THREE ESSAYS EVALUATING NEW JERSEY'S INDIVIDUAL TRAINING GRANT PROGRAM

by

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ABSTRACT OF THE DISSERTATION

Three Essays Evaluating New Jersey's Individual Training Grant Program

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This thesis evaluates the impact of New Jersey's Individual Training Grant (ITG) program on participants. . Through non-experimental matching methods, we find ITG participants experience a higher reemployment rate than their comparison group in the 8th, 12th, and 16th quarters after claiming Unemployment Insurance (UI). The reemployment advantage in the 8th quarter is about 6% and 5% in the 16th quarter. The wage recovery of the ITG group is statistically indistinguishable from the comparison group's wage recovery in the 16th quarter. However, the combined reemployment and wage return for ITG participants amounts to $474 in the 8th quarter after claiming UI (approximately 9.5% of 8th quarter wages). Applying this economic return, the lifetime monetary returns to training exceed the cost in foregone wages by the 5th year after claiming UI.

The thesis also estimates impacts for demographic groups that face a variety of barriers to employment, such as weak education and job skills, and access to networks. The specific groups are high school drop outs, females pursuing training in the male dominated fields of computer programming and engineering, and older workers who may have out dated skills or face age discrimination. Female enrolled in engineering or computer programming experience reemployment rates that are lower than or similar to those in the comparison group, but they do experience a $758 greater quarterly wage recovery in the 8th quarter after claiming UI. Hispanic high school dropouts experience both higher reemployment and wage recovery rates than their comparison group, but the wage recovery advantage disappears after removing those enrolled in truck driving training.

High school dropouts previously employed in manufacturing and white males age 51 to 65 experience a reemployment advantage in the 8th quarter after UI relative to seven comparison groups, each obtained by a different matching model. For both ITG subgroups the reemployment rate is 7-8 percentage points higher than their comparison group. However, there is no significant difference between the wage recovery rates of either ITG group and their comparison groups. Using multiple matching methods we demonstrate that these results are robust to the matching model. We find that both propensity score matching and stratified random sampling can be sensitive to ties, which illustrates the importance of using multiple matching methods.

Dedication

I dedicate this work to my family.

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I am very fortunate to have had a two-year hiatus to conduct the research for this thesis. It would not have been possible without Sergey Pankov's generous support. I am ever thankful for his patience, curiosity, and questioning.

Finally, I would be remiss if I did not recognize all those who have lost their jobs involuntary. This thesis relies on data from their experiences to answer questions that are intended to improve how government addresses the problem of involuntary job loss.

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Chapter 1
Introduction

I. Overview

Structural and cyclical unemployment are an inherent part of a capitalist economy. Wage loss associated with unemployment in the United States has been documented to range between 13% and 20% during the 1980s, 1990s, and 2000s (Farber 2005) (Farber 1997) (Jacobson, Lalonde, Sullivan, 1993). The federal Unemployment Insurance (UI) system and worker retraining programs are intended to serve as a social safety net that insulates workers from economic insecurity resulting from job loss. Individual states have also sponsored their own retraining programs.

In 1992, the New Jersey State Legislature created the Individual Training Grant (ITG) program to assist workers in obtaining skills needed for new jobs. The ITG program provides unemployed workers with vouchers of up to $4,000 to pay for training at proprietary training schools and community colleges.

The three essays in this thesis evaluate whether training offered through the ITG program increases the odds of reemployment and mitigates the wage loss experienced after job loss. Unlike the previous evaluations of the ITG program, this study uses multiple matching methods including exact-matching, propensity score matching, and Abadie-Imbens (2004) bias-adjusted matching, as well as a more extensive set of matching variables than previous studies (Whittaker, 2002) (Van Horn et al., 2000) (Benus et al., 1996). Applying multiple methods provides a measure of how sensitive impact results are to the matching method. Recent studies illustrate that applying different evaluation methods to the same data yield unexpectedly different results

(Dehejia, 2005) (Smith and Todd, 2005). As suggested by research, using a richer set of variables for matching improves matching. (Diaz and Handa, 2006) (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998). This study also, for the first time, assesses the estimated economic benefits in relation to the costs.

II. Policy Context of the Individual Training Grant Program

Government policies directed at regulating the labor market date back to the early 1900s. In response to the Great Depression in the 1930s, the U.S. Congress passed the Social Security Act of 1935. The act created the Unemployment Insurance (UI) program, which still exists today. It provides temporary income support to the unemployed and serves as an economic stabilizer by maintaining workers' purchasing power during economic downturns. Opponents of the act argued that it violated individual state's rights by forcing states to create an unemployment compensation fund by taxing employers. In 1937, the U.S. Supreme Court (in *Steward Machine Company vs. Davis*) ruled that the act did not violate states' rights given the severity of the Great Depression. The court's ruling helped cement government's role in providing an insurance safety net for those loosing their job as a result of the business cycle or industrial shifts.

In addition to temporary income assistance, the government also created training programs to address the labor market failures associated with structural unemployment. Structural unemployment occurs when jobs are eliminated due to permanent shifts in the production process. For example, in the 1980s steel plants and car plants closed in the U.S. and relocated to other countries where the labor costs were cheaper. In the short term, structural shifts result in unemployment and ease over time as workers switch to jobs in other industries. Often workers have to learn new skills before switching jobs. In the U.S. the burden of obtaining these skills typically falls to individuals because firms are reluctant to offer skill training that is easily transferable to other employers. In contrast, firms are much more willing to offer job-specific training because such skills are not transferable to other firms (Becker, 1962). Consequently there is typically an under-investment in general training for adult workers.[1] In response to this under-investment and to help the structurally unemployed find new jobs, in the 1950s, the U.S. government began providing training to the unemployed. The first major federal legislation to focus on workforce training for the unemployed was the Manpower Development and Training Act of 1962. Over the next two decades, Congress passed the Comprehensive Employment and Training Act of 1974 and the Job Training Partnership Act (JTPA) of 1982. Both directed federal resources toward training for unemployed and disadvantaged adults.

The Workforce Investment Act (WIA) of 1998 was designed to address concerns about the JTPA's contract-based system by replacing it with a voucher system. Under the JTPA contract model, state administrators would select a handful of training providers through a bidding process. The government then reserved a number of training slots at these training providers, and those seeking training chose from these slots. Several concerns about this training framework emerged over time. First, customer or trainee choices were restricted to only those schools with contracted slots. Second, the contracting process itself was not always competitive.

[1]One notable exception is Germany, where firms offer employees general training. Institutional factors, such as unions and employer associations, create an environment were a large number of employers offer apprentices general training. In the 1990s, 60% of the workforce had participated in an apprenticeship (Harhoff and Kane, 1997) (Soskice, 1994).

WIA broke from the contract model by creating training vouchers called Individual Training Accounts (ITA). ITA vouchers are intended to give individuals a wider choice of training options. Eligible unemployed workers select training courses using a Consumer Report Card (CRC). The CRC is intended to help ITA holders find the training providers that best suit their needs, and includes course descriptions, costs, pre-requisites, public transportation information, and sometimes employment rates of recent graduates. The ITA system was designed to broaden the training choices for customers and increase the quality of training.

The ITA voucher system is based on the principle of competition. The underlying rationale presumes that vouchers will encourage providers with low-quality services to improve in order to compete with others for voucher customers. Evidence on the implementation of the ITA program in eight states indicates that the number of training providers increased, and some training providers increased their recruitment efforts after WIA replaced JTPA.[2] Under JTPA, once a contract was established, providers did not need to engage in recruiting efforts to attract students. In contrast, under WIA, providers have developed marketing strategies to attract students such as making presentations and leaving pamphlets at state employment centers, and encouraging employment counselors to make referrals to their institution (Berkeley Policy Associates, 2003). The WIA system was reauthorized in the Workforce Investment Act Amendments of 2005.

III. The Individual Training Grant Program

New Jersey's Individual Training Grant (ITG) program, also a voucher program, was established six years before the ITA program was created. In 1992, New Jersey's state legislature created the program to help the unemployed find jobs faster and mitigate their wage losses.[3] The program was one of the first to combine a voucher format with job search information that identifies high-demand occupations. The New Jersey Department of labor defines high-demand occupations as those where the expected number of job openings exceeds the number of graduates (from New Jersey colleges) that are estimated to be qualified for the job openings. Currently the United States House of Representatives is considering revising the ITA program to also be directed toward high-demand occupations.

New Jersey's ITG program is designed for unemployed dislocated workers. Dislocated workers are generally those who are eligible for UI and are unlikely to return to work in their previous industry or occupation, or who lost their jobs because of a permanent plant closure, or their position was abolished.

Additionally, job counselors weigh a series of other factors to determine whether an individual is eligible for the program. Factors taken into consideration include current skill level, previous occupation, previous industry, and current demand for the type of occupation sought. Figure 1 illustrates the general client flow from initial UI claim to entering training. After being laid off, individuals apply for Unemployment Insurance (UI) benefits. Then after being deemed eligible, UI recipients are required to attend a mandatory orientation where they learn about all the services available to them including Individual Training Grant vouchers. Those interested in the ITG program can sign up for an orientation workshop. After attending the training orientation, interested people set up

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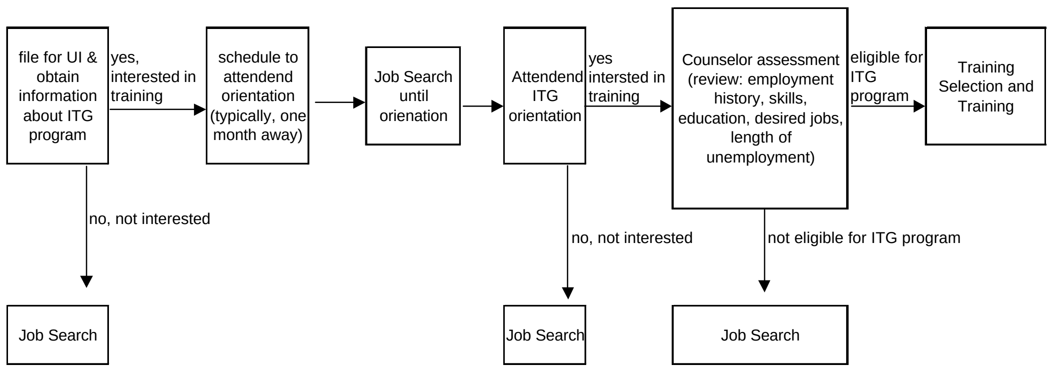
[2] Florida, Massachusetts, Nevada, New Jersey, Oregon, Pennsylvania, Texas, and Wisconsin

[3] New Jersey Public Law-L.1992,c.43,s1, established the Workforce Development Partnership Program (WDPP), which includes the ITG program.

appointments with job counselors to determine whether they are eligible for a training voucher. In some cases, job counselors may refer individuals directly to training.

Illustration 1.1 ITG Program Eligibility Process



Source: Author's rendering

Candidates deemed eligible for ITG vouchers must choose training providers from the state's Eligible Training Provider List (ETPL). In 1999 the list had over 200 schools, as indicated by the number of schools that accepted an ITG voucher that year. Counselors sometimes assist participants in researching school options, but the final decision is left to the participant. Using the taxonomy created in a recent study of the federal voucher program (Perez-Johnson, et al., 2004), the ITG program best fits the guided customer choice typology: participants are encouraged to think through their training options with the job counselor, but the final decision is theirs.

As noted earlier, another criterion specifies that training should be related to a demand occupation, which the act defines as "an occupation for which there is likely to be an excess demand for adequately trained workers ."[4] The state's county-level demand occupation list identifies occupational-training eligible for funding. This demand occupation list combines the New Jersey Department of Labor's county-level occupational demand projections with data on New Jersey college graduates, resulting in an estimate of the shortages in occupational and training areas. Local areas also have the ability to petition the state to have occupations added to the list when the locality can demonstrate the occupation is in high-demand.

The maximum allowable amount for a training grant is $4,000, though on occasion people may be awarded two grants. The average training grant during the study period, 1995-1999, was $3,864. The median amount was $3,995. The average amount of a grant varies with training type. Grants for Transportation-related training amounted to $2,971 on average, and grants for Health-related training were $4,143 on average. For comparison, 9 credit hours at a New Jersey Community College cost $1,183 in 2006. Approximately 75% of participants obtained training at proprietary training schools, such as Devry or the Chubb Institute. Another 19% obtained their training at Community Colleges, and the remaining obtained training at adult-education or vocational institutes.

The ITG program served approximately 17,000 unemployed workers in the period examined in this study, 1995-1999.[5] In 1995, New Jersey's civilian labor force amounted to approximately 4.1 million workers, and close to 275,000 workers claimed UI in New Jersey. In a typical year, ITG participants are 1 to 2% of New Jersey UI claimants. The program is funded through a portion of the UI payroll tax. In 1993, 0.025% of each worker's Unemployment Insurance (UI) taxable wages were allocated for the programs. Spending on ITG grants amounted to $107 million for approximately 30,000 individuals between 1995 and 2001 (Van Horn et al., 2002).

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[4]Section 34:15D-3 of New Jersey Public Law-L.1992,c.43,s1.
[5] All statistics are from New Jersey's Bureau of Labor Force Statistics, Seasonally Adjusted Estimates 1976-2000. Accessed May 17, 2006
http://www.wnjpin.net/OneStopCareerCenter/LaborMarketInformation/lmi11/njsa.xls

IV. Evaluating the Individual Training Grant Program

Governments face the general question of whether the spending of tax revenues on programs such as the ITG, ITA, or JTPA is an effective use of resources. To assess the impact of programs such as the ITA or ITG program, one must compare the post-program outcomes to what the outcomes would have been in the absence of the program. The preferred method for measuring the impact of a program is a random assignment experiment, where eligible participants are randomly assigned to participation or non-participation groups. This method is often costly and difficult to implement. As an alternative, this study employs non-experimental methods to estimate the counterfactual of what happens to reemployment and wage recovery in the absence of program participation.

Two previous studies of the ITG voucher program also used non-experimental methods to estimate program impact. The Benus et al. (1996) study, which measured the impact of the program from 1992-1994, found mixed results. In their regression on quarterly wage in the 4th quarter after UI claim, they found a negative participation coefficient of -$2,252 (s.e. 332) for ITG participants receiving Additional Benefits during Training (ABT) and a positive coefficient of $869 (s.e.220) for ITG participation only. They selected their comparison group from UI claimants by first matching on date of claim. Then they used stratified random sampling to match on gender, race, and education. To control for unobservable differences, they applied a Residual From Trend (RFT) regression model.[6] The study did not examine impact on reemployment.

[6] The model is similar to a difference-in-difference model, but it does not constrain the individual specific effects to be constant over time. It allows them to vary by including as independent variables the intercept and slope of the pre-program earnings trend regression. In addition to the slope coefficient, they control for, age, gender, and education, and include an indicator for high-income individuals.

Whittaker (2002) also used a non-experimental design to measure the impact of the ITG program for participants enrolled in the program between 1994-1996. She found ITG participation had no significant impact on wages but a positive impact on reemployment. To construct her comparison group, she uses stratified random sampling to ensure the education, race, gender, and weekly benefit rate (WBR) quartile distribution of the ITG group and the comparison group are the same. To adjust for possible differences within subgroups, she weights the results so the distributions match at the WBR-race-gender-education level.

The OLS (Ordinary Least Squares) regression on wage recovery indicated there was no significant impact. Wage recovery was measured using the ratio of bi-quarterly wage recovery in the 7^{th} and 8^{th} quarter after claiming UI to the 4^{th} quarter prior to claiming UI, and she found an imprecisely measured impact of -14.71 (s.e. 8.322). The probit regressions on reemployment indicated that ITG participation had a positive impact on reemployment rates. In particular Whittaker estimated the marginal impact of ITG participation on reemployment in the 7^{th} and 8^{th} quarters after UI claim to be .08.

This study builds on the previous two by using multiple comparison groups yielded via exact matching, propensity score matching, and Abadie-Imbens bias-adjusted matching (Abadie and Imbens, 2004). The previous studies only relied on one-to-one stratified samplings, which can face limitations from small sample size. For instance, there may be an ITG participant for whom there is no observation in the comparison group that is a good match. Propensity score matching attempts to adjust for this by using a composite measure to gauge similarity. Multiple comparison groups help determine whether any estimated impacts are robust to the matching method. Also, by including pre-unemployment tenure, pre-unemployment industry, county of residence, and using exact matching, this study relies upon a more comprehensive set of matching variables than the prior evaluations. Research has demonstrated that using comparison groups from the same local labor market and a richer set of variables to model the eligibility determination process is a preferred approach (Michalopoulos, Bloom, and Hill, 2004) (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998). To that end, this evaluation expands the matching variables to include county of residence, pre-unemployment industry and tenure. This study also improves upon the previous studies by considering the costs and comparing the private costs of the program to the estimated economic return.

To mitigate the selection bias concerns that accompany all non-experimental studies, we apply a difference-in-difference model to eliminate the time invariant differences in unobservables. Further, the Abadie-Imbens matching procedure involves a bias-adjustment factor that controls for post-matching differences in observables.

The relatively small percentage (1-3%) of UI claimants participating in the program per year occurs because of limited funding. Also some UI claimants may receive federal funds for worker training. That said, we cannot exclude the possibility that members of the matched comparison groups receive some form of training separate from the ITG program. However the probability is low because those receiving UI benefits are not supposed to be engaged in full-time training because their time is supposed to be focused on job searching. For those enrolled in the ITG program the rule is waived. Nonetheless, strictly speaking we are measuring the impact of the offer of an ITG voucher to pay for training.

In addition to evaluating the overall impact of the ITG program on reemployment and wage recovery, this study estimates the impacts for subgroups with barriers to reemployment, such as: low skill levels, out-of-date skills, or limited relevant work experience. These barriers are of particular interest because of the potential for training to assist in overcoming them, which may not be feasible in the absence of training.

Arguably any number of subgroups could face these barriers, but some are more likely to face them. High school dropouts typically have low skill levels and no formal degree. Recent studies suggest that employers increasingly value skill certifications in their hiring decisions. Data from the U.S. Bureau of Labor Statistics also confirm that high school dropouts have higher unemployment rates than those with higher levels of education.[7]

We examine older workers because they are expected to be 20% of the U.S population by 2030 and face barriers to employment such as out-of-date skills and age discrimination by employers. Finally, lack of relevant work experience is a barrier for those switching careers later in life. It is an even greater barrier for females transitioning to male dominated careers (such as computer programming or engineering) because the lack of experience and associated job networks is combined with employer stereotypes about female being less adept at abstract thinking (Panteli, et. al, 2001). We are particularly interested in computer programming and engineering because of the tight labor market for information technology workers during the study period. During tight labor markets, we expect employers may be more inclined to accept skill training as a

[7] U.S. Bureau of Labor Statistics, Employment Situation News Release, Table A-4. Labor force status of the civilian population 25 years and over by educational attainment, http://www.bls.gov/webapps/legacy/cpsatab4.htm

substitute for work experience and other screening mechanism (such as job network connections).

High school dropouts face increasing disadvantages in the U.S. economy because of the increasing importance employers place on education and skill credentials when hiring new workers (Holzer, 1996) (Holzer, Rapael, and Stoll, 2006). Training opportunities, such as the ITG program, can assist adults who may have missed educational opportunities earlier in life. Cameron and Heckaman (1993) find that wage increases experienced by high school dropouts obtaining a GED result mostly from access the GED provides to further post-secondary training. Murnane, Willett, and Boudett (1999) also find that GED provides benefits via the access it grants to further training. We expand on these findings in chapter 2, *The Individual Training Grant Program: Its Impact on Uncommonly Served Groups*, by examining impacts for an older cohort of high school dropouts (median age of 39) than examined in the previously cited studies (most are in their 30s). Chapter 3, *A Multi-Method Impact Evaluation of the Individual Training Grant Program on Participants Facing Barriers to Employment*, also examines the program impact for a group of high school dropouts, but it uses multiple methods to test how robust the results are to the estimation method chosen.

In addition to providing opportunities to high school dropouts, training opportunities encountered later in life present a chance to purse a different career, perhaps even a nontraditional career path. Lovell and Negrey (2001) found little evidence of transitions to nontraditional occupations in welfare-to-work training programs; however, results may be different among dislocated workers. In the first essay, we examine the reemployment and wage recovery impacts for female ITG participants enrolled in engineering or computer programming training. Those working in occupations related to these areas tend to be mostly men. Current Population Survey data for the core information technology professions (computer scientists, computer engineers, systems analysts, and programmers) in 1998 showed that 73% of these workers are men (Ellis and Lowell, 1999). Similarly, within the ITG program men enroll disproportionately in these training areas. While men are 39% of all ITG participants, they comprise 56% of those enrolled in computer programming training and 80% of those enrolled in engineering training. The reasons for this disparity are complex. Occupational choice is a life-long process extending from primary school through retirement and incorporating factors as varied as parental influence, mainstream media, and teachers. The opportunity for additional training presents itself as a factor that could perhaps help people pursue their previously unfulfilled interest in a non-traditional career.

Finally, we examine how program impact varies by age. Research suggests that the older workforce faces lower odds of reemployment. Chan and Stevens (2001) show that the odds of reemployment decrease as workers get older. Hirsch and Macpherson (2000) demonstrate that workers over 50 face barriers to entry in jobs with steep wage profiles, pension benefits, and computer usage. They find that occupations that involve high levels of computer usage employ few older workers and are less accessible to older workers. An experimental job search study found that younger female workers are 40% more likely to be interviewed by an employer than older female workers (Lahey, 2006). Also upon returning to work, older workers are less likely than younger workers to earn an amount similar to their prior wages (O'Leary and Eberts, 2007). This may occur because older workers take a "bridge-job", one they use to transition to retirement, which tends to pay less then their career job (Quinn, 1998). It may also be the case that employers pay older workers less because of concerns of lower productivity. These trends raise the question of whether the impacts of training have a differential effect by age. Jacobson, Lalonde, and Sullivan (2004) estimate that the social returns to training (incorporates private and social costs) are lower for older unemployed workers obtaining training at community colleges than for younger workers. We expand on these results by examining impact of the ITG program for older and younger workers.

The second essay examines the impact for older workers using multiple matching methods, and chapter 4, *The Monetary Returns to the Individual Training Grant Program*, examines the extent to which the net returns to training for dislocated workers are sensitive to age, prior education, and retirement age. The essay also provides the first estimate of the economic returns to the Individual Training Grant program.

Chapter 2

Vocational Training for the Unemployed: Its Impact on Uncommonly Served Groups

ABSTRACT

This essay examines the impact of vocational training on unemployed workers not typically studied: women enrolled in engineering or computer programming training and high school dropouts. Using data from New Jersey's Individual Training Grant (ITG) Program and a non-experimental design, we compare the ITG groups' re-employment and wage recovery rates to a matched comparison group. We find that women enrolled in the male-dominated fields of engineering or computer programming experience re-employment rates that are lower than or similar to those in the comparison group, but they experience higher wage recovery in 8th and 12th quarters after claiming Unemployment Insurance (UI). Hispanic high school dropouts experience both higher re-employment and wage recovery rates than their comparison group, but the wage recovery advantage disappears when those enrolled in truck driving training are removed from the sample. Further, white and black high school dropouts experience no re-employment or wage recovery advantage. For all participants, we find participants experience a higher re-employment rate than the comparison group beginning in the 5th quarter and experience no wage recovery advantage. To address the concern of selection bias, a difference-in-difference wage model controls for time-invariant differences in unobservables and an employment regression model controls for remaining differences in the matching variables. These results suggest that training improves re-employment chances and that type of training matters with respect to wage recovery. In this sample, those enrolled in truck driving training, engineering, and computer programming tended to experience higher wage recovery than their comparison group.

I. Introduction

Most countries have active labor market programs that provide training for the unemployed. The programs are intended to assist the unemployed in obtaining jobs faster and mitigate their wage losses. The wage loss experienced by dislocated workers can be substantial. A survey of dislocated workers, who lost their jobs in the United States between 1999 to 2001, indicates that 29% of those re-employed in January 2002 were experiencing earning losses of 20% or more relative to what they earned on their prior job. (U.S. Bureau of Labor Statistics, 2002). The general rationale is that the new skills gained through training will provide better outcomes than would have occurred in the absence of training.

Using a non-experimental design and data from New Jersey’s Individual Training Grant (ITG) program, this thesis examines the general impact of training on the unemployed who are eligible for Unemployment Insurance (UI). Training can yield different impacts on different groups. To illustrate the variation this essay examines two groups not commonly studied in the dislocated worker training literature: women enrolled in engineering or computer programming training and high school dropouts.

Although the groups appear unrelated, both groups represent limitations of the general education system: under representation of women in science fields and the skill level of high school dropouts. Training opportunities encountered later in life may compensate for such shortcomings encountered during earlier educational experiences. This notion leads some to characterize the U.S. adult-training system as a “second chance” system because it provides adults with a publicly funded training opportunity that is separate and apart from the general public education system that is freely available to children and teenagers.

New Jersey's ITG program is part of this system. It was created in 1992 by the state legislature to assist dislocated workers in obtaining the skills they need to find new jobs faster and mitigate their wage loss. To be eligible for the program one must be eligible for UI. Additionally, job counselors weigh a series of other factors including current skill level, previous occupation, previous industry, and current demand for the type of occupation they are seeking. The ITG program is a voucher program, therefore participants can choose from training programs offered by approximately 250 state-approved schools. On average participants spend 5 months in a training program. Using the taxonomy created in a recent study of the federal voucher program (Perez-Johnson, et al., 2004), the ITG program best fits the guided customer choice typology. Participants are encouraged to think through their training options with the job counselor before deciding.

Previous evaluations of the ITG program found a positive impact on re-employment, but no consistent impact on wage recovery (Van Horn, et. al 2000) (Benus, et al., 1996). Unlike the previous evaluations, this study uses a more extensive set of matching variables by matching on pre-unemployment tenure, pre-unemployment wage distribution, pre-unemployment industry, and demographic characteristics. These matching variables attempt to comprehensively parallel the actual eligibility determination process as suggested by existing research (Heckman, Ichumara, and Todd, 1997) (Heckman, Ichumara, Todd, and Smith, 1998).

We find ITG participation has a positive impact on re-employment beginning in the 7^{th} quarter after claiming UI. However, among the employed, participation has no impact on wage recovery. This is consistent with the findings from past evaluations of the ITG program. Additionally, the general literature on the impact of training for the unemployed is mixed, with some finding a positive impact on wages and re-employment and others finding no impact on wages (Leigh, 2000).

The average treatment effect, while useful, does not expose possible variation in the treatment effect for sub-groups. The two sub-groups examined in this essay (high school dropouts and women enrolled in engineering or computer programming) are of interest to policy makers for different reasons. For policy makers interested in ways to achieve gender parity in occupations and industries, the outcomes of women enrolled in non-traditional training and the degree of gender segregation in the ITG program are of interest. For policy makers interested in assisting high school dropouts, this study is particularly valuable because it is one of the few studies to look at how vocational training impacts older high school dropouts with work experience.

First, we examine how the ITG program impacts the 542 women enrolled in engineering or computer programming training. Research has shown that women are under-represented in technical occupations and are much less likely to graduate from college with a degree in engineering or computer science. Current Population Survey data for the core information technology professions (computer scientists, computer engineers, systems analysts, and programmers) in 1998 showed that 73% of these workers are men, that they are on the average 37 years old, and that they are highly educated (Ellis and Lowell, 1999). Also survey results indicate that in 2000 only 19% of bachelors degrees in computer science and computer engineering were awarded to women (Bryant and Irwin, 2001).

Training obtained through the ITG program is also segregated by gender. Although 61% of ITG participants are women (39% men), 89% of those enrolled in health-related training are women and 80% of those enrolled in engineering training are men. Additionally, 56% of those enrolled in computer programming are men.

Programs aimed at lessening such gender imbalances have yielded mixed results (Kerka, 1999). Little is known about how general training programs, like the ITG program, impact women enrolled in non-traditional fields of study. A study by Lovell and Negrey (2001) found little evidence of transitions to non-traditional occupations in welfare-to-work training programs in seven cities. We find unemployed women enrolled in engineering or computer programming have similar or slightly lower re-employment rates than their comparison group, but once employed they have higher wage recovery than their comparison group.

Second we examine the approximately 900 ITG participants (6% of the sample) who do not have a high school degree. This group of participants is disproportionately Hispanic and black—30% of all participants are Hispanic or black, while 66% of high school dropouts are Hispanic or black. Similarly, in the general U.S. population Hispanics and blacks are more likely not to have a high school degree. According to the U.S. Census Bureau, in 1998 44% of Hispanics and 24% of blacks had completed less than 4 years of high school, compared to 16% of whites.[1] For high school dropouts the prospect of a training voucher provides an opportunity to obtain skills and perhaps a vocational certificate in an economy, which increasingly values skills and degrees.

