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The Promise of Worker Training: New Insights into the Effects of Government Funded Training Programs

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THE PROMISE OF WORKER TRAINING: NEW INSIGHTS INTO THE EFFECTS OF GOVERNMENT FUNDED TRAINING PROGRAMS

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THE PROMISE OF WORKER TRAINING: NEW INSIGHTS INTO THE EFFECTS OF GOVERNMENT FUNDED TRAINING PROGRAMS

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Using data on worker training programs in South Dakota over the years 2002–11, this study estimates the employment and earnings effects of occupational skills training and on-the-job training. Average treatment effects for the first and third calendar quarters after training are reported by: gender, worker type, demographic group, region of residence, and time period of job loss.

Both occupational skills training and on-the-job training effectively increased the employment rates and incomes of participants. The effectiveness of occupational skills training tended to grow as time passed, but the effectiveness of on-the-job training tended to fade over time. Three calendar quarters after leaving training, the effects of occupational skills training were generally higher than they were after only one quarter. In contrast, three calendar quarters after training, on-the-job treatment effects tended to be smaller than after one quarter.

On-the-job training had large impacts on employment but disproportionately small impacts on earnings. The employment effects of on-the-job training were typically 2 to 3 times larger than the employment effects of occupational skills training. But when considering earnings effects, the impacts of occupational skills training were often larger than those of on-the-job training.

Training was generally more effective for men than women in the period immediately following training, but after three calendar quarters the effects of training were typically

larger for women than for men. The short- and longer-run effects of training were greater for the non-dislocated jobless than for displaced workers. The demographic results showed that Native Americans benefited more from training than did any other demographic groups.

From a regional perspective, training effectively increased employment rates and incomes for both rural and urban areas, with the impacts being slightly larger for rural areas than for urban. Finally, training was less effective in the wake of the 2007–09 recession than beforehand. Training had no significant employment effects and only modest income effects following the Great Recession.

The results reported in this study suggest that occupational skills training and on-the-job training effectively increased employment rates and quarterly earnings across numerous sub-populations, regions, and time periods. The findings of this study will help guide policy makers going forward so that they might maximize the potential impacts of worker training programs.

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Contents

List of Tables

List of Figures

Chapter 1

Worker Training Programs as Active Labor Market Policy: A Review of Programs in the United States

1.1 Introduction

The United States labor market has seen many ups and downs in its history. Without a doubt, the recent past has witnessed a down period like few others. The recent 2007–09 recession was the deepest and most protracted slowdown since the Great Depression. The civilian unemployment rate peaked at 10% in October 2009. Perhaps more troubling though has been the sclerotic recovery of both national and regional labor markets. In July 2011 the mean duration of unemployment crossed the 40 week threshold and remained near that level through October 2012. At no time since 1948, when calculation of this figure began, had the average unemployment spell ever exceed 25 weeks. Yet it has continuously exceeded 25 weeks since July 2009.

The nation has clearly been stuck in a so-called "jobless recovery," characterized by the return of steady, albeit modest, GDP growth but only tepid employment growth. One potentially contributing factor to the slow labor-market recovery is more pronounced skills mismatch in the economy. In recent years the unemployment rate has remained stubbornly high while job vacancies have simultaneously increased (Pissarides, 2011). It seems that the Beveridge curve has shifted outward, indicating greater matching inefficiency in the labor market. As a result, policy makers have become increasingly concerned with the lack employment growth, and with potential countermeasures.

This confluence of high and extended unemployment, seemingly increased skills mismatch, and stalled wage growth has led to greatly renewed interest in continuing education and worker training programs. Might such programs succeed and help return the labor market and the wider economy to health? Training programs promise to change workers by providing them with new skills and access to new opportunities that they would not have otherwise. In the current economic environment such active labor market programs might be much more effective at fostering employment than traditional passive policies, i.e. unemployment insurance or food stamps.

A successful training program must have two qualities. First, it must improve the employment prospects of those who participate. Second, the program should lead to higher earnings for participants. Do today's training programs provide these benefits or do they fall short? This study provides economists and policy makers with much needed insights into whether current worker training programs are accomplishing the above goals.

Analysis of training programs is notoriously difficult, and few data exist that allow for even cursory estimation of program effectiveness. What limited data do exist come from periodic surveys, and do not contain detailed and accurate records regarding: (1) the type of training, (2) what occupations persons trained for, (3) the timing of program participation, (4) pre- and post-training employment status, (5) or even pre- and post-training earnings. Additionally, detailed geographic records are often absent leaving, the researcher unable to control for idiosyncratic regional labor-market characteristics.

The ideal data for training evaluation would address all of the shortcomings listed above. Unfortunately, there are no publicly available federal, or even state, level data sets that can overcome these failings. Fortunately, this dissertation introduces a novel data set that can address the usual data shortcomings. This study uses a unique administrative data set created by the South Dakota Department of Labor and Regulation (SDDLR) for its internal evaluation of State funded worker training programs. The data derive from SDDLR's administrative records and contain very detailed micro-level information on individuals, training programs, and employment outcomes.

The administrative records contain all of the crucial information necessary for a thorough treatment evaluation. Reporting on all persons accessing the State's employment services between the years 2002–11, the data follow individuals from registration, through training, and for up to a year afterwards. The data detail: (1) whether persons were enrolled in classroom versus on-the-job training, (2) the occupation for which a trainee trained, (3) when individuals registered for and stopped using SDDLR's employment services, and, finally, (4) pre- and post-program employment status and earnings.

No prior study can boast such an expansive and detailed sample. Thus, with this data set it is possible to evaluate worker training in ways that have not been possible before. This study is the first to explore the impacts of worker training over an entire decade. Moreover, it provides new and important insights into the benefits of training: for Native Americans, for workers in rural versus urban areas, and for workers displaced at different points over the business cycle.

The remainder of this dissertation is constructed as follows. I begin with a discussion of the history of worker training programs in the United States. I trace out the history of worker training in the post-war period, as well as, the evaluations of these programs. Thereafter I provide detailed descriptions of the programs evaluated in this study and the new and unique data employed. Following the data discussion I describe the empirical methods used to estimate the treatment effects of current training programs. The narrative then moves on to an in-depth investigation into the effectiveness of Workforce Investment Act training programs. I assess the training programs from several different aspects and provide extensive commentary regarding my results. Finally I summarize the conclusions and policy implications of the findings of this dissertation.

1.2 A History and Review of Training Programs in the United States

Perhaps the first nationwide employment programs in the United States grew out of the Roosevelt administration's New Deal programs of the 1930s and 1940s. These Depression era programs, such as the Civilian Conservation Corps, Civil Works Administration, and Works Progress Administration were a first of their kind. As opposed to more modern employment programs these New Deal programs were relatively narrow in focus. Primarily concerned with providing employment to the greatest number of persons possible, these Roosevelt-era programs tended to focus on large scale infrastructure and public works projects — e.g. roads, dams and bridges. Following World War II there was little need for such employment programs, and the government abandoned them as the economy enjoyed sustained post-war prosperity.

The 1960s saw renewed interest in ambitious nation-wide employment programs. Successive Congresses have enacted or re-approved national employment and training programs with regularity. Each program evolved from those that came before, but each has contained a worker training component that was central to its mission.

In order to understand the important contributions made by this study, one must first understand the programs that have come before. I begin with the Kennedy administration in the early 1960s (with all dollar amounts quoted in constant 2004 dollars).

1.2.1 Manpower Development Training Act

In 1962 Congress approved the Manpower Development Training Act (MDTA), which ushered in the modern era of national employment assistance programs. O'Leary et al. (2004) explain that MDTA was not a general employment support program in the spirit of previous New Deal era programs. MDTA legislation had aimed to combat rising worker dislocation and displacement, but in a smaller scale and more targeted manner than had New Deal programs. MDTA's mission changed, however, in 1964 with passage of the Economic Opportunity Act. After modification the MDTA primarily focused on providing employment and training services for disadvantaged youths and welfare recipients (Lalonde, 1995).

Funding for MDTA programs was controlled at the national level by the Bureau of Labor Statistics (BLS), but was administered through twelve regional offices that reviewed grant proposals submitted by the states. The regional system allowed for some flexibility and variation, but regional funding decisions were largely controlled from Washington (Bradley, 2013).

The centralized nature of the MDTA was one of its primary criticisms. States and localities complained that the MDTA's funding mechanisms prevented them from experimenting and developing unique programs to suit their local labor markets. As a result, subsequent legislation has continued to divest both program administration and funding decisions towards states and localities (Leigh, 1990).

MDTA training programs primarily included classroom instruction (CI) and on-the-job training (OJT), with classroom instruction receiving most of the attention.[1] During the period 1962–68, 1.34 million persons enrolled in an MDTA training program. Slightly more than two-thirds of those (714,000) enrolled in some form of classroom instruction, while the remaining portion (321,000) enrolled in an on-the-job training program. CI enrollment dominated in most every region of the US. Interestingly, South Dakota was the only state to buck the national trend and saw greater enrollment in the OJT program than in classroom instruction (United States Department of Labor, 1968).[2]

No clear consensus ever developed concerning the effectiveness of MDTA training programs. Researchers, then as now, focused on estimating the impacts of training on the post-training earnings of participants. One advantage to the MDTA evaluations was that they were all based on national data sets, and therefore supported very large and representative samples. Nevertheless, the results of MDTA evaluations were mixed and several studies at the time found conflicting results.

Perhaps the first major analysis of MDTA training programs was conducted by Ashenfelter (1978), who studied a single 1964 cohort of MDTA participants. Ashenfelter compared the earnings paths of trainees and a random sample drawn from the BLS'

[1] Unlike future classroom instruction or similar training, CI under MDTA was provided primarily by contracting entities and enrollees had little control over the types of training that they could receive. Potential classes were predetermined and trainees had little to no choice in providers.

[2] The District of Columbia also had greater enrollment in OJT than in classroom instruction. All other states and territories had greater enrollment in classroom instruction.

Continuous Work History Sample. Using a difference-in-differences approach, he compared the impacts of training across gender and racial groups. No distinction was made for different types of training. He found that training raised the yearly earnings of black and white males by \$751 and \$1,084 respectively. Black and white females increased their yearly incomes by \$2,226 and \$2,515. Later studies tended to find much smaller impacts for males and more varied impacts for women.

Gay and Borus (1980) studied the outcomes of MDTA trainees who began training between the years 1968 and 1970. They estimated a series of cross-sectional earnings equations to assess the effects of training on yearly earnings. Similar to Ashenfelter, they found that training benefited females more than males, but the relative effects were much larger — participation increased white and black females' earnings by \$4,816 and \$1,323 but only increased white and black male earnings by \$566 and \$527.

Not all researchers found evidence of clear and positive gains from training, however. Lalonde (1995) cited Kiefer (1979), for example, which actually found negative earnings impacts for male MDTA enrollees. Lalonde explained that Kiefer (1979) found annual earnings *penalties* of \$2,413 and \$2,673 for white and black men who participated in MDTA training programs. Very few other studies found such large and negative effects from training.

Much of the disagreement between the aforementioned studies stemmed from the use of differing comparison groups and estimation methods that did not account for possible selection biases. Unfortunately, researchers never did agree upon common methods and no definitive study of MDTA training ever materialized. Ultimately, Congress allowed MDTA to expire in 1969. The nation was not long without an overarching employment and training regime, though, and new legislation emerged before the close of President Nixon's tenure in the White House.

1.2.2 Comprehensive Employment and Training Act

In 1973 Congress passed the Comprehensive Employment and Training Act (CETA), which, though largely an evolution of MDTA, introduced several important changes and innovations. First, CETA placed a greater emphasis on youth services. Second, training options were expanded to include public service employment (PE) and work experience (WE) in addition to the CI and OJT programs of the MDTA (Barnow, 1987; O'Leary et al., 2004).[3] Third, CETA divested the BLS of some oversight powers and allowed States more control over program administration and funding. Lastly, CETA called for the creation of local advisory boards representing both private and public interests to guide and shape training programs in their regions (Bradley, 2013; O'Leary et al., 2004).

Once again researchers reported varying, and sometimes conflicting, estimates of program effectiveness. Although economists achieved greater consensus in the construction of estimation samples, which enabled direct comparison of some studies, disagreements remained regarding the magnitudes of impact estimates. Barnow (1987) has provided an excellent and thorough review of this literature.

The US Department of Labor (USDOL) contracted with Westat Inc. to collect, administer, and analyze the data collected on CETA programs. Consequently, Westat Inc. (1981, 1982) and Westat Inc. (1984) were the only final assessments of CETA programs, official or otherwise, to be published prior to the sunsetting of CETA in 1982. These official reports provided the first insights into program performance, and subsequent researchers were greatly influenced by Westat's estimation and sampling methods.

Barnow (1987) wrote that Westat Inc. (1981) isolated a sample of CETA partici-

[3] CETA public employment and work experience training programs were highly similar. WE trainees received subsidized employment in the private sector firms, while PE trainees were given jobs in the public sector.

pants for study.[4] At the same time, Westat created a comparison group from Current Population Survey (CPS) records using a cell matching technique whereby treated and comparison records were matched based on: gender, race, age, education, family income, and several other socioeconomic variables. Several subsequent studies used these same methods and samples with only slight modifications (Westat Inc., 1984; Bassi, 1983; Geraci, 1984).[5] As a result, the decisions and methods of Westat greatly influenced the work of many subsequent researchers.

Even though several studies used the same treatment and comparison samples, they still disagreed about the significance and magnitude of training treatment effects. For simplicity I will only refer to the overall effects of training, and do not discuss the separate effects of classroom instruction, on-the-job training, public service employment, or work experience.

Lalonde (1995) and Barnow (1987) reported significant annual effects for white and minority women. These effects ranged between \$402–\$1,857 for white women and between \$1,247–\$1,664 for minority women. The effects for males were much smaller, rarely significant, and sometimes negative (Westat Inc., 1981, 1984). Bassi (1983) similarly found that training had no effect on male earnings, but that training increased the annual earnings of white and minority females by \$2,375 and \$1,664.

Later studies attempted to improve upon the econometric methods established by Westat, but were hampered by problems of their own. One of the first such studies was Bloom and McLaughlin (1984). The authors improved upon previous work by using a fixed effects estimation technique to account for individual heterogeneity. Using this

[4] Sample eligibility was based on participant age, family income, and enrollment in at least one CETA training program.

[5] Westat Inc. (1984) did not require prior year family income to be less than 30,000 in the treatment group. Geraci (1984) used the same treatment sample as Westat Inc. (1984) but excluded youths under twenty-two years old. The comparison group and matching procedure in Geraci (1984) and Westat Inc. (1984) was identical to that of Westat Inc. (1982).

method they estimated that training increased female participants' annual earnings between $\$1,604$ and $\$3,209$. In accordance with prior work, the authors found no statistically significant training effect for men. Unfortunately, the authors used unique treatment and comparison samples, making it difficult to compare directly their results with those of previous researchers.[6]

Two additional CETA studies also merit discussion. Dickinson et al. (1986) was notable for its use of nearest neighbor matching in order to create its comparison group.[7] This method was used to match records on CETA enrollees during calendar year 1975 to CPS records from the same period. Nearest neighbor matching was the most sophisticated matching technique used at the time, yet it was not without shortcomings. The nearest neighbor method is highly sensitive to the ordering of observations, and changing the sorting order can drastically alter the composition of the comparison group.

Controversially, Dickinson et al. found that training had a strong and significantly *negative* impact on male earnings. This was the only CETA evaluation to report such negative impact estimates. Male CETA trainees earned an estimated $\$1,582$ less in 1978 than males in the comparison cohort. The effects of training were not significant for females. These results did not agree with any previous studies, but it is

[6]Bloom and McLaughlin (1984) used unique treatment and comparison samples that differed from the official samples used in Westat Inc. (1981) and Westat Inc. (1984) in key ways. The first changes involved new eligibility requirements. The minimum age requirement was raised from 14 to 25 years, and income restrictions were removed. Second, Bloom and McLaughlin (1984) did not use a matching mechanism to ensure similarity of the treatment and comparison group. All eligible CPS records were used in the comparison group.

[7]Dickinson et al. (1986) performed a one-to-one match of treatment and comparisons records. Treatment records were matched to their nearest neighbor in the comparison group based on the Mahalanobis distance between the two. Originally developed by Mahalanobis (1936), the Mahalanobis distance is a measure of the geometric distance between two vectors. Dickinson et al. (1986) used a modification of this measure to calculate the "distance" between observations in the treatment and comparison samples. Page 5 of DJW explains that the distance measure was calculated as $D = (X_1 - X_2)S^{-1}(X_1 - X_2)$, where D was the distance, X_1 was a vector of matching covariates in the treatment sample, X_2 was a similar vector for the comparison group, and S^{-1} was the covariance matrix of the matching regressors.

difficult to pinpoint the cause given the novel matching methods, and unique sample construction.[8]

As had happened in the previous MDTA period, estimates of program effectiveness under CETA were highly variable across multiple investigations.[9] Westat Inc. (1984), in its role as the contracting entity for the USDOL, provided the final policy analysis of CETA training programs, but academic researchers never reached agreement on either the effects of participation or the proper methods of measurement. The disagreements between various studies, largely stemming from different matching procedures, left too many questions unanswered and provided inadequate guidance for policy makers.

1.2.3 Job Training Partnership Act

In the early 1980's the Reagan administration pushed for the replacement of CETA. The primary impetus for change came from the administration's desire to end the public employment programs authorized by CETA. The public employment program had come under numerous attacks for corruption and waste (Barnow, 1987; Friedlander et al., 1997). As a result, the program had "[become] a target for national media

[8]Dickinson et al. (1986) (DJW) measured the treatment period as the 1975 calendar year (Jan. – Dec. 1975) as opposed to the 1976 fiscal year (July 1975 – June 1976) which was used by Westat Inc. (1981); Bassi (1983); Westat Inc. (1984) and Bloom and McLaughlin (1984). This was potentially problematic because the first two quarters of calendar year 1975 were considered experimental by Westat (Barnow, 1987). In another departure with previous work, DJW did not impose a "time in program" eligibility restriction. Previous studies had required treated persons to have spent at least seven to ten days in their training program. DJW dropped this restriction. Lastly, DJW did not attempt to control for any sort of individual heterogeneity or selection bias and used simple dummy variables in OLS regressions to measure the effect of training.

[9]CETA evaluations were particularly sensitive to whether treatment and comparison records were matched based on prior earnings. (Heckman et al., 1999, p. 2066) explains that,

> [...] substantial bias may result when evaluators create comparison groups by matching on serially correlated pre-program outcomes. Matching on such variables alters the properties of the unobservables in the comparison sample in ways that do not guarantee that it will mimic the unobservables of trainees during the post training period. The bias introduced by this practice in the CETA studies can account for their sharply different estimates.

criticism when careless management of funds and enrollment of program ineligibles were widely reported" (O'Leary et al., 2004, p. 8–9). In the face of such criticisms, Congress and the Reagan administration were keen to end the program.

Ultimately CETA was replaced in October 1982 by the Job Training Partnership Act (JTPA). In addition to eliminating public service employment, JTPA deviated from CETA in several important ways. Three of the more important changes are as follows.

First, JTPA expanded upon CETA by creating and extending employment services to two separate, but related, constituencies. Title II of JTPA followed MDTA and CETA by authorizing training for the economically disadvantaged.[10] Eligibility for Title II employment services was largely based on family income and receipt of welfare benefits, e.g. food stamps or Aid to Families with Dependent Children (AFDC). Title III of JTPA authorized employment and training benefits for displaced workers. Initially secondary in importance to Title II, Title III's focus on dislocated workers would increasingly come to define JTPA's mission (U.S. General Accounting Office, 1989).

Introduction of accountability standards was the second important innovation of JTPA. JTPA required that training programs be evaluated against performance standards set by the US Secretary of Labor, and administrators were subject to rewards and sanctions based on their performance. In addition to establishing performance standards, JTPA also authorized several "demonstration" projects and one national study to evaluate the performance of JTPA training programs. These projects are discussed in greater detail below.

The third major innovation was the creation of Service Delivery Areas (SDAs).

[10]Title II-A of JTPA dealt with disadvantaged adults, and Title II-C dealt with disadvantaged youths.

JTPA authorized over six hundred SDAs across the country. Each SDA was governed by a local advisory board charged with administering JTPA employment programs within its SDA. Importantly, private sector interests were represented on local advisory boards. Industry representation was intended to ensure that training programs would be directed towards the needs of local markets. Giving local advisory boards control over employment services in their SDAs represented a further decentralization of decision making and program administration under JTPA.

JTPA Demonstrations.

As discussed above, no consensus developed around the effectiveness of CETA training programs, mainly due to disagreements about proper non-experimental evaluation methods. JTPA therefore authorized several experimental demonstrations to evaluate JTPA training programs. The JTPA demonstrations involved specific and targeted training programs that were often confined to one or two SDAs at a time. These projects were typically small, allowing administrators to control program access and execution. Unfortunately, many of the demonstrations were afflicted by the same issues as prior non-experimental studies.

The first issues with the demonstration projects were the demonstration sites themselves. USDOL chose demonstration sites from competitive proposals submitted by SDAs across the US. The demonstration sites ultimately chosen by USDOL were supposed to be representative of the greater United States, but some researchers disagreed. Heckman and Smith (1995, p. 104) claimed that incentives offered by USDOL biased the pool of potential demonstration sites, and thus ensured that impact estimates could not be generalized. On the other hand, Friedlander et al. (1997, p. 1821) argued that such bias is unlikely as, "[t]here may be, however, only minimal correlation between local operators' self-appraisals and the results of a rigorous third-party evaluation.[11]"

The first of the major JTPA demonstrations was known as the Downriver Demonstration. Occurring in two phases between 1980 and 1983, the Downriver Demonstration studied the employment outcomes of males dislocated from automobile and automobile part manufacturing plants in Detroit and surrounding areas. The first phase studied workers displaced from BASF and DANA auto part manufacturing plants. The second phase followed workers laid off from Ford and Chrysler manufacturing facilities. Eligible workers self-selected into treatment, but comparison workers were chosen at random.

Confoundingly, Leigh (1990) reported that training had a positive and significant impact on workers in the first phase, but had no statistical effect on earnings for the second phase. Weekly earnings for treated workers in phase one rose between $60 and $221, while weekly earnings in the second phase declined between $4 and $35.

USDOL authorized a second demonstration in Buffalo, NY between 1982 and 1983. The Buffalo Demonstration included two impact studies. The first study compared the outcomes of workers displaced from steel and auto manufacturing. Subjects were randomly assigned to either a treatment group or a comparison group. The second study compared outcomes for a diverse group of workers displaced from local businesses. Workers in the second study self-selected into either the treatment or control groups. In both studies treated workers were offered training services but treatment was not mandatory.

Neither classroom training nor on-the-job training had any statistical impact on post program earnings in the study with random assignment. The second study, where displaced workers were allowed to self-select into training, found very large treatment

[11]See Footnote 22 on page 1821 of Friedlander et al. (1997) for the relevant discussion.

effects for OJT. In the second study OJT increased weekly earnings by \$247, which translated into an annual treatment effect of \$12,350 (Leigh, 2000). No evaluation before or since has found such extremely large income effects for OJT. Once more, however, there was cause to question the findings.

Bloom (1990) pointed out that participation rates among the treatment groups were very low. Only 16% of workers who were offered treatment in the first study actually enrolled in a training program. The figure was slightly higher in the second study, but, even then, only 26% of those offered services actually availed themselves of training. In both instances, low participation rates presented significant opportunities for selection bias.

Another well known USDOL demonstration was the Texas Worker Adjustment Demonstration (Texas Demonstration) of 1982–85. Unlike the Downriver or Buffalo Demonstrations, the Texas Demonstration included many women and minorities, had high participation rates, and randomized samples across multiple treatment levels.[12]

The primary Texas Demonstration sites were Houston and El Paso. The Houston sample was comprised of mainly white males with a large number of blacks as well. The El Paso sample, on the other hand, was mainly female and almost entirely Hispanic. Individuals were randomly assigned to one of three cohorts: Tier I, Tier I/II, or control. Persons assigned to the Tier I cohort were provided with basic job search assistance (JSA). Tier I/II provided mixed services, where JSA was freely available and classroom training or on-the-job training was later provided if necessary. The control cohort was offered no JTPA services but was notified of other non-demonstration services available in local communities.

[12]See Bloom (1990) for a complete and exhaustive review of the Texas Demonstration project. Howard Bloom was the principle investigator for the Texas Worker Adjustment Demonstration. Bloom (1990) reviews and updates the work he and others conducted at Abt Associates Inc. while under contract to evaluate the Texas Demonstration.

Abt Associates Inc. calculated impact estimates for the first year following training. The findings reinforced the findings of many previous CETA studies. All impact estimates are from Bloom (1990). JSA and mixed services raised the quarterly earnings of men by \$680 in their second quarter following treatment. There was no statistically significant impact on earnings in any other quarter for male participants. Women, however, fared much better than did males.[13] Participation had a significant and positive impact on female earnings in the first, second, and third quarters following completion of training. The total yearly impact on female earnings was $\$1,660$.

Perhaps the most authoritative and well known of the JTPA demonstrations was the New Jersey UI Reemployment Demonstration (New Jersey Demonstration). The New Jersey Demonstration was the last and largest of the JTPA demonstration studies. The purpose of the demonstration was to study whether training programs could be used as an early intervention that could prevent long-term unemployment.

Evaluators identified a population of displaced UI recipients who did not have a definite recall date from their previous employer. Roughly $10,000$ of these unemployed workers were selected for the demonstration. Participants were required to have: (1) collected at least five weeks of unemployment between the summer of 1986 and the fall of 1987, (2) been twenty-five or older, and (3) to have had at least three years of tenure at their previous job.

Once identified, individuals were randomly assigned to either one of three treatment groups or a control group. All persons were given basic job search assistance (e.g. skill assessments, interview counseling, and a job-search workshop), but only individuals selected for treatment were given access to additional services.

Corson et al. (1989) have explained that the first treatment group was given more intensive job search assistance. For example, program staff would maintain

[13] Female impact estimates were only available for the El Paso demonstration site.

weekly contact with subjects and provide them with updates on job opportunities. Additionally, these persons were given access to technology to facilitate their job search. The second treatment group given access to classroom training and on-the-job training in addition to intensified JSA.[14] Individuals in the third treatment group received intensive JSA, but were also given the opportunity to collect a reemployment bonus, that is a cash payment, if they quickly found employment on their own.

Results from the New Jersey Demonstration were positive but small in magnitude, and indicated that JTPA training had generally increased both the employment rates and earnings of participants. First, Corson et al. (1989) found that all three training cohorts had elevated employment rates. The differences were only significant, however, in the first three quarters for the JSA only cohort, and only in the first two quarters for the cohorts provided training or an employment bonus.[15] In all cases the employment rate differential ranged between two and five percentage points.

Evaluators found even smaller earnings impacts. The JSA only cohort had statistically higher earnings in the first and third quarters, $190 and $400 respectively, but Corson et al. (1989) found no impact in the third and fourth quarters. The training cohort responded even more poorly to treatment. Corson et al. found no statistically meaningful training effect within the first year, nor even the subsequent six months.[16] Finally, the reemployment bonus cohort experienced similar earnings effects to the JSA only cohort. Earnings for this cohort increased by $243 and $423 in the first and second quarters. There was no statistical effect in the third and fourth quarters.

[14] Classroom training programs were restricted to six weeks or less. This differs from current classroom training programs which can last up to two years.

[15] All quarters are measured relative to the quarter in which subjects initially filed for UI benefits.

[16] Suspecting that the effects of classroom training might take longer to manifest, Corson et al. (1989) followed this cohort for an additional six months in an attempt to measure a classroom training effect. The authors were unable to detect any changes relative to the control cohort. However, severe sample attrition in the post study period may have biased these results. See Corson et al. (1989, p. 289) footnote 7.

