September

2021

Net Impact Study for the Training Benefits Program 2002 through 2016 – technical report

Employment Security Department
WASHINGTON STATE

Net Impact Study for the Training Benefits Program 2002 through 2016 – technical report

Published September 2021

Washington State Employment Security Department
Cami Feek, *commissioner*

Labor Market and Economic Analysis
Steven Ross, *director of labor market information*

This program performance report was prepared in accordance with the Revised Code of Washington, section 50.22.157.

Prepared by:

Jose Antonio Hernandez C., *statistical program research specialist*
Daegoon Lee, *operations research specialist*
Matthew Klein, *operations research specialist*

Contents

Executive summary

The goal of this study is to analyze the net impact of the unemployment insurance (UI) training benefits (TB) program on participants' earnings, employment, and training achievements. The program targets dislocated workers, veterans, members of the National Guard, low-income individuals and the disabled. It is intended to assist participants as they train for high-demand work and enhance their marketable skills.

To conduct our study, we grouped TB participants into 15 yearly cohorts beginning in 2002 and ending in 2016. This allowed us to evaluate program outcomes for different economic events such as the Great Recession of 2008 to 2009 and the expansionary period of 2010 to 2016.

To measure the program's effects, we compared TB participants to groups of UI claimants who did not participate in the program but were statistically similar. We refer to these groups as a comparison or control group throughout the report. We used Employment Security Department (ESD) administrative data, and data from the Washington Education Research and Data Center (ERDC) to assess earnings, time employed, and enrollment in training programs.

During the first few years, TB participants are far more likely than their peers to seek training at training institutions. During this training period, they are less likely than their peers to work, and forego roughly one year of earnings. After this training period, the TB participants are somewhat more likely than their peers to be employed, but do not earn more than their peers. As such, the average net impact of the program is an overall loss of earnings.

However, there are two groups of people that tend to benefit from the TB Program: (1) younger people and (2) poorer people. The average net impact of the TB Program for younger people and for poorer people is an increase in lifetime earnings. We also find that young TB Program participants who enroll in healthcare, education, mechanics, transportation, or physics classes tend to benefit more from the program than young TB participants who enroll in other classes.

Methods

We followed a two-pronged approach to obtain plausibly-unbiased estimates of program effects. First, we created suitable comparison groups using three matching methods.

1. Propensity score matching (PSM).
2. Mahalanobis distance matching (MDM).
3. Coarsened exact matching (CEM).

Each of these methods selects UI claimants that did not enroll into the TB Program but were statistically similar to TB participants, creating a pool of control group subjects. By using three matching methods, we create three samples to compare the TB participants to. We present results using the PSM comparison group in the main body of the report and present the results from the other two methods in appendices. Results were generally the same with all three matching methods.

After creating suitable comparison groups, we used difference-in-differences (DID) regression models to obtain intent-to-treat (ITT) effects on earnings, percent of time employed, and probability of attempting training course credits. ITT estimates give the effects of the program on outcomes for *all* participants, irrespective of whether they trained or not after being admitted into the TB Program. Roughly 85 percent of TB participants do end up training and claiming additional UI compensation.

As a robustness check, we repeated this two-pronged approach using an alternative control group. For this alternative group, we restrict our attention to individuals that applied to the program but were rejected. We fit a difference-in-differences model that compares TB participants' outcomes to outcomes of this alternative control group. We found qualitatively similar results using this alternative control group.

To study program effects for younger people and poorer people specifically, we augment the DID model that uses the PSM comparison group. In this augmented model, we interact the treatment variable with observable characteristics like gender, age, and industry. This model lets us calculate ITT estimates that are specific to each demographic group.

To study the correlation between the TB Program's net impact and the course of study that individuals' chose, we first predict the net impact of the TB Program using the DID augmented model for all TB participants. Then regress the course of study on these predictions. This allows us to assess which courses of study are correlated with positive TB Program net impacts.