Cameron and Heckaman (1993) find that wage increases for high school dropouts obtaining a GED result mostly from the access a GED provides to further post-secondary training. Similarly, Murnane, Willett, and Boudett (1999) also find that GED provides benefits via the access it grants to further training. Using a NLSY (National Longitudinal Study of Youth) sample of youth age 16 in 1979 and following them for 15 years, they find there is a wage gain for the very small percentage of GED recipients who subsequently obtain post-secondary education or on-the-job training experience. In contrast, for those few GED holders who go on to obtain training at proprietary schools or community-based organizations there is no associated wage gains.

We extend this line of investigation by studying a sample of unemployed high school dropouts with a median age of 39 at the time of unemployment. Specifically, we examine how UI eligible high school dropouts *without* GEDs, but *with* access to ITG vocational training vouchers fare in the labor market compared with a group of UI eligible high school dropouts without access to the vouchers.

We find the impact on re-employment for high school dropouts varies by race. Hispanic high-school dropouts experience a higher re-employment rate than their comparison group in the 8^{th} quarter after claiming UI. In contrast, Black and white high-school dropouts experience no significant advantage. Hispanics experience a quarterly wage recovery advantage, but the advantage disappears after those enrolled in truck-driving training (comprising 45% of the sample) are removed from the sample. As with re-employment, black and white high-school dropouts experience no significant wage recovery advantage.

More generally we find that training type matters, especially with respect to wage recovery. Both male and female participants enrolled in computer programming or engineering training experience higher wage recovery than their comparison group. Also, those enrolled in truck driving training experience higher wage recovery than their comparison group. However, overall training has no significant impact on participant wage recovery.

II. Methodology

A. Estimating the Impact of Training

To estimate the impact of training, one must compare post-training employment and wages with what these outcomes would have been in the absence of training. In the absence of experiments that randomly assign people to a training group or a group that does not get training, researchers use statistical matching methods to construct a comparison group similar to the training group. These matching techniques are applied to obtain a comparison group for the ITG participant group, where members of the comparison group did not obtain an ITG voucher, but are comparable to those in the ITG sample in observable characteristics. Comparing the outcomes of the matched comparison group with the ITG group provides an estimate of the impact of the training voucher.

The impact estimates rely on the mean Conditional Independence Assumption (CIA), which assumes that any difference in the mean outcome is attributed to training participation. Therefore the average treatment effect can be obtained by simply comparing the average outcome of the two groups. More formally it can be expressed as

$$\Delta Y = \frac{\sum_{i=1}^{k1} Y_{i,1}}{K_1} - \frac{\sum_{i=0}^{ko} Y_{i,0}}{K_0} \qquad (1)$$

where, $Y_{i,1}$ denotes the post-program outcome for members of the participant group; K_1 denotes the number of participant group members; $Y_{i,0}$ denotes the post-program outcome for members of the non-participant group; and K_0 denotes the number of the non-participant group members. Equation 1 is equivalent to an regression of the outcome (Y) on an indicator variable that is 1 if a person is a training participant and 0 if they are in the comparison group.

To test the validity of the mean CIA assumption for this data, we use the method proposed in Heckman and Hotz (1989) to test for any pre-program differences between the ITG group and the matched comparison groups. The test assesses whether the coefficient on participation in a *pre-program* wage equation is significantly different from zero. The main limitation of this test is that the absence of *pre-program* differences does not imply the absence of *post-program* differences in unobservable characteristics. Nonetheless, it is reliable measure of differences between the two groups.

A second assumption underlying matching is called common support and it requires that there are enough people in the comparison group with similar observable characteristics as the training group. We are fortunate to have a large and diverse comparison group to match from: so this assumption is easily invoked.

The comparison group selection process is designed to parallel the counselor's process for determining ITG eligibility. A counselor determines whether a person interested in the ITG program is eligible by first establishing that the candidate is eligible for unemployment insurance benefits and not on temporary layoff. Then the counselor examines the person's skills and work experience in detail, on a case-by-case basis. Accordingly, our comparison group population is comprised of all those non-participants claiming unemployment benefits who are not on temporary layoff. A comparison group is selected from this population by matching on the following nine characteristics: year of UI claim, previous industry, prior wage, prior tenure, education, age, gender, race, and county of residence. Though gender, age, and race are not directly considered in the eligibility process, the general literature in labor economics indicates that labor force participation decisions and wages do vary based on age, gender, and race. Matching on county of residence helps to control for regional labor market differences.

Using the above nine characteristics, the comparison group is selected through stratified random sampling at the cell level. Stratified sampling at the cell level involves dividing the training group into mutually exclusive categories using the characteristics listed previously. Then a comparison group is randomly selected so that the number of comparison group individuals in each mutually exclusive category matches the number training participants that fall in that category. For example, if there are three ITG participants who are: white women between the age of 37 and 50, with a high school degree, residing in Middlesex County, who were employed in the service industry at the same employer for the twelve quarters prior to claiming UI and who earned in the top 25% prior to claiming UI, then the resulting comparison group has approximately three such individuals. In cases where there are no individuals from the comparison population with the same combination of characteristics (i.e. not matches in a cell), a weaker criteria for a match is used.

The weaker criterion reduces the education categories from four to two (high school or less and some college or more), the race categories from five to two (white and non-white), and the county categories to three regional variables. Approximately 25% of the overall and female sample were selected using the weaker criterion, while 50% of the high-school dropout sample was selected using a different weaker criteria which only eliminated county as a matching variable.

There is little difference between the above covariate cell-level matching and the more common method of matching on propensity scores. Both rely on the conditional independence assumption. Propensity score matching is more widely used because it eliminates the difficulty of matching on a large set of covariates, often referred to as the curse of dimensionality (Rosenbaum and Rubin, 1983). However those with the same propensity scores do not necessarily have the same values for their covariates. Given the large diverse comparison group population available for this study, we choose to match directly on covariates. Consequently there is no need to adjust our standard errors for the additional variance created by propensity score matching. If propensity score matching techniques are used, then the standard errors should be adjusted for the randomness introduced by the score-estimation procedure.

Exact cell matching does involve a trade-off between bias and efficiency. Using one-to-one matching reduces the bias when compared to one-to-many matching. Utilizing a single comparison group member amounts to incorporating less information than if one used a weighted-average of multiple-comparison group members. Less information reduces the likelihood of poor matches (i.e. bias) but at the same time using less information decreases the efficiency (i.e. increases the variance).

Three comparison groups are created by the previously described cell-level stratified random sampling. The first comparison group is matched to the full ITG sample. This sample is used to estimate the overall impact. A second sample is selected to estimate the impact for women enrolled in non-traditional training and a third sample serves as the comparison group for ITG participants who are high school dropouts. These additional comparison groups are created because these groups are noticeably different from the overall group.

While we cannot exclude the possibility that members of the matched comparison groups receive some form of training, the probability is low for one main reason. Those receiving UI benefits are not supposed to be engaged in full-time training because their time is supposed to be focused on job searching. For those enrolled in the ITG program the rule is waived. Nonetheless, strictly speaking we are measuring the impact of the offer of an ITG voucher to pay for training.

B. Regression Models

In the case of wages, a difference-in-difference regression model is used to control for possible dissimilarities between the training and comparison group in unobservable characteristics. A difference-in-difference model controls for unobserved differences by assuming any unobservable differences between the two groups are constant over time, and therefore are removed when subtracting wages in quarter t from t+1. The model is specified as:

$$\Delta W_i = \mathrm{B}_0 + T_i g \quad + a \quad Z_i + \Delta e_i \qquad (2)$$

ΔW_i denotes the difference in the quarterly wage between a post-UI quarter and 4th quarter before UI for person i. Equation 2 is estimated for post-UI quarter 1 to 8, 12, 16, 20, and 24. T_i denotes a program participation status variable; Z_i denotes a vector of time variant factors that include age, year of UI claim, and whether still working in the same industry, and for program participants a variable indicating completion of training. The e_i denotes the error term.

The difference-in-difference model is not feasible with the employment variables because all ITG and comparison group participants are employed in the quarters prior to becoming unemployed. Consequently for employment there is no variation between these groups in the pre-unemployment quarters. Therefore for employment a linear probability model is used to further adjust for differences in observable characteristics between the training sample and matched sample:

$$E_i = a_0 + a_1 T_i + a_2 X_i + e_i \qquad (3)$$

E_i represents the outcome variable of employment status. T_i is a variable denoting ITG participation, and X_i contains the following personal characteristics: gender, prior educational attainment, race, year and quarter of UI claim, potential work experience, industry of employment prior to unemployment, county of New Jersey residence, job tenure at the time of unemployment, pre-UI wage quartile, reason for separation from job, local unemployment rate, and training completion (which is only valid for the ITG group). By the 7th quarter after claiming UI, the completion variable is no longer needed because all participants have completed training. The completion variable simply controls for the time spent in training. The data available does not allow us to identify those who drop out of training. Consequently there is no way for identifying or controlling for attrition bias. Therefore, as noted earlier what is being measured is really the offer of an ITG voucher.

III. Data

A. Program Participants

The administrative data for the ITG program are maintained by the New Jersey Department of Labor and Workforce Development and contain information on a participant's age, race, educational attainment, gender, the dates that training begin and end, the type of training to be provided, and the type of provider of this training. These administrative data are collected when an individual first becomes a participant in the ITG program and are updated when an individual is issued a training contract. The results presented in this essay are based on data from the 16,001 participants who both claimed Unemployment Insurance and were deemed eligible for the ITG program between 1995-1999.

The administrative data for these individuals are merged with their wage data from New Jersey's Unemployment Insurance Wage Record system. One limitation of the wage data is that it excludes those employed in out-of-state jobs, the self-employed, and federal employees. This limitation should not bias the estimates as long as the probability of out- of-state employment, self-employment, or federal employment for the ITG and the comparison group is the same. To minimize this potential bias, county of residence is one of the 9 observable characteristics that are used to create the stratified random sample. This helps lessen the bias because it insures that the same portion of ITG and comparison group members reside in those counties bordering other states, where the likelihood of out-of-state employment is high.

It is important to note that although the state refers to the data as "wage record" data, the information available is earnings data and does not include hours worked.

Therefore, we are not able to differentiate what portion of earnings increases (if any) result from more hours worked and/or a greater hourly wage.

B. The Comparison Group

The matched comparison group is obtained from the population of approximately 800,000 UI claimants on permanent layoff that claimed UI between 1995 and 1999. The database for all UI claimants is maintained by the New Jersey Department of Labor and contains information on date of claim, age, race, educational attainment, gender, and county of residence. All ITG participants are also UI claimants on permanent layoff therefore they are removed from the database before conducting the match.

As delineated in Table 2.1, the matched-comparison group for the overall sample, for the female sample, and for the high school dropout sample all have similar characteristics to their corresponding ITG sub-group. For instance, 28% of the overall ITG group are college graduates, compared with 27.7% of its comparison group. A chi-square test indicates that there is no significant difference between the distributions of the ITG groups and their corresponding comparison group.

Table 2.2 provides the p-values for the ITG coefficient in the regression of pre-program wages on ITG participation (also referred to as the Heckman-Hotz test) and the regression of pre-program wage growth on ITG participation. These p-values indicate the coefficient on participation is not statistically different from zero for the overall sample, the female sample, or the high school dropout sample. This indicates that prior to the program, participation status had no significant influence on wages or wage growth. Taken together the chi-squared test and the Heckman-Hotz test provide evidence that the comparison and ITG groups have similar observable characteristics.

IV. Results

A. Overall Impact

ITG participation has a positive impact on re-employment. However, among the employed, participation has no impact on wage recovery. Table 2.3 lists the difference in average re-employment rates for the two groups. Initially the comparison group has a higher reemployment rate, but beginning in the 5th quarter after claiming UI the ITG group has a 3.3% higher reemployment rate than the comparison group. This advantage rises to 4.7% in the 7th quarter after UI claim, and remains at similar levels through the 24th quarter. The initially lower re-employment rate for the ITG groups occurs because by the 2nd quarter after UI only 37% of ITG participants had completed training. By the 4th quarter after claiming UI, when the employment rate begin to converge, 84% of ITG participants had completed their training.

Table 2.3 also reports both the ITG coefficient and the sum of the ITG coefficient and the completion coefficient.[3] The sum represents the joint influence of participation and completion on the re-employment probability. The results from the regression-adjusted model vary slightly from the difference in mean reemployment rates. For instance, in the 5th quarter after claiming UI the joint effect of ITG and completion amounts to 4.7%. However, the ITG coefficient on its own is –14% because in the 5th quarter, 10% of ITG participants are still in training. By the 7th quarter after UI when all ITG participants have completed training, the ITG coefficient is 4%. These positive results are consistent with the other ITG evaluation that that examined re-employment (Whittaker, 2002) and the evaluation of a training program in Washington state (Hollenbeck, 2003).

Unlike the effect on reemployment, ITG participation has no consistent positive impact on wage recovery for the overall sample. The differences in means in the 8th quarter indicate that the ITG group experiences a $177 greater loss in quarterly wages than the comparison group. After controlling for other factors, the gap, embodied in the ITG coefficient, changes to $-51.56 in the 8th quarter after UI. By the 16th quarter after claiming UI, the ITG coefficient (generated by the difference in means) is insignificant, and remains so in the 20th and 24th quarter. In the 20th quarter, the regression model indicates the ITG coefficient is positive and significant, but it is the only instance in the post-UI period where this occurs. The general insignificant impact on wages is consistent with the evaluation literature. Two previous ITG studies found no positive impacts of ITG participation on wages (Benus, et al., 1996) (Whittaker, 2002). Studies of other programs also found no impact on wages (Corson and Haimson, 1996) (Decker and Corson, 1995). However, some non-experimental studies have found positive impacts on wages (Benus and Byrnes 1993) (Jacobson, et al, 1994) (Hollenbeck, 2003). The inconsistent findings across the studies may stem from a variety of factors including: regional labor market variations, differences in program implementation and populations served.

B. Women Enrolled in Non-traditional Training Fields

Prior to looking at the impact for women enrolled in computer programming or engineering classes, we first look at the overall training enrollment patterns for the ITG program. As reported in Table 2.4, women enroll disproportionately more in health-related training and business-related training, while men enroll disproportionately more in transportation related training and engineering. Computer training appears to be more gender balanced; however, when examining the sub-categories of computer training a different trend appears. Women tend to enroll in more data processing related areas, while men tend to enroll in computer programming and systems analysis areas. This parallels occupational segregation patterns found in the U.S. economy. According to 2000 Census data, over 80% of nurses, health technologists, and home health aides are women. Similarly 70% of those in computer or occupational occupations are men (Caiazza, 2004).

The remainder of this section examines the outcomes for the 5.6% of women ITG participants who enroll in engineering or computer programming. These are areas where men are a majority—men are 80% of those in engineering training and 56% of those in computer programming training.

A priori it is unclear what outcomes to expect for these 5.6% of women participants engaged in computer programming or engineering training. Training in computer programming during the 1990s, a time of tight labor markets in the computer field, would imply higher than average employment and wage outcomes for those engaged in computer training. However, 40% of women engaged in computer programming or engineering training came from secretarial occupations, which may imply little relevant work experience and therefore, difficulty in breaking into the field.

The results indicate that women engaged in these non-traditional areas do not experience a re-employment advantage. In the 8th and 12th quarter after UI, the ITG coefficient is not significantly different from zero. However, in 8th and 12th quarter after claiming UI they experience a significant wage gain relative to the comparison group. These results are noticeably different from the overall sample where there was no discernable wage recovery impact for ITG participants but there was a re-employment impact.   The differences in mean re-employment rates between the two groups are listed in Table 2.5. Women enrolled in computer or engineering training have a lower re-employment rate than the comparison group from the 1st quarter to the 6th quarter after unemployment, with an 8% lower reemployment rate in the 6th quarter. Beginning in the 7th quarter, the difference is –1% and is not statistically different from zero. The difference in mean re-employment remains insignificant in the subsequent quarters.

Controlling for other factors in the expanded regression does not improve the impact results. Prior to the 6th quarter after claiming UI, the ITG coefficient is negative. Then in the 7th quarter there is no significant difference between the ITG and comparison group re-employment rates, as with the difference in means.

A slightly different picture emerges when examining wages. In the first three quarters after claiming UI, women engaged in computer programming or engineering training experience a lower wage recovery than the comparison group. Then in the 4th and 5th quarter there is no measurable difference in wage recovery between the ITG and comparison group. By the 8th and 12th quarter, a wage recovery advantage appears for women engaged in computer or engineering training. The difference in means listed in Table 2.5 shows that in the 8th quarter women experience a $760 greater quarterly wage gain than the comparison group. The regression-adjusted difference is virtually the same, at $758. In the 12th quarter the ITG advantage increases to $1,042. The variation between quarters occurs because the comparison group's average wage recovery drops. To illustrate the differences, we compare the 99th percentile of the wage recovery distribution for each group in the 8th and 12th quarter. In the 8th quarter after UI, the 99th percentile of the comparison group wage recovery distribution is $10,768 (and $13,554 for the ITG group). In contrast in the 12th quarter, the 99th percentile for the comparison group drops to $8,462 and remains relatively unchanged for the ITG group at $13,982.

For comparison, the impacts for male ITG participants in engineering or computer programming training are also estimated. The comparison group sample for the 1,286 male ITG participants enrolled in engineering or computer programming training was selected using the same methodology described earlier. In contrast to women, male ITG participants enrolled in engineering or computer programming training had higher re-employment rates than their comparison group. With regard to wage recovery, men enrolled in engineering or computer programming training also experienced a wage recovery advantage in the post-unemployment period.

The negligible impact on reemployment for women in engineering and computer programming may imply that women have a harder time finding jobs in non-traditional areas, especially because 40% of these women were in secretarial occupations prior to ITG participation. Steedman (1997) has demonstrated that career transitions for secretaries within firms is not widespread because secretarial work is not perceived as an occupation that develops creative thinking and decision-making skills. However, if these ITG participants are obtaining jobs related to their computer programming or engineering training, then we expect higher levels of wage recovery (relative to the comparison group) for these fields because they tend to pay a higher than average wage.

To assess the influence of previous employment as a secretary on reemployment, we included an interaction term between ITG participation and previously working in a secretarial occupation. The coefficient on the interaction term was insignificant in all quarters. It was negative in eight of the twelve quarters examined, and fluctuated from -.0134 in 1st quarter to .06 in the 16th quarter and -.11 in the 24th quarter. Additionally, the re-employment impact results did not change when the 16% of female ITG participants who were previously employed in computer occupations were removed from the sample. This suggests that previous occupation does not fully explain the low re-employment impact for women enrolled in engineering or computer programming training. Further, the wage recovery impacts for women enrolled in computer or engineering training changed very little when removing the 16% previously employed in computer occupations. For instance, the simple difference in mean wage recovery in the 8th quarter fell from \$760 (standard error \$381) to \$732 (standard error \$384) after removing the 16%. Similarly the impact in the 12th quarter fell from \$1042 (standard error \$403) to \$1145 (standard error \$414).

C. High School Dropouts

The second sub-population we examine is high school dropouts. Among those without a high school degree, Hispanic ITG participants experience significantly higher re-employment and wage recovery rates. However, the wage recovery advantage for Hispanics is largely driven by a group enrolled in truck-driving training. White and black high school dropouts experience no statistically significant advantage in re-employment or wage recovery.

Table 2.6 provides the difference in mean re-employment rates for Hispanic, white, and black ITG high school drops outs and their corresponding comparison groups. Hispanic ITG participants are the only group with a consistent statistically significant reemployment advantage in the 8th to 20th quarter after claiming UI. In the 8th quarter, Hispanic ITG participants have an average re-employment rate 7.5% higher than the comparison group. This advantage rises to 10.6% in the 12th quarter. A similar trend emerges in the regression-adjusted results.

Table 2.7 lists the average wage impacts for high school dropouts. As with the re-employment results, Hispanic high school dropouts are the only group to experience a consistently significant wage recovery advantage. In the 8th quarter after claiming UI, the difference in means column indicates that Hispanic ITG participants experience a $771 greater quarterly wage gain than the comparison group. Similarly, in the regression model for Hispanics the ITG coefficient is on the order of $780 in the 8th quarter. The difference in means and the regression-adjusted results also indicate there is no consistent significant wage recovery advantage for white or black high school dropouts.

The positive wage recovery impacts for Hispanics are driven by a group enrolled in truck driving training. Approximately 45% (164/364) of ITG Hispanic high school dropouts enrolled in truck driving training. When these 164 participants are removed from the sample, the wage recovery advantage for Hispanics disappears. In both the difference in means and the regression-adjusted results there is no longer a significant wage advantage. In the 8th quarter, the coefficient of participation for Hispanics falls to a statistically insignificant $247, and the difference in means falls from a statistically significant $771 to statistically insignificant $159. The advantage also disappears after removing both the 164 Hispanic ITG participants enrolled in truck driving training and their corresponding 127 comparison group matches.

For all the 1,391 ITG participants enrolled in truck-driving training, truck-driving training has both a positive impact on re-employment rates and wage recovery. The difference in means reveals a statistically significant ITG re-employment advantage of 3.6% as early as the 3rd quarter after claiming UI. This is noticeably different than the overall impact results, where the ITG re-employment advantage appears in the 5th quarter after claiming UI. The difference occurs because truck-driving training programs are of relatively short duration. Approximately 87% of those enrolled in truck-driving training complete their training by the 3rd quarter after UI whereas only 67% of all ITG participants have completed by the 3rd quarter after UI.

Also, unlike for the overall sample, there is evidence of a positive impact of truck driving training on wage recovery. The difference in means indicate that in the 7th quarter after claiming UI, the ITG group experiences a $370 greater gain in quarterly wages than the comparison group. The gain is statistically significant. The gain in wages is measured as the difference between the wage in the 7th quarter after claiming UI and the wage in the 4th quarter *prior* to claiming UI. By construction the two groups have similar wages in the 4th quarter prior to claiming UI. The regression-adjusted results are similar. In the 7th quarter after UI claim, the ITG coefficient is $506 and increases to $622 in the 16th quarter. In the 20th and 24th quarter the ITG coefficient is no longer significant.[4] The comparison group for the truck-driving training group was selected using the same methodology described earlier.

V. Conclusions and Policy Implications

Overall ITG participants experienced higher average re-employment rates than the comparison group beginning in the 5th quarter after claiming UI. ITG participants' wage recovery levels were similar to the comparison group beginning in the 16th quarter after claiming UI. However, these impact results show some variation across groups.

Women that used their ITG vouchers as opportunities to pursue training in the male-dominated fields of engineering and computer programming have similar or lower re-employment rates than their comparison group. However, once reemployed these women experience a significant wage recovery advantage. Men pursuing engineering or computer programming training experience both a re-employment and wage recovery advantage. Additionally female ITG participants (irrespective of training area) do experience a higher reemployment rate than their comparison group beginning around 7th quarter after claiming UI. Together these results suggest that women enrolled in engineering or computer programming training face more difficulty in obtaining employment than men in these training areas. Presuming they are searching for a training-related job, part of the difficulty may stem from women not having sufficient previous work experience in technical fields. While 40% of women enrolled in engineering training were previously employed as secretaries, 31% of men were previously employed in machine trades, bench-work, structural work, or processing occupations. However, once employed, women trained in these areas experience higher wage recovery than their comparison group. These results suggest that more research is needed to understand why women in engineering and computer programming training have difficulty findings jobs. For instance, do these women have limited access to professional networks which could function as a job network? The results also suggest that upon reemployment, training for high-wage occupations pays off for both men and women in the form of a wage recovery advantage over their comparison group. Both engineering and computer programming jobs generally pay above average wages as indicated by Occupational Employment and Wage Estimates by the U.S. Department of Labor.

For high school dropouts the re-employment impact of training varies by race, and any wage-recovery advantage stems from the type of training pursued. This is similar in spirit to Cameron and Heckman's (1993) finding that the advantage afforded by a GED for high school dropouts is in the access it grants to further training opportunities. Hispanic high school dropouts experience higher re-employment rates than their corresponding comparison group whereas black and white high school dropouts have similar re-employment rates to their comparison group. Hispanic high school dropouts are also the only group to experience a higher wage recovery than their comparison group, but the advantage stems from a large portion of Hispanic high school dropouts that enroll in truck-driving training. When those obtaining truck-driving training are removed from the analysis, the wage recovery advantage for Hispanics disappears. The re-employment advantage for Hispanics does not dissipate after removing the truck driving training group. The variance in the reemployment advantage suggests there is more to learn about different barriers of reemployment faced by different race groups. The wage recovery advantage for those enrolled in truck-driving training suggests that wage recovery advantages (relative to the non-training comparison group) appear in higher paying occupations. In New Jersey, the median wage for truck drivers tends to be above the overall average.[5]

As with all non-experimental studies, there is the concern that the estimates reported here are subject to selection bias. The regression models have attempted to reduce this potential bias. In the case of wages, a difference-in-difference model removes differences in time invariant unobservables. In the case of reemployment, a regression controls for remaining differences in observable characteristics and prior work history. The procedure of creating separate comparison groups for each sub group also serves to improve the process of matching on observable characteristics. Chi-square tests and the Heckman-Hotz test confirm that the demographic characteristics, pre-program wages, and wage growth for the ITG groups and their corresponding comparison groups are statistically similar. Having similar pre-unemployment average wages ensures the groups have similar starting points for the wage recovery measures.

In addition to our findings for high school dropouts and women enrolled in computer programming or engineering training, a more general policy conclusion emerges from this research. The re-employment and wage impacts vary by field of training and by demographic group. For instance, those enrolled in truck driving training, engineering, and computer programming tended to experience higher wage recovery than their comparison group. This suggests that the decision of what training to enroll in is very important and therefore confirms the importance of having good information when making that choice.

Governments can play a role in providing information for those considering which training to enroll in. For instance, governments can encourage training for demand occupations, but leave the final choice to the participants. This model resembles the ITG program structure. Moreover, this model is conducive to U.S. style capitalism because it is sensitive to market forces. Governments can also provide information on prevailing wages, wage growth, and employment rates for occupations, irrespective of local demand. These types of information can assist the unemployed in making a more informed decision on the type of training in which to enroll.

Tables

Table 2.1. Means

Characteristics	Full sample		Women in engineering or computer programming		High-School Dropouts	
	ITG	Comp. Group	ITG	Comp. Group	ITG	Comp. Group
Total Participants	16001	14818	542.0	543.0	926.0	799.0
Female	60.9	60.5	100.0	100.0	44.5	42.2
Male	39.1	39.5	-	-	55.5	57.8
White	66.1	66.4	62.4	61.5	37.3	36.9
Black	21.2	21.1	29.0	29.1	23.3	23.0
Hispanic	12.8	12.5	8.7	9.4	39.4	40.1
Less than High School	5.8	6.1	3.0	2.6	100.0	100.0
High School	50.0	50.2	38.0	37.4	-	-
Some College	28.1	27.7	34.3	36.1	-	-
College or More	16.2	16.0	24.7	23.9	-	-
age 18-36	33.8	34.3	38.0	36.7	41.9	43.1
age 37-50	42.5	42.6	48.0	49.4	36.7	35.4
age 51-65	22.4	22.1	13.8	14.0	21.1	21.2
age 66 or over	1.3	1.0	0.2	0.0	0.3	0.4
same employer 12 qt prior UI	33.8	33.3	28.8	25.2	34.0	33.2
same employer 11-4 qt prior UI	37.5	38.4	39.9	44.0	36.9	38.9
employed continuously 12-4 qt prior UI	12.8	12.4	13.3	12.9	12.0	11.8
employed continuously less 4qt prior UI	15.9	16.0	18.1	17.9	17.1	16.2
Mean qt. wage in 4th qt. prior to UI	$8,325	$8,312	$9,146	$9,145	$6,810	$6,628

Notes: Pre-unemployment industry and region variables are not shown. For all three samples, the ITG distribution for these variables is not significantly different from the comparison group distribution.

Table 2.2 Heckman-Hotz Test

Heckman-Hotz Test	**Full sample**		**Women in engineering or computer programming**		**High-School Dropouts**	
dependent variable quarterly wage in 4th qt. Prior to UI	ITG Coeff	P-value	ITG Coeff	P-value	ITG Coeff	P-value
Model with no covariates	13.5	0.25	1.1	1.00	182.9	0.28
Model with covariates	-70.7	0.14	-118.3	0.68	102.8	0.50
dependent variable wage in 2nd qt prior minus 4th qt. Prior to UI						
Model with no covariates	41.4	0.20	320.0	0.09	136.5	0.22
Model with covariates	20.6	0.53	302.7	0.12	119.6	0.30

Notes: *** indicates significance at .01 level. ** indicates significance at .05 level. * indicates significance at .10 level. The results in the rows labled "with covariates" control for the following factors: education, region, tenure, and prior industry, reason for job loss, age, gender, race, year of job loss.

