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ADVANCEMENT

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ABSTRACT

We examine the effects of temporary help service employment on later earnings and employment for individuals participating in three federal programs providing supportive services to those facing employment difficulties. The programs include Temporary Assistance for Needy Families, whose participants are seriously disadvantaged; a job training program with a highly heterogeneous population of participants; and employment exchange services, whose participants consist of Unemployment Insurance claimants and individuals seeking assistance in obtaining employment. We undertake our analyses for two periods: the late 1990s, a time of very strong economic growth, and shortly after 2000, a time of relative stagnation. Our results suggest that temporary help service firms may facilitate quicker access to jobs for those seeking employment assistance and impart substantial benefits as transitional employment, especially for individuals whose alternatives are severely limited. Those who do not move out of temporary help jobs, however, face substantially poorer prospects, and we observe that nonwhites are more likely than whites to remain in THS positions in the two years following program participation. Our results are robust to program and time period.

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I. Introduction

The large increase in temporary help service (THS) employment in recent years—from less than 0.5 percent in 1982 to approximately 2.5 percent by 2004 (U.S. Bureau of Labor Statistics, 2005)—has been particularly dramatic for low-skilled, less-educated and minority workers, who are now greatly overrepresented in the temporary help workforce (Autor and Houseman, 2005; Heinrich, Mueser and Troske, 2005; DiNatale, 2001). This disproportionate concentration of disadvantaged workers in THS employment, combined with the growing use of temporary help service firms as labor market intermediaries by both private firms and public social welfare programs, has engendered an active policy and research debate about the consequences of such mediated employment for workers' wages, job stability, access to fringe benefits, and labor market advancement. In addition, the literature on the effects of THS employment has more recently begun to address some of the more complex questions about the implications of temporary help employment for workers' labor market outcomes, including these workers' subsequent labor market transitions, occupational mobility, and longer-term earnings trajectories.

In general, two competing arguments have been advanced about temporary help employment: (1) employment through THS firms may provide a path to permanent and stable employment for workers who might otherwise be excluded from such labor market opportunities, and (2) temporary help jobs supplant productive employment search and reduce access to better employment opportunities, ultimately depressing workers' wages and opportunities for advancement. The former argument is consistent with the basic premise underlying current U.S. public welfare and employment and training policies, which assumes that helping individuals to get jobs (even low-wage jobs) will give them the opportunity to gain

on-the-job skills and experience and move up the career ladder to better positions (i.e., “a foot in the door” or a “stepping stone”). With this greater policy emphasis on short-term, work-oriented social services, the role of THS firms in facilitating job placements has naturally grown, particularly for disadvantaged workers served by such programs.

In order to examine whether employment in the temporary help industry helps or hurts workers relative to other employment in the long run, we explore the subsequent employment dynamics of workers in this industry and compare their experiences with those of workers who either do not have jobs or who take jobs in other industries (i.e., in end-user firms). We focus our analysis on individuals in the state of Missouri who have sought employment assistance or cash support through any of three federal programs, Temporary Assistance for Needy Families (TANF), a job-training or intensive work search program (Job Training Partnership Act in 1997, Workforce Investment Act in 2001), and employment exchange services (Wagner-Peyser services). It is *not* a goal of this study to understand the impact of participation in these three programs, but rather to understand the impact of employment in the temporary help industry. We draw individuals from each of these programs in order to identify a diverse sample of individuals who are facing employment difficulties.

For many participants in these programs, entry into the program identifies a point of potential crisis in their work lives or careers. Participants in the job training program and the labor exchange services are explicitly seeking services to support employment efforts. The TANF program is designed to provide support for low income families with children, but it emphasizes entry into employment, and recipients who do not have an exemption face employment and job training requirements. Our analysis allows us to consider the role that

temporary employment and other industries play at such critical junctures in determining future labor market outcomes. Given that each program attracts individuals with markedly differing employment histories, work opportunities and other demographic characteristics, separate analyses of participants in these programs allow us to examine the role of selection into THS.

We begin our analysis by examining whether there are other industries that serve a role similar to that of the temporary help industry. We observe that individuals in our sample are particularly likely to move into temporary help employment when they enter these programs, and we consider whether this pattern can be observed for any other industries. Next we look at employment during the quarter following program entry, examining how employment and wages two years later are influenced by the sector of this initial employment, and, in particular, temporary help services. We limit the sample to those 18-64 years of age and conduct analyses separately for men and women. We report analyses initially for those who begin participation during calendar year 1997 and then consider analyses for those entering these programs in 2001. Our use of large and long panels of state-level administrative data on participants in three federal programs allows us to extend previous research on the effect of employment in THS by examining the impact of THS over an extended period and at different points in the business cycle and by comparing individuals who obtain employment in various industries and who have very different demographic characteristics.

Our main findings are as follows. First, we find that THS is unique in serving as a general transitional industry. Second, we find that working in the THS sector has very little long-term negative impact on either earnings or employment for workers in any of the three programs. If we believe that for workers in THS the next best opportunity is not having a job in

a quarter, working in the THS sector imparts significant benefits. Third, we find that worker success is contingent on transitioning out of the THS sector; workers who remain in the THS sector have long-run earnings that are substantially below workers in other sectors. Finally, we find that our results are strikingly consistent across the business cycle, and that the experience of nonwhites in THS jobs is very similar to that of whites.

In the next section we review the literature on the temporary help service industry. In section III we discuss our data and in section IV we consider the role of the temporary help service industry in providing transitional employment. We also examine the factors determining who takes a temporary help job. Section V presents estimates of the impact of temporary help employment on later earnings and employment, and section VI considers the role that movements between jobs has in helping individuals achieve higher earnings and stable employment. In section VII, we consider the degree to which results are replicated for individuals participating in these programs in 2001 (a time when economic growth had slowed). Section VIII focuses on the experience of nonwhites in temporary help jobs. Section IX turns to the issue of how robust our results are if the OLS assumption of an independent error is violated. The final section concludes.

II. Literature

There is strong agreement among a large number of studies that temporary help services jobs pay lower wages, offer fewer work hours, are shorter in tenure, and are significantly less likely to provide health insurance coverage or other fringe benefits (Autor and Houseman, 2005; Andersson, Holzer and Lane, 2002; Blank, 1998; Booth, Francesconi and Frank, 2002; Cohany, 1998; Heinrich et al., 2005; Houseman and Polivka, 1999; Houseman, Kalleberg and Erickcek,

2003; Lane et al., 2003; Nollen, 1996; Pavetti et al., 2000; Pawasarat, 1997; Segal and Sullivan, 1997). A smaller number of studies go beyond descriptive statistics to examine the employment and earnings paths or trajectories of welfare recipients and other low-wage workers who enter temporary help services employment.

Using matched samples of “at-risk disadvantaged workers”¹ from the Survey of Income and Program Participation (SIPP), Lane et al. (2003) find that individuals who take temporary help services jobs have better employment and “job quality” outcomes than those who do not enter employment. Temporary help workers fare slightly worse than those who enter other employment sectors in terms of earnings and benefits, although differences are generally small and not statistically significant. In addition, they conclude that the effects of temporary help employment in reducing welfare receipt and poverty relative to no employment are substantial, and that there is no difference in these outcomes between those in temporary and conventional employment.

Despite different populations of study (welfare recipients in Missouri and North Carolina), the findings of Heinrich et al. (2005) mirror those of Lane et al. (2003). After following welfare recipients who go to work for temporary help services for two years, Heinrich et al. find very small differences (1-7 percent) in earnings between those who initially took temporary help jobs and those who entered jobs in other sectors, with measured characteristics explaining most of the differentials. The earnings of welfare recipients initially entering THS jobs increased faster over the two-year period, in part due to their movement from temporary

¹ Lane et al. (2003) use propensity score matching to define comparison groups of “at-risk” workers (with incomes less than 200 percent of the poverty level) for their THS worker sample.

help into higher-paying industries. In addition, temporary help workers were no more likely to be out of a job two years later and only slightly more likely to return to welfare than workers in end-user firms, and they were substantially more likely to be employed and off of welfare two years later than recipients without a job.

Andersson, Holzer and Lane (2002) use data from five states (California, Florida, Illinois, Maryland and North Carolina) in the Longitudinal Employer Household Dynamics (LEHD) program at the U.S. Census Bureau to analyze a sample of workers with persistently low labor market earnings. Like Heinrich et al. (2005) they find that low-wage workers starting in THS employment earn lower pay while employed by the temporary agency but that subsequent job changes lead to higher wages and better job characteristics for these workers. Both Heinrich et al. and Anderson et al. observe that low-wage workers who begin work with THS firms are more likely to move to higher-paying industries, such as manufacturing, than those working in other sectors (or not working). Such mobility provides the primary path through which temporary help employment boosts later earnings; workers who do not leave the temporary help industry suffer an earnings shortfall. Andersson, Holzer and Lane (2008) also use this five-state LEHD sample, but consider a longer follow-up period and more sophisticated methods. Their results are substantively similar.

Autor and Houseman (2005) take advantage of random assignment of welfare recipients to welfare-to-work contractors, where contractors vary in their referrals to THS firms. Under the assumption that such referrals are not correlated with other contractor practices that influence client success, they estimate the effects of holding a THS job on low-skilled workers' labor market outcomes. Initial earnings increments among their THS workers do not persist, in part

due to declines in rates of employment, and THS workers fare more poorly over the subsequent two years in terms of their earnings than “direct-hire” placements. Point estimates imply that THS workers also earn less than welfare recipients with no job placements, although these differences are not statistically significant. When they examine the impact of temporary help employment using OLS, they obtain results consistent with others, that is, implying a substantial benefit of temporary help employment, so their results differ from others because of their identification methods, not because of their sample.

There is also a growing literature examining temporary help firms in Europe.² Booth et al. (2002) study temporary help employment in Britain using data from the British Household Panel Survey and methods similar to Heinrich et al. and find temporary employment to be an effective “stepping stone” to permanent employment. Kvasnicka’s (2005) study of temporary help workers in Germany does not produce evidence that these workers are more likely to move into permanent employment than unemployed workers, but neither does the analysis suggest that they suffer any adverse effects from temporary work. In their study of temporary help workers in Spain, Garcia-Perez and Munoz-Bullon (2005) find that temporary help workers in low occupational groups had much lower probabilities of securing a permanent job than more skilled workers. They concluded that these workers would have fared better had they not worked through these intermediaries.

The findings of these and related studies speak to important, cross-national public policy

² In the European literature, many studies examine jobs classified as “temporary” based on the contract under which an individual is hired. Such jobs account for over 10 percent of employment in France and Germany, and over 30 percent of employment in Spain (Gagliarducci, 2005). We limit our review to European studies that consider mediated employment corresponding to the temporary help service sector in the U.S.

questions about the role of labor market intermediaries as a solution to the problem of low-wage worker advancement (Poppe et al., 2003). A recent study by Even and Macpherson (2003) found that “switching jobs is vital to significant wage growth among minimum wage workers, particularly for young workers who find themselves in ‘low-training’ occupations” (p. 677). And Andersson, Holzer and Lane (2005: 143) similarly concluded that “job changes account for the vast majority of ‘complete’ transitions out of low earnings and even for most partial changes.” We expect the results of our study to contribute to these policy debates about the role of public and private intermediaries in helping workers connect with and advance in jobs.

The use of state-level administrative data allows us to expand the scope of our analyses beyond these existing studies in several ways. First, the long panel allows us to follow workers for an extended period after we first observe them in the temporary help industry. Our replication of the analysis over two time periods enables us to examine whether the effect of working in the temporary help industry varies across the business cycle. Second, because we have large sample sizes, we are able to compare the effect of working in a variety of industries. For example, we can compare the long-run impact of working in the temporary help industry with the impact of working in another service industry or in the retail trade industry, which may be the most relevant comparison for these workers. Finally, the fact that we have data from three federal assistance programs, containing workers with very different characteristics and coping with different types of employment shocks, allows us to examine the role that nonrandom selection has on our results.

It is important to emphasize that the only “treatment” we are considering in this analysis is the industry or employment sector of the firm into which individuals in our sample select after

entering one of these three public programs. We have no information in our data about whether individuals who take temporary help services jobs are directed to these jobs by counselors in the program. A 2001 survey of TANF recipients who had engaged in temporary help services employment in North Carolina found that most (77 percent) did not learn about these jobs through TANF counselors, but rather through other channels, including word-of-mouth, newspaper ads, or by contacting the firm directly (Heinrich, 2005).

III. Data

Our basic sample consists of individuals who entered one of the three public programs described above during 1997 or 2001. In each case, entry is defined as participation in a given quarter for an individual who was not a participant in the prior quarter. An individual who entered one of these programs during the year, exited and remained off for at least one quarter, and then reentered, will be included twice in the file for a given year. The number of such cases is very small. Information on program participation, as well as demographic information on individuals, comes from data maintained by the state of Missouri to administer these programs.

