).

Characteristic	Full sample	Workers employed in end-user firms	Temporary help services (THS) workers	Difference in means
Firm headquarters	<i>N</i> = 16,948	<i>N</i> = 14,254	<i>N</i> = 2,694	
Wisconsin	51.3	51.0	52.7	–1.7
Target group	<i>N</i> = 16,785	<i>N</i> = 14,135	<i>N</i> = 2,650	
Welfare (WOTC only)	9.9	10.6	6.5	4.1***
WtW (or both WOTC and WtW) ^a	24.7	24.5	25.4	–0.91
Veteran	1.6	1.5	2.3	–0.84***
Ex-felon	9.6	6.8	24.7	–17.9***
High-risk youth (EZ/EC)	13.6	13.6	13.3	–0.36
Vocational rehabilitation	9.2	9.4	7.7	1.7***
Summer youth	1.4	1.6	0.3	1.3***
SSI	8.9	9.7	4.6	5.1***
Food stamp youth (18–24)	21.2	22.3	15.3	7.0***
Starting wage	<i>N</i> = 16,695	<i>N</i> = 14,056	<i>N</i> = 2,639	
< Minimum wage	2.3	2.7	0	2.7***
\$5.15 – \$5.99	23.5	26.7	6.4	20.3***
\$6.00 – \$6.99	36.1	37.4	29.3	8.1***
\$7.00 – \$7.99	17.4	15.9	25.4	–9.5***
\$8.00 – \$8.99	12.6	10.5	23.8	–13.3***
\$9.00 and up	8.0	6.7	15.1	–8.4***
Occupational category	<i>N</i> = 16,713	<i>N</i> = 14,073	<i>N</i> = 2,640	
Clerical and sales	33.0	36.3	15.6	20.7***
Service	34.9	33.1	44.7	–11.6***
Professional/technical/managerial	9.6	11.0	2.3	8.7***
Farm/forestry/fishery	0.6	0.2	2.7	–2.5***
Machine trades	1.4	1.1	3.0	–1.9***
Other	20.5	18.4	31.7	–13.3***

NOTES: The total number of worker-jobs in the WOTC records for this time period is 20,577. Of these, 725 (about 3.5 percent) did not have any UI records in 1999–2004 (including 27 whose records had not been requested from UI), and about 2,571 did not successfully match with a UI record for the WOTC job (either there was no UI record indicating the employer listed in the WOTC record or no record in the appropriate quarter that could be identified as the WOTC job). We dropped these WOTC records, as well as those for which temporary work status was unknown (about 1.5 percent of the remaining sample). The resulting sample, used for this table, contains 17,018 observations, of which 2,695 are THS jobs. These reflect the records of 14,005 distinct individuals, some of which had more than one WOTC record during 1999–2002. Separate sample sizes are given for each variable since some records have missing data for some variables. Mean differences are labeled: * if significant at 10%, ** if significant at 5%, and *** if significant at 1%.

^a Workers who qualify for the WtW (which requires 18 months of welfare receipt) also qualify for the WOTC (which requires 9 months). Employers can apply for both simultaneously, but can claim only one.

Table 2B Demographic Characteristics of WOTC-Certified Workers

Sample: All WOTC certifications in Wisconsin for workers with WOTC jobs starting in 7/99–12/01 who had some welfare or food stamp receipt in 1998–2001 (sample means).

Characteristic	Full sample	Workers employed in end-user firms	Temporary help services (THS) workers	Difference in means
Gender	<i>N</i> = 6,374	<i>N</i> = 5,693	<i>N</i> = 681	
Female	0.803	0.816	0.695	0.121***
Race	<i>N</i> = 6,374	<i>N</i> = 5,693	<i>N</i> = 681	
White	0.296	0.296	0.305	−0.009
Black	0.453	0.456	0.432	0.024
Hispanic	0.051	0.050	0.057	−0.007
Other race (nonwhite)	0.200	0.199	0.206	0.007
Education	<i>N</i> = 5,490	<i>N</i> = 4,928	<i>N</i> = 562	
Less than high school	0.500	0.510	0.413	0.097***
High school or equivalent	0.422	0.415	0.489	−0.075***
Some college	0.074	0.072	0.093	−0.021*
College degree or higher	0.004	0.003	0.005	−0.002
Children	<i>N</i> = 5,614	<i>N</i> = 5,039	<i>N</i> = 575	
Number of children < 18 in household	2.21	2.23	1.97	0.267 ***
Number of children < 6 in household	1.23	1.24	1.09	0.156***
Age	<i>N</i> = 6,312	<i>N</i> = 5,643	<i>N</i> = 669	
Age in quarter of job start	25.4	25.2	27.0	−1.76***
Residence	<i>N</i> = 5,575	<i>N</i> = 5,002	<i>N</i> = 573	
Lives in Milwaukee	0.630	0.631	0.621	0.010