Table 2.3 Re-employment & Wage Impact for All ITG Participants

Dependent Variable	Re-employment Rate				Post UI wage - Wage in 4th qt. Prior to UI		
Quarter after UI Claim	Mean Difference between ITG & comparison group	ITG Coeff	ITG Coeff + Completion Coeff	Sample Size & Adj R-sq.	Mean Difference between ITG & comparison group	ITG Coeff	Sample Size & Adj R-sq.
1	-0.217*** (0.005)	-0.234*** (0.006)	-0.186*** (0.012)	30,819 0.066	-1170.462*** (117.457)	-1134.094*** (123.073)	7,344 0.039
5	0.033*** (0.005)	-0.143*** (0.013)	0.047*** (0.013)	30,819 0.029	-457.638*** (64.310)	-1425.328*** (173.373)	16,676 0.050
6	0.043*** (0.005)	-0.09*** (0.015)	0.045*** (0.015)	30,819 0.028	-433.313*** (63.842)	-1204.045*** (193.340)	17,361 0.051
7	0.047*** (0.005)	0.039*** (0.005)	- -	30,819 0.027	-265.283*** (63.714)	-168.777*** (62.813)	17,628 0.053
8	0.058*** (0.005)	0.049*** (0.005)	- -	30,819 0.032	-177.105*** (64.113)	-51.567 (63.242)	17,564 0.050
12	0.054*** (0.006)	0.046*** (0.006)	- -	29,418 0.033	-129.681* (68.830)	-7.282 (67.416)	16,264 0.058
16	0.049*** (0.007)	0.039*** (0.007)	- -	21,614 0.038	20.826 (83.143)	142.646* (80.872)	11,674 0.067
20	0.059*** (0.008)	0.049*** (0.008)	- -	15,803 0.040	140.538 (102.734)	261.349*** (99.863)	8,219 0.071
24	0.056*** (0.010)	0.042*** (0.010)	- -	10,471 0.043	130.134 (130.461)	187.878 (127.653)	5,357 0.077

Notes: Entries in the “Mean Difference” columns are the difference between the ITG mean and comparison group mean as illustrated by equation 1. Entries in the ITG coefficient column for re-employment are from equation 3 in this essay. Entries in the ITG coefficient column for wages are from equation 2 in this essay. No completion rate coefficient is available after the 6th quarter because by that time all ITG participants had completed training. “***” indicates significance at the .01 level “**” indicates significance at the .05 level, and “*” indicates significance at the .10 level

Table 2.4 Enrollment Patterns in ITG Training Areas

Type of Training	Male	Female
Overall ITG Sample (N=16,001)	39.09	60.91
Business (N=7,837)	21.02	78.98
Computer, data processing (N=471)	26.96	73.04
Computer, general (N=1232)	35.63	64.37
Computer programming (N=750)	56.53	43.47
Engineering (N=1,078)	79.96	20.04
Health (N=970)	10.72	89.28
Marketing and Distribution (N=164)	40.24	59.76
Transportation (N=1439)	94.86	5.14
Other (N=2,060)	59.22	40.78

Table 2.5
Re-employment & Wage Impact for Women in Computer Programming or Engineering

Dependent Variable	Re-employment Rate			Post UI wage - Wage in 4th qt. Prior to UI		
Quarter after UI Claim	Mean Difference between ITG & comparison group	ITG Coeff	Sample Size & Adj R-sq.	Mean Difference between ITG & comparison group	ITG Coeff	Sample Size & Adj R-sq.
1	-0.255*** (0.028)	-0.262*** (0.028)	1,083 0.106	-1147.624 (703.816)	-459.927 (696.883)	254 0.116
5	-0.047 (0.029)	-0.187*** (0.065)	1,083 0.017	-502.099 (367.706)	-1316.019* (750.537)	574 0.089
6	-0.084*** (0.029)	-0.321*** (0.075)	1,083 0.023	147.864 (366.573)	-637.737 (975.82)	606 0.077
7	-0.01 (0.029)	-0.014 (0.029)	1,083 0.025	341.166 (376.194)	366.358 (387.244)	604 0.061
8	-0.001 (0.029)	-0.008 (0.029)	1,083 0.018	760.771** (381.174)	758.516* (389.842)	595 0.059
12	0.009 (0.03)	-0.004 (0.03)	1,012 0.026	1042.975*** (403.156)	1046.279*** (398.807)	547 0.085
16	-0.017 (0.039)	-0.022 (0.04)	628 -0.008	596.017 (523.384)	555.902 (511.961)	344 0.088
20	0.009 (0.048)	0.016 (0.049)	420 0.034	1013.509 (759.658)	654.846 (765.111)	217 0.100
24	0.063 (0.059)	0.066 (0.062)	272 -0.021	3431.57*** (862.391)	2739.868*** (901.373)	147 0.197

Notes: Entries in the "Mean Difference" columns are the difference between the ITG mean and comparison group mean as illustrated by equation 1. Entries in the ITG coefficient column for re-employment are from equation 3 in this essay. Entries in the ITG coefficient column for wages are from equation 2 in this essay. "***" indicates significance at the .01 level "**" indicates significance at the .05 level, and "*" indicates significance at the .10 level

Table 2.6 Re-employment Impact for High School Dropout

Quarter after UI Claim	Whites: Difference in ITG & Comp. Means	Whites: Sample Size	Blacks: Difference in ITG & Comp. Means	Blacks: Sample Size	Hispanics: Difference in ITG & Comp. Means	Hispanics: Sample Size
1	-0.192*** (0.037)	640	-0.189*** (0.046)	400	-0.203*** (0.035)	685
5	0.084** (0.037)	640	-0.001 (0.049)	400	0.103*** (0.036)	685
6	0.077** (0.038)	640	0.025 (0.049)	400	0.061* (0.036)	685
7	0.055 (0.038)	640	-0.001 (0.049)	400	0.051 (0.036)	685
8	0.087** (0.038)	640	0.034 (0.047)	400	0.075** (0.036)	685
12	0.02 (0.040)	606	-0.014 (0.051)	362	0.106*** (0.039)	629
16	0.009 (0.048)	421	-0.032 (0.064)	242	0.111** (0.047)	424
20	0.041 (0.055)	326	0.03 (0.076)	178	0.164*** (0.059)	281
24	0.104 (0.066)	229	0.021 (0.095)	115	0.142* (0.074)	185

Notes: Difference in means is calculated as specified in equation 1. “***” indicates significance at the .01 level “**” indicates significance at the .05 level, and “*” indicates significance at the .10 level

Table 2.7 Wage Impact for High School Dropouts

	Whites		Blacks		Hispanics	
Quarter after UI Claim	Difference in Means	Sample Size	Difference in Means	Sample Size	Difference in Means	Sample Size
1	-1152.277* (654.310)	156	-1514.105** (766.027)	70	-228.342 (539.714)	125
5	-444.469 (447.919)	363	360.22 (488.688)	188	330.105 (324.909)	357
6	-375.223 (427.311)	371	460.255 (452.728)	202	437.183 (330.555)	366
7	-272.672 (447.211)	362	475.158 (490.103)	194	652.08* (345.846)	369
8	34.225 (409.430)	352	20.731 (504.036)	196	771.976** (328.777)	373
12	-70.934 (449.922)	305	269.76 (477.418)	183	1053.093*** (361.206)	321
16	213.786 (537.713)	209	293.356 (637.910)	109	412.278 (476.833)	221
20	552.019 (663.753)	165	583.225 (745.068)	73	864.901* (525.766)	140
24	1042.347 (793.969)	113	-415.641 (1177.811)	50	239.2 (777.913)	84

Notes: Difference in means is calculated as specified in equation 1. “***” indicates significance at the .01 level “**” indicates significance at the .05 level, and “*” indicates significance at the .10 level.

Chapter 3

A Multi-Method Impact Evaluation of the Individual Training Grant Program on Participants Facing Barriers to Employment

Abstract

This study examines the impact of the New Jersey Individual Training Grant Program on two groups facing more re-employment barriers than the average unemployed person: high school dropouts previously employed in manufacturing, and older white males (age 51 to 65). The re-employment outcomes of these two groups are compared to seven different non-participant comparison groups yielded by different non-experimental matching methods. Both groups experience a re-employment advantage relative to all seven of their comparison groups in the 8th quarter after claiming Unemployment Insurance (UI). A conservative estimate of the advantage amounts to an ITG re-employment rate that is 7-8% higher than the comparison group. The advantage for the high school dropout group is sustained to the 12th quarter. Neither group experiences a wage recovery advantage. Methodologically, we find that both propensity score matching and stratified random sampling can be sensitive to ties. Two stratified random samples that only differ by the random seed yield estimates that are seven percentage points apart.

I. Introduction

Federal and state governments have designed numerous programs to assist unemployed workers find new jobs. One such program is New Jersey's Individual Training Grant (ITG) program. Established in 1992, it provides training vouchers to workers eligible for unemployment insurance (UI) to obtain training at their choice of hundreds of state-approved programs at proprietary training schools and community colleges. Previous research has shown that the chances of re-employment differ based on factors such as age, prior education, and previous industry (Farber, 2005) (Hipple, 1999). Any number of sub-groups could be chosen to capture the variation. We chose two groups that represent two different ends of the income scale and varying barriers to re-employment: 1) white males who were age 51 to 65 in 1995 when they became unemployed, and have an average quarterly wage of $12,610 in the 4th quarter prior to filing for UI, and 2) high school dropouts who were previously employed in manufacturing and claimed Unemployment Insurance (UI) between 1995 and 1999, and have an average quarterly wage of $7,351 in the 4th quarter prior to filing for UI. We refer to these two groups as the older white male and high school dropout group. We focus on these groups with barriers to employment because previous studies have already examined the general impact of the ITG program on all participants (Van Horn et. al, 2000) (Benus et al., 1996).

With respect to the first group, non-Hispanic whites are generally documented to face the fewest labor market barriers in the U.S. economy; however, research has shown that re-employment is especially difficult for older unemployed workers. Chan and Stevens (2001) find that the chances of re-employment decrease with age, and O'Leary and Eberts (2007) find that older unemployed workers earn less after returning to work than younger workers. Also Hirsch and Macpherson (2000) demonstrate that workers over 50 face barriers to entry in jobs with steep wage profiles, pension benefits, and computer usage requirements.

Our second group was deliberately selected to have two known re-employment barriers: no high school diploma and previously employed in the manufacturing sector, which is characterized by its declining employment. In New Jersey, the percent of the workforce employed in the manufacturing sector fell from 17% in 1995 to 13% in 2000. Also in its 2002 Displaced Worker Survey, the U.S. Bureau of Labor Statistics found that one third of displaced workers through the 1990s were manufacturing workers. In contrast, manufacturing jobs only constitute one eighth (13%) of all jobs in 2002, a slight fall from 16% in 1995. Workers displaced from the manufacturing industry between 1995-1996 experienced a median of 12 weeks without work, compared to 7.6 for all workers (Hipple, 1999). Not having a high school degree is a well known barrier to employment given the increased importance of education in today’s labor market (Holzer, 1997).

To measure the impact of the ITG program on these groups, the preferred approach is to compare post-program outcomes to what the outcomes would have been in the absence of program participation. Methodologically, the best way to measure the impact is an experiment in which people are randomly assigned to a program or a control group that does not participate into the program. However, if this method is not available, researchers often use propensity score matching or stratified random sampling to construct a comparison group similar to the training (participant) group. These non-experimental methods, referred to as matching models, essentially involve finding a comparison group whose observable characteristics match the participant groups' characteristics. The average outcome for the participant group is compared to the average outcome for the non-participants to obtain the average treatment effect on the treated (ATT).

There is a large body of literature about matching models. One set of studies compares experimental results with non-experimental estimates and concludes that non-experimental data does a poor job in replicating the experimental findings (Lalonde, 1996) (Fraker and Maynard, 1987) (Arceneaux, Gerber, and Green, 2006) (Wilde and Hollister, 2007). Another set of studies has found that non-experimental estimates can perform well relative to experimental data; however, much depends on the comparison group used and model specification (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998) (Dehejia and Wahba, 1999) (Smith and Todd, 2005) (Dehejia, 2005). On the theoretical side, many papers examine the statistical and asymptotic properties of the ATT (Rosenbaum and Rubin, 1983) (Abadie and Imbens, 2002) (Hahn, 1998) (Zhao, 2004) (Abadie and Imbens, 2006).

The literature has tended to focus on propensity score matching because it eliminates the curse of dimensionality (i.e., eliminates matching on numerous variables) and thus is computationally faster than other methods. We expand on the matching literature discussion by comparing seven different matching methods, including propensity score methods. Multiple methods are used to assess whether the estimated impacts are sensitive to the matching method used.

We generate the first and second group by using stratified random sampling at the cell level. The only difference between the two groups is the random seed that determines the candidate picked when there are multiple candidates with exact matches. We refer to these groups as stratified random sample 1 and stratified random sample 2.

We generate the third, fourth, and fifth comparison groups via propensity score matching. These three different samples are generated as a way of addressing tie propensity scores. Smith and Todd (2005) demonstrated that ties could influence the impact estimates obtained; both upward and downward bias is possible. We generate the third and fourth group using two different random seeds to see how samples vary depending on the tie candidate chosen. We generate the fifth comparison group using all the tie candidates and give them proportional weights. We refer to these three samples as propensity score sample 1, propensity score sample 2, and tie-propensity-score sample.[1]

Finally, we create the sixth and seventh comparison groups by minimizing the Mahalanobis distance between the matching covariates and simultaneously apply the Abadie and Imbens (2004) variance and bias-correction. One of the Mahalanobis samples uses a single closest match, while the other uses the five nearest matches. We refer to these samples as the Abadie-Imbens one-neighbor and Abadie-Imbens five-neighbor sample.

Though we do not have the advantage of an experimental impact estimate to use as a benchmark, we determine the extent to which these seven samples, generated by different matching methods, produce similar impact results for the two groups. Specifically we examine impacts on re-employment and wage recovery in the 4th, 8th, and

[1] Each method is described in detail later in the essay

12th quarter after claiming UI. Wage recovery is measured relative to the quarterly wage in the 4th quarter prior to UI claim.

We find that both groups experience higher re-employment rates than their comparison group in the 8th quarter after claiming UI. This suggests that the offer of the ITG voucher helps these groups overcome their barriers to employment. This advantage is consistent across all seven estimation methods, yielding the same relative impact and statistical significance, but the magnitude of the impact estimate varies. For instance, for the high school dropout group, the average re-employment probability ranges from a .08 to .15 re-employment advantage. If we only used the propensity score methods, we would have a smaller estimate range than if we used all methods. Using more methods provides a richer level of detail.

The offer of the ITG voucher has less of an impact on wage recovery. The high school dropout group experiences a higher wage recovery than the comparison group in the 8th and 12th quarter after claiming UI, which is consistent across the seven comparison groups. However, this wage recovery advantage disappears when ITG participants enrolled in truck driving training and their comparison groups are removed from the sample. The older white male group experiences wage recovery levels that are statistically similar to their comparison groups.

Also, consistent with the findings of Smith and Todd (2005), we find that impacts are sensitive to which tie candidate is chosen. For the older white male sample, we find that in the 4th quarter after claiming UI, the wage recovery impact is statistically insignificant when one tie candidate is randomly selected. However, when all tie candidates are used, by way of weighting each so that in sum the tie candidates are equivalent to one person (i.e., the weights sum to one.), the wage recovery impact is statistically significant. These findings indicate the importance of using a variety of methods to estimate the impact of returns to training. Using a spectrum of methods provides an upper- and a lower-bound estimate of the re-employment and wage recovery impacts.

II. Matching Assumptions and The Average Treatment Effect

Research has demonstrated that matching is best used when i) participant and non-participant data is obtained from the same data source, ii) the non-participant comparison group is from same local labor market, and iii) the matching variables are a good proxy for the eligibility criteria, and the matching variables are not influenced by participation (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998) (Michalopoulos, Bloom, and Hill, 2004). Using data on the National JTPA experiment, Heckman et al. (1998) find when such conditions are met and a difference-in-difference estimator is used, propensity score matching is effective in eliminating bias.[2]

All matching models are based on the conditional independence assumption (CIA), also referred to as "selection on observables," which assumes that the training group and comparison group only differ in terms of the variables used for matching (Rosenbaum and Rubin, 1983) (Heckman and Robb, 1985). Therefore, any difference in outcomes can be attributed to training participation. When estimating the mean impact of training (that is, comparing the average participant outcome to the average comparison group outcome) it is enough to assume that any difference in the mean outcome is

[2] Difference-in-difference estimators control for differences in time invariant unobservable between the two groups. Unobservables that are constant over time fall away when examining the difference over time.

attributed to differences in the matching variables. This is a weaker assumption than the CIA because it only applies to the mean not the entire distribution (Heckman et al., 1998).

Therefore, the average treatment effect on the treated (ATT) can be obtained by simply comparing the average outcome of the matched comparison group and the treatment group. More formally it can be expressed as

$$\Delta Y_{ATT} = \frac{\sum_{i=1}^{k1} Yi,1}{K1} - \frac{\sum_{i=1}^{k1} Yi,0}{K1} \qquad (1)$$

$Y_{i,1}$ denotes post-program outcome for the i^{th} individual of the participant group; K_1 denotes number of participant group members; $Y_{i,0}$ denotes post-program outcome for the i^{th} individual of the non-participant group. Since matching is only taking place for the participant group, K_1 will necessarily be the denominator of the second term in one-to-one matching. Equation 1 is equivalent to regression of the outcome (Y) on an indicator variable that equals 1 if the person is a training participant and 0 if the person is in the comparison group.

A second assumption underlying matching is called common support. It requires that one has enough people in the comparison group with similar observable characteristics as the training group. To test for a common support, we will compare the distributions of the propensity scores for the ITG group and the comparison group population. We will compare the maximum, minimum, median, and mean of the distributions. To further ensure that matched comparison groups are similar to the ITG group, we conduct three tests: a chi-square test on the distributions, the Heckman and

Hotz test (1989), and a post-matching propensity score regression. These tests are described in detail later.

Additionally matching models and the resulting ATT implicitly assume that there are no general equilibrium effects of participation. In other words, they assume that the program under analysis does not indirectly affect the non-participant group (Rubin, 1974).[3]

Properties of the ATT

An estimator, such as the ATT, that is both asymptotically consistent and efficient is referred to as an unbiased and efficient estimator. In the case of one-to-one matching where only one of the matching variables is a continuous variable, the average treatment effect is unbiased in large samples, but it is not efficient because the number of matches remains fixed (Abadie and Imbens, 2002) (Imbens, 2004). In practice, a variance can still be obtained. Though it is not efficient, the one-to-one matched estimator provides an estimate of the ATT with minimal bias.

In the case when matching is not one-to-one, but rather one-to-many, there is a trade-off between bias and efficiency. A one-to-many match uses a weighted average of multiple non-participants to serve as a match for a given participant. Using more information, by way of using more than one non-participant, increases the efficiency (lowers the variance) of the estimate but does not lead to asymptotic efficiency.

[3] These assumptions also apply to the Average Treatment Effect (ATE). The ATT is simply the ATE as estimated only for the treated. In other words for the ATT, matches are obtained only for the treatment group, whereas for the ATE, matches are obtained for both the treated and the comparison population. Simultaneously, each treatment unit is matched with the closest comparison unit, and each comparison population candidate is matched with the closest treatment unit. The results discussed in this study focus on the ATT because we are most interested on the program effects on the participants as opposed to the estimated effects on the larger population of UI claimants. This is also the unit of analysis used in prior evaluations of the Individual Training Grant program.

The additional information, however, also increases the bias. Using more information increases the area of the approximation and thus affects the computation of the asymptotic consistency. Intuitively, there is a higher likelihood of a poorer match the more nearest neighbors one uses. This can even be the case with tie propensity scores, because two candidates with the same propensity score do not necessarily have the same covariate values.

Abadie and Imbens (2002, 2006) examine in detail this trade-off between bias and efficiency, and they derive an analytical variance for the average treatment effect. With respect to bias, Abadie and Imbens show that if the matching covariates contain only one continuous variable, then the bias in the average ATT disappears in the limit and is normally distributed. They also show that by increasing the number of matches from 1 to 5, the efficiency of the estimate moves from being 50% higher to only 10% higher than the semi-parametric efficiency bound established by Hahn (1998).

III. Matching Models

The previous "matching-on-observables" assumptions and the general form of the ATT apply to all three models considered in this paper: stratified random sampling at the cell level, propensity score matching, and Abadie-Imbens matching. However, the matching models differ in three main ways. First, the models differ in how they determine which non-participant is the “closest” or “best” match for a given participant. Second, the models differ in the degree to which the resulting ATT estimator is efficient. The Abadie-Imbens five-neighbor sample is more efficient than the others because it uses more matches. Third, the methods differ in how they weigh the observable characteristics used to match the comparison group and the treatment group. Eligibility determination for the ITG program is done on a case-by-case basis, so weighting of the different factors is not known systematically. Therefore, we use the three different estimation methods each of which weights factors differently.

Stratified random sampling at the cell level gives all matching variables equal weight; therefore, this method should be used in cases where the actual assessment process weights eligibility factors equally. Propensity score matching gives more weight to variables that are better predictors of program participation. In instances where the program eligibility process weighs factors differently, propensity score matching would be more appropriate than stratified random sampling. The Mahalanobis matching metric in the Abadie-Imbens algorithm assigns variables with higher variance less weight.

We use these three different matching models to construct seven different comparison groups. Comparing the results yielded by different matched samples allows us to effectively evaluate the robustness of our impact estimates. This study deals exclusively with models that involve matching on covariates or propensity scores. For a full discussion of the ATT estimated using regression, matching, and other methods, see Imbens (2004).

In practice, each method has its own benefits and costs. Table 3.1 provides a comparison of the three methods. Propensity score matching simplifies the matching by rolling the covariates into one weighted score and matching on that score. However, the cost for that convenience is a method that can be sensitive to ties and relies on bootstrapping to obtain standard errors. In contrast, and Abadie-Imbens matching and stratified random sampling match directly on the covariates, but both require relatively more computing time than propensity score matching. The Abadie-Imbens method has the added benefit of having an analytically derived standard error that does not impose a functional form. In contrast, the standard error for the stratified random sample model relies on a linear regression. The remainder of this section reviews each method in detail.

A. Cell-Level Stratified Random Sample

Stratified sampling at the cell level involves dividing the training group into mutually exclusive categories using the matching characteristics. A comparison group is randomly selected such that the number of comparison group individuals in each mutually exclusive category matches the number training participants that fall in that category. For example, if there are three ITG participants who are white females between the age of 37 and 50, with a high school degree, residing in Middlesex County, who were employed in the service industry at the same employer for the twelve quarters prior to claiming UI, and who earned in the top 25% prior to claiming UI, then the resulting comparison group will have approximately three such individuals. In case of ties, when there is more than one person with the same set of characteristics as the ITG participant, then a random sorting of the ties determines which candidate is used. To address the instances of ties, we use two stratified random samples each using a different random seed. We refer to the impacts generated by these two samples as stratified random sample 1 and stratified random sample 2.

In cases where there are no individuals from the comparison population with the same combination of characteristics (i.e., no matches in a cell), a weaker criteria for a match is used. The weaker criteria reduces three of the variables to have fewer categories. Specifically, the education categories are reduced from four to two (high school or less, and some college or more), the race categories go from five to two (white and non-white), and the twenty-one county categories are condensed to three regional variables. We use these variables because they have many possible values, and reducing them to binary values increases the likelihood of finding matches. More formally, this can be stated as follows: For treatment person i, we select a comparison person j such that one of the following conditions applies:

$$X_i^{exact} = X_j^{exact} \quad \text{OR} \quad X_i^{weak} = X_j^{weak} \qquad (3)$$

X^{exact} denotes a vector containing all the matching variables for person i or j. i and j are indices for the participant group and non-participant group, respectively. The matching variables are identified in Section V. X^{weak} denotes a vector containing the matching variables, where the possible values for each variable are smaller than in X^{exact} . If the first condition is not met first, then the second condition must hold. We conduct this process without replacement[4]. Therefore, a given matched comparison group member is only used once.

The variance for the ATT in the stratified random sampling is obtained using the typical variance formula. It can be written in terms of an ordinary least- square model when the regression equation contains only an intercept and an independent variable that captures participation status. In the linear regression model of Y on X, suppose X is 1 for those in group i and 0 for those in group j. Then X's coefficient represents the difference between the mean of Y for group i and the mean for group j. The coefficient is equivalent to the ATT. The standard deviation is computed in the normal fashion by using the estimated residuals. Given the CIA and one-to-one matching, the ATT is as close to unbiased as possible because relatively speaking one match is less biased than using more

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[4] Matching without replacement increases the bias because it reduces the chances of a good match, however it reduces the variance because more observations are used.

than one match. However, the variance of the ATT is not asymptotically efficient given a fixed number of matches (Imbens, 2004).

B. Propensity Score Matching

Matching on propensity scores is analogous to matching directly on characteristics, but propensity scores represent a combination of the matching characteristics that gives more weight to those that better predict participation. Rosenbaum and Rubin (1983) showed that the CIA assumptions are also true for propensity scores. We use one-to-one propensity score matching, which entails finding the comparison group person with the closest propensity score to each treatment group participant. More formally, for treatment person i, we select a comparison person j such that the following distance is minimized:

$$\left|P_i(X) - P_j(X)\right| = Min_{k \in \{j\}}\left\{\left|P_i(X) - P_k(X)\right|\right\} \qquad (5)$$

P(X) denotes the propensity score. It is estimated using a probit model where the dependent variable is participation status and the independent variables are the matching variables identified. The subscripts i and j are indices for the participant group and non-participant group, respectively. k is index assigned to the sub-set of non-participants (j) who are the closest match for each participant.

We choose one-to-one propensity score matching (as opposed to kernel density or multipleneighbor matching) because it is the closest in spirit to cell-level matching. As with the cell-level stratified sampling, we match without replacement. We also impose a common support. Caliendo and Kopeing (2005) examine in detail the practical and theoretical issues of propensity score matching, such as matching with or without replacement. Matching without replacement decreases the variance because more information/observations are used; however, it also increases the bias because eliminating the chance of using an observation twice reduces the chance of a quality match. This trade-off between bias and efficiency is inherent when employing matching methods.

There are two general ways of handling tie propensity scores. Tie propensity scores arise when there exist more than one potential comparison group candidate with the same propensity score as an ITG participant. One option is to randomly select one of the tie comparison candidates to serve as the comparison person. The other is to allow all tie candidates to serve in the comparison but to weight them proportionately so their weights sum to one. We use both methods in this analysis and obtain three comparison groups. We obtain two samples using two different random seeds. By varying the random seed, we essentially ensure that a different tie candidate is chosen in each sample. We refer to these two samples as propensity score sample 1 and propensity score sample 2. Our third propensity score sample includes all tie candidates and weights them proportionately. It is referred to as tie propensity score sample.[5] As noted earlier, by including more matches, the tie propensity score sample has less bias (but a higher variance) than propensity score samples 1 and 2.

In all cases, the variance for the propensity-score-derived ATT is estimated using bootstrapping. Although there is no theoretical justification for bootstrapping, it is widely used in practice. Bootstrapping attempts to numerically estimate the standard error by taking repeated samples, computing the standard error, and then averaging over the samples. Eichler and Lechner (2002) demonstrate that the bootstrapped standard error is comparable to the simple variance esimator. Nonetheless, the variance for the propensity

[5] These estimates will be generate using Leuven and Sianesi's (2003) psmatch2 program for Stata. The standard errors are generated by way of bootstrapping.

score ATT is not efficient for the same reason given earlier: that the number of matches is taken as fixed.

With regard to bias, Abadie and Imbens (2002) show that the ATT is asymptotically unbiased because matching on the propensity score means that there is only one continuous matching variable, thus the bias disappears asymptotically. They further show that if there are two continuous variables, then the bias does not disappear.

C. Abadie-Imbens Matching

In applying the Abadie-Imbens bias adjusted matching algorithm, we determine the closest match using the Mahalanobis distance. Specifically, we select the comparison group candidate that is closest, as measured by the Mahalanobis distance, to a given ITG participant. The Mahalanobis distance measures the distance between two random variables X_i and X_k and scales the difference by the covariance matrix between the two variables. In contrast, the Euclidian distance does not consider the covariance. It is the simple geometric distance between two points in space. The Mahalanobis distance is commonly used in statistics because it gives less weight to those variables with high variances and variables that are highly correlated.

