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The Role of Temporary Help Employment in Low-Wage Worker Advancement

Carolyn J. Heinrich, Peter R. Mueser, and Kenneth R. Troske

12.1 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

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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: (a) 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 (b) 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 sought job search assistance from employment exchange services funded under federal Wagner-Peyser legislation.

We draw on clients in this program in order to identify a diverse sample of individuals who are facing employment difficulties or who are entering the labor force.[1] Many clients of employment exchange services are facing an important juncture in their work lives or careers, as they are explicitly seeking services to support employment efforts. Our analysis allows us to consider the role that temporary employment and other industries play at such critical points in determining future labor market outcomes. This study does not consider the effects of employment exchange services.

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 first seek employment exchange services, and we consider whether this pattern can be observed for any other industries. Next

1. An earlier version of this study considered participants in Missouri's job training program and in the Temporary Assistance for Needy Families program (Heinrich, Mueser, and Troske 2007). The results for participants in these programs are very similar to those presented here, suggesting that the findings are not an artifact of the particular experience of employment exchange service recipients.

we look at employment during the quarter following initial participation, examining how employment and wages two years later are influenced by the sector of employment, and, in particular, temporary help services. We limit the sample to those eighteen to sixty-four years of age and conduct analyses separately for men and women. We report analyses initially for those who obtain employment exchange services during calendar year 1997 and then consider analyses for an analogous sample in 2001. Our use of large and long panels of state-level administrative data 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 who access employment exchange services. 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 12.3 we discuss our data and in section 12.4 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 12.5 presents estimates of the impact of temporary help employment on later earnings and employment, and section 12.6 considers the role that movements between jobs has in helping individuals achieve higher earnings and stable employment. In section 12.7, we consider the degree to which results are replicated for a similar sample of employment exchange participants in 2001 (a time when economic growth had slowed). Section 12.8 focuses on the experience of nonwhites in temporary help jobs. Section 12.9 turns to the issue of how robust our results are if the ordinary least squares (OLS) assumption of an independent error is violated. The final section concludes.

12.2 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, Mueser, and Troske 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"[2] 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 to 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 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 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, Holzer, and Lane (2002) and observe that low-wage workers

2. 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.

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 (chapter 11, this volume) 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.[3] Booth, Francesconi, and Frank (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. Kvasnikca'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-skill 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 questions about the role of labor market intermediaries as a solution to the problem of low-wage worker advancement (Poppe,

3. 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 services sector in the United States.

Strawn, and Martinson 2003). A recent study by Even and Macpherson (2003, 677) 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." 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 effects 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.

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 undertaking employment exchange activities. 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 employment exchange service. A 2001 survey of public assistance recipients who had engaged in temporary help services employment in North Carolina found that most (77 percent) did not learn about these jobs through program counselors, but rather through other channels, including word of mouth, newspaper ads, or by contacting the firm directly (Heinrich 2005).

12.3 Data

Employment exchange files from Missouri identify individuals in the state who register for services provided under federal Wagner-Peyser legislation.[4] Most individuals who receive Unemployment Insurance (UI) payments are required to register for these services, and a substantial portion of employment exchange registrants are UI recipients.[5] However, anyone in the state

4. Greater detail on the characteristics of individuals in our sample, including tabulations comparing employment exchange participants with participants in other programs, can be found in Heinrich, Mueser, and Troske (2007).

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.

is eligible to use these services, so registrants include employed individuals seeking better employment prospects as well as other job-seekers who are not receiving unemployment compensation. Information on program participation and demographic information about participants comes from data maintained by the state of Missouri to administer these programs.

Our basic sample consists of individuals who participated in the employment exchange service during 1997 or 2001 and who did not receive job exchange services in the prior quarter. An individual who obtained services during the first six months of the year, received no services for at least one quarter, and then again obtained services in a later quarter, will be included twice in the file for a given year. The number of such cases is very small.

Our data on earnings, employment history, and the industrial classification of the job come from the Unemployment Insurance (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 employment exchange 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 UI 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 the THS 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 program we are considering.[7]

Table 12.1 provides means and standard deviations for our samples of males and females receiving employment exchange services in 1997 and 2001. The samples are large and provide a substantial array of demographic measures, as well as prior labor market experience. The table also provides statistics about industry of employment in the quarter subsequent to program participation. We see that THS makes up 6 to 9 percent of employment at this point, but that, eight quarters later, THS is less important.

6. The largest concentration of Missouri residents holding jobs outside the state are those in Jackson County, Missouri, the central county for Kansas City, who cross the border to work 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 (2006) report that 7 percent of the employees in temporary help firms in Germany are permanent administrative staff.