In summary, the demonstration projects authorized by USDOL were an attempt to provide definitive evaluations of JTPA training. Unfortunately, the various demonstrations were unable to provide such an analysis, as treatment effect estimates varied greatly. Treatment effects estimates ranged from $12,350 in the Buffalo Demonstration, to only $680 per year in the Texas Demonstration. The New Jersey Demonstration even found that training had no significant affects on earnings in the first year after training. It seems that the demonstration projects were simply too unique, and their results were of limited applicability beyond their original time and place.

The National JTPA Study.

In and effort to produce a more broadly applicable treatment evaluation, USDOL moved away from small demonstration projects, and authorized a national evaluation study. The National JTPA Study (NJS) was the final major impact analysis of JTPA employment and training programs. Running from November 1987 through September 1988, NJS was the only national evaluation of JTPA. The NJS sample was drawn from 16 of the 600 plus SDAs across the country. Unlike prior demonstration evaluations, SDAs did not submit project proposals; rather, DOJ contacted over 200 SDAs and requested their participation in the study of ongoing JTPA training programs. Only 16 of the 200 petitioned sites agreed to participate (Doolittle and Traeger, 1990).

Even though only 16 sites were willing to participate, NJS was the largest training evaluation to date, with more than 20,000 persons included in the study. Individuals were assigned to either treatment or control groups. The treatment group was further subdivided into three different subgroups: (1) classroom training/basic education, (2) OJT/JSA, and (3) other services. Unlike the New Jersey Demonstration, persons in the control group were barred from receiving any JTPA services. However, follow up surveys indicated that a meaningful fraction of control group assignees received similar services from non-JTPA sources (Bloom et al., 1993).

Bloom et al. (1993) reported the impacts of training on employment status and earnings for six quarters, or eighteen months. Program length was not equal for men and women, however. As a result, post-program impacts were reported for roughly 4 quarters in the male subsample, but only about 3 quarters in the female subsample.[17]

Results from the NJS reinforced much of the prior literature. Bloom et al. (1993) found larger absolute earnings and employment effects for the females than males. Additionally, training had more persistent and longer-term effects for women than for men. The only exception being that impact estimates for male employment rates in the OJT/JSA subgroup exceeded those of females. In general the effects of training were only significant for whites, and not for any other racial groups.

Regardless of the male/female disparities, Bloom et al. (1993) found that the training treatment effects were typically small in magnitude. The overall treatment effects of training ranged from \$91 in the second quarter to \$215 in the sixth quarter for females, and were significant in all but the first quarters. Over the entire 18-month study period, training was estimated to have only increased female earnings by \$812. In contrast, the impacts of training were only significant in the second and third quarters for men where training increased relative earnings by \$184 and \$210. The overall 18 month long earnings effect was not significant for men.

The largest treatment effects in the female sample were found in the OJT/JSA subgroup where JTPA services were found to have significantly increased earnings in

[17] One of the unique features of NJS relative to prior demonstrations was its analysis of currently ongoing JTPA programs. Therefore, the "program" as such was not strictly identified. Bloom et al. (1993) used an enrollment cutoff of 15% to identify and separate the study and post-study periods. The program was said to have ended when participation in training programs by the treatment group fell below the arbitrary 15% cutoff. For men the threshold was crossed in month 7, whereas, female participation dropped below the cutoff in month 10.

all but the second quarter. In this female subgroup, training increased overall earnings by $1,130. Classroom training also significantly influenced female earnings, but only in the fifth and sixth quarters by $220 and $286 respectively. Classroom training did not have a significant effect on overall earnings.

The largest subgroup treatment effects in the male sample were also found in the OJT/JSA subgroup. For these men, OJT/JSA significantly increased earnings in the third and sixth quarters by $250 and $306. The overall effect of $1,189 was also significant but only at the ten percent level. Bloom et al. (1993) found no significant effects of training in the classroom training/basic education subgroup.

1.3 Worker Training in the Present Day

1.3.1 The Workforce Investment Act

During the Clinton administration of the 1990s, concerns grew that that JTPA was no longer serving its intended purposes. Policy makers had become concerned that the entire system of JTPA employment services contained too many overlapping programs, resulting in redundancies and inefficiencies (U.S. General Accounting Office, 1994). In response to these concerns, Congress passed the Workforce Investment Act (WIA) of 1998 in an effort

> [t]o consolidate, coordinate, and improve employment, training, literacy, and vocational rehabilitation programs in the United States [...] (Workforce Investment Act, 1998, p. 1).

WIA, which is still in effect today, introduced several important innovations that might call previous results into question.[18] First, the new "customer focused" methods

[18] WIA will be replaced in July 2015 by the Workforce Innovation and Opportunity Act (WIOA). Passed in July 2014, WIOA is best characterized as an evolution of WIA. As of this writing, official

of service delivery, especially in relation to training, might mean that training today is more effective than in the past. In the past, training programs were one-size-fits-all. Now participants have the opportunity to complete their training with any number of approved providers.

Additionally, WIA gave states even more control over their workforce development activities when it replaced the local advisory councils with Workforce Investment Boards (WIBs). JTPA had required private representation on local advisory boards, but WIA required that the majority of board members, including board chairs, come from business. By changing the composition of the WIBs, Congress hoped that states and localities would be able to focus on the programs that were most needed, and thereby increase their impact.

In light of the above WIA innovations, it is time to re-evaluate the findings of the past. Evaluations of CETA and JTPA were often conflicting and typically found training to be only slightly effective. But what about now? Has training been successful under the present WIA regime? Has WIA, with its new customer focus and universal access, been a success? This dissertation will present evidence that the answers to these questions are a qualified, "yes." Some groups have benefited greatly, some modestly, but there were certainly gains to be had from training. Before I discuss my results, however, I must describe WIA in more depth, as well as, the innovative data used to assess WIA training programs.

rulemaking process for WIOA is still underway, and the full scope of changes from WIA are unclear. For the most current developments on WIOA see http://www.doleta.gov/wioa/.

1.3.2 The Local Office System for Employment Services

The Purpose and Organizational Structure of the Local Office System

WIA governs the provision and administration of most employment programs in the US. Funds are apportioned to the states by the federal government in the form of block grants. The State's internal Department of Labor (or similarly functioning entity) then apportions WIA funds to the State's various WIBs.[19] WIBs are given much latitude in how WIA funds are allocated, and are then responsible for setting workforce development priorities and allocating block grant funds.[20]

In addition to forming workforce investment boards, WIA requires states to maintain and staff at least one physical location where citizens may go in order to access employment services. These offices are typically referred to as "One-Stop Career Centers" or simply "One-Stop Centers". In South Dakota these locations are known simply as "Local Offices" and I adopt this naming convention going forward.[21]

South Dakota, like most states, has a network of Local Offices throughout the state, which I refer to broadly as the South Dakota Local Office System (SDLOS). The State of South Dakota staffs eighteen regional SDLOS locations throughout the state.[22] Local office activities are coordinated and managed from the state capital in Pierre. SDLOS is responsible for implementing the development plans of the WIB and for providing additional services as mandated by the Governor or Legislature. The

[19]For a comprehensive explanation of WIA programs, funding mechanisms, and rules, especially as they relate to Title I which authorizes training services see Bradley (2013).

[20]WIA regulations set some limits on how funds can be allocated. A potion of a State's block grant is controlled directly by the Governor. Additionally, There are broad guidelines dictating that a minimum percentage of the block grant must be directed towards training.

[21]In South Dakota the decision was made to change the naming of the local employment services offices from One-Stop Centers to Local Offices in order to avoid confusion. In South Dakota a large number of convenience stores and gas stations use the phrase "One-Stop" in their branding. Apparently a sufficient number of persons expressed confusion regarding the similar naming of these very different entities that the State decided to rename its One-Stop Career Centers to simply Local Offices.

[22]See Appendix A for full list of these offices and their locations.

services offered in furtherance of these goals are broadly categorized as either: Core, Intensive, or Training. While there may be some policy heterogeneity across states, the contours of these categories are stipulated by WIA legislation and so the following discussion can be considered generally applicable.

Core Services.

Core services are the most basic services offered and are intended simply to provide information regarding local labor-market conditions and employment opportunities. Core services are somewhat unique in that the SDLOS simultaneously serves both job seekers and job creators. In its service to job seekers, the SDLOS provides information about employment opportunities in order to facilitate job search. Regional offices maintain databases of job openings in the area. Both regional and statewide listings of job openings are accessible at physical locations and via the Internet. Local offices also provide computer access so that job seekers might search job postings or use various software programs in order to prepare resumes or fill-out applications.

As part of its service to industry, SDLOS also collects data on economic conditions and labor force characteristics. While such information is typically made available to the public at large, the intent is to provide business interests and policy makers with information so that they can effectively plan for the future. Most core services may be accessed via the Internet so that neither individuals nor businesses need ever physically visit a local office.

Intensive Services.

The next level of services provided by SDLOS staff is called Intensive Services. WIA emphasizes self-help first, and staff do not immediately extend intensive or training services; rather, SDLOS staff encourage job seekers to take advantage of core services first. Eligibility for intensive and training services generally demands that persons be at least eighteen years old.[23] Once staff authorize intensive services, job seekers will have access to additional services such as: interview coaching, skills assessments, career counseling, and career planning.

Training Services.

Training is the highest level of service authorized under the WIA, and SDLOS staff only extend access and funding for training programs after several one-on-one meetings with individuals. In each case SDLOS staff work with the job seeker to develop a career plan. Once training is authorized, SDLOS personnel and the job seeker choose the type of training to best suit the job seeker's career plan.

WIA provides for two types of training. The first is Occupational Skills Training (OST). OST allows workers to develop general human capital directed towards specific occupations. Such skills are acquired by attending training seminars, certification programs, or enrolling in either a community college or technical school. WIBs certify various providers and enrollees choose the provider that best suits them. OST pays for a portion of tuition costs for the certification or degree program pursued by the trainee. WIA guidelines require that the program be completable within four semesters. The OST program focuses on developing specific skill that will facilitate employment. As a general rule, OST will not fund a bachelor's degree.

The second type of training available to workers is On-the-Job Training (OJT).

[23]Intensive and training services are generally only extended to WIA Adult and WIA Dislocated workers in accordance with WIA rules. WIA Adult workers include job seekers at least eighteen years old who qualify for, or have exhausted their, unemployment insurance benefits. WIA Dislocated workers must meet the same criteria as WIA Adult workers, but, additionally, their job loss must stem from a business closure or layoff due to economic conditions. WIA also identifies WIA Youth workers as job seekers who are under the age of eighteen at the time of registration. Youths are generally not eligible for intensive or training services.

Workers who receive OJT are placed with a firm for a trial period stipulated in an employment contract between the firm and the State. Employment contracts may authorize up $1,040$ hours of OJT employment, but the typical contract is for 480 hours. Upon completion of the contract, the State reimburses the firm for up to 50% of the employee's wages paid out during the contracted period. The firm then has the option to keep the worker as a normal employee should both the business and the trainee desire. Workers develops both occupation and firm specific human capital that will serve them in the future. At the same time, the firm gets a chance to evaluate the employee and determine if she would be a good match.

Chapter 2

South Dakota Administrative Data

Given the long history of worker training in the US, and evaluations thereof, why do we need a new evaluation now? How does this analysis go beyond what has come before? What contributions can it offer?

To address the question of, "Why now?", it is important to recognize that WIA has introduced many new policy innovations, which might support new findings. Training programs today are much more responsive to the demands of local labor-markets. In addition, training is now much more client focused. Trainees have more control over the type of training they receive, and even the occupations for which they train. Given these changes a reexamination of training efficacy is in order.

Focusing on the second, and, perhaps, more important question. This dissertation surpasses prior studies by bringing forth the most comprehensive and long running data set ever used to evaluate publicly funded worker training programs. The data used in this study were collected directly by the State of South Dakota for the purpose of internal training program evaluation. The data follow individuals as they enter SDLOS, through their training, and for up to a year after their exit. The data are highly accurate, comprehensive, and specifically tailored to the task of training evaluation.

Second, most previous CETA and JTPA studies were limited in their ability to estimate training impacts. Many early studies were only able to estimate differential impacts by gender, and only later evaluations estimated differential impacts for whites and blacks. This dissertation is the first analysis to estimate treatment effects for: (1) dislocated workers versus the non-dislocated jobless, (2) Native Americans, (3) workers located in different geographic regions, and (4) workers displaced at different points in the business cycle.

Lastly, perhaps the single greatest differentiator between this study and previous works is that the South Dakota administrative data contain ten years of cross-sections. Prior evaluations of federal worker training programs were limited to at most two years of training cross-sections. With such an extensive history it is much more plausible that the effects of training are not random, or due to short-term effects that arise as a function of the study itself.

These unique data therefore deserve a careful treatment. The remainder of this chapter is devoted to exploring the administrative data used in this study. I begin with brief discussions of how the data are collected, and how the timing of events are recorded. Thereafter I explore the data in detail.

2.1 Collection of the Data

The data used in this paper were collected in accordance with the 1998 Workforce Investment Act by the SDLOS on behalf of SDDLR. The data report on *all* persons who registered with and exited from the SDLOS between January 2002 and December 2011. In order for an individual to access various employment services he or she was required to register with SDLOS, either at a physical location or via the Internet.

SDLOS then created a profile that followed the registrant until he or she was removed from monitoring. The removal process is detailed in the following section.

During the registration process, registrants supplied information regarding their employment status, place of residence, age, race, ethnicity, educational attainment, criminal background, and any welfare benefits received. Staff at local offices collected and aggregated registration information into a centralized database containing all current and past registration profiles. Staff also verified and augmented the registration profiles with records matched from other state agencies, e.g. earnings records were matched from state payroll tax records

Particular care is taken to record information on worker training programs. SDLOS staff work with job seekers to assess their skills, and to determine what occupation they would like to work in after training. Staff record the Standard Occupational Classification (SOC) code for this "intended" occupation so that they may later assess if trainees find employment in a related occupation.[1] For example, suppose someone in OST enrolls in a technical school for the purposes of becoming a radiation technician. Upon completing the program and exiting the system, a SDLOS employee sees that the worker found employment as a registered nurse. In this case, even though the worker did not find a job as a radiation technician, he or she would be considered to have found employment in a related occupation.

2.2 How Dates are Measured in the Data

Before discussing the sample data in detail, it is important to discuss briefly the outcome measures that are so central to this study. The SDLOS data report both

[1] In the case of OJT the intended occupation code matches that of the occupation the worker is placed in. For OST trainees, SDLOS personnel record the SOC code identifying the desired occupation towards which training was aimed.

Table 2.1: Registration dates and pre-training outcomes

Registration date	1st Prior quarter	2nd Prior quarter
Jan 5, 2004	Oct 1 – Dec 31, 2004	July 1 – Sept 30, 2004
Feb 15, 2004	Oct 1 – Dec 31, 2004	July 1 – Sept 30, 2004
July 1, 2005	Apr 1 – June 31, 2005	Jan 1 – Mar 31, 2006
Oct 27, 2006	July 1 – Sept 30, 2006	Apr 1 – June 31, 2006
Dec 31, 2006	July 1 – Sept 30, 2006	Apr 1 – June 31, 2006

employment status and quarterly earnings at multiple points in time. Pursuant to WIA rules, SDLOS records the employment status and earnings of all registrants during: (1) the first and second quarters prior to registration, and (2) during the first and third quarters after breaking contact with SDLOS.

Employment status is measured at the beginning of the quarter while earnings are reported for the entire quarter. Both variables are collected from from payroll tax records. Earnings data therefore reflect an accurate accounting of all labor earnings during the relevant calendar quarter.[2]

When a job seeker registers with the SDLOS a profile is created. The registration data, also known as the entry date, is the first key timing event. As mentioned above, the SDLOS data report on employment and earnings both one and two *full* calendar quarters prior to registration. The quarter of registration can be denoted, q_r. Thus, prior employment and earnings report on quarters q_{r-1} and q_{r-2}. Table 2.1 illustrates this relationship.

Post-exit outcomes are recorded for the first and third *full* calendar quarters after exit. The exit date and quarter are determined by SDLOS staff. SDLOS staff remove individuals from the system, i.e. an account is closed, once ninety days pass without

[2]Payroll tax records only report on taxable labor earnings. Workers paid on a cash basis rarely pay payroll taxes, and, their earnings, therefore, do not appear in either payroll tax records or the SDLOS data.

Table 2.2: Exit dates and post-training outcomes

Exit date	1st Post quarter	3rd Post quarter
Jan 1, 2008	Apr 1 – June 31, 2008	Oct 1 – Dec 31, 2009
Mar 20, 2008	Apr 1 – June 31, 2008	Oct 1 – Dec 31, 2009
July 20, 2009	Oct 1 – Dec 31, 2009	Apr 1 – June 31, 2010
Oct 12, 2010	Jan 1 – Mar 31, 2011	July 1 – Sept 30, 2011
Dec 31, 2010	Jan 1 – Mar 31, 2011	July 1 – Sept 30, 2011

contact between registrants and the SDLOS.[3,4]

Once staff close an account, the exit date is backdated to the date of last service. The backdated exit date then determines the periods for which employment and earnings are measured. As with prior outcomes, post-exit outcomes are recorded relative to the exit date. Denoting the exit quarter, q_e, post-exit employment and earnings reflect outcomes in q_{e+1} and q_{e+3}. Table 2.2 illustrates this relationship.

2.3 Exploring the Administrative Sample

The individual data profiles introduced above are the source of the sample data used in this study. As discussed above, the 1998 WIA introduced several innovations that might make earlier conclusions no longer applicable. The data used here are the first of their kind to analyze employment outcomes of individuals trained in accordance with updated WIA guidelines.

Included in the estimation sample are unemployed persons who registered with SDLOS between the years of 2002 and 2011, and were 20–65 at registration. These persons lived in South Dakota or a contiguous border county in neighboring Nebraska

[3]SDLOS staff ensure that trainees are not removed from the system while training is ongoing.

[4]Examples of contact include a job seeker: visiting a physical location, using online services, or speaking with SDLOS personnel via phone or email.

or Iowa.[5,6]

Excluded from the sample are persons with more than a Bachelor's degree. Such highly educated persons are not part of the target population for WIA training programs. The universal access provisions in WIA legislation do not prohibit such persons from enrolling in training programs, but state WIB funding priorities ensure that persons with lower levels of education are the primary targets for training.[7]

2.3.1 Characteristics of Job Seekers

One of the strengths of the data employed here is the breadth of detailed information provided on individuals. Contained in the sample are records on $6,322$ individual episodes of joblessness occurring between 2002 and 2011.[8] The data provide detailed information on personal characteristics, family structure, educational attainment, receipt of welfare benefits, and episodes of job training. I begin by discussing the unique characteristics and demographics of the SDLOS sample, all of which are summarized by Table 2.3 (below).

The average SDLOS client was slightly older than 38 at registration, and spent almost exactly one year in the system. In other words, the typical registrant was near the middle of a typical work-life and appears to be attempting a mid-life course correction. Can training programs effectively influence the employment prospects of trainees at such an advanced stage in their work lives?

[5] Appendix B contains a full list of included South Dakota, Iowa, and Nebraska counties.

[6] The states of Nebraska and Iowa share earnings information with the SDDLR when residents of those states register with the SDLOS. Persons from Nebraska and Iowa are eligible for intensive and training services if they work in South Dakota.

[7] After applying the age and geographic exclusion restrictions, only seventy-three persons with a Master's degree and no persons with a Ph.D. appear in the data.

[8] Once an account is closed, a new account must be created in order for a job seeker to again access SDLOS services. Both accounts are unique, and, as a result, it is possible for individuals to appear within the data at multiple points in time.

Table 2.3: Descriptive Statistics: Individual Characteristics [1]

	Mean	Std. Dev.	Count
Individual Characteristics			
Age at Entry	38.698	(11.178)	
Age at Exit	39.699	(11.171)	
Native American	0.098	(0.297)	618
Neither White nor Native American	0.055	(0.228)	348
White	0.847	(0.360)	5356
Male	0.454	(0.498)	2870
Single Parent	0.233	(0.423)	1475
Offender	0.144	(0.351)	909
Veteran	0.072	(0.258)	455
Educational Attainment			
Less than High School	0.111	(0.314)	703
High School Grad	0.566	(0.496)	3579
GED or Equivalent	0.142	(0.349)	897
Associate or License	0.110	(0.313)	698
Bachelor Degree	0.070	(0.254)	440
Literacy Deficiency	0.356	(0.479)	2252
Welfare Related			
Low Income	0.581	(0.493)	3676
Tempory Assistance for Needy Families	0.039	(0.193)	246
Trade Adjustment Assistance	0.077	(0.266)	485
Supplemental Nutritional Assistance	0.205	(0.404)	1294
Observations	6322		

[1] With the exception of age, all variables are categorical.

In many respects, the SDLOS sample is representative of South Dakota's population as a whole. According to 2008–2012 American Community Survey estimates, the overall South Dakota population was 85.9% white, 8.8% Native American, and the remaining 5.3% were from various racial and ethnic groups, predominantly Hispanic.[9] The SDLOS sample was similarly 84.7% white, 9.8% Native American. The remaining 5.5% of the sample belonged to several smaller racial and ethnic groups, primarily black and Hispanic. None of these smaller ethnic or racial groups comprised more than 3% of the sample.

[9]Data on the racial and ethnic makeup of South Dakota come from American Community Survey (ACS) estimates. The data are found by following the "Demographic and Housing Estimates" link in the ACS section at http://quickfacts.census.gov/qfd/states/46000lk.html.

The descriptive statistics in Table 2.3 also provide insight into individuals' gender, veteran status, household composition, and criminal history. For example, 45.4% of the sample were male, as compared to 50.8% in the South Dakota population at large. Table 2.3 also reports that 23.3% of sample persons were single parents,[10] and 14.4% of sample persons reported having criminal records.[11]

Persons with criminal backgrounds are afforded some special considerations under WIA, especially with respect to training. OJT employment contracts typically provide for 50% wage reimbursement, and only pay out if the employee remains employed for the duration of the contract. For OJT workers with a criminal record, however, the State reimburses 100% of wages paid regardless of whether employment lasts for the duration of the employment contract.

The educational attainment statistics show that workers who use SDLOS employment services typically lack extensive formal education. The majority of the sample, nearly 57%, have a high school diploma as their highest level of educational attainment. In fact, 82% of the sample had not earned any form of post-secondary degree. Only 7% of sample persons had a bachelor's degree, and only 11% had earned an Associate's degree or some form of occupational license.

The data also report that 35% of the sample was identified as having a literacy deficiency.[12] Literacy testing was typically limited to users of intensive and training

[10] Pursuant to SDDLR WIA guidelines, the SDLOS registration questionnaire does not ask after an individual's marital status, or number of children. It does ask if the registrant is a single parent.

[11] Criminal history is self reported and generally not verified by staff unless on-the-job training has been authorized. Registrants are instructed to answer in the affirmative if

> [they] have been subject to any stage of the criminal justice process or require additional assistance in overcoming barriers to employment resulting from a record of arrest or conviction for committing delinquent acts, such as crimes against a person, property, status offenses or other crimes.

[12] Literacy deficiency is established based on an individual's test results from Tests of Adult Basic Education (TABE). These are national standardized states that measure basic ability in reading, language, math and spelling.

services, and even then testing was not mandatory. As a result, the literacy deficiency statistic reported above in Table 2.3 almost certainly under-reports the true sample figure.[13]

The welfare section of Table 2.3 indicates that SDLOS serves an economically disadvantaged population. Local office personnel designated almost 60% of the sample as low income according to either the Lower Living Standard Income Level (LLSIL) or the federal poverty line.[14] Additionally, slightly more than 20% of the sample received Supplemental Nutritional Assistance (food stamps). The data also report that nearly 4% of the sample received Temporary Assistance for Needy Families (TANF) benefits.[15] Another 8% received Trade Adjustment Assistance (TAA) payments.

2.3.2 WIA Training Programs

Having characterized the individuals in the SDLOS sample, I now turn to a discussion of the WIA training programs. Table 2.4, on the following page, provides a detailed breakdown of training episodes during the sample period. The table aggregate OST/OJT training episodes into eleven different major occupational categories based on three digit SOC codes.

[13]Literacy Deficiency was not used as an explanatory variable in econometric modeling. Its purpose is purely illustrative.

[14]Both states and the federal government use the poverty line and the Lower Living Standard Income Level (LLSIL) to identify persons and household with low income so that they may be targeted for various welfare programs. Pursuant to federal guidelines, local office employees designate individuals as Low Income if their income over the six month period prior to registration was below either the federal poverty line or seventy percent of the LLSIL, whichever is higher. The LLSIL is adjusted yearly to account for regional and metropolitan income variations. For more information on the LLSIL see http://www.doleta.gov/llsil/2014/.

[15]TANF is a welfare program that provides temporary supplemental income to qualifying individuals. The federal government provides block grants to the state which they administer. Qualifying individuals must have children under the age of nineteen in the home, and TANF benefits are tied to the number of qualifying children. Recipients are generally required to find and maintain employment while receiving benefits. For specific information on TANF in South Dakota see http://dss.sd.gov/tanf/. For information on federal legislation regarding TANF see http://www.acf.hhs.gov/programs/ofa/programs/tanf.

Table 2.4: Descriptive Statistics: Major Occupation Groups

	OST		OJT	
	Mean	Count	Mean	Count
Management	0.072	162	0.029	10
Business and Financial Operations	0.027	61	0.009	3
Sciences, Computer, and Mathematical[1]	0.051	116	0.015	5
Architecture and Engineering	0.027	62	0.032	11
Community and Social Service	0.006	14	0.003	1
Legal	0.004	10	0.006	2
Education, Training, and Library	0.015	34	0.003	1
Arts, Design, Entertainment, Sports, and Media	0.006	14	0.006	2
Healthcare Practitioners and Technical	0.106	240	0.003	1
Healthcare Support	0.119	270	0.041	14
Protective Service	0.008	17	0.006	2
Service: Food or Personal Care[2]	0.004	8	0.015	5
Building and Grounds Cleaning and Maintenance	0.003	6	0.015	5
Sales and Related	0.008	19	0.047	16
Office and Administrative Support	0.239	540	0.166	57
Construction and Extraction	0.032	72	0.113	39
Installation, Maintenance, and Repair	0.024	55	0.116	40
Production	0.092	209	0.326	112
Transportation and Material Moving	0.156	354	0.052	18
Total for Training Type		2263		344
Training Related Employment after one Quarter	0.495	1121	0.782	269
Received Training		2607		2607
Did not Receive Training		3715		3715
Observations		6322		6322

[1] Combines categories: Computer and Mathematical with Life, Physical, and Social Sciences
[2] Combines categories: Food Preparation and Service with Personal Care Services

Of the $6,322$ persons in the sample, roughly 41% of the sample ($2,607$ persons) participated in some form of occupational training. There appears to have been a clear preference on the part of administrators and/or job seekers for OST over OJT: 86.8% of all training episodes were OST. Only 344 workers undertook on-the-job training, but $2,263$ persons participated in an occupational skills training program.

The primary reason for the OST and OJT enrollment disparity is that on-the-job training requires a double coincidence of wants. A jobless person must be matched with a firm that is willing to train him; at the same time, a company must be willing to enter into an employment contract with the State before the OJT placement can occur. Coordination with SDLOS staff and compliance with WIA rules is not costless for firms, and these non-wage costs might deter some firms from participating.

The disparity between the number of OST and OJT training occurrences was, however, not the only difference of note. I found that OST and OJT seemed to lend themselves to very different types of occupations. There was little overlap in the occupations most preferred by OST and OJT trainees. In fact, the Office and Administrative Support category was the only occupational group to see high enrollment by both OST and OJT workers. It was the most popular occupation category for OST training, 23.9%, and the second most popular for OJT training, 16.6%.