Stated more formally, for treatment person i, we select a comparison person j such that the following distance is minimized:

$$\left| X_i - X_j \right| = Min_{k \in \{j\}} \left\{ \left\| \left[(X_i - X_k)' S^{-1} (X_i - X_k) \right]^{1/2} \right\| \right\}, \quad (6)$$

where X denotes a vector containing all the matching variables for person i or j. The letters “i” and “j” are indices for the participant group and non-participant group, respectively. k is index assigned to the sub-set of non-participants (j) who are the closest match for each participant. S denotes the covariance matrix for the vector X.

Unlike the previous two methods, this method combines the matching with a regression to adjust for any remaining differences in the matched covariates. This is referred to as bias adjustment. Obtaining the bias-adjusted impacts involves estimating a model using the matched comparison group, using that model to predict the outcomes, and then obtaining an estimate of the bias by comparing the actual outcome with the predicted outcome (Abadie et al., 2004). This effectively adjusts the average treatment effect for any remaining differences between the matched sample and the ITG group in terms of the matching variables. Abadie and Imbens (2002, 2006) show that this bias-adjusted ATT is unbiased. With regard to efficiency, they demonstrate that by increasing the number of matches from one to five, the efficiency of the ATT estimate moves from being 50% higher to only 10% higher than the semi-parametric efficiency bound established by Hahn (1998). Moreover, they show that when the number of potential comparison group matches is much larger than the treatment units, then in large samples the bias introduced by increasing the matches disappears in the Average Treatment Effect on the treated (Abadie and Imbens, 2006) (Imbens, 2004). The data used in this essay meet these conditions. Therefore, despite the additional neighbors, the estimated ATT is unbiased for the five-neighbor sample just at is for the one-neighbor sample. However, because of the larger number of matches in the five-neighbor sample, the resulting variance is more efficient than in the one-neighbor sample. We refer to the impacts estimated by these two methods as the Abadie-Imbens 1-neighbor sample and Abadie-Imbens 5-neighbor sample.[6]

IV. Estimating Impact on Labor Market Outcomes

To estimate the economic impact of the ITG program, we will compare ITG participants' re-employment and wage recovery to the rates of each of the matched comparison groups. This is the average treatment effect for ITG participants and is illustrated by Equation 1. We will examine the impact on re-employment and wage recovery at three points in time: 4th, 8th, and 12th quarters (one, two, and three years) after claiming UI. We use these three time periods because a study comparing experimental and non-experimental results has demonstrated the bias in evaluation is less two years after the program than five years after the program (Michalopoulos, Bloom, and Hill, 2004). Wage recovery is measured as the difference between the wage in the 4th quarter prior to claiming UI and each of the post-UI quarters. The wages have been adjusted for inflation using the Consumer Price Index. A person is counted as re-employed if he has positive wages and weeks worked in the given post-unemployment quarter.

All impact estimates obtained from non-experimental matching methodologies face the potential problem of selection bias. Selection bias occurs when the treatment group (in this case the ITG group) and the comparison group differ systematically with respect to some unobserved characteristic that influences wages and re-employment. For instance, suppose we did not have information on prior education, and ITG group members were more educated than comparison group members. Then by not controlling

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[6] These estimates and the analytical variances are obtained using the *nnmatch* program for stata by Abadie, et al. 2004. nnmatch handles ties candidates by weighting them proportionately. For instance, if an ITG participant has 5 potential comparison group candidates with the same matching propensity score, then each of the five is given a weight of 1/5 (.20). Therefore, in sum they are equivalent to one person and thus effectively yield a one-to-one match.

for this difference, we would be unable to distinguish between what part of the impact estimate is due to program participation and what is due to prior education.

We address the possibility of selection bias in two ways. First, we provide a bias-adjusted treatment effect using the Abadie-Imbens matching model. Second, our measure of wage recovery allows us to control for time-invariant differences in unobservable characteristics. When subtracting wages (or any outcome) in quarter *t-1* from wages in quarter *t*, any time-invariant unobservable characteristics fall away in the difference. More specifically:

$$\Delta Y_{dnd} = \frac{1}{N}\sum_{i=1}^{N}\left\{\left(Y_{1,q,i} - Y_{1,qm4,i}\right) - \left(Y_{0,q,i} - Y_{0,qm4,i}\right)\right\} \quad (7)$$

where $Y_{1,q,i}$ denotes post unemployment quarterly wage for members of the ITG group; $Y_{1,qm4,i}$ denotes the quarterly wage in the 4^{th} quarter prior to unemployment for the ITG group. Analogously, those terms subscripted with 0 represent the comparison group wages. N denotes the total number of paired ITG-comparison group matches.

This is analogous to the difference-in-difference estimator proposed by Heckman, Ichimura, and Todd (1997), but we do not have the weight term outside the comparison group difference because we illustrate the case of the one-to-one matching. In one-to-one matching there is no need to weight the comparison group cases. However, in the presence of ties, there is a weight term, and for any given group of tie candidates, these weights will sum to one. This construction is not feasible with the re-employment variable because all ITG and comparison group participants are employed in the quarters prior to becoming unemployed.

V. The Individual Training Grant Program

We compare the matching methods outlined in Section III using data from New Jersey's Individual Training Grant program, a training voucher program for dislocated workers eligible for Unemployment Insurance (UI) benefits. To be eligible for the ITG program, one must first be eligible to claim Unemployment Insurance (UI) in New Jersey. Once a person expresses interest in the ITG program, a job counselor then determines whether the person is eligible for the program by examining his skills and work experience on a case-by-case basis.

To find a comparison group for the 219 ITG participants in the older white male group, we select the seven different matched comparison group from the 11,015 white males age 51-65 who claimed UI but did not participate in the ITG program. We parallel the eligibility determination process by matching on the following six characteristics: previous industry, wage prior to unemployment, prior tenure, education, region of residence, and quarter claim was filed. The first four characteristics capture skill and prior work experience. We match on county of residence to control for regional labor market differences and quarter of claim to control for seasonal variation. For our second sample of 277 high school dropouts, we repeat this process by selecting the seven different comparison groups from the 30,871 high school dropouts previously employed in the manufacturing sector.

Another eligibility criterion used by counselors is the current demand for the type of occupation a participant is seeking. Although our matching variables do not explicitly capture the intended occupation of participants and non-participants, we argue that on average ITG participants and comparison group members will be facing the same distribution of available jobs, especially because they are from the same local labor markets. We expect that the job availability distribution is a proxy for occupation demand. Presuming the comparison group members generally target their search toward those jobs most available, we contend that on average both groups are searching for jobs where the occupation is in demand.

As suggested by existing research, our matching variables attempt to comprehensively parallel the actual eligibility determination process. These matching variables influence both participation and outcomes, but at the same time, these variables are not likely influenced by participation (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998). Additionally, our data minimize measurement error because all the data are from the same source (Heckman, Lalonde, and Smith, 1999).

To obtain information on post-UI re-employment status and wages, these administrative data were merged with Unemployment Insurance wage records obtained from the New Jersey Department of Labor for 1994 through the third quarter of 2002. The wages have been adjusted for inflation using the Consumer Price Index.

Unemployment Insurance wage records consist of quarterly wage information collected from employers covered by the New Jersey Unemployment Compensation Law. It is important to note that not all New Jersey residents who are employed are included in the UI wage database. New Jersey residents who work out of state, are self-employed, are employed by religious organizations, are federal civilian employees, or are military personnel are not included. Therefore, the employment rates and wage recovery reported here are only a measure of employment at employers in New Jersey covered by the UI trust fund. This limitation should not bias the impact estimates as long as the probability of out-of-state employment and self-employment for the ITG and the comparison group is the same. To this end, the comparison group is selected so that the county of residence distribution of the two groups is the same.

VI. Results

A. Match Quality

The critical component of all impact studies is the comparison group because its outcomes serve as the benchmark of what would have happened in the absence of program participation. To assess how similar the seven comparison groups are to the ITG group, Appendix A and Tables 3.2-3.7 illustrate four different measures of the similarity between the ITG group and its comparison groups. Sometimes such tests of similarity are referred to as balancing tests.

First, appendix A provides side-by-side frequency tables of the ITG group's characteristics and each of the 7 comparison groups' characteristics. Tables are presented for both the white male and high school dropout groups. A chi-square test indicates the ITG and comparison group distributions are not statistically different from each other in all cases.

Second, as suggested by Sianesi (2004), Tables 3.2-3.5 show the distribution for the propensity scores for the unmatched comparison group, matched comparison groups, and the ITG group. For both samples (older white males and high school dropouts), the three propensity score methods yield comparison groups with propensity score distributions statistically similar to the ITG propensity score distribution. As displayed in Panel A of Tables 3.2-3.5, all of the matched comparison group samples have mean, median, 5th percentile, and 95th percentile propensity scores that are the same as the ITG group. For instance in Panel A of Table 3.2, the 219 comparison group members that make up propensity score sample 1 have a mean propensity score of .028, which is not statistically different from the .03 mean score of the 219 older white males ITG participants. Similarly in Panel A of Table 3.3, the 277 ITG participants without a high school degree and previously employed in manufacturing have mean propensity score of .03, which is not statistically different from the mean of the comparison group yielded by the propensity score sample 1. The same trends hold for propensity score sample 2 and the tie propensity score sample. The stratified random sample and the Abadie-Imbens are not shown in the table because the methods did not involve propensity score matching.

Third, we conduct a model chi-square test after matching on propensity scores. The chi-square stat is obtained by comparing a model where the only covariate is ITG participation status to one that also includes all the matching covariates. Because the test occurs on the post-matched sample, we expect a high p-value, indicating that matched characteristics would not explain a significant portion of the variation in the propensity score. In contrast, we expect a low p-value for the chi-square model test conducted on the unmatched sample. As expected in both ITG samples, the p-value for the probit model chi-square test (i.e., an F-test) is nearly 1 for all three propensity score matched groups. These p-values are also displayed in Panel B of Tables 3.2-3.5. It indicates that together the observable characteristics do not explain a significant portion of the variation in the propensity score. This is expected because the matching process is supposed to reduce the systematic variation in the propensity score between the ITG group and comparison group. As with the previous test, the stratified random sample and the Abadie-Imbens are not shown in the table because the methods did not involve propensity score matching.

Fourth, the results of the final test of similarity (the Heckman-Hotz test) are presented in Tables 3.6 and 3.7. Heckman and Hotz (1989) proposed testing for any pre-program differences between the ITG group's and the matched comparison group's wages. This is accomplished by examining whether the coefficient on participation in a pre-program wage equation is significantly different from zero. The main limitation of this test is that the absence of pre-program differences does not imply the absence of post-program differences. Nonetheless, it is reliable measure of differences between the two groups.

We conduct this test using two measures of pre-UI wages. First, we use the quarterly wage level in the 4^{th} quarter prior to UI claim, and we find that in all samples (for both the white male and high school dropout group), there is no significant difference between the pre-program wage of the ITG group and each of its seven comparison groups. For instance, in the columns labeled "full" in Table 3.6, all the coefficients are insignificant, indicating that the wages of the two groups are similar in the pre-program period. Second, we use a measure of wage growth, which we define as the difference between the quarterly wage in the 2^{nd} quarter prior and 4^{th} quarter prior to UI claim. We find mixed results with this test. In most cases there was no significant difference between the pre-program wage growth of the ITG group and the comparison group. However, there are some notable exceptions. In the white male sample, the Abadie-Imbens one-neighbor and five-neighbor comparison groups used for measuring employment did not pass the Heckman-Hotz growth test (Table 3.6, Panel A). Similarly, in the high school dropout sample, the propensity score comparison groups used for measuring employment did not pass the test (Table 3.7, Panel A). Also for the high school dropout sample, all but the Abadie-Imbens matched comparison group samples fail the wage growth test in the 4th quarter wage recovery sample.

There are two reasons that minimize the concern produced by the mixed results of the Heckman-Hotz test. First, these exceptions largely occur in samples used to measure re-employment. It is reasonable to assume that pre-unemployment wage growth has little impact on re-employment probabilities because wage growth is conditional on employment. Second, for both ITG sub-groups, the seven comparison groups did pass the Heckman-Hotz level test. This suggests that all comparison groups have similar pre-unemployment wages to the ITG group.

Another concern arising from the Heckman-Hotz wage growth test is the apparent inconsistency. For the older white male group, the two comparison groups yielded by the Abadie-Imbens methods do not pass the wage growth test, whereas the analogous groups for the high school dropout sample do pass the test. That the same test produces different results for the different samples suggests results are also sensitive to the sample chosen. In this case, the two samples have considerably different pre-unemployment wage levels and trajectories. For instance, the sample of older white males used to assess the impact of re-employment in the 4th quarter after claiming UI had an average quarterly wage of $12,610 compared with an average of $7,351 for the sample of high school dropouts previously employed in the manufacturing industry. Additionally the median difference between the quarterly wage in the 2nd quarter prior to UI and the 4th quarter prior is zero for older white male group and $136 for the high school dropout sample.

B. Impact on Re-employment

ITG participation increases the chances of re-employment for both groups although the magnitude of the impact varies. By the 8th quarter after claiming UI, white males age 51-65 experience a statistically significant higher re-employment rate than their comparison group counterpart. However, the advantage dissipates by the 12th quarter after UI claim. This trend is depicted in Panel A of Table 3.8. In Table 3.8, the dependent variables are re-employment in the 4th (Columns 1 and 2), 8th (Columns 3 and 4), and 12th (Columns 5 and 6) quarters. The coefficients (ATTs) in the rows represent the additional probability of re-employment (which could be negative or positive) associated with ITG participation in a given quarter. In Column 3 of Table 3.8, the magnitude of the re-employment advantage in the 8th quarter varies across the comparison groups used. For 5 of the 7 samples displayed in Panel A of Table 3.8, the average ITG treatment effect is between .068 and .0940 in the 8th quarter after UI. For instance, the ITG older white male re-employment advantage amounts to 6.8% when measured against the comparison group yielded by the tie propensity score method. The two exceptions are the ITG effects-yielded comparison groups generated by stratified random sample 1 and stratified random sample 2. These effects are .181 and .105, respectively. This large range illustrates an important lesson: the choice of random seed can yield noticeably different results even when the method is the same. Therefore, it is important to vary the random seed when tie-comparison group candidates are found for any of the participants. Software packages typically have a “tie” option that allows the user to vary the random seed.

High school dropouts formerly employed in the manufacturing sector also experience a re-employment advantage in the 8^{th} quarter after claiming UI, with respect to all comparison groups. This advantage is sustained in the 12^{th} quarter after claiming UI, where the ITG group has an advantage over 5 of the 7 comparison groups. For instance, in Panel B of Table 3.8 the row labeled propensity score sample 1 indicates that in the 8^{th} quarter after claiming UI, the high school dropout group experienced a re-employment rate 9% higher than the comparison group yielded by this method. As with the older white male group, the magnitude of the 8^{th} quarter advantage varies depending on the comparison group used. All but one of the estimated ITG treatment effects falls in the range .078 to .112. The exception is the stratified random sample 1, where the average treatment effect is .148. This, again, illustrates that choice of random seed matters.

Together these results suggest that training improves the chances of re-employment even for groups that face barriers to employment such as having a lower level of education, working in a declining industry such as manufacturing, and looking for a job when one is near retirement age. From a methodological perspective, it is important to note that in all our re-employment estimates, the sign and significance of the coefficient is generally consistent across all the comparison groups; however, there is noticeable variation in the magnitude of the average ITG effect. This variation is expected because no two comparison groups are expected to be the same even in a randomized experiment. That said, the impacts presented here are non-experimental results, so they are still subject to the standard selection bias concerns underlying all non-experimental studies.

C. Impact on Wage Recovery

The second labor market outcome examined is wage recovery. Wage recovery is measured relative to the quarterly wage in the 4th quarter prior to claiming UI. As indicated by the average treatment effects in Panel A of Table 3.9, the older white male ITG group does not experience a wage recovery advantage over the comparison groups in the 4th, 8th, or 12th quarter after claiming UI. This result is not unexpected. Previous evaluations of the ITG program found no overall impact on wages (Hebbar, 2005) (Benus et. al., 1996). Also an evaluation of training obtained through the Federal Trade Adjustment Assistance Program found an imprecise impact of $294 on quarterly wage change in the 12th quarter after claiming UI. The standard error was 270 (Decker and Corson, 1995).[7]

In contrast, the high school dropout group does experience a wage recovery advantage in the 8th quarter after claiming UI relative to 6 of the 7 comparison groups. For instance, in the 8th quarter after claiming UI, this ITG group experiences a $884 greater wage recovery than the comparison group yielded by the propensity score seed 1 method. The advantage is sustained in the 12th quarter after UI. This group experiences an advantage relative to 5 of the 7 comparison groups in the 12th quarter after UI. As with the re-employment effects, the range of the average treatment effect varies across the different comparison groups.

However, the wage recovery advantage for the high school dropout group is largely driven by those participants enrolled in truck-driving training. Compared to the overall

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[7] To estimate the impact for a group of Trade Readjustment Allowance (TRA) recipients that received training, Decker and Corson use a quasi-experimental methodology. They use as their comparison group TRA recipients that did not participate in training. Nearly half (47%) of TRA recipients participated in training. Trade Readjustment Allowances serve as extended UI benefits.

sample, a disproportionate portion of high school dropouts enrolled in truck driving training: a total of 34% of high school dropouts, compared with 8% of all ITG participants. When ITG participants enrolled in truck driving training are removed from the sample, the wage recovery advantage disappears for the high school dropout group though the employment advantage described earlier remains. This suggests that the type of training matters, with respect to wage recovery.

VII. Conclusions

This study examines the re-employment probabilities and wage recovery rates of two New Jersey ITG participant groups which face barriers to employment: having no high school degree, previous employment in manufacturing (a declining industry), and searching for a job when near retirement age. The two groups are 1) white males who were age 51 to 65 when they became unemployed and 2) high school dropouts who were previously employed in manufacturing. Our research demonstrates that the offer of an ITG training voucher increases the re-employment chances of both groups in the 8th quarter after UI. However, it generally has an insignificant impact on wage recovery, a result consistent with the general training evaluation literature.

To assess the impact of ITG participation, the outcomes of these groups are measured against seven separate matched comparison groups. The comparison groups are generated by three different methods: two by stratified random sampling, three by propensity score matching, and two by Abadie-Imbens matching. Using multiple comparison groups helps establish a range of the economic impact estimates.

We find that both ITG groups experience a re-employment advantage in the 8th quarter after UI claim. The advantage continues to the 12th quarter for the high school dropout group but not the older white male group. We also find the high school dropout group experiences wage recovery greater than its comparison groups in the 8th and 12th quarter after UI. However, this result is attributed to the large portion of this group (34%) that enrolled in truck driving training. In 2005, the median wage of truck drivers in New Jersey was higher than that of the median wage of all workers ($18.37 vs. $16.68).[8] When this group and its comparison group are removed, the wage recovery advantage disappears.

In using multiple comparison groups, we find that the magnitude of the estimated impacts varies depending on which comparison group is used as a reference point. For example, in the 8th quarter after claiming UI, the high school dropout group has a 7.8% higher re-employment rate than the comparison group generated by the Abadie-Imbens five-neighbor method and a 14.8% advantage when compared to the comparison group yielded by the stratified random sampling seed 1 method. The remaining impacts fall within these bounds. The bounds for the older white male group's reemployment advantage in the 8th quarter after claiming UI are 6.8% to 18.1%.

Prior to embarking on any non-experimental evaluation, it is important to note that all impacts obtained via non-experimental program evaluations, such as the one examined here, are limited by the possibility of selection bias. To minimize selection bias, evaluation strategies must include matching variables which thoroughly characterize program eligibility and the outcomes to be evaluated but are not likely influenced by participation. In the case of estimating impacts on employment related outcomes the better the variables describe previous work history and skills of the sample, the more

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[8] U.S. Bureau of Labor Statistics: May 2005, New Jersey State Occupational Employment and Wage Estimates

likely it is to reduce the presence of selection bias. By including in our matching variables education, pre-unemployment tenure, pre-unemployment wage distribution, and pre-unemployment industry, we have done our best to capture the previous work experience of our sample.

Second, evaluation strategies should also include tests to assess the similarity of the matched comparison groups to the treatment sample. These are sometimes referred to as balancing tests. Our tests of match quality tend to indicate similarity between the comparison groups and ITG groups; however, the Heckman-Hotz test on wage growth indicates some dissimilarity. For instance, the Heckman-Hotz test on wage growth indicates that there is a pre-program difference between the older white male ITG group and the Abadie-Imbens comparison groups, but this is not so for the high school dropout group. This dissimilarity demonstrates that methods are sensitive to the underlying sample. In this case our two samples have dissimilar pre-unemployment wage histories. The older white male sample experiences an average decline of $23 in quarterly wages between the 2nd and 4th quarter prior to claiming UI, and average quarterly wages are $12,610 in the 4th quarter prior to UI. In contrast, the sample of high school dropouts previously employed in manufacturing experiences an average increase of $136 in quarterly wages between the 2nd and 4th quarter prior to claiming UI. Average quarterly wages are $7,351 in the 4th quarter prior to UI. The matching variables used in this study do not control for differences in pre-unemployment wage growth trajectories but do control for pre-unemployment wage levels. Future work will match directly on both wage growth and wage level prior to unemployment to address this issue.

Despite these limitations, our results also show that the statistical significance of the impacts can be sensitive to the tiecandidate chosen. For the older white male sample, we find that in the 4th quarter after claiming UI, the wage recovery impact is statistically insignificant when one tie candidate is randomly selected. However, when all tie candidates are used, the wage recovery impact is statistically significant. Together these results suggest that utilizing more than one matched-comparison group is an important way to determine whether impact results are robust (in sign, significance, and magnitude) to the choice of comparison group. In the case of tie propensity scores, it is advisable to examine whether the results are sensitive to the tie candidate chosen.

Theoretically, the Abadie-Imbens matching algorithm seems to be the most appealing for three main reasons. First, it includes a technique to adjust for bias resulting from remaining differences in the matching covariates. Second, the variance used in the method does not rely on bootstrapping. Third, when the comparison population is sufficiently larger than the treatment population, one can increase the efficiency of the estimated ATT, without an increase in bias. However, this method is limited by its large computational cost in large samples.[9] One possible strategy is to estimate program impacts for sub-sample based on demographic characteristics. We effectively used this strategy on a small scale by using two narrowly defined sub-samples of ITG program participants.

Practically speaking, the three methods tend to produce similar results for our two samples. Therefore, in these samples, propensity score matching used fewer computational resources and yielded similar results to the more computationally involved

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[9] It took 9 hours and 18 minutes to estimate the ATT for 3 outcomes under the following conditions: 1) treatment sample=277 2) comparison populaton=30,871 3)RAM set at 300M 4) processor speed=1.59 Ghz

Abadie-Imbens method. This suggests that propensity score matching can be a computationally cheaper method of arriving at the same result. However, when feasible we recommend using all three methods. Short of that, we recommend using at least two methods to estimate the impacts. This provides a test of the robustness of the results to the matching model used.

Tables

Table 3.1. Pros and Cons for Different Matching Methods

3 Methods	7 Groups	Benefits	Costs
Stratified random sampling	1. stratified random sample seed1 2. stratified random sample seed 2	1. Exact matches 2. no additional adjustment of standard errors from propensity score estimation	1. curse of dimensionality 2. computationally intense
Propensity score matching	3. propensity score seed 1 4. propensity score seed 2 5. propensity score with ties	1. simplified score avoids curse of dimensionality 2. computationally fast	1. no analytical variance, relies on bootstrapping 2. can be sensitive to ties 3. exact score not imply exact matching characteristics
Abadie-Imbens matching	6. one-neighbor 7. five-neighbors	1. Matches directly on covariates 2. analytical variance gets closer to efficient bound when more matches used 3. can adjust for bias	1. computationally intense 2. multiple neighbors introduces some bias

Table 3.2 Propensity Score Distributions
Sample Used For Re-employment 4th, 8th, & 12th Quarter after UI
White males age 51-65 at the time of UI claim

PANEL A Propensity Score Statistics	**ITG sample**	**Unmatched Sample**	**propensity score sample 1**	**propensity score sample 2**	**tie propensity score**
Mean	0.03	0.019[b]	0.028	0.028	0.028
std.dev	0.01	0.0129	0.0110	0.0110	0.0038
5th percentile	0.007	0.002	0.007	0.007	0.007
10th percentile	0.014	0.003	0.014	0.014	0.014
50th percentile	0.028	0.018	0.028	0.028	0.028
90th percentile	0.04	0.041	0.044	0.044	0.044
sample size	219	11,015	219	219	1,846
PANEL B chi-square stat (post-match, F-test)	na	109.40	9.0597	7.6554	19.583
pval	na	0	0.9887	0.9964	0.5478

Notes: “a” indicates comparison group mean is significantly different from mean ITG pscore at the 1% level and ‘b’ indicates significance at 5% level. The propensity score was generated by regressing ITG participation status on the following variables: education, industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim. Sample 1 was generated using random seed=56895 and sample 2 was generated with seed=95 The chi-square stat is obtained by comparing a model where the only covariate is ITG participation stauts to one that also includes all the matching covariates. Because the test occurs on the post-matched sample, we expect a high p-value. The stratified random sample and the Abadie-Imbens are not shown because the did not involve propensity score matching.

Table 3.3 Propensity Score Distributions
Sample Used For Wage Recovery in the 4th, 8th, & 12th Quarter after UI
White males age 51-65 at the time of UI claim

	Wage Recovery in 4th Quarter after UI					Wage Recovery in 8th Quarter after UI					Wage Recovery in 12th Quarter after UI				
PANEL A Propensity Score Statistics	**ITG**	**unmatched**	**PS1**	**PS2**	**PSTie**	**ITG**	**unmatched**	**PS1**	**PS2**	**PSTie**	**ITG**	**unmatched**	**PS1**	**PS2**	**PSTie**
Mean	0.03	0.021[b]	0.029	0.029	0.029	0.03	0.021[b]	0.029	0.029	0.029	0.03	0.021[b]	0.028	0.028	0.028
std.dev	0.01	0.0130	0.0107	0.0107	0.0044	0.01	0.0129	0.0109	0.0109	0.0047	0.01	0.0130	0.0110	0.0110	0.0049
5 percentile	0.009	0.002	0.009	0.009	0.009	0.008	0.002	0.008	0.008	0.008	0.006	0.002	0.006	0.006	0.006
10 percentile	0.016	0.004	0.016	0.016	0.016	0.015	0.004	0.015	0.015	0.015	0.010	0.004	0.010	0.010	0.010
50 percentile	0.029	0.021	0.029	0.029	0.029	0.030	0.021	0.030	0.030	0.030	0.028	0.021	0.028	0.028	0.028
90 percentile	0.041	0.039	0.041	0.041	0.041	0.04	0.039	0.041	0.041	0.041	0.040	0.039	0.040	0.040	0.040
sample size	93	4,404	93	93	544	117	4,253	117	117	621	116	4,096	116	116	587
PANEL B Chi-square stat (post-match, F-test)	na	67.58	7.7295	7.7295	12.984	na	71.88	15.496	15.496	16.538	na	49.67	10.441	11.856	11.149
pval	na	0	0.9962	0.9962	0.9092	na	0	0.7973	0.7973	0.7387	na	0	0.9726	0.9434	0.9598

Notes: PS1 refers to the propensity score matching method using random seed 9549, 95895, 951295 in each time period respectively. PS2 refers to the propensity score matching method using random seed 50395, 90895, 30595. PSTie refers to the propensity score sample where observations with tie propensity scores were weighted. a indicates comparison group mean is significantly different from mean ITG p-score at the 1% level and b indicates significance at 5% level. The propensity score was generated by regressing ITG participation status on the following variables: education, industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim. The chi-square stat is obtained by comparing a model where the only covariate is ITG participation stauts to one that also includes all the matching covariates. Because the test occurs on the post-matched sample, we expect a high p-value.