Table 12.1 **Means and standard deviations for employment exchange participants, 1997 and 2001**

	1997				2001			
	Females		Males		Females		Males	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	34.2	11.0	33.7	10.9	35.9	12.1	35.9	12.0
Age squared	1,293.9	825.0	1,255.1	812.9	1,433.4	923.2	1,432.1	921.5
Number of years of education	12.31	1.64	12.30	1.67	12.45	1.45	12.36	1.46
High school degree[a]	0.871	0.336	0.870	0.337	0.934	0.249	0.925	0.263
College degree	0.077	0.267	0.077	0.266	0.081	0.273	0.075	0.264
Nonwhite	0.267	0.442	0.234	0.424	0.238	0.426	0.225	0.418
Proportion of previous eight quarters working	0.628	0.395	0.632	0.395	0.700	0.376	0.690	0.381
Working all of previous eight qtrs	0.391	0.488	0.396	0.489	0.474	0.499	0.462	0.499
No work in any of previous eight qtrs	0.180	0.385	0.180	0.385	0.141	0.348	0.149	0.356
Total annual earnings in the prior year	8,944	10,319	12,747	15,349	13,792	15,203	19,302	29,751
Total annual earnings two years prior	7,847	10,197	11,355	15,330	12,489	14,730	17,663	23,801
St. Louis County and St. Louis City	0.218	0.413	0.205	0.404	0.205	0.403	0.188	0.391
Kansas City central area (Jackson County)	0.101	0.301	0.105	0.306	0.114	0.318	0.124	0.329
Suburban areas	0.126	0.332	0.141	0.348	0.172	0.377	0.170	0.375
Small metro	0.125	0.331	0.134	0.340	0.131	0.337	0.136	0.343
Outside metro	0.430	0.495	0.416	0.493	0.378	0.485	0.383	0.486
Earnings in quarter following program entry	1,845	3,002	2,591	3,903	2,213	3,354	3,041	6,541
Earnings eight quarters after reference quarter	2,430	2,841	3,353	4,035	2,691	3,321	3,476	4,568
Employment eight quarters after reference quarter	0.642	0.479	0.626	0.484	0.625	0.484	0.600	0.490

Industry in reference quarter								
No job	0.334	0.471	0.325	0.468	0.358	0.479	0.360	0.480
THS	0.044	0.205	0.046	0.208	0.031	0.173	0.032	0.175
Manufacturing	0.086	0.281	0.131	0.337	0.086	0.280	0.145	0.352
Retail trade	0.127	0.333	0.098	0.297	0.125	0.330	0.100	0.300
Service (excluding THS)	0.192	0.394	0.089	0.285	0.196	0.397	0.096	0.294
Other	0.086	0.280	0.182	0.386	0.100	0.300	0.168	0.374
Multiple sectors: THS and any other	0.048	0.214	0.048	0.215	0.031	0.173	0.032	0.176
Multiple sectors: Any not THS	0.083	0.276	0.081	0.273	0.074	0.262	0.067	0.250
Industry eight quarters after reference quarter								
No job	0.358	0.479	0.374	0.484	0.375	0.484	0.400	0.490
THS	0.023	0.151	0.024	0.154	0.014	0.119	0.016	0.126
Manufacturing	0.086	0.281	0.135	0.342	0.077	0.267	0.135	0.341
Retail trade	0.113	0.317	0.085	0.279	0.112	0.315	0.092	0.290
Service (excluding THS)	0.220	0.414	0.096	0.294	0.223	0.416	0.104	0.305
Other	0.116	0.320	0.209	0.406	0.139	0.346	0.200	0.400
Multiple sectors: THS and any other	0.024	0.154	0.024	0.154	0.011	0.103	0.013	0.113
Multiple sectors: Any not THS	0.059	0.236	0.054	0.225	0.048	0.215	0.041	0.198
Number of observations	114,375		135,911		79,042		94,466	

Note: SD = Standard deviation.

[a]The high school degree dummy is coded 1 for those with at least a high school degree.

In the next three sections, we focus exclusively on individuals who obtain job exchange services in 1997. In section 12.7 we compare the experiences of entrants in 2001 with those who enter in 1997.

12.4 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 12.2 provides a comparison of the industry of employment four quarters prior to program entry and in the quarter subsequent to entry. The first row in the table shows the proportion of people without jobs. We see that employment increases in the first quarter after contact with the employment exchange system, presumably reflecting the fact that some individuals are seeking employment following a period out of the labor force. The

Table 12.2 **Distribution of employment across industries prior and subsequent to program entry in 1997**

	Females		Males	
	Four quarters before entry	One quarter after entry	Four quarters before entry	One quarter after entry
No job	36.26	33.39	35.25	32.05
Major industry group				
0 Agriculture, forestry, and fishing	1.16	0.59	1.88	1.50
1 Mining, construction	0.91	1.01	9.86	11.09
2, 3 Manufacturing	11.38	13.16	16.88	19.21
4 Transportation, communications, electric, gas, and sanitary services	2.72	3.09	5.00	5.56
5 Wholesale trade, retail trade	21.83	21.66	19.29	19.14
6 Finance, insurance, and real estate	3.60	3.97	1.52	1.58
7, 8 Services	29.01	35.58	16.81	22.15
9 Public administration	1.62	1.79	1.81	1.71
Four-digit industry				
5810 Eating and drinking places	8.71	8.18	6.42	6.15
7363 THS	3.87	8.73	4.08	8.78

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

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 lower panel provides the figures for THS and for eating and drinking establishments, the largest four-digit industry category in this sample.

The role that temporary help jobs play in this structure can be seen in the figures of THS and the comparison with other industries. Of the major industry groups, services display the largest increase, and this growth is largely due to growth in THS employment. The proportion of individuals in THS jobs more than doubles, increasing by nearly 5 percentage points, following contact with the job exchange system, reaching a level of about 9 percent. 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. Both in terms of absolute size and proportional increase, THS is unique among industries that we can identify.

Table 12.3 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.[8] In 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.[9] Nonetheless, for many of the variables, coefficients for the three employment categories are similar, so that substantive differences in the determinants are small.