TANF data are from Missouri's Department of Social Services Income Maintenance file, which includes information on services received for all program recipients. The data are extracted on a monthly basis, and individuals are identified as new payees in a quarter if they are receiving cash payments under this program in a given quarter and were not recipients in the prior quarter. We omit a small number of payees who are males, in the two-parent program, and/or receiving payments on behalf of "child only" cases.³

³ We omit payees in child only cases because these individuals are exempt from employment and training requirements of the program.

Job training in 1997 was provided under the Job Training Partnership Act (JTPA), identified on administrative files of Missouri's Division of Job Development and Training (administered within the Department of Economic Development). Job training in 2001 was provided under the Workforce Investment Act (WIA), which replaced JTPA in July 2000 and was administered within the same department in the new Division of Workforce Development. All individuals enrolling in the "adult" or "dislocated worker" programs are included.⁴ Under JTPA, the adult program was means-tested, limited to individuals whose income in the prior six months is below specified levels. Although WIA allows universal access, participants generally have low earnings. Dislocated workers are typically individuals who have lost their jobs in firm-wide layoffs. WIA regulations place less emphasis on job training than did JTPA, although, in practice, participants for both programs are a select group of individuals who receive a level of attention far beyond those who obtain employment exchange services, and we refer to participants in both programs as undertaking "job training."

Employment exchange files identify individuals who register for services provided under federal Wagner-Peyser legislation.⁵ Most individuals who receive Unemployment Insurance (UI) payments are required to register for these services and a substantial portion of job exchange registrants are UI recipients. However, anyone in the state is eligible to use job

⁴ Since individuals in the adult program are younger, less well educated and have dramatically lower prior earnings than those in the dislocated worker program, we undertook our basic analysis separately for these two groups. As differences in results were usually not statistically significant, we combined the adult and dislocated worker programs.

⁵ In 1997, the state's job exchange service was administered by Missouri's Division of Employment Security in the Department of Labor and Industrial Relations. In 1999, the program was transferred to the Division of Workforce Development in the Department of Economic Development.

exchange services, so registrants include employed individuals seeking better employment prospects as well as other job seekers who are not receiving unemployment compensation.

Our data on earnings, employment history and the industrial classification of the job come from the UI programs in the states of Missouri and Kansas. Earnings for individuals in a quarter are reported by employers, and we are able to match these to program participants using Social Security numbers. Although these data exclude the self-employed, those in informal or illegal employment, and a small number of jobs exempt from Unemployment Insurance reporting requirements, they include the overwhelming majority of employment in these states. These data allow us to identify all employers for an individual during a quarter, but we cannot determine whether jobs were held simultaneously or sequentially. A very small proportion of Missouri residents hold jobs in states other than Kansas.⁶ All earnings in the analyses have been adjusted for inflation based on the consumer price index using quarter 2 of 1997 as the base.

The industrial classification is taken from information about the employer on these files, and our identification of temporary help workers is based on the convention that individuals working on a temporary assignment from a THS firm are listed as employees of that firm. Although the THS firm's own direct employees (e.g., office staff) will also be included, the proportion of such cases is expected to be small, especially among participants in the three programs we are considering.⁷

⁶ Approximately one in six TANF residents in Jackson County, Missouri, the central county for Kansas City, holds a job in Kansas. The proportion of St. Louis residents with jobs in Illinois is much smaller due to the depressed economy of East St. Louis, Illinois. No other significant concentrations of population are close to Missouri's borders.

⁷ Antoni and Jahn (2007) report that 7 percent of the employees in temporary help firms in Germany are permanent administrative staff.

We have described these programs and their selection criteria, as we expect them to have implications for the level of disadvantage and other characteristics of our sample members. Tables A-1 and A-2 provide means and standard deviations for each of our samples in 1997 and 2001, respectively.⁸ Looking at the panels for females, the statistics confirm that TANF entrants are substantially disadvantaged relative to the two other groups. For example, the mean number of years of completed schooling for welfare recipients is 11.3 in both years, at least a full year less than for the other groups. TANF recipients are younger, are more likely to be nonwhite, and have mean prior earnings that are generally less than half those of the others. As might be expected, TANF recipients have lower levels of job experience.

Participants in the job training and employment exchange programs differ from one another in somewhat more complex ways. Female job training participants are older but have about the same levels of schooling, employment and earnings as employment exchange participants. Nonetheless, employment exchange participants are more likely to have worked none of the prior eight quarters, implying somewhat greater variation in the sample.

When we consider males (Tables A-1 and A-2, right panels), we see that comparisons between job training and employment exchange show patterns that are similar to those for females. There are, however, differences in the job training program participants by year. In 1997, job training participants have appreciably higher levels of education than do those receiving job exchange services, and nearly 17 percent—more than twice as many—have a college degree. In contrast in 2001, the level of education for the two programs is very similar, with 7-8 percent of individuals with college degrees in both programs.

⁸ See appendix tables at <http://www.nber.org/data-appendix>.

The statistics also provide information about industry of employment four quarters prior to program entry and in the immediately subsequent quarter. We see that THS employment exhibits a substantial increase for all our samples at program entry, but that, eight quarters later, THS is less important. It appears that THS employment is particularly important for individuals facing some kind of employment crisis as compared to those same individuals at other points in their careers. In the next section, we consider whether THS employment is unique in this respect.

In the next three sections, we focus exclusively on individuals who enter one of the three programs in 1997. In section VII we compare the experiences of entrants in 2001 with those who enter in 1997.

IV. Temporary Help Services as Transitional Employment

Our analysis focuses on individuals who are likely to be at a juncture in their careers, either because they have lost a job or because they are making plans to pursue alternative employment or vocational training. Given its explicit temporary structure, it is natural to view THS as a transitional industry. In this section, we begin by looking at the patterns of job shift following program entry and examining the kinds of industries that may serve this kind of transitional role. Our conclusion is that THS appears to be unique among industries in filling this role. We then turn to an examination of the factors that are associated with employment in the THS industry.

Table 1 provides a comparison of the industry of employment four quarters prior to program entry and in the quarter subsequent to entry. The first line in the table shows the proportion of people without jobs. Given the income test for TANF, it is not surprising that

substantially more of these individuals are without jobs than in the other programs and that the proportion without jobs is particularly high in the quarter immediately following program entry. Although more of those in the job training program have a job prior to program entry, we also observe that enrollment is associated with an increase in joblessness. The reverse is true for those who have contact with the employment exchange services, presumably reflecting the program's concern with immediate employment.

The percentages in the table for each industry group identify the proportion of the sample that is employed in a job in the specified industry group in a given quarter. Individuals with jobs in more than one industry contribute multiple counts. We include all major industry categories in the upper panel. The panel for four-digit industries lists only those industry groups that include at least 5 percent of jobs for at least one of our samples.

The role that temporary help jobs play in this structure can be seen in the figures for the four-digit industries. The proportion of individuals in such jobs increases following program enrollment for each of our samples, with the proportional increase ranging from nearly 50 percent to over 100 percent. In the quarter following enrollment, the proportion with THS jobs is in the range of 10 percent for all samples. We undertook tabulations for all two-, three- and four-digit industries to see if we could identify sets of industries that served the same function as THS employment. Where we identified specific industries that attracted increases in employment following enrollment, we found them to be of little quantitative importance. Often an industry that appeared to serve as a transitional industry in one of our samples did not fill this role in others. These comparisons suggest that THS is unique among industries that we can identify.

Table 2 provides information on factors associated with having jobs in THS in the quarter

following initial program participation. Since we are concerned about the impact of industry of employment during this quarter, we refer to it as the “reference quarter.” For ease of interpretation, we have divided employment into three categories: THS only, THS and some other industry, and other industry only. The table reports coefficients from a multinomial logit model predicting type of job, with the omitted category no employment during the quarter.⁹ In almost every case, a likelihood ratio test rejected alternative models that combined these employment categories, and in every case we rejected models that combined THS with other employment.¹⁰ Nonetheless, for many of the variables, coefficients for the three employment categories are similar, so that substantive differences in the determinants are small.

Among TANF and both samples of employment exchange participants, those who are older are less likely to be working, whereas older individuals are more likely to be working among job training participants.¹¹ In all three samples, the relationship between age and employment is nonlinear, as indicated by a squared term that is negative in all cases but one, and in most cases is statistically significant. This implies that as individuals get older, in those samples where older individuals are more likely to work, an additional year of age is associated with smaller increases in levels of employment; and in those samples where older individuals are

⁹ We also fitted models that controlled for industry of employment in the year prior to program entry. As expected, such controls reduce the impact of stable characteristics on industry choice, since such factors would partly affect industry choice through previous industry choices.

¹⁰ We tested models that constrained coefficients of all employment categories to be the same, as well as models that combined two of the three employment categories, performing a total of 20 tests. In two cases, we were unable to reject the hypothesis of equal coefficients. In the JTPA samples, for both men and women, the test failed to reject the hypothesis that categories THS and TSH plus another jobs could be combined.

¹¹ Inferences about the impact of age are based on evaluating the derivative of the quadratic of the age function at age 33.

less likely to work, this effect is stronger at higher ages.

Our specification controls for education using years of education and dummies for high school and bachelor's degrees. The dummy coefficients identify effects of degrees beyond the linear impacts of years of schooling. In general, greater schooling is associated with higher levels of employment, and there is little evidence for deviations from a linear relationship. The exception is that, in the employment exchange samples (both for males and females), those with high school degrees are more likely to be working than the simple linear model would imply.

As might be expected, prior employment is a strong predictor of employment in the reference quarter; we see that the three coefficients measuring employment in the prior eight quarters are substantial, implying an impact of employment of roughly similar size in all our samples. Those who have no observed employment during the prior eight quarters are particularly unlikely to hold a job in the reference quarter. While there are few consistent differences in the determinants of THS and the determinants of other employment, we do observe that those who have worked continuously in the prior eight quarters are generally less likely to be in THS than in other employment.

Prior earnings are related to employment in a complex way. The coefficients for earnings in the year immediately prior are generally positive, while the coefficients for earnings two years earlier are generally negative. This may be interpreted as implying that it is growth in earnings that is predictive of employment. In most cases, the sum of these coefficients is positive, as might be expected, so higher average earnings are associated with a greater chance of employment. As a rule, prior earnings are less positively associated with temporary help work than with other employment, and in some samples, those with higher prior earnings are *less*

likely to be employed in temporary help than to be not employed at all.

The coefficients for county unemployment rate confirm that those in depressed counties are less likely to be employed; in four of the five samples, they are particularly unlikely to combine a temporary help job with another job. There is no consistent relationship between the county unemployment rate and holding a temporary help job as compared with another job. In addition, those in metropolitan counties are much more likely to be in temporary help jobs than those in nonmetropolitan counties. Differences between large and small metropolitan areas are modest, as are differences between suburban and central metropolitan counties.

Overall, we can conclude that age, education, prior work experience and the local economy predict who will be employed, but these variables contribute relatively little toward distinguishing temporary help employment from other employment. In contrast, race is among the most important predictors of temporary help employment, with nonwhites much more likely to be in temporary help employment in all of our samples.¹² This is particularly notable, since the relationship between other employment and race is generally small and inconsistent across our samples. Andersson et al. (2002) and Heinrich et al. (2005) similarly find that both black and other nonwhite minorities are more likely to be employed in the temporary help services sector. Andersson et al. also find that black males are more likely than any other group to “escape” a pattern of persistently low earnings through temporary help employment.

These results suggest that explanations about selection into temporary help jobs that rest primarily on arguments about general levels of human capital miss the mark. What matters most

¹² The overwhelming majority of nonwhites in the programs we are considering are African American.

is “race and place.” The explanation for the concentration of temporary help employment in metropolitan areas is undoubtedly the need for temporary help services to operate in an environment with a sufficient number of primary employers. We suspect that the large impact of race stems from employer difficulty judging worker productivity. If employers believe they are less able to judge the ability of nonwhite workers or that nonwhite workers are generally less productive, they may be less willing to hire nonwhite workers into regular jobs that imply long-term commitments. In the absence of effective legal prohibition against use of race by employers in hiring, temporary help jobs may provide valuable opportunities for nonwhites. In section XIII below, we return to the question of how the nonwhite experience may differ from that of whites in our sample.

V. Impacts of Temporary Help Experience on Earnings and Employment

To examine the impact of temporary help employment on ultimate earnings, we estimate a model that predicts earnings eight quarters after the reference quarter. Controls include basic human capital measures as well as indicators of prior employment experience, corresponding to the control variables in the logit equations reported in Table 2. In addition, we control for industry prior to program entry, since we are interested in determining the impact of a temporary help job following program participation, not effects of prior experience.¹³ Based on the same model, we also perform a difference-in-difference analysis, where the dependent variable is the difference

¹³ The measure of prior industry is based on industry of employment in all four quarters prior to program entry. Each industry dummy is coded one if there is any quarter in which the industry of employment falls in the specified category. Results are not sensitive to inclusion of these measures.

in earnings between the outcome quarter and the quarter nine quarters prior to program entry.¹⁴

The program evaluation literature underscores the importance of taking account of the way in which program participants are selected (as reflected, for example, in the “Ashenfelter dip”) in any attempt to identify program effects on the basis of comparisons between participants and others (Heckman, LaLonde and Smith, 1999). The analysis here differs from the standard evaluation in that all individuals in our sample participate in a program. Insofar as selection into a given program *per se* is important in determining outcomes, our design controls for this selection. Nonetheless, prior employment experiences must be controlled for, as we expect them to be related to job entry following program participation.¹⁵ The difference-in-difference analysis allows us to control for stable differences across individuals that may lead them to take different kinds of jobs.