OST was primarily, but not always, directed towards occupations requiring formal education. Four major occupation groups accounted for 61% of all OST services: (1) Office and Administrative Support, (2) Transportation and Material Moving,[16] (3) Healthcare Support, and (4) Healthcare Practitioners and Technical.

Most OJT training, in contrast to OST training, was directed towards occupations requiring lower levels of formal education. The top four OJT occupations accounted for 72% of all OJT training: (1) Production, (2) Office and Administrative Support, (3) Installation Maintenance and Repair, and (4) Construction and Extraction.

2.3.3 Regional Considerations in the Data

The South Dakota administrative data also provide detailed geographic information. The data report the registrant's county of residence and the regional office that manages training operations for that county. The detailed geographic information

[16]In the SDLOS sample, training in Transportation and Material Moving is primarily directed towards acquiring a Commercial Driver's License (CDL). Several approved OST providers in the state offer a five week CDL training program. It is one of the states more popular OST programs.

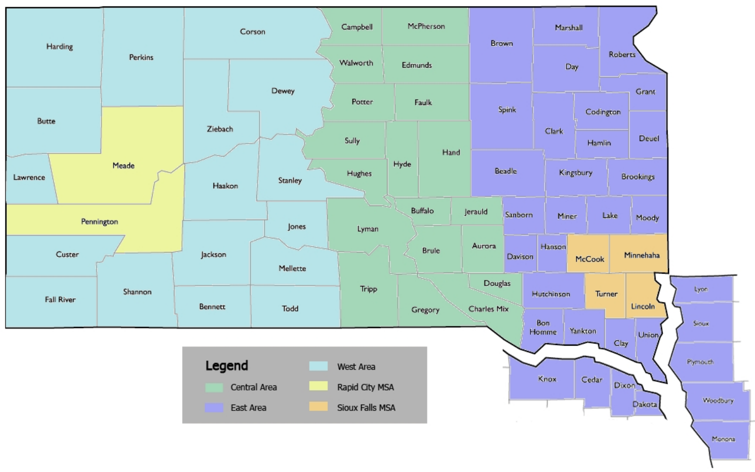

Figure 2.1: Geographic regions: South Dakota, Iowa, and Nebraska counties.

allows me to include regional fixed effects in the econometric models discussed in Chapter 3. Regional fixed effects control for important unobservable regional variation that might impact labor market outcomes.

South Dakota is the 17th largest state in the United States in terms of land area, but ranks 46th in both absolute population and population density. The state is predominantly rural and is home to only three designated Metropolitan Statistical Areas (MSAs): the Sioux City and Sioux Falls MSAs in the southeast, and the Rapid City MSA in the west. The state is roughly three hundred eighty miles across from east to west, and nearly three hundred and twenty five miles of Great Plains prairie separate Sioux Falls and Rapid City. Following Occupational Employment Statistics (OES) regional divisions, I additionally divided the state into three geographic regions: Eastern, Central, and Western.[17] Figure 2.1 presents a visual representation of these regional divisions.

I begin with the Eastern region and move westward. Agriculture is a leading industry in the Eastern region, but economic activity in this region is quite diverse. South

[17]Appendix B provides a list of counties included in this study and which geographic regions they are located in.

Dakota's two main public universities are located in the Eastern region. Daktronics, the world's leading manufacturer of electronic scoreboards and digital displays, has it corporate headquarters and primary manufacturing plant in Brookings, which is also home to South Dakota State University. Sioux Falls is a rapidly growing metropolitan area which has attracted many large and prospering employers in both the health care and finance industries. The two largest employers in the Sioux Falls MSA are Sanford Medical Center and Citibank South Dakota. As one moves west the population and economic activity falls off rapidly.

The state capital, Pierre, is the largest population center in the Central region, with a population of $13,646$ (2010 Census). Agriculture is perhaps the largest industry in the Central region, but it is followed closely by tourism. Hunting and fishing drive tourism in the Central region, which is generally considered to have some of the best pheasant hunting in the world.

The Western region of South Dakota is also very sparsely populated. Tourism is the most important economic driver in the Western region, which is home to several prominent national parks and monuments, such as the Black Hills National Forest and Mount Rushmore. The small and seemingly unassuming town of Sturgis is also in this region. Sturgis lies only twenty-eight miles north of Rapid City and is home to fewer than $7,000$ persons. Yet every August, between $400,000$–$600,000$ people come to Sturgis for the week-long, world-famous Sturgis Motorcycle Rally.

The vast majority of the South Dakota population lives in the Eastern region, and this holds true in the SDLOS sample as well. Table 2.5 shows that nearly 70% of sample persons resided in either the Eastern region or the Sioux Falls MSA. Fourteen percent of the sample resided in the Rapid City MSA. The remaining 16.6% live in one of the remaining South Dakota regions or else in one of the Nebraska or Iowa counties.

Table 2.5: Descriptive Statistics: Regional Characteristics

	Mean	Std. Dev.	Count
Unemployment Rate[1]	3.934	(1.148)	
Region of Residence			
East	0.519	(0.500)	3278
South Dakota	0.454	(0.498)	2872
Iowa	0.054	(0.227)	343
Nebraska	0.010	(0.099)	63
Central	0.049	(0.215)	307
West	0.048	(0.214)	303
Sioux Falls	0.245	(0.430)	1547
Rapid City	0.140	(0.347)	887
Year of Registration			
2002	0.045	(0.207)	283
2003	0.176	(0.380)	1110
2004	0.109	(0.312)	690
2005	0.068	(0.252)	432
2006	0.092	(0.289)	583
2007	0.103	(0.304)	652
2008	0.095	(0.293)	601
2009	0.210	(0.407)	1325
2010	0.059	(0.237)	376
2011	0.043	(0.202)	270
Observations	6322		

[1] Unemployment rate is specific to county and year of exit

The number of yearly registrations with SDLOS was relatively stable over the sample period. The fewest registrants occurred in 2002, while the greatest number occurred in 2009. The effects of the 2007–09 recession are evident in the high enrollment for 2009. The unusually large enrollment during 2003 was due to the closure of two facilities owned and operated by Gateway Inc.; a large manufacturing plant in North Sioux City, and a technical support branch in Sioux Falls.

South Dakota usually has one of the lowest unemployment rates in the nation.[18] The statewide unemployment rate during the sample period ranged from a low of 2.8% in 2007 to a high of 5% in 2010. In contrast, the national unemployment rate

[18] Annual statewide and regional unemployment rates are located in Appendix B.

was 4.5% in 2007 and 9.6% in 2010. This low average unemployment rate masks some regional variations, however. Unemployment was typically lowest in the Sioux Falls MSA, and highest in the Western region, although all regions experienced increased unemployment beginning in late 2008. Using this variation in unemployment I test whether WIA training programs were more effective in the wake of the Great Recession than they were before.

Chapter 3

Measuring the Effects of Program Participation: Econometric Methods.

Having introduced the SDLOS administrative data, I now move on to a discussion of estimation methods. It can be difficult to estimate the true effects of training programs; certain assumptions must be made and proper econometric techniques must be used. I address these issues in this chapter. I begin with brief review of program evaluation in Economics, focusing on relevant econometric issues. Afterward I discuss the identification assumptions necessary for treatment effect estimation. Finally, I discuss the econometric methods used in this evaluation.

3.1 Introductory Remarks Regarding Program Evaluation

Both economists and policy makers have long been interested in program evaluation. Will participation in a job training program increase employment rates and earnings? Will attending a financial literacy seminar improve participants' credit rating? Does union membership increase wages? The preceding are all examples of situations where policy makers and researchers have been interested in quantifying the impact of program participation on observed outcomes.

Program evaluation is often problematic though. For ethical and/or political reasons programs rarely operate under strict experimental conditions. In many scientific studies randomization of treatment is key component of a successful evaluation, but randomization is often not possible in social experiments. Random assignment guarantees treatment is uncorrelated with potential outcomes. In the absence of such randomization, selection bias can make it difficult to measure the impacts of treatment in an accurate manner.

Selection bias can be particularly problematic in evaluations of job training programs. For example, administrators might select individuals for treatment precisely because they believe the training will benefit the trainee. If only the most able are given training, how can the treatment effect be disentangled from the effect of unobserved ability?

Lalonde (1986) demonstrated the difficulties of using standard econometric techniques to evaluate post-program outcomes. In this important work, LaLonde studied employment outcomes of persons in the National Supported Work Demonstration (NSWD).[1] Study participants in NSWD were randomly assigned to either treatment or control groups. Random assignment insured unbiased impact estimates through the comparison of mean outcomes across the treatment and control cohorts. Lalonde found that econometric impact estimates did not coincide with the non-parametric difference in means estimators which compared the mean outcomes of treatment and control groups. This work has long served as a cautionary tale for researchers, and may

[1]NSWD was a training program for disadvantaged workers in the mid 1970s. NSWD was not part of CETA, but was a parallel project managed by the Manpower Demonstration Research Corporation operating in 10 cites around the US.

explain the many inconsistencies in prior evaluations of MDTA and CETA training programs.

In response to Lalonde's critique, economists and others have developed new parametric and semiparametric impact estimators that perform well even when confronted with non-experimental data.[2] Heckman et al. (1999) has provided a thorough survey of this literature, discussing newer techniques that remain valid even in the face of the Lalonde critique. Regardless, they are quick to point out that:

> [t]he best solution to the evaluation problem lies in improving the *quality of the data* [emphasis added] on which evaluations are conducted and not in the development of formal econometric methods to circumvent inadequate data. (Heckman et al., 1999, p. 1869)

The SDLOS administrative data used here are just the sort of quality data needed for such evaluations. Unlike many CETA and JTPA evaluations, which used inadequate controls, this study takes advantage of the numerous and detailed controls provided by the SDLOS data that surpass prior data in two important ways.

First, the SDLOS data contain a natural comparison group. As discussed in Chapter 1, many prior studies were forced to create comparison groups from national survey data such as the CPS. As a result, treatment and control data were gathered at different times and in different methods, ensuring that treatment and control groups would never be entirely comparable.

Secondly, the data derive from administrative records rather than from surveys. Administrative records ensure the accurate measurement and recording of key outcome and control variables. Crucially, the administrative data report all control variables *prior* to treatment. This prior measurement prevents any endogeneity between

[2] Rosenbaum and Rubin (1983); Imbens and Angrist (1994); Robins and Rotnitzky (1995); Angrist et al. (1996) and Cattaneo (2010) are examples of such works.

observed outcomes and included controls which can occur, for example, if education were measured after the training program.

3.2 Quantifying the Treatment Effect of Program Participation

3.2.1 Defining the Problem and the Treatment Effect

This study estimates the treatment effects of training at two points in time. First it looks at the impacts of training on employment and earnings in the first full calendar quarter following exit from the local office system, and secondly at impacts on employment and earnings in the third full calendar quarter following exit.

Following the notation of Heckman et al. (1999) and Cameron and Trivedi (2005), I define the treatment indicator for individual i as $D_i = j,$ for $j = 0, 1, 2$ and $i = 1 \ldots n$, where $j = 0$ indicates the control group which does not receive training, $j = 1$ indicates enrollment in OST, and $j = 2$ indicates enrollment in OJT.[3] Further, I define the outcome variable y_{ij}. For simplicity, I limit the current discussion to the binary employment status case where $y_{ij} = 1$ if individual i in treatment state j is employed, and $y_{ij} = 0$ otherwise.

There are two common measures of treatment effects common to the program evaluation literature. The first, called the Average Treatment Effect (ATE), is the expected impact of training for a random individual in the population. Equation (3.1) defines the ATE as the difference in means estimator which compares the average

[3]The primary distinction between the treated and untreated groups is the provision of WIA training services. This study does not differentiate between core and intensive services. As a result, all job seekers, regardless of treatment status, are eligible for various JSA services offered through SDLOS. See Chapters 1.3.2 and 1.3.2 for details regarding core and intensive services.

outcomes of the trained cohorts (y_1, y_2) with those of the control cohort (y_0):

$$E(\Delta_{j0}) = E(y_j - y_0) \quad j = 1, 2. \tag{3.1}$$

The second common impact measure is the Average Treatment Effect on the Treated (ATET), shown below in Equation (3.2):

$$E(\Delta_{j0}) = E(y_j - y_0 | D = j) \quad j = 1, 2. \tag{3.2}$$

The ATET, unlike the ATE, is conditional upon treatment, and therefore represents the expected change in employment or earnings for persons who received treatment. This study uses the ATE as its primary impact measure due to its greater policy relevance when treatment is universally available (Cameron and Trivedi, 2005), which is the case given the universal access provision in WIA.

Of course individuals cannot exist in multiple treatment states simultaneously. For example, subjects cannot be in both the control group and receive on-the-job training at the same time. Likewise, WIA prohibits persons from enrolling in OST and OJT at the same time. As a result, y_j and y_{-j} cannot be observed at the same time, and the ATE, $E(\Delta_{j0}) = E(y_j - y_0)$, is not explicitly identified within the data.

Analysis is still possible, however, even though the true ATE is unknowable. Proceeding *as if* individuals could exist in multiples treatment states, the Rubin (1974) potential-outcome model uses counterfactuals to provide an identification framework. Identification relies upon two essential assumptions that are discussed in greater detail below.

3.2.2 Identification Assumptions

Independence of outcomes and treatment

The primary barrier to impact evaluations in economics is the lack of strict experimental conditions. The hallmark of rigorous experimental treatment evaluation is random assignment of subjects to treatment and control. Random assignment results in the independence of treatment and outcomes, and ensures no correlation between individual outcomes and the treatment selection mechanism. When outcomes and treatment are independent, the researcher can be sure that observed differences in average outcomes are due to treatment.

Unfortunately most economic impact studies utilize observational rather than experimental data. Economic treatment evaluations must therefore account for potential selection bias. In the case of job training programs, selection bias is possible on both sides of the selection process. First, more employable job seekers might actively seek out training while the less employable might forgo training. Additionally, program administrators might assign the most gifted and motivated persons to training while excluding the less able from training. Either type of self-selection could bias econometric impact estimates.

Non-experimental studies therefore rely on the slightly weaker conditional independence assumption for identification. The conditional independence assumption states that outcomes and treatment are independent conditional upon a set of known and exogenous factors.[4] In the current context, the conditional independence assumption

[4] This weaker assumption is referred to by many names in the literature. The phrase *conditional independence* is seen most often in the literature but other phrases with similar meaning include: *ignorability* Rubin (1974), *unconfoundedness* or *weak unconfoundedness* (Imbens, 2000; Imbens and Wooldridge, 2009), or *exogeneity* (Heckman and Robb, 1985; Heckman et al., 1998). While the terminology varies across the literature the underlying assumption is the same. Each case assumes that the conditional mean of the outcome variable is independent of selection such that $E(y_j|\boldsymbol{X}, D) = E(y_j|\boldsymbol{X}) = \mu_j$.

is formally stated as

$$y_0, y_1, y_2 \perp\!\!\!\perp D \,|\boldsymbol{x}, \tag{3.3}$$

where $\boldsymbol{x}$ is a set of conditioning variables, and D is the treatment indicator. Appendix C.1 provides a test of this crucial assumption. I find evidence that the conditional independence assumption is satisfied, and that outcomes are conditionally independent of treatment within the SDLOS data.

Conditional independence requires a rich set of conditioning variables. Lechner and Wunsch (2009) used the phrase "selection on observables" to describe conditional independence. Selection on observables emphasizes that conditional independence is satisfied if the researcher can control for all of the relevant factors that influenced an individuals decision to enroll in training, e.g. age, gender, educational attainment, marital status, children, employment status etc.[5]

Using the weaker conditional independence assumption, Equation (3.1) becomes Equation (3.4) where the average treatment effect in now conditional upon $\boldsymbol{x}$.

$$E(\Delta_{j0}|\boldsymbol{x}) = E(y_j - y_0|\boldsymbol{x}) \quad j = 1, 2 \tag{3.4}$$

Taking the expectation of the right hand side of Equation (3.4) produces Equation (3.5). Equation (3.5) now defines the average treatment effect as the difference between the mean outcomes of either the OST or OJT cohort and the control cohort.

$$E(\Delta_{j0}|\boldsymbol{x}) = \widehat{\mu}_j(\boldsymbol{x}) - \widehat{\mu}_0(\boldsymbol{x}) \quad j = 1, 2 \tag{3.5}$$

Equation (3.5), known as the difference in means estimator, is used to estimate all

[5]Importantly, conditioning variables can be related to treatment but should not be a result of treatment. As a result, any conditioning covariates that might change with time such as: age, educational attainment, earnings, or geographic location should be measured prior to treatment

average treatment effects in this study.

Likelihood of treatment

The second important assumption for identification of treatment effects is known as the overlap assumption. The overlap assumption, shown in Equation (3.6), requires that no persons be either excluded from or guaranteed treatment:

$$0 < P(D = j|\boldsymbol{x}) < 1. \tag{3.6}$$

More specifically, the overlap assumption requires that all sample persons have a positive probability of assignment to each possible treatment state. The universal access provisions of WIA legislation should ensure that the overlap assumption holds within the data, but the statistical reality of treatment assignment might not conform to legal requirements. Appendix C.2 discusses tests of the overlap assumption and provides evidence that it generally holds in the SDLOS data.

3.3 Econometric Methods

Most treatment evaluations focus on cases of binary treatment. Binary treatment describes situations where treatment is either administered or withheld. An example from the field of medicine is a drug trial where some participants are given an experimental drug and others are not. Another example of binary treatment is when one group of students attends a financial literacy course while another group does not. Researchers might then wish to study how the savings decisions of the treatment group differ from those of the control group.[6]

[6] Heckman and Robb (1985); Heckman et al. (1999) and Imbens (2000) discuss numerous ways in which researchers might estimate the effectiveness of binary treatment even when the underlying

The current analysis, however, does not concern itself with a binary worker training program. Instead, I estimate the treatment effects of two different training programs. Cases of multivalued treatment occur when participants might find themselves in one of many possible treatment states. For example, in modern drug trials a participant might be administered: no treatment, a placebo, or the actual drug. The worker training programs studied here are similar because there are multiple treatment states: control, OST, and OJT. Non-experimental methods for identifying the treatment effects of multivalued treatments are less developed, but recent work, discussed below, has developed new and powerful techniques for estimating multivalued treatment effects.

3.3.1 Semi-parametric treatment effect estimators

This study estimates the treatment effects of occupational skills training and on-the-job training using semiparametric estimators based on the recent work of Cattaneo (2010) and Cattaneo et al. (2013), who developed a new Efficient Influence Function (EIF) impact estimator. The EIF estimators were designed for multivalued treatments. Additionally, the EIF method produces doubly-robust estimators which are key to non-experimental treatment evaluations.

Initially developed by Robins and Rotnitzky (1995) and Robins et al. (1995), "doubly-robust" estimators are examples of multi-stage estimators. Such estimators attempt to correct the selection biases which can plague non-experimental program evaluations. Doubly-robust treatment effect estimators typically require modeling the selection and outcome processes in order to estimate the treatment effect of interest. The primary benefit of these methods is their consistent estimation of treatment effects as long as either the selection or outcome models are correctly specified (Kang and

data are non-experimental. Imbens and Wooldridge (2009) provides a recent review of this literature.

Schafer, 2007). I use the Cattaneo (2010) EIF method to estimate the primary impact estimates of this study. The EIF method employs a multistage procedure to estimate the employment rates and mean quarterly earnings of each cohort. Following the literature, I refer to these mean outcomes as Potential Outcome Means (POMs). The average treatment effect of training is then the difference between the estimated mean outcomes of the training and control cohorts.

The EIF estimation procedure comprises three stages: (1) specification and estimation of a treatment assignment equation, (2) specification and estimation of an outcome equation, and (3) solving a series of propensity score weighted moment conditions to find the POMs for each training cohort. The multi-stage procedure is key to the double robustness of the Cattaneo EIF method. POM estimates from the third stage are consistent as long as either the treatment or outcome stages are correctly specified. I begin by first describing the estimation of cohort specific employment rates before discussing estimation of mean quarter earnings.

Estimating the Employment Treatment Effect

Stage 3: Estimating the Potential Outcome Means.

It is helpful to introduce briefly the final stage of the EIF method before explaining the first and second stages. The EIF method identifies the employment rate for each training cohort as the solution to a series of moment conditions.

There are three unique population moment conditions, one for each training state $j = 0, 1, 2$. The moment conditions are solved to find μ_j, which is the cohort specific employment rate, or mean employment probability. These population moments are depicted below by Equation (3.7)

$$E\left[\frac{D_i(j)(y_i-\mu_j)}{p_j(\boldsymbol{x_i})}-\frac{e_j(\boldsymbol{x_i};\mu_j)}{p_j(\boldsymbol{x_i})}[D_i(j)-p_j(\boldsymbol{x_i})]\right]=0, \qquad j=1,2,3 \tag{3.7}$$

where y_i is the observed outcome of individual i, and $D_i(j)$ is the treatment state indicator.

The propensity score weights, $p_j(\boldsymbol{x_i}) = P(D = j|\boldsymbol{x_i})$, are the probability of assignment to treatment state j. Following Imbens (2000), I refer to the treatment probabilities as Generalized Propensity Scores (GPSs). Propensity scores are estimated in Stage 1 and are discussed below.

Lastly, $e_j(\boldsymbol{x_i};\mu_j) = E(y-\mu_j \mid \boldsymbol{x}, D=j)$ is a bias correction term that captures deviations of the observed outcome from its expected mean value. The bias correction term is estimated in Stage 2 and is also discussed in more detail below.

For estimation, the population moments are replaced with their sample counterparts. The sample moments are solved to find the POM estimators, $\widehat{\mu}_j$ for $j=0,1,2$. The sample moments are shown in Equation (3.8) below.

$$\frac{1}{n}\sum_{i=1}^{n}\left[\frac{D_i(j)(y_i-\widehat{\mu}_j)}{\widehat{p}_j(\boldsymbol{x_i})}-\frac{\widehat{e}_j(\boldsymbol{x_i};\widehat{\mu}_j)}{\widehat{p}_j(\boldsymbol{x_i})}[D_i(j)-\widehat{p}_j(\boldsymbol{x_i})]\right]=0, \qquad j=1,2,3 \tag{3.8}$$

Stage 1: Estimating the Likelihood of Treatment.

Stage 1 of the Cattaneo et al. (2013) EIF procedure requires estimation of the general propensity score, $\widehat{p}_j(\boldsymbol{x_i})$. Taking a flexible parametric approach to GPS estimation, the probability of treatment is modeled as a function of observable characteristics, but the EIF method does not restrict the functional form of the underlying data generating process. It requires only that the chosen model provide the best fit to the observed data.

In order to determine the functional form which best fits the data, I identified a set of potential control variables, $\boldsymbol{X}$, containing all variables from the SDLOS data. For example, the likelihood of assignment to training could be a function of age, education, gender, criminal background, or even geographic region. I used a subset of the potential control variables, $\boldsymbol{x} \in \boldsymbol{X}$, to approximate the unknown population function $p_j(\boldsymbol{x_i}) = P(D = j|\boldsymbol{x_i})$.

Following Cattaneo et al. (2013), I estimated the GPSs, $\widehat{p}_j(\boldsymbol{x_i})$, using first order polynomials in $\boldsymbol{x}$, which allowed for quadratic continuous variables and interaction effects between all potential variables.[7] For example, one model would contain no interaction terms, and a second would be fully interacted. A third model would contain no quadratic terms, but a fourth model would contain only quadratic terms. A fifth model would contain only a constant term. All potential GPS specifications were estimated and ranked according to adjusted Akaike Information Criterion (AICc).[8] I chose the model that minimized the AICc to generate the propensity score weights.

There are three possible treatment states within the data corresponding to the control, OST, and OJT cohorts. This is demonstrated below in Equation (3.9):

$$D_i = \begin{cases} 0 & \text{if individual } i \text{ receives no training,} \\ 1 & \text{if individual } i \text{ enrolls in OST,} \\ 2 & \text{if individual } i \text{ enrolls in OJT.} \end{cases} \tag{3.9}$$

[7]See Appendix D for a more detailed description of the flexible estimation procedure.

[8]The AICc provide a method for evaluating model fit. A lower score indicates better fit. See Judge et al. (1985, p 870-871) for a discussion regarding model selection and information criterion. The AICc is calculated according to

$$AICc = -2\ln(\mathcal{L}) + \frac{2kn}{n-k-1}$$

where n is the number of sample observations, k is the number of model covariates, and $\ln(\mathcal{L})$ is the log-likelihood of the estimated model.

The probability that person i is in state j thus follows a multinomial distribution. The density function can be described as

$$f(s) = \prod_{j=1}^{3} p_j^{D_j} \tag{3.10}$$

where D_j is the indicator function equal to one if $D_j = j$ and zero otherwise.

I therefore estimated the generalized propensity scores using a multinomial logit where the dependent variable was the individual's treatment state, D_i. Each potential model specification was estimated and its AICc calculated by maximizing the likelihood function and obtaining parameter estimates according to

$$\widehat{\boldsymbol{\beta}}_j = \arg\max_{\beta} \sum_{i=1}^{n} \sum_{j=0}^{2} D_i(j) \ln \left[\frac{\exp(\boldsymbol{x}_i \boldsymbol{\beta}_j)}{\sum_{j=0}^{2} \exp(\boldsymbol{x}_i \boldsymbol{\beta}_j)} \right], \quad j = 0, 1, 2 \tag{3.11}$$

with the standard normalization of $\beta_0 = 0$.

The functional form of $\exp(\boldsymbol{x}_i \boldsymbol{\beta}_j)$ is flexible but a basic description is given below

$$\exp(\boldsymbol{x}_i \boldsymbol{\beta}_j) = \exp(\boldsymbol{x}_i \boldsymbol{\beta} + \boldsymbol{r}_i \boldsymbol{\alpha} + \boldsymbol{t}_i \boldsymbol{\delta} + \varepsilon_i) \tag{3.12}$$

where $\boldsymbol{x}_i$ is a vector of personal and work history descriptors including age, gender, race, educational attainment. The remaining vectors, $\boldsymbol{r}_i$ and $\boldsymbol{t}_i$, indicate controls for region of residence and year of registration.

As discussed above, each estimated model is ranked according to its AICc, and the model specification with the lowest AICc is used to estimate the treatment probabilities. The GPSs are calculated using the predicted values from the multinomial logistic regression as defined by Equation (3.13). The estimated treatment probabilities are used as inverse probability weights in the third stage sample moment conditions.[9]

$$\widehat{p}_j(\boldsymbol{x_i}) = P[D = j|\boldsymbol{x}_i] = \frac{\exp(\boldsymbol{x}_i\widehat{\boldsymbol{\beta}}_j)}{1 + \sum_{j=1}^{2}\exp(\boldsymbol{x}_i\widehat{\boldsymbol{\beta}}_j)}, \quad j = 0, 1, 2 \tag{3.13}$$

Stage 2: Estimating the Likelihood of Employment.

Having estimated the treatment selection model in Stage 1, it was necessary to estimate the outcome model in the second stage. Stage 2 of the Cattaneo EIF procedure is much like Stage 1. For simplicity I again limit the current discussion to estimating employment status. Earnings are discussed in the following section. In Stage 2 a flexible parametric approach was used to estimate a bias correction term $e_j(\boldsymbol{x_i}; \mu_j) = E(y - \mu_j \mid \boldsymbol{x}, D = j)$. The bias correction term captures deviations of the outcome from its expected mean, and ensures consistent estimation of the POM even if the treatment model in Stage 1 was misspecified.

The first step in creating the bias correction term was to construct a model for individual employment outcomes. Once more following Cattaneo et al. (2013), I estimated a series employment models in order to find a suitable candidate. The models were ranked according to their AICc and the model with the lowest AICc was selected to estimate individual employment status.