Table 3.4 Propensity Score Distributions
Sample Used For Re-employment 4th, 12th, & 8th Quarter after UI
Previously employed in manufacturing industry and no high school degree

PANEL A Propensity Score Statistics	Re-employment 4th & 8 Quarter after UI					Re-employment 12th Quarter a		
	ITG	**unmatched**	**PS1**	**PS2**	**PSTie**	**ITG**	**unmatched**	**PS1**
Mean	0.03	0.009**	0.027	0.027	0.027	0.03	0.008**	0.026
std.dev	0.02	0.0122	0.0221	0.0221	0.0215	0.02	0.0120	0.0220
5 percentile	0.003	0.000	0.003	0.003	0.003	0.003	0.000	0.003
10 percentile	0.004	0.001	0.004	0.004	0.004	0.004	0.001	0.004
50 percentile	0.021	0.004	0.021	0.021	0.021	0.020	0.004	0.020
90 percentile	0.058	0.022	0.058	0.058	0.058	0.057	0.022	0.057
sample size	277	30,871	277	277	293	258	29,799	258
PANEL B Chi-square stat (post-match, F-test)	na	336.48	16.525	16.525	16.103	na	315.91	16.055
Pval	na	0	0.7395	0.7395	0.7638	na	0	0.7666

Notes: PS1 refers to the propensity score matching method using random seed=311206. PS2 refers to the propensity score matching method using random seed=98989. PSTie refers to the propensity score sample where observaion with tie propensity scores were weighted. a indicates comparison group mean is significantly different from mean ITG pscore at the 1% level and b indicates significance at 5% level. The propensity score was generated by regressing ITG participation status on the following variables: gender, age, year of UI claim, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim. The chi-square stat is obtained by comparing a model where the only covariate is ITG participation status to one that also includes all the matching covariates. Because the test occurs on the post-matched sample, we expect a high p-value.

Table 3.5 Propensity Score Distributions
Sample Used For Wage Recovery in the 4th, 8th, & 12th Quarter after UI
Previously employed in manufacturing industry and no high school degree

	Wage Recovery in 4th Quarter after UI					Wage Recovery in 8th Quarter after UI					Wage Recovery in 12th Quarter after UI				
PANEL A Propensity Score Statistics	**ITG**	**unmatched**	**PS1**	**PS2**	**PSTie**	**ITG**	**unmatched**	**PS1**	**PS2**	**PSTie**	**ITG**	**unmatched**	**PS1**	**PS2**	**PSTie**
mean	0.03	0.011**	0.029	0.029	0.029	0.03	0.011**	0.030	0.030	0.030	0.03	0.011**	0.029	0.029	0.029
std.dev	0.02	0.0136	0.0233	0.0233	0.0231	0.02	0.0138	0.0250	0.0250	0.0247	0.02	0.0134	0.0243	0.0243	0.0239
5 percentile	0.003	0.001	0.003	0.003	0.003	0.003	0.001	0.003	0.003	0.003	0.003	0.001	0.003	0.003	0.003
10 percentile	0.005	0.001	0.005	0.005	0.005	0.005	0.001	0.005	0.005	0.005	0.005	0.001	0.005	0.005	0.005
50 percentile	0.025	0.006	0.025	0.025	0.025	0.024	0.006	0.024	0.024	0.024	0.024	0.006	0.024	0.024	0.024
90 percentile	0.064	0.027	0.065	0.065	0.064	0.069	0.028	0.069	0.069	0.069	0.069	0.026	0.069	0.069	0.069
sample size	138	12,980	138	138	141	167	12,924	167	167	171	146	11,963	146	146	151
PANEL B															
Chi-sq (post-match, F-test)	na	149.23	9.6056	9.6056	9.6056	na	168.70	13.408	13.408	13.408	na	149.03	17.615	17.615	17.615
Pval	na	0	0.9836	0.9836	0.9836	na	0	0.8937	0.8937	0.8937	na	0	0.6732	0.6732	0.6732

Notes: PS1 refers to the propensity score matching method using random seed=311206. PS2 refers to the propensity score matching method using random seed=98989. PSTie refers to the propensity score sample where observaion with tie propensity scores were weighted. a indicates comparison group mean is significantly different from mean ITG pscore at the 1% level and b indicates significance at 5% level. The propensity score was generated by regressing ITG participation status on the following variables: gender, age, year of UI claim, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim. The chi-square stat is obtained by comparing a model where the only covariate is ITG participation stauts to one that also includes all the matching covariates. Because the test occurs on the post-matched sample, we expect a high p-value.

Table 3.6. Heckman-Hotz Test on Pre-UI wage and wage growth
Sample: White males age 51-65 at the time of UI claim

PANEL A	re-emp 4th, 8th, and 12th quarter after UI			
Group	qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI	
	base	Full	base	full
SR1 Comp	-86.89	-157.65	-10.01	-36.21
	(661.18)	(590.36)	(331.04)	(327.62)
SR2 Comp	270.95	319.22	282.83	197.79
	(658.53)	(595.68)	(339.79)	(333.89)
PS1 Comp	-408.9	-381.83	-58.05	-8.28
	(654.13)	(582.57)	(383.98)	(390.7)
PS2 Comp	-589.37	-622.78	-343.12	-332.59
	(672.81)	(585.37)	(352.94)	(357.42
PS Tie Comp	571.38[c]	125.83	-259.77	-227.34
	(300.38)	(269.84)	(177.16)	(178.73)
ABN1 Comp	-5.46	-348.71	-595.93[c]	-613.05[c]
	(555.83)	(484.02)	(320.7)	(321)
ABN5 Comp	-183.61	-214.18	-503.89[b]	-521.47[b]
	(349.67)	(302.42)	(203.23)	(203.92)

Table 3.6. Continued

PANEL B	wage recovery 4th quarter after UI				Wage recovery 8th quarter after UI				wage recovery 12th quarter after UI			
	qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI		qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI		qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI	
	base	full	base	full	Base	full	base	full	base	full	base	full
SR1 Comp	-121.16	-78.75	-647.65	-605.82	-726.57	-435.27	-376.34	-314.01	-469.74	-427.34	47.14	-74.3
	(924.16)	(884.13)	(485.57)	(505.94)	(935.69)	(877.9)	(479.19)	(501.35)	(874.16)	(814.7)	(490.29)	(510.07)
SR2 Comp	777.77	732.63	29.85	177.83	26.49	455.77	397.72	549.83	92.4	684.88	564.27	469.91
	(894.55)	(837.06)	(514.14)	(525.72)	(883.41)	(832.9)	(486.63)	(502.52)	(833.23)	(774.02)	(465.32)	(461.13)
PS1 Comp	-374.42	-506.8	-683.28	-607.65	-243.82	39.83	-151.87	-10.07	-355.46	-142.65	-375.07	-319.4
	(916.02)	(841.54)	(660.72)	(677.14)	(786.51)	(707.06)	(505.13)	(507.36)	(752.2)	(680.25)	(467.98)	(468.62)
PS2 Comp	-405.19	-586.03	-772.33	-643.12	133.58	385.61	-1035.67[b]	-853.63[c]	-676.61	-500.92	-523.84	-461.43
	(915.69)	(811.66)	(601.24)	(617.41)	(758.39)	(693.78)	(487.53)	(504)	(797.63)	(711.11)	(456.64)	(474.99)
PS Tie Comp	394.71	-80.19	-822.21[a]	-676.79[b]	128.86	228.06	-744.62[a]	-623.74[b]	127.9	133.14	-578.34[b]	-520.04[b]
	(481.06)	(424.67)	(310.54)	(317.73)	(448)	(396.46)	(261.22)	(260.5)	(433.61)	(388.24)	(257.08)	(258.4)
ABN1 Comp	21.62	-8.85	-1351.93[a]	-1257.14[a]	125.26	162.25	-936.54[b]	-931.62[b]	-121.43	97.55	-637.3	-658
	(786.33)	(677.03)	(457.84)	(468.94)	(729.78)	(629.08)	(395.19)	(395.36)	(702.89)	(633.57)	(393.66)	(400.47)
ABN5 Comp	-415.44	-355.92	-592.65[b]	-491.51[c]	-116.75	46.85	-385.84	-351.83	-417.88	-235.16	-336.05	-349.17
	(482.97)	(411.7)	(291.4)	(290.68)	(431.01)	(374.74)	(234.85)	(234.86)	(412.87)	(359.91)	(242.62)	(241.94)

Notes: [a] indicates significance at 1% level. [b] indicates significance at 5% level. [c] indicates significance at 10% level. SR refers to the stratifed random sample comparison group. SR1 refers to the stratified random sampling group using seed=5. SR2 refers to the stratified random sampling group using seed= (PS1 refers to the propensity score matching method using random seed=311206. PS2 refers to the propensity score matching method using random seed=98989. PSTie refers to the propensity score sample where observaion with tie propensity scores were weighted. ABN1 refers to the Abadie-Imbens one-neighbor comparison group matching method. ABN5 refers to the Abadie-Imbens five-neighbor comparison group matching method. The results in the columns labled "with covariates" control for the following factors:itg participation, pre-ui tenure, region, gender, race, age, year and quarter of UI claim. variables for education and industry are not included because the sample is restricted to those with less than high school education previously employed in the manufacturing industry.

Table 3.7. Heckman-Hotz Test on Pre-UI wage and wage growth
Previously employed in manufacturing industry and no high school degree

PANEL A	re-emp 4th & 8th quarter after UI				Re-emp 12th quarter after UI			
Group	qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI		qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI	
	base	Full	base	full	Base	full	base	full
SR1 Comp	-17.53	145.2	296.99	289.51	266.75	319.63	120.39	169.35
	(341.07)	(287.14)	(210.22)	(209.31)	(341.31)	(283.98)	(216.19)	(217.94)
SR2 Comp	-4.18	183.06	263.8	260.08	319.55	383.32	83.57	148.72
	(340.84)	(287.57)	(213.38)	(211.49)	(342.2)	(285.93)	(220.13)	(220.35)
PS1 Comp	114.64	316.76	564.21[a]	506.23[b]	78.33	317.11	488.79[b]	422.99[b]
	(293.97)	(261.27)	(205)	(204.84)	(310.03)	(271.41)	(213.47)	(213.09)
PS2 Comp	103.1	304.67	573.49[a]	514.94[b]	66.02	304.02	498.78[b]	430.67[b]
	(293.71)	(260.94)	(204.48)	(204.32)	(309.75)	(271.08)	(212.9)	(212.55)
PS Tie Comp	112.28	307.29	567.28[a]	505.97[b]	75.81	307.58	492.09[b]	421.65[b]
	(290.97)	(256.94)	(202.1)	(200.95)	(306.64)	(267.02)	(210.22)	(208.77)
ABN1 Comp	187.54	277.31	174.79	150.76	188.68	263.67	127.37	108.72
	(273.6)	(230.35)	(165.5)	(162.6)	(283.18)	(237.68)	(172.61)	(169.9)
ABN5 Comp	118.9	339.88[b]	79.69	55.82	154.1	355.25[b]	35.73	15.49
	(173.65)	(143.55)	(110.18)	(107.75)	(179.43)	(147.19)	(115.72)	(113.23)

Table 3.7. Continued.

PANEL B	wage recovery 4th quarter after UI				wage recovery 8th quarter after UI				wage recovery 12th quarter after UI			
	qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI		qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI		qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI	
	base	full	base	full	Base	full	base	full	base	full	base	full
SR1 Comp	68.82	121.37	738.71[a]	709.59[a]	404.28	280.64	356.06	318.5	448.59	508.19	230.7	277.44
	(478.75)	(393.51)	(275.77)	(264.18)	(464.11)	(387.92)	(273.39)	(267.35)	(439.97)	(358.88)	(290.46)	(289.65)
SR2 Comp	85.72	178.27	704.19[b]	593.2[b]	219.13	182.77	360.71	334.07	403.87	413.2	297.15	354.4
	(476.28)	(392.51)	(282.44)	(276.07)	(453.01)	(380.91)	(272.99)	(265.31)	(439.78)	(357.62)	(293.97)	(292.7)
PS1 Comp	330.03	218.49	420.71	451.13[c]	154.03	324.89	405.55	248.56	55.99	128.29	297.36	392.29
	(416.86)	(340.12)	(273.8)	(268.5)	(389.45)	(330.64)	(264.22)	(259.33)	(415.64)	(332.13)	(275.14)	(275.4)
PS2 Comp	334.42	222.88	458.75[c]	491.96[c]	149.36	319.09	415.55	256	38.37	111.83	354.89	430.03
	(416.97)	(340.19)	(275.25)	(270.68)	(389.3)	(330.09)	(266.17)	(261.18)	(414.75)	(330.72)	(270.24)	(269.71)
PS Tie Comp	327.6	224.48	434.76	460.17[c]	147.87	315.75	406.39	250.63	43.89	106.72	324.79	410.76
	(414.97)	(338.49)	(273.04)	(266.46)	(387.27)	(329.34)	(263.58)	(259.01)	(411.33)	(329.18)	(269.96)	(267.63)
ABN1 Comp	360.41	445.26	236.73	150.11	246.05	339.43	156.91	127.73	321.65	444.69	130.74	114.28
	(402.44)	(319.59)	(252.46)	(239.87)	(383.1)	(316.06)	(214.5)	(207.97)	(380.1)	(301.49)	(234.58)	(231.95)
ABN5 Comp	206.61	380.44[b]	241.3	186.89	273.11	497.16[a]	-11.56	-53.61	256.09	466.54[b]	34.51	13.08
	(250.58)	(193.38)	(154.48)	(147.45)	(225.73)	(182.41)	(141.36)	(136.54)	(237.2)	(187.02)	(152.82)	(146.45)

Notes: [a] indicates significance at 1% level. [b] indicates significance at 5% level. [c] indicates significance at 10% level. SR refers to the stratifed random sample comparison group. SR1 refers to the stratified random sampling group using seed=5. SR2 refers to the stratified random sampling group using seed= (PS1 refers to the propensity score matching method using random seed=311206. PS2 refers to the propensity score matching method using random seed=98989. PSTie refers to the propensity score sample where observaion with tie propensity scores were weighted. ABN1 refers to the Abadie-Imbens one-neighbor comparison group matching method. ABN5 refers to the Abadie-Imbens five-neighbor comparison group matching method. The results in the columns labled "with covariates" control for the following factors:itg participation, pre-ui tenure, region, gender, race, age, year and quarter of UI claim. variables for education and industry are not included because the sample is restricted to those with less than high school education previously employed in the manufacturing industry.

Table 3.8. Average Treatment Effect for the Treated on Re-employment
Dependent Variable=Reemployment after UI Claim

PANEL A Sample: White males age 51-65 at the time of UI claim

	4th qt after UI Claim		8th qt after UI Claim		12th qt after UI Claim	
Estimation Method	Avg. treatment effect on ITG	N ^	Avg. treatment effect on ITG	N^	Avg. treat-ment Effect on ITG	N ^
Stratified sample 1	0.02	418	0.181^{a}	418	0.103^{b}	418
	(0.049)	418	(0.048)	418	(0.048)	418
Stratified sample 2	-0.022	418	$.105^{b}$	418	.057	418
	(0.049)	418	.048	418	.049	418
One-to-one propensity score sample 1	-0.037	438	0.082^{c}	438	0.064	438
	(0.046)	11,015	(0.048)	11,015	(0.045)	11,015
One-to-one propensity score sample 2	0.027	438	0.078^{c}	438	0.068	438
	(0.045)	11,015	(0.045)	11,015	(0.044)	11,015
Tie propensity score	-0.032	2,065	0.068^{c}	2,065	0.057	2,065
	(0.04)	11,015	(0.039)	11,015	(0.041)	11,015
Abadie-Imbens, 1 neighbor	-0.028	3,183	0.093^{b}	3,183	0.0842^{c}	3,183
	(0.0439)	3,183	(0.043)	3,183	(0.0446)	3,183
Abadie-Imbens, 5 neighbors	-0.046	3,747	0.0940^{a}	3,747	0.0553	3,747
	(0.0358)	3,747	(.0351)	3,747	(.0375)	3,747

PANEL B Sample: Previously employed in manufacturing industry and no high school degree

Stratified sample 1	-0.027	528	0.148^{a}	528	0.079^{c}	477
	(0.042)	528	(0.041)	528	(0.044)	477
Stratified sample 2	-0.043	528	0.112^{a}	528	0.075^{c}	479
	(0.42)	528	(0.041)	528	0.044	479
One-to-one propensity score sample 1	-0.022	554	0.09^{b}	554	0.089^{c}	516
	(0.038)	31,148	(0.04)	31,148	(0.046)	31,148
One-to-one propensity score sample 2	-0.011	554	0.112^{a}	554	0.093^{b}	516
	(0.043)	31,148	(0.039)	31,148	(0.043)	31,148
tie propensity score	-0.015	570	0.101^{a}	570	0.089^{b}	531
	(0.038)	31,148	(0.037)	31,148	(0.042)	31,148
Abadie-Imbens, 1 neighbor	0.021	2514	0.111^{a}	2514	0.060	2427
	(0.039)	2,514	(0.037)	2,514	(0.039)	2,427
Abadie-Imbens, 5 neighbors	-0.021	3305	0.078^{a}	3305	0.043	3172
	(0.032)	3,305	(0.030)	3,305	(0.32)	3,172

Notes: a indicates significance at 1% level. b indicates significance at 5% level. c indicates significance at 10% level. ^The larger sample size for the propensity score standard error is the sample size for the bootstrapped standard errors. The matching variables are education, age, race, age and, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim. industry prior to unemployment, tenure prior to unemployment, wage quartile prior to UI, region, and quarter of UI Claim.

Table 3.9. Average Treatment Effect for the Treated on Wage Recovery
Dependent Variable=qt wage *after UI Claim* - qt wage in 4th qt. *Prior to UI Claim*

PANEL A Sample: White males age 51-65 at the time of UI claim

	4th qt after UI Claim		8th qt after UI Claim		12th qt after UI Claim	
Estimation Method	Avg. treatment effect on ITG	N^	Avg. treatment Effect on ITG	N	Avg. treatment effect on ITG	N
Stratified sample 1	-541.45	178	-469.28	188	-277.75	195
	(842.9922)	178	(910.8055)	188	(862.48)	195
Stratified sample 2	-1264.47	180	408.72	194	-172.44	200
	(766.68)	180	(778.31)	194	(851.52)	200
One-to-one propensity score sample 1	-1298.27	186	-287.42	234	-471.66	232
	(800.184)	11,015	(772.863)	11,015	(729.866)	11,015
One-to-one propensity score sample 2	-710.05	186	-710.05	234	562.72	232
	(675.513)	11,015	(675.513)	11,015	(805.62)	11,015
Tie propensity score	-1527.17[b]	637	-835.10	738	-254.97	703
	(637.689)	11,015	(596.75)	11,015	(703.631)	11,015
Abadie-Imbens, 1 neighbor	-1756.97[a]	847	-1101.47	919	-643.82	863
	(654.73)	847	(687.24)	919	(673.22)	863
Abadie-Imbens, 5 neighbors	-772.44	1,000	-282.09	1,178	-40.77	1,143
	(503.12)	1,000	(535.21)	1,178	(556.40)	1,143

PANEL B Sample: Previously employed in manufacturing industry and no high school degree

Estimation Method						
Stratified sample 1	360.74	277	524.86	298	493.39	254
	(457.804)	277	(421.388)	298	(414.16)	254
Stratified sample 2	225.76	279	908.58[b]	302	543.66	256
	(451.46)	279	(415.44)	302	(427.40)	256
One-to-one propensity score sample 1	9.97	276	884.35[b]	334	821.79[c]	292
	(423.126)	31,148	(402.143)	31,148	(423.194)	31,148
one-to-one propensity score sample 2	29.52	276	872.21[c]	334	880.71[b]	292
	(458.513)	31,148	(448.241)	31,148	(434.35)	31,148
Tie propensity score	24.71	279	871.03[b]	338	856.15[b]	297
	(425.343)	31,148	(374.212)	31,148	(360.601)	31,148
Abadie-Imbens, 1 neighbor	426.22	763	1153.29[a]	914	952.92[a]	789
	(355.663)	763	(332.17)	914	(326.63)	789
Abadie-Imbens, 5 neighbors	46.82	1216	684.84[b]	1504	733.14[b]	1263
	(305.21)	1,216	(279.11)	1,504	(287.16)	1,263

Notes: a indicates significance at 1% level. b indicates significance at 5% level. c indicates significance at 10% level. ^The larger sample size for the propensity score standard error is the sample size used to generate the bootstrapped standard errors. The matching variables used for all methods are education, age, race, age and, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim.

Chapter 4

The Monetary Returns to the Individual Training Grant Program

Abstract

This study compares the projected return to training obtained through the New Jersey Individual Training Grant (ITG) program to the opportunity cost of wages lost while enrolled in training. When considering the joint effect of the re-employment and wage recovery effect, we find an overall 9.5% positive expected return to training in the 8th quarter after unemployment. Presuming these returns continue at a constant rate, the expected gain in lifetime earnings exceed the foregone earnings cost by the 5th quarter after UI. Presuming the return diminishes at 20% a quarter, the cross-over point occurs at the 13th year after UI.

However, conditional on re-employment, we find no positive return to training through the 12th quarter after unemployment. This suggests the importance of the reemployment advantage associated with ITG participation.

Relying on the estimate of the return to training that is conditional on re-employment, we estimate the net return for the 32 gender-age-education groups. We examine these groups because of the expected aging of the U.S. population. We find that five groups experience significantly higher quarterly wages than their comparison groups, and their increase in lifetime earnings outweighs the private cost of training. We estimate that the female high school group, age 50 to 54, experience a 4.8% increase in wages in the 8th quarter after training, and by the 11th year the cumulative return to training exceeds the foregone earnings. Assuming retirement at 65, the average increase in lifetime earnings is $313 greater than the foregone wages. The other four groups experience similar and some times higher net benefits from training.

I. Introduction

In recent years the probability of being displaced has increased the most for older workers. The job loss rate for workers age 55 to 64 increased from 9.4% in the three years from 1999-2001 to 10.8% in 2001-2003, a 1.4 percentage point increase. In contrast, the job loss rate decreased for workers age 20-24 and increased less than 15% for other age groups (Farber, 2005). Structural changes in the economy are largely responsible for these job losses; therefore, many workers will switch to new careers. Changing careers involves costs like the loss of human capital, of experience gained in one's previous career, and of time and money involved in obtaining training for a new job. Younger workers have a longer time to recover from such costs than older workers. Further, Chan and Stevens (2001) find that the chances of re-employment decrease with age, and O'Leary and Eberts (2007) find that upon reemployment earnings recovery is lower for older workers than younger workers. These trends raise two important questions: 1) What are the economic returns to training for older workers? and 2) At what point after displacement do the returns to training exceed the cost of foregone wages while in training?

We answer these two questions using data from New Jersey's Individual Training Grant (ITG) program for dislocated workers. Research by Jacobson, Lalonde, and Sullivan (2004) estimates that the net return (conditional on reemployment) to training for male dislocated workers 35 and over is 5.1% and falls to 2.6% under slightly different assumptions that exclude what they call "the showing up effect." We expand on these estimates by demonstrating the extent to which returns to training for dislocated workers are sensitive to 1) prior education and 2) retirement age. We examine impacts by prior education because research suggests that training has differential effects on those with different levels of education (Murnane et al., 1999) (Whittaker, 2002) (Hebbar, 2006). Additionally this is the first time an economic returns calculation is being estimated for the ITG program. Two prior evaluations of the ITG program did not include any comparisons of the cost and economic returns to training (Benus, et al. 1996) (Van Horn, et al., 2000).

In the absence of an experimental design, this study uses propensity score matching to estimate the impact of the ITG program (if any) on post-unemployment wage levels. We estimate the ITG impacts for the whole sample and for 32 gender-age-education groups. We arrive at 32 using 2 gender groups, 4 age groups, and 4 education groups. Then in the instances where we observe an economically meaningful and statistically significant return, we proceed to an economic returns analysis. We compute the returns two different ways. First, we construct the returns, irrespective of re-employment status. Second, we construct returns that are conditional on re-employment. This first method factors in both the potential gain from faster re-employment and wage recovery, and can be thought of as an expected economic return.

We find that the expected earnings, the joint reemployment and wage returns to ITG participant in the 8th quarter after UI claim are sufficiently high (approximately 9.5%) that the average expected gain in lifetime earnings exceeds foregone earnings by the 5th year after claiming UI. If we assume the return diminishes at a rate of 10% per year, the cross-over point occurs at 8th year after UI. If we assume a 20% depreciation rate, the cross-over point occurs at the 13th year. No positive returns to training are observed when we condition on re-employment, thus illustrating the strength of the reemployment gain associated with the offer of an ITG voucher.

For our subgroup analysis we rely on the returns estimated by the second method, (conditional on reemployment) because we examine groups by age and cannot observe a person's retirement status. Our measure for reemployment relies on observing non-zero wages, however older workers may have zero wages because they are retired. Therefore our subgroup analysis conditions on employment. It is plausible that the ITG group and comparison group have equal likelihood of retirement, therefore we also computed the unconditional estimates (i.e. expected earnings method). However, there was no substantial difference in the subgroup results. Still given our selection bias concerns, we report the conditional estimates. We find five of the 32 groups experience a statistically positive wage impact. These five groups amount to 25% of the sample. This is consistent with past studies of the ITG program and other dislocated worker studies, which find training has a limited impact on wages (Benus et al., 1996) (Whittaker, 2002) (Corson and Haimson, 1996) (Decker and Corson, 1995). Two of the groups that experience positive wage impacts are older: female high school graduates who are age 50-54 and those age 60-64 at the time of claiming Unemployment Insurance (UI). The remaining three groups are all younger groups, age 18-49 at the time of UI, and have a high school education or less.[1]

For all five groups, the gain in lifetime earnings resulting from training exceeds the cost of foregone earnings while in training and private costs not covered by the ITG

[1] The estimated earnings method yields a slightly different set of five subgroups with a positive return in the 8th quarter. Two male groups are males age 18-19 with less than a high school education and male high school graduates age 60-65. Three female high school graduate groups are those age 18-49, age 50-54, and age 55-59.

grant. This is the case for scenarios when we assume retirement at age 65, 70, and 73, with the exception of the "female age 60-65" group, where the benefits exceed cost only in the age 70 and 73 scenarios. These estimates should be treated as back-of-the-envelope calculations, especially since they only account for private returns and benefits. To incorporate social costs and benefits of training, we would have to account for the deadweight loss due to the taxes used to finance the ITG program and the increased tax revenues resulting from any gains resulting from ITG (e.g. income tax). Nonetheless, these results suggest that there can be lifetime returns to training for the unemployed among both older and younger workers.

The next section describes the ITG program. Section III describes the methodology and data used to estimate the impact of the ITG program and the cost-benefit calculations. Section IV details the results, and section V concludes.

II. The Individual Training Grant Program

The Individual Training Grant program provides a training voucher to eligible people claiming Unemployment Insurance (UI) in New Jersey. Participants can use the voucher at any state-approved school. During the period covered by this evaluation (1995-1999), there were 100-2000 approved schools. Eligibility for the voucher is determined on a case-by-case basis by counselors at the local workforce services office. Counselors first determine that the person is eligible or currently claiming UI benefits in New Jersey. Then the counselor considers factors such as a person's previous work experience, occupation, tenure prior to unemployment, education level, and whether the job the candidate is seeking is currently in demand.

This last criteria is determined by examining the New Jersey "in-demand occupation list." The New Jersey Department of Labor generates this list by comparing the projected occupational growth to the number of graduates from New Jersey colleges in the corresponding field. Those where demand is greater than supply are on the list. However, if a local area determines the list is incomplete, it can appeal to the state to have an occupation added. For instance in Atlantic City, there are casino related jobs that have in the past been added to the list at the request of a local area.

The maximum denomination of a voucher is $4,000 today and was the same during the study period. The average grant amount for our sample of 16,001 ITG participants who claimed UI between 1995-1999 is $3,864.[2] In 2004, the average grant amount could purchase approximately 7.36 credits at DeVry University, a private training school in New Jersey. These credits amount to 10-11% of the credits needed for an associates degree in Web graphic design, accounting technology, or health information technology. The same amount of money can purchase approximately 27 credit hours at a New Jersey community college, which amount to 37-40% of an associates degree in dental hygiene, accounting, or telecommunications networks. Approximately 75% of participants used their voucher at a private proprietary training provider, 19% of participants used it at a community college, and the remaining 6% used it at a community service organization, government agency, or union organization.

Although the grant covers the cost of training, the participants still bear the opportunity cost of training. The opportunity costs amount to the wages they would have

[2] Between 1995 and 1999 approximately 17,552 obtained an ITG voucher. The ITG sample size falls to 16,581 after removing those with missing demographic data. It falls to 16,001 after we drop native Americans and Asians. We drop these groups because these groups represent a small portion of the comparison group and finding near matches for them will be difficult. Further we do it to remain consistent with previous work where we used this restriction to reduce the dimension of the exact matching procedure.

earned had their continued job searching resulted in a new job. However, instead of continuous job searching, the ITG participants enter training where presumably not all their day can be spent job searching. The economic returns methodology outlined later estimates these foregone earnings, which are then used to determine the cost of entering training. To obtain the net economic return, we also need to estimate the monetary gain, if any, resulting from the training itself.