Estimated effects on earnings

Estimated coefficients for these regression equations are reported in Table A-3. In general, coefficients for control variables are as expected, and, although there are some differences across our five samples, few are statistically significant and substantively important.¹⁶ Among the control variables for prior employment, the most important are the

¹⁴ Such a symmetrical difference-in-difference specification controls for program selection by earnings if the time-varying component of earnings has a simple autoregressive structure (Ashenfelter and Card, 1985).

¹⁵ Dyke et al. (2006) evaluate job training for TANF participants using a similar design, although they control for prior labor market activity with a matching methodology.

¹⁶ One inconsistency across samples is in the effect of race. We find that nonwhite TANF recipients have higher earnings than other TANF recipients, whereas in the other samples nonwhite earnings are lower, in keeping with findings based on representative samples. The estimated impact in the TANF sample very likely reflects the strong selection of nonwhites into welfare. In a study of six metropolitan areas, Hotchkiss, King and Mueser (2005) also find that

measures of earnings, both in the year immediately prior to program entry and in the previous year.

Table 3 reports predicted quarterly earnings in the eighth quarter after the reference quarter based on the regressions in Table A-3 using the mean values of variables in the specified sample. For comparison, unadjusted earnings in the reference quarter and the outcome quarter are presented, along with predicted impacts of employment in various sectors relative to those not employed.¹⁷ Focusing first on the samples of females, line 1 shows that mean earnings in the reference quarter of those with only a temporary help job are below those for individuals employed in all the other sectors, and that, except for retail trade jobs, the differences are substantial. Controlling for individual characteristics (not shown) confirms that these patterns are not primarily due to differences in measured characteristics. Clearly, entering temporary help employment in the quarter after program entry is associated with a substantial immediate income decrement relative to most other kinds of employment. On the other hand, looking at those who hold jobs in multiple sectors, the role of temporary help employment is less clearly damaging, since those who hold THS jobs and other jobs have earnings at or close to the level for those in most other sectors. Among those with jobs in a single major industry, those with manufacturing jobs usually have the highest earnings, although service and “other” jobs have similar or higher earnings in some cases.

employment and earnings for nonwhites among TANF and AFDC recipients are higher than for whites.

¹⁷ Changes in the relative impacts of industries between lines 2 and 3 are equivalent to the explained portion of the Oaxaca-Blinder decomposition. Our use of a single equation constrains variable impact estimates to be the same for all industries, so the explained portion of the difference between industries i and j can be written unambiguously as $(\bar{X}_i - \bar{X}_j)B$, where \bar{X}_i and \bar{X}_j are vectors of means for the industries and B is a vector of coefficients indicating

Line 2 shows that, eight quarters later, the relative earnings of those initially in THS jobs have at least partly caught up with others. Earnings for temporary help workers increase by more than 50 percent in this period, an appreciably larger rate of growth than for any of our other industry categories.¹⁸

Continuing our focus on the three samples of females, line 3 shows that the impact of controls is somewhat different for the programs. In the case of TANF, it appears that those who take temporary help jobs are somewhat more advantaged than those in manufacturing, retail trade and service jobs, since controlling for background characteristics reduces the relative predicted earnings of those in temporary help jobs. About a third of this advantage can be traced to the higher proportion nonwhite in the THS sample, and the remainder to the concentration of the THS workers in large metropolitan areas (characteristics associated with higher earnings in the TANF sample). In contrast, for the other samples, THS workers are disadvantaged, so the relative benefits of having a manufacturing job are explained in part by observable differences among people (this is reflected largely in lower preprogram earnings for THS workers). For TANF and employment exchange participants, ultimate earnings are up to 20 percent greater for those who had a manufacturing job rather than a temporary help job; for job training participants, the increment is small.

variable effects.

¹⁸ Data from the Current Population Survey show that almost 40 percent of THS workers are working in service sector jobs, while 30 percent are working in manufacturing jobs (DiNatale, 2001). Since many of these workers will transit into permanent jobs with the same employer where they are assigned as THS workers, and since manufacturing jobs in particular tend to pay above-average wages, such moves may be at least partly responsible for the rapid growth in wages for those initially in THS jobs. In the next section we explore more thoroughly the transition of THS workers into permanent jobs.

The largest categories of employment for all our samples are retail trade and service. For all female samples, the estimated impact on ultimate earnings of a retail trade job is close to that of a temporary help job. Service jobs produce incomes about 10 percent higher than temporary help jobs. Those with jobs in multiple sectors—whether or not they hold a THS job—generally have higher earnings than those with jobs in single sectors, except for manufacturing.

Line 4 indicates that the impact of holding any job—regardless of industry—is positive across the three samples of females. The employment exchange sample yields estimates of the impact of holding a job that are substantially above estimates for the other samples. Parallel (and very similar) estimates based on the difference-in-difference model are presented in Table A-4.

If we aggregate all of the industries other than THS into a single category, this allows us to compare THS workers with the “average” alternative. Earnings in the outcome quarter for this category are about 10 percent higher than for THS workers, a difference that is statistically significant in about a third of the cases.¹⁹

Our conclusion is that temporary help employment has few deleterious effects on earnings relative to other industries for women eight quarters later. Earnings growth is greater than any other employment sector and ultimate earnings are on a par with those obtained in the most common industries. Outcomes for those with any employment in the reference quarter are appreciably better than for those who don’t obtain employment.

Patterns for males are similar to those for females. Earnings in the reference quarter for those in THS jobs alone are appreciably below earnings in all other industry categories, and less

¹⁹ Across the three programs, we consider the direct and difference-in-difference estimates, observing two statistically significant estimates out of six. See Table A-5.

than half of earnings in manufacturing. However, earnings growth for those who begin in temporary help is much higher, about 50 percent over the two year period, compared to less than 25 percent for other categories. As a result, the difference between temporary help and the highest paid industries is substantially reduced in the outcome quarter. Line 3 indicates that more than half of the remaining difference is explained by individual characteristics and prior labor market measures.²⁰ In the employment exchange sample, we see that those with any employment have appreciably higher earnings than those without jobs, but that those in temporary help have earnings at least slightly below those in every other sector. Those with manufacturing jobs have ultimate earnings that are predicted to be 43 percent above observationally similar individuals with temporary help jobs. If we aggregate all industries outside of THS, the increment is 31 percent (see Table A-5). Finally, looking at predicted earnings of males who hold both a THS job and a job in another sector, we see that the predicted earnings are somewhat higher than for those with just THS jobs and comparable to those for all industry groups except for manufacturing and “other.”

Estimated effects on employment

Table A-6 provides estimated results for a linear probability model in which the dependent variable is employment eight quarters after the reference quarter. Control variables are identical to those in the Table A-3. Table 4 provides parallel measures indicating expected levels of employment eight quarters later based on sector of employment in the reference quarter.

The patterns of results parallel those for earnings (reported in Table 3) fairly closely. The

²⁰Up to a fifth of the original difference is explained by the larger number of nonwhites and slightly lower level of education in the THS sample. The remainder is explained by the lower level of prior earnings we observe among THS workers.

likelihood of employment eight quarters later is strongly associated with employment in any sector in the reference quarter. Differences between men and women are small in the two programs they have in common. Although those in temporary help jobs are somewhat less likely to work in the outcome quarter than those in most other categories, the difference declines once we control for individual characteristics (line 2). In fact, the difference between temporary help workers and others in terms of ultimate employment is, as might be expected, substantially smaller than the difference in earnings.²¹ Those who combine jobs in more than one industry during the reference quarter generally have higher rates of later employment than other categories.²²

VI. Transitions between Sectors

The pattern described above in which individuals in temporary help service jobs begin with lower earnings that increase faster over time reflects in part their movement into more remunerative jobs outside the temporary help sector. In Table 5, we examine movements between sectors over eight quarters. The tabs on the left of the table indicate the employment sector during the reference quarter, and row entries indicate the percentages of each group in the indicated categories eight quarters later. These tabulations show that those in THS jobs are much more likely to move into another major sector than are individuals in any other major sector.

Consider the proportion of individuals in temporary help service positions who remain in

²¹ Table A-7 presents results based on aggregating all employment outside THS.

²² As in the case of earnings, substantive conclusions for the difference-in-difference analyses are similar. A partial exception is that for employment exchange participants (both male and female) the impact of reference quarter employment is approximately half as large in the difference-in-difference estimates. Compare Table A-4 with line 3 of Table 4.

any service position. Among TANF recipients, some 28 percent of THS employees are in service positions (including THS) eight quarters later, whereas 42 percent of other service workers are in some kind of service position. The comparisons are even more dramatic for females entering job training or the employment exchange, where over 50 percent of other service workers remain in service positions, compared to 28 percent of THS workers.

We can also see that temporary help workers are more likely to move into manufacturing positions than are any other category of worker, with the exception of those in manufacturing or in multiple sectors. For example, among females receiving employment exchange services who are in THS positions in the reference quarter, 8.9 percent are in manufacturing eight quarters later.²³ For those in retail trade, service or other industries, no more than 4 percent move to the manufacturing sector eight quarters later. THS workers are also very likely to end up in jobs in multiple sectors, with more than one in ten THS workers so classified eight quarters later.

The importance of moves between industries is illustrated in Table 6. Lines 1 and 2 are based on estimates from a model that controls for *both* reference quarter industry and outcome quarter industry. The estimates in line 1 confirm the view that once we have taken into account whether the individual is employed and the industry of employment in the outcome quarter, prior industry of employment is relatively *unimportant*. Among TANF participants, those with temporary help jobs are predicted to have earnings in the outcome quarter that are \$421 higher than those with no jobs (line 4 of Table 3); once industry in the outcome quarter is controlled, that increment declines to \$123 (line 1 of Table 6). Similarly, ultimate earnings are expected to

²³ Moves by THS workers to manufacturing may partly reflect reclassification of temporary help workers to permanent status within a firm. See footnote 18.

be \$263 higher for those with manufacturing jobs than for temporary help jobs, a difference that declines to \$81 (and is not statistically significant) when ultimate industry is controlled. This basic pattern is the same for all programs and for males and females; the primary way that reference quarter industry influences outcomes is through its impact on ultimate industry of employment.

Coefficients in line 2 show that movement into other employment is particularly valuable for those with reference quarter jobs in temporary help. In every sample, those who ultimately end up in temporary help jobs have the lowest earnings of any industry category, and the difference is often substantial. This contrasts with estimates in Table 3, which show that a temporary help job in the reference quarter is not associated with appreciably lower earnings than many other categories. Clearly, those who do not move out of temporary help jobs face substantially poorer prospects. This contrasts with individuals initially in retail trade jobs, who do less well than those in temporary help (Table 3), but if they stay in retail trade, their earnings are actually higher than temporary help workers who stay in temporary help (line 2 of Table 6).

VII. Changes in the Role of Temporary Help Employment: Comparisons with 2001

The previous analyses consider the impacts of temporary help employment for those facing employment difficulties in 1997, a period of extraordinary economic growth in Missouri and the nation as a whole. Missouri's unemployment rate was approximately 4 percent during 1997 and early 1998 when individuals entered the programs and obtained initial jobs, and it had declined further, to around 3 percent, eight quarters later when we consider their employment outcomes.

Over the three years 1997-1999, employment in Missouri grew by 4.4 percent.²⁴ It is possible that the role of temporary help may not be reproduced in a period of slower growth. Temporary help jobs may be harder to get when the economy is not growing, and those who take them may have a harder time moving onward from them.

We have therefore replicated our analysis for those entering these programs in 2001. During 2001, the unemployment rate in Missouri increased from about 4 percent at the beginning of the year to about 5 percent at the end. Eight quarters later, unemployment had increased to over 5.5 percent, peaking at 6 percent around the middle of 2004. Missouri experienced an overall employment decline of 1.5 percent during the period.²⁵ Thus, although the recession in Missouri and the rest of the nation was mild by historical standards, the difference in labor market conditions between 1997-1999 and 2001-2003 was substantial.

The programs underwent changes between 1997 and 2001, and there is no certainty that the selection of individuals or the program impacts will be precisely the same. Welfare reform at the state and federal levels affected the TANF program over this period, although the basic structure of the program, and especially its emphasis on employment, was in place by 1997. In addition, the formal structure of job training programs under WIA, which replaced JTPA in 2000, was altered in many ways. Among the most important differences is that WIA formally provides sequential access to several levels of job search services, with significantly fewer clients receiving training. Nonetheless, WIA has much in common with JTPA. Like JTPA, much effort is focused on identifying prospective participants and selecting those viewed as most

²⁴ Specifically, employment growth was measured for January 1997-January 2000.