The employment status equation defines the binary employment status y_i such that

$$y_i = \begin{cases} 0 & \text{if individual } i \text{ is unemployed.} \\ 1 & \text{if individual } i \text{ is employed.} \end{cases} \tag{3.14}$$

Given the binary nature of the dependent variable, I used a logistic regression to

[9] See Robins et al. (1995) for a discussion of the PSW method and its use in controlling biases introduced via sampling methods. Kang and Schafer (2007) provides an excellent overview of the method and its specific use in the construction of doubly-robust treatment effect estimators.

model the underlying data generating process.

$$\widehat{y}_i(\boldsymbol{x}_i) = P[y = 1|\boldsymbol{z}_e(\boldsymbol{x}_i)] = \frac{\exp(\boldsymbol{z}_e(\boldsymbol{x}_i)\boldsymbol{\gamma})}{1 + \exp(\boldsymbol{z}_e(\boldsymbol{x}_i)\boldsymbol{\gamma})}, \tag{3.15}$$

Once again, I identified a set of potential control variables, $\boldsymbol{X}$, and approximated the unknown population function $\boldsymbol{z}_e(\boldsymbol{x}_i)\boldsymbol{\gamma}$ using all possible polynomials in $\boldsymbol{x}$ where $\boldsymbol{x} \in \boldsymbol{X}$. A general depiction of the outcome specification is given below in Equation (3.16). Potential regressors in the outcome stage included all possible regressors from the treatment stage, as well as, additional controls for county level unemployment rates.

$$\exp(\boldsymbol{z}_e(\boldsymbol{x}_i)\boldsymbol{\gamma}) = \exp(\boldsymbol{z}_i\boldsymbol{\beta} + \boldsymbol{r}_i\boldsymbol{\alpha} + \boldsymbol{t}_i\boldsymbol{\delta} + \varepsilon_i) \tag{3.16}$$

After identifying the vector $\boldsymbol{z}_e(\boldsymbol{x}_i)$ which best fits the observed employment outcomes, I solved the linear sieve

$$\widehat{\boldsymbol{\delta}}_j(\mu_j) = \arg\max_{\delta_j} \sum_{i=1, D_i=j}^{n} \left[y_i - \mu_j - \boldsymbol{z}_e(\boldsymbol{x})'\boldsymbol{\delta}_j(\mu_j)\right]^2. \tag{3.17}$$

to find the coefficient vector $\widehat{\boldsymbol{\delta}}_j(\mu_j)$ which, as $\boldsymbol{z}_e(\boldsymbol{x})'\widehat{\boldsymbol{\delta}}_j(\mu_j)$, provided the best possible estimate of the true employment status. The bias correction term was thus created as $\widehat{e}_j(\boldsymbol{x_i}; \mu_j) = \boldsymbol{z}_e(\boldsymbol{x})'\widehat{\boldsymbol{\delta}}_j(\mu_j)$

Having completed Stages 1 and 2, i.e. having identified the proper selection and outcome model specifications, it was possible to proceed to Stage 3 and estimate the potential outcome means.

Stage 3 Revisited: Estimating the Potential Outcome Means.

After completion of Stages 1 and 2, I solved Equation 3.8 using the Generalized

Method of Moments (GMM). In doing so I identified the POM estimators $\widehat{\mu}_j = E(y_i|\boldsymbol{x}_i, D = j)$ for $j = 0, 1, 2$ as the solutions to the series of sample moment conditions given below.

$$\phi_{EIF}(\boldsymbol{z}_i; \mu_j, p_j(\boldsymbol{x_i}), e_j(\boldsymbol{x}_i; \mu_j)) = 0 \qquad j = 0, 1, 2 \tag{3.18}$$

$$\phi_{EIF}(\cdot) = \frac{1}{n}\sum_{i=1}^{n}\left[\frac{D_i(j)(y_i - \widehat{\mu}_j)}{\widehat{p}_j(\boldsymbol{x_i})} - \frac{\widehat{e}_j(\boldsymbol{x_i}; \widehat{\mu}_j)}{\widehat{p}_j(\boldsymbol{x_i})}[D_i(j) - \widehat{p}_j(\boldsymbol{x_i})]\right] \tag{3.19}$$

The EIF procedure then yielded $\widehat{\mu}_j = E(y_i|\boldsymbol{x}_i, D = j)$; a consistent and efficient estimate of the conditional mean outcome of persons treatment state j. I used the POM estimates to calculate the average treatment effects of training using the differences in average cohort employment rates according to:

$$ATE_{OST} = E(y_1 - y_0|X, D = 1) = \widehat{\mu}_1 - \widehat{\mu}_0 \tag{3.20}$$

and

$$ATE_{OJT} = E(y_2 - y_0|X, D = 2) = \widehat{\mu}_2 - \widehat{\mu}_0. \tag{3.21}$$

Estimating the earnings treatment effect

The procedure described above requires only slight modification before being applied to the estimation of average quarterly earnings. The outcome of interest is now the post-training earnings of trainees and non-trainees. I therefore reestimated the bias correction term, $\widehat{e}_j(\boldsymbol{x}; \mu_j) = \boldsymbol{z}_e(\boldsymbol{x})'\widehat{\boldsymbol{\delta}}_j(\mu_j)$, from the second stage of the EIF procedure to reflect the change in dependent variable. Stage 1 remains unchanged.

Of course, earnings are observed conditionally upon employment. As a result the potential for additional selection bias is introduced. To control for this potential bias, I employed a two-stage procedure and included the inverse Mill's ratio in the set of potential estimation covariates. This technique is known by many names such as a Type II Tobit (Amemiya, 1985), but is most often referred to in the economics literature as the Heckman two-stage procedure following its development by Heckman (1979). This method is detailed below in Chapter 3.3.2.

Stage 3 of the EIF procedure now returns the estimated mean quarterly earnings rather than the mean employment probability. The average treatment effect of training are still defined according to Equations (3.20) and (3.21).

3.3.2 Parametric estimator

The EIF estimator is preferred due to its double-robustness, but its robustness relies on the inverse probability weights in the GMM moment conditions. Estimators using propensity score weighting become unstable, however, when the treatment probabilities approach zero. I such found unstable treatment probabilities in two instances. First, when estimating treatment effects for minorities. And, second, when estimating treatment effects in the Western and Central regions of South Dakota. In both of these cases small sample sizes result in low OJT enrollment probabilities.

In these instances of low treatment probability, I employ traditional techniques that do not rely upon propensity score weighting. These methods lack the double-robustness of the EIF estimator, but remain valid under the conditional independence and overlap assumptions. In essence, the effects of training were estimated using only Stage 2 of the EIF procedure. I estimated the impacts of training on employment rates using a logistic regression which included training dummy variables to measure the impact of training. Similarly, I used a two-stage Heckman style earnings model with training dummies to measure the impact of training on quarterly earnings. These methods are discussed in more detail below.

Estimating the employment treatment effect

In order to maintain comparability between the semiparametric and fully-parametric treatment effect estimates, I based my parametric estimation methods on those of the EIF procedure. I therefore used the same logistic regression model from Stage 2 of the EIF procedure to construct the parametric estimator of employment status and the ATE of training.

Using the methods explained above, I estimated a series of potential employment models described below in Equation (3.22). The models were ranked according to their AICc, and the model with the best fit was chosen to estimate the ATEs of training. The dependent variable in the employment equations, y_i, was again the binary employment indicator where $y_i = 1$ if employed and $y_i = 0$ otherwise. This model was used to estimate the effects of training for minorities and across geographic regions. I therefore estimate this model for each geographic region and for each racial group to allow for differential impact estimates across subgroups.

$$\Lambda(\boldsymbol{x}_i'\boldsymbol{\beta}) = P[y = 1|\boldsymbol{x}_i] = \frac{\exp(\boldsymbol{x}_i\boldsymbol{\beta})}{1 + \exp(\boldsymbol{x}_i\boldsymbol{\beta})}. \tag{3.22}$$

The unknown population function $\exp(\boldsymbol{x}_i\boldsymbol{\beta})$ was approximated using polynomials in $\boldsymbol{x}_i$. A general form of $\exp(\boldsymbol{x}_i\boldsymbol{\beta})$, shown below, included controls for training status, individual characteristics, geographic location, year of registration, and county level unemployment rates.

$$\exp(\boldsymbol{x}_i\boldsymbol{\beta}) = \exp(x_{i,ost}\beta_{ost} + x_{i,ojt}\beta_{ojt} + \boldsymbol{z}_i\boldsymbol{\gamma} + \boldsymbol{r}_i\boldsymbol{\alpha} + \boldsymbol{t}_i\boldsymbol{\delta} + \varepsilon_i) \tag{3.23}$$

Once the proper covariate vector was chosen, I maximized the logistic log likelihood function in Equation (3.24) to obtain the estimated coefficient vector $\widehat{\boldsymbol{\beta}}$.

$$\widehat{\boldsymbol{\beta}} = \arg\max_{\beta} \sum_{i=1}^{n} \ln \left[\frac{\exp(\boldsymbol{x}_i \boldsymbol{\beta})}{1 + \exp(\boldsymbol{x}_i \boldsymbol{\beta})} \right] \tag{3.24}$$

The marginal effects, $h(\boldsymbol{x}, \widehat{\boldsymbol{\beta}})$, were found via the coefficient transformation

$$h(\boldsymbol{x}, \widehat{\boldsymbol{\beta}}) = [\Lambda(\boldsymbol{x}\widehat{\boldsymbol{\beta}})][1 - \Lambda(\boldsymbol{x}\widehat{\boldsymbol{\beta}})]\widehat{\boldsymbol{\beta}}. \tag{3.25}$$

Standard errors and significance levels for the marginal effects were calculated using the delta method. See Wooldridge (2010, p. 576) for details.

I averaged the predicted employment probabilities, $\widehat{y}_i$, across each training cohort to determine the mean employment rates for each cohort. This is depicted below where n_j is the number of observations in the given training cohort.

$$\widehat{\mu}_j = \frac{1}{n_j} \sum_{1}^{n_j} \widehat{y}_{ji} \qquad \text{for } j = 0, 1, 2 \tag{3.26}$$

Estimating the earnings treatment effect

As is common with observational data on earnings, the data only report on the earnings of individuals who worked during the sample period. As a result, there is potential for a type of selection bias due to the presence of an unobserved latent variable describing the employment decision. Modeling individual earnings is commonly accomplished in these situations via the selection model first popularized by Heckman (1979).

The selection model begins by recognizing that the researcher only observes positive income for persons who actively supplied labor to the labor market.[10] In light of this

[10]This strain of literature is only concerned with studying labor income. Therefore simplifying assumptions are made to abstract away other forms of non-labor income and home production. It is

fact, the wage analysis is broken up into two unique problems, or stages. First, the researcher observes the latent variable y_1^* which describes the labor supply decision. The labor supply decision can be summarized as

$$y_1 = \begin{cases} 1 & \text{if } y_1^* > 0 \\ 0 & \text{if } y_1^* \leq 0. \end{cases} \tag{3.27}$$

Next, the researcher can observe, y_2^*, corresponding to individual wages or earnings. However, income is observed if an only if persons work and thus the latent variable $y_1^* > 0$. This situation is summarized by the following earnings equation

$$y_2 = \begin{cases} y_2^* & \text{if } y_1^* > 0 \\ 0 & \text{if } y_1^* \leq 0. \end{cases} \tag{3.28}$$

The sample selection model assumes the following linear relationships for the participation and earnings equations

$$y_1^* = \boldsymbol{X_1'\beta} + \varepsilon_1 \tag{3.29}$$

$$y_2^* = \boldsymbol{X_2'\beta} + \varepsilon_2. \tag{3.30}$$

The selection model additionally assumes: (1) $\varepsilon_1, \varepsilon_2 \perp\!\!\!\perp \boldsymbol{x}$, (2) $\varepsilon_1 \backsim \mathcal{N}(0, 1)$, and (3) $E(\varepsilon_2|\boldsymbol{x}, \varepsilon_1) = \gamma\varepsilon_1$.

It is critical to acknowledge and correct for the cross equation error correlation in order to estimate the earnings equation properly. I used a two-stage Heckit, or Type II Tobit, model to estimate the earnings equations which allows for non-normal and conditionally heteroskedastic errors in the earnings equation. I therefore first estimated the participation equation using a Probit regression. Using the results of

assumed that labor supply decisions are made purely on the basis of the reservation wage, and that non-positive labor supply is an indicator that prevailing wages fall short of an individual's reservation wage.

participation regression, I calculated the inverse Mill's ratio (IMS). The IMS was then used as a regressor in the augmented earnings regression to correct for selection bias.

Assuming the joint normal distribution of the error terms, $\varepsilon_1, \varepsilon_2$, the earnings equation can be rewritten as

$$\begin{aligned} E(y_2^*|\boldsymbol{X}) &= \boldsymbol{X}_2'\boldsymbol{\beta}_2 \\ E(y_2|\boldsymbol{X}, y_1^* > 0) &= \boldsymbol{X}_2'\boldsymbol{\beta}_2 + E(\varepsilon_2|\varepsilon_1 > -\boldsymbol{X}_1'\boldsymbol{\beta}_1) \end{aligned} \tag{3.31}$$

where $E(\varepsilon_2|\varepsilon_1 > -\boldsymbol{X}_1'\boldsymbol{\beta}_1)$ simplifies to $\varepsilon_2 = \gamma\varepsilon_1 + \psi$. The random component ψ is mean zero and independent of ε_1 given assumption (3) above.

Equation (3.31) therefore simplifies to

$$\begin{aligned} E(y_2|\boldsymbol{X}, y_1^* > 0) &= \boldsymbol{X}_2'\boldsymbol{\beta}_2 + E((\gamma\varepsilon_1 + \psi)|\varepsilon_1 > -\boldsymbol{X}_1'\boldsymbol{\beta}_1) \\ &= \boldsymbol{X}_2'\boldsymbol{\beta}_2 + \gamma E(\varepsilon_1|\varepsilon_1 > -\boldsymbol{X}_1'\boldsymbol{\beta}_1) \\ &= \boldsymbol{X}_2'\boldsymbol{\beta}_2 + \gamma\lambda(\boldsymbol{X}_1'\boldsymbol{\beta}_1). \end{aligned}$$

The term $\lambda(\boldsymbol{X_1}\boldsymbol{\beta}_1) = \phi(\boldsymbol{X}_1'\boldsymbol{\beta}_1)/\Phi(\boldsymbol{X}_1'\boldsymbol{\beta}_1)$ is the inverse Mill's ratio, where $\phi(\cdot)$ is the PDF of the standard normal distribution and $\Phi(\cdot)$ is the CDF of the standard normal distribution. The augmented two-stage earnings regression is now characterized as

$$y_2 = \boldsymbol{X}_2'\boldsymbol{\beta}_2 + \gamma\widehat{\lambda}(\boldsymbol{X}_1'\widehat{\boldsymbol{\beta}}_1) + u \tag{3.32}$$

and was estimated using the positive earnings values. As in all previous cases, multiple models were estimated and the model which minimized the AICc was chosen for final estimation.

The final estimation model included indicators for training, variables describing individual characteristics, and controls for time, region of residence, and county level unemployment rates. The estimation equation is summarized below.

$$y_{i2} = x_{i,ost}\beta_{ost} + x_{i,ojt}\beta_{ojt} + \boldsymbol{z}_i\boldsymbol{\gamma} + \boldsymbol{r}_i\boldsymbol{\alpha} + \boldsymbol{t}_i\boldsymbol{\delta} + \gamma\widehat{\lambda}(\boldsymbol{x}_{i1}\widehat{\boldsymbol{\beta}}_1) + u \tag{3.33}$$

I identified the average treatment effects in this model by the coefficient estimates, β_{ost} and β_{ojt}. Standard errors were calculated following Heckman (1979).

The predicted earnings, $\widehat{y}_{2j}$, were used to calculate the mean earnings for each training cohort according to

$$\widehat{\mu}_j = \frac{1}{n_j}\sum_{1}^{n_j}\widehat{y}_{2j} \qquad \text{for } j = 0, 1, 2 \tag{3.34}$$

where n_j is the number of observations with positive earnings in the given training cohort.

3.4 Concluding Remarks

In the following chapters I discuss my results surrounding the impacts of training. The discussion proceeds in two primary directions. In Chapter 4 I evaluate the affects of training on mean employment rates. Then in Chapter 5 I shift the analysis to explore the impacts of training on average quarterly earnings.

As the discussion moves through each topic, I present the estimated potential outcome means and associated average treatment effects in a series of tables. In the title of each table I indicate the econometric method used to calculate the displayed results. I indicate the semiparametric EIF method by including (SP) in the table title. Similarly, I indicate the parametric methods by including (FP), for fully-parametric, in the table title.

Chapter 4

Assessing Program Effectiveness: Employment Effects of Occupational Skills and On-the-job Training

4.1 Introduction

Having explained my estimation methods, I turn to the empirical investigation. This chapter explores the effects of WIA training on employment rates. With these results, I show how training affected the employment prospects of job seekers in both the immediate and longer terms.[1] The current chapter explores the effects of training for multiple groups of unemployed workers, at different points in the business cycle, and in different geographic locations.

I begin by establishing "baseline" results for the 2002–11 period. My baseline results follow previous training evaluations and report estimated treatment effects for:

[1]Following the discussion in Chapter 2.2, I refer to the first full calendar quarter after exit from SDLOS as the short run. I refer to the third full calendar quarter after exit as the longer run.

(1) the entire sample, (2) males, and (3) females. These baselines, particularly the sample-wide estimators, are the broadest measures of program effectiveness.

After establishing baseline results, I contrast the effects of training for job seekers with different job-loss situations. WIA recognizes two types of job seekers, differentiated only by the circumstances surrounding their job loss. The first group, called dislocated workers, includes persons whose unemployment stems from a permanent layoff or business closing. Under WIA, dislocated workers receive special attention and often have priority when funds for training are limited. The second group, whose members include all other unemployed job seekers, is not so statutorily privileged. I call these workers the "non-dislocated jobless."[2]

Another innovation of this study is its ability to investigate the impacts of training for the Native American population. Native Americans have been designated a special "at risk" population by USDOL, and have access to special training funds. No prior study has been able to explore the affects of training in this minority group.

Next, I address questions of program effectiveness in the face of both geographic and economic variation across South Dakota. The state contains several largely distinct geographic zones, with different industries dominating in different zones. How well do training programs perform across these different geographic areas? The unique SDLOS data provide insights into this important issue.

Lastly I investigate how training programs performed in periods of unusually high unemployment. The period prior to the 2007–09 recession was one of low and stable unemployment in South Dakota. Unemployment began to rise rapidly in late 2008, however, and remained relatively high for the remainder of the sample period. I use this exogenous unemployment shock to test whether training can be an effective anti-unemployment tool.

[2]This distinction is discussed in greater detail in Chapter 4.3.

4.2 The Employment Effects of Training: Baseline Specification

In this section I develop my baseline impact estimates of the OST and OJT programs. These baseline estimates provide the broadest possible measures of the employment effects attributable to WIA training. These results serve as the foundation upon which the analysis builds. Moreover, a key innovation of this dissertation is in its ability to go beyond the baseline specification, and to explore the affects of training along the various dimensions already discussed.

Before ensuing, it will be helpful to provide a short introduction to the layout of the result tables. The present example refers to Table 4.1, but all result tables in this work follow a similar presentation style. The leftmost column of Table 4.1 identifies the study cohorts: Control, OST, and OJT. The term cohort always refers the persons in treatment state j. To the right are a series of columns identifying distinct sample groups differentiated by some characteristic other than treatment. For example, Table 4.1 divides the sample into three groups: Combined, Male, and Female. The combined results report on the entire sample. The column labeled "Male," naturally reports estimates for men, and the column labeled "Female" reports estimates for women. The term "group" will always refer to a column of a result table.

4.2.1 Baseline Specification: Short-run Estimates

Table 4.1 presents short-run impact estimates for the baseline specification, where the short run refers to the first full calendar quarter following exit from SDLOS. The upper portion of Table 4.1 displays the mean employment rates for each of the training cohorts. Focusing first on the combined sample, I find that job seekers in the control

Table 4.1: Baseline Specification: Effect of training on short-runn employment. (SP)

	Combined		Male		Female	
Employment Probability	Mean	SD	Mean	SD	Mean	SD
Control	0.803	(0.006)	0.797	(0.010)	0.810	(0.009)
OST	0.830	(0.009)	0.832	(0.013)	0.825	(0.013)
OJT	0.889	(0.017)	0.911	(0.017)	0.878	(0.026)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	0.027**	(0.011)	0.035**	(0.016)	0.015	(0.016)
OJT vs. Control	0.086***	(0.019)	0.114***	(0.020)	0.068***	(0.027)
Observations	6322		2870		3452	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

cohort had, on average, an 80.3% employment probability in the first quarter after breaking contact with SDLOS. Stated otherwise, these job seekers had a 19.7% chance of remaining unemployed or leaving the labor force entirely during the first quarter after their exit from SDLOS.

The lower portion of Table 4.1 shows the average treatment effect for each training program. As defined in Equation 3.1, the ATE is the difference between the estimated mean employment rates of a cohort with training, and the control cohort, where the ATE estimator is given by $\widehat{\Delta}_{j0} = \widehat{\mu}_j - \widehat{\mu}_0,$ for $j = 1, 2$.

Occupational Skills Training.

Table 4.1 shows that OST increased the short-run likelihood of employment across all three specifications, albeit only to a modest degree. The mean employment rates for the OST cohort were: 83% for the combined sample, 83.2% for men, and 82.5% for women. In contrast, mean employment rates in the control cohort were 80.3%, 79.7%, and 81.0% for the combined, male, and female groups respectively.

For the combined sample, I found that training increased the probability of employment by 2.7 percentage points. Interestingly, the sample-wide effect masked some heterogeneity across gender. My findings show that OST had a stronger immediate impact on male employment than it did on female employment. OST increased male employment by 3.5 percentage points, but had no statistically significant impact on female employment.

On-the-job Training.

The employment rates of the OJT cohort were noticeably higher than those of the control and OST cohorts. Table 4.1 clearly illustrates this disparity. The estimated employment rate for the OJT cohort in the combined sample was 88.9%, 8.6 percentage points higher than the estimated 80.3% employment rate of the control cohort. From the perspective of unemployment, OJT reduced the probability of remaining unemployed from 19.7% to 11.1%. As a result, OJT reduced the short-run probability of unemployment by 44% $((11.1 - 19.7)/19.7)$.

Males experienced larger boosts to their employment prospects than did females. The estimated mean employment rate of the male OJT cohort was 91.1%. The average treatment effect of OJT for these males was 11.4 percentage points, leading to an unemployment rate of only 8.9% instead of the 20.3% for the male control cohort. OJT participants were, therefore, 56% $((8.9 - 20.3)/20.3)$ less likely to be unemployed in the first quarter after exit from the SDLOS than were their counterparts in the control cohort.

The employment effects of OJT for the female group, while smaller, were still statistically significant. Females with on-the-job training had, on average, an 87.8% probability of being employed in the short run. Their employment rate was 6.8 percentage points higher than that of females without any WIA training. As a result, OJT enrollment reduced the probability of female unemployment by 36% $((12.2 - 19)/19)$ relative to the control cohort.

4.2.2 Baseline Specification: Longer-run Estimates

Table 4.1 focused on immediate employment effects associated with training programs. Table 4.2 (following page) presents the longer-run treatment effect estimates of the OST and OJT programs. The longer-run estimates demonstrate how training influenced employment rates in the third full calendar quarter after exit from SDLOS.

Before discussing the outcomes of the OST and OJT cohorts, I first turn to the results for job seekers without training. Comparing the results in Tables 4.1 and 4.2, I found that the mean sample-wide employment rate for the control cohort fell from 80.3% in the first quarter to 77.6% in the third quarter. The first-to-third quarter employment rates for the male control cohort fell from 79.7% to 76.8%. Employment rates in the female control cohort also fell from 81% in the first quarter to 78.4% in the third.

The results indicate a roughly three percentage point decline in employment rates from the first to the third quarter. This secular employment decline is seen in all three training cohorts, and persists across multiple specifications. As will be seen below, the employment declines were smallest for dislocated workers, whose joblessness was a result of economic conditions. These findings suggest that declining employment effects of training were likely due to weak labor-force attachment in the population of workers served by SDLOS.[3]

Occupational Skills Training

[3] A systematic investigation into this phenomenon is beyond the scope of this study. The question remains for future research.

Table 4.2: Baseline Specification: Effect of training on longer-run employment. (SP)

	Combined		Male		Female	
Employment Probability	Mean	SD	Mean	SD	Mean	SD
Control	0.776	(0.007)	0.768	(0.010)	0.784	(0.009)
OST	0.803	(0.010)	0.791	(0.015)	0.817	(0.013)
OJT	0.851	(0.029)	0.828	(0.021)	0.875	(0.033)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	0.027**	(0.012)	0.023	(0.018)	0.032**	(0.016)
OJT vs. Control	0.074**	(0.030)	0.060***	(0.023)	0.091***	(0.034)
Observations	6322		2870		3452	

* p < 0.1, ** p < 0.05, *** p < 0.01.

OST trainees were not able to buck the downward employment trend. For the combined sample, I found that the employment rate of the OST cohort fell from 83% to 80.3%. A similar employment decline was experienced by men and women with occupational skills training. The average male employment rate fell from 83.2% to 79.1%, while average female employment rate dropped from 82.5% to 81.7%.

Perhaps more interesting than the evolution of the mean employment rates, however, was the evolution of the OST treatment effects. Comparing the results of Tables 4.1 and 4.2, it becomes clear that OST was a program with relatively small immediate benefits, but with the potential for bigger longer-term payoffs.

Women experienced an *increase* in the longer-term relative benefits of occupational skills training. Table 4.1 showed that women experienced no meaningful gain from OST in the short run. In the longer run, however, the treatment effect of OST was statistically significant, indicating that the benefits of OST might take time to develop. Table 4.2 reports that OST increased the probability of third quarter female employment by 3.2 percentage points.

Men, in contrast, witnessed a decline in the relative benefits of occupational skills training. Males with OST were still employed at a higher rate than males in the control cohort, 79.1% versus 76.8%. After only three quarters, the average treatment effect of OST was no longer statistically different from zero. It appears, for males at least, that the benefits of OST were not long-lived. This result was not seen in other cases, where the benefits of OST typically increased over time.

On-the-job Training.

Employment outcomes of the OJT cohort were more volatile than the outcomes of the OST cohort. Comparing the results in Tables 4.1 and 4.2, employment rates for the combined sample fell from 88.9% in the first quarter to only 85.1% in the third quarter. Male OJT employment dropped sharply from 91.1% to 82.8%. Female employment declined only marginally from 87.8% to 87.5%.

Employment in the male OJT cohort declined dramatically from the first to the third quarters after exit. There is no clear reason for the inability of OJT to support longer-run employment for males. A likely cause was that male OJT placements were with firms offering only temporary work. In the first quarter, the employment rate for males with on-the-job training was 11.4 percentage points higher than that of males in the control cohort. By the third quarter, the employment edge had narrowed to 6.0 percentage points. As a result, the ATE of on-the-job training for the male group fell by 47% $((6.0 - 11.4)/11.4)$.

Although the benefits of OJT attenuated over time for men, the gains from OJT increased for women. Table 4.2 shows that OJT *increased* the longer-run employment probability of women by 9.1 percentage points, up 33.8% $((9.1 - 6.8)/6.8)$ from 6.8 percentage points in the first quarter.

4.2.3 Baseline Specification: Summary and Context.

For the baseline results, I found that the employment effects of training were positive but not uniform. Both OST and OJT had positive, and generally significant, effects on short and longer-run employment rates. Results differed, though, between men and women.

Based on the short-run impact estimates, it seemed that OJT was the better performing training program. OJT strongly affected both male and female first quarter employment probabilities, e.g. the short-run treatment effects of OJT were roughly three times greater than those of OST. As a result, employment rates in the OJT cohort exceeded those of the control and OST cohorts by as much as 14%.