Those who are older are less likely to be working, but the relationship between age and employment is nonlinear, as indicated by the coefficient on the squared term that is negative and statistically significant.[10] 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

8. 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.

9. 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 eight tests.

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

Table 12.3 Multinomial logit estimation of job choice: Quarter following program entry in 1997

	Females			Males		
	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	**0.054**	**0.018**	**–0.013**	**0.053**	**0.039**	**–0.026**
	(0.009)	(0.009)	(0.004)	(0.008)	(0.009)	(0.004)
Age squared · 100	**–0.092**	**–0.063**	–0.008	**–0.089**	**–0.095**	0.002
	(0.013)	(0.013)	(0.005)	(0.012)	(0.012)	(0.005)
Years of education	0.084	**0.117**	**0.041**	0.022	0.019	0.014
	(0.019)	(0.018)	(0.008)	(0.016)	(0.016)	(0.008)
High school degree	0.084	**0.149**	**0.206**	0.035	**0.204**	**0.371**
	(0.062)	(0.062)	(0.028)	(0.053)	(0.053)	(0.026)
College degree	–0.131	**–0.219**	**0.101**	–0.063	–0.079	**–0.093**
	(0.098)	(0.092)	(0.044)	(0.088)	(0.087)	(0.041)
Nonwhite	**0.445**	**0.336**	**–0.103**	**0.501**	**0.386**	**–0.169**
	(0.038)	(0.038)	(0.019)	(0.034)	(0.034)	(0.017)
Proportion of previous eight quarters working	**1.033**	**1.383**	**1.152**	**1.275**	**1.719**	**1.187**
	(0.089)	(0.091)	(0.041)	(0.079)	(0.081)	(0.038)
Working all of previous eight quarters	–0.045	**0.134**	**0.307**	**0.260**	**0.402**	**0.424**
	(0.054)	(0.051)	(0.025)	(0.049)	(0.047)	(0.023)
No work in any of previous eight quarters	**–0.396**	**–0.296**	**–0.451**	**–0.481**	**–0.269**	**–0.388**
	(0.063)	(0.069)	(0.028)	(0.055)	(0.059)	(0.026)
Total annual earnings in the prior year/1,000	**–0.008**	**0.012**	**0.015**	**–0.032**	**–0.015**	**0.020**
	(0.003)	(0.003)	(0.001)	(0.003)	(0.002)	(0.001)
Total annual earnings two years prior/1,000	**–0.021**	**–0.027**	**–0.015**	**–0.024**	**–0.035**	**–0.013**
	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
St. Louis central	**0.711**	**0.574**	**–0.159**	**0.555**	**0.356**	**–0.040**
	(0.046)	(0.046)	(0.021)	(0.041)	(0.042)	(0.019)
Kansas City central	**0.764**	**0.742**	**–0.223**	**0.689**	**0.827**	**0.138**
	(0.054)	(0.052)	(0.026)	(0.049)	(0.047)	(0.024)
Suburban metro	**0.679**	**0.755**	**–0.059**	**0.696**	**0.855**	**0.250**
	(0.054)	(0.050)	(0.023)	(0.050)	(0.046)	(0.022)
Small metro	**0.646**	**0.724**	**0.086**	**0.719**	**0.893**	**0.133**
	(0.055)	(0.051)	(0.023)	(0.046)	(0.043)	(0.021)
Unemployment rate in county at current quarter	**–4.664**	**–5.628**	**–2.822**	**–1.187**	**–2.017**	**–2.352**
	(0.725)	(0.744)	(0.243)	(0.597)	(0.638)	(0.247)

Notes: Coefficients for the dummy variables for each of the four quarters and the constant are not reported. Coefficient standard errors are in parentheses. Statistically significant estimates are in boldface.

increases in levels of employment, and in those samples where older individuals are 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;

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. Those who have no observed employment during the prior eight quarters are particularly unlikely to hold a job in the reference quarter. Prior earnings are related to employment in a complex way. The coefficients for earnings in the two prior years are in several cases negative. The coefficient in the immediately prior year is algebraically larger, implying that, controlling for the overall earnings level, growth in earnings 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. Overall, prior earnings are less positively associated with temporary help work than with other employment, and those with higher prior earnings are *less* likely to be employed in temporary help than to be not employed at all. Note that this is after employment is controlled, so this implies that employed individuals with low incomes are likely to be in THS employment.

The coefficients for county unemployment rate confirm that those in depressed counties are less likely to be employed, and among women, 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.[11] This is particularly notable, since the relationship between other employment and race is generally small and inconsistent across our samples. Andersson, Holzer, and Lane (2002) and Heinrich, Mueser, and Troske (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.

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

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 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 12.8 that follows, we return to the question of how the nonwhite experience may differ from that of whites in our sample.

12.5 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 12.3. 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.[12] Based on the same model, we also perform a difference-in-difference analysis, where the dependent variable is the difference in earnings between the outcome quarter and the quarter nine quarters prior to program entry.[13]

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 receive the employment exchange services. Insofar as selection of such individuals per se is important

12. The measure of prior industry is based on industry of employment in all four quarters prior to program entry. Each industry dummy is coded 1 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.

13. 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).

in determining outcomes, our design controls for this selection. Nonetheless, prior employment experiences must be controlled, as we expect them to be related to job entry following program participation.[14] The difference-in-difference analysis allows us to control for stable differences across individuals that may lead them to take different kinds of jobs.