²⁵ Employment growth for January 2001-January 2004.

appropriate for the program, and services are often provided by the same organizations.

There have been some changes in administration of employment exchange services over the period 1997-2001. By 2001, most job exchange services were provided in “one-stop” centers offering a variety of job-related services (including job training under WIA), replacing the stand-alone offices that previously supported the state’s Unemployment Insurance program.

Nonetheless, in both 1997 and 2001, a large share of clients consisted of individuals receiving Unemployment Insurance payments who were required to participate in the program. In both periods, program access remained open, so anyone could obtain services. The amount of time a client spent with a counselor or in job-related programs was generally quite limited.

As noted above, when we compare the characteristics of individuals in the three programs (Tables A-1 and A-2), the patterns of participant characteristics across programs are similar for the two periods. Comparing Table 7 with Table 1, we see that in 2001 THS employment continues to play the transitional role that we observed in 1997, with increased temporary help employment immediately following program entry. There are some differences, however. Both TANF and job training participants have appreciably higher levels of THS employment prior to program participation than do participants in 1997. Thus, for job training participants the increase in THS employment between the prior quarter and the immediately subsequent quarter in 2001 is smaller, and for TANF recipients we observe a small decline. Nonetheless, there is no alternative industry that serves as a transition structure, and the decline observed for THS among TANF recipients is smaller than the overall decline in employment.

We replicate our analysis predicting industry of employment in the quarter following program entry; results for 2001 are reported in Table A-8, paralleling those reported in Table 2.

The similarities in the patterns of the coefficients are striking, and most differences are not statistically significant.²⁶ We see that employment is more strongly associated with education—but not necessarily high school graduation—in 2001 than in 1997. One difference is that the selection of nonwhites into THS employment is somewhat weaker in 2001, and THS employment is somewhat less strongly associated with the large metropolitan areas. Still, the conclusion that “race and place” are the two most important determinants of THS employment continues to be true in 2001.

Table 8 provides estimates based on program participants in 2001 of the effect of THS and other employment during the quarter following participation on earnings eight quarters later. The first and most important conclusion is that the pattern of results is very similar to that for 1997 program participants. Yet there are a number of statistically significant differences. For example, mean initial (reference year) earnings are often higher for TANF participants in 2001, whereas earnings eight quarters later are lower. For females obtaining employment exchange services, earnings are, similarly, initially higher in 2001, but they are also higher in the outcome quarter. For females and males in job training, differences between years are small and not statistically significant. For males in the job exchange program, initial earnings are higher in 2001 than in 1997, but outcome earnings are inconsistent across initial occupation.

The patterns of effects for industries correspond closely. Perhaps most significant, if we examine the impact of a THS job as compared to no job (column 2, line 4), the difference between the estimated effects for 1997 and 2001 is never statistically significant. In both

²⁶ Statistical significance for differences between the 1997 and 2001 analyses are based on one-to-one comparisons of parallel coefficient estimates for a given program.

periods, temporary help employment is associated with a larger expected benefit for those in the employment exchange program than for those in other programs.

Relative to other employment, the impact of THS employment is estimated to be slightly less beneficial in the later period. For example, for male employment exchange participants in 1997, the benefit of having an initial THS job relative to no job was \$915 (line 4, Table 3). The additional increment of having a manufacturing job was \$1,401. In 2001, the comparable benefit for a THS job was similar (\$1,049), but the increment over that for a manufacturing job had increased to \$1,756. This is typical of the observed differences for both men and women. The differences over time are never more than a few hundred dollars, but they are generally consistent across programs even where they are not statistically significant. Based on the two estimation approaches (line 4 and Table A-9), if we consider all alternative industries and combinations of industries, we have 60 comparisons of the increment of an industry relative to THS. In 48 of these comparisons, the benefit of having an alternative job relative to a THS job increased between 1997 and 2001. We see the same pattern if we consider analyses that compare THS with an aggregated category of other industries (compare Tables A-5 and A-10).

We also examined the effect of initial THS employment for the 2001 samples on whether the individual is employed eight quarters later, corresponding with the estimates reported in Table 4 for 1997.²⁷ Our findings for employment correspond closely with those for earnings. Although the benefit of having a temporary help job relative to having no job remains unchanged, the incremental benefit of other kinds of jobs has increased in 2001. This

²⁷ Estimates for industries are reported in Table A-11; those aggregating non-THS employment are reported in Table A-12.

improvement is apparent for all samples except for the sample of males participating in the employment exchange services. In this latter, very large sample, the effects of THS employment relative to other industries are essentially the same for 1997 and 2001.

Taken together, the comparison of estimates of impact on earnings and employment for program participants in 2001 and 1997 confirms the view that, in a sluggish labor market, alternatives to temporary help employment provide greater relative benefits than when the economy is strong. The consistency of these results across programs implies that our findings are not an artifact of the changes in program structure that occurred over this period.

We performed analyses for program enrollees in 2001 looking at the transitions between sectors over the eight quarters following the reference quarter and the relative importance of initial industry and ultimate industry in determining earnings. These analyses, which parallel those reported in Tables 5 and 6 for 1997 are reported in Tables A-13 and A-14. As might be expected, in the more recent period, individuals are more likely to find themselves without a job in the final quarter, but the pattern of results is very similar to the earlier results.

Notwithstanding the differences highlighted in this section, analyses for 2001 produce substantive conclusions that are identical to those for 1997. It is clear that whatever role the temporary help sector plays in the careers of individuals facing employment difficulties, this does not depend critically on economic growth.

VIII. Nonwhites

We have observed that nonwhites are appreciably more likely to work for THS firms than are whites and that this relationship remains strong even after controlling for demographic characteristics and metropolitan status. In order to provide insight into the role that THS

employment may play for nonwhites, we have undertaken separate analyses for this group.

First, we have examined the pattern of THS employment prior to and immediately following program participation, considering nonwhites separately by gender for each of our programs. We observe that THS employment for nonwhites increases as it does for the full sample. Measured as a proportion of all nonwhite workers, the growth in THS employment is greater than that for whites, but as a proportion of prior THS employment, the increase is somewhat smaller. This suggests that the transitional role of THS employment is at least as important for nonwhites as for whites but that THS employment provides *nontransitional* employment for a larger share of nonwhite workers.

Replicating the analysis predicting THS employment (three categories of employment contrasted to not employed) in the reference quarter, we found that the pattern of coefficients corresponded, in substance, to those reported above (Tables 2 and A-8). As in the full sample, we found no evidence that differences in human capital (as proxied by age and education) played an important role in allocating nonwhites to THS jobs. We conclude that it is unlikely that the overrepresentation of nonwhites in THS employment reflects differences in unmeasured levels of human capital. As expected, we found that metropolitan status was strongly related to THS employment, paralleling the results in the full sample.

If the returns for THS employment are greater for nonwhites, this may provide an explanation for the overrepresentation of THS workers. Alternatively, if nonwhites face discrimination in hiring for direct employment jobs, this could increase hiring rates of nonwhites by THS firms, causing nonwhites to gravitate toward such jobs even in the absence of greater benefits. Estimates of the impacts of reference quarter industry on earnings in our sample of

nonwhites differ in modest ways from those for the full sample reported in Tables 3 and 8.

Among TANF recipients, earnings for nonwhites tend to be above those for whites, a difference that remains when individual characteristics are controlled. For other programs, however, the reverse is true. The impact of holding a job appears to differ by program as well. For TANF recipients, the impact of employment on ultimate earnings is generally 10-30 percent greater in the nonwhite samples. In contrast, for the employment exchange participants, nonwhite impacts are generally smaller than for whites. When we compare THS employment with employment in other industries, there is little evidence that nonwhite returns differ from those of whites.

We do observe that nonwhites are more likely than whites to remain in THS positions in the two years following program participation. For example, among all men in the employment exchange sample who were in THS positions in 1997, only 20 percent remained in those jobs two years later (Table 5). In contrast, among nonwhites, this proportion was 27 percent. It also appears that nonwhites are less likely to move from THS jobs into manufacturing jobs than are whites. Yet analyses that examine the importance of movement out of temporary help positions (corresponding to Tables 6 and A-14) indicate that such movement is as important for nonwhites as whites. These results imply that although nonwhites experience lower levels of mobility toward high paying jobs, the benefits of employment in particular industries are similar. Overall, analyses focusing on the nonwhite sample suggest that the mechanisms underlying THS employment for nonwhites operate much the same as for whites.

IX. Robustness Tests of Industry Impact Estimates

Implicit in our estimates of the effect of current industry of employment on later earnings and employment is the assumption that no unmeasured individual characteristics affect both industry

and ultimate earnings. We believe the approach taken here minimizes the importance of such factors. The analysis above controls for a variety of measures reflecting pre-program labor market experience, as well as standard demographic characteristics. Because we observe people in a period when they are experiencing employment distress, the randomness of the labor market may be of greater importance than at other times in their lives. The assumption that unmeasured factors do not seriously bias results is supported by our earlier results based on TANF recipients in Missouri and North Carolina (Heinrich, et al., 2005), which found no evidence that selection into initial jobs altered estimates.

Nonetheless, it is difficult to assure that the individuals who obtain jobs, or obtain jobs in various industries, are not different in unmeasured ways that influence ultimate employment. In a recent analysis of the effects of Catholic school attendance on student outcomes, Altonji, Elder and Taber (2005) suggest that information on the likely impact of unmeasured factors can be obtained by examining those variables used to control for measured differences. In particular, they argue that individual characteristics captured in measured variables may be expected to be similar to unmeasured factors influencing individual outcomes. Following an earlier analysis by Murphy and Topel (1990), they propose a statistical test to determine whether observed estimates of causal impacts are likely to be spurious.

*Formal structure*²⁸

Consider our estimation equation

$$Y = D\alpha + X\gamma + \varepsilon + u, \tag{1}$$

²⁸ For details of this approach, see Altonji, et al. (2005), from which the following discussion is largely drawn.

where Y is the outcome measure (quarterly earnings or employment), D is a vector of dummy variables identifying industry of employment in the reference quarter with no job the omitted category, X is a vector of control variables (including a constant), ε is the component of unmeasured determinants that reflects factors that may be associated with industry of employment in the reference quarter, and u is an independent error reflecting variation that is unstable from quarter to quarter. α and γ are vectors of coefficients that we have estimated by OLS under the assumption that $(\varepsilon + u)$ is uncorrelated with D or X . The methods presented here are designed to help in considering whether the correlation between D and ε may cause the estimated coefficients $\hat{\alpha}$ to be spurious.

We wish to separately consider each of the seven industry categories that are used to identify employment during the reference quarter. We therefore focus on individuals in each industry category, comparing them with individuals with no jobs. For simplicity, our analysis will assume that there are no interaction effects between D and X in predicting earnings or employment.

Consider the relationship between the dummy identifying employment in a particular industry k and the other factors predicting the outcome variable, that is, $X\gamma$ and ε . Focusing on the sample limited to those with no job ($D_0 = 1$) or those with a job in industry k ($D_k = 1$), consider D_k^* , the linear projection of D_k onto $X\gamma$ and ε ,

$$D_k^* = \phi_{0k} + \phi_{X\gamma,k}(X\gamma) + \phi_{\varepsilon k}\varepsilon. \quad (2)$$

If $\phi_{\varepsilon k} > 0$, this implies that the estimate of α_k based on (1) will be biased. In particular, the standard formula for bias implies that

$$E(\hat{\alpha}_k) = \alpha_k + \phi_{\varepsilon k} \frac{\text{Var}(\varepsilon)}{\text{Var}(\tilde{D}_k)}, \quad (3)$$

where \tilde{D}_k is the industry dummy purged of its correlation with X .²⁹ If unmeasured factors influencing earnings and employment are similar to measured factors, we might expect that $\phi_{X\gamma,k}$ and $\phi_{\varepsilon k}$ would be similar. Altonji et al. (2005) show that if there are a large enough number of variables predicting the outcome and if no small subset is disproportionately important in terms of explanatory power, we expect $\phi_{\varepsilon k} = \phi_{X\gamma,k}$. Since the error term is likely to contain some factors that are truly random, they argue that it is plausible to assume that $\phi_{\varepsilon k} = \rho \phi_{X\gamma,k}$ with $0 \leq \rho \leq 1$.

Using the bias estimate in (3), we can see that the true coefficient would be zero if $\phi_{\varepsilon k} = \phi_{\varepsilon k}^*$, with $\phi_{\varepsilon k}^*$ defined by

$$\phi_{\varepsilon k}^* \equiv \hat{\alpha}_k \frac{\text{Var}(\tilde{D}_k)}{\text{Var}(\varepsilon)}, \quad (4)$$

where we have substituted the estimated value $\hat{\alpha}_k$ for $E(\hat{\alpha}_k)$. The ratio $\phi_{\varepsilon k}^* / \phi_{X\gamma,k}$ indicates how large the coefficient for the unobserved error term in (2) would have to be relative to the coefficient for observed determinants of the outcome in order for $\hat{\alpha}_k$ to be entirely spurious.