Key findings

TB Program participants experience initial decreases, then increases, in the likelihood of being employed

Typically, TB participants enroll in training and remain unemployed for two to three years while they are in training. Therefore, the program has a large negative "lock-in" effect on percent of time employed during the first three years after enrollment. After five years, program participants in the 2002 to 2005 cohorts are more likely to be employed than those who did not participate. Overall, participants in these early cohorts enjoy an increase in the likelihood that they are employed because of their program participation.

For the 2006 to 2016 cohorts, program participation has mixed effects on employment. For instance, the 2006 cohort had positive effects in follow-on years four to seven,[1] the 2007 cohort had a positive effect in follow-on years eight to 11, and the 2008 cohort did not have an increase in employment probability at all. Overall, the results for these later cohorts are somewhat positive starting in follow-on year four. The average net effect of the program, however, is that TB participants in these cohorts tend to spend less time employed than their peers in the follow-on years we observe. The modest increase in the likelihood of employment after follow-on year four does not offset the large initial reduction in the likelihood of employment.

The average TB program participants experience large initial decreases, then small increases, in earnings

The TB participants forgo roughly one year of earnings in their lock-in period; the net impact of the program on earnings during the first three follow-on years is negative and statistically significant. Program participants tend to earn about $40 thousand less than people in the control group in the first three years after entering the program.

Most study cohorts eventually catch up to the same level of annual earnings as the control group. After five years, the typical cohort has no statistically significant differences between TB participant and control group earnings. As such, the net effect of the program on earnings is large and negative. The 2002 and 2003 cohorts were an exception.

[1] We call the year after treatment "follow-on year one," the second year after treatment "follow-on year two," and so on.

Young people and poorer people benefit from the TB Program

The average ITT estimates hide substantial heterogeneity of the program's effects on earnings across demographic groups. In particular, we find a significant negative correlation between the ITT estimates and age, and between the ITT estimates and earnings in the year prior to program participation. Young people and poorer people benefit more from participating in the TB Program. In fact, the average person under the age of 28 benefits from participating in the TB Program.

In all age groups, the poorer people in that group tend to benefit from the TB Program. People under the age of 36 who earned less than $40 thousand in the year before enrolling in the TB Program benefited from participating in the TB Program on average. More than half of the TB participants under the age of 36 earned less than this amount. People between the ages of 36 and 46 who earned less than $30 thousand in the year before enrolling in the TB Program benefited on average. People older than 46 who earned less than $17 thousand in the year before enrolling in the TB Program benefited from the TB Program on average.

TB Program participants show an increase in the probability of training after enrollment

Program participants are significantly more likely than members of the control group to attempt to earn training credits in the first three years of TB Program enrollment. TB participants had a 60 to 90 percent higher likelihood of training, depending on the cohort. Over the follow-on years, this difference between the TB participants and the control group decreases towards zero. It becomes statistically insignificant four years after program enrollment.

The 2002 and 2003 cohorts are atypical

The program has large and statistically significant ITT effects on earnings and percent of time employed for subjects in the 2002 and 2003 cohorts. In contrast to the 2004 to 2016 cohorts, a substantial number of people who enrolled into the program in 2002 and 2003 were male and came from the aerospace industry (approximately 45 percent of participants in these cohorts had jobs in this industry before being admitted into the program). These subjects, during their time in the program, likely trained in very technical or specialized training areas (e.g., aerospace engineering), which may have reinforced their already high marketable job skills. After leaving the program, these participants found well-paid jobs relatively quickly.

After 2004, the proportion of participants coming from the transportation manufacturing industry (which includes the aerospace sub-sector) is very low. The difference in results across cohorts can be partially explained by the differences in the composition of the cohorts. Almost half of the people in the early cohorts came from the aerospace industry, and the TB Program has a positive impact for these people. Roughly three percent of the later cohorts are comprised of individuals from this industry. As such, the average impacts are much larger for the early cohorts.