NOTES: These data are a subset of those used in Table 2A. That data set had 17,018 observations. We limit the sample to those starting a job in the period 7/99–12/01 and then keep only those workers who received welfare or food stamps sometime in 1998–2001, since we have demographic characteristics for this subset. These restrictions result in a sample size of 6,374 person-jobs, of which 681 are THS jobs. The total number of workers in the sample is 5,928. Mean differences are labeled: * if significant at 10%, ** if significant at 5%, and *** if significant at 1%.

Table 3 Employment Outcomes in WOTC and THS Employment

Sample: Jobs obtained by THS and/or WOTC workers, as used in Tables 1 and 2B.

Job characteristics	Temporary help services firm	End-user firm
WOTC-certified		
Quarters at job:	2.03 (1.51)	3.07 (3.33)
Total earnings at job:	\$2,868 (4,863)	\$5,972 (13,636)
Avg. earnings per quarter:	\$1,079 (963)	\$1,174 (1,168)
WOTC-eligible but not certified		
Quarters at job:	1.80 (1.40)	—
Total earnings at job:	\$1,656 (4,016)	—
Avg. earnings per quarter:	\$643 (822)	—

NOTES: This table contains sample means and standard deviations, by subgroup, using information from each record in Tables 1 and 2B, based on the UI data for the relevant job. (The upper left cell uses the data on THS WOTC workers from Table 2B, which is a slightly larger sample than that in Table 1.)

Table 4 Propensity Score Estimation for Selection into WOTC Certification

Sample: This sample is based on the sample of THS workers in Table 1. We drop workers with more than one WOTC certification in the period 1996-early 2003 and workers with missing (and not easily imputed) data on the independent variables or with UI matches that have job start dates inconsistent with the WOTC records. The last variable was constructed by assembling all records for the relevant firm that were in our data in the last year; all these workers' earnings were added together and divided by the number of workers to get a sense of the usual earnings of workers at this firm.

Variable	Odds ratio	Z-statistic
Age	1.120***	1.73
Age squared	0.999	-1.35
Female	1.108	0.57
High school diploma	1.448***	2.94
Some college	1.510*	1.77
College degree	1.372	0.30
Black	0.423***	-5.11
Hispanic	0.520**	-2.65
Other race (nonwhite)	0.723*	-1.77
Number of children under 6	0.765***	-4.46
Number of children 6-17	0.934	-1.41
Milwaukee resident	0.544***	-4.13
Firm headquarters in Wisconsin	0.813*	-1.73
Size of disadvantaged workforce at firm (in 000s)	0.997	-0.89
Last year's average quarterly earnings/worker at firm	1.0007***	6.97

NOTES: N = 10,962 (of which 320 are WOTC workers). Estimated via logit; pseudo $R^2 = 0.101$. * significant at 10%; ** significant at 5%; *** significant at 1%. Quarter indicators and an intercept are also included in the estimation. Omitted indicators: Education = Less than high school; Race = Caucasian.