The extent of the bias is conditional on ρ , which is not observed. When $\phi_{\varepsilon k}^* / \phi_{X\gamma,k} > \rho$, the bias toward zero in α_k is less than the absolute value of $\hat{\alpha}_k$. If $0 \leq \phi_{\varepsilon k}^* / \phi_{X\gamma,k} \leq \rho$, this implies that the bias toward zero exceeds $\hat{\alpha}_k$, so that α_k is expected to have the opposite sign of

²⁹ $\tilde{D}_k = D_k - X \hat{\beta}_k$, where $\hat{\beta}_k$ is the vector of coefficients estimated from a regression of D_k on X .

$\hat{\alpha}_k$. When $\phi_{\varepsilon k}^* / \phi_{X\gamma, k} < 0$, the unbiased estimate of α_k will be greater in absolute value than $\hat{\alpha}_k$, i.e., the bias is away from zero for any $\rho > 0$.³⁰

Since there is no way to determine the exact size of ρ , we will interpret $\phi_{\varepsilon k}^* / \phi_{X\gamma, k}$ in terms of plausible possible values. If $\phi_{\varepsilon k}^* / \phi_{X\gamma, k}$ is larger than one, this implies that in order for α_k to be zero (or of opposite sign of $\hat{\alpha}_k$), unmeasured determinants would have to be more strongly related to the industry than observed variables, that is, $\rho > 1$. Assuming this is implausible, we can take this as evidence that the estimate is not entirely spurious. A negative ratio suggests that unmeasured determinants would need to be qualitatively different than measured determinants to render the estimated coefficient entirely spurious, that is, it would require $\rho < 0$, which we again view as implausible.

If the ratio $\phi_{\varepsilon k}^* / \phi_{X\gamma, k}$ is between zero and one, the estimated coefficient would be spurious for some ρ between zero and one. Since this is a plausible range, implying that the unmeasured determinants were similar to the measured determinants, we conclude that the estimated coefficient could be entirely spurious, or even of opposite sign from the true value. Of course, in the absence of an independent measure of ρ , we have essentially no information on the true coefficient value.

The details of the implementation of this test are provided in the Appendix.

³⁰ Estimating the exact size of the bias conditional on ρ is somewhat involved; see Altonji et al. for details.

Results

Table 9 provides diagnostics relevant to estimated effects of industry in the quarter following program entry on earnings eight quarters later, which are reported in Table 3 (program entry in 1997) and Table 8 (program entry in 2001). We focus on estimates of the impact relative to the no employment category. The estimates in line 4 of those tables are reproduced in line 2 of Table 9. Line 1 of Table 9 presents the simple difference in earnings between those with reference category jobs in a given industry and those with no jobs. The difference between estimates in lines 1 and 2 indicates how controls affect the estimate. Where the difference is large, this implies that controls predicting earnings are strongly related to the industry, and in those cases we expect that our diagnostics will imply that the observed coefficient could be spurious. Line 4 lists the value of the implied ratio $\phi_{ek}^* / \phi_{X\gamma,k}$, where $\phi_{X\gamma,k}$ is based on all variables taken together, as specified in the formal structure presented above. In lines 5 and 6, we have used an estimate that decomposes the factors predicting earnings into educational measures, labor force indicators, and other controls. In line 5, we use the education measures in constructing the ratio, and in line 6 labor market experience is used. (Further details are provided in the Appendix.) In the discussion that follows, if the implied ratio is between 0.0 and 1.2, we assume that the estimated coefficient could well be spurious; a ratio outside that range will be taken as an indicator that the estimated coefficient is not spurious.³¹

Considering first TANF recipients in 1997, we see that line 4 implies that the estimated

³¹ Our choice of the 1.2 threshold is somewhat arbitrary, reflecting our view that a difference in the relationship between measured and unmeasured factors greater than 20 percent may be viewed as implausible if one believes that these should be “similar,” as implied by the argument in Altonji et al.

effects could easily be spurious. The same conclusion holds if we consider prior market activity, as indicated in line 6. In contrast, the results reported in line 5 show that, for five of the seven industry categories, the unmeasured determinants would have to be related to industry in a very different fashion than is education for estimates to be completely spurious.

As we look across the different samples, we see that there are substantial differences in the value of the ratio, although there are also some regularities. For THS, considering the results in line 4 (based on all variables), we see that seven of the ten ratios are outside the range (0.0-1.2), implying that the estimated coefficients are not spurious. If we use education as the comparison measure (line 5), we count four outside that range, and if we examine prior market activity (line 6), there are six. Hence, in appreciably more than half of the cases, the unmeasured determinants of earnings would have to differ quite dramatically from the measured variables—in terms of their relationship with THS employment—for the estimated impact of THS employment to be spurious.

If we look at other industries, the categories of retail trade and the multiple industries category that includes THS also yield ratios that are usually outside the 0-1.2 range, suggesting a robust underlying impact. In contrast, the implied ratios for coefficients of the three other industry categories support the robustness of these coefficients in only about a third of the cases.

These results reflect the fact that in some cases, the measured variables that predict earnings are more strongly associated with industry differences than in other cases. The variation across tests presented here underscores the point that these tests are not definitive. Not only is there no certainty that unmeasured factors will be related to industry as are measured determinants, but it is clear that there is no “typical” measured determinant. Nonetheless, the

tests do allow us to reject the view that estimated coefficients can be fully explained by unmeasured factors that are similar to measured factors.

We estimated ratios corresponding to those in Table 9 using employment during the outcome quarter as the dependent variable. In this case, results are somewhat different. For every industry, the ratios were in the range 0-1.2 in at least two-thirds of the cases. For THS, in 80 percent of the tests, the ratio was in this range. Although individual industry results for specific samples differed depending on which ratio was considered, the overall pattern of results was the same. These results suggest that in the case of employment, it is much easier to argue that unmeasured factors may be responsible for inducing spurious coefficients estimates.

Differences in the results for earnings and employment may reflect difficulties in applying these diagnostic methods. Altonji et al. (2005) caution that where measured variables explain only a small portion of the variance in the dependent variable, making inferences about the structure of unmeasured factors is risky. In the case of earnings, measured variables explain between 28 and 57 percent of the variance in the stable portion of earnings, with most between 40 and 50 percent. This means that slightly more than half of the variation is due to unmeasured factors. In contrast, in the case of employment, in most cases, measured variables explain less than 25 percent of the variance. The test is therefore less likely to provide useful information in the latter case.

Overall, our results support the view that estimated effects of reference quarter industry—and especially THS employment—on outcome earnings are very likely at least partly causal. Although one cannot reject the possibility that unmeasured factors both induce individuals to take certain kinds of jobs and affect earnings, the particular structure of the

unmeasured correlates of industry would have to be quite different than measured factors in most cases to imply that estimated effects are zero. It appears implausible that all estimated effects of THS and other industries on earnings are spurious. Although findings are less supportive of the view that employment effects of reference quarter industry are causal, our ability to make inferences is limited by the structure of the test we are using.

IX. Conclusion

Perhaps the most notable finding of this study is that the basic patterns of THS effects are very similar across participants in three different programs and two points in time for both men and women. The female samples receiving employment exchange services and job training are very heterogeneous and substantially different from the TANF recipients, yet the role of temporary help employment is remarkably similar. While earnings for men are higher and estimated impacts are somewhat larger, again it is the similarities in results that are most striking.

In all of our samples, there is little question that, on average, those who can obtain manufacturing jobs or jobs in selected other industries during the reference quarter have higher ultimate earnings than those who obtain THS jobs. This earnings advantage is larger in a recessionary period. But for many of these individuals, job choices are undoubtedly very limited, and difficulties obtaining desirable jobs are particularly severe during economic downturns. We see no other jobs filling a similar transitional role to that of temporary help employment for individuals facing employment difficulties. For many individuals, temporary help employment may well be available when other kinds of jobs are not. The concerns that individuals who make the choice to take such a job will remain trapped in low wage and unstable jobs appear to be unfounded; we see no evidence that a strategy of waiting for a “better” job

yields any benefits at all.

In terms of the implications for workforce development policies, our results imply that both males and females, coming through the employment exchange, job training or TANF programs, fare better in terms of earnings and earnings growth when they take jobs with temporary help service firms if the alternative is no employment. If temporary help service firms facilitate quicker access to jobs for those seeking employment assistance, then encouraging the use of these labor market intermediaries to expand access to employment networks for individuals seeking jobs should generate net benefits. And even if temporary help jobs do supplant some jobs, since many of these jobs are in the retail trade and service sectors, the costs are small. Nonetheless, it is clear that, for most low-wage or disadvantaged workers, the key to labor market success via the path of a temporary help services firm is through a subsequent transition to a job in another sector. Those who do not move out of temporary help jobs face substantially poorer earnings prospects. If policymakers consider a greater role for temporary help services firms for those seeking employment assistance, tracking these firms' success in facilitating placements of workers into permanent jobs in other sectors is particularly important in evaluating and improving the effectiveness of such policies.

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Appendix: Implementation of Robustness Tests of Industry Impact Estimates

We reproduce equations (1) and (4) for convenience:

$$Y = D\alpha + X\gamma + \varepsilon + u \quad (1)$$

$$\phi_{\varepsilon k}^* \equiv \hat{\alpha}_k \frac{Var(\tilde{D}_k)}{Var(\varepsilon)} \quad (4)$$

The estimate $\hat{\alpha}_k$ is based on (1); however, the other terms in $\phi_{\varepsilon k}^*$ are based on the null hypothesis that this coefficient is zero and so are estimated in a regression corresponding to (1) but omitting D . $\phi_{X\gamma,k}$ is calculated using $\hat{\gamma}$ estimated in that same equation.

It is also necessary to identify ε , which is the component in earnings or employment that may be tied to individual characteristics or decisions made eight quarters earlier; which is in contrast to random variation in earnings due to variation in u . This is accomplished using earnings in adjacent quarters for a given individual. In particular, we rewrite the equation identifying the determinants of outcome earnings or employment to distinguish across quarters:³²

$$Y_t = X\gamma_t + [\pi^t \varepsilon + u_t], \quad (5)$$

where we assume an autoregressive error structure of the form $u_t = ru_{t-1} + v_t$, v_t is an independent error term, and π and r are parameters. The variable t indexes quarters, and we take the outcome quarter (which is eight quarters after the reference quarter) as $t=0$, so we have $\pi^t = 1$ at $t = 0$. At $t=0$, (5) is equivalent to (1) with the industry dummies omitted. The term ε identifies the stable component of the unmeasured determinants of Y , and the term π allows for it to grow or decline in importance.

³² Reference quarter industry is omitted, given the null hypothesis that industry has no causal impact.

The expression in brackets, $[\pi' \varepsilon + u_t]$, can be estimated as the residual of a regression of earnings on X in a given quarter t . The variances and covariances of the residuals for three successive quarters (the outcome quarter, and quarters immediately prior and subsequent to the outcome quarter) can then be used to estimate π , r , and $Var(\varepsilon)$.³³

The estimate of $\phi_{X\gamma,k}$ is obtained directly from the regression of the industry dummy for k in a regression limited to those in that industry and in no job during the reference quarter, i.e.,

$$D_k = \phi_{0k} + \phi_{X\gamma,k}(X\gamma) + v.$$

However, as Murphy and Topel (1990) note, it may be that we believe omitted determinants of income are more closely associated with certain observed measures than with others. We have grouped selected variables so that the relationship for each grouping can be considered. In particular, we estimate ϕ_{ik} in the equation

$$D_k = \phi_{0k} + \sum_i \phi_{ik} Z_i + v',$$

where $Z_i = \sum_{X_j \in G_i} \hat{\gamma}_j X_j$, $\hat{\gamma}_j$ is the estimated coefficient of X_j in the regression predicting the

outcome, and G_i is the set of variables in group i . The groups are constructed to include all

³³ Denoting $\tilde{Y}_t = [\pi' \varepsilon + u_t]$, the six equations defining the system are written as

$$Cov(\tilde{Y}_{-1}, \tilde{Y}_0) = \pi^{-1} Var(\varepsilon) + r Var(u_{-1})$$

$$Cov(\tilde{Y}_0, \tilde{Y}_1) = \pi Var(\varepsilon) + r Var(u_0)$$

$$Cov(\tilde{Y}_{-1}, \tilde{Y}_1) = Var(\varepsilon) + r^2 Var(u_{-1})$$

$$Var(\tilde{Y}_{-1}) = \pi^{-2} Var(\varepsilon) + Var(u_{-1})$$

$$Var(\tilde{Y}_0) = Var(\varepsilon) + Var(u_0)$$

$$Var(\tilde{Y}_1) = \pi^2 Var(\varepsilon) + Var(u_1)$$

These six equations can be solved for the six unknowns, π , r , $Var(\varepsilon)$, $Var(u_{-1})$, $Var(u_0)$, and $Var(u_1)$. Murphy and Topel use a related method to identify the stable and transient components of earnings.

variables in X . If we believe that unmeasured determinants of earnings or employment are similar to a particular set of variables, the value of ϕ_{ik} associated with that group may provide a better comparison to the error term than the full set of variables. In addition to reporting $\phi_{\varepsilon k}^* / \phi_{X\gamma, k}$ (based on all variables), we report the implied ratio $\phi_{\varepsilon k}^* / \phi_{ik}$ for education variables (years of education, high school degree, college degree) and for prior employment activities (five measures of work activity in the two years prior to program entry).