Chapter 1: Background

The TB Program was created in 2000, when the Washington State Legislature passed Substitute House Bill 3077 (SHB 3077). According to section six of this bill, the TB Program provides "unemployment insurance benefits to unemployed individuals who participate in training programs necessary for their reemployment." The TB Program is targeted to dislocated workers.[2]

For individuals who enroll full-time in approved training programs, the TB Program extends the number of weeks of unemployment insurance (UI) benefits by another 26 weeks. TB participants can receive up to 52 weeks of UI benefits, including 26 weeks of regular and 26 weeks of training benefits. Enrollees must exhaust regular and extended recessionary benefits before drawing training benefits. TB participants do not have to look for work to maintain UI and TB benefits if they are making satisfactory progress in approved coursework.[3]

Changes to the TB Program over time

The program, and characteristics of the program participants, changed over time. Most importantly, section 8 of the 2002 update to SHB 3077 made special provisions for workers from the following industries:

- Aerospace (NAICS 372 and 336411)
- Logging and timber (NAICS 24 or 26)
- Fishing (NAICS 0912)

Applicants from these industries were exempt from requirements designed to establish a "long-term attachment to the labor force." The aerospace industry was included in the legislation after a sizable layoff by Boeing, an aircraft production company then located in King County. Boeing experienced a large decrease in demand in 2001. A substantial number of enrollees in 2002 and 2003 came from the aerospace industry (46 and 40 percent respectively), likely because of the section 8 provisions and the large layoff by the major company in the industry. These participants were atypical of participants in other years. These two early cohorts tended to have a higher percentage of male participants and have participants with higher earnings than later cohorts.

Before April 2009, TB Program applicants had to develop individual training plans and, if accepted into the program, start full-time training within 90 days of notification of TB Program admittance. In 2009, the Washington State Legislature passed Engrossed Substitute House Bill 1906 (ESHB 1906), which increased the 90-day window to 120 days, and significantly expanded the program's target population. ESHB expanded eligibility to:

[2] Dislocated workers are those who have been terminated from employment and are "unlikely to return to employment in [their] principal occupation or previous industry because of a diminishing demand for their skills in that occupation or industry" (Revised Code of Washington - RCW 50.04.075). On an annual basis, ESD develops a list that identifies occupations that are "in demand," "balanced" and "not in demand" in each workforce development area (WDA). Local workforce development councils (WDCs) then review, adjust and approve that list according to their knowledge of local labor market conditions.

[3] See the Revised Code of Washington (RCW) section 50.20.043.

- Individuals whose hourly wage is less than 130 percent of the state's minimum wage and whose earning potential could go up with training (i.e., low-income individuals),
- U.S. military veterans,
- Active Washington State National Guard members, and
- Individuals with mental or physical disabilities.

The final significant change to the TB Program came in 2011, with the passage of Engrossed House Bill 1091 (EHB 1091). This bill removed the requirement for dislocated workers to enroll full-time in training programs. After the bill passed, they could also enroll in part-time training. The bill also required applicants to submit plans and enroll in approved training programs before the end of their unemployment insurance claim years (that is, enroll in a training program no more than 52 weeks after the approval of an application for UI benefits). TB adjudicators who were interviewed for this study said this last requirement resulted in a substantial number of claimants applying to the program just before the end of their claim years.

Literature review on training programs

Social scientists have conducted many evaluations of training programs in the United States. The most relevant of these for the present study is Aviles et al. (2015), which assess the Washington TB Program using similar methods to those we employ here. Aviles et al. (2015) found that all TB participants experienced an initial decrease in earnings while training. In the 2002 and 2003 cohorts, participants' earnings caught up to and surpassed the earnings of non-participating peers. The TB Program had a positive effect on long-term earnings for these cohorts. But this was not true for the other cohorts. Aviles et al. (2015) wrote that, in general, the "TB Program is not cost-effective." Studying the 2002 to 2012 cohorts, with follow-on data up to 2013, they found that the net present value of participating in the program was -$482. That is, compared to their peers, participants forego earnings. In addition, Aviles et al. (2015) found that program recipients in cohorts 2007 to 2012 never became more likely to be employed than their peers. These findings, unsurprisingly, mirror those reported in this study.