The short-run estimates also indicated that men benefited more than women from treatment. The first quarter OST response was twice as large for males than for females. Further, the first quarter OJT response was 1.67 times larger for males than it was for females.

The longer-run results, however, signaled that females gained more from training than did males. For females, the effects of OST and OJT rose from the first to the third quarter. In contrast, the treatment effects of both OST and OJT were smaller in the third quarter than in the first.

According to the baseline results, the effects of training varied depending on the population being studied. Fundamentally, the above results show that males benefited from training in the period immediately following their training, but the effects were much less potent in the longer-run. For females, in contrast, the benefits of training were small at first but increased greatly with time. In the following sections I delve deeper, exploring the benefits of training for several important sub-populations.

4.3 Dislocated Workers Versus the Non-dislocated Jobless: Who Benefited More from Training?

As discussed in Chapter 1, WIA training and employment services are available to all job seekers. The universal access provisions in the act enshrine this principle. But WIA also effectively ensures that certain groups of workers are first among equals. As with JTPA that came before it, WIA emphasizes assistance for dislocated workers over other unemployed persons.[4] Dislocated workers are given special consideration for training programs, and are sometimes given access to special services.

To illustrate the preferential treatment for dislocated workers, the Worker Adjustment and Retraining Notification Act (WARN) requires firms, under certain conditions, to report business closures or mass layoffs to their respective state and local governments.[5] When the South Dakota Department of Labor and Regulation receives WARN notices, SDLOS personnel visit the notifying firm and hold meetings with the soon-to-be-dislocated workers. During these meetings SDLOS staff inform workers of the employment and training services that are available to them should they be dislocated. The state does not engage in similar outreach for non-dislocated jobless people. The above is only one example, but is illustrative of the prioritization afforded to dislocated workers relative to other unemployed workers. Is such prioritization effective?

It is critical to examine how training affected the labor-market outcomes of

[4]Dislocated workers are unemployed due to business closure or mass layoff as a result of economic conditions. These workers are unlikely to find reemployment in their old occupations.

[5]Enacted in 1988, the WARN act ensures that State governments are aware of large worker dislocation events. In brief, firms with 100 or more employees are generally subject to WARN reporting requirements. Businesses are required to notify their State Department of Labor, or similar agency, of an impending closure or layoff if 50 or more employees will be effected. See http://www.doleta.gov/programs/factsht/warn.htm for general information regarding the WARN Act.

dislocated workers relative to the non-dislocated jobless. It may be that training helps dislocated workers find reemployment, or not. Because WIA implicitly de-emphasizes training for the non-dislocated jobless, it is important to examine whether training is more, or less, effective for dislocated workers.

4.3.1 Dislocated Workers Versus the Non-dislocated Jobless: Short-run Estimates

Table 4.3 (below) presents the short-run employment rate and treatment effect estimates for both dislocated workers and the non-dislocated jobless. As with the baseline estimates presented above, the upper panel of Table 4.3 displays estimated employment rates. The lower panel of the table reports the average treatment effects of each training program. The rightmost columns report on dislocated workers. The middle columns reports on the non-dislocated jobless.

In general, Table 4.3 reports substantially higher first quarter employment rates for dislocated workers than for the non-dislocated jobless. Higher employment rates, however, were not a result of training. In fact, training was *less effective* at increasing employment for dislocated workers than it was for the non-dislocated jobless, at least in the short run.

Beginning with the control group, Table 4.3 shows that the non-dislocated jobless, on average, had only a 74.6% employment probability in the first quarter following their exit from SDLOS. Dislocated workers with no training, in contrast, had a mean employment rate of 87.3%. Without any training, dislocated workers were, therefore, 17% more likely to have found employment in the short run than were the non-dislocated jobless. Stated otherwise, the non-dislocated jobless were twice as likely to be unemployed in the short run than their dislocated counterparts.

Table 4.3: Dislocated workers vs. the non-dislocated jobless: Effect of training on short-run employment. (SP)

	Non-dislocated		Dislocated	
Employment Probability	Mean	SD	Mean	SD
Control	0.746	(0.008)	0.873	(0.007)
OST	0.778	(0.011)	0.892	(0.007)
OJT	0.862	(0.020)	0.937	(0.010)
Avg. Treatment Effects	ATE	SD	ATE	SD
OST vs. Control	0.032**	(0.012)	0.019**	(0.007)
OJT vs. Control	0.116***	(0.022)	0.064***	(0.012)
Observations	3533		2789	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Occupational Skills Training.

The results presented in Table 4.3 show that OST increased the short-run employment rates for both dislocated workers and the non-dislocated jobless. The estimated mean employment rate for the OST cohort of non-dislocated jobless was 77.8%. The mean employment rate for the OST cohort of dislocated workers was 89.2%.

The bottom panel of Table 4.3 shows that first quarter employment effects of OST were relatively small, but still statistically significant. The average treatment effect of OST was 3.2 percentage points for the non-dislocated jobless; for dislocated workers the ATE was only 1.9 percentage points. OST was, therefore, nearly twice as effective for the non-dislocated jobless than for dislocated workers.

On-the-job Training.

Unsurprisingly, OJT, which by definition involves placing a trainee with a firm, had much stronger short-run employment effects than did OST. Table 4.3 (above) shows that OJT participants from the non-dislocated group had an 86.2% employment rate in the first quarter after exit from SDLOS. Dislocated workers with OJT experience were more likely to have been employed than their similarly trained, but non-dislocated counterparts. These dislocated workers with OJT experience had a mean employment rate of 93.7%.

My estimates indicate that OJT increased the employment rate of non-dislocated jobless by 11.6 percentage points, as compared to the control cohort. Dislocated workers who received on-the-job training were 6.4 percentage points more likely to be employed than were dislocated workers without training.

I report that OJT increased the short-run probability of employment by 15.5% (11.6/74.6) for the non-dislocated group, but by only 7.3% (6.4/87.3) for the dislocated worker group. Additionally, the already high employment rate of dislocated workers in the OJT cohort, 93.7%, left relatively little room for improvement. While the differences between the treatment effects of OST across worker types were not large — being only 1.3 percentage points — the differences between the OJT impact estimates were large and have important implications, which are discussed in Chapter 4.3.3 below.

4.3.2 Dislocated Workers Versus the Non-dislocated Jobless: Longer-run Estimates

The longer-run employment results exhibited a general decline in employment, a familiar finding which is revealed here by comparing Tables 4.3 and 4.4. The largest short-to-longer-term employment declines were in the group of non-dislocated jobless, indicating lower relative labor-force attachment. Nevertheless, the average treatment effects of OST and OJT were larger for the non-dislocated jobless than for dislocated workers.

Focusing first on the control cohort, the results of Tables 4.3 and 4.4 show that the

Table 4.4: Dislocated workers vs. the non-dislocated jobless: Effect of training on longer-run employment. (SP)

	Non-dislocated		Dislocated	
Employment Probability	Mean	SD	Mean	SD
Control	0.706	(0.008)	0.863	(0.007)
OST	0.746	(0.010)	0.886	(0.007)
OJT	0.794	(0.020)	0.911	(0.010)
Avg. Treatment Effects	ATE	SD	ATE	SD
OST vs. Control	0.040**	(0.013)	0.023**	(0.007)
OJT vs. Control	0.088***	(0.024)	0.048***	(0.013)
Observations	3533		2789	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

employment rate of the non-dislocated jobless declined by 4 percentage points from the first to the third quarters, falling from 74.6% to 70.6%. Dislocated workers in the control cohort exhibited much more stable employment over time. The employment rate of these workers declined by only 1 percentage point from the first to the third quarters, falling from 87.3% to 86.3%.

The control cohort results indicate that dislocated workers, even without training, were more able to find and maintain employment. But how did the OST and OJT cohorts fare in the longer run?

Occupational Skills Training.

The third quarter outcomes for the OST cohort were not as good as they had been in first quarter. The OST cohort of non-dislocated jobless witnessed a 3.2 percentage points decrease in their longer-run probability of employment, falling from 77.8% to 74.6%. In contrast, the employment rate in the dislocated worker group was relatively stable, dropping by only 0.6 percentage points, from 89.2% in the first quarter to 88.6% in the third quarter.

As was the case for the short run, in the longer run the non-dislocated jobless gained more from OST than did dislocated workers. Table 4.4 shows that the average treatment effect of OST increased the third quarter rate of employment for the non-dislocated jobless by 4.0 percentage points. OST increased the employment rate of dislocated workers, in contrast, by only 2.3 percentage points. In addition, comparing the results in Tables 4.3 and 4.4, reveals a repeated finding. The benefits of OST increased over time for both dislocated workers and the non-dislocated jobless. The evidence is mounting that OST was a training program with long-term benefits.

On-the-job Training.

Employment rates for the OJT cohort evolved similarly to those of the control and OST cohorts. The estimated employment rate of the OJT cohort of non-dislocated jobless dropped by 6.8 percentage points from the first to the third quarters, from 86.2% to 79.4%. Employment for the OJT cohort of dislocated workers, on the other hand, fell by only 2.6 percentage points from 93.7% to 91.1%

Both dislocated workers and the non-dislocated jobless experienced declines in their respective average treatment effects. Table 4.4 reports that the third quarter OJT treatment effect estimate for the non-dislocated jobless was 8.8 percentage points. This ATE estimate was down from 11.6 percentage points in the first quarter (see Table 4.3). For dislocated workers, the ATE of on-the-job training fell from 6.4 to 4.8 percentage points (according to Tables 4.3 and 4.4).

OJT was, therefore, an effective training program for dislocated workers and the non-dislocated jobless. On-the-job training produced both short and longer-run employment gains for these trainees, but the effects of OJT lacked staying power and faded somewhat as time passed.

4.3.3 Dislocated Workers Versus the Non-dislocated Jobless: Summary and Context.

When evaluating the employment outcomes of SDLOS job seekers, it seems that the circumstances of job loss were indeed important. Dislocated workers were employed at rates, on average, 8–14 percentage points higher than were the non-dislocated jobless, indicating that dislocated workers found reemployment more easily than the non-dislocated jobless. Additionally, dislocated workers tended to stay employed once they found work, as indicated by their relatively stable employment rates from the first to the third quarters. These findings, however, do not mean that training was ineffectual for the non-dislocated jobless.

The non-dislocated jobless experienced *larger* employment gains from training than did dislocated workers. Amongst the non-dislocated jobless, OJT ensured that 310 more persons had jobs in the third quarter after exit than would have in the absence of OJT.[6] Amongst dislocated workers, in contrast, OJT resulted in only 133 more third quarter jobs than would have been without OJT.[7] Moreover, dislocated workers readily found and maintained employment even in the absence of training. There was less relative gain from training dislocated workers. By focusing more resources towards the non-dislocated jobless, administrators can have a larger overall impact on the number of unemployed workers. If the goal is to get people back to work, the goal might be better served by increasing training for the non-dislocated jobless. Put another way, training may be more effective for the non-dislocated jobless than from incumbent workers dislocated by layoff or plant closings.

[6] There were $3,533$ non-dislocated jobless in the sample. Their third quarter ATE estimate for OJT was 8.8 percentage points. $3,533 \cdot .088 = 310.904$

[7] There were $2,789$ dislocated workers in the sample. Their third quarter ATE estimate for OJT was 4.8 percentage points. $2,789 \cdot .048 = 133.872$

4.4 Racial and Ethnic Disparities in Program Effectiveness: Did Minority Employment Respond to Training?

The previous section focused on dislocated workers and the non-dislocated jobless because WIA specifically attempts to help dislocated workers. But dislocated workers were not the only group to receive special considerations under WIA. As recently as 2014, USDOL announced an additional $58 million in WIA grant funding specifically allocated for training Native Americans (Kuruvilla, 2014). Because South Dakota is home to a meaningful Native American population, the SDLOS administrative data provide a unique opportunity to study the effects of training for this minority group.

I separated the sample into three distinct groups based on self-reported racial and ethnic identification. The first group was comprised of Native Americans. The second group, termed "White", was comprised of persons identifying as white. The last group, termed "Other", included all persons belonging to other demographic groups.[8]

4.4.1 Racial and Ethnic Comparison: Short-run Estimates.

The South Dakota administrative data indicate that Native Americans had much lower employment rates than did other racial and ethnic groups. Table 4.5 shows that the control cohort of Native American had a low 64.8% employment rate in the first quarter after exiting SDLOS. In contrast, the control cohorts in the White and Other groups had much higher employment rates, 82.1% and 79.1% respectively. Native Americans without WIA training had very low employment rates, leaving much room for training to increase employment, but did it?

[8] As discussed in Chapter 2.3, the Other category is comprised persons from varied racial and

Table 4.5: Training effectiveness across demographic groups: Effect of training on short-run employment. (FP)

	Native Am.		White		Other	
Employment Probability	Mean	SD	Mean	SD	Mean	SD
Control	0.648	(0.020)	0.821	(0.006)	0.791	(0.022)
OST	0.687	(0.021)	0.846	(0.007)	0.819	(0.021)
OJT	0.796	(0.031)	0.907	(0.014)	0.889	(0.021)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	0.039**	(0.015)	0.025**	(0.009)	0.028**	(0.011)
OJT vs. Control	0.148***	(0.030)	0.086***	(0.015)	0.098***	(0.020)
Observations	618		5356		348	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Occupational Skills Training.

Occupational skills training resulted in significant employment gains for all demographic groups. OST was most effective for Native Americans, however, as the average treatment effects of OST were larger for Native Americans than for the White or Other groups.

The short-run employment rate of Native Americans with OST experience was 68.7%. As a result, the employment rate of the Native American OST cohort was 3.9 percentage points higher than that of the control cohort. First quarter Employment rates for the White and Other groups were 84.6% and 81.9% respectively. For these groups, the treatment effect of OST increased the probability of employment by 2.5 and 2.8 percentage points.

On-the-job Training.

Once again the treatment effects of OJT were roughly three times larger than those of OST. OJT increased the employment rate of Native Americans by 14.8

ethnic backgrounds including: black, Hispanic, Asian, and Pacific Islander.

percentage points. The data show that placing Native Americans with employers via OJT increased the likelihood of employment by 22.8% (14.8/64.8).

For the White group, OJT increased the probability of third quarter employment by 8.6 percentage points, which was a 10.5% (8.6/82.1) increase over the employment rate for the White control cohort. For the Other group, the average treatment effect of OJT was 9.8 percentage points. As a result, OJT increased the probability of employment for the Other group by 12.8% (9.8/79.1).

So in terms of short-run employment, OJT was much more effective at increasing employment for Native Americans than it was for the other racial and ethnic groups. The average treatment effects of OJT were larger both relatively and in absolute magnitude for Native Americans than for either the White or Other groups.

4.4.2 Racial and Ethnic Comparison: Longer-run Estimates.

When the focus is extended from one quarter after training to three quarters, it can be seen that the longer-run employment prospects for Native Americans were much poorer than those of the other demographic groups. Perhaps worse than the low third quarter employment rates, though, was the general decline in Native American employment from the first to third quarters.

Comparing Table 4.6 with Table 4.5 shows that Native Americans were much less attached to their employment than the other racial and ethnic groups. Looking first at the control cohort, the first-to-the-third quarter employment rate amongst Native Americans fell by 5.7 percentage points: employment slid from the already low rate of 64.8% to the even lower rate of 59.1%. Persons without training in the White, and Other groups saw their employment rates drop by only slightly more than 2 percentage points.

Table 4.6: Effectiveness across demographic groups: Effect of training on longer-run employment. (FP)

	Native Am.		White		Other	
Employment Probability	Mean	SD	Mean	SD	Mean	SD
Control	0.591	(0.020)	0.797	(0.006)	0.768	(0.023)
OST	0.639	(0.022)	0.828	(0.008)	0.802	(0.022)
OJT	0.699	(0.034)	0.863	(0.017)	0.841	(0.025)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	0.048**	(0.016)	0.031**	(0.010)	0.034**	(0.011)
OJT vs. Control	0.108***	(0.031)	0.066***	(0.018)	0.074***	(0.021)
Observations	618		5356		348	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Occupational Skills Training.

While Native American employment may have been down in the longer-run, training still proved effective. OST was even more effective for Native Americans in the longer run than it was in the short run. The third quarter outcomes of Native Americans in the OST cohort were significantly better than for those in the control cohort. The mean employment rate for the OST cohort of Native Americans was 63.9% in the third quarter, compared to 59.1% for the control cohort. The OST treatment effect for the Native American group was 4.8 percentage points,up from 3.9 percentage points for the first quarter.

The employment outcomes of the White, and Other groups were objectively better than those of Native Americans. Longer-run employment rates for these groups exceeded 80%. The average treatment effects of OST in these groups were, however, smaller in magnitude than for the Native American group. The results, therefore, indicate that OST was more effective for Native Americans than the other racial and ethnic groups in both the short and longer-run.

On-the-job Training.

The longer-run OJT results reinforced the conclusion that Native Americans benefited more from training than did other groups. Table 4.6 shows that the longer-run OJT treatment effect was larger for Native Americans than for the other demographic groups, 10.8 percentage points versus 6.6 percentage points for the White group and 7.4 percentage points for the Other group. So the results show that OJT had its largest effects amongst Native Americans.

Nevertheless, the Native American OJT cohort also experienced a striking decrease in its likelihood of employment from the first to the third quarters, indicating that Native Americans, on average, were less likely to maintain jobs in the longer-run. Native American OJT enrollees had a 79.6% probability of employment in the first quarter after exit from SDLOS. By the third quarter, Native American OJT enrollees had only a 69.9% probability of employment. The OJT cohorts for the White and Other groups did not see such dramatic declines in their employment rates, signifying that Native Americans in South Dakota exhibited much weaker labor-for attachment than did the other demographic groups.

Previously I had reported that the effects of OJT faded with time. It seems that Native Americans were more susceptible to this fading effect than were the other demographic groups. Nevertheless, high first and third quarter ATE estimates indicate that Native Americans benefited the most from OJT.

4.4.3 Racial and Ethnic Comparison: Summary and Context.

Clearly Native Americans benefited greatly from WIA training services. The first and third quarter average treatment effects of both OST and OJT were larger for Native Americans than for other demographic groups. But, perhaps more importantly, Native

Americans had the most to gain from training. Native American employment rates were much lower than those of the White or Other groups. Native Americans also exhibited less attachment to their jobs, third quarter employment rates were as much as 9.7 percentage points lower than first quarter employment rates. Nevertheless, even in the face of reduced third quarter employment, the average treatment effects of training remained large and highly significant for Native Americans.

It seems that directing additional funding towards Native Americans might have been a worthwhile investment. Especially if the funds were directed towards OJT, as the longer-run effects of OJT were twice those of OST. Of course, the results here speak only to employment. In the next chapter I show that training also had disproportionately large affects on Native Americans earnings.

4.5 Regional Variation in Program Effectiveness: Does Geography Matter?

The previous sections have looked at training from a microeconomic perspective. Now I take a geographic perspective, exploring the effectiveness of training across the different regions of South Dakota. Might regional economic or geographic variation alter the ability of training to support employment?

As discussed in Chapter 2.3.3, South Dakota contains several distinct regions, each tending to concentrate on different types of economic activity. In this section, I report evidence that training was effective across all regions of South Dakota, being somewhat more effective in rural areas of the state where job opportunities tend to be more limited.

Table 4.7: Regional Variation: Effect of training programs on short-run employment. (FP)

Employment Probability	East Mean	Central Mean	West Mean	SF Mean	RC Mean
Control	0.816	0.754	0.740	0.819	0.762
OST	0.841	0.785	0.772	0.844	0.792
OJT	0.904	0.866	0.857	0.905	0.871
Avg. Treatment Effects	ATE	ATE	ATE	ATE	ATE
OST vs. Control	0.025**	0.031**	0.032**	0.024**	0.030**
OJT vs. Control	0.088***	0.112***	0.117***	0.086***	0.108***
Observations	3278	307	303	1547	887

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.5.1 Regional Variation: Short-run Estimates.

The upper panel of Table 4.7 displays the mean employment rates in each of the five regions studied here. The three leftmost columns correspond to the larger economic regions defined according to OES boundaries. The two rightmost columns refer to the state's two MSAs: Sioux Falls and Rapid City.

Two patterns emerge when looking at the short-run effects in Table 4.7. First, there were clear regional differences in overall employment rates. Employment rates were lowest in the most rural areas of South Dakota and higher in the more urban areas. Second, both OST and OJT had stronger effects in these same rural areas.

Occupational Skills Training.

The short-run employment results in Table 4.7 reveal considerable variation in mean employment rates across regions. Employment rates were lowest for the OST cohort in the Western region (77.2%) and were highest in the Sioux Falls MSA (84.4%). The estimated employment rates in the Central region and in the Rapid City MSA were 78.5% and 79.2% respectively. The employment rate of the OST cohort in the

Eastern region was 84.1%.

Table 4.7 also shows that there was only modest variation in the OST treatment effects across regions. The OST effects were biggest for the rural Western and Central regions, and smallest for the Sioux Falls MSA. So while previous results have shown considerable variation in the effects of OST across different *types* of individuals, the current results exhibit only modest variation across regions of South Dakota. OST was successful in both rural and urban environments, if somewhat more effective in rural areas.

On-the-job Training.

I found that the short-run employment rates of OJT cohort were similar to those of the OST cohort. The highest OJT cohort employment rates were in the Eastern region and in the Sioux Falls MSA. The lowest employment rates were in the more rural Western and Central regions. The largest OJT employment effects were found in the rural Central and Western regions, but OJT was also highly effective in the more urban regions, such as the Sioux Falls MSA.

4.5.2 Regional Variation: Longer-run Estimates.

The longer-run regional effects are displayed in Table 4.8. Each region, with the except for the Central region, experienced the same general decline in employment that was witnessed in previous specifications, with the Western region hit especially hard.

Occupational Skills Training.

The longer-run employment effects of OST were generally similar across all regions. The smallest ATE estimate was for the Sioux Falls MSA, where OST increased

Table 4.8: Regional Variation: Effect of training programs on longer-run employment. (FP)

Employment Probability	East Mean	Central Mean	West Mean	SF Mean	RC Mean
Control	0.788	0.767	0.648	0.789	0.751
OST	0.820	0.801	0.691	0.820	0.786
OJT	0.856	0.840	0.745	0.856	0.828
Avg. Treatment Effects	ATE	ATE	ATE	ATE	ATE
OST vs. Control	0.032^{***}	0.034^{***}	0.044^{***}	0.031^{***}	0.035^{***}
OJT vs. Control	0.068^{***}	0.074^{***}	0.097^{***}	0.067^{***}	0.077^{***}
Observations	3278	307	303	1547	887

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

employment by 3.1 percentage points. The largest ATE estimate was in the Western region, where OST increased employment by 4.4 percentage points. So the longer-run effectiveness of OST varied by only 1.3 percentage points from the most urban to the most rural regions. The effectiveness of OST, therefore, showed little sensitivity to regional variation within South Dakota.

But not only was OST similarly effective across regions, the third quarter average treatment effects of OST exceeded those of the first quarter. The results of Tables 4.7 and 4.8 demonstrate, once again, that OST was a training program whose impacts grew as time passed.

On-the-job Training.

Table 4.8 shows that the third quarter OJT average treatment effects exhibited greater regional variation than did those of OST. The largest average treatment effects were in the rural Western region, 9.7 percentage points, and the smallest effects were in the urban Sioux Falls MSA, 6.7 percentage points. So OJT too was more effective in the rural regions than in the urban regions, although it was effective in all regions.

The longer-run regional OJT results also exhibited the same fading effect observed in the baseline and dislocated worker versus non-dislocated jobless specifications. While still larger than those of OST, the third quarter OJT impact estimates were smaller than the first quarter OJT impact estimates.

4.5.3 Regional Variation: Summary and Context.

The short and longer-run regional employment results of this section paint a clear picture: both OST and OJT effectively increased employment across all South Dakota regions. The employment effects were slightly larger in rural areas than in urban ones, but training had significant employment effects across all regions.

The general consistency of the regional results indicates that WIA training programs were able to increase employment regardless of region. Previous training evaluations have not been able to address this issue, but the above results indicate that training can be effective in both urban and rural areas. It remains to be seen if training was also able to increase incomes across all regions.

4.6 Training Before and After the Great Recession: Does Employment Respond in Periods of Higher Unemployment?

Having explored the effects of WIA training from microeconomic and regional perspectives, I turn now to a more macroeconomic perspective, and look at the performance of training along the business cycle. Since the 2007–09 recession, policy makers have become more interested in training programs as an active labor-market policy instrument. As such a policy tool, WIA training programs might be used to counter the negative labor-market effects of recessions. The SDLOS data allow me to explore this possibility.

The 2007–09 recession saw the highest unemployment rates in the US since the 1980–81 recession. Even more problematic, however, was the increased length of typical unemployment spells. In July 2011 the average unemployment spell peaked at 40.6 weeks. As of this writing, it still exceeds 30 weeks.

In a climate of recession, training programs have been touted as a potentially useful employment tool. What do the South Dakota data have to say about this? Were these training programs successful in South Dakota during the Great Recession? I have reported that OST and OJT were able to increase employment across worker types, and even across regions. But what do the data say about the relative effectiveness of OST and OJT in periods of unusually high unemployment?

To analyze the performance of training programs in periods of higher unemployment, I split the data into two time periods surrounding the 2007–09 recession. South Dakota witnessed an unemployment shock as did much of the country following the recession. The shock hit South Dakota during the latter half of 2008, causing unemployment to rise rapidly.[9] I therefore broke the data into two time periods centered around the fourth calendar quarter of 2008 (Q4 2008). The first group I examined exited SDLOS prior to October 2008, and the second group exited after October 1, 2008. I analyzed the employment outcomes of these two groups of job seekers in order to evaluate the relative performance of WIA training programs in periods of lower versus higher unemployment.

[9]Table B.1 in Appendix B provides summary data on unemployment rates in South Dakota during the study period.

Table 4.9: Training in periods of high unemployment: Effect of programs on short-run employment. (SP)

	Pre Q4 2008		Post Q4 2008	
Employment Probability	Mean	Std. Dev.	Mean	Std. Dev.
Control	0.804	(0.008)	0.806	(0.011)
OST	0.835	(0.012)	0.830	(0.013)
OJT	0.896	(0.025)	0.877	(0.020)
Avg. Treatment Effects	ATE	Std. Dev.	ATE	Std. Dev.
OST vs. Control	0.031**	(0.015)	0.025	(0.016)
OJT vs. Control	0.092***	(0.027)	0.071***	(0.022)
Observations	3795		2527	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Sample divided based on exit data of October 1, 2008.

4.6.1 Training Before and After the Great Recession: Short-run Estimates.

Table 4.9 reports on the short-run returns to training for job seekers in the lower and higher unemployment periods. The leftmost results column presents the results for the period of lower unemployment and is labeled, "Pre-Q4 2008." The rightmost results column displays the estimates for the period of higher unemployment and is labeled, "Post-Q4 2008."

I found the employment rates for the control, OST, and OJT cohorts were generally in-line with the combined-sample baseline estimates presented earlier (as seen in Table 4.1). There was very little difference in the employment rates of the three training cohorts across time periods. Table 4.9 shows that the largest employment differential was in the OJT cohort, and even then, the difference between the pre- and post-Q4 2008 employment rates was only 1.9 percentage points. So it appears that training was less effective during the period of higher unemployment than in the period of lower unemployment.

Occupational Skills Training.

Short-run employment rates for the OST cohort were stable over the business cycle. Prior to Q4 2008 the employment rate for the OST cohort was 83.5%. After Q4 2008 the employment rate for the OST cohort was 83%.

While I found similar employment rates for the OST cohort in both the lower and higher unemployment periods, I also found that the average treatment effect of OST was only significant in the lower unemployment period. Prior to Q4 2008, OST increased employment by 3.1 percentage points, and the ATE estimate was significant at the 5 percent level. After Q4 2008, the ATE of occupational skills training was not statistically significant. So while OST was effective prior to Q4 2008, it did not support immediate reemployment in the wake of the Great Recession.