III. Methodology

A. Estimating the Impact of Training on Wages

To estimate the monetary impact of training, we need an estimate of what wage outcomes would have been in the absence of participating in the ITG program. To estimate this counterfactual, we use one-to-one propensity score matching to obtain a comparison group with similar observable characteristics as the ITG group, but who did not participate in the program. Propensity score matching, and matching in general, relies on two key assumptions: the conditional independence assumption (CIA) and the common support assumption (Rosenbaum and Rubin, 1983).

The CIA assumption assumes that conditional on finding a comparison group with similar observable characteristics, the difference in the mean outcomes between the two groups can be attributed to the offer of the ITG training voucher. This difference is sometimes referred to as the Average Treatment Effect of the Treated (ATT). In our case the outcome is wages, and the ATT can be specified as follows.

$$\Delta Y_{ATT_q} = \frac{\sum_{i=1}^{k1} Y_{i,q,1}}{K1} - \frac{\sum_{i=0}^{ko} Y_{i,q,0}}{K0} \qquad (1)$$

where $Y_{i,q,1}$ denotes the quarterly wage for members of the participant group in the "qth" quarter after UI Claim; K_1 denotes the number of participant group members; $Y_{i,q,0}$ denotes the quarterly wage for members of the comparison group in the "qth" quarter after UI Claim; and K_0 denotes the number of comparison group members. The "qth" quarter is the 4th, 8th, and 12th quarter after claiming UI. Hebbar (2006) examines the intervening quarters.

Strictly speaking the ATT is measuring the impact of the offer of the ITG voucher because we cannot exclude the possibility that the comparison group obtains training. However the likelihood is low that the comparison group would obtain training elsewhere because those receiving UI benefits are not supposed to be engaged in full-time training. Their time is supposed to be focused on job searching. The state has audit mechanisms in place that randomly check to see that people are conducting job searches and not enrolled in training.

To test the CIA assumption, we perform three tests. First we simply test that the two groups are alike in terms of the matching variables. As noted in Tables 1 and 2, the characteristics of the two groups are statistically indistinguishable from one another.[3] Second, we use a test proposed by Heckman and Hotz (1989) which involves comparing the pre-program wages of the two groups using a standard regression model. The dependent variable is pre-program wages, and the independent variable is ITG participation status. An insignificant coefficient on the ITG participation variable would suggest there are no pre-program differences in the wages of the two groups though this does not imply no post-program differences exist. It is another way to test the similarity

[3] Because of space constraints, we only display the means for the group of workers re-employed in the 8th quarter after claiming UI. However, the covariates match similarly for the other outcome samples.

of the groups. Third, we repeat the Heckman and Hotz test using change in pre-program wages as the dependent variable. The change is measured as the difference in the wage in the 2nd quarter prior to claiming UI and the 4th quarter prior to claiming UI. This tests the similarity in the pre-program wage trajectories of the two groups. Table 4.3 provides the results for both pre-program wage tests, and it shows that 8 of the 32 groups do not pass one or both of the Heckman-Hotz tests. This has the effect of weakening our results and points to the shortcomings of non-experimental methods. However, only one of these eight groups corresponds with the five groups for which we find positive wage impacts. The one group is females without a high school degree and age 18-49 at the time of UI claim. This group did not pass the Heckman-Hotz test on pre-UI wage growth without covariates, but when covariates were included, there was no significant difference between the pre-unemployment wage growth of the ITG and comparison groups. Further, the group did pass both of the Heckman-Hotz tests on wage levels. Despite these shortcomings, our results still show that the majority of the 32 groups do pass both Heckman-Hotz tests.

The common support assumption presumes that there are enough people in the comparison group with similar observable characteristics as the training group. We impose a common support by restricting the comparison group propensity score range to be within the ITG group propensity score range. We have the advantage of a large and diverse comparison group; therefore, in practice the majority of comparison group observations are on the support (i.e. within this range). In the case where multiple comparison group members have identical propensity scores that match an ITG participant score, we use all the tie-candidates. For instance, in the case of three-tie candidates, each would carry a weight of one-third.

B. Using Administrative Data to Matching on Observable Characteristics

Administrative data on the ITG program comes from the New Jersey Department of Labor. When individuals initially sign up for the ITG program, they provide the counselor with basic demographic data such as age, race, and prior education. When the participants decide on the type of training, this information is added to the file. Administrative data is also obtained from the UI claimant data base, which includes information on prior industry. These administrative data are merged with state wage records to obtain pre- and post-unemployment wage histories. Given the data available, we generate our propensity scores using six matching variables that approximate the actual eligibility determination process used by counselors. The six characteristics are pre-unemployment industry, tenure prior to losing job, pre-unemployment wage quartile, race, year and quarter of UI claim, and county-of-residence. Though county of residence and race are not considered by counselors in determining eligibility, we match on them for three reasons. First, research has demonstrated that outcomes vary by race (Blinder, 1973) (Oaxaca and Ransom, 1994) (Rodgers, 1997). Second, because our wage data does not include information on out-of-state employment, we match on county of residence. This helps ensure that the portion of people living in counties bordering other states is similar for both groups, which has the effect of ensuring the probability of out-of-state employment is similar. Third, research has shown that a non-participant comparison group from same local labor market perform better than groups from different regions (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998) (Michalopoulos, Bloom, and Hill, 2004).

We first match on propensity scores for the entire 16,001 participant sample. Consequently we obtain a comparison group of 16,001 non-participants. Second we divide the sample into 32 mutually exclusive age-gender-education groups and then match on propensity scores. We conduct the sub-group matching for two main reasons. First, we stratify based on gender and education because employment outcomes generally tend to differ across gender and education levels. Second, we stratify based on age since our economic return analysis depends on our assumed retirement age. We use four age categories that focus on older workers. The first category group is workers age 18-49 at the time of displacement. We refer to this group as "younger workers" because the literature on "older workers" generally considers workers over 50 as old. Those age 50-54 at the time of displacement constitute the second group. This older group is the least likely to retire as a result of job loss (Wadner and O'Leary, 2000) (Chan and Huff Stevens, 2001). The third group consists of those age 55-59, and the fourth group contains those age 60-65. A fifth group, age 65 and older, is only 1% of the sample and is at what is generally considered retirement age and therefore not included in this age analysis.

C. Selection Bias Concerns

In addition to the usual selection bias concerns of non-experimental methods, we also encounter a second type. Examining results by age introduces potential selection bias. Those older participants (age 50 and over) who take up training are presumably less likely to retire. Entering training could be an implicit signal for delayed retirement. Since we do not have data on retirement status, we cannot confidently attribute differentials in employment rates among older workers solely to program participation because part of that difference could be due to a larger percentage of the non-training group retiring. Therefore, for our subgroup analysis that includes age, we condition our analysis on those who are already employed and examine return to training only for those who are employed.

D. Comparing the Estimated Cost and Returns to Training

Broadly speaking, the economic returns analysis compares the estimated lifetime gains from training to the cost of training for the whole sample and each of the 32 subgroups. To estimate the gains, we compute the estimated increase in the present value of expected lifetime earnings yielded from the ITG training program. Kane and Rouse (1999) and Jacobson, Lalonde, and Sullivan (2004) use this general approach to approximate the returns to community college. We estimate the cost as the foregone earnings while in training plus $500 to account for other training-related expenses such as books. We estimate the $500 based on the average amount college students spend on textbooks per year. A study conducted in New York estimates that in 2003, college students spent an average of $922 on textbooks (Schumer, 2004). Another study estimates that the cost of textbooks has risen 40% between 1997 and 2004 (California Public Research Interest Group, 2004). Therefore, using this growth rate, the $922 is equivalent to $658 in 1997, the mid-point of our study period. Given that the average length of training for ITG participants is about four months, we estimate the cost to be slightly lower than $658 at $500.

Since we are computing the private out-of-pocket costs, we don't include the value of the training grant here. To estimate the social return, we'd have to factor in the average ITG grant amount plus the deadweight loss due to the additional taxation used to generate funds for the ITG program. A general limitation of our economic return calculations is that they fail to account for non-monetary benefits from training, such as boosts to self-confidence and job-networking access.

Present Value of Lifetime Earnings

To compute the present value of expected lifetime earnings, we must define a time period, assume a discount rate (to account for inflation and growth), and estimate rate of return to training. Three time periods are used, which assume someone retires at age 65, 70, and 73, respectively. We assume a 4% discount rate as Jacobson et al. (2004) did. Further, this rate is between the average federal funds rate over the study period (4%) and the average rate for a 10-year Treasury Bill (5.2%). Also the rate on the 10-year bill has been declining since 2000.

The rate of return to training is derived from the ATT, determined using Equation 1. For instance, if the ATT in the 8th quarter after claiming Unemployment Insurance (UI) was \$326, and the average ITG participant wage in the 8th quarter after UI was \$6,772, then the rate of return on would be 4.8% (\$326/\$6,772). In the formal model below, a is analogous to 4.8% in this example. So 4.8% of expected lifetime earnings would represent the gain from training.

We estimate a scenario where the rate of return (a) is constant over the worker's remaining working life and one where the rate diminished over time. If the rate diminishes over time, then it would decrease the rate at which individuals would accrue returns to their training. Currently if the expected lifetime earnings were to decrease, then it would take longer for the return to training to exceed the cost. More formally, we estimate the expected increase in the present value of lifetime earnings as

$$\Delta PV = a * PV \qquad (2)$$

where

$$a = \frac{\hat{ATT}_8}{PDE_8} \qquad (3)$$

and

$$PV = ALE + \sum_{y=1}^{Y} \frac{ALE}{(1+.04)^y} \qquad (4)$$

Y = the number of years between 8th quarter after UI claim and retirement at age Z, where Z is 65, 70, or 73.

$\hat{ATT}_8$ = the average treatment effect in the 8th quarter after claiming UI as derived in Equation 1.

ALE ≡ average lifetime earnings for displaced workers who retire at age Z[4].
PDE ≡ average post-displacement earnings in the 8th quarter after claiming UI.

We select the 8th quarter after claiming UI because this is the first post-UI quarter where one or more of the 32 groups experiences a significant and positive wage advantage over the comparison group.

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[4] Because we have pre-unemployment and post-unemployment wage data, we can use a snapshot of the pre-dislocation wage data to estimate lifetime earnings of an average worker who eventually experiences unemployment. Specifically, we use the average yearly earnings for workers age 20 to 65 in the 3rd year prior to their dislocation event. To accommodate different retirement age scenarios, we also estimate average yearly earnings for workers age 20 to 70 and 20 to 73.

Private Cost of Training

The above framework represents the economic returns side of the equation. On the cost side, foregone earnings are estimated using the same comparison group that was used to estimate the ATT. We first estimate the average number of quarters an ITG participant spends in training. The average tends to be slightly less than 3 quarters but varies slightly across the 32 groups. Then we estimate the average comparison group earnings in the time period equivalent to the average time the ITG group spent in training. This comparison group average serves as our estimate of the wages ITG participants would have earned had they not spent an average of 3 quarters in training. We call this the foregone earnings for the ITG group. As noted earlier, we add $500 to the foregone earrings cost to account for training-related expenses. More formally,

$$\text{Private Cost}_{ITG} = F + \$500$$

where F= earnings lost while in training.

Economic Return vs. Cost: An Example

To illustrate the economic returns calculation framework above, we present an example. Suppose the impact estimate yielded from propensity score matching was an average gain of $300 in the 8th quarter after claiming UI for the ITG group. Supposing the average quarterly earnings in this quarter were $8,000 for the ITG group, then we'd estimate the percentage gain to be 3.75% (300/8,000*100). Using data on dislocated workers in New Jersey and the assumed parameters outlined in Section III.D, we estimate the average lifetime earnings to be $480,000 if this hypothetical group retires at age 65.

Then multiplying 3.75% by these average lifetime earnings yields an estimated gain of $15,000.

To estimate the foregone earnings, we first estimate that this group spent an average of 2.8 quarters in training. Then we estimate that their comparison group earned an average of $13,000 in these same 2.8 quarters. We assume another $500 in costs for books and other training costs not covered by the grant. Then we arrive at $13,500 as the private cost for training. Therefore, in our example, there is a net gain of $1,500 (=$15,000-$13,500) from training upon retiring at age 65.

Using this method we also estimate the break-even point, the point at which the return from training just equals the cost. We only report a break-even point if it occurs before retirement age. A break-even point that occurs beyond the assumed retirement age is considered to be a situation of net monetary loss.

IV. Results

A. ITG Impact on Wages and the Return to Training

As illustrated in Table 3A, the impact of ITG participation on wage levels changes from statistically insignificant to positive and significant after accounting for the re-employment effect. In the 8th quarter, the impact goes from a statistically insignificant -$65.39 to a significant $474.33 advantage. In the former case, we compute the impact just for the employed. In the latter, we compute the average wage advantage for the entire sample (i.e. including those with and without earnings in the average). By doing this we account for the re-employment advantage documented in previous chapters. This trend is repeated in the 12th quarter after training, with the advantage increasing from an insignificant –$0.52 to a significant $568 advantage in average quarterly wage.

To assess the return to training, Table 3A also compares the expected gain in lifetime earnings resulting from the $474.33 per quarter advantage to the estimated cost of training (foregone earnings plus $500). Converting the $474 to a percentage gain (equation 3), we estimate a 9.5% return. If the 9.5% return continues, then the average gain in lifetime earnings will exceed the cost by the 5th year after claiming UI. Assuming the return diminishes at a rate of 10% per year, the cross-over point occurs at 8th year after UI. A diminishing rate of 20% moves the cross-over point to the 13th year after UI.[5] These returns largely result from the reemployment advantage associated with ITG participation. When returns are calculated conditional on reemployment no significant advantage appears.

B. ITG Impact on Wages for Gender-education-age groups

As the U.S. population is aging, it becomes increasingly important to consider how policy impacts vary by age. Age-earnings profiles have been documented to increase at a decreasing rate (Mincer, 1974) (Murphy and Welch, 1990). Similarly, we except the estimated gains to be non-linear and thus smaller for older workers. We therefore examine the variation of the wage impact by age, prior education level, and gender. As noted earlier, prior education and gender are included because the returns to education vary by education and gender. Together Tables 4 to 7 provide the impact of the ITG program for 32 gender-education-age groups (i.e. 2 gender groups x 4 education groups x 4 age groups). These are the returns that are conditional on re-employment because of possible retirement-related selection bias concerns explained earlier. As detailed below,

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[5] The skills learned through the ITG program may depreciate over time as the nature of work (and skills required) changes (especially given rapid changes in technology). We chose various rates of diminishing returns to examine the sensitivity of the results.

five groups experience a wage gain beginning in the 8th quarter after claiming UI. Table 4.4 displays the results for ITG participants who did not have a high school diploma prior to starting the ITG program. The only groups at this education level to experience a positive and significant impact on wages are males and females who were age 18-49 at the time they filed for UI. Males age 18-49 experience an average $597 greater quarterly wage than the comparison group in the 8th quarter after claiming UI. Females age 18-49 experience an average wage advantage of $678. These two groups also experience a significant advantage in the 12th quarter after claiming UI.

Table 4.5 provides the wage impact for those whose highest level of education prior to the ITG program was high school. Three of the high-school subgroups experience a statistically significant wage advantage over their corresponding comparison group in the 8th quarter after claiming UI: males age 18-49 ($261 advantage), females age 50-54 ($326), and females age 60-65 ($864). These three groups also experience a significant wage advantage in the 12th quarter after UI claim. Females age 18-49 first exhibit a wage advantage in the 12th quarter after UI claim.

Table 4.6 and 4.7 provide the wage impact for ITG participants with some college education or a college education prior to entering the ITG program. None of the gender-age sub groups experience a positive and significant wage advantage. Indeed, some of the higher-education groups exhibit a wage disadvantage relative to their comparison groups. This is not unusual. Whittaker (2002) found a positive wage impact for the less than high school group but not other groups. Similarly Hebbar (2006) found a positive impact on wage for high school dropouts. This result was driven by a large portion of high school dropouts that enrolled in truck driving training.

To summarize, five of the 32 gender-education groups experience a positive impact on wage in the 8th quarter after UI. This is consistent with the literature on dislocated worker training, which finds mixed results. Several studies have found no significant impact on wages (Benus et al., 1996) (Whittaker, 2002) (Corson and Haimson, 1996) (Decker and Corson, 1995) while others have found positive impacts on wages (Benus and Byrnes, 1993) (Jacobson et al, 1994) (Hollenbeck, 2003).

Among the five groups, females tend to experience a higher return to training. This is consistent with the general training literature which finds that women tend to benefit more from training (Greenberg et. al., 2006). None of the 32 groups experiences a positive and significant wage advantage in the 4th quarter after claiming UI. Some actually experience a significant negative wage disadvantage in the 4th quarter. This negative effect occurs in part because ITG participants typically spend the first three quarters after claiming UI in training. Therefore, by the 4th quarter, they have had little time to reap any wage gains from their training.

The five groups experiencing a significant and positive return in the 8th quarter after claiming UI constitute 25% of the sample. The remaining 75% belong to groups where there was either no significant effect or negative impact on post-UI wages in the 8th quarter. However, many of these groups do end up earning wages that are statistically indistinguishable from the comparison group wages by the 16th quarter after claiming UI. Overall, in the 8th quarter after claiming UI, ITG participants have a 6% higher chance of being employed than their comparison group (Hebbar, 2006).

Further, it is important to note that wage impact for the subgroups was only computed for those that are employed and consequently does not factor in the demonstrated positive re-employment impact of the ITG program. When we do not condition on reemployment, the number of groups experiencing a positive and significant wage in the 8th quarter stays generally the same, and the number of groups experiencing a significant wage gain in the 12th quarter increases from 6 to 12.

B. The Individual Cost and Economic Return for Gender-education-age groups

For each of the five groups experiencing a positive wage impact in the 8th quarter, we compare the cost of training (measured as foregone earnings plus $500 in costs not covered by the grant) with the estimated increase in lifetime earnings resulting from training. We estimate lifetime earnings under three scenarios: retirement at age 65, age 70, and age 73. The estimates in Table 4.8 indicate that under "the retire at 65" scenario, all groups except high school educated females age 60-65 have an increase in lifetime earnings that is greater than the cost of training. For instance, males age 18-49 whose highest level of education prior to the ITG program was a high-school degree experience an estimated $13,517 loss of earnings while in training. However, beginning in the 8th quarter after claiming UI, we estimate they begin experiencing a 3.3% increase in their wages resulting from training. We estimate that 3.3% of this group's average present value of lifetime earnings (assuming retirement at age 65) amounts to $15,901. This is $2,384 greater than the loss in wages. The net gain for this group increases to $4,282 under the retire-at-73 scenario. This is necessarily higher because the estimated lifetime earnings is higher under the retire-at-73 scenario than the retire-at-65 scenario.

Some of the groups experience as high as 15.7% return to training in the 8th quarter after claiming UI. This is an admittedly high estimate. Jacobson, Lalonde, and Sullivan (2004) estimate a 7% return to training at community colleges for displaced male workers age 35 and older, who were UI claimants in Washington state between 1990-1994. They estimate a return of 10% for females. Further, more generally the annual return to two years of community college has been estimated to be 5.7% for college-aged males and 6.5% for females (Kane and Rouse, 1995).

In light of these estimates, it should be emphasized that these ITG estimates are back-of-the-envelope calculations. Moreover, the calculations do not account for the social costs or benefits of operating the ITG program. To estimate the social cost, we'd have to calculate the deadweight lost resulting from the payroll tax used to fund the ITG program. Given these cautionary notes, we also provide a lower bound estimate. We perform the same calculation for a much shorter time span of 1 year and 2 years after the 8th quarter of claiming UI. The last two columns of Table 4.8 provide these estimates for each of the five groups. Under these two scenarios, none of the five groups experiences a net gain in wages when comparing the foregone earnings to the estimated increase in lifetime wages for this short period.

We also attempt to find the "break-even point," the point in time where the estimated monetary return equals the estimated cost. These durations are listed in Table 4.9 for each of the five groups. For those age 18-49 at the time of UI claim and without a high school degree, we find that it takes 7 to 8 years for the gain in wages to almost cancel out the cost in foregone earnings. For males age 18-49, it takes an average of 23 years because the estimated return to training for this group is lower than the other groups. Females with a high school degree and age 50-54 at the time of UI reach the break-even point at about 10.5 years after claiming UI.

Exhibit 9 illustrates how the break-even point changes when the rate of return is assumed to diminish over time. We assume the rate of return falls by 10% each quarter. This is a purposely high rate of diminishing return, more than double the rate of inflation between 1997 and 1998, which illustrates a worst-case scenario. Three of the five groups still break even, but it takes a longer time to reach this point. For instance, when assuming diminishing returns to training, the break-even point for younger (18-49) male high school dropouts occurs 6 quarters later than when constant returns are assumed (8^{th} quarter versus 14^{th} quarter). The two groups that do not recover from the hypothetical 10% depreciation are males age 18-49 and females age 50-54 whose highest level of education, at the time of unemployment, is a high school degree. This occurs because their return to training is lower than the other groups and cannot overcome the high hypothetical depreciation rate. We are more confident about the estimated long run gains to training because three groups still exhibit lifetime gains even assuming a high rate of depreciation.

V. Conclusions

Enrolling in training represents a significant investment of time and money. This essay demonstrates that those investments can pay off in the long run for both younger and older workers. Short-run losses can be discouraging, but this study provides evidence that private returns to training can end up materializing 5 to 11 years after initial job loss.

Factoring in both the returns from faster reemployment and wage gain, the average lifetime return from training exceeds the opportunity cost of foregone earnings by the 5^{th} year after claiming UI. Assuming the estimated rate of return diminishes at 10% per year, the cross-over point moves to the 8^{th} year after claiming UI.

Conditioning on reemployment and the 8th quarter returns to training, we find that in the long run 25% of ITG participants experience monetary returns that exceed the private cost of training. For the remaining 75% of participants, returns to training (conditioned on reemployment) do not tend to exceed that of their comparison for the period observed (up to 16 quarters after claiming UI). This is not an unusual result. Several other studies have found no significant impact of training on wages in a similar time period (Benus et al., 1996) (Whittaker, 2002) (Corson and Haimson, 1996) (Decker and Corson, 1995).

For the 25% for whom private returns outweigh the foregone earnings, the break-even point— the point at which the returns to training are nearly equal to the foregone wages incurred while in training—occurs 7 to 8 years after claiming UI for younger (age 18-49) males and females without a high school education. The break-even point occurs at 11 years after claiming UI for high-school educated females who were 50-54 at the time of claiming UI. For the whole sample, the combined reemployment and wage returns to training exceed the foregone earnings by the 5th year after claiming UI.

These results suggest the importance of the reemployment advantage and that in the long run the monetary returns from training can exceed the cost of investment. Therefore, sticking with training-related jobs can pay off in the long run, and short-run decisions to switch out of training-related jobs may prevent the returns to training from being realized.

There are three important limitations of the results presented here. First, as is the case with all non-experimental studies, the results in this study assume that selection bias is removed by matching. In the absence of experimental methods, this is the next best solution to estimating the impact of a program. Second, the economic returns estimates in this study rely on several assumed parameters, such as the 4% discount rate, the estimated foregone earnings, and the presumed age to retirement. As illustrated in the essay, changing the retirement age changes the results. Third, the economic return estimates do not account for the social costs that incur from the taxes levied to fund the program or possible taxes gained resulting from post-training earnings gains. Therefore, the reader should consider the economic return results as simulations.

Modeling the cost and the return to training, to the extent possible, is useful. The estimates help policymakers assess the extent of the return on investments of tax revenue. Despite the modeling limitations, these results demonstrate that long-run payoffs to training investments exist. The results also contribute to the policy debates over raising the retirement age, by demonstrating a possible benefit of delayed retirement—an increased likelihood that those who enrolled in training would eventually experience returns to training that exceed the cost.

Table 4.1 Means Table: Female Sample, Re-employed in the 8th Quarter after Claiming UI

Less than High School		qt wage in 4th qt prior UI	constr.	finance	manufac.	public admin.	retail trade	service	transp. & utilities	wholesale trade	tenure group 1	tenure group 2	tenure group 3	tenure group 4	s. atlantic region	south region	north region
age 18-49	itg (n=182)	$6,021	1.7	8.8	23.1	3.3	15.9	34.6	1.1	11.5	15.9	11.0	42.3	30.8	7.7	13.7	78.6
	comp (n=185)	$5,929	2.8	8.8	26.4	2.2	14.3	36.3	0.6	8.8	13.7	13.7	37.4	35.2	6.6	14.8	78.6
age 50-54	itg (n=36)	$7,025	-	16.7	36.1	0.0	5.6	25.0	5.6	11.1	8.3	2.8	36.1	52.8	11.1	8.3	80.6
	comp (n=37)	$7,104	-	8.3	44.4	5.6	13.9	22.2	0.0	5.6	5.6	11.1	27.8	55.6	8.3	25.0	66.7
age 55-59	itg (n=22)	$7,241	4.6	9.1	22.7	0.0	18.2	31.8	4.6	9.1	13.6	13.6	36.4	36.4	0.0	13.6	86.4
	comp (n=23)	$7,873	0.0	9.1	18.2	13.6	18.2	36.4	0.0	4.6	13.6	4.6	27.3	54.6	9.1	18.2	72.7
age 60-65	itg (n=9)	$7,503	-	11.1	11.1	11.1	11.1	33.3	-	22.2	11.1	0.0	22.2	66.7	11.1	11.1	77.8
	comp (n=9)	$5,903	-	11.1	22.2	11.1	22.2	22.2	-	11.1	11.1	11.1	11.1	66.7	11.1	44.4	44.4
High School																	
age 18-49	itg (n=2572)	$7,071	1.9	17.1	14.9	2.8	16.1	30.8	5.9	10.2	14.0	12.8	36.6	36.7	13.6	24.1	62.3
	comp (n=2667)	$7,222	1.7	18.3	15.4	2.6	13.4	31.6	6.2	10.7	13.5	13.9	36.6	36.0	13.0	24.4	62.6
age 50-54	itg (n=544)	$7,780	2.76	18.9	17.5	1.5	12.3	28.1	5.5	13.4	12.5	10.9	32.4	44.3	18.6	21.3	60.1
	comp (n=562)	$7,528	2.02	16.5	17.5	2.2	15.1	29.6	5.5	11.4	10.3	10.7	35.3	43.8	13.4	21.0	65.6
age 55-59	itg (n=360)	$8,240	1.4	19.4	18.1	1.9	12.8	29.4	5.3	11.7	13.3	8.1	31.1	47.5	13.6	20.0	66.4
	comp (n=365)	$7,753	2.2	16.7	20.6	2.2	13.6	28.9	6.1	9.7	10.3	8.1	27.2	54.4	15.6	23.6	60.8
age 60-65	itg (n=84)	$8,294	2.38	17.9	25.0	0.0	13.1	25.0	3.57	13.1	9.5	14.3	20.2	56.0	7.1	15.5	77.4
	comp (n=84)	$7,970	1.19	21.4	8.3	7.1	16.7	29.8	8.33	7.1	7.1	6.0	28.6	58.3	14.3	19.1	66.7
Some college																	
age 18-49	itg (n=1496)	$7,669	1.3	16.7	12.6	2.7	11.6	39.2	5.9	9.6	13.8	14.2	40.8	31.3	15.2	23.5	61.3
	comp (n=1532)	$7,698	1.5	17.4	11.2	2.7	10.6	41.5	5.2	9.7	13.3	13.4	43.1	30.2	17.2	25.0	57.8
age 50-54	itg (n=212)	$8,711	0.47	16.5	12.3	2.4	9.4	35.9	8.0	14.6	12.3	9.0	34.0	44.8	23.1	21.7	55.2
	comp (n=216)	$8,532	1.42	16.5	15.1	5.2	12.3	33.0	6.1	9.9	10.9	13.2	37.3	38.7	17.0	22.2	60.9
age 55-59	itg (n=110)	$9,284	0.9	17.3	15.5	3.6	11.8	32.7	5.5	12.7	13.6	15.5	33.6	37.3	21.8	20.0	58.2
	comp (n=110)	$8,598	0.0	19.1	17.3	6.4	10.0	40.0	0.9	5.5	8.2	10.9	32.7	48.2	13.6	20.9	65.5
age 60-65	itg (n=47)	$9,643	4.26	21.3	10.6	4.3	2.1	44.7	6.38	6.4	10.6	14.9	29.8	44.7	8.5	21.3	70.2
	comp (n=48)	$7,974	2.13	12.8	12.8	0.0	10.6	44.7	8.51	8.5	14.9	8.5	31.9	44.7	19.2	21.3	59.6
College																	
age 18-49	itg (n=633)	$9,277	1.3	16.1	10.9	3.0	11.1	43.3	4.6	9.8	13.7	16.4	43.8	26.1	17.7	16.1	66.2
	comp (n=650)	$9,339	1.7	15.8	10.0	4.1	7.9	44.4	6.8	9.3	14.4	15.2	46.8	23.7	17.7	17.2	65.1
age 50-54	itg (n=118)	$9,710	1.69	11.0	16.1	4.2	6.8	49.2	2.5	8.5	10.2	10.2	42.4	37.3	18.6	16.1	65.3
	comp (n=120)	$9,542	0	8.5	11.9	6.8	7.6	56.8	5.1	3.4	13.6	11.0	49.2	26.3	14.4	18.6	67.0
age 55-59	itg (n=64)	$10,042	-	7.8	15.6	6.3	9.4	48.4	6.3	6.3	15.6	6.3	45.3	32.8	10.9	21.9	67.2
	comp (n=64)	$9,585	-	7.8	9.4	6.3	14.1	50.0	0.0	12.5	17.2	14.1	35.9	32.8	9.4	15.6	75.0
age 60-65	itg (n=24)	$13,071	-	16.7	12.5	0.0	8.3	54.2	4.17	4.2	12.5	12.5	41.7	33.3	20.8	8.3	70.8
	comp (n=24)	$11,114	-	16.7	4.2	4.2	4.2	66.7	4.17	0.0	4.2	20.8	29.2	45.8	25.0	25.0	50.0

Notes: [a] indicates the distributions are significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The ITG sample sizes in tables 1 and 2 do not sum to 16,001 because the means are only for those with positive wages in the 8th quarter after claiming UI, which is a subset of the 16,001. For those with positive wages we can estimate a monetary return to training and thus compute a economic returns comparison as provided in table 4.8. Tenure group 1 is employed 3 or less consecutive quarters prior to UI. Tenure group 2 is employed 7-4 quarters. Tenure group 3 is employed 8 tc and tenure group 4 is employed at the same employer for at least 12 quarters prior to UI.