Analyses of comparable programs in the United States and in Germany yield corroborating results. Researchers typically find negative program effects for dislocated workers. Andersson et al. (2013) studied dislocated workers in two states, finding "persistently negative impacts in one state and initially negative and later marginally positive impacts in the other." They report that their results are in line with earlier studies by Heinrich et al. (2012) and Hollenbeck (2009).[4] In general, these researchers find the same "lock-in" pattern, where participants forgo earnings opportunities while training, and never regain their lost earnings in future work. In an analysis of a German training program, Doerr et al. (2017) documented similar results, writing

> "…after the award, [program] recipients experience long periods of lower labor market success compared to had they not received training... Small positive employment effects and no gains in earnings were observed four to seven years after the receipt of the [program]."

[4] See also Barnow and Smith (2015).

The main take away from the literature is that dislocated workers do not benefit from training programs in general. Others may benefit from these programs – see LaLonde (2003) for a review of programmatic effects for workers who have not recently lost a job and who are not about to lose a job – but dislocated workers typically do not.

A further comment pertains to the methods used in these studies. Almost all of them are similar in evaluation design to our analysis. They use administrative data, apply a matching technique to construct a comparison group, and then use some econometric estimation technique to compare outcomes for those who trained and those who did not. Our analysis is situated squarely in the center of the literature in terms of the methods we use, and the results we find.

Chapter 2: Methods

We analyze the TB Program's net impact for all 30,164 people who enrolled between January of 2002 and December of 2016. Our data run from January of 1999, three years before any TB participant that we study joined the program, to December of 2019, three years after the final cohort that we study joined the TB Program.[5]

Our primary analysis is the same as that of the 2015 report: create a comparison group using matching method, then apply a difference-in-differences model to compare the TB participants' outcomes to the comparison groups' outcomes. We used one-to-one propensity score nearest-neighbor matching without replacement to create a control group of UI claimants that are similar to TB participants on numerous observable characteristics like age and veteran status. This method helps us achieve the goal of matching: i.e., on average, the control group and the TB participants are similar. This allows us to compare the outcomes between the control group members and the TB participants (from here on out, we sometimes refer to the TB Program enrollees as the "treated group" or "treatment group"). If the outcomes in the control and treatment groups are statistically indistinguishable, we can conclude that the TB Program had no effect on participants' outcomes. Otherwise, we can conclude that the TB Program impacted participants' outcomes.

In this chapter, we first review the methods used in the 2015 study, describe an assessment of the quality of these methods, and discuss how we update the methods in this analysis in response to that assessment. Then, we describe the data we used. We follow this with a description of the matching methods and difference-in-differences models we used. We conclude the chapter with a short description of the dependent variables we studied: earnings, employment, and probability of taking courses (i.e., probability of training).

Comparing the 2015 and 2021 net impact studies

As required by [RCW 50.22.157](), the ESD published a report on the net impact of the TB Program in December 2015. In this study, the authors used a PSM method to construct a control group, then compared the outcomes for the TB participants and control group in each cohort using a difference-in-differences approach. They found that the TB Program had positive effects for the participants in the 2002-2003 cohorts, but negative long-run effects for participants in later cohorts. Washington State Joint Legislative Audit and Review Committee (JLARC) conducted a thorough review of that report and made the following recommendation:[6]

> "ESD should explore, in consultation with the State Board of Community and Technical Colleges and other relevant organizations, possible causes why the Program has not had more positive impacts on recent participants' employment and earnings. ESD suggests poor labor conditions during the recent recession may partly explain the lower earnings of some participants who entered the Program in later years. However, without additional years of data, it is unclear whether the influence of the recession explains the variation in performance between early and later years

[5] As our data ends in 2019, we are unable to assess the effects of the program during the 2020 pandemic and recession.