Table 1: Distribution of Employment Across Industries Prior and Subsequent to Program Entry in 1997

	Females						Males			
	TANF		Job Training		Employment Exchange		Job Training		Employment Exchange	
	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry
No job	47.49	52.68	36.60	40.75	36.26	33.39	33.36	34.74	35.25	32.05
Major industry group										
0 Agriculture, forestry, and fishing	0.68	0.35	1.11	0.83	1.16	0.59	1.52	0.83	1.88	1.50
1 Mining, construction	0.43	0.53	0.50	0.69	0.91	1.01	3.53	4.00	9.86	11.09
2, 3 Manufacturing	5.99	5.01	16.06	9.81	11.38	13.16	21.43	17.73	16.88	19.21
4 Transportation, communications, electric, gas, and sanitary services	1.50	1.42	3.15	2.95	2.72	3.09	6.87	9.31	5.00	5.56
5 Wholesale trade, retail trade	22.82	18.85	16.97	14.73	21.83	21.66	17.31	15.59	19.29	19.14
6 Finance, insurance, and real estate	1.59	1.51	3.65	4.79	3.60	3.97	1.68	1.72	1.52	1.58
7, 8 Services	28.79	28.66	29.29	35.28	29.01	35.58	23.22	29.76	16.81	22.15
9 Public administration	0.68	0.57	1.17	2.15	1.62	1.79	1.55	2.41	1.81	1.71
4-digit industry										
5810 Eating and drinking places	11.98	8.90	5.10	4.38	8.71	8.18	4.33	3.50	6.42	6.15
7363 THS	5.86	8.55	5.18	9.11	3.87	8.73	6.54	11.10	4.08	8.78
8051 Skilled nursing care facilities	5.32	4.51	3.17	3.67	3.04	3.33	0.96	0.79	0.68	0.69
8062 General medical and surgical hospitals	1.51	1.18	5.21	2.10	2.22	2.09	1.39	0.56	0.69	0.64

Note: Counts include any job, so individuals who hold more than one job are counted multiple times

Table 2: Multinomial Logit Estimation of Job Choice: Quarter Following Program Entry in 1997

	Females									Males					
	TANF			Job Training			Employment Exchange			Job Training			Employment Exchange		
	Job in THS	Job in THS and Other Industry	Job, but None in THS	Job in THS	Job in THS and Other Industry	Job, but None in THS	Job in THS	Job in THS and Other Industry	Job, but None in THS	Job in THS	Job in THS and Other Industry	Job, but None in THS	Job in THS	Job in THS and Other Industry	Job, but None in THS
Age	0.131 (0.030)	0.133 (0.036)	0.026 (0.013)	0.071 (0.049)	0.071 (0.052)	0.045 (0.021)	0.054 (0.009)	0.018 (0.009)	-0.013 (0.004)	0.108 (0.061)	0.057 (0.059)	0.080 (0.029)	0.053 (0.008)	0.039 (0.009)	-0.026 (0.004)
Age square *100	-0.203 (0.048)	-0.237 (0.058)	-0.065 (0.020)	-0.093 (0.061)	-0.103 (0.065)	-0.063 (0.026)	-0.092 (0.013)	-0.063 (0.013)	-0.008 (0.005)	-0.126 (0.076)	-0.083 (0.075)	-0.119 (0.035)	-0.089 (0.012)	-0.095 (0.012)	0.002 (0.005)
Years of education	0.063 (0.034)	0.031 (0.036)	0.055 (0.015)	0.226 (0.070)	0.067 (0.078)	0.038 (0.033)	0.084 (0.019)	0.117 (0.018)	0.041 (0.008)	-0.019 (0.078)	0.060 (0.076)	-0.143 (0.038)	0.022 (0.016)	0.019 (0.016)	0.014 (0.008)
High school degree	-0.022 (0.100)	0.015 (0.108)	-0.030 (0.045)	-0.553 (0.275)	-0.208 (0.290)	-0.259 (0.126)	0.084 (0.062)	0.149 (0.062)	0.206 (0.028)	-0.121 (0.313)	-0.312 (0.310)	0.456 (0.167)	0.035 (0.053)	0.204 (0.053)	0.371 (0.026)
College degree	-0.220 (0.315)	-0.278 (0.363)	-0.127 (0.145)	-0.577 (0.390)	-0.272 (0.443)	0.121 (0.189)	-0.131 (0.098)	-0.219 (0.092)	0.101 (0.044)	-0.171 (0.430)	-1.309 (0.454)	0.171 (0.190)	-0.063 (0.088)	-0.079 (0.087)	-0.093 (0.041)
Nonwhite	0.884 (0.082)	0.714 (0.087)	0.248 (0.037)	0.520 (0.181)	0.686 (0.198)	0.121 (0.088)	0.445 (0.038)	0.336 (0.038)	-0.103 (0.019)	0.994 (0.235)	1.082 (0.226)	0.225 (0.120)	0.501 (0.034)	0.386 (0.034)	-0.169 (0.017)
Proportion of previous 8 quarters working	1.224 (0.176)	1.545 (0.199)	1.130 (0.079)	1.817 (0.393)	2.373 (0.450)	0.968 (0.175)	1.033 (0.089)	1.383 (0.091)	1.152 (0.041)	1.419 (0.478)	0.839 (0.520)	0.888 (0.245)	1.275 (0.079)	1.719 (0.081)	1.187 (0.038)
Working all of previous 8 quarters	0.072 (0.104)	0.340 (0.108)	0.183 (0.051)	-0.308 (0.221)	-0.232 (0.229)	0.007 (0.105)	-0.045 (0.054)	0.134 (0.051)	0.307 (0.025)	-0.186 (0.280)	0.460 (0.282)	-0.059 (0.140)	0.260 (0.049)	0.402 (0.047)	0.424 (0.023)
No work in any of previous 8 quarters	-0.328 (0.134)	-0.038 (0.158)	-0.233 (0.054)	-0.232 (0.319)	0.328 (0.366)	-0.433 (0.125)	-0.396 (0.063)	-0.296 (0.069)	-0.451 (0.028)	-0.992 (0.412)	-0.399 (0.423)	-0.412 (0.183)	-0.481 (0.055)	-0.269 (0.059)	-0.388 (0.026)
Total annual earnings in the prior year /1000	0.029 (0.009)	0.046 (0.009)	0.051 (0.004)	0.007 (0.011)	0.008 (0.012)	0.005 (0.005)	-0.008 (0.003)	0.012 (0.003)	0.015 (0.001)	-0.025 (0.013)	0.007 (0.011)	0.017 (0.004)	-0.032 (0.003)	-0.015 (0.002)	0.020 (0.001)
Total annual earnings two years prior /1000	-0.041 (0.009)	-0.032 (0.009)	-0.035 (0.004)	-0.032 (0.013)	-0.023 (0.013)	0.001 (0.005)	-0.021 (0.003)	-0.027 (0.003)	-0.015 (0.001)	-0.027 (0.012)	-0.019 (0.011)	-0.006 (0.004)	-0.024 (0.003)	-0.035 (0.003)	-0.013 (0.001)

Table 2 -- Continued

	Females									Males					
	TANF			Job Training			Employment Exchange			Job Training			Employment Exchange		
	Job in THS		Job, but None in THS	Job in THS		Job, but None in THS	Job in THS		Job, but None in THS	Job in THS		Job, but None in THS	Job in THS and Industry		Job, but None in THS
	Job in THS	Other Industry	None in THS	Job in THS	Other Industry	None in THS	Job in THS	Other Industry	None in THS	Job in THS	Other Industry	None in THS	Job in THS	Other Industry	None in THS
St. Louis central	0.606 (0.104)	0.248 (0.110)	-0.014 (0.044)	1.476 (0.274)	0.780 (0.246)	0.195 (0.101)	0.711 (0.046)	0.574 (0.046)	-0.159 (0.021)	0.193 (0.289)	0.769 (0.307)	0.170 (0.128)	0.555 (0.041)	0.356 (0.042)	-0.040 (0.019)
Kansas City central	0.855 (0.107)	0.662 (0.111)	-0.074 (0.047)	1.637 (0.284)	0.795 (0.266)	0.035 (0.114)	0.764 (0.054)	0.742 (0.052)	-0.223 (0.026)	0.535 (0.332)	1.016 (0.343)	0.294 (0.161)	0.689 (0.049)	0.827 (0.047)	0.138 (0.024)
Suburban metro	0.684 (0.125)	0.431 (0.130)	-0.088 (0.050)	1.174 (0.287)	0.385 (0.269)	-0.011 (0.101)	0.679 (0.054)	0.755 (0.050)	-0.059 (0.023)	0.452 (0.324)	0.755 (0.337)	-0.107 (0.140)	0.696 (0.050)	0.855 (0.046)	0.250 (0.022)
Small metro	0.659 (0.124)	0.569 (0.125)	0.006 (0.048)	1.277 (0.299)	-0.037 (0.333)	-0.185 (0.113)	0.646 (0.055)	0.724 (0.051)	0.086 (0.023)	0.347 (0.371)	0.800 (0.371)	-0.267 (0.173)	0.719 (0.046)	0.893 (0.043)	0.133 (0.021)
Unemployment rate in county at current quarters	-0.905 (1.721)	-3.359 (1.976)	-3.188 (0.719)	-7.893 (4.133)	-3.065 (3.811)	-1.356 (1.501)	-4.664 (0.725)	-5.628 (0.744)	-2.822 (0.243)	0.341 (5.610)	-3.807 (5.648)	-0.191 (2.574)	-1.187 (0.597)	-2.017 (0.638)	-2.352 (0.247)

Note: Coefficients on the dummy variables for each of the four quarters and the constant are not reported. Coefficient standard errors are in parentheses.

Table 3: Predicted Earnings and Impact by Industry of Employment in Quarter Following Program Entry in 1997

	One Industry						Multiple Industries	
	No Job	THS	Manufac- turing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
<i>Panel A - Females</i>								
TANF								
1. Initial mean earnings	0 (0)	1,131 (35)	1,763 (52)	1,188 (19)	1,547 (22)	2,147 (60)	1,632 (38)	1,766 (42)
2. Mean earnings 8 quarters later	1,008 (14)	1,818 (58)	1,831 (75)	1,597 (31)	1,953 (34)	2,449 (81)	2,060 (67)	1,922 (57)
3. Mean earnings 8 quarters later controlling characteristics	1,164 (15)	1,585 (51)	1,848 (62)	1,556 (31)	1,747 (29)	2,051 (58)	1,737 (55)	1,730 (49)
4. Impact on earnings, relative to no job category	0 (0)	421 (54)	684 (64)	393 (35)	584 (33)	887 (61)	574 (57)	566 (52)
Job Training								
1. Initial mean earnings	0 (0)	1,529 (95)	2,748 (122)	1,727 (100)	2,968 (98)	3,315 (147)	2,494 (148)	2,849 (148)
2. Mean earnings 8 quarters later	1,941 (52)	2,838 (172)	2,968 (138)	2,657 (139)	3,464 (101)	3,300 (130)	3,140 (172)	3,352 (174)
3. Mean earnings 8 quarters later controlling characteristics	2,193 (55)	2,789 (159)	2,909 (137)	2,882 (123)	3,057 (80)	3,063 (116)	3,040 (166)	3,214 (140)
4. Impact on earnings, relative to no job category	0 (0)	596 (169)	716 (147)	689 (135)	864 (99)	870 (129)	847 (176)	1,021 (152)
Employment Exchange								
1. Initial mean earnings	0 (0)	1,745 (23)	3,748 (61)	1,877 (14)	2,639 (15)	3,724 (42)	2,616 (30)	3,056 (27)
2. Mean earnings 8 quarters later	1,252 (11)	2,515 (37)	3,535 (33)	2,215 (19)	2,956 (19)	3,821 (34)	3,112 (38)	3,250 (29)
3. Mean earnings 8 quarters later controlling characteristics	1,578 (13)	2,605 (35)	3,186 (28)	2,449 (21)	2,783 (17)	3,192 (26)	2,986 (33)	3,020 (25)
4. Impact on earnings, relative to no job category	0 (0)	1,027 (37)	1,608 (31)	872 (25)	1,205 (22)	1,614 (30)	1,408 (36)	1,443 (29)

Table 3 -- Continued

	One Industry						Multiple Industries	
	No Job	THS	Manufac- turing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
<i>Panel B - Males</i>								
Job Training								
1. Initial mean earnings	0 (0)	1,661 (158)	3,795 (172)	2,412 (233)	4,739 (230)	5,557 (516)	3,014 (190)	5,648 (676)
2. Mean earnings 8 quarters later	2,402 (113)	2,590 (249)	4,603 (202)	2,894 (226)	4,774 (269)	5,004 (243)	3,822 (302)	4,484 (260)
3. Mean earnings 8 quarters later controlling characteristics	2,574 (121)	3,458 (303)	4,386 (210)	3,490 (272)	4,340 (202)	4,526 (182)	4,216 (293)	4,283 (252)
4. Impact on earnings, relative to no job category	0 (0)	884 (329)	1,812 (243)	915 (301)	1,766 (238)	1,952 (222)	1,642 (322)	1,708 (284)
Employment Exchange								
1. Initial mean earnings	0 (0)	1,716 (25)	5,119 (33)	2,628 (24)	3,369 (30)	4,519 (31)	2,667 (25)	4,133 (51)
2. Mean earnings 8 quarters later	1,575 (14)	2,393 (37)	5,218 (35)	3,161 (29)	3,706 (35)	4,954 (29)	3,082 (39)	4,434 (38)
3. Mean earnings 8 quarters later controlling characteristics	2,147 (17)	3,061 (44)	4,463 (28)	3,400 (31)	3,646 (32)	4,227 (24)	3,507 (42)	4,133 (32)
4. Impact on earnings, relative to no job category	0 (0)	915 (47)	2,316 (34)	1,254 (36)	1,499 (36)	2,081 (29)	1,360 (45)	1,986 (37)

¹Excluding THS.