Table 4.2 Means Table: Male Sample, Re-employed in the 8th Quarter after Claiming UI

Less than High School		qt wage in 4th qt prior UI	constr.	finance	manufac.	public admin.	retail trade	service	transp. & utilities	wholesale trade	tenure group 1	tenure group 2	tenure group 3	tenure group 4	s. atlantic region	south region	north region
age 18-49	itg (n=274)	$7,254	4.7	1.8	35.4	2.2	11.0	17.5	9.1	16.4	20.4	13.1	38.3	28.1	7.3	19.3	73.4
	comp (n=279)	$6,917	4.4	2.9	39.1	1.5	13.9	17.5	4.7	14.2	16.8	11.3	41.2	30.7	11.0	12.8	76.3
age 50-54	itg (n=31)	$8,464	0	3.2	38.7	-	16.1	22.6	6.5	12.9	16.1	9.7	29.0	45.2	6.5	22.6	71.0
	comp (n=32)	$9,055	6.45	6.5	48.4	-	12.9	12.9	3.2	6.5	19.4	9.7	35.5	35.5	6.5	19.4	74.2
age 55-59	itg (n=18)	$8,309	11.1	5.6	27.8	-	16.7	16.7	11.1	11.1	11.1	5.6	44.4	38.9	-	11.11a	88.89a
	comp (n=20)	$8,195	5.6	0.0	38.9	-	5.6	11.1	22.2	16.7	27.8	16.7	22.2	33.3	-	38.9	61.1
age 60-65	itg (n=5)	$11,809	-	-	60.0	-	20.0	0.0	20	-	20.0	20.0	20.0	40.0	-	40.0	60.0
	comp (n=5)	$11,979	-	-	60.0	-	20.0	20.0	0	-	0.0	0.0	80.0	20.0	-	40.0	60.0
High School																	
age 18-49	itg (n=1458)	$8,172	3.7	4.5	29.1	2.3	17.1	20.8	8.4	13.7	16.3	15.4	39.5	28.8	12.9	23.1	64.0
	comp (n=1510)	$8,162	3.2	4.1	30.1	2.2	17.8	21.9	7.7	12.5	15.9	16.0	39.7	28.4	13.8	22.5	63.7
age 50-54	itg (n=167)	$10,792	1.8	6.6	29.3	2.4	15.0	27.0	6.0	12.0	13.2	7.8	40.1	38.9	15.0	25.2	59.9
	comp (n=175)	$10,338	2.99	4.8	44.3	4.2	15.3	14.4	7.2	6.9	14.4	13.2	29.3	43.1	15.9	23.4	60.8
age 55-59	itg (n=122)	$11,148	0.0	6.6	40.2	1.6	9.8	23.8	9.0	9.0	11.5	9.8	36.1	42.6	13.1	20.5	66.4
	comp (n=126)	$10,311	3.3	2.5	45.1	1.6	9.8	14.8	13.9	9.0	8.2	11.5	31.2	49.2	13.1	23.0	63.9
age 60-65	itg (n=43)	$10,502	2.33	11.6	23.3	4.7	14.0	18.6	9.3	16.3	16.3	11.6	27.9	44.2	7.0	11.6	81.4
	comp (n=47)	$8,642	4.65	4.7	27.9	0.0	25.6	14.0	9.3	14.0	9.3	16.3	34.9	39.5	9.3	25.6	65.1
Some college																	
age 18-49	itg (n=851)	$8,644	2.8	5.8	23.9	2.6	16.0	27.3	6.6	14.7	16.0	16.9	41.0	26.1	14.7	21.2	64.2
	comp (n=875)	$9,106	3.1	5.8	23.7	2.8	16.6	27.5	7.5	13.1	15.1	13.5	43.6	27.8	14.1	20.3	65.6
age 50-54	itg (n=118)	$11,236	1.69	3.4	32.2	1.7	9.3	21.2	10.2	20.3	18.6	6.8	38.1	36.4	16.1	16.1	67.8
	comp (n=118)	$11,784	5.08	5.9	33.1	0.9	7.6	30.5	1.7	15.3	12.7	10.2	43.2	33.9	12.7	19.5	67.8
age 55-59	itg (n=66)	$12,176	0.0	7.6	30.3	4.6	12.1	25.8	1.5	18.2	15.2	7.6	40.9	36.4	19.7	16.7	63.6
	comp (n=66)	$13,099	1.5	4.6	34.9	6.1	9.1	22.7	9.1	12.1	12.1	9.1	40.9	37.9	15.2	25.8	59.1
age 60-65	itg (n=26)	$11,810	0	0.0	38.5	15.4	11.5	19.2	7.69	7.7	26.9	0.0	23.1	50.0	11.5	11.5	76.9
	comp (n=27)	$10,144	3.85	3.9	23.1	7.7	3.9	30.8	11.54	15.4	11.5	7.7	42.3	38.5	19.2	15.4	65.4
College																	
age 18-49	itg (n=518)	$10,247	2.7	9.5	19.7	2.9	11.4	33.8	7.3	12.6	15.1	14.5	40.5	29.9	18.5	16.2	65.3
	comp (n=535)	$11,346	3.4	10.6	23.6	2.8	10.8	33.8	4.8	10.2	16.2	12.6	41.7	29.5	15.3	18.8	65.9
age 50-54	itg (n=117)	$13,229	0.85	13.7	23.9	0.9	9.4	30.8	2.6	18.0	16.2	16.2	30.8	36.8	20.5	19.7	59.8
	comp (n=119)	$14,052	1.71	9.4	23.9	3.4	10.3	33.3	3.4	14.5	12.0	12.8	39.3	35.9	21.4	14.5	64.1
age 55-59	itg (n=73)	$15,067	1.37	9.6	31.5	4.1	5.5	32.9	4.1	11.0	12.3	6.9	41.1	39.7	15.1	12.3	72.6
	comp (n=76)	$15,361	8.22	8.2	19.2	1.4	8.2	35.6	4.1	15.1	12.3	11.0	42.5	34.3	15.1	13.7	71.2
age 60-65	itg (n=40)	$12,493	2.5	10.0	32.5	2.5	2.5	30.0	7.5	12.5	10.0	10.0	47.5	32.5	10.0	17.5	72.5
	comp (n=41)	$12,943	5	7.5	32.5	5.0	10.0	30.0	0	10.0	17.5	17.5	40.0	25.0	15.0	12.5	72.5

Notes: [a] indicates the distributions are significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The ITG sample sizes in tables 1 and 2 do not sum to 16,001 because the means are only for those with positive wages in the 8th quarter after claiming UI, which is a subset of the 16,001. Tenure group 1 is employed 3 or less consecutive quarters prior to UI. Tenure group 2 is employed 7-4 quarters. Tenure group 3 is employed 8 to 11 quarters, and tenure group 4 is employed at the same employer for at least 12 quarters prior to UI.

Table 4.3 Heckman-Hotz Test of Pre-Unemployment Wages
Females and Males

	Females				Males			
	qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI		qt. wage in 4th qt. Prior to UI		diff. btw 2nd qt and 4th qt. Prior to UI	
Less than High School	base model ITG coeff (std. dev)	full model ITG coeff (std. dev)	base model ITG coeff (std. dev)	full model ITG coeff (std. dev)	base model ITG coeff (std. dev)	full model ITG coeff (std. dev)	base model ITG coeff (std. dev)	full model ITG coeff (std. dev)
age 18-49	-12.87 (420.44)	268.19 (427.14)	464.78c (278.16)	268.77 (287.31)	291.01 (374.56)	379.95 (358.33)	-147.05 (240.34)	-106.56 (240.81)
age 50-54	-487.52 (1008.28)	-176.77 (1280.46)	156.71 (681.81)	222.68 (912.3)	-550.17 (1690.99)	-678.17 (1781.02)	355.41 (926.49)	105.07 (1172.68)
age 55-59	-228.13 (1819.56)	201.32 (4160.36)	-770.83 (1075.34)	303.58 (2330.65)	-1320.5 (1374.03)	778.69 (1838.4)	-1539.06 (1711.4)	229.07 (801.78)
age 60-65	772.02 (1304.89)	3731.96a (250.08)	-143.87 (624.15)	-1716.21a (467.85)	-170.63 (3783.53)	7967.2 (11009.09)	-2334.13b (1158.14)	-163.29a (47.36)
High School								
age 18-49	-172.14 (117.61)	-106.94 (110.25)	56.54 (81.03)	39.72 (80.61)	192.76 (196.11)	240.84 (180.38)	-16.19 (132.95)	-79.61 (131.74)
age 50-54	325.93 (261.11)	230.65 (255.14)	-46.8 (135.75)	-5.8 (136.79)	472.04 (704.1)	615.58 (702.14)	-373.08 (487.11)	-441.94 (519.28)
age 55-59	526.19c (307.97)	454.03 (301.74)	383.57c (223.35)	371.75c (219.44)	314.7 (809.34)	638.33 (863.19)	-485.27 (509.2)	-676.92 (527.58)
age 60-65	261.69 (548.03)	-637.55 (577.77)	-188.7 (436.71)	-356.87 (535.28)	1351.19 (1107.45)	2106.7 (1304.18)	-11.21 (457.15)	172.61 (558.35)
Some college								
age 18-49	-144.88 (170.43)	-122.19 (163.74)	79.83 (117.86)	76.88 (116.36)	-606.01b (266.55)	-548.44b (251.02)	47.52 (167.12)	-34.09 (165.43)
age 50-54	540.68 (462.3)	156.36 (475.27)	-187.11 (347.69)	-105.07 (349.09)	-400.84 (900.04)	-212.23 (931.55)	530.62 (531.17)	687.47 (568.58)
age 55-59	284.94 (752.61)	556.28 (784.26)	-333.19 (446.05)	-138.18 (469.27)	-1298.88 (1291.91)	-1500.63 (1337.88)	-191.38 (521.32)	-58.38 (548.49)
age 60-65	1515.52 (1094.33)	1433.68 (1332.35)	-103.79 (491.66)	57.8 (587.56)	2143.46 (1738.32)	2498.79 (3254.96)	-848.91 (939.67)	185.25 (1178.36)
College								
age 18-49	-77.68 (363.44)	-45.03 (347.19)	-372.87 (243.64)	-335.08 (244.12)	-751.15 (473.25)	-826.53c (453.24)	272.16 (288.18)	243.84 (294.22)
age 50-54	282.95 (1000.86)	-340.75 (1058.8)	-241.79 (534.04)	282.67 (569.47)	-391.49 (1256.01)	-950.23 (1204.69)	317.94 (705.94)	527.93 (757.94)
age 55-59	1362.85 (1212.98)	2058.55 (1452.25)	-256.24 (859.43)	-2004.48b (968.74)	800.93 (1441.18)	-583.93 (1721.83)	-48.94 (902.04)	678.33 (1054.03)
age 60-65	4116.2b (1999.63)	4369.09 (2743.14)	-1794.45 (1315.62)	-66.83 (1250.32)	1323.55 (1937.13)	1073.56 (2581.39)	340.39 (1229.78)	-45.16 (1738.69)

Notes: ‘a’ indicates significance at 1% level. ‘b’ indicates significance at 5% level. ‘c’ indicates significance at 10% level. The first four columns of the table are the results for female and the second four columns are for males. The dependent variable is the 4th quarter prior to claiming UI in columns 1,2,5, and 6, and the dependent variable in columns 3,4,7, and 8 is the difference between the wage in the 2nd quarter prior to UI and the wage in 4th quarter prior to UI. The results in the columns labeled “base model” include only an intercept and the ITG participation variable. The results in the columns labeled "full model" control for the following factors: itg participation, pre-ui tenure, region, gender, race, age, year and quarter of UI claim. Variables for age, education, and gender are not included because the sample is divided into sub-groups by these variables.

Table 4.3A The Impact of ITG Participation on Wages and The Estimated Economic Return

PANEL A
Treatment on Treated Effect on Quarterly Wage
Full Sample

	Quarterly Wage in 4th quarter after UI Claim		Quarterly Wage in 8th quarter after UI Claim		Quarterly Wage in 12th quarter after UI Claim	
	Avg treatment effect on ITG	N, ATT & bootstrap std error (ITG + Comp)	Avg treatment effect on ITG	N, ATT & bootstrap std error (ITG + Comp)	Avg treatment effect on ITG	N, ATT & bootstrap std error (ITG + Comp)
Employed	-659.86[a]	17,044 (8,522 + 8,522)	-65.38	21,040 (10,520 + 10,520)	-0.52	19,744 (9,872 + 9,872)
	(66.53)	651,688	(66.36)	651,688	(63.09)	651,688
Employed & Unemployed	-637.50[a]	32,002 (16,001 + 16,001)	474.33[a]	32,002 (16,001 + 16,001)	568.49[a]	32,002 (15,427 + 15,427)
	(50.44)	651,688	(54.41)	651,688	(56.75)	629,239

PANEL B
Economic Returns

	Cost= foregone earnings + $500	**return to ITG participation (ATT8 / q8)**	**Increase in Earnings (assuming retire at 65)**	**net=cost - increase in earnings (yrs till cost equals net increase)**
Full sample (constant returns)	$10,885	9.5%=($474/$4983)	$11,936	net= $1051 (5 years)
Full sample (returns diminish at 10% per quarter)	$10,885	9.5%=($474/$4983)	$11,560	net=$675 (8 years)
Full sample (returns diminish at 20% per quarter)	$10,885	9.5%=($474/$4983)	$10,933	net=$48 (13 years)

Notes: [a] indicates significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The column labeled Average Treatment Effect provides the ATT (derived in equation 1) and the standard error appears in parenthesis. The column labeled N provides the sample size used to generate the ATT and the bootstrapped standard errors. The outcome variable is quarterly wage, and the ATT results were generated using Leuven and Sianesi's (2003) psmatch2 program for Stata. A common support without replacement was imposed. A seed of 311206 was used to break ties. The propensity score was generated by regressing ITG participation status on the following variables: industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim.

Table 4.4 Treatment on Treated Effect on Quarterly Wage
Prior Education: Less than High School

Less than High School	Quarterly Wage in 4th quarter after UI Claim		Quarterly Wage in 8th quarter after UI Claim		Quarterly Wage in 12th quarter after UI Claim	
	Avg treatment effect on ITG	N, ATT & bootstrap std error (ITG + Comp)	Avg treatment effect on ITG	N, ATT & bootstrap std error (ITG + Comp)	Avg treatment effect on ITG	N, ATT & bootstrap std error (ITG + Comp)
Males						
age 18-49	187.914 (250.152)	526 (260 + 266) 65,019	597.091[b] (252.337)	553 (274 + 279) 65,019	671.938[b] (335.509)	469 (233 + 236) 65,019
age 50-54	559.617 (910.987)	62 (31 + 31) 65,019	-272.997 (789.633)	63 (31 + 32) 65,019	-426.936 (925.122)	62 (30 + 32) 65,019
age 55-59	398.168 (1257.019)	23 (12 + 11) 65,019	317.33 (938.799)	38 (18 + 20) 65,019	1216.261 (1289.516)	31 (15 + 16) 65,019
age 60-65	-1190.808 (1849.644)	8 (4 + 4) 65,019	-2806.067 (6429.022)	10 (5 + 5) 65,019	-1352.024 (2517.292)	4 (2 + 2) 65,019
Females						
age 18-49	-176.007 (308.524)	307 (153 + 154) 45,642	673.793[a] (247.031)	367 (182 + 185) 45,642	932.518[a] (257.059)	332 (165 + 167) 45,642
age 50-54	-577.336 (678.699)	54 (27 + 27) 45,642	693.639 (638.412)	73 (36 + 37) 45,642	631.304 (675.491)	76 (36 + 40) 45,642
age 55-59	93.115 (918.63)	35 (17 + 18) 45,642	-0.179 (660.91)	45 (22 + 23) 45,642	754.558 (729.68)	34 (17 + 17) 45,642
age 60-65	-1183.15 (976.122)	18 (9 + 9) 45,642	419.153 (905.334)	18 (9 + 9) 45,642	-390.682 (1157.662)	15 (7 + 8) 45,642

Notes: [a] indicates significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The column labeled Average Treatment Effect provides the ATT (derived in equation 1) and the standard error appears in parenthesis. The column labeled N provides the sample size used to generate the ATT and the bootstrapped standard errors. The outcome variable is quarterly wage, and the ATT results were generated using Leuven and Sianesi’s (2003) psmatch2 program for Stata. A common support without replacement was imposed, and ties were used and weighted accordingly. The propensity score was generated by regressing ITG participation status on the following variables: industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim.

Table 4.5 Treatment on Treated Effect on Quarterly Wage
Prior Education: High School

High School	Quarterly Wage in 4th quarter after UI Claim: Avg treatment effect on ITG	Quarterly Wage in 4th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)	Quarterly Wage in 8th quarter after UI Claim: avg treatment effect on ITG	Quarterly Wage in 8th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)	Quarterly Wage in 12th quarter after UI Claim: avg treatment effect on ITG	Quarterly Wage in 12th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)
Males						
age 18-49	-597.187[a]	2400 (1184 + 1216)	260.456[c]	2968 (1458 + 1510)	523.34[a]	2764 (1361 + 1403)
	(142.778)	146,844	(149.9)	146,844	(161.906)	146,844
age 50-54	-1432.545[a]	287 (141 + 146)	-440.512	342 (167 + 175)	-531.551	316 (156 + 160)
	(408.703)	146,844	(509.242)	146,844	(527.895)	146,844
age 55-59	-589.612	229 (113 + 116)	-663.647	248 (122 + 126)	-924.233	228 (112 +
	(563.39)	146,844	(589.232)	146,844	(574.663)	146,844
age 60-65	207.818	62 (27 + 35)	451.271	90 (43 + 47)	-900.213	84 (40 + 44)
	(1010.235)	146,844	(860.9)	146,844	(1011.564)	146,844
Females						
age 18-49	-379.934[a]	4102 (2021 + 2081)	-5.414	5239 (2572 + 2667)	201.265[b]	4869 (2399 + 2470)
	(90.835)	134,868	(73.322)	134,868	(78.999)	134,868
age 50-54	-324.703[c]	898 (445 + 453)	325.898[c]	1106 (544 + 562)	318.598[c]	1004 (494 + 510)
	(172.51)	134,868	(169.317)	134,868	(189.532)	134,868
age 55-59	252.584	551 (273 + 278)	174.076	725 (360 + 365)	181.766	625 (309 +
	(242.891)	134,868	(200.197)	134,868	(216.903)	134,868
age 60-65	-700.058[c]	155 (77 + 78)	863.654[b]	168 (84 + 84)	875.091[b]	172 (86 + 86)
	(392.623)	134,868	(382.806)	134,868	(436.398)	134,868

Notes: [a] indicates significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The column labeled Average Treatment Effect provides the ATT (derived in equation 1) and the standard error appears in parenthesis. The column labeled N provides the sample size used to generate the ATT and the bootstrapped standard errors. The outcome variable is quarterly wage, and the ATT results were generated using Leuven and Sianesi's (2003) psmatch2 program for Stata. A common support without replacement was imposed, and ties were used and weighted accordingly. The propensity score was generated by regressing ITG participation status on the following variables: industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim.

Table 4.6 Treatment on Treated Effect on Quarterly Wage
Prior Education: Some College

Some College	Quarterly wage 4th quarter after UI Claim: Avg treatment effect on ITG	Quarterly wage 4th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)	Quarterly wage in 8th quarter after UI Claim: avg treatment effect on ITG	Quarterly wage in 8th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)	Quarterly wage in 12th quarter after UI Claim: avg treatment effect on ITG	Quarterly wage in 12th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)
Males						
age 18-49	-1249.581[a] (231.865)	1387 (689 + 698) 72,598	-445.767[b] (218.323)	1726 (851 + 875) 72,598	-479.8[b] (228.934)	1656 (817 + 839) 72,598
age 50-54	-2020.056[a] (695.125)	190 (94 + 96) 72,598	-471.585 (627.342)	236 (118 + 118) 72,598	-1655.78[a] (602.403)	233 (116 + 117) 72,598
age 55-59	-1273.054 (809.985)	112 (56 + 56) 72,598	410.25 (871.004)	132 (66 + 66) 72,598	-17.573 (662.521)	132 (66 + 66) 72,598
age 60-65	775.232 (1392.483)	43 (21 + 22) 72,598	714.219 (1333.332)	53 (26 + 27) 72,598	-1673.291 (1082.757)	48 (24 + 24) 72,598
Females						
age 18-49	-700.993[a] (111.104)	2353 (1167 + 1186) 74,817	-207.891 (134.213)	3028 (1496 + 1532) 74,817	-153.453 (126.998)	2888 (1432 + 1456) 74,817
age 50-54	-628.606 (416.462)	343 (170 + 173) 74,817	193.807 (338.27)	428 (212 + 216) 74,817	-153.699 (352.761)	384 (189 + 195) 74,817
Age 55-59	-1138.721[b] (451.012)	202 (101 + 101) 74,817	579.143 (427.523)	220 (110 + 110) 74,817	550.058 (433.22)	224 (112 + 112) 74,817
Age 60-65	490.972 (827.399)	63 (31 + 32) 74,817	676.098 (642.554)	95 (47 + 48) 74,817	14.092 (701.581)	91 (45 + 46) 74,817

Notes: [a] indicates significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The column labeled Average Treatment Effect provides the ATT (derived in equation 1) and the standard error appears in parenthesis. The column labeled N provides the sample size used to generate the ATT and the bootstrapped standard errors. The outcome variable is quarterly wage, and the ATT results were generated using Leuven and Sianesi's (2003) psmatch2 program for Stata. A common support without replacement was imposed, and ties were used and weighted accordingly. The propensity score was generated by regressing ITG participation status on the following variables: industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim.

Table 4.7 Treatment on Treated Effect on Quarterly Wage
Prior Education: College

College	Quarterly wage in 4th quarter after UI Claim: Avg treatment effect on ITG	Quarterly wage in 4th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)	Quarterly wage in 8th quarter after UI Claim: avg treatment effect on ITG	Quarterly wage in 8th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)	Quarterly wage in 12th quarter after UI Claim: avg treatment effect on ITG	Quarterly wage in 12th quarter after UI Claim: N, ATT & bootstrap std error (ITG + Comp)
Males						
age 18-49	-2600.787[a] (330.234)	863 (425 + 62,513	-1699.503[a] (319.745)	1053 (518 + 62,513	-1410.634[a] (314.689)	983 (486 + 62,513
age 50-54	-1935.577[b] (850.792)	209 (104 + 105) 62,513	-2429.317[a] (833.612)	236 (117 + 119) 62,513	-1608.954[c] (832.274)	223 (110 + 113) 62,513
age 55-59	-965.74 (1160.099)	122 (61 + 61) 62,513	-1487.9 (1017.958)	149 (73 + 76) 62,513	-2020.363[b] (956.467)	135 (67 + 68) 62,513
age 60-65	-2233.375[c] (1231.653)	77 (38 + 39) 62,513	-2958.95[b] (1223.762)	81 (40 + 41) 62,513	-646.628 (1132.1)	85 (42 + 43) 62,513
Females						
age 18-49	-1403.24[a] (260.405)	1067 (527 + 49,387	-832.187[a] (257.693)	1283 (633 + 49,387	-907.761[a] (295.162)	1200 (591 + 49,387
age 50-54	-465.389 (676.344)	201 (100 + 101) 49,387	592.036 (654.217)	238 (118 + 120) 49,387	142.305 (584.71)	229 (114 + 115) 49,387
age 55-59	-2399.955[b] (961.692)	110 (55 + 55) 49,387	-1322.192[c] (745.742)	128 (64 + 64) 49,387	281.646 (905.213)	115 (57 + 58) 49,387
Age 60-65	-1558.736 (1582.365)	38 (19 + 19) 49,387	-478.722 (1292.624)	48 (24 + 24) 49,387	158.299 (1170.245)	45 (22 + 23) 49,387

Notes: [a] indicates significance at .01 level. [b] indicates significance at .05 level. [c] indicates significance at .10 level. The column labeled Average Treatment Effect provides the ATT (derived in equation 1) and the standard error appears in parenthesis. The column labeled N provides the sample size used to generate the ATT and the bootstrapped standard errors. The outcome variable is quarterly wage, and the ATT results were generated using Leuven and Sianesi's (2003) psmatch2 program for Stata. A common support without replacement was imposed, and ties were used and weighted accordingly. The propensity score was generated by regressing ITG participation status on the following variables: industry prior to unemployment, tenure prior to unemployment, wage quartile prior to unemployment, region, and quarter of UI claim.

Table 4.8 Foregone Earnings vs. Estimated Increase in Earnings
for 5 groups under 5 scenarios

Group	**Cost= foregone earnings + $500**	**return to ITG participation (ATT8 / q8)**	Estimated increase in lifetime earnings by Retirement Age *net= cost – increase in earnings (yrs till retirement)* retire at 65	retire 70	retire 73	Estimated increase in years after 8th qt UI 1 year	3 years
Prior Education: Less than High School							
1. Male, age 18-49 at UI	$11,014	8.3%=($597/$7214)	$32,247 **Net= $21,233 (29.5 yrs)**	$34,796 **net= $23782 (34.5 yrs)**	$36,087 **net= $25073 (37.5 yrs)**	$1,809 **net= $-9205 (1 yr)**	$3,540 **net= $-7474 (2 yrs)**
2. Female, age 18-49 at UI	$11,873	11.5%=($674/$5858)	$30,037 **Net= $18164 (29.5 yrs)**	$32,445 **net= $20572 (34.5 yrs)**	$33,674 **net= $21801 (37.5 yrs)**	$1,685 **net= $-10188 (1 yr)**	$3,301 **net= $-8572 (2 yrs)**
Prior Education: High School							
3. Males, age 18-49 at UI	$13,517	3.3%=($261/$7953)	$15,901 **net= $2384 (29.5 yrs)**	$17,156 **net= $3639 (34.5 yrs)**	$17,799 **net= $4282 (37.5 yrs)**	$892 **net= $-12625 (1 yr)**	$1,745 **net= $-11772 (2 yrs)**
4. Females, age 50-54 at UI	$10,480	4.8%=($326/$6772)	$10,793 **net= $313 (11 yrs)**	$14,008 **net= $3528 (16 yrs)**	$15,658 **net= $5178 (19 yrs)**	$1,185 **net= $-9295 (1 yr)**	$2,267 **net= $-8213 (2 yrs)**
5. Females, age 60-65 at UI	$8,509	15.7%=($864/$5487)	$5,118 **net= $-3391 (1.5 yrs)**	$15,943 **net= $7435 (5.5 yrs)**	$22,761 **net= $14253 (8.5 yrs)**	$3,445 **net= $-5064 (1 yr)**	$6,199 **net= $-2310 (2 yrs)**

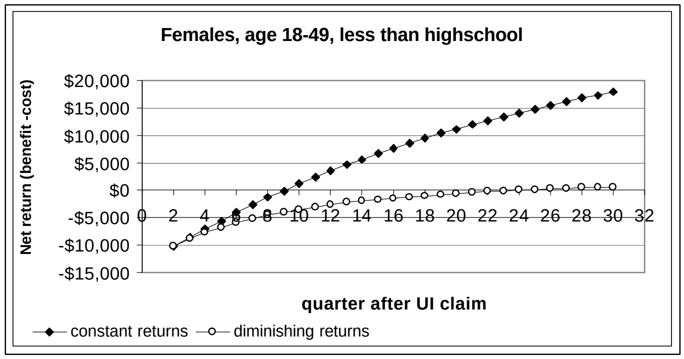
Notes: Foregone earnings (column 1) amounts to the average comparison group earnings accrued during the average time an ITG participant spends in training. The return to ITG participation (column 2) is the ratio of the Average Treatment Effect to the average wage in the 8th quarter after claiming UI. Each column in the retirement scenario represents the lifetime gain in earnings for different retirement ages. The gain in lifetime earnings is obtained by taking the difference between foregone earnings (column 1) and the expected gain. The expected gain is the product of the return displayed in column 2 and the average lifetime earnings for the comparison group population (not shown in the table). For group 5 (females age 60-65), the retire at age 65 scenario does not include those age 64 and 65 at the time of unemployment. They are excluded because by the 8th quarter after claiming UI they have passed the assumed retirement age of 65.