Table 5 Effects of WOTC Certification on Wages and Tenure of THS workers

Outcome of interest	Effect of WOTC certification
Total wages (all jobs) in first year following THS job start	\$310 (425)
Total wages (all jobs) in second year following THS job start	\$333 (461)
Total quarters employed in first year following THS job start	0.071 (0.099)
Total quarters employed in second year following THS job start	0.141 (0.114)

NOTES: Standard errors are estimated via 100 bootstrap replications. We match on log-odds ratios of the propensity score to account for choice-based sampling (see Smith and Todd 2005). We use a bandwidth of 2, but point estimates with bandwidths of 0.5, 1, 3, and 4 are all quite similar. We also use 2% trimming of the treated individuals to address common support concerns, as described in Todd (1999). The measures with “year following” do not include the quarter in which the job started. Sample sizes are smaller than in Table 1 due to additional restrictions regarding missing data and multiple records. Panel estimates use a slightly smaller data set because we drop those who were under age 16 two years before the relevant job start.

Sample Size: $N_{\text{treatment}} = 314$, $N_{\text{comparison}} = 10,642$

Table 6 Propensity Score Estimation for Selection into THS Employment

Sample: All WI workers who were WOTC/WtW certified for a job starting in 7/99–12/01 and who had welfare or food stamp receipt sometime in 1998–2001. We drop workers with more than one WOTC certification in the period 1996–early 2003 and workers with missing (and not easily imputed) data on the independent variables or with UI matches that have job start dates inconsistent with the WOTC records.

Variable	Odds Ratio	Z-statistic
Age	1.082*	1.75
Age squared	0.999	−1.56
Female	0.738**	−2.12
High school diploma	1.307**	2.38
Some college	1.404*	1.73
College degree	1.643	0.69
Black	0.932	−0.47
Hispanic	1.224	0.83
Other race (nonwhite)	1.186	1.04
Number of children under 6	0.937	−1.21
Number of children 6–17	0.986	−0.32
Milwaukee resident	1.089	0.64
Target group = Poor veteran	1.797*	1.82
Target group = Ex-felon	3.026***	4.14
Target group = (EZ/EC) high risk youth	1.635**	2.03
Target group = Vocational rehabilitation	0.646	−1.23
Target group = Food stamp youth (ages 18–24)	1.295	1.35
Target group = SSI	0.816	−0.72
Target group = WtW	1.333*	1.61
Size of disadvantaged workforce at firm (in 000s)	0.991***	−3.97
Occupation: clerical/sales	2.053**	2.19
Occupation: service	6.413***	5.81
Occupation: other	7.412***	6.21

NOTES: N = 3,978 (of which 453 are THS workers). Estimated via logit; pseudo $R^2 = 0.102$. * significant at 10%; ** significant at 5%; *** significant at 1%. Quarter indicators and an intercept are also included in the estimation. Omitted indicators: Education = Less than high school; Race = Caucasian; Target Group = Welfare; Occupation = professional/technical/managerial.

Table 7 Effects of THS Employment on Wages and Tenure of WOTC workers

Outcome of Interest	Effect of THS Job
Total wages (all jobs) in first year following WOTC job start	-\$247 (417)
Total wages (all jobs) in second year following WOTC job start	\$589 (501)
Total quarters employed in first year following WOTC job start	-0.086 (0.094)
Total quarters employed in second year following WOTC job start	0.164 (0.118)

NOTES: Standard errors are estimated via 100 bootstrap replications. We use a bandwidth of 0.20, but results with bandwidths of 0.10, 0.15, 0.30, and 0.40 are all quite similar. Wages are recoded from categories (approximately \$1 wide) using the midpoints of the range for each category. The measures with “year following” do not include the quarter in which the job started. Panel estimates use a slightly smaller data set because we drop those who were under age 16 two years before the relevant job start.

Sample Size: $N_{\text{treatment}} = 444$, $N_{\text{comparison}} = 3,525$

APPENDIX

ASSEMBLING DATA SETS FOR TABLES 1, 2A, 2B

Tables 2A and 2B

Sample assembly is straightforward to explain if we begin with Tables 2A and 2B. The total number of worker-jobs in the WOTC records in 1999–2002 is 20,577. The sample is then matched first to the UI data for the certified jobs (resulting in the sample for Table 2A) and then linked to further demographic information for the subset of these workers who had welfare or food stamp receipt sometime during 1998–2001 (Table 2B). The details are as follows:

A few of these 20,577 were missing from our UI data request.