On-the-job Training.

The short-run employment rates of the OJT cohort, as in all previous specifications, were higher than those of the control and OST cohorts. The estimated mean employment rates of the OJT cohort were 89.6% in the pre-Q4 2008 period, and 87.7% post-Q4 2008.

The ATE of on-the-job training was 9.2 percentage points in the lower unemployment period, falling to 7.1 percentage points for the higher unemployment period. The short-run results, therefore, indicate that OJT was slightly more effective prior to the Great Recession than afterwards. The short-run effect of OJT was significant post-Q4 2008, unlike the short-run effect of OST, but it was, nevertheless, smaller in magnitude than prior to Q4 2008. Thus, the results in Table 4.9 indicate that OJT was not as effective in the period of unusually high unemployment.

4.6.2 Training Before and After the Great Recession: Longer-run Estimates.

The longer-run estimates presented in Table 4.10 (below) present evidence of new employment trends surrounding the control and OST cohorts. First, the employment rates of the control cohort did not seem to behave similarly to those of the OST and OJT cohorts. Pre-Q4 2008 employment rates were higher than post-Q4 2008 employment rates for the OST and OJT cohorts. For the control cohort, however, post-Q4 2008 employment rates actually exceeded pre-Q4 2008 employment rates. The control cohort had an expected employment rate of 76.9% prior to Q4 2008, but after Q4 2008 the expected employment rate of the control cohort was 78.8%.

According to Table 4.10, in contrast, neither the OST nor the OJT cohorts experienced relative increases in their longer-run employment rates over the business cycle. Moreover, looking at Tables 4.9 and 4.10, one sees that the control cohort had more stable employment rates *during* the recessionary period of higher unemployment than it did in the period of lower unemployment.[10] The control cohort has not exhibited such first-to-third quarter employment stability in any previous specification. Why should the control cohort, which had no WIA training, have fared so well in a recessionary period? As discussed in more detail below, there appears to be increased competition for jobs from "higher quality" job seekers, who entered into SDLOS as a consequence of the Great Recession.

Occupational Skills Training.

OST again proved particularly ineffectual during the period of higher unemploy-

[10] Prior to Q4 2008, the control cohort's employment rate fell 3.5 percentage points from the first to the third quarters (76.9 – 80.4). After Q4 2008, the control cohort's employment rate fell 1.8 percentage points from the first to the third quarters (78.8 – 80.6).

Table 4.10: Training in periods of high unemployment: Effect of programs on longer-run employment. (SP)

	Pre Q4 2008		Post Q4 2008	
Employment Probability	Mean	Std. Dev.	Mean	Std. Dev.
Control	0.769	(0.009)	0.788	(0.009)
OST	0.812	(0.013)	0.793	(0.014)
OJT	0.858	(0.023)	0.860	(0.035)
Avg. Treatment Effects	ATE	Std. Dev.	ATE	Std. Dev.
OST vs. Control	0.043***	(0.016)	0.005	(0.019)
OJT vs. Control	0.089***	(0.024)	0.072*	(0.037)
Observations	3795		2527	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Sample divided based on exit data of October 1, 2008.

ment. Tables 4.9 and 4.10 show that OST increased first and third quarter employment prior to Q4 2008, but not after Q4 2008. The implication is clear: OST increased employment prior to the recession, but was ineffective afterwards. OST did not succeed as an anti-recession program. What could have hampered the ability of OST to support employment in the post-Q4 2008 period? Again, as discussed below, the answer seems to be increased competition from persons recently made unemployed due to the Great Recession.

On-the-job Training.

Persons in the OJT cohort were employed, as always, at higher rates than were persons in the control or OST cohorts. In the period of lower unemployment, members of the OJT cohort had an 85.8% probability of employment during their third quarter after exit from SDLOS. These job seekers were employed at a rate 8.9 percentage points higher than were their untrained counterparts during the same period.

During the period of higher unemployment, the OJT cohort had a mean employment rate of 86%. So the employment rate of the OJT cohort remained basically unchanged over the business cycle. But the post-Q4 2008 ATE estimate was actually smaller that it was prior to Q4 2008, now being only 7.2 percentage points. As a result, OJT was slightly less effective after the recession than before.

4.6.3 Training Before and After the Great Recession: Summary and Context.

I find little evidence to support the conclusion that training programs can counteract the negative effects of the business cycle. I found training was less effective at increasing the employment of trainees during the post recession period. OJT treatment effect estimates were larger prior to Q4 2008 than they were afterwards. Moreover, after Q4 2008 the employment effect of OST was not statistically different from zero in either the short or longer-run. These findings indicate that training was less effective in the post-recession period. But do the data give clues as to why training became less effective? In fact the data do provide such clues, and it appears the answer could lie with the control cohort.

Control cohort employment during both the first and third quarters was actually higher in the post-Q4 2008 period than it was in the pre-Q4 2008 period. This phenomenon was not observed for either of the OST or OJT cohorts, indicating that untrained workers were relatively more effective at finding jobs during the recessionary period than were trained persons.

The most probable cause of this employment reversal, i.e. the increased employment for the control cohort but decreased employment for the OST and OJT cohorts, was a change in the relative composition of the SDLOS sample. Table 4.11 (below) demonstrates how levels of educational attainment changed in the SDLOS sample over the course of the business cycle. I found greater numbers of better educated people

Table 4.11: Summary of education attainment in periods of low and high unemployment

	Pre Q4 2008		Post Q4 2008	
Educational Attainment	Mean	Count	Mean	Count
Sample Wide				
Less than High School	0.126	480	0.088	223
High School Grad	0.585	2222	0.537	1357
GED or Equivalent	0.147	559	0.133	338
Associate or License	0.083	318	0.150	380
Bachelor's Degree	0.055	212	0.090	228
Observations		3795		2527
Control Cohort				
Less than High School	0.169	393	0.126	176
High School Grad	0.540	1254	0.514	717
GED or Equivalent	0.144	335	0.124	174
Associate or License	0.086	200	0.139	195
Bachelor's Degree	0.059	137	0.093	130
Observations		2322		1393
OST Cohort				
Less than High School	0.054	69	0.041	41
High School Grad	0.676	858	0.570	568
GED or Equivalent	0.133	169	0.146	146
Associate or License	0.081	103	0.157	157
Bachelor's Degree	0.053	68	0.083	83
Observations		1268		995
OJT Cohort				
Less than High School	0.087	18	0.043	6
High School Grad	0.536	110	0.517	72
GED or Equivalent	0.268	55	0.129	18
Associate or License	0.073	15	0.201	28
Bachelor's Degree	0.034	7	0.107	15
Observations		205		139

entered the sample after Q4 2008 than beforehand.

Table 4.11 shows that the number of "high quality" job seekers in the sample *increased* after Q4 2008, likely as a result of the 2007–09 recession.[11] The number of persons with Associate's degrees, professional licenses, and Bachelor's degrees,

[11]In the current context, "high quality" simply indicates persons with higher levels of educational attainment.

increased in both absolute and relative frequency after Q4 2008.

This development is most easily seen by considering persons in the OST cohort with an Associate's degree or some form of professional license. In the 7-year period prior to Q4 2008, 103 of these persons had enrolled in OST versus 157 of these persons in the 3 years after Q4 2008. The higher absolute enrollment of these people after Q4 2008 could only have occurred if they were registering with SDLOS at a greatly increased rate, as compared to their enrollment rate prior to Q4 2008. This enrollment pattern can be seen across all training cohorts.

It seems likely that the increased post-recession entry of higher quality job seekers, especially into the control cohort, reduced the relative efficacy of WIA training. These atypical SDLOS users competed with more traditional SDLOS users, and therefore depressed the observed treatment effects of training.

In summary, I found that training programs did not mitigate cyclical unemployment. Training may be effective in combating long-term issues, such as skills mismatch, but I found no evidence that training was particularly effective in the period of economic downturn. On the contrary, my analysis indicates that training was less effective in periods of economic stress than it was in periods of stability.

4.7 Summary of Employment Effects.

To summarize the myriad results presented in this chapter, I have reported that WIA training programs had positive impacts on employment prospects, but the effects were not uniform. Regardless, some clear trends emerged.

First, OJT was more effective at increasing both short and longer-term employment than was OST. Across all specifications, employment rates for the OJT cohort were higher than those of the OST cohort. Additionally, average treatment effects of OJT were consistently larger than those of OST. Even though the effects of OJT diminished with time, the employment effects of OJT remained larger than those of OST.

Second, training had larger third quarter effects for women than for men. The average treatment effects of OST and OJT were larger for males in the first quarter than they were for females. By the third quarter, however, the employment effects of both OST and OJT were larger for females than for males.

Third, training was more effective at increasing employment for the non-dislocated jobless than it was for dislocated workers. Both OST and OJT had larger average treatment effects for the non-dislocated jobless than for dislocated workers.

Fourth, training was effective for all racial and ethnic groups, but was especially effective for Native Americans. The Native American employment effects owing to OST and OJT were consistently larger than those for the White or Other groups.

Fifth, training was generally effective across both rural and urban areas. My results indicated that training was slightly more effective in rural areas, but training was also able to increase employment in urban areas as well.

Sixth, and finally, OST was not effective in the wake of the Great Recession. OJT did increase both short and longer-term employment in the period of higher unemployment, but OST had no influence on employment after Q4 2008.

Chapter 5

Assessing Program Effectiveness: Earnings Effects of Occupational Skills and On-the-job Training.

The next step in the analysis is naturally to investigate how WIA training affected the post-training earnings of trainees. Following the pattern established by the previous chapter, I report the estimated effects of training on quarterly earnings in the first and third full calendar quarters after exit from SDLOS.

I begin once more by discussing baseline effects. After establishing baseline earnings effects, I expand the analysis to explore the effects of training for: (1) dislocated workers and the non-dislocated jobless, (2) different demographic groups, (3) workers residing in different geographic regions of South Dakota, and (4) workers displaced at different points along the business cycle. All results are presented in constant 2004 US dollars.

5.1 The Earnings Effects of Training: Baseline Specification

In this section I present baseline earnings estimates, which like the baseline employment results, lay the foundation for the more in-depth results to come. It is important to note that estimated average earnings reflect quarterly earnings, that is, total labor income for a given quarter. Thus, average treatment effects are quarterly effects. Because the South Dakota data do not report on hours worked, it is not possible to calculate hourly wage rates. Thus, this study cannot *directly* measure the effects of training on hourly wages.

5.1.1 Baseline Specification: Short-run Estimates

The short-run estimates in Table 5.1 (below) provide a first look into the income effects of WIA training. The table presents the estimated average first quarter earnings of the control, OST, and OJT cohorts. Average quarterly earnings are reported separately for the combined sample, males, and females.

The upper portion of Table 5.1 gives the average first quarter earnings of each cohort and group. For example, the table shows the typical individual in the control cohort earned $\$4,240$ in his or her first full calendar after exit from SDLOS. Referring again to the control cohort, men with no WIA training earned, on average, $\$4,885$ in their first quarter after exit from SDLOS. The average untrained woman earned $\$3,718$ in her first quarter after exiting SDLOS, implying a gender earnings gap similar to that observed nationwide.

Table 5.1: Baseline Specification: Effect of training on short-run quarterly earnings. (SP)

	Combined		Male		Female	
Mean Earnings	Mean	SD	Mean	SD	Mean	SD
Control	4240	(58)	4885	(101)	3718	(66)
OST	4745	(86)	5392	(140)	4188	(102)
OJT	5060	(195)	5581	(385)	4997	(303)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	505^{***}	(100)	507^{***}	(168)	470^{***}	(117)
OJT vs. Control	819^{***}	(202)	695^{*}	(396)	1278^{***}	(308)
Observations	6322		2870		3452	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Occupational Skills Training.

The results in Table 5.1 show that the OST cohort outearned the control cohort in each of the three baseline specifications. The sample-wide results indicate that the average OST enrollee earned $4,745 in the first quarter after exit from SDLOS. As a result, the typical OST enrollee earned $505, or 11.9% (505/4,240), more in the first quarter than did the typical member of the control cohort.

The average man in the OST cohort earned more than his counterpart in the control cohort. Average first quarter earnings for the male OST cohort were $5,392. The average treatment effect of OST on male earnings was, therefore, $507, which represented a 10.4% (507/4,885) premium over the control cohort's earnings.

Interestingly, whereas OST did not significantly affect short-run female employment (that is, the short-run OST employment effect was not statistically significant for females), I found significant OST income effects amongst women. Average first quarter earnings for the female OST cohort were $4,188, meaning the typical woman with occupational skills training earned $470, or 12.6% (470/3,718), more in the first quarter than did the typical woman without training. As a result, OST was slightly more effective for women than men when it comes to boosting income, at least in the short run.

On-the-job Training.

The short-run average earnings estimates for the OJT cohort were larger than those of either the control or OST cohorts. The typical male in the OJT cohort was expected to earn $5,581 in his first quarter after exit from SDLOS, as opposed to $4,885 for the male control cohort. On-the-job training, therefore, increased his relative first quarter earnings by $695, or 14.2% (695/4,885).

Short-run female earnings responded much more strongly to OJT than did male earnings. The typical female with OJT experience earned, on average, $4,997 in the first quarter. The short-run average treatment effect of OJT was an astounding $1,278, or 34.3% (1,278/3,718), increase in first quarter income relative to females without training. Thus, OJT was much more effective at increasing female incomes than it was at increasing male incomes. It remains to be seen if these immediate earnings gains persisted over time.

5.1.2 Baseline Specification: Longer-run Estimates

The longer-run earnings results are shown in Table 5.2 (below). Two notable trends appear when comparing these longer-run findings with the short-run estimates in Table 5.1.

First, the longer-run results indicate declining incomes for the control and OJT cohorts. The earnings shortfall was small for the control cohort, but much larger for the OJT cohort. The earnings shortfall was likely related to the generally lower employment rates in the third quarter versus the first quarter, as discussed in the

Table 5.2: Baseline Specification: Effect of training on longer-run quarterly earnings. (SP)

	Combined		Male		Female	
Mean Earnings	Mean	SD	Mean	SD	Mean	SD
Control	4123	(59)	4733	(100)	3632	(68)
OST	4831	(101)	5432	(171)	4338	(113)
OJT	4691	(234)	5107	(281)	4525	(312)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	708***	(113)	699***	(193)	705***	(127)
OJT vs. Control	567**	(240)	374	(294)	892***	(318)
Observations	6322		2870		3452	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

previous chapter. For example, I found that quarterly earnings for the male OJT cohort fell by 8.5% over time, falling from $5,581 in the first quarter to $5,107 in the third quarter.

The second trend is the increased earnings of the OST cohort. Whereas employment and earnings simultaneously fell for the control and OJT cohorts, the OST cohort saw its average quarterly earnings rise, even in the face of declining employment. For example, the third quarter earnings for the female OST cohort were $4,338, up 3.6% from $4,188 in the first quarter. Evidently OST engendered positive wage and/or labor supply effects that OJT could not. In other words, schooling had a more persistent impact on earnings than did on-the-job training.

Occupational Skills Training.

Comparing Table 5.1 with Table 5.2 shows that both men and women increased their third quarter earnings as a result of OST participation. Average third quarter earnings of the male OST cohort were $5,432, up slightly from $5,392 for the first quarter. Female earnings increased by $150 from the first to the third quarters after exit, rising from $4,188 to $4,338.

Occupational skills training also increased the relative earnings of males and females. The third quarter earnings of the typical male OST trainee exceeded those of a typical untrained male by $699, or 14.8% (699/4,733). But the female OST cohort experienced an even stronger relative income boost than did the male OST cohort. During the third quarter following exit from SDLOS, the typical woman with occupational skills training was expected to earn $4,338, which was $705, or 19.4% (705/3,632), more than the typical woman without training.

Even more important than the third quarter income effects, though, was the increased effectiveness of OST from the first to the third quarters. The third quarter male ATE estimate of $699 is 37.9% more than the first quarter ATE estimate of $507. For females, the average treatment effect of OST grew from $470 for the first quarter to $705 for the third, a 48.8% increase in only six months. Thus, in the short run OST was not only effective at increasing incomes, but its effects grew substantially with time.

On-the-job Training.

The longer-run results paint a more complicated picture for the OJT cohort than for the OST cohort. While still earning more than their respective control cohorts, both the male and female OJT cohorts saw declines in their absolute and relative earnings. These findings reinforce the baseline results from last chapter. OJT was generally more effective than OST at increasing earnings, but the benefits of OJT were shorter lived, whereas the benefits of OST grew over time.

Tables 5.1 and 5.2 show that the mean third quarter earnings of the male OJT cohort were $5,107, down 8.5% from the first quarter ((5,107 − 5,581)/5,581). The mean third quarter earnings for the female OJT cohort were $\$4,525$, down 9.4% from the first quarter $((4,525 - 4,997)/4,997)$.

In addition to the drop in absolute earnings, the male OJT cohort also experienced a drop in its relative gains from training. The third quarter ATE estimate for men was no longer statistically significant, indicating a collapse in the ability of OJT to increase longer-run male earnings.

Female earnings were also hit hard. I found that the income effects of OJT dropped dramatically amongst females, falling from $\$1,278$ for the first quarter to \$892 in the third. Thus women experienced a 30% $((892 - 1,278)/1,278)$ decline in the relative benefits of OJT by the third quarter.

5.1.3 Baseline Specification: Summary and Context.

The above baseline income and treatment effect estimates illustrate several important results. Financially, women benefited more from training than did men. In the short run, OST increased male earnings by 10.4%, but increased female earnings by 12.6%. Also in the short run, OJT drove up male earnings by 14.2%, but boosted female earnings by 34.3%. Thus, female earnings responded more strongly to training than did male incomes, at least in the short run. The same also held true for longer-run earnings.

The earnings results reveal clear trends. Training was effective at increasing the earnings of both men and women. Females, however, benefited more from training than did males, as the average treatment effects of OST and OJT were generally larger for women than for men.

The earnings impact of OST also bears notice. Whereas the control and OJT cohorts saw declines in their average earnings from the first to the third quarters after leaving SDLOS, the OST cohort did not. The average third quarter earnings of the male and female OST cohorts exceeded their first quarter earnings. As a result, the average treatment effects of OST were larger in the third quarter than they were in the first quarter. The opposite was true for OJT. This pattern of increasing income gains from OST was found in every other estimated specification.

Given the foregoing, it is tricky to identify the superior training program. OJT put people back to work quickly, leading immediately to higher employment rates and higher earnings for trainees. But the effects of OJT were already fading by the third quarter after leaving the program. OST, on the other hand, did not produce the same immediate employment results that OJT did, but OST income effects were more durable. Evidently schooling had more lasting effects on earnings than did on-the-job training.

5.2 Dislocated Workers Versus the Non-dislocated Jobless: Who Benefited More from Training?

Building on the baseline results, I now present the income estimates for dislocated workers and the non-dislocated jobless. In the previous chapter I showed that training disproportionately benefited the non-dislocated jobless. Dislocated workers were employed at higher rates than the non-dislocated jobless, but the average treatment effects of OST and OJT were roughly twice as large for the non-dislocated jobless as they were for dislocated workers.

The results of this chapter reinforce the findings of the previous chapter. I report that dislocated workers, on average, earned far more than the non-dislocated jobless, in some cases as much as 50% more. Yet, whereas training did not significantly affect

Table 5.3: Dislocated workers vs. the non-dislocated jobless: Effect of training on short-run quarterly earnings. (SP)

	Non-dislocated		Dislocated	
Mean Earnings	Mean	SD	Mean	SD
Control	4156	(138)	5800	(95)
OST	4813	(154)	5980	(104)
OJT	4719	(258)	6223	(264)
Avg. Treatment Effects	ATE	SD	ATE	SD
OST vs. Control	657***	(124)	179	(132)
OJT vs. Control	563**	(242)	423	(278)
Observations	3533		2789	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the relative earnings of dislocated workers, it did significantly boost the earnings of the non-dislocated jobless.

5.2.1 Dislocated Workers Versus the Non-dislocated Jobless: Short-run Estimates

Table 5.3 (below) displays the estimated mean earnings of dislocated workers and the non-dislocated jobless. Once again, I found that training strongly affected the outcomes of the non-dislocated jobless, but had very little impact on the outcomes of dislocated workers. Dislocated workers still outearned their non-dislocated contemporaries, however.

The average untrained dislocated worker earned $5,800 for the first quarter after leaving SDLOS. In contrast, the average earnings for the control cohort of the non-dislocated jobless were only $4,156 in the first quarter. Therefore, even without any influence from training, dislocated workers outearned the non-dislocated jobless on average by $1,644, or 39.6% (1,644/4,156), by the end of the first quarter following exit from SDLOS.

Occupational Skills Training.

While the previous chapter showed that OST significantly increased the employment rates of both dislocated workers and the non-dislocated jobless, the same cannot be said about earnings effects. Table 5.3 shows that OST significantly improved the earnings of the non-dislocated jobless, but had no such effect on the earnings of dislocated workers.

The mean first quarter earnings for the non-dislocated jobless were $4,813. Enrollment in occupational skills training, therefore, increased first quarter earnings by $657 relative to what they would have been in the absence of training. As a result, the typical OST trainee earned 15.8% more in her first quarter after exit from SDLOS than she would have earned without training. Importantly, Table 5.3 shows that the short-run ATE estimate for OST is actually larger than the ATE estimate for OJT. This is the first occurrence of a short-run OST effect dominating a short-run OJT effect.

On-the-job Training.

Table 5.3 also shows that OJT had no statistically significant effects on the short-run earnings of dislocated workers. Average first quarter earnings for dislocated OJT enrollees were $6,223. But the ATE estimate is not significant at any statistically meaningful level. As a result, on-the-job training had no effect on the short-run incomes of dislocated workers.

The non-dislocated jobless, in contrast, experienced significant income gains from OJT. Average first quarter earnings for the OJT cohort of non-dislocated jobless were $4,719. The average treatment effect was $563, meaning a 13.5% (563/4156) premium over the earnings of the control cohort.

It appears that training did nothing meaningful for the short-run incomes of dislocated workers. Both OST and OJT increased employment amongst dislocated workers, but training did not improve earnings. Perhaps training had a meaningful impact on the longer-run earnings of dislocated workers.

5.2.2 Dislocated Workers Versus the Non-dislocated Jobless: Longer-run Estimates

Table 5.4 (below) reports the longer-run earnings findings for the two types of unemployed job seekers. These longer-run results, when viewed in conjunction with earlier results in this chapter and those from Chapter 4, reinforce previously identified trends.

First, OST produced larger income effects than did OJT. Second, the income effects of OST increased with time, whereas the effects of OJT faded. Finally, the third quarter earnings of both dislocated workers and the non-dislocated jobless were generally lower than for the first quarter.

Looking first at the experiences of the control cohort, I found that dislocated workers earned, on average, $5,546 in the third quarter after exit from SDLOS. In stark contrast, average third quarter earnings for the control cohort of non-dislocated jobless were only $3,675. Even in the absence of training, the longer-run earnings of dislocated workers exceeded those of the non-dislocated jobless by $1,871, or 51% $((5,546 - 3,675)/3,675)$.

Occupational Skills Training.

Table 5.4 shows that OST significantly increased the third quarter earnings of both dislocated workers and the non-dislocated jobless. Average third quarter earnings for the non-dislocated jobless were $4,535. In contrast, average third quarter earnings for

Table 5.4: Dislocated workers vs. the non-dislocated jobless: Effect of training on longer-run quarterly earnings. (SP)

	Non-dislocated		Dislocated	
Mean Earnings	Mean	SD	Mean	SD
Control	3675	(148)	5546	(122)
OST	4535	(168)	5971	(137)
OJT	4037	(288)	5833	(329)
Avg. Treatment Effects	ATE	SD	ATE	SD
OST vs. Control	860***	(140)	424**	(167)
OJT vs. Control	362	(274)	287	(343)
Observations	3533		2789	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

dislocated workers were $5,971.

While dislocated workers might have earned more than the non-dislocated jobless, schooling was more effective for the non-dislocated jobless. OST resulted in an average treatment effect of $860 for the non-dislocated jobless, increasing the relative earnings of trainees by 23.4%. The ATE for dislocated workers was only $424, representing only a 7.6% earnings premium. So while occupational skill training increased incomes, the effects of OST were much larger for the non-dislocated jobless than for dislocated workers.

On-the-job Training.

There is little to say regarding the third quarter affects of OJT. The OJT cohorts fared no better than their respective control cohorts. The estimated third quarter average treatment effects of OJT were not statistically significant at even the 10% level. The findings reported in Table 5.4 indicate that OJT had no longer-term effects on the earnings of either dislocated workers or the non-dislocated jobless. As was seen in the baseline earnings results, the treatment effects of OST grew with time, but the treatment effects of OJT disappeared.

5.2.3 Dislocated Workers Versus the Non-dislocated Jobless: Summary and Context.

The earnings results of this section confirm, perhaps even more so than the employment results of Chapter 4, that dislocated workers had better labor-market outcomes than did the non-dislocated jobless. Regardless of training, displaced workers had much higher earnings than did the non-dislocated jobless. Average earnings amongst displaced workers exceeded those amongst the non-dislocated jobless in both the first and third quarters after exit from SDLOS. Nevertheless, the incomes of dislocated workers were less responsive to training than were the incomes of the non-dislocated jobless.

Comparing the effectiveness of OST and OJT reveals that OST was much more effective at increasing the earnings of both displaced workers and the non-dislocated jobless. First, OJT had no effect on the relative earnings of displaced workers in either the short or longer run. I did find evidence that OJT increased the first quarter earnings of the non-dislocated jobless, but the effect was short-lived and disappeared by the third quarter.

OST, on the other hand, was associated with large and increasing income effects for the non-dislocated jobless. OST drove up the first quarter earnings of the non-dislocated jobless by 15.8% and boosted third quarter earnings by 23.4%. In contrast, OST had no effect on the first quarter earnings of dislocated workers, but increased their third quarter earnings by only 7.6%.

It seems clear now that WIA training was more effective for the non-dislocated jobless than it was for displaced workers. The employment results of the previous chapter indicated that training was slightly more effective for the non-dislocated jobless. The results of this chapter were more emphatic. Training, especially OST, was much more effective for the non-dislocated jobless than displaced workers.

5.3 Racial and Ethnic Disparities in Program Effectiveness: Did Minority Earnings Respond to Training?

In this section I examine how WIA training affected the incomes of different racial and ethnic groups. As in the previous chapter, I separated the sample into three groups: Native American, White, and Other, where "Other" refers to persons who identified as neither white nor Native American. In the previous chapter I showed that the employment rates of the White and Other groups were much higher than those of Native Americans, but the employment effects of training were actually larger for Native Americans than for the White or Other groups.

The earnings results presented here follow the same pattern. Native Americans typically earned much less than did persons from the White or Other groups. I also found that WIA training led to larger absolute and relative gains for Native Americans than for non-Native Americans.

5.3.1 Racial and Ethnic Comparison: Short-run Estimates.

Table 5.5 presents the short-run quarterly earnings estimates for each demographic group. The table shows that first quarter earnings were lower for Native Americans than they were for either of the other two groups.

Looking first at the outcomes of the control cohort, Native Americans without training earned, on average, only $3,984 during the first quarter after exiting SDLOS.

Table 5.5: Training effectiveness across demographic groups: Effect of training on short-run quarterly earnings. (FP)

	Native Am.		White		Other	
Employment Probability	Mean	SD	Mean	SD	Mean	SD
Control	3984	(249)	4366	(65)	5837	(269)
OST	4583	(295)	4631	(80)	6040	(380)
OJT	5277	(528)	4832	(181)	6019	(688)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	598**	(283)	265***	(88)	203	(391)
OJT vs. Control	1292**	(528)	466**	(185)	182	(723)
Observations	618		5356		348	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The average first quarter earnings for the White and Other control cohorts were $4,366 and $5,837, respectively.