Note: Standard errors are in parentheses.

Table 4: Predicted Probability of Employment by Industry in Quarter Following Program Entry in 1997

	One Industry						Multiple Industries	
	No Job	THS	Manufac- turing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
Panel A - Females								
TANF								
1. Probability of employment 8 quarters later	0.455 (0.004)	0.682 (0.013)	0.650 (0.017)	0.679 (0.008)	0.706 (0.007)	0.725 (0.015)	0.744 (0.014)	0.727 (0.013)
2. Probability of employment 8 quarters later controlling characteristics	0.494 (0.004)	0.628 (0.014)	0.657 (0.017)	0.643 (0.008)	0.663 (0.008)	0.668 (0.016)	0.670 (0.015)	0.671 (0.013)
3. Impact on probability of employment, relative to no job category	0 (0)	0.134 (0.014)	0.163 (0.017)	0.149 (0.009)	0.169 (0.009)	0.174 (0.016)	0.176 (0.015)	0.177 (0.014)
Job Training								
1. Probability of employment 8 quarters later	0.575 (0.011)	0.753 (0.027)	0.761 (0.022)	0.731 (0.021)	0.767 (0.013)	0.741 (0.020)	0.819 (0.025)	0.807 (0.022)
2. Probability of employment 8 quarters later controlling characteristics	0.603 (0.010)	0.736 (0.028)	0.733 (0.024)	0.719 (0.022)	0.750 (0.014)	0.726 (0.020)	0.791 (0.029)	0.773 (0.025)
3. Impact on probability of employment, relative to no job category	0 (0)	0.132 (0.030)	0.130 (0.026)	0.115 (0.024)	0.147 (0.018)	0.122 (0.023)	0.188 (0.031)	0.170 (0.027)
Employment Exchange								
1. Probability of employment 8 quarters later	0.417 (0.003)	0.698 (0.006)	0.775 (0.004)	0.708 (0.004)	0.749 (0.003)	0.787 (0.004)	0.786 (0.006)	0.799 (0.004)
2. Probability of employment 8 quarters later controlling characteristics	0.473 (0.002)	0.694 (0.006)	0.738 (0.005)	0.692 (0.004)	0.727 (0.003)	0.742 (0.005)	0.755 (0.006)	0.754 (0.005)
3. Impact on probability of employment, relative to no job category	0 (0)	0.221 (0.007)	0.265 (0.006)	0.218 (0.005)	0.254 (0.004)	0.268 (0.005)	0.281 (0.006)	0.281 (0.005)
Panel B - Males								
Job Training								
1. Probability of employment 8 quarters later	0.470 (0.015)	0.646 (0.038)	0.790 (0.022)	0.706 (0.032)	0.711 (0.023)	0.763 (0.020)	0.806 (0.030)	0.783 (0.027)
2. Probability of employment 8 quarters later controlling characteristics	0.517 (0.014)	0.658 (0.035)	0.753 (0.025)	0.682 (0.032)	0.703 (0.024)	0.730 (0.021)	0.782 (0.034)	0.737 (0.029)
3. Impact on probability of employment, relative to no job category	0 (0)	0.142 (0.038)	0.236 (0.028)	0.166 (0.035)	0.187 (0.028)	0.213 (0.026)	0.266 (0.038)	0.221 (0.033)
Employment Exchange								
1. Probability of employment 8 quarters later	0.377 (0.002)	0.641 (0.006)	0.784 (0.003)	0.725 (0.004)	0.721 (0.004)	0.754 (0.003)	0.723 (0.006)	0.796 (0.004)
2. Probability of employment 8 quarters later controlling characteristics	0.451 (0.002)	0.660 (0.006)	0.736 (0.004)	0.693 (0.004)	0.703 (0.004)	0.707 (0.003)	0.709 (0.005)	0.740 (0.004)
3. Impact on probability of employment, relative to no job category	0 (0)	0.210 (0.006)	0.286 (0.004)	0.242 (0.005)	0.252 (0.005)	0.257 (0.004)	0.258 (0.006)	0.289 (0.005)

¹Excluding THS.

Note: Standard errors are in parentheses.

Table 5: Transition between Sectors Over Eight Quarters: Program Entry, 1997

Reference Quarter Employment		Employment Eight Quarters Later (Percent)						Total
		No Job	Service, including THS	Manufacturing	Retail trade	Other	Multiple sectors	
<i>Panel A - Females</i>								
TANF	No job	54.5	18.8	3.9	11.5	4.5	6.7	100.0
One Sector	THS	31.8	28.0	6.3	10.3	10.3	13.3	100.0
	Manufacturing	35.0	14.5	24.0	11.4	4.3	10.8	100.0
	Retail trade	32.1	18.0	4.0	28.7	5.3	12.0	100.0
	Service ¹	29.4	42.5	3.2	8.9	5.2	10.8	100.0
	Other	27.6	19.2	2.4	10.0	29.6	11.3	100.0
Multiple Sectors	THS and any other industry	25.6	30.1	4.3	10.1	10.5	19.4	100.0
	Any industry not THS	27.3	26.0	5.0	15.7	9.2	16.9	100.0
Job Training	No job	42.5	28.2	6.0	7.7	9.3	6.2	100.0
One Sector	THS	24.7	28.2	7.3	5.4	21.6	12.7	100.0
	Manufacturing	23.9	16.1	39.1	7.8	8.6	4.6	100.0
	Retail trade	26.9	22.6	2.7	28.8	10.7	8.2	100.0
	Service ¹	23.3	54.1	2.2	4.7	7.9	7.8	100.0
	Other	25.9	15.2	3.0	5.3	40.5	10.1	100.0
Multiple Sectors	THS and any other industry	18.1	35.3	6.9	7.3	13.4	19.0	100.0
	Any industry not THS	19.3	28.6	9.3	8.1	18.3	16.5	100.0
Employment Exchange	No job	58.3	17.7	4.0	8.5	6.9	4.5	100.0
One Sector	THS	30.2	27.6	8.9	8.0	14.0	11.3	100.0
	Manufacturing	22.5	9.6	49.6	5.3	5.8	7.3	100.0
	Retail trade	29.2	15.1	4.0	35.0	7.0	9.7	100.0
	Service ¹	25.1	51.5	2.8	5.9	6.4	8.4	100.0
	Other	21.3	13.6	3.3	6.1	47.0	8.6	100.0
Multiple Sectors	THS and any other industry	21.4	28.4	10.0	9.3	15.0	15.8	100.0
	Any industry not THS	20.1	24.9	9.5	13.4	15.7	16.5	100.0
<i>Panel B - Males</i>								
Job Training	No job	53.0	13.1	9.2	5.0	15.4	4.5	100.0
One Sector	THS	35.4	19.9	13.7	4.4	15.5	11.2	100.0
	Manufacturing	21.0	6.6	46.6	4.9	12.9	8.1	100.0
	Retail trade	29.4	18.6	5.4	24.0	10.3	12.3	100.0
	Service ¹	28.9	41.4	7.9	4.1	11.8	5.9	100.0
	Other	23.7	10.8	6.7	3.2	48.2	7.5	100.0
Multiple Sectors	THS and any other industry	19.4	25.1	15.4	4.0	18.3	17.7	100.0
	Any industry not THS	21.7	18.3	17.0	5.2	21.7	16.1	100.0
Employment Exchange	No job	62.3	9.2	6.0	5.9	12.7	3.9	100.0
One Sector	THS	35.9	20.4	13.2	6.8	12.8	10.9	100.0
	Manufacturing	21.6	4.6	53.3	3.3	10.3	6.9	100.0
	Retail trade	27.5	10.3	6.0	34.1	11.5	10.6	100.0
	Service ¹	27.9	39.9	5.4	6.1	11.4	9.3	100.0
	Other	24.6	5.6	5.5	3.6	54.7	6.1	100.0
Multiple Sectors	THS and any other industry	27.7	16.1	15.2	8.7	16.9	15.3	100.0
	Any industry not THS	20.4	13.7	14.6	11.0	23.3	17.1	100.0

¹Excluding THS.

Table 6: Predicted Earnings and Impact by Employment in Reference and Outcome Quarter: 1997

Impacts Relative to No Job	One Industry					Multiple Industries	
	THS	Manufacturing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
Panel A - Females							
TANF							
1. Impact of reference quarter industry, controlling outcome industry	123 (40)	204 (48)	97 (26)	187 (25)	280 (46)	184 (43)	124 (39)
2. Impact of outcome quarter industry, controlling reference quarter industry	1250 (46)	3054 (41)	1920 (26)	2371 (23)	3314 (36)	1928 (44)	2346 (36)
Job Training							
1. Impact of reference quarter industry, controlling outcome industry	76 (127)	45 (114)	456 (102)	319 (76)	269 (99)	242 (133)	355 (115)
2. Impact of outcome quarter industry, controlling reference quarter industry	2194 (157)	4278 (113)	2597 (101)	3705 (69)	4217 (88)	2712 (155)	3337 (122)
Employment Exchange							
1. Impact of reference quarter industry, controlling outcome industry	283 (29)	362 (25)	269 (20)	370 (17)	422 (24)	420 (28)	411 (22)
2. Impact of outcome quarter industry, controlling reference quarter industry	2011 (38)	4301 (24)	2686 (20)	3291 (16)	4187 (20)	2706 (37)	3520 (25)
Panel B - Males							
Job Training							
1. Impact of reference quarter industry, controlling outcome industry	328 (254)	378 (194)	356 (234)	842 (186)	614 (174)	430 (250)	522 (219)
2. Impact of outcome quarter industry, controlling reference quarter industry	3117 (290)	5957 (184)	4329 (246)	5170 (177)	6099 (161)	3471 (321)	5242 (259)
Employment Exchange							
1. Impact of reference quarter industry, controlling outcome industry	70 (36)	593 (27)	320 (28)	404 (29)	637 (23)	181 (35)	553 (29)
2. Impact of outcome quarter industry, controlling reference quarter industry	2451 (47)	5718 (25)	3855 (28)	4366 (27)	5390 (21)	3175 (47)	4692 (33)

¹Excluding THS.

Note: Standard errors are in parentheses.