* DROP 27 records

- Remaining: N = 20,550

Some workers in the WOTC records had no UI records at all in 1999–2004.

* DROP 698 records

- Remaining: N = 19,852

Some workers had UI records but none in the appropriate quarter or with the appropriate employer.

* DROP 305 records

- Remaining: N = 19,547

Some workers had records in UI in the correct quarter, but they were either labeled as a different employer or had multiple jobs so that it was impossible to match to the WOTC job.

* DROP 1739 records

- Remaining: N = 17,808

Some workers had a record matching the correct employer, but the job start date reported in WOTC was more than one quarter away from the job start date reported in UI.

* DROP 527 records

- Remaining: N = 17,281

Some jobs were not identifiable as “temporary help” or “non-temporary help”.

* DROP 263 records

- **Final sample size for Table 2A: N = 17,018 (of these, 2,695 are THS jobs)**

Table 2B keeps only those within the Table 2A sample who received welfare or food stamps sometime in 1998–2001 (this is the group for which demographic characteristics are available).

- **Final sample size for Table 2B: N = 6,374 (of these, 681 are THS jobs)**

Table 1

Assembling the sample for Table 1 was done in two parts: first assembling the treatment group (WOTC-certified THS workers) and then assembling the comparison group (WOTC-eligible but uncertified THS workers). Note that only eligibility via welfare or food stamps can be established with these data.

Treatment group:

We began with the WOTC-eligible THS workers assembled in Table 2B (N = 681).

Because these workers are to be compared to a group of workers who are WOTC-eligible based on their welfare or food stamp receipt, we would like the treatment group to contain comparable workers. For instance, if some of the WOTC-certified THS workers were claimed based on ex-felon status, and were not eligible for WOTC under a welfare/food stamp category, we would prefer not to use them in the comparison. We thus drop workers if both (a) they appear ineligible for WOTC via their welfare or food stamp records and (b) they were claimed for WOTC under a non-welfare/food stamp target group. Equivalently, we keep those who appear eligible via welfare/food stamps OR at least were claimed under a welfare/food stamp category (even if we don't find them to be eligible; we effectively assume that the authorities granting the certification are better able to establish eligibility than we are).

* DROP 204 records

- **Final Treatment Group for Table 1: N = 477**

Comparison Group:

We begin with the CARES data, and we identify all persons who became eligible for the WOTC (via welfare or food stamp receipt) during 7/99–12/01. This initial sample is 44,018 people. However, these people could only truly qualify for the WOTC if they began a job during the time they were eligible. We therefore match them to UI records, where we find no job records for 10,347 of these individuals during 7/99–12/01. Note that this does not indicate failure of the merge between datasets. Rather, most of those who are unmatched are likely to be out of the labor force, and thus legitimately missing UI records. We only want workers in our comparison group, so these people are intentionally excluded rather than being dropped due to a failure to merge with UI.

The remaining individuals have a total of 124,848 person-job records during this time period. However, not all of these jobs began in a quarter during which the worker was eligible to be

claimed for the WOTC. We therefore trim the sample down to the 48,872 person-job records for which a new job began during a quarter in which the worker was WOTC eligible. Some individuals obtain more than one new job during their time of WOTC eligibility, and each record is included here.

- Remaining: N = 48,872

We then remove records that are not THS jobs.

* DROP 36,535

- Remaining: N = 12,337

There are some duplicate records in the sample (same worker ID, employer ID, and same first quarter of work). We assume these are referring to the same job and just keep one copy of each.

* DROP 206

- Remaining: N = 12,131

Some UI records do not contain sufficient information to identify whether a firm is THS or not.