Occupational Skills Training.

Both the absolute and relative impacts of OST were larger for the Native American group than the other demographic groups. The average for first quarter earnings for Native Americans is $4,583, only slightly less than the earnings for similarly trained whites ($4,631). The ATE of occupational skills training for the Native American group is $598, meaning 15% higher relative quarterly income for trainees.

White job seekers also benefited from occupational skills training, but to a lesser degree than Native Americans. For whites, the estimated average for first quarter earnings is $4,631. The ATE of occupational skills training in the first quarter is $265, representing only a 6% relative increase in quarterly income.

OST did not significantly affect incomes for the Other group. So OST was most effective at boosting the immediate earnings of Native Americans, with OST producing the largest absolute and relative treatment effects for this group.

On-the-job Training.

The first quarter effects of OJT were much larger than those of OST, for Native Americans at least. Average first quarter earnings of Native American OJT trainees were $5,277, with the average treatment effect being $1,292. Thus, Native American OJT trainees earned 32.4% (1,292/3,984) more than did their untrained counterparts.

Average quarterly earnings for the White OJT cohort were actually *less* than those of the Native American OJT cohort. The average first-quarter earnings for the White group is $4,832, with the short-run ATE estimate for OJT being $466. These income gains represent only a 6.1% (466/4,366) boost in first quarter earnings relative to the control cohort.

As was the case with OST, OJT did not significantly affect incomes for the Other group. Another similarity, the short-run effects of OJT were largest for Native Americans. The effects of OJT were also much larger than those of OST, meaning that OJT was more effective than OST at increasing incomes for Native Americans.

5.3.2 Racial and Ethnic Comparison: Longer-run Estimates.

Table 5.6 (below) reports the estimated third quarter income estimates for each demographic group. When comparing the results of Table 5.6 with those of Table 5.5, I found especially large income gains for white job seekers. The third quarter incomes of whites rose in all three training cohorts.

The White group's control cohort income rose by 16.3% $((5,076 - 4,366)/4,366)$ from the first to the third quarters. The white OST cohort saw its quarterly earnings rise by 26.3% $((5,850 - 4,631)/4,631)$. And, finally, the White group with on-the-job training experienced an 8.5% $((5,244 - 4,832)/4,832)$ increase in its quarterly earnings over time. No other group of job seekers experienced such large first-to-third quarter

Table 5.6: Training effectiveness across demographic groups: Effect of training on longer-run quarterly earnings. (FP)

	Native Am.		White		Other	
Employment Probability	Mean	SD	Mean	SD	Mean	SD
Control	3265	(258)	5076	(158)	5267	(366)
OST	4099	(308)	5850	(168)	5751	(457)
OJT	4314	(532)	5244	(274)	5803	(816)
Avg. Treatment Effects	ATE	SD	ATE	SD	ATE	SD
OST vs. Control	834***	(282)	774***	(124)	484	(450)
OJT vs. Control	1049**	(527)	168	(250)	536	(826)
Observations	618		5356		348	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

income gains.

Comparing the short and longer-run estimates also provides additional confirmation that the effects of OST grew with time, while the effects of OJT diminished. I discuss these results below.

Occupational Skills Training.

Focusing first on the Other group, training had no statistically significant impact on earnings. In contrast, OST did lead to large income gains for the White and Native American groups. Third quarter average earnings for the white OST cohort were $5,850, a substantial $774, or 15.2% (774/5,076), more than the $5,076 average earnings of the white control cohort.

Native Americans actually benefited more from OST in the longer run than did the White or Other groups. The average third quarter earnings of the Native American OST cohort were $4,099, which was $834 more than the control cohort. According to the average treatment effect, OST increased Native American earnings by 25.5% (834/3,265). Once again, the ATE estimates increased from the first to third quarters, continuing the trend that OST increased in potency over time.

On-the-job Training.

On-the-job training had less impact on the longer-run earnings of job seekers than short-run earnings. While Table 5.5 shows that OJT was effective at increasing the short-run earnings of whites, Table 5.6 indicates that OJT did not affect the third quarter earnings of whites. Further, OJT did not affect the third quarter earnings for the Other group.

Native Americans, in contrast, benefited greatly from OJT. The estimated mean third quarter earnings for the Native American OJT cohort were $\$4,314$, which was $\$1,049$, or 32.1% $(1,049/3,265)$, more than the estimated average earnings for the control cohort. Comparing Tables 5.5 and 5.6, third quarter earnings for the Native American OJT cohort were down sharply from first quarter levels. Although the average treatment effect estimate for OJT remained significant in the longer run, the magnitude of the effect diminished.

5.3.3 Racial and Ethnic Comparison: Summary and Context.

What do the above results imply about training programs and outcomes for Native Americans? Overall, Native Americans had worse employment outcomes than either of the White or Other groups. After exiting SDLOS, Native Americans had both lower earnings and lower employment rates than did the White or Other groups. Moreover, Native Americans experienced larger income and employment declines from the first to the third quarters than did the White or Other groups.

Focusing on the effects of training, Native Americans did gain more from training than did either the White or Other groups. Both OST and OJT had large and significant effects on the short- and longer-run earnings of Native Americans. OST increased the short-run earnings of Native Americans by 15%, compared to 6% for the White group and no effect at all for the Other group. Moreover, OST increased the longer-run earnings of Native Americans by 25.5%, but only increased the third quarter earnings of the White group by 15.2%; once again, OST had no effect on earnings for the Other group.

Finally, OJT was most effective for Native Americans. OJT had no effect, in either the short or longer run, on earnings for the Other group; OJT increased the short-run earnings for the White group by 6%, but the longer-run effect was not statistically significant. For Native Americans, in contrast, OJT proved very effective. OJT increased their short-run earnings by 32.4%, and boosted their longer-run earnings by 32.1%.

5.4 Regional Variation in Program Effectiveness: Does Geography Matter?

Having explored the affects of training on individuals, I turn again to a regional assessment of training. In this section I explore the influence of training on earnings across the geographic regions of South Dakota.

5.4.1 Regional Variation: Short-run Estimates.

Table 5.7 provides insights into the effects of WIA training across South Dakota. These estimates indicate that first quarter earnings were generally highest in the Eastern region and in the Sioux Falls MSA, the most populous regions of the state. The lowest quarterly earnings were in the mostly rural Western region.

Table 5.7: Regional Variation: Effect of training programs on short-run quarterly earnings. (FP)

Mean Earnings	East Mean	Central Mean	West Mean	SF Mean	RC Mean
Control	5293	4502	3670	4700	4473
OST	5751	4400	5025	4912	5590
OJT	5657	4453	2841	5401	4995
Avg. Treatment Effects	ATE	ATE	ATE	ATE	ATE
OST vs. Control	457^{***}	–101	1355^{**}	212	1117^{***}
OJT vs. Control	363	–48	–828	700^{**}	521
Observations	3278	307	303	1547	887

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Occupational Skills Training.

With the focus on geography, the short-run OST earnings effects were quite varied. OST proved particularly effective for the Western region and for the Rapid City MSA. The average treatment effect of OST is $\$1,355$ for the Western region, increasing first quarter earnings by 36.9% $(1,355/3,670)$. Similarly, for the Rapid City MSA, OST increased the short-run earnings of trainees by $\$1,117$, or 24.8% $(1,117/4,473)$. OST was also effective at increasing earnings in the Eastern region, where it increased first quarter earnings by $457, or 8.6% $(457/5,293)$.

OST was not effective at boosting earnings for the Central region or for the Sioux Falls MSA. Given OST's ability to increase employment across all regions of South Dakota, it is, perhaps, puzzling that OST did not increase earnings for each of South Dakota's regions. Regional differences in OST effectiveness might be a consequence of the different types of jobs for which participants were training in different regions of the state.

For the Western and Eastern regions, along with Rapid City, a larger relative fraction of OST was directed towards well paying occupations, like health care, or production. For the Central region and the Sioux Falls MSA, in contrast, a larger relative fraction of OST was devoted to less well paying occupations, such as office and administrative support, and transportation and materials moving.[1] Thus the regional earnings effects could be a consequence of the relative mix of occupations for which OST trainees directed their training.

On-the-job Training.

OJT proved particularly ineffective in the regional context. The results in Table 5.7 indicate that OJT was effective only in the Sioux Falls MSA, where the OJT earnings effect was $700 in the first quarter. This isolated earnings effect could have stemmed from the fact that the Sioux Falls MSA had relatively more OJT placements in management and science related occupations than did the other South Dakota regions.

5.4.2 Regional Variation: Longer-run Estimates.

The longer-run earnings results, reported in Table 5.8, are similar to the short-run results presented in Table 5.7. From the perspective of earnings, OST again proved to be the more effective program: OST effectively boosted earnings in several South Dakota regions while the OJT effects lacked significance across all of South Dakota's regions.

Occupational Skills Training.

Table 5.8 shows OST had large and significant impacts on the earnings of persons in the Rapid City MSA, as well as those in the Eastern and Western regions. The largest effect is for the Western region, where quarterly earnings for the OST cohort

[1] These regional differences are documented in Appendix E.

Table 5.8: Regional Variation: Effect of training programs on longer-run quarterly earnings. (FP)

Mean Earnings	East Mean	Central Mean	West Mean	SF Mean	RC Mean
Control	4981	4635	4475	5457	4106
OST	5770	4367	7119	5689	5411
OJT	5219	4716	2394	5503	4645
Avg. Treatment Effects	ATE	ATE	ATE	ATE	ATE
OST vs. Control	789***	–268	2644**	231	1304***
OJT vs. Control	238	81	–2080	45	538
Observations	3278	307	303	1547	887

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

increased from $5,025 in the first quarter to $7,119 in the third quarter. The third quarter ATE estimate for the Western region is $2,644, a 59% (2,644/4,475) premium over the earnings of the control cohort. According to the average treatment effect, OST increased relative incomes for the Rapid City MSA by 32.7% (1,304/4,106), and by 15.8% (789/4,981) for the Eastern region.

The ATE estimates increased from the first to the third quarters for both the Eastern and Western regions, as well as for Rapid City. Thus, the regional results of Tables 5.7 and 5.8 confirm the trend of increased OST effectiveness over time.

On-the-job Training.

OJT had no statistically significant effects in the third quarter, meaning that OST, rather than OJT, led to longer-term *relative* income gains for trainees. From a regional point of view, OJT had no appreciable effects on the longer-run earnings ability of trainees.

5.4.3 Regional Variation: Summary and Context.

What conclusions can be drawn from the regional results presented above? OJT was not particularly effective in any of South Dakota's regions. I did observe a short-run OJT income effect for Sioux Falls, but the effect was not significant in the longer run. In light of these findings, the regional OJT results reinforce the theme that the earnings effects of OJT were transitory.

The regional OST results reinforced a repeated trend that OST effectiveness grew over time. OST was effective for the Eastern and Western regions, along with Rapid City, and OST effectiveness grew from the first to the third quarters — by 8.6% for the Eastern region, by 32.7% for the Western region, and by 15.8% for the Rapid City MSA. The regions experiencing significant OST earnings effects had a larger relative portion of their OST training directed towards higher paying occupations than did the regions without significant OST income effects. Thus, the effectiveness observed for OST could have been due in part to the types of occupations for which persons trained.

5.5 Training Before and After the Great Recession: Do Earnings Respond in Periods of Higher Unemployment?

Having explored the ability of WIA training to increase the earnings of different individuals and across different regions, I now evaluate the effectiveness of training across different stages of the business cycle. The previous chapter reported that training was less effective at supporting employment in the period following the Great Recession than beforehand. Both OST and OJT boosted employment during the period of lower unemployment, but only OJT supported employment during the period of higher unemployment. Did the income effects of training follow a similar pattern?

To explore the income effects of training over the business cycle, I again broke the sample into two groups. The first group exited SDLOS prior to Q4 2008 and the second group exited after Q4 2008. I then compared the observed real earnings (constant 2004 dollars) of these two groups to evaluate the performance of WIA training over the business cycle.

5.5.1 Training Before and After the Great Recession: Short-run Estimates.

Table 5.9 (below) presents the initial short-run earnings estimates for the pre- and post-Q4 2008 periods. The table shows that both OST and OJT were effective in the short run. Moreover, both OST and OJT were effective during periods of lower and higher unemployment.

Table 5.9 shows that, in the absence of training, the typical job seeker earned $\$3,625$ in her first quarter after exiting SDLOS. After Q4 2008, the average for first quarter earnings for the control cohort is $\$4,104$. Thus, average first quarter earnings for the control cohort were 13.2% $((4,104-3,625)/3,625)$ *higher* during the post-recession period than during the pre-recession period.

The short-run earnings of the OST and OJT cohorts were also higher in the post-recession period. The average first quarter earnings of the OST cohort were 15.1% $((4,649-4,039)/4,039)$ higher after Q4 2008 than they were prior to Q4 2008. The OJT cohort did even better. Its average first quarter earnings were 17.6% $((5,099-4,336)/4,336)$ higher after Q4 2008 than they were earlier.

Table 5.9: Training in periods of high unemployment: Effect of programs on short-run quarterly earnings. (SP)

	Pre Q4 2008		Post Q4 2008	
Employment Probability	Mean	Std. Dev.	Mean	Std. Dev.
Control	3625	(62)	4104	(116)
OST	4039	(104)	4649	(153)
OJT	4336	(208)	5099	(362)
Avg. Treatment Effects	ATE	Std. Dev.	ATE	Std. Dev.
OST vs. Control	414***	(119)	544***	(164)
OJT vs. Control	710***	(215)	994***	(331)
Observations	3795		2527	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Sample divided based on exit data of October 1, 2008.

Occupational Skills Training.

Occupational skills training had significant short-run impacts on the earnings of trainees in both time periods. Prior to Q4 2008, the typical OST trainee earned $4,039 in his first quarter after exit from SDLOS. The average treatment effect of OST was $414, meaning he earned 11.4% (414/3,625) more than the typical member of the control cohort during that period.

After Q4 2008, the short-run ATE of occupational skills training increased to $544, with the typical OST trainee earning $4,649 in her first quarter after exit. Thus, OST was associated with a 13.2% (544/4,104) earnings premium during the post-recession period. So, OST was slightly more effective in boosting earnings during the higher unemployment period than during the period of lower unemployment.

On-the-job Training.

On-the-job training was even more effective than OST, both prior to and after Q4 2008. Prior to Q4 2008, average first quarter earnings for the OJT cohort were

$4,336 (see Table 5.9). The average treatment effect of OJT is $710, representing a 19.6% (710/3,625) premium over the average first quarter earnings of the control cohort.

After Q4 2008, average first quarter earnings of the OJT cohort were $5,099, resulting in an average treatment effect of $994. At 24.2% (994/4,104), the relative OJT premium was larger after Q4 2008 than it was beforehand. Nevertheless, the immediate earnings response to OJT could not be maintained in the longer run.

5.5.2 Training Before and After the Great Recession: Longer-run Estimates.

Comparing Tables 5.9 and 5.10 with the employment results of the previous chapter demonstrates that earnings and employment effects evolved similarly over the business cycle. Two findings are noteworthy. First, focusing on post-Q4 2008, the quarterly earnings for both the OST and OJT cohorts fell from the first to the third quarters, but the quarterly income for the control cohort remained stable. The same pattern was observed in the previous chapter regarding employment rates for the OST, OJT, and control cohorts.

Second, prior to Q4 2008 the effects of OST increased with time, but post-Q4 2008 the effects of OST diminished; moreover, the OJT average treatment effect estimate is no longer significant. The employment results of the last chapter indicated that training was less effective during the period of higher unemployment. The income results of this chapter are consistent with the employment findings.

Occupational Skills Training.

Beginning with the pre-Q4 2008 period, Tables 5.9 and 5.10 demonstrate that

Table 5.10: Training in periods of high unemployment: Effect of programs on longer-run quarterly earnings. (SP)

	Pre Q4 2008		Post Q4 2008	
Employment Probability	Mean	Std. Dev.	Mean	Std. Dev.
Control	3431	(62)	4103	(91)
OST	4192	(131)	4432	(128)
OJT	4244	(225)	3892	(226)
Avg. Treatment Effects	ATE	Std. Dev.	ATE	Std. Dev.
OST vs. Control	761***	(142)	329**	(151)
OJT vs. Control	813***	(231)	–210	(241)
Observations	3795		2527	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Sample divided based on exit data of October 1, 2008.

the benefits of OST increased from the first to the third quarters. The average OST enrollee earned $4,192 in the third quarter following exit from SDLOS, up 4% $((4,192-4,039)/4,039)$ from the first quarter. The relative gains also grew. The third quarter average treatment effect of OST is $761, which is 83.8% ((761 – 414)/414) higher than for the first quarter.

OST remained potent in the post-Q4 2008 period, but it produced smaller absolute and relative income gains for trainees. The average for third quarter earnings of the OST cohort is $4,432, down 4.6% from $4,649 for the first quarter. The average treatment effect of OST also shrinks, falling 40% from $549 for the first quarter to $329 for the third quarter. OST was, therefore, substantially less effective in boosting earnings after the Great Recession than beforehand.

On-the-job Training.

The previous chapter showed that OJT generally supported the longer-run employment of trainees. These effects were visible both before and after Q4 2008. In terms of earnings, however, Table 5.10 shows that OJT had nothing significant to offer during the period of unusually high unemployment.

Looking first to the period prior to Q4 2008, average third quarter earnings for the OJT cohort were $\$4,244$, down slightly from $\$4,336$ for first quarter earnings. At the same time, however, the average treatment effect of OJT actually grows \$113, from \$710 for the first quarter to \$813 for the third quarter, an increase of 16%.

For the period after Q4 2008, the longer-run results are very different. During this period, the average third quarter earnings for the OJT cohort were $\$3,892$, *down* $\$1,207$ from $\$5,099$ in the first quarter. The third quarter OJT average treatment effect is no different from zero.

5.5.3 Training Before and After the Great Recession: Summary and Context.

The earnings results of this section reinforce some previously observed trends. First, OST had stronger relative impacts on the earnings of trainees than did OJT. Whatever OJT effects were observed in the short run, they tended to vanish over the longer run. In contrast, the effects of OST were large and statistically significant in periods of both lower and higher unemployment. Second, the earnings results demonstrated again that training was less effective during the period of unusually high unemployment than beforehand.

5.6 Summary of Income Effects.

In this chapter I have undertaken a systematic investigation of the income effects of OST and OJT. To put the various findings into perspective, I summarize the various findings.

First, the income results are more subtle than the employment results reported in the last chapter. In general, OST was more effective at increasing the incomes of trainees than was OJT. More often than not, the income effects of OST were larger, both relatively and absolutely, than the income effects of OJT. More importantly, the income effects of OST increased over time. In contrast, the earnings effects of OJT dissipated over time. OST resulted in longer-term income gains in a way that OJT did not.

Second, as in the last chapter, females benefited more from training than did males. The relative income gains due to OST and OJT were larger for women than for men. Females also experienced larger absolute gains than males from OJT in the short run, and from both OST and OJT in the longer-run.

Third, training was once again more effective for the non-dislocated jobless than for dislocated workers. Both OST and OJT led to large and significant first quarter income gains for the non-dislocated jobless. Neither OST nor OJT had any significant impact on first quarter income for dislocated workers. OST had significant effects on third quarter incomes for both the non-dislocated jobless and dislocated workers, but the effects were larger for the non-dislocated jobless. OJT had no impact on the third quarter incomes of either type of job seeker.

Fourth, Native Americans experienced larger absolute and relative income gains from training than did any other demographic group. The income effects of OST and OJT were both large and highly significant for Native Americans in both the first and third quarters following exit from SDLOS. The effects of OJT were larger than those of OST for Native Americans, indicating that, for Native Americans at least, OJT was more effective than OST.

Fifth, the regional income effects were less dramatic than the regional employment effects. OST, as opposed to OJT, was the more effective program across urban and rural regions, but OST was not effective all across the state. It seems possible that some of the regional disparities in the effectiveness of OST and OJT could have been tied to differences in the occupations for which individuals trained in the different regions.

Finally, while training didn't boost employment after the Great Recession, training did effectively increase incomes in the post-recession period. The first and third quarter income effects of OST were significant after Q4 2008. The first quarter income effects of OJT were significant post-Q4 2008, but the third quarter OJT effects were not significant. So while OST, and OJT in the short run only, did produce income gains post-Q4 2008, neither OST nor OJT boosted employment in the wake of the Great Recession. As a result, training was not unambiguously an effective policy tool for combating cyclical unemployment.

Chapter 6

Summary and Conclusions

In this dissertation I have presented new evidence regarding the effectiveness of worker training programs in the United States. I have provided the first comprehensive evaluation of public worker training programs since the passage of the 1998 Workforce Investment Act (WIA). Central to this effort was the South Dakota administrative data set, which reported extensively on WIA training in South Dakota during the years 2002–11. Using these unusually detailed data, I estimated the average treatment effects of WIA training programs on post-training employment rates and earnings.

Unlike any publicly available data set, the administrative data used in this study provided detailed micro-level records on persons before, during, and after accessing employment services provided by the State of South Dakota. The data were collected directly by the State of South Dakota for the purpose of internal evaluation, thus were well suited for the current analysis.

Using the novel administrative data set, I have improved upon prior training evaluations in several distinct ways. First, I uncovered relevant results regarding the effectiveness of current WIA training programs.

Second, this dissertation is the first to analyze the effects of training across various worker types and geographic regions. No prior study has simultaneously estimated the effectiveness of training for: dislocated workers versus the non-dislocated jobless, Native Americans versus other demographic groups, and job seekers in rural versus urban areas.

Finally, the South Dakota administrative data contain ten years of cross-sections. With such an extensive history I was able to provide new and important insights into the effectiveness of training over the business cycle.

6.1 Summary of Results

The many results presented in this dissertation can be quickly summarized by stating that training was effective at increasing both the employment rates and incomes of participants. I found that Occupational Skills Training (OST) and On-the-job Training (OJT) effectively increased employment rates and quarterly earnings across many of the sub-populations, regions, and time periods evaluated here. But before providing a detailed summary of the results, I offer a general overview.

First, the effectiveness of OST tended to grow as time passed, but the effectiveness of OJT tended to fade over time. Three calendar quarters after leaving training, the effects of OST were generally higher than they were after only one quarter. In contrast, three calendar quarters after training, OJT treatment effects tended to be smaller than after one quarter.

Second, OST had small impacts on employment but disproportionately large impacts on earnings. The employment effects of OJT were typically 2 to 3 times larger than the employment effects of OST. But when considering income effects, the average treatment effects of OST were often larger than those of OJT.

Finally, OJT had larger impacts on employment rates than on incomes. The employment effects of OJT were typically large and significant in both the short and longer run, but the same was not true of OJT's income effects. The longer-run income effects of OJT were typically smaller in magnitude than the income effects of OST, and were often no different from zero. Having characterized the findings in general, I present a more detailed summary below.

Baseline Results.

I began my evaluation by looking at the effects of WIA training in very general terms. These baseline results showed that both OST and OJT positively affected short- and longer-run employment rates and earnings, but that OJT was generally more effective that OST.

The findings for males and females showed that training was generally more effective for men in the short run, but more effective for women in the longer term. The first quarter employment effects of both OST and OJT were larger for men than for women. But by the third quarter the trend was reversed: both training programs had larger employment effects for women than for men.

Female incomes also responded more strongly to treatment than did male incomes. To illustrate, OJT increased first and third quarter female earnings by 34.3% and 48.4% respectively. For males, OJT increased first quarter earnings by 14.2%, but had no effect on third quarter earnings.

Dislocated Workers Versus the Non-dislocated Jobless.

A great deal of time and training is directed towards improving the labor-market outcomes of displaced workers. I found strong evidence that such preferential treatment did not translate into larger treatment effects for dislocated workers. My estimates indicated that training actually had larger absolute and relative effects for the non-dislocated jobless than for dislocated workers. The short- and longer-run employment effects of OST and OJT were roughly twice as large for the non-dislocated jobless than they were for dislocated workers. Further, both OST and OJT significantly increased the first quarter earnings of the non-dislocated jobless, but neither had any significant effects on the short-run earnings of displaced workers.

In the longer-run, the effects of OST were significant for both types of workers, but the income effects for the non-dislocated jobless were twice as large as those for dislocated workers. OJT had no effect on the longer-run earnings of either dislocated workers or the non-dislocated jobless.

Based on these results, the implication is clear: WIA training programs were more effective for the non-dislocated jobless than for displaced workers. Regardless of the resources directed towards dislocated workers, training was most effective for the non-dislocated jobless.

It seems that the circumstances surrounding an individual's job loss greatly influenced his or her future employment prospects. Dislocated workers could point to an external cause for their joblessness, like a layoff or business closing. Thus, their current unemployment was not a negative signal to prospective employers, enabling them to find employment more readily than the non-dislocated jobless. The non-dislocated jobless, in contrast, used training to demonstrate their employability. As a result, the marginal impact of training was higher for the non-dislocated jobless than for dislocated workers.

Racial and Ethnic Disparities in Program Effectiveness.

In recent years the US Government has earmarked additional funding for WIA training programs with the intent of improving the labor-market outcomes of Native Americans. Such funding was likely effective, for I reported that Native Americans gained more from training than did any other demographic group.

Specifically, the relative and absolute effects of training were greatest for Native Americans compared to other demographic groups. Both OST and OJT led to sizable gains in employment and earnings for Native Americans. Moreover, unlike other cases, OJT proved to have larger short- and longer-run effects than did OST. For Native Americans at least, training on the job proved more effective than schooling.

Regional Variation in Program Effectiveness.

Regional differences across South Dakota might have had an influence on the effectiveness of training. The results presented here showed that training was generally effective across all regions, both urban and rural.

Specifically, for both OST and OJT I found strong and sustained employment effects across all regions of South Dakota. Once again, the effects of OJT were roughly three times those of OST. The employment effects were statistically significant for both the first and third quarters following training, and were slightly larger in rural areas than in urban areas. In light of these findings, it seems that both OST and OJT effectively increased employment in both rural and urban areas.

The regional income effects of training were more complex. OST proved to be effective at increasing incomes across several regions, both in the short and longer run. Interestingly, regional income effects may have been due in part to the regional mix of occupations for which persons trained. For example, I observed large and significant OST earnings effects in regions where a relatively large fraction of OST was directed toward higher paying fields, like health care or production. OJT exhibited little influence on earnings for any of the regions of South Dakota.

Training Before and After the Great Recession.

Turning to a more macroeconomic perspective, I found that training was not as effective following the Great Recession as it had been beforehand. Prior to the recession, both OST and OJT exhibited significant- and longer-run employment and earnings effects. After the recession, the beneficial effects of training were less obvious. OST had no effect on employment after the recession, but it did have positive and significant effects on short- and longer-run earnings. Similarly, OJT exhibited large and significant first and third quarter employment effects prior to the recession, but had little impact on earnings after the recession.

Evidently training programs were less effective when unemployment was unusually high. According to the South Dakota administrative data, training may be an effective tool, but not a countercyclical tool. It appears that training is effective in overcoming long-run problems such as skills-mismatch or the decline of manufacturing. But it also appears that training is not particularly effective in countering the cyclical effects of a strong recessionary downturn.

6.2 Conclusions and Policy Recommendations

Based on the many findings summarized above, what policy recommendations can this study offer? To begin, policy makers must consider short-run versus longer-run impacts of training programs. This study found the effects of OST were typically small in the period immediately following training but grew over time. In contrast, the effects of OJT were more immediate but dissipated quickly.

OST lends itself to the type of training that can lead to a new career, rather than simply a new job. While the effects of OST may appear small early on, it has the potential to pay significant dividends in the future.