12.5.1 Estimated Effects on Earnings

Table 12.4 reports predicted quarterly earnings in the eighth quarter after the reference quarter based on linear regression equations as described previously, using the mean values of variables for the female and male samples.[15] 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.[16] Focusing first on 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 difference is 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 closer 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.

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.[17]

14. 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.

15. For details of model specification and coefficient estimates, see Heinrich, Mueser, and Troske (2007).

16. 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 as $(\overline{\mathbf{X}}_i - \overline{\mathbf{X}}_j)\mathbf{B}$, where $\overline{\mathbf{X}}_i$ and $\overline{\mathbf{X}}_j$ are vectors of means for the industries and $\mathbf{B}$ is a vector of coefficients indicating variable effects.

17. 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.

Table 12.4 **Predicted earnings and impact by industry of employment in quarter following program entry in 1997 and impact eight quarters later**

	One industry						Multiple industries	
	No job	THS	Manufacturing	Retail trade	Service[a]	Other	THS and any other industry	Any industry not THS
			Panel A—Females					
1. Initial mean earnings	0	1,745	3,748	1,877	2,639	3,724	2,616	3,056
	(0)	(23)	(61)	(14)	(15)	(42)	(30)	(27)
2. Mean earnings eight quarters later	1,252	2,515	3,535	2,215	2,956	3,821	3,112	3,250
	(11)	(37)	(33)	(19)	(19)	(34)	(38)	(29)
3. Mean earnings eight quarters later controlling characteristics	1,578	2,605	3,186	2,449	2,783	3,192	2,986	3,020
	(13)	(35)	(28)	(21)	(17)	(26)	(33)	(25)
4. Impact on earnings, relative to no job category	0	**1,027**	**1,608**	**872**	**1,205**	**1,614**	**1,408**	**1,443**
	(0)	(37)	(31)	(25)	(22)	(30)	(36)	(29)
5. Difference-in-difference estimate of impact on earnings, relative to no job category	0	**1,333**	**1,853**	**943**	**1,306**	**1,486**	**1,424**	**1,569**
	(0)	(109)	(91)	(73)	(64)	(87)	(105)	(84)
			Panel B—Males					
1. Initial mean earnings	0	1,716	5,119	2,628	3,369	4,519	2,667	4,133
	(0)	(25)	(33)	(24)	(30)	(31)	(25)	(51)
2. Mean earnings eight quarters later	1,575	2,393	5,218	3,161	3,706	4,954	3,082	4,434
	(14)	(37)	(35)	(29)	(35)	(29)	(39)	(38)
3. Mean earnings eight quarters later controlling characteristics	2,147	3,061	4,463	3,400	3,646	4,227	3,507	4,133
	(17)	(44)	(28)	(31)	(32)	(24)	(42)	(32)
4. Impact on earnings, relative to no job category	0	**915**	**2,316**	**1,254**	**1,499**	**2,081**	**1,360**	**1,986**
	(0)	(47)	(34)	(36)	(36)	(29)	(45)	(37)
5. Difference-in-difference estimate of impact on earnings, relative to no job category	0	**1,300**	**2,507**	**1,376**	**1,684**	**2,046**	**1,670**	**2,147**
	(0)	(113)	(81)	(86)	(88)	(71)	(110)	(90)

Note: Standard errors are in parentheses. Statistically significant impact estimates (shown in lines 4 and 5) are in boldface.

[a]Excluding THS.

Line 3 shows the impact of controls. Temporary help services workers are disadvantaged relative to other workers, so the relative benefits of having a manufacturing job are explained in part by observable differences among people.[18]

The largest categories of employment for females are retail trade and service, and 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 they hold a THS job—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. Parallel (and very similar) estimates based on the difference-in-difference model are presented in line 5. 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 borders on statistical significance.[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 only slightly below the average for other industries. Outcomes for those with any employment in the reference quarter are appreciably better than for those who do not 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 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]

Those with any employment have appreciably higher earnings than those without jobs, but 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

18. Our earlier analysis (Heinrich, Mueser, and Troske 2007) shows that when we consider participants in TANF or job training programs, those who take THS work are not necessarily disadvantaged relative to others.

19. The direct estimate is statistically significant and the difference-in-difference estimate is not statistically significant.

20. Up 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.

similar individuals with temporary help jobs. If we aggregate all industries outside of THS, the increment is 31 percent. 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."

12.5.2 Estimated Effects on Employment

We also estimated a linear probability model in which the dependent variable is employment eight quarters after the reference quarter. Control variables are identical to those used in the previous analysis. Table 12.5 provides measures of the impact on probability of employment (line 1) and the difference-in-difference estimate of the impact (line 2) relative to no job eight quarters later, based on sector of employment in the reference quarter.

The patterns of results parallel those for earnings fairly closely. The 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. Although those in temporary help jobs are somewhat less likely to work in the outcome quarter than those in most other categories, 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. Those who combine jobs in more than one industry during the reference quarter generally have higher rates of later employment than other categories. As in the case of earnings, substantive conclusions for the difference-in-difference analyses are similar, although the impact of reference quarter employment is approximately half as large in the difference-in-difference estimates.