[6] JLARC's recommendation is available here: https://leg.wa.gov/jlarc/reports/2016/UnemploymentTrainingBenefits/f/default.htm#Recommendations (last accessed on November 18, 2020).

of participants. The Department should consider other variables such as whether differences between participants' and non-participants' occupations, industries, and employers may affect employment and earnings."

To address this recommendation, the 2021 report makes the following important updates to the 2015 analysis:

- Expands the time period of analysis.
- Uses additional econometric strategies to analyze data.
- Examines training data for participants enrolling into the program after 2004, thanks to a data sharing agreement with the Washington State Education Research and Data Center (ERDC).
- Compares outcomes for people coming from the aerospace industry to outcomes for everyone else, since the 2002 and 2003 cohorts had a disproportionately large percent of people coming from this industry.
- Pools multiple cohorts' data, in addition to conducting individual year-cohort analyses, to estimate an overall average treatment effect of the program over time.
- Compares outcomes for people of different ages and varying income levels.

Defining cohorts

In both the 2015 and 2021 analyses, we define the cohorts by the calendar year in which the individuals opened unemployment insurance claims. For example, all individuals who claimed UI benefits between January 1 and December 31 of 2007 belong to the 2007 cohort. Everyone we study, TB participants and comparison individuals alike, became an unemployment claimant in the same year. So, for the 2007 cohort, all TB participants filed their UI claim of interest between January 1 and December 31 of 2007, and all comparison group individuals filed their UI claim of interest between January 1 and December 31 of 2007. The comparisons we make for the purpose of this study are between UI claimants who participate in the TB Program and UI claimants who do not participate in the TB Program.

Time period covered

In both the 2015 and 2021 reports, everyone who opened UI claims in the same year was grouped into annual cohorts.[7] In both studies, data for each claimant was aggregated into four-quarter periods following the end of the quarter when the claims were opened. We call these "follow-on" years.

The 2015 study had 11 cohorts and included all participants who enrolled in the TB Program from the beginning of 2002 to the end of 2012. It had data from January of 1999 through December of 2013 covering both pre- and post-enrollment data. Five of the cohorts (2006 through 2010) were impacted by the Great Recession. The JLARC report review of the 2015 ESD report noted that additional research is required to know whether the results reported were caused by low program effectiveness or by poor macroeconomic conditions.

[7] Because only UI claimants can apply for the TB Program, the TB enrollment date is always after the date they open their UI claim. This sometimes makes the two dates, i.e., UI claim opening date and TB enrollment date, fall in different calendar years.

This study adds four cohorts of enrollees: those who entered the program between the beginning of 2013 and the end of 2016. In addition, we studied six additional follow-on years, analyzing information up to December 2019. The additional data allow us to disentangle the effects of the Great Recession from the TB Program's effects. If we observe similar outcomes among participants that joined the program *during* versus *after* the Great Recession, we cannot attribute the lower program effects detected in the 2015 report solely to the recession.

Updates to the econometric method

In this study, we augment and refine the 2015 econometric methods by:

- Using multiple matching techniques.
- Introducing a new comparison group.
- Introducing a new outcome measure.
- Adding Education Research and Data Center (ERDC) data.
- Measuring how the net impact of the TB Program on earnings changes with the participant's age and their earnings level in the year prior to the UI claim of interest.

A major improvement in this study is the use of three matching methods and a robustness check. This allows us to assess whether our results are artifacts of the statistical method used, or representative of the true effects of the TB Program. We find that the results are consistent across models, and so can be confident that the results we find are informative in learning the true program effects. When compared to subjects in any of the control groups, TB participants experience a reduction in earnings on average. They initially suffer a reduction in earnings while training. The hope is that this investment eventually leads to an increase in earnings potential, leading to increased life-time earnings. There is no evidence that this occurs for the typical TB Program participant. Instead, the initial costly investment never pays off. The repetition of this pattern across models gives us confidence that these results are not the outcome of econometric errors, but truly reflect the programs' net impact.