OJT, on the other hand, is a rapid response program designed to put people back to work as quickly as possible. Participants are unlikely to develop entirely new skills and abilities that could fundamentally alter their long-run labor-market prospects. Rather, OJT offers an opportunity whereby semi-qualified job seekers can become qualified and competent workers. Thus, OJT provides immediate benefits to trainees, but the potential for longer run impacts are smaller than those of OST.

South Dakota, like most states, has implicitly chosen to emphasize the long run by preferring OST to OJT, mainly due to difficulties in finding willing private sector partners for OJT. As a result, South Dakota has chosen to sacrifice short-run employment gains for longer-run income gains. Ultimately, policy makers must identify the goals they wish to achieve before deciding whether to emphasize either OST or OJT. If the goal is to support employment, OJT is the proper tool, but if the goal is to boost incomes, then OST may be the preferred program.

Second, training programs are not particularly effective anti-recession tools. WIA training programs were highly effective before the Great Recession, when both OST and OJT supported positive outcomes for trainees. In the wake of the Great Recession, however, training was largely ineffective. Even OJT performed poorly during the period of high unemployment. Policy makers should not expect worker training programs to return labor markets back to health during or after recessionary periods.

A final policy conclusion of this dissertation, WIA programs should be more targeted towards the non-dislocated jobless rather than dislocated workers. Dislocated workers in South Dakota benefited far less from training than did the non-dislocated jobless. The experience of South Dakota indicates that dislocated workers readily found work even without WIA training. The results of this analysis suggest that funds spent on dislocated workers might be more effective if spent elsewhere.

6.3 Future Research

The results of this dissertation have opened up new avenues for future research. One such avenue is an exploration into the relative benefits of training in one occupation over another one. The regional findings in this dissertation hinted that training in certain occupations might be more beneficial than training in others. While such analysis was beyond the scope of this investigation, the current data set may facilitate further investigation into these potential occupation effects.

Future research is also likely to continue contrasting the outcomes of dislocated workers and the non-dislocated jobless. It could be that training is more effective for dislocated workers in areas of the country that have experienced more severe worker dislocations than has South Dakota. As no suitable data are publicly available, such investigations will necessarily require developing relationships with policy makers and program administrators in other states in order to gain access to data similar those I obtained from South Dakota.

Finally, this dissertation has evaluated the effectiveness of training programs at increasing employment and incomes, but has not evaluated the cost effectiveness of training. A cost-benefit analysis of WIA training in South Dakota would require detailed information on expenditures for OST and OJT programs, which has been outside the realm of this study. In the future, I expect to secure access to program expenditure data for a sub-sample of the program years evaluated here. Once data are obtained, the cost-benefit analysis will be able to access the cost effectiveness of WIA training.

Appendix A

Local Office Locations

The State of South Dakota staffs eighteen Local Offices across the state. Each office is responsible for overseeing programs in its area. The top panel of Table A.1 (below) provides a list of the cities and counties of official SDLOS locations. The bottom panel of Table A.1 provides names and locations of third party centers approved by the South Dakota WIB to provide core, intensive, and training services. The Star Academy is a juvenile detention facility and primarily works with youths. No persons in the estimation sample received services through the Star Academy.

Table A.1: Official and Unofficial Local Office Locations

Region	Location	County
Central	Pierre	Hughes
	Winner	Tripp
East	Mitchell	Sanborn
	Watertown	Codington
	Huron	Beadle
	Yankton	Yankton
	Madison	Brookings
	Aberdeen	Brown
	Sioux Falls	Yankton
	Brookings	Brookings
	Vermillion	Plymouth
Rapid City	Rapid City	Meade
reservation	Pine Ridge	Shannon
West	Spearfish	Lawrence
	Hot Springs	Fall River
Location	**Center Name**	**County**
Rapid City	Career Learning Center Of The Black Hills	Pennington
Custer	Star Academy	Charles Mix
Sioux Falls	Volunteers Of America	Minnehaha

Appendix B

Regional Data Summary

B.1 Regional Designations and Included Counties

South Dakota counties in the "Eastern"

- Beadle, Bon Homme, Brookings, Brown, Clark, Clay, Codington, Davison, Day, Deuel, Grant, Hamlin, Hanson, Hutchinson, Kingsbury, Lake, Marshall, Miner, Moody, Sanborn, Spink, Union, Yankton

South Dakota counties in the "Central"

- Aurora, Brule, Campbell, Douglas, Edmunds, Faulk, Gregory, Hand, Hughes, Hyde, Jerauld, Mcpherson, Potter, Sully, Tripp, Walworth

South Dakota counties in the "Western"

- Butte, Custer, Fall River, Haakon, Harding, Jones, Lawrence, Mellette, Perkins, Stanley

South Dakota MSA counties

- Sioux Falls (Lincoln, McCook, Minnehaha, Turner); Rapid City (Meade, Pennington)

Nebraska and Iowa counties included in the Eastern region

- Nebraska (Cedar, Dakota, Dixon, Knox); Iowa (Lyon, Monana, Plymouth, Woodbury)

B.2 Historical South Dakota Labor-Market and Population Data

Table B.1: Historical Unemployment Rates by Region.

	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
State Wide	3.20	3.50	3.70	3.80	3.10	2.80	3.10	4.90	5.00	4.70
East	3.68	4.01	4.05	4.07	3.40	3.18	3.37	5.16	5.10	4.79
Central	3.38	3.67	4.03	4.43	3.81	3.44	3.46	4.59	4.64	4.62
West	3.96	4.34	4.68	5.06	4.18	4.00	4.13	5.53	5.95	6.26
Sioux Falls	2.72	3.05	3.38	3.40	2.85	2.63	3.03	4.97	4.60	4.15
Rapid City	2.95	3.35	3.55	3.70	3.10	2.75	3.00	4.95	5.25	4.85

Regional averages are not population weighted. Source: BLS Local Area Unemployment Statistics.

Table B.2: Historical Population: in Thousands

	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
State Wide	760	764	770	775	783	792	799	807	816	824
East	470	469	468	465	466	467	468	470	479	481
Central	82	81	80	80	79	78	78	78	79	80
West	86	87	88	88	88	89	88	89	90	91
Sioux Falls	198	203	209	214	221	227	232	237	229	232
Rapid City	115	115	117	118	119	120	122	124	127	128

Regional averages are not population weighted. Source: BLS Local Area Unemployment Statistics.

Table B.3: Historical Employment Growth by Region.

	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
State Wide	0.01	0.01	0.01	0.02	0.01	0.01	–0.02	–0.00	0.01	0.01
East	0.68	0.05	0.09	0.86	0.40	0.34	–2.18	–0.10	0.31	0.83
Central	1.59	–0.85	–2.07	–0.58	–2.10	0.83	0.24	1.53	–0.91	0.24
West	2.10	–0.77	–0.91	1.63	–3.15	0.27	0.12	1.39	–1.70	–0.44
Sioux Falls	1.37	1.73	0.86	2.19	–0.73	0.34	–3.84	4.70	1.27	1.32
Rapid City	1.08	1.07	0.09	0.72	0.19	0.13	–3.09	1.23	1.34	0.52

Regional averages are not population weighted. Source: BLS Local Area Unemployment Statistics.

Appendix C

Testing the Identification Assumptions

Identification of the average treatment effect of training is only possible given the conditional independence and overlap assumptions. If these assumptions are invalid then the estimated treatment effects do not provide consistent estimates of the true impact of program participation. Testing these assumptions is, therefore, crucial in order to establish the validity of this study's results.

C.1 Testing the Conditional Independence Assumption

The central assumption necessary for identification of treatment effects is the conditional independence assumption given in Equation (3.3). There is no direct test of conditional independence, but indirect tests are possible. Imbens (2004) and Imbens and Wooldridge (2009) recommend testing the statistical relationship between training programs and employment outcomes prior to treatment. Following their recommendation, I estimated the effects of training on employment status and quarterly earnings in the first and second full calendar quarters prior to registration with SDLOS.

The results of this test provide evidence that the conditional independence assumption was not violated. Table C.1 shows that there was no significant relationship between either OST or OJT and employment status in the first and second quarters prior to registering with SDLOS. The estimated mean employment rates for the first and second prior quarters differ by no more than two and half percentage points across the three training cohorts, and the differences are not significant.

Table C.1: Testing conditional independence. Effect of training on prior employment status.

	1 Qtr Prior		2 Qtr Prior	
Employment Probability	Mean	Std. Dev.	Mean	Std. Dev.
Control	0.470	(0.008)	0.719	(0.007)
OST	0.448	(0.012)	0.717	(0.011)
OJT	0.469	(0.040)	0.742	(0.032)
Avg. Treatment Effects	ATE	Std. Dev.	ATE	Std. Dev.
OST vs. Control	–0.022	(0.014)	–0.002	(0.013)
OJT vs. Control	–0.001	(0.041)	0.022	(0.033)
Observations	6322		6322	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The SDLOS data also report earnings for two quarters prior to registration with SDLOS. Table C.2 shows the results of the second CIA test. The results are not as straight forward as the employment results, but, when viewed in conjunction with the employment results, indicate that the CI assumption was not violated.

Table C.2 reports that there were no significant OJT earnings effects in the first or second quarters prior to registration with SDLOS. I did find significant OST earnings effects, in contrast. The average treatment effects of OST were negative and significant in the first and second quarters prior to registration, but the effect was smaller in magnitude in the second quarter than in the first. This trend indicates that any fundamental differences between the OST and control cohorts were likely transitory. I therefore conclude that selection bias is not driving my results and that the Conditional Independence Assumption is not violated.

Table C.2: Testing conditional independence. Effect of training on prior earnings.

	1 Qtr Prior		2 Qtr Prior	
Employment Probability	Mean	Std. Dev.	Mean	Std. Dev.
Control	2599	(76)	4164	(67)
OST	2310	(77)	3992	(79)
OJT	3074	(399)	4437	(317)
Avg. Treatment Effects	ATE	Std. Dev.	ATE	Std. Dev.
OST vs. Control	-289^{***}	(100)	-172^{*}	(93)
OJT vs. Control	474	(403)	272	(319)
Observations	6322		6322	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Overlap Test: Estimated Propensity Scores

Conditional probability of assignment to control group

	Mean	Std. Dev.	p(5)	p(50)	p(95)
$\mathrm{P}(D = 0 \mid D = 0)$	0.609	(0.200)	0.298	0.609	0.921
$\mathrm{P}(D = 0 \mid D = 1)$	0.587	(0.208)	0.263	0.587	0.911
$\mathrm{P}(D = 0 \mid D = 2)$	0.602	(0.176)	0.328	0.602	0.877

Conditional probability of assignment to OST group

	Mean	Std. Dev.	p(5)	p(50)	p(95)
$\mathrm{P}(D = 1 \mid D = 0)$	0.382	(0.203)	0.066	0.382	0.699
$\mathrm{P}(D = 1 \mid D = 1)$	0.397	(0.213)	0.065	0.397	0.729
$\mathrm{P}(D = 1 \mid D = 2)$	0.356	(0.177)	0.081	0.356	0.632

Conditional probability of assignment to OJT group

	Mean	Std. Dev.	p(5)	p(50)	p(95)
$\mathrm{P}(D = 2 \mid D = 0)$	0.133	(0.078)	0.012	0.133	0.255
$\mathrm{P}(D = 2 \mid D = 1)$	0.131	(0.076)	0.012	0.131	0.249
$\mathrm{P}(D = 2 \mid D = 2)$	0.139	(0.080)	0.015	0.139	0.264

C.2 Testing the Overlap Assumption

Defined in Equation (3.6), the overlap assumption requires that no person was either guaranteed or excluded from treatment. In order to test this assumption, I estimated the likelihood that person i was in treatment state j using a multinomial logistic regression following Equation (3.13). This process is detailed in Chapter 3.3.1 where I explained the estimation of the propensity score weights.

Using the predicted probabilities from the multinomial logistic regression I calculated the conditional propensity scores $P(D_{jk})$, which provided the probability of assignment to treatment state j given assignment to state k:

$$P(D_{jk}) = P(D = j|D = k) \quad \text{for } j = 0, 1, 2; \ k = 0, 1, 2.$$

Summary statistics for the conditional propensity scores are presented in Table C.3. The distributions of propensity scores are highly similar and exhibit a great deal of overlap across all treatment groups, indicating that the overlap assumption holds within the data.

I also present a visual test of the overlap assumption in Figure C.1. I used a kernel density estimator and the raw propensity scores summarized in Table C.3 to produce smoothed density functions for the conditional treatment probabilities.

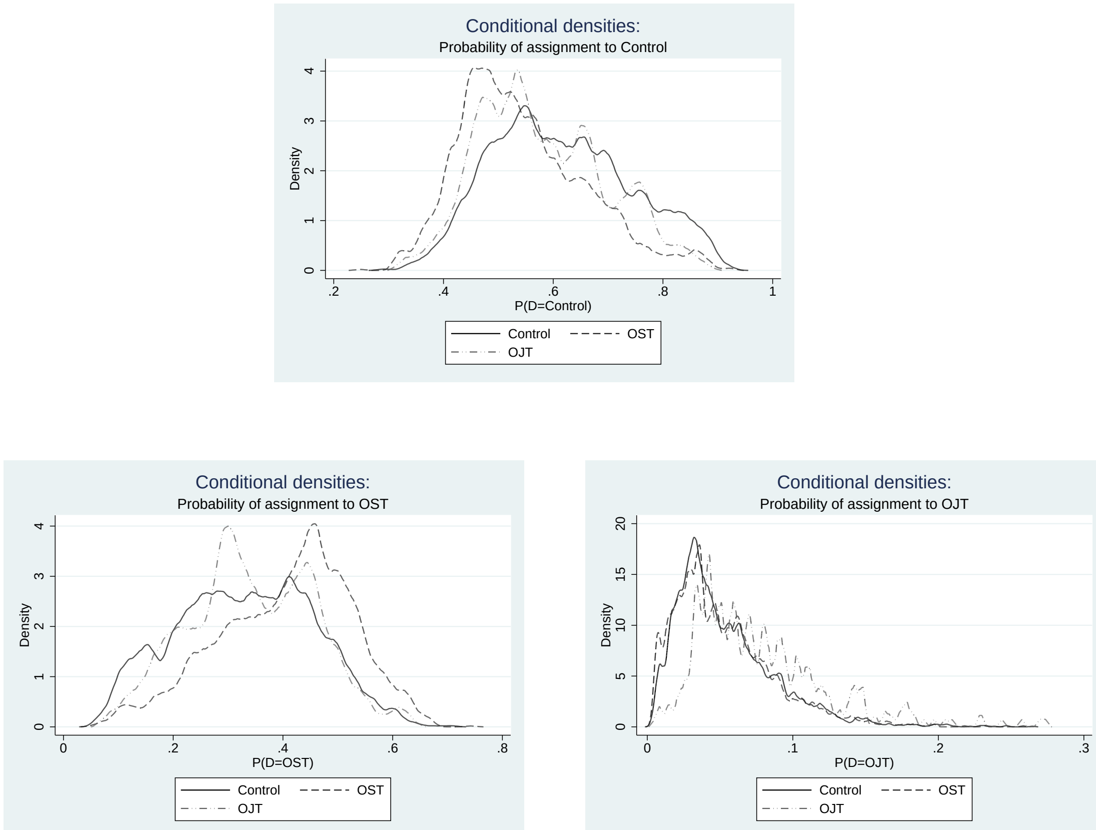

Figure C.1: Overlap Test: Graphical summary of propensity scores

Following (Cattaneo et al., 2013), I used a triangle kernel when constructing the smoothed density functions. I used a bandwidth of $h = .032$ for the control and OST states, but used a bandwidth of $h = .004$ for the OJT state. The distribution of conditional assignments probabilities for OJT was highly skewed and a smaller bandwidth was necessary to prevent over smoothing near zero (Cattaneo et al., 2013).

The visual test reconfirms a high degree of overlap between the various conditional treatment probabilities. For the Control and OST groups the propensity scores are well behaved without significant mass near either zero or one. However, due to the small number of OJT training events in the sample, some conditional OJT assignment probabilities lie near zero. These are fringe cases tough and the vast majority of conditional treatment probabilities are well behaved. Figure C.1, therefore, provides additional evidence that the overlap assumption is not violated within the SDLOS data.

Appendix D

Technical Appendix

D.1 Treatment Stage Model Selection

This study follows Cattaneo et al. (2013) in its implementation of the Cattaneo (2010) EIF method. This technical appendix details the model selection process used for the flexible parametric estimation of the EIF moment conditions. I use the Stata command `bfit` to estimate and rank multiple model specifications. Table D.1 provides a full list of potential regressors and short descriptions. The omitted education category is high school graduate, and the omitted region is the eastern region.

Table D.1: Treatment stage variable descriptions

`wia_dislocated`	Dislocated worker
`male`	Male
`native`	Native American
`nonwnat`	Neither white nor Native American
`sngleprnt`	Single parent (self-reported)
`taa`	Trade Adjustment Assistance
`lowincome`	Low-Income (income below federal poverty line or LLISL)
`offender`	Criminal Record (self-reported misdemeanor or felony)
`lths`	No high school diploma or equivalent
`ged`	GED certificate
`assoc`	Associate's degree
`bach`	Bachelor's degree
`reg20**`	Year of registration with SD LOS
`regctrl1`	Regional control - Sioux Falls
`regctrl2`	Regional control - Rapid City
`regctrl3`	Regional control - Central region
`regctrl4`	Regional control - Western region
`startage`	Age are registration with SD LOS
`startage2`	Squared age at registration

The code excerpt below depicts the estimation command used to fit and rank the various treatment selection specifications.

```
*   Defining variables for model selection. The TREATMENT global variable
*   contains the potential regressors for specifying the propensity scores.

global treatment ///
        male native nonwnat sngleprnt taa lowincome lths ged assoc bach  ///
        reg2004 reg2005 reg2006 reg2007 reg2008 reg2009 reg2010 reg2011  ///
        regctrl1 regctrl2 regctrl3 regctrl4 startage startage2           ///
        wia_dislocated

*   Treatment Stage used in all models
bfit logit trained2 $treatment , corder(1) base(0) sort(aic)
    qui mlogit trained2 r(bvlist)
    disp e(cmdline)
```

The output presented on the following page is created by the preceding commands. All possible models are estimated and ranked according to the AIC. The model specification that minimizes the AIC is selected for estimation. Its covariates are captured and displayed.

```
*       Treatment State

bfit logit results sorted by aic
-----------------------------------------------------------------------------
    Model |     Obs     ll(null)    ll(model)      df          AIC          BIC
----------+------------------------------------------------------------------
 _bfit_24 |    6322    -5301.421    -4984.945      50     10069.89     10407.48
 _bfit_23 |    6322    -5301.421    -4987.443      48     10070.89     10394.97
 _bfit_93 |    6322    -5301.421    -4897.664     138     10071.33     11003.08
 _bfit_71 |    6322    -5301.421    -4984.676      52     10073.35     10424.45

                             [ ...  Intentionally Omitted   ...]

  _bfit_2 |    6322    -5301.421    -5273.503       6     10559.01     10599.52
 _bfit_47 |    6322    -5301.421    -5272.362       8     10560.72     10614.74
  _bfit_1 |    6322    -5301.421    -5299.167       4     10606.33     10633.34
 _bfit_46 |    6322    -5301.421    -5298.066       6     10608.13     10648.64
-----------------------------------------------------------------------------

.          qui mlogit trained2 r(bvlist)

.          disp e(cmdline)

mlogit trained2 i.(wia_dislocated male native nonwnat sngleprnt taa
                lowincome lths ged assoc bach reg2004 reg2005 reg2006
                reg2007 reg2008 reg2009 reg2010 reg2011 regctrl1
                regctrl2 regctrl3 regctrl4 offender) c.(startage)
```

D.2 Employment Stage Model Selection

In order to determine the proper specification for the outcome equation I follow a similar procedure to the one described above. The set of potential covariates for this stage is a superset of the potential treatment stage covariates. I include several other potential regressors in addition to those detailed above in Table D.1: `tanf` (Temporary Assistance for Needy Families), `urate` (unemployment rate in county of residence), and `urate2` (squared unemployment rate).

While selection into treatment is only measured at one point in time, I observe individual employment outcomes at two points after a person exits the LOS system. It is therefore necessary to estimate two outcome specifications; one for the employment status in the first quarter and one for the employment status in the third quarter following exit. It is possible to impose a single specification for both time periods. Doing so does not change the findings of this paper. In fact, the above procedure returns the same specification for both the first and third quarter earnings regressions.

The following code excerpt depicts the estimation commands used to fit and rank the various employment status specifications. The same method is used for the quarterly earnings specifications except a linear regression is used. In the logistic regressions below the dependent variable is the binary employment status in the first quarter (`q1emp`) and in the third quarter (`q3emp`) after exit.

```
*   Defining model selection variables. The OUTCOME global variable contains
*   the potential regressors for estimating the bias correction term.

global outcome ///
        male native nonwnat sngleprnt taa lowincome lths ged assoc bach ///
        reg2004 reg2005 reg2006 reg2007 reg2008 reg2009 reg2010 reg2011 ///
        regctrl1 regctrl2 regctrl3 regctrl4 startage startage2          ///
        wia_dislocated tanf urate urate2

*   Q1 Employment
bfit logit q1emp $outcome , corder(1) base(0) sort(aic)
    qui logit q1emp r(bvlist)
    disp e(cmdline)

*   Q3 Employment
bfit logit q3emp $outcome , corder(1) base(0) sort(aic)
    qui logit q3emp r(bvlist)
    disp e(cmdline)
```

The following code excerpt demonstrates the output created by the preceding commands. As shown, the potential models are estimated and then ranked according to the AIC. In total, two-hundred and four models are estimated. Note the slight difference between the two outcome specifications. The Q3 employment specification includes two regional controls not found in the Q1 specification.

```
*       Q1 Employment

bfit logit results sorted by aic
---------------------------------------------------------------------------
    Model |     Obs    ll(null)   ll(model)     df          AIC         BIC
----------+----------------------------------------------------------------
 _bfit_177 |    6322   -3013.311   -2859.246     28     5774.493    5963.543
 _bfit_179 |    6322   -3013.311   -2857.663     30     5775.326     5977.88
 _bfit_176 |    6322   -3013.311   -2860.823     27     5775.645    5957.943
 _bfit_175 |    6322   -3013.311   -2861.922     26     5775.843     5951.39

                             [ ...  Intentionally Omitted   ...]

 _bfit_2   |    6322   -3013.311   -3011.145      3      6028.29    6048.545
 _bfit_53  |    6322   -3013.311   -3010.661      4     6029.322    6056.329
 _bfit_27  |    6322   -3013.311   -3010.949      4     6029.898    6056.906
 _bfit_78  |    6322   -3013.311   -3010.492      6     6032.984    6073.495
---------------------------------------------------------------------------

.          qui logit q1emp r(bvlist)

.          disp e(cmdline)
logit q1emp i.(male native nonwnat sngleprnt taa tanf veteran offender
            lths ged assoc bach wia_dislocated reg2004 reg2005 reg2006
            reg2007 reg2008 reg2009 reg2010 reg2011 regctrl1 regctrl2)
            c.(urate urate2 startage startage2)
```

```
*       Q3 Employment

bfit logit results sorted by aic
----------------------------------------------------------------------------
     Model |     Obs    ll(null)   ll(model)     df          AIC         BIC
-----------+----------------------------------------------------------------
 _bfit_179 |    6322   -3247.086    -3043.12     30     6146.241    6348.794
 _bfit_128 |    6322   -3247.086   -3046.724     29     6151.449    6347.251
 _bfit_175 |    6322   -3247.086   -3051.827     26     6155.654      6331.2
 _bfit_178 |    6322   -3247.086   -3049.591     29     6157.183    6352.985

                              [ ...  Intentionally Omitted   ...]

 _bfit_103 |    6322   -3247.086   -3237.171      4     6482.342    6509.349
  _bfit_27 |    6322   -3247.086    -3239.09      4      6486.18    6513.187
   _bfit_2 |    6322   -3247.086   -3240.813      3     6487.626    6507.881
   _bfit_1 |    6322   -3247.086   -3243.785      2     6491.571    6505.074
----------------------------------------------------------------------------

.          qui logit q3emp r(bvlist)

.          disp e(cmdline)
logit q3emp i.(male native nonwnat sngleprnt taa tanf veteran offender
            lths ged assoc bach wia_displaced reg2004 reg2005 reg2006
            reg2007 reg2008 reg2009 reg2010 reg2011 regctrl1 regctrl2
            regctrl3 regctrl4) c.(urate urate2 startage startage2)
```

Appendix E

Specialization in Regional Training

As referenced in Chapter 6, certain regions of South Dakota tended to favor training in certain occupations. Below I present tables detailing the relative number of training episodes in each occupation category. Columns may not sum to one due to rounding.

Table E.1: Regional training preferences for OST

	East Mean	Central Mean	West Mean	RC Mean	SF Mean
Management	0.05	0.03	0.04	0.07	0.13
Business and Financial Operations	0.02	0.04	–	0.02	0.04
Sciences, Computer, and Mathematical [1]	0.04	0.06	0.05	0.03	0.09
Architecture and Engineering	0.02	–	0.04	0.04	0.02
Community and Social Service	0.01	–	–	0.01	–
Legal	–	0.01	–	0.02	–
Education, Training, and Library	0.02	–	0.01	0.01	0.01
Arts, Design, Entertainment, Sports, and Media	0.01	–	–	–	–
Healthcare Practitioners and Technical	0.08	0.06	0.13	0.15	0.10
Healthcare Support	0.07	0.07	0.23	0.17	0.16
Protective Service	–	–	0.03	0.01	0.01
Service: Food or Personal Care [2]	–	0.01	0.01	–	–
Building and Grounds Cleaning and Maintenance	–	0.02	–	–	–
Sales and Related	0.02	0.01	–	–	–
Office and Administrative Support	0.27	0.36	0.15	0.27	0.14
Construction and Extraction	0.04	0.02	0.03	0.01	0.04
Installation, Maintenance, and Repair	0.03	0.02	0.02	0.03	0.02
Production	0.15	0.08	0.01	0.04	0.07
Transportation and Material Moving	0.17	0.20	0.28	0.12	0.16
Observations	964	98	109	393	517

[1] Combines categories: Computer and Mathematical with Life, Physical, and Social Sciences
[2] Combines categories: Food Preparation and Service with Personal Care Services

Table E.2: Regional training preferences for OJT

	East Mean	Central Mean	West Mean	RC Mean	SF Mean
Management	–	0.03	0.05	–	0.07
Business and Financial Operations	0.01	–	–	–	0.01
Sciences, Computer, and Mathematical [1]	0.01	–	–	–	0.04
Architecture and Engineering	0.04	0.03	–	0.03	0.03
Community and Social Service	0.01	–	–	–	–
Legal	0.01	–	–	–	–
Education, Training, and Library	0.01	–	–	–	–
Arts, Design, Entertainment, Sports, and Media	0.01	–	–	–	0.01
Healthcare Practitioners and Technical	0.01	–	–	–	–
Healthcare Support	0.06	0.03	–	0.09	0.01
Protective Service	0.01	–	–	–	0.01
Service: Food or Personal Care [2]	0.03	–	–	–	0.01
Building and Grounds Cleaning and Maintenance	0.02	–	–	0.03	0.01
Sales and Related	0.04	0.06	0.05	0.03	0.06
Office and Administrative Support	0.13	0.09	0.43	0.28	0.14
Construction and Extraction	0.06	0.19	0.19	0.19	0.12
Installation, Maintenance, and Repair	0.12	0.25	0.05	0.13	0.08
Production	0.41	0.22	0.24	0.19	0.31
Transportation and Material Moving	0.04	0.09	–	0.03	0.07
Observations	156	32	21	32	97

[1] Combines categories: Computer and Mathematical with Life, Physical, and Social Sciences
[2] Combines categories: Food Preparation and Service with Personal Care Services

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