12.6 Transitions between Sectors

The pattern previously described, 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 12.6 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 industry 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 any service position. For women, some 28 percent of THS employees are in service positions (including THS) eight quarters later, whereas 52 percent of other service workers are in some kind of service

Table 12.5 **Predicted probability of employment by industry in quarter following program entry in 1997**

Impacts relative to no job	One industry					Multiple industries	
	THS	Manufacturing	Retail trade	Service[a]	Other	THS and any other industry	Any industry not THS
			Panel A—Females				
1. Impact on probability of employment	**0.221**	**0.265**	**0.218**	**0.254**	**0.268**	**0.281**	**0.281**
	(0.007)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)	(0.005)
2. Difference-in-difference estimate of impact on probability of employment	**0.092**	**0.141**	**0.103**	**0.122**	**0.145**	**0.138**	**0.150**
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)
			Panel B—Males				
1. Impact on probability of employment	**0.210**	**0.286**	**0.242**	**0.252**	**0.257**	**0.258**	**0.289**
	(0.006)	(0.004)	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)
2. Difference-in-difference estimate of impact on probability of employment	**0.087**	**0.156**	**0.121**	**0.124**	**0.140**	**0.124**	**0.158**
	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)

Note: Standard errors in parentheses. Statistically significant impact estimates are in boldface.

[a]Excluding THS.

Table 12.6 Transition between sectors over eight quarters: Program entry in 1997

Reference quarter employment	Employment eight quarters later (%)					
	No job	Service, including THS	Manufacturing	Retail trade	Other; multiple sectors	Total
			Panel A—Females			
No job	58.3	17.7	4.0	8.5	11.5	100.0
THS	30.2	27.6	8.9	8.0	25.3	100.0
Manufacturing	22.5	9.6	49.6	5.3	13.1	100.0
Retail trade	29.2	15.1	4.0	35.0	16.7	100.0
Service excluding THS	25.1	51.5	2.8	5.9	14.7	100.0
Other; multiple sectors	20.9	21.2	7.2	9.6	41.2	100.0
			Panel B—Males			
No job	62.3	9.2	6.0	5.9	16.6	100.0
THS	35.9	20.4	13.2	6.8	23.7	100.0
Manufacturing	21.6	4.6	53.3	3.3	17.3	100.0
Retail trade	27.5	10.3	6.0	34.1	22.1	100.0
Service excluding THS	27.9	39.9	5.4	6.1	20.7	100.0
Other; multiple sectors	24.0	9.3	9.3	6.3	51.0	100.0

position. 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. For example, for women in THS positions in the reference quarter, 8.9 percent are in manufacturing eight quarters later.[21] For those in retail trade, service or other industries, no more than 4 percent move to the manufacturing sector eight quarters later. Cross tabulations for males display the same patterns.

The importance of moves between industries is illustrated in table 12.7. 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 no longer important for predicting earnings. Among women, those with temporary help jobs are predicted to have earnings in the outcome quarter that are $1,027 higher than those with no jobs (line 4 of table 12.4); once industry in the outcome quarter is controlled, that increment declines to $283 (line 1 of table 12.7). Similarly, ultimate earnings are expected to be $581 higher for those with manufacturing jobs than for temporary help jobs, a difference in impacts that declines to $79 when ultimate industry is controlled. This basic pattern is the same for

21. Moves by THS workers to manufacturing may partly reflect reclassification of temporary help workers to permanent status within a firm. See footnote 17.

Table 12.7 **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[a]	Other	THS and any other industry	Any industry not THS
			Panel A—Females				
1. Impact of reference quarter industry, controlling outcome industry	**283**	**362**	**269**	**370**	**422**	**420**	**411**
	(29)	(25)	(20)	(17)	(24)	(28)	(22)
2. Impact of outcome quarter industry, controlling reference quarter industry	**2,011**	**4,301**	**2,686**	**3,291**	**4,187**	**2,706**	**3,520**
	(38)	(24)	(20)	(16)	(20)	(37)	(25)
			Panel B—Males				
1. Impact of reference quarter industry, controlling outcome industry	70	**593**	**320**	**404**	**637**	**181**	**553**
	(36)	(27)	(28)	(29)	(23)	(35)	(29)
2. Impact of outcome quarter industry, controlling reference quarter industry	**2,451**	**5,718**	**3,855**	**4,366**	**5,390**	**3,175**	**4,692**
	(47)	(25)	(28)	(27)	(21)	(47)	(33)

Note: Standard errors in parentheses. Statistically significant impact estimates are in boldface.

[a]Excluding THS.

males; 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. Those who ultimately end up in temporary help jobs have the lowest earnings of any industry category, and the difference is substantial. This contrasts with estimates in table 12.4, which show that a temporary help job in the reference quarter provides ultimate earnings that are comparable to those of several other industry 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 12.4) but have higher earnings if they stay in retail trade than temporary help workers who stay in temporary help (line 2 of table 12.7).

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

Analyses to this point 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 obtained employment exchange services and started target quarter jobs, and it had declined further, to around 3 percent (eight quarters later), when we consider their employment outcomes. Over the three years 1997 to 1999, employment in Missouri grew by 4.4 percent.[22] 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 employment exchange services 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.[23] 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 to 1999 and 2001 to 2003 was substantial.

The employment exchange system 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. By 2001, most job exchange services

22. Employment growth was measured for January 1997 to January 2000.
23. Employment growth for January 2001 to January 2004.

were provided in one-stop centers offering a variety of job-related services (including job training under the Workforce Investment Act), 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.