Table 4.9 Approximate Break-Even point for
Private Cost vs. Estimated Increase in Earnings
for 5 groups under 5 scenario

Group	**Cost= foregone earnings + $500**	**return to ITG participation (ATT8 / q8)**	net=cost - increase in earnings (yrs till cost equals net increase)
Prior Education: Less than High School			
1. Male, age 18-49 at UI	$11,014	8.3%=($597/$7214)	$11,293 **net= $279 (7 yrs)**
2. Female, age 18-49 at UI	$11,873	11.5%=($674/$5858)	$11,799 **net= $-74 (8 yrs)**
Prior Education: High School			
3. Males, age 18-49 at UI	$13,517	3.3%=($261/$7953)	$13,784 **net= $267 (23 yrs)**
4. Females, age 50-54 at UI	$10,480	4.8%=($326/$6772)	$10,396 **net= $-84 (10.5 yrs)**
5. Females, age 60-65 at UI	$8,509	15.7%=($864/$5487)	$9,120 **net= $612 (3 yrs)**

Notes: Foregone earnings (column 1) amounts to the average comparison group earnings accrued during the average time an ITG participant spends in training. The return to ITG participation (column 2) is the ratio of the Average Treatment Effect to the average wage in the 8th quarter after claiming UI. The third column includes the expected increase in lifetime earnings at the point at which it is approximately equal to the cost (column 1). The third column also includes the number of years it takes to reach this equilibrium point. The expected gain in lifetime earnings is the product of the return displayed in column 2 and the average lifetime earnings for the comparison group population (not shown in the table). There is no equilibrium point for group 5 within the assumed retirement age of age 65.

Illustration 4.1
Net Return to Training Assuming Constant and Diminishing Returns to Training Through the ITG Program



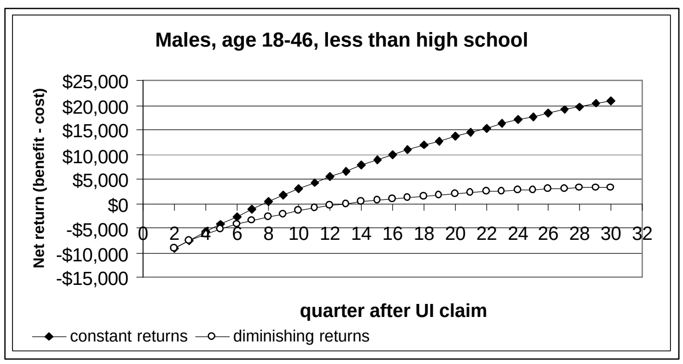
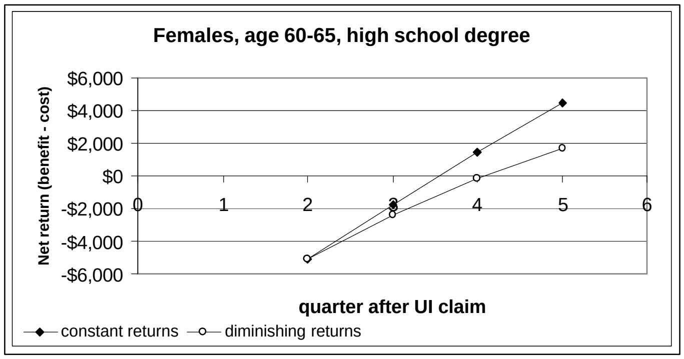
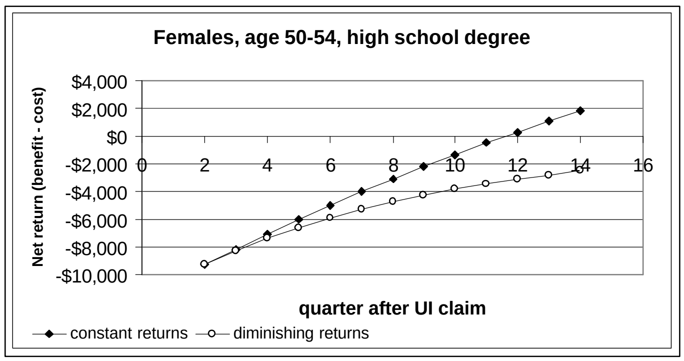
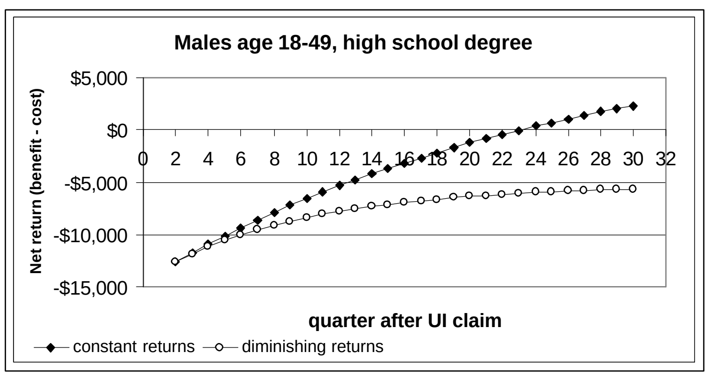


Illustration 4.1 (Continued)







Chapter 5
Conclusions and Policy Implications

I. Introduction

Unemployment is a consequence of cyclical and structural shifts in an economy. Cyclical unemployment occurs as a result of a recession. Structural unemployment occurs as a result of permanent shifts in how an economy produces its goods and services. For example, in the 1970s and 1980s, steel plants, car plants, and other industries closed in the U.S. and the corresponding jobs relocated to other countries where the labor costs were cheaper (Bluestone and Harrison, 1982). Structural and cyclical unemployment do not include those who are fired or voluntarily quite. In an effort to address structural unemployment, federal and state governments provide unemployed workers with training to assist them in obtaining new skills for jobs in different occupations or industries.

Occupational training is a vehicle that enables the unemployed to adjust their skill sets to changes in the demand for labor. New Jersey's Individual Training Grant Program (ITG) applies this principle by providing unemployed workers with vouchers (worth up to $4,000) to obtain training in a demand occupation. A demand occupation is defined as one where the projected number of job openings is greater than the available graduates in the field.[1] Previous non-experimental evaluations of the ITG program found that it generally had no significant impact on wages but had a positive impact on re-employment (Whittaker, 2002) (Van Horn et al., 2000) (Benus et al., 1996). This thesis improves upon the previous evaluations by using a broader set of matching variables and multiple matching methods. The additional matching variables of industry prior to unemployment and county coincide with the eligibility determination process and local

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[1]Section 34:15D-3 of New Jersey Public Law-L.1992,c.43,s1.

labor market as recommended in the matching literature that compares experimental and non-experimental results (Diaz and Handa, 2006) (Michalopoulos, Bloom, and Hill, 2004) (Heckman, Ichimura, and Todd, 1997) (Heckman, Ichimura, Todd, and Smith, 1998). Using three non-experimental design methods (exact matching, propensity score matching and Abadie-Imbens bias adjusted matching), this thesis examines the extent to which the impact estimates on re-employment and wage recovery rates are sensitive to the method used (Abadie-Imbens 2004). Previous studies relied on a single method. Using more than one method assesses whether results are consistent across methods. Recent research has demonstrated that different methods applied to the same data can yield different results (Dehejia, 2005) (Smith and Todd, 2005).

We find that the ITG program increases the odds of re-employment. In the 8th quarter after becoming unemployed, ITG participants have an average re-employment rate 6% points higher than that of their comparison group. This estimate is consistent across the different estimation methods and with the Whittaker (2002) study. Overall, the ITG program has no impact on wage recovery. By the 12th quarter after becoming unemployed, there is no significant difference between the wage recovery rates of ITG participants and their comparison group. This is also consistent across the estimation methods and with the previous ITG evaluations (Whittaker, 2002) (Benus et al., 1996).

II. Policy Implications

These results help inform the debate over two pieces of legislation currently under consideration in the United States Congress. The Workforce Investment Improvement Act of 2007 (H.R. 3747) would require local workforce areas to "ensure that training services are linked to occupations that are in demand." Our evaluation demonstrates that the ITG program, characterized by its demand-driven structure, is associated with increased odds of re-employment and in some instances greater wage recovery for participants. This evidence suggests that if, as proposed, training assistance is targeted at occupations where shortages exists, then training can on average yield higher odds of reemployment than would occur in the absence of training. Though the possibility of selection bias still exists. Further, a 2006 randomized experiment found that choice of occupational training was similar for a guided choice model (similar to the ITG program) and one with no mandatory counseling. However, it is critical to note that the study did not exam the extent to which workers used information to guide their decision making process.

A second piece of legislation currently in committee is the Trade and Globalization Adjustment Assistance Act of 2007 (S.1848). Currently only those workers who lose their jobs as result of foreign imports or the shifting of production plants from the U.S. to a foreign country participating in a trade agreement with the U.S are eligible for Trade Adjustment Assistance program benefits. This is based on the criteria set forth in the Trade Adjustment Assistance Act of 1962. The new proposed legislation would extend TAA to those previously employed in the services sector. Results from our evaluation provide an example of how a training program can be structured to serve workers dislocated from a wide-range of industries. Further, when examining impacts by industry of previous employment we find that positive re-employment impacts for all industries. As with the overall results, we find no significant impact on wage recovery among the industry groups. Therefore this suggests that workers tend to benefit from training, irrespective of their industry of previous employment. Although this study is based on a sample of unemployed workers from New Jersey, our sample has similar characteristics as a national sample of unemployed workers.

A. Barriers to Re- Employment

Our research also suggests that training is beneficial to those facing known barriers to re-employment. Studies have shown that training for disadvantaged workers can improve their odds of re-employment (King, et. al., 2000). We find that among ITG participants, high school dropouts and older white males tend to have a 7 to 8 percentage point higher re-employment rates than their comparison group but experience no wage recovery advantage. Though high school dropouts enrolled in truck driving training do experience both a re-employment and wage recovery advantage. High school dropouts enroll disproportionately in truck driving training. While 8% of all ITG participants are enrolled in truck driving training, 34% of high school dropouts are. For females enrolled in the male-dominated fields of computer programming or engineering training, we find no re-employment advantage, but once employed these workers experience a wage recovery advantage over their comparison group. The advantage amounts to $758 greater wage recovery in the 8th quarter after claiming UI. Wage recovery is measured relative to wages earned in the 4th quarter *prior* to claiming UI. Together, these results suggest that training can assist those with barriers to employment to find jobs and/or alleviate wage loss.

B. Delayed Retirement

Examining how impacts vary by age is especially important given that research has demonstrated that wage growth decreases with age (Mincer 1974) (Murphy and

Welch, 1990). Jacobson, Lalonde, and Sullivan (2004) estimate that the social returns to training (incorporating private and social costs) at community colleges are lower for older unemployed workers than for younger unemployed workers. We build upon their results by examining how the returns to the ITG program vary by age, education, and gender.

Our cost-benefit estimates indicate that five groups, consisting of 25% of the sample, experience a gain in lifetime earnings (resulting from training) that exceeds the cost of foregone earnings while in training. The five groups are 1) female high school graduates who are age 50-54 at the time of claiming Unemployment Insurance (UI); 2) female high school graduates, age 60-64 at UI claim; 3) male high school graduates, age 18-49 at UI claim; 4) male high school dropouts, age 18-49 at UI claim; and 5) female high school dropouts, age 18-49 at UI claim.

Assuming retirement at 65, we estimate that the female high school graduates age 50 to 54 at the time of UI claim experience an average increase in lifetime earnings $313 greater than the wages foregone while enrolled in training. The approximate breakeven point (where the benefits from training just surpass the cost of foregone wages) occurs in the 11th year after claiming UI. The other four groups experience similar and some times higher net benefits from training.

Given that two of the five groups are over 50, these results tentatively suggest that government-sponsored training could be an incentive that induces older workers to delay their retirement. One-fifth of the population will be age 65 or older by 2030, according the U.S. Census Bureau. This raises concerns about the impact that retiring Baby Boomers will have on Social Security and Medicare spending, policy makers are weighing options for how to encourage workers to delay retirement. Results from this research demonstrate that some older unemployed workers who obtain training do experience lifetime wage gains greater than the cost of foregone earnings. Training can thus provide a cost-effective way to delay retirement and assist older workers in transitioning to “bridge jobs,” jobs unrelated to their career jobs (Cahill, Giandrea, and Quinn, 2005). However, these results should be taken as tentative results because the cost-benefit estimates do not account for the social costs incurred from the taxes levied to fund the program or possible taxes resulting from an increase in post-training earnings.

C. Informed Decision Making

Our research indicates wage recovery advantage for males and females enrolled in computer programming or engineering training. Further, on average, participants enrolled in truck driving training experienced a $370 greater wage gain in the 7th quarter after claiming UI. This suggests that training type matters with regard to wage recovery impacts. Therefore, although on average training had no impact on wage recovery, truck driver training, computer programming, and engineering-related training are associated with higher wage recovery. This implies that type of training is a critical variable that influences wage recovery, and confirms the value of providing information that helps guide a participant’s decision- making process. This suggests that governments should continue to provide information on prevailing wages, wage growth, and employment rates for occupations. Such information can assist the unemployed in making informed decisions on whether to enroll in training and, if so, the type of training to choose.

D. Program Evaluation

A central theme underlying all of our results is evaluation of the impact of a government-sponsored program: What would outcomes have been in the absence of program participation? In the absence of experimental (random assignment) designs, non-experimental methods are used to answer this question. This thesis expands on the debate over non-experimental methods by including stratified random sampling and Abadie-Imbens matching. The non-experimental evaluation literature tends to focus on propensity score matching because it reduces the curse of dimensionality, whereas both stratified random sampling and Abadie-Imbens match directly on covariates. Our results demonstrate that all three methods tend to produce coefficients that are similar in significance and sign, however are less similar in their magnitude. This shows the importance of relying on multiple estimates to establish a range for the magnitude of the estimated impact.

Our results also confirm Smith and Todd's (2005) finding that propensity score matching can be sensitive to ties. Ties occur when there are multiple comparison group candidates with the same propensity score as a given ITG participant. Results, consequently, can vary depending on which tie candidate is chosen. For the older white male sample, we find that in the 4th quarter after claiming UI, the wage recovery impact is statistically insignificant when one tie candidate is randomly selected. However, when all tie candidates are used, the wage recovery impact is statistically significant. This suggests the importance of using multiple estimations when tie candidates are present. More broadly, this thesis illustrates the importance of using multiple non-experimental methods to estimate the impact of a program because results can be sensitive to the methodology chosen.

IV. Future Research

Our evaluation of the ITG program showed that it can increase the odds of re-employment and that some types of training improve wage recovery. Conducting an experimental random assignment evaluation of the ITG program would improve this research because selection bias concerns are fewer in a randomized study. Random assignment reduces selection bias because it eliminates systematic pre-program differences between the training and non-training groups. Although experiments can be costly to set up, the value of minimally biased results can make them worth the investment. One experimental strategy could involve interactions between the Worker Profiling and Reemployment Services (WPRS) system and the ITG program. If there are a limited number of ITG slots available for those identified through WPRS, then one could address the shortage by randomly assigning people to the slots (Black et. al., 2003). Those not receiving an ITG grant would serve as the comparison group. Another option would be to use examine the marginal benefit an additional $1,000 by randomly awarding some participants a $4,000 voucher and others a $5,000 voucher. Results from such studies would provide an experimental benchmark for the results in this thesis and past ITG evaluations.

In addition to showing how wage recovery varies by training type, these results also show that groups with barriers to employment (high school dropouts and older workers) increase their odds of re-employment through program participation. Making these impact results publicly available may assist the unemployed in choosing among training options. Moreover, these impact results broaden the training evaluation literature.

To further improve the information available to ITG participants, we suggest examining the decision-making process itself. While much is known about the impacts of training in general, much less is known about how the unemployed make their decisions. A recent study compared how individuals choose training fields under three scenarios: counselor guided, moderately counselor guided, and no counseling required. Researchers found that the training take-up rate is highest for the no-counseling group and that the type of training chosen did not vary much across approaches. Nor did they find any meaningful differences between the employment and earnings rates of participants in the three approaches (McConnel, et al., 2006). However, the study did not examine the weight that job seekers give Labor Market Information (LMI) in their decision-making process.[2] When choosing a training area or making career transitions, what relative weight do the unemployed give to factors such as prevailing wage, employment information, word-of-mouth information, their personal interest in the area? Understanding these weights would be instrumental in identifying informational gaps on LMI web sites.

These suggested research extensions build on one of the principles of the ITG and ITA programs: A system that emphasizes informed decision-making among a wide range of training options. When the unemployed are guided by enhanced information, such as better impact estimates, information on high-demand occupations, and generally more relevant LMI, they can better plan for heir transition to a new job. Ideally, the additional information will improve U.S. labor market dynamics by shortening the time the unemployed spend out of the workforce.

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[2] Labor market information includes information on average wage in an occupation, employment rates of recent graduates, and projections of available job openings.

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Appendix A

Comparison of Matching Characteristics

Sample: White Males Age 51-65 at time of UI Claim in 1995															
		Stratified sample 1		Stratified sample 2		propensity score sample 1		propensity score sample 2		tie propensity score		Abadie-Imbens, 1 neighbor		Abadie-Imbens, 5 neighbor	
Matching Characteristic	ITG	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value
sample size	219	199	na	199	na	219	na	219	na	1,846	na	2,964	na	3,309	na
white	100%	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na
male	100%	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na
UI claim 1995	100%	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na
age 51-65	100%	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na
less than high school	3.7%	4.5%	0.96	4.5%	0.96	5.0%	0.74	5.0%	0.74	5.0%	0.91	3.7%	0.99	3.9%	0.97
high school	39.7%	38.2%	0.96	38.2%	0.96	37.9%	0.74	37.9%	0.74	39.7%	0.91	41.1%	0.99	41.2%	0.97
some college	22.8%	22.6%	0.96	22.6%	0.96	20.1%	0.74	20.1%	0.74	21.9%	0.91	21.5%	0.99	21.15%	0.97
college	33.8%	34.7%	0.96	34.7%	0.96	37.0%	0.74	37.0%	0.74	33.3%	0.91	33.8%	0.99	33.75%	0.97
UI claim 1st qt	31.1%	32.7%	0.27	32.7%	0.27	30.6%	0.99	30.6%	0.99	30.6%	0.94	28.8%	0.96	29.17%	0.93
UI claim 2nd qt	29.2%	21.1%	0.27	21.1%	0.27	28.8%	0.99	28.8%	0.99	28.8%	0.94	30.6%	0.96	28.34%	0.93
UI claim 3rd qt	20.5%	25.1%	0.27	25.1%	0.27	21.9%	0.99	21.9%	0.99	22.8%	0.94	21.5%	0.96	21.06%	0.93
UI claim 4th qt	19.2%	21.1%	0.27	21.1%	0.27	18.7%	0.99	18.7%	0.99	17.8%	0.94	19.2%	0.96	21.43%	0.93
s. atlantic region	11.9%	11.1%	0.96	11.1%	0.96	11.9%	0.77	11.9%	0.77	14.2%	0.72	11.9%	0.77	9.42%	0.46
northern region	66.7%	67.8%	0.96	67.8%	0.96	69.4%	0.77	69.4%	0.77	22.4%	0.72	18.7%	0.77	18.5%	0.46
souther region	21.5%	21.1%	0.96	21.1%	0.96	18.7%	0.77	18.7%	0.77	63.5%	0.72	69.4%	0.77	72.08%	0.46
agriculture	0.5%	0.5%	1.00	0.5%	1.00	0.0%	0.84	0.0%	0.84	0.0%	0.34	0.5%	1.00	0.46%	1.00
construction	2.7%	3.0%	1.00	3.0%	1.00	1.4%	0.84	1.4%	0.84	1.4%	0.34	2.7%	1.00	2.74%	1.00
finance	5.9%	5.5%	1.00	5.5%	1.00	3.7%	0.84	3.7%	0.84	5.0%	0.34	5.9%	1.00	5.94%	1.00
manufacture	34.2%	36.2%	1.00	36.2%	1.00	34.2%	0.84	34.2%	0.84	25.1%	0.34	34.3%	1.00	34.08%	1.00
public admin.	3.7%	3.5%	1.00	3.5%	1.00	5.0%	0.84	5.0%	0.84	6.4%	0.34	3.7%	1.00	3.65%	1.00
retail trade	9.1%	9.0%	1.00	9.0%	1.00	11.0%	0.84	11.0%	0.84	11.4%	0.34	9.1%	1.00	9.22%	1.00
service	23.7%	23.1%	1.00	23.1%	1.00	24.2%	0.84	24.2%	0.84	25.6%	0.34	23.7%	1.00	23.82%	1.00
transport. & utilities	6.8%	7.0%	1.00	7.0%	1.00	7.8%	0.84	7.8%	0.84	9.6%	0.34	6.9%	1.00	6.85%	1.00
wholesale trade	13.2%	12.1%	1.00	12.1%	1.00	12.8%	0.84	12.8%	0.84	15.5%	0.34	13.2%	1.00	13.24%	1.00
tenure group1	39.7%	39.2%	0.99	39.2%	0.99	41.6%	0.91	41.6%	0.91	35.6%	0.32	39.7%	1.00	40.2%	1.00
tenure group2	31.1%	32.2%	0.99	32.2%	0.99	30.6%	0.91	30.6%	0.91	29.7%	0.32	31.1%	1.00	31.27%	1.00
tenure group3	12.3%	12.6%	0.99	12.6%	0.99	13.2%	0.91	13.2%	0.91	18.7%	0.32	12.3%	1.00	12.18%	1.00
tenure group4	16.9%	16.1%	0.99	16.1%	0.99	14.6%	0.91	14.6%	0.91	16.0%	0.32	16.9%	1.00	16.35%	1.00

Sample: High School Dropouts Previously Employed in Manufacturing

		Stratified sample 1		Stratified sample 2		propensity score sample 1		propensity score sample 2		tie propensity score		Abadie-Imbens, 1 neighbor		Abadie-Imbens, 5 neighbor	
Matching Characteristic	ITG	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value	Comp.	p-value
sample size	277	251	na	251	na	277	na	277	na	293	na	2237	na	3,028	na
less than high school	100%	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na
manufacture	100%	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na	100%	na
age 18-36	39%	37%	0.93	37%	0.93	40%	0.51	40%	0.51	40%	0.53	36%	0.83	32%	0.26
age 37-50	39%	40%	0.93	40%	0.93	37%	0.51	37%	0.51	37%	0.53	41%	0.83	42%	0.26
age 51-65	21.7%	22.7%	0.93	22.7%	0.93	22.4%	0.51	22.4%	0.51	22.4%	0.53	22.8%	0.83	24.9%	0.26
male	63.2%	63.7%	0.89	63.7%	0.89	67.1%	0.33	67.1%	0.33	66.8%	0.37	65.7%	0.53	66.0%	0.48
white	30.7%	31.5%	0.98	31.5%	0.98	27.1%	0.61	27.1%	0.61	27.1%	0.61	30.0%	0.98	30.9%	1.00
black	14.8%	14.7%	0.98	14.7%	0.98	16.6%	0.61	16.6%	0.61	16.6%	0.61	14.8%	0.98	14.9%	1.00
Hispanic	54.5%	53.8%	0.98	53.8%	0.98	56.3%	0.61	56.3%	0.61	56.3%	0.61	55.2%	0.98	54.2%	1.00
tenure group1	41.9%	43.0%	0.99	43.0%	0.99	44.4%	0.67	44.4%	0.67	44.0%	0.70	41.9%	1.00	42.1%	1.00
tenure group2	36.5%	35.5%	0.99	35.5%	0.99	33.6%	0.67	33.6%	0.67	33.9%	0.70	36.5%	1.00	36.5%	1.00
tenure group3	9.4%	9.2%	0.99	9.2%	0.99	7.6%	0.67	7.6%	0.67	7.6%	0.70	9.4%	1.00	9.2%	1.00
tenure group4	12.3%	12.4%	0.99	12.4%	0.99	14.4%	0.67	14.4%	0.67	14.4%	0.70	12.3%	1.00	12.2%	1.00
UI claim 1st qt	35.7%	27.9%	0.02	25.5%	0.02	31.8%	0.23	31.8%	0.23	31.8%	0.23	35.0%	0.99	35.8%	1.00
UI claim 2nd qt	20.9%	22.7%	0.02	23.5%	0.02	17.3%	0.23	17.3%	0.23	17.3%	0.23	20.9%	0.99	20.8%	1.00
UI claim 3rd qt	22.7%	18.3%	0.02	21.1%	0.02	30.0%	0.23	30.0%	0.23	30.0%	0.23	22.4%	0.99	22.5%	1.00
UI claim 4th qt	20.6%	31.1%	0.02	29.9%	0.02	20.9%	0.23	20.9%	0.23	20.9%	0.23	21.7%	0.99	20.9%	1.00
UI claim 1995	22.7%	22.7%	1.00	22.7%	1.00	22.4%	0.80	22.4%	0.80	22.4%	0.80	22.4%	1.00	22.7%	1.00
UI claim 1996	10.1%	10.0%	1.00	10.0%	1.00	13.4%	0.80	13.4%	0.80	13.4%	0.80	10.5%	1.00	10.5%	1.00
UI claim 1997	10.1%	10.0%	1.00	10.0%	1.00	10.8%	0.80	10.8%	0.80	10.8%	0.80	10.1%	1.00	10.3%	1.00
UI claim 1998	21.3%	21.9%	1.00	21.9%	1.00	19.9%	0.80	19.9%	0.80	19.9%	0.80	21.3%	1.00	20.9%	1.00
UI claim 1999	35.7%	35.5%	1.00	35.5%	1.00	33.6%	0.80	33.6%	0.80	33.6%	0.80	35.7%	1.00	35.6%	1.00
northern region	82.7%	82.5%	0.52	82.9%	0.55	81.9%	0.97	81.9%	0.97	82.0%	0.97	82.7%	1.00	83.7%	0.94
souther region	13.4%	15.1%	0.52	14.7%	0.55	13.7%	0.97	13.7%	0.97	13.7%	0.97	13.4%	1.00	12.3%	0.94
s. atlantic region	4.0%	2.4%	0.52	2.4%	0.55	4.3%	0.97	4.3%	0.97	4.3%	0.97	4.0%	1.00	4.0%	0.94

Curriculum Vita
Leela Hebbar

Education

Rutgers University
Ph.D., Planning and Public Policy 2008
M.A., Economics 1998

University of Cambridge
M.Phil., Development Studies 1996

Florida State University
B.A., Economics, Minor Biology 1995

Employment

MGT of America, Inc., Tallahassee, FL
Consultant, July 2007-present
Senior Analyst, June 2006-July 2007

The John J. Heldrich Center for Workforce Development, Rutgers University, New Brunswick, NJ
Project Manager July 2003-February 2004
Project Director, July 2000-July 2003
Research Assistant, July 1998-July 2000

Publications

"Vocational Training for the Unemployed: Its Impact on Uncommonly Served Groups," *International Journal of Manpower*, Vol. 27, No. 4, pp 377-395, 2006.

"Apprenticeships," in *Work in America: An Encyclopedia of History, Policy, and Society* Edited by Carl Van Horn and Herbert Schaffner, Santa Barbara, California: ABC-CLIO, 2003.

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Needs Assessment Study: Employ Florida Banner Center for Homeland Security and Defense. Indian River, Florida: Indian River Community College, May 2007.

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Salary and Benefit Study, Sarasota, Florida: New College of Florida, November 2006.

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