Table 7: Distribution of Employment Across Industries Prior and Subsequent to Program Entry in 2001

	Females						Males			
	TANF		Job Training		Employment Exchange		Job Training		Employment Exchange	
	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry	4 Quarters before Entry	1 Quarter after Entry
No job	40.99	52.32	25.41	37.29	33.47	38.61	22.54	34.85	34.77	39.03
Major industry group										
0 Agriculture, forestry, and fishing	0.57	0.42	1.08	0.76	0.60	0.58	0.80	0.75	0.87	1.09
1 Mining, construction	0.67	0.44	0.98	0.82	1.24	1.13	3.57	3.62	6.52	7.48
2, 3 Manufacturing	6.03	2.93	23.20	9.63	11.55	10.14	34.90	19.14	19.00	17.23
4 Transportation, communications, electric, gas, and sanitary services	2.23	1.65	3.71	3.13	4.04	2.94	6.06	6.92	5.82	4.82
5 Wholesale trade, retail trade	24.46	18.98	19.33	17.19	21.79	20.23	18.21	17.94	19.84	18.36
6 Finance, insurance, and real estate	2.17	1.41	4.58	4.40	4.95	4.46	1.73	1.96	1.99	1.70
7, 8 Services	33.80	29.29	31.58	38.62	28.34	31.45	23.11	28.18	16.64	19.46
9 Public administration	0.75	0.55	1.12	1.65	1.79	1.71	1.37	1.96	1.63	1.60
4-digit industry										
5810 Eating and drinking places	11.67	8.88	5.60	5.32	7.35	6.95	5.17	4.76	5.76	5.60
7363 THS	7.23	6.81	7.49	9.12	3.11	5.94	8.58	11.34	3.19	6.39
8051 Skilled nursing care facilities	5.38	5.01	2.95	3.59	2.41	2.67	0.68	0.77	0.49	0.58

Table 8: Predicted Earnings and Impact by Industry of Employment in Quarter Following Program Entry in 2001

	One Industry						Multiple Industries	
	No Job	THS	Manufacturing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
Panel A - Females								
TANF								
1. Initial mean earnings	0 (0)	1,158 (43)	2,046 (79)	1,213 (18)	1,671 (23)	2,367 (68)	1,897 (55)	1,777 (54)
2. Mean earnings 8 quarters later	767 (13)	1,488 (67)	1,770 (103)	1,227 (28)	1,675 (30)	2,097 (81)	1,900 (80)	1,635 (63)
3. Mean earnings 8 quarters later controlling characteristics	878 (15)	1,331 (54)	1,681 (75)	1,238 (28)	1,523 (25)	1,764 (57)	1,642 (59)	1,492 (50)
4. Impact on earnings, relative to no job category	0 (0)	453 (56)	803 (77)	360 (32)	645 (30)	886 (59)	764 (61)	614 (52)
Job Training								
1. Initial mean earnings	0 (0)	1,711 (114)	3,918 (217)	1,722 (60)	2,361 (62)	3,444 (167)	2,523 (103)	3,110 (138)
2. Mean earnings 8 quarters later	1,914 (51)	2,593 (173)	3,622 (160)	2,048 (85)	2,709 (80)	3,679 (141)	2,928 (166)	3,084 (135)
3. Mean earnings 8 quarters later controlling characteristics	1,930 (52)	2,526 (152)	3,067 (126)	2,377 (102)	2,860 (68)	3,308 (111)	2,944 (143)	3,050 (118)
4. Impact on earnings, relative to no job category	0 (0)	596 (162)	1,137 (135)	447 (115)	930 (87)	1,379 (123)	1,014 (153)	1,120 (130)
Employment Exchange								
1. Initial mean earnings	0 (0)	2,246 (44)	4,558 (45)	2,153 (29)	3,277 (26)	4,615 (53)	3,105 (51)	3,844 (53)
2. Mean earnings 8 quarters later	1,408 (16)	2,818 (63)	4,056 (41)	2,326 (26)	3,384 (28)	4,321 (48)	3,327 (64)	3,567 (43)
3. Mean earnings 8 quarters later controlling characteristics	1,636 (17)	2,922 (57)	3,776 (41)	2,757 (30)	3,215 (24)	3,616 (34)	3,326 (57)	3,424 (37)
4. Impact on earnings, relative to no job category	0 (0)	1,286 (60)	2,140 (45)	1,121 (35)	1,579 (30)	1,980 (38)	1,690 (60)	1,788 (41)

Table 8 -- Continued

	One Industry						Multiple Industries	
	No Job	THS	Manufacturing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
<i>Panel B - Males</i>								
Job Training								
1. Initial mean earnings	0 (0)	1,734 (133)	5,026 (225)	2,558 (105)	2,848 (116)	3,683 (134)	2,469 (130)	4,044 (200)
2. Mean earnings 8 quarters later	2,416 (85)	2,080 (193)	4,866 (167)	2,804 (157)	2,971 (142)	4,080 (179)	2,920 (204)	3,953 (192)
3. Mean earnings 8 quarters later controlling characteristics	2,364 (82)	2,755 (207)	4,067 (139)	3,134 (169)	3,318 (142)	3,889 (133)	3,668 (202)	3,928 (174)
4. Impact on earnings, relative to no job category	0 (0)	392 (225)	1,703 (161)	771 (188)	955 (165)	1,525 (156)	1,305 (220)	1,564 (193)
Employment Exchange								
1. Initial mean earnings	0 (0)	2,062 (50)	5,965 (53)	3,213 (99)	4,372 (76)	5,605 (70)	3,052 (52)	4,893 (86)
2. Mean earnings 8 quarters later	1,808 (20)	2,458 (63)	5,333 (38)	3,300 (51)	4,335 (52)	4,928 (41)	3,011 (61)	4,495 (55)
3. Mean earnings 8 quarters later controlling characteristics	2,068 (22)	3,116 (73)	4,873 (40)	3,712 (44)	4,077 (44)	4,493 (34)	3,576 (72)	4,363 (50)
4. Impact on earnings, relative to no job category	0 (0)	1,049 (76)	2,805 (47)	1,644 (49)	2,010 (49)	2,425 (41)	1,509 (76)	2,296 (55)

¹Excluding THS.

Note: Standard errors are in parentheses.

Table 9: Robustness Tests for Estimated Impact of Industry on Earnings

Dependent Variable: Earnings 2 Years After Reference Quarter	Industry in Reference Quarter	One Industry					Multiple Industries	
		THS	Manufacturing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
		<i>Panel A - Females, Program Entry 1997</i>						
TANF								
1. Simple difference between industry and no job		810	823	589	945	1,440	1,052	914
2. Estimated industry impact, all measured factors controlled		421	685	393	584	888	574	566
3. (Standard error)		(54)	(64)	(35)	(33)	(61)	(57)	(52)
4. Implied ratio: All measured and unmeasured determinants		0.27	1.15	0.50	0.41	0.43	0.31	0.42
5. Implied ratio: Education and unmeasured determinants		1.21	-0.81	-2.60	0.93	0.66	22.99	1.45
6. Implied ratio: Prior market activity and unmeasure determinants		0.77	0.44	0.45	0.41	0.54	0.46	0.42
Job Training								
1. Simple difference between industry and no job		897	1,027	716	1,523	1,359	1,199	1,411
2. Estimated industry impact, all measured factors controlled		597	717	690	865	871	849	1,023
3. (Standard error)		(169)	(147)	(135)	(100)	(129)	(177)	(152)
4. Implied ratio: All measured and unmeasured determinants		0.54	0.73	3.06	0.42	0.54	0.66	0.82
5. Implied ratio: Education and unmeasured determinants		0.85	-0.34	-0.67	0.45	-7.50	-3.17	-3.42
6. Implied ratio: Prior market activity and unmeasure determinants		0.76	0.38	5.31	0.62	0.49	0.68	0.81
Employment Exchange								
1. Simple difference between industry and no job		1,263	2,283	963	1,704	2,569	1,860	1,998
2. Estimated industry impact, all measured factors controlled		1,027	1,608	872	1,205	1,614	1,408	1,443
3. (Standard error)		(37)	(31)	(25)	(22)	(30)	(36)	(29)
4. Implied ratio: All measured and unmeasured determinants		1.36	0.62	1.91	0.80	0.56	1.03	0.82
5. Implied ratio: Education and unmeasured determinants		1.63	-0.60	-1.13	0.60	0.71	1.49	1.37
6. Implied ratio: Prior market activity and unmeasure determinants		2.24	0.53	1.68	0.85	0.58	1.24	0.80
Panel B - Males, Program Entry 1997								
Job Training								
1. Simple difference between industry and no job		-247	1,768	57	1,938	2,168	985	1,647
2. Estimated industry impact, all measured factors controlled		429	1,170	96	1,369	1,628	1,224	1,118
3. (Standard error)		(816)	(602)	(744)	(589)	(548)	(797)	(703)
4. Implied ratio: All measured and unmeasured determinants		-0.28	0.49	2.03	0.58	0.87	-4.74	0.57
5. Implied ratio: Education and unmeasured determinants		-0.08	-0.15	-0.01	0.27	-0.47	-0.22	-0.74
6. Implied ratio: Prior market activity and unmeasure determinants		-0.99	0.27	-0.07	0.37	0.40	0.76	0.47
Employment Exchange								
1. Simple difference between industry and no job		818	3,643	1,586	2,131	3,379	1,507	2,859
2. Estimated industry impact, all measured factors controlled		915	2,317	1,254	1,499	2,081	1,360	1,986
3. (Standard error)		(47)	(34)	(36)	(36)	(29)	(45)	(37)
4. Implied ratio: All measured and unmeasured determinants		-55.08	0.59	1.09	0.86	0.52	1.99	0.79
5. Implied ratio: Education and unmeasured determinants		-1.60	-0.84	-59.07	0.26	4.68	-8.91	0.98
6. Implied ratio: Prior market activity and unmeasure determinants		3.72	0.62	1.08	0.91	0.57	1.77	0.83

Table 9 - Continued

Dependent Variable: Earnings 2 Years After Reference Quarter	Industry in Reference Quarter	One Industry					Multiple Industries	
		THS	Manufacturing	Retail Trade	Service ¹	Other	THS and Any Other Industry	Any Industry Not THS
		<i>Panel C - Females, Program Entry 2001</i>						
TANF								
1. Simple difference between industry and no job		721	1,003	460	908	1,331	1,133	868
2. Estimated industry impact, all measured factors controlled		453	804	360	645	887	765	615
3. (Standard error)		(56)	(77)	(32)	(30)	(59)	(61)	(52)
4. Implied ratio: All measured and unmeasured determinants		0.38	0.89	0.73	0.49	0.51	0.48	0.53
5. Implied ratio: Education and unmeasured determinants		0.80	-3.75	-137.96	1.26	0.95	2.39	-2.63
6. Implied ratio: Prior market activity and unmeasure determinants		0.46	0.50	0.63	0.47	0.50	0.47	0.41
Job Training								
1. Simple difference between industry and no job		679	1,708	134	794	1,765	1,014	1,170
2. Estimated industry impact, all measured factors controlled		597	1,139	447	931	1,381	1,016	1,122
3. (Standard error)		(162)	(135)	(115)	(87)	(123)	(153)	(130)
4. Implied ratio: All measured and unmeasured determinants		1.36	6.25	-0.46	-3.74	0.84	3.77	2.87
5. Implied ratio: Education and unmeasured determinants		0.27	-6.70	-0.35	1.24	0.52	1.81	0.79
6. Implied ratio: Prior market activity and unmeasure determinants		1.10	5.34	-2.49	5.53	1.37	1.44	1.37
Employment Exchange								
1. Simple difference between industry and no job		1,411	2,648	918	1,976	2,913	1,919	2,159
2. Estimated industry impact, all measured factors controlled		1,286	2,140	1,121	1,579	1,980	1,690	1,788
3. (Standard error)		(60)	(45)	(35)	(30)	(38)	(60)	(41)
4. Implied ratio: All measured and unmeasured determinants		3.21	0.73	-2.94	1.35	0.71	2.40	1.57
5. Implied ratio: Education and unmeasured determinants		1.04	-0.45	-1.57	0.38	0.60	1.04	0.83
6. Implied ratio: Prior market activity and unmeasure determinants		-48.84	0.72	-3.11	1.70	0.79	6.46	2.12
Job Training								
<i>Panel D - Males, Program Entry 2001</i>								
1. Simple difference between industry and no job		-336	2,451	389	555	1,664	505	1,537
2. Estimated industry impact, all measured factors controlled		390	1,705	771	955	1,527	1,307	1,567
3. (Standard error)		(225)	(161)	(189)	(166)	(157)	(221)	(194)
4. Implied ratio: All measured and unmeasured determinants		-0.32	0.83	-1.01	-1.27	3.43	-1.00	19.71
5. Implied ratio: Education and unmeasured determinants		0.33	-0.80	-4.05	0.45	2.30	-1.40	-5.79
6. Implied ratio: Prior market activity and unmeasure determinants		-1.79	1.07	-1.23	-11.71	2.62	-11.09	2.52
Employment Exchange								
1. Simple difference between industry and no job		649	3,524	1,491	2,527	3,119	1,203	2,687
2. Estimated industry impact, all measured factors controlled		1,049	2,806	1,644	2,010	2,425	1,509	2,296
3. (Standard error)		(76)	(47)	(49)	(49)	(41)	(76)	(55)
4. Implied ratio: All measured and unmeasured determinants		-1.51	0.67	-7.72	1.50	0.90	-4.61	1.66
5. Implied ratio: Education and unmeasured determinants		-0.72	-0.40	-1.10	0.26	-2.02	-0.69	1.41
6. Implied ratio: Prior market activity and unmeasure determinants		-2.71	1.10	15.03	1.64	0.99	-11.05	2.20

¹Excluding THS.

Note: Line 4 indicates the ratio between the coefficient of the error term and the coefficient for measured variables in a regression predicting industry during the reference quarter that would be necessary in order for the estimated coefficient in line 2 to be spurious. Line 5 provides the implied ratio where the measured variable is a composite variable identifying educational attainment, and line 6 a composite variable identifying prior employment activity.