Comparing table 12.8 with table 12.2, 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 employment exchange participation. We replicated our analysis, predicting industry of employment in the quarter following program entry. The similarities in the patterns of the coefficients were striking, with relative minor differences. Employment was more strongly associated with education—but not necessarily high school graduation—in 2001 than in 1997. The selection of nonwhites into THS employment was somewhat weaker in 2001, and THS employment was 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 was clearly true in 2001.

Table 12.9 provides estimates based on program participants in 2001 of

Table 12.8 **Distribution of employment across industries prior and subsequent to program entry in 2001**

	Females		Males	
	Four quarters before entry	One quarter after entry	Four quarters before entry	One quarter after entry
No job	33.47	38.61	34.77	39.03
Major industry group				
0 Agriculture, forestry, and fishing	0.60	0.58	0.87	1.09
1 Mining, construction	1.24	1.13	6.52	7.48
2, 3 Manufacturing	11.55	10.14	19.00	17.23
4 Transportation, communications, electric, gas, and sanitary services	4.04	2.94	5.82	4.82
5 Wholesale trade, retail trade	21.79	20.23	19.84	18.36
6 Finance, insurance, and real estate	4.95	4.46	1.99	1.70
7, 8 Services	28.34	31.45	16.64	19.46
9 Public administration	1.79	1.71	1.63	1.60
Four-digit industry				
5810 Eating and drinking places	7.35	6.95	5.76	5.60
7363 THS	3.11	5.94	3.19	6.39

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

Table 12.9 **Predicted earnings and impact by industry of employment in quarter following program entry in 2001 and impact eight quarters later**

Impacts relative to no job	One industry					Multiple industries	
	THS	Manufacturing	Retail trade	Service[a]	Other	THS and any other industry	Any industry not THS
			Panel A—Females				
1. Impact on earnings	**1,286**	**2,140**	**1,121**	**1,579**	**1,980**	**1,690**	**1,788**
	(60)	(45)	(35)	(30)	(38)	(60)	(41)
2. Difference-in-difference estimate of impact on earnings	**1,339**	**2,097**	**1,270**	**1,723**	**1,947**	**1,847**	**1,899**
	(79)	(59)	(46)	(39)	(50)	(79)	(54)
			Panel B—Males				
1. Impact on earnings	**1,049**	**2,805**	**1,644**	**2,010**	**2,425**	**1,509**	**2,296**
	(76)	(47)	(49)	(49)	(41)	(76)	(55)
2. Difference-in-difference estimate of impact on earnings	**1,458**	**2,635**	**1,825**	**2,118**	**2,351**	**1,815**	**2,302**
	(117)	(72)	(76)	(76)	(63)	(117)	(85)

Note: Standard errors are in parentheses. Statistically significant impact estimates are in boldface.

[a]Excluding THS.

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 participants. Yet there are a number of statistically significant differences. For females, earnings are initially higher in 2001, but they are also higher in the outcome quarter. For males, initial earnings are higher in 2001 than in 1997, but outcome earnings differences 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], lines 1 and 2), the difference between the estimated effects for 1997 and 2001 is quite modest—and is not statistically significant in three out of four comparisons between these years.

Relative to other employment, the impact of THS employment is estimated to be slightly less beneficial in the later period. For example, for males in 1997, the benefit of having an initial THS job relative to no job was $915 (line 4, table 12.4). The additional increment of having a manufacturing job was $1,401. In 2001, the comparable benefit for a THS job was similar at $1,049 (panel B, line 1), but the additional increment 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 consistent. Based on the two estimation approaches (lines 4 and 5 of table 12.4 and lines 1 and 2 of table 12.9), if we consider the four alterative industries, we have sixteen comparisons of the increment of an industry relative to THS. In eleven 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 compare THS with an aggregated category of other industries.

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 12.5 for 1997.[24] Our findings for employment are similar to those for earnings. As is the case with earnings, for women the benefit of having a temporary help job relative to having no job remains unchanged, whereas the incremental benefit of other kinds of jobs has increased in 2001. In contrast, for men, 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.

We performed analyses for program enrollees in 2001, looking at the transitions between sectors over the eight quarters following the reference

24. For details, see Heinrich, Mueser, and Troske (2007).

quarter and the relative importance of initial industry and ultimate industry in determining earnings. 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 critically depend on economic growth.

12.8 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. 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 *non*transitional 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 previously in table 12.3. 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 their overrepresentation in THS jobs. On the other hand, 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. Table 12.10 reports how estimates limited to the nonwhite sample differ from those for the full sample; thus, the numbers shown in table 12.10 are not the impact estimates (as in prior tables), but the difference in impacts

Table 12.10 **Difference in impact of industry of employment in quarter following participation: Nonwhites versus full sample**