* DROP 1079

Final Comparison Group for Table 1: N = 11,052

Table A.1 Balancing Test Results for Selection into THS Employment

Conditioning variable	Sample	Mean of Conditioning Variable		Test results: initial bias and % reduction in bias after matching		p-value (matched)
		Treatment	Comparison			
age	Unmatched	27.329	26.069	% initial bias	15.6	0.972
	Matched	27.137	27.118	% reduction in bias	98.4	
female	Unmatched	0.731	0.856	% initial bias	-31.3	0.99
	Matched	0.746	0.746	% reduction in bias	99.7	
education	Unmatched	1.706	1.616	% initial bias	13.8	0.87
	Matched	1.694	1.701	% reduction in bias	92	
black	Unmatched	0.435	0.469	% initial bias	-6.9	0.937
	Matched	0.435	0.432	% reduction in bias	92.3	
Hispanic	Unmatched	0.060	0.052	% initial bias	3.5	0.927
	Matched	0.059	0.057	% reduction in bias	81.9	
other race	Unmatched	0.196	0.168	% initial bias	7.3	0.992
	Matched	0.196	0.196	% reduction in bias	99.1	
total kids under 6	Unmatched	1.042	1.255	% initial bias	-19.4	0.86
	Matched	1.049	1.062	% reduction in bias	94	
total kids 6-17	Unmatched	0.896	0.901	% initial bias	-0.4	0.93
	Matched	0.910	0.902	% reduction in bias	-57.4	
Milwaukee	Unmatched	0.607	0.609	% initial bias	-0.4	0.852
	Matched	0.606	0.600	% reduction in bias	-255.7	
firm “size” in 000s	Unmatched	15.826	23.430	% initial bias	-32	0.538
	Matched	15.949	16.777	% reduction in bias	89.1	
WOTC target group	Unmatched	5.943	6.214	% initial bias	-9.5	0.839
	Matched	6.002	5.963	% reduction in bias	85.7	
occupation	Unmatched	3.119	2.552	% initial bias	65.7	0.221
	Matched	3.106	3.038	% reduction in bias	87.9	

NOTES: This balancing test follows the propensity score matching estimation of THS employment effects. The interpretation of the p-values is that low values (such as those below 0.10 or 0.05) suggest that there is remaining selection bias for a given variable even after matching.

Table A.2 Balancing Test Results for Selection into WOTC Certification

Conditioning variable	Sample	Mean of conditioning variable		Test results: initial bias and % reduction in bias after matching		p-value (matched)
		Treatment	Comparison			
age	Unmatched	25.812	25.044	% initial bias	11.1	0.908
	Matched	25.735	25.668	% reduction in bias	91.4	
female	Unmatched	0.84688	0.84937	% initial bias	-0.7	0.916
	Matched	0.84395	0.84699	% reduction in bias	-22.1	
education	Unmatched	1.6719	1.46	% initial bias	34.3	0.935
	Matched	1.6656	1.6698	% reduction in bias	98	
black	Unmatched	0.39687	0.60083	% initial bias	-41.6	0.775
	Matched	0.40446	0.39325	% reduction in bias	94.5	
Hispanic	Unmatched	0.075	0.09688	% initial bias	-7.8	0.955
	Matched	0.07643	0.07524	% reduction in bias	94.5	
other race	Unmatched	0.20313	0.17741	% initial bias	6.5	0.842
	Matched	0.20701	0.20059	% reduction in bias	75.1	
total kids under 6	Unmatched	1.1844	1.514	% initial bias	-28.5	0.726
	Matched	1.1911	1.2213	% reduction in bias	90.8	
total kids 6-17	Unmatched	1.0313	1.2035	% initial bias	-11.5	0.924
	Matched	1.0287	1.0178	% reduction in bias	93.7	
Milwaukee	Unmatched	0.58437	0.79525	% initial bias	-46.8	0.644
	Matched	0.59554	0.57733	% reduction in bias	91.4	
firm headquarters in WI	Unmatched	0.5625	0.66162	% initial bias	-20.4	0.93
	Matched	0.56688	0.57038	% reduction in bias	96.5	
firm "size" in 000s	Unmatched	15.048	18.806	% initial bias	-22.3	0.965
	Matched	14.87	14.923	% reduction in bias	98.6	
avg. earnings/quarter at firm last year	Unmatched	1324	1027.1	% initial bias	75.7	0.265
	Matched	1316	1273.9	% reduction in bias	85.8	

NOTES: This balancing test follows the propensity score matching estimation of WOTC certification effects. The interpretation of the p-values is that low values (such as those below 0.10 or 0.05) suggest that there is remaining selection bias for a given variable even after matching.

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