Impacts relative to no job	One industry					Multiple industries	
	THS	Manufacturing	Retail trade	Service[a]	Other	THS and any other industry	Any industry not THS
			Panel A—Females, 1997				
1. Impact on earnings	**–201**	64	**–194**	**–162**	**–188**	**–261**	–60
	–20%	4%	**–22%**	**–13%**	**–12%**	**–19%**	–4%
2. Difference-in-difference estimate of impact on earnings	**–431**	**–382**	**–192**	**–220**	**–165**	**–266**	**–255**
	–32%	**–21%**	**–20%**	**–17%**	**–11%**	**–19%**	**–16%**
			Panel B—Males, 1997				
1. Impact on earnings	37	–187	**–349**	**–309**	**–417**	–157	–397
	4%	–8%	**–28%**	**–21%**	**–20%**	–12%	–20%
2. Difference-in-difference estimate of impact on earnings	**–243**	**–544**	**–361**	**–356**	**–421**	**–376**	**–479**
	–19%	**–22%**	**–26%**	**–21%**	**–21%**	**–23%**	**–22%**
			Panel C—Females, 2001				
1. Impact on earnings	–77	**220**	**–224**	**–194**	–120	**–324**	–150
	–6%	**10%**	**–20%**	**–12%**	–6%	**–19%**	–8%
2. Difference-in-difference estimate of impact on earnings	–126	162	**–242**	**–222**	–138	**–414**	–220
	–9%	8%	**–19%**	**–13%**	–7%	**–22%**	–12%
			Panel D—Males, 2001				
1. Impact on earnings	**–275**	36	**–448**	**–396**	**–367**	**–259**	**–218**
	–26%	1%	**–27%**	**–20%**	**–15%**	**–17%**	**–9%**
2. Difference-in-difference estimate of impact on earnings	**–380**	–216	**–628**	**–416**	**–402**	**–454**	**–357**
	–26%	–8%	**–34%**	**–20%**	**–17%**	**–25%**	**–15%**

Note: Significant differences are in boldface. Percentages use the full sample estimate as the base.

on earnings between nonwhites and the full sample for 1997 and 2001 and by gender.

Table 12.10 shows that effects for nonwhites are generally smaller than the full sample. Differences in estimated impacts are in the range of a few hundred dollars, with most between 15 and 25 percent. We are interested in whether there is any evidence that nonwhites may benefit more from THS employment, relative to other employment. This would reflect in smaller differences for the THS category than for the other industries. In fact, whether we look at percentages or absolute differences, we are unable to see any clear patterns of such differences between THS estimates and those for other industries.

When we look at mobility tables over the two years following employment exchange participation, we do find 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 12.6). 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 table 12.7) 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.

12.9 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 previous analysis controls for a variety of measures reflecting preprogram 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 Temporary Assistance for Needy Families (TANF) recipients in Missouri and North Carolina (Heinrich, Mueser, and Troske 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.

12.9.1 Formal Structure[25]

Consider our estimation equation

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

where Y is the outcome measure (quarterly earnings or employment), $\mathbf{D}$ is a vector of dummy variables identifying industry of employment in the reference quarter with no job the omitted category, $\mathbf{X}$ 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. Vectors of coefficients α and γ have been estimated by OLS under the assumption that $(\varepsilon + u)$ is uncorrelated with $\mathbf{D}$ or $\mathbf{X}$. The methods presented here are designed to help in considering whether the correlation between $\mathbf{D}$ and ε may cause the estimated coefficients $\hat{\alpha}$ to be spurious.

We separately consider each of the seven industry categories that are used to identify employment during the reference quarter and focus on individuals in each industry category, comparing them with individuals with no jobs. For simplicity, our analysis assumes that there are no interaction effects between $\mathbf{D}$ and $\mathbf{X}$ in predicting earnings or employment. Consider now the relationship between the dummy identifying employment in a particular industry k and the other factors predicting the outcome variable—that is, $\mathbf{X}\gamma$ and ε. Focusing on the sample limited to those with no job ($\mathbf{D}_0 = 1$) or those with a job in industry k ($\mathbf{D}_k = 1$), we specify $\mathbf{D}_k^*$ as the linear projection of $\mathbf{D}_k$ onto $\mathbf{X}\gamma$ and ε,

$$\mathbf{D}_k^* = \phi_{0k} + \phi_{X\gamma,k}(\mathbf{X}\gamma) + \phi_{\varepsilon k}\varepsilon. \tag{2}$$

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

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

25. For details of this approach, see Altonji, Elder, and Taber (2005), from which the following discussion is largely drawn.

where $\tilde{\mathbf{D}}_k$ is the industry dummy purged of its correlation with $\mathbf{X}$.[26] 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, Elder, and Taber (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

$$\phi^*_{\varepsilon k} \equiv \hat{\alpha}_k \frac{Var(\tilde{\mathbf{D}}_k)}{Var(\varepsilon)}, \tag{4}$$

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 $\hat{\alpha}_k$. When $\phi^*_{\varepsilon k}/\phi_{X\gamma,k} < 0$, the unbiased estimate of α_k will be greater in absolute value than $\hat{\alpha}_k$; that is, the bias is away from zero for any $\rho > 0$.[27]

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 1, 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 1. 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.[28] The details of the implementation of this test are provided in the appendix.

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

27. Estimating the exact size of the bias conditional on ρ is somewhat involved; see Altonji, Elder, and Taber (2005) for details.

28. Of course, in the absence of an independent measure of ρ, we have essentially no information on the true coefficient value.

12.9.2 Results

Table 12.11 provides diagnostics relevant to estimated effects of industry in the quarter following employment exchange participation on earnings eight quarters later, which are reported in table 12.4 (1997) and table 12.9 (2001). We focus on estimates of the impact relative to the no-employment category. The estimates in line 4 of table 12.4 and line 1 of table 12.9 are reproduced in line 2 of table 12.11; standard errors are reported in line 3. Line 1 of table 12.11 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 of table 12.11 indicates how controls affect the estimates. 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 $\phi^{*}_{\varepsilon k}/\phi_{X\gamma,k}$, where $\phi_{X\gamma,k}$ is based on all variables taken together, as specified in the formal structure presented earlier. 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.[29]

Looking across the calculated ratios in table 12.11, we see that there are substantial differences, although there are also some regularities. For THS, considering the results in line 4 (based on all variables), we see that all four ratios are outside the range (0.0 to 1.2), implying that the estimated coefficients are *not* spurious. If we use education as the comparison measure (line 5), we count three of four ratios outside that range, and if we examine prior market activity (line 6), all four are outside the range. In almost all cases, our tests therefore suggest that 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, retail trade and the multiple industries categories also yield ratios that are usually outside the 0 to 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.

29. 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, Elder, and Taber (2005).

Table 12.11 **Robustness tests for estimated impact of industry on earnings: Employment exchange participants**

Dependent variable Earnings two years after reference quarter Industry in reference quarter	One industry					Multiple industries	
	THS	Manufacturing	Retail trade	Service[a]	Other	THS and any other industry	Any industry not THS
	Panel A—Females, 1997						
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 to unmeasured determinants	1.36	0.62	1.91	0.80	0.56	1.03	0.82
5. Implied ratio: education to unmeasured determinants	1.63	–0.60	–1.13	0.60	0.71	1.49	1.37
6. Implied ratio: prior market activity to unmeasure determinants	2.24	0.53	1.68	0.85	0.58	1.24	0.80
	Panel B—Males, 1997						
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 to unmeasured determinants	–55.08	0.59	1.09	0.86	0.52	1.99	0.79
5. Implied ratio: education to unmeasured determinants	–1.60	–0.84	–59.07	0.26	4.68	–8.91	0.98
6. Implied ratio: prior market activity to unmeasure determinants	3.72	0.62	1.08	0.91	0.57	1.77	0.83

Panel C—Females, 2001							
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 to unmeasured determinants	3.21	0.73	–2.94	1.35	0.71	2.40	1.57
5. Implied ratio: education to unmeasured determinants	1.04	–0.45	–1.57	0.38	0.60	1.04	0.83
6. Implied ratio: prior market activity to unmeasure determinants	–48.84	0.72	–3.11	1.70	0.79	6.46	2.12
Panel D—Males, 2001							
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 to unmeasured determinants	–1.51	0.67	–7.72	1.50	0.90	–4.61	1.66
5. Implied ratio: education to unmeasured determinants	–0.72	–0.40	–1.10	0.26	–2.02	–0.69	1.41
6. Implied ratio: prior market activity to unmeasure determinants	–2.71	1.10	15.03	1.64	0.99	–11.05	2.20

Notes: 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.

[a]Excluding THS.

The variation in these results reflects 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.[30]

Overall, our results support the view that estimated effects of reference-quarter industry 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.

12.10 Conclusion

Perhaps the most notable finding of this study is that the basic patterns of THS effects are very similar for women and for men and for individuals seeking employment in an economic boom (1997 to 1999) and in a period of relative stagnation (2001 to 2003).

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

30. We also estimated ratios corresponding to those in table 12.11, 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 to 1.2 in most of the cases. For THS, in more than two-thirds 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 coefficient estimates. Altonji, Elder, and Taber (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, in most cases, nearly half of the variation is explained by measured factors, whereas for employment, the proportion of variance explained is less than 30 percent. The test is therefore less likely to provide useful information in the latter case.

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, 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 may be important in evaluating and improving the effectiveness of such policies.

Appendix

Implementation of Robustness Tests of Industry Impact Estimates

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

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

$$\phi^{*}_{\varepsilon k} \equiv \hat{\alpha}_k \frac{Var(\tilde{\mathbf{D}}_k)}{Var(\varepsilon)} \tag{4}$$

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**. Thus, $\phi_{X\gamma,k}$ 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:[31]

$$Y_t = \mathbf{X}\gamma_t + [\pi^t\varepsilon + u_t], \tag{A1}$$

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 $t = 0$, (A1) 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.

The expression in brackets, $[\pi^t\varepsilon + u_t]$, can be estimated as the residual of a regression of earnings on $\mathbf{X}$ 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 $Var(\varepsilon)$.[32]

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—that is,

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

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

$$\mathbf{D}_k = \phi_{0k} + \sum_i \phi_{ik}Z_i + v',$$

where $Z_i = \sum_{\mathbf{X}j \in Gi} \hat{\gamma}_j X_j$, $\hat{\gamma}_j$ 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

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

32. Denoting $\tilde{Y}_t \equiv [\pi^t\varepsilon + u_t]$, the six equations defining the system are written as

$$\begin{aligned}
Cov(\tilde{Y}_{-1}, \tilde{Y}_0) &= \pi^{-1}Var(\varepsilon) + rVar(u_{-1}) \\
Cov(\tilde{Y}_0, \tilde{Y}_1) &= \pi Var(\varepsilon) + rVar(u_0) \\
Cov(\tilde{Y}_{-1}, \tilde{Y}_1) &= Var(\varepsilon) + r^2Var(u_{-1}) \\
Var(\tilde{Y}_{-1}) &= \pi^{-2}Var(\varepsilon) + Var(u_{-1}) \\
Var(\tilde{Y}_0) &= Var(\varepsilon) + Var(u_0) \\
Var(\tilde{Y}_1) &= \pi^2Var(\varepsilon) + Var(u_1).
\end{aligned}$$

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 (1990) use a related method to identify the stable and transient components of earnings.

are constructed to include all 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).

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