There are some exceptions to this finding. The 2002 and 2003 cohort benefit from the TB Program on average. Also, in all cohorts, young people tend to benefit from the program. In all cohorts, poorer people of all ages tend to benefit from the program as well. Even while the average net impact of the TB Program is negative for the 2004 to 2016 cohorts, it is positive for *younger* and *poorer* people in these cohorts.

New comparison group

We created a fourth comparison group by studying UI claimants who applied to the TB Program but were denied by program adjudicators. A comparison of the average outcomes for this control group and our treatment group can be interpreted as the causal effect of the TB Program under the assumption that rejections are uncorrelated with the outcomes we study.

An advantage of using this comparison group is that denied TB Program applicants are likely similar to TB Program participants in an important unobserved characteristic, i.e., interests in training and education which may be tied to self-motivation. Our three statistical matching methods do not let us select a comparison group with *verifiably* similar unobserved characteristics; we must assume that unobserved characteristics in the comparison group are similar to those in the treatment group. As such, this denied group provider a particularly nice addition to the three statistical matching methods we use. It is the comparison group for which the unobservable characteristics are most likely to be statistically similar to the TB participants' unobservable characteristics (though the observed characteristics may not be similar). In any case, also assessing the results with this comparison group gives us qualitatively different information about whether our results are robust, nicely complementing our statistical methods.

New outcome measure

In this study, we add a new outcome measure: likelihood of training after enrollment. This helps us understand whether program participants are incentivized to train by the TB Program.

Data used in this study

We used ESD data augmented with training information from the ERDC. These data include:

- Quarterly earnings.
- Age.
- Gender.
- Veteran status.
- Disability status.
- Low income status.
- Last occupation before claiming UI benefits.
- Workforce development area where the individual opened their UI claim.
- Ethnicity.
- Education.
- Indicators for whether the individual got denied enrollment into the TB Program.
- Indicator for whether the participant experienced a loss of earnings before filing for UI benefits (called the "Ashenfelter dip").
- Training credits attempted by the individual.
- Program of enrollment at a training institution.
- Industry and sub-sector of employment (available before and after an unemployment spell.

The quarterly earnings variable is the sum of the following earnings types paid by employers who are covered by the Washington state UI program:[8]

- Salary.
- Commissions.
- Bonuses.
- Value of gifts before deductions.
- Compensation paid in lieu of cash.
- Tips reported for Federal income tax purposes.
- Vacation and holiday pay.
- Unsegregated expense allowances.
- Severance pay.
- Employees' entire gross pay if they share the cost of a 401(K) or cafeteria plan through salary reduction.
- Meals and lodging if the employer requires an employee to eat and live onsite and the total value of meals and lodging is 25 percent or more of total compensation.

[8] The variable does not include any of the following: sick leave, allocated tips, jury duty pay not reported for federal income tax purposes, death benefits, and employee exercised stock options. See Employment Security Department (2020, 9).

ERDC data

Following JLARC's recommendation, ESD established a data sharing agreement with Washington State's Office of Financial Management (OFM) Education Research and Data Center (ERDC). The ERDC provided data for 2004 to 2016 cohorts, including all follow-on years. They also provided data for the 2002 and 2003 cohorts, starting in 2004 (i.e., missing follow-on years zero and one for the 2002 cohort, and follow-on year zero for the 2003 cohort). The data include an extensive list of training variables. As a result, we have information on training for multiple years for most of the individuals we studied.

The number of course credits participants attempt to earn through training and higher education is a key variable in our analysis. We are interested in whether:

- Participants enter training programs.
- What type of training participants seek.

We use the Classification of Instructional Programs (CIP) codes associated with courses of study to understand what kind of classes TB participants enroll in.

Econometric strategy part one: constructing comparison groups

All matching methods have four key steps (Stuart 2010, 5-13):

1. *Defining closeness*. The analyst uses a distance measure to determine how similar each TB participant is to each non-participant. The primary difference between the matching methods is in how to aggregate a lot of information about individuals into a single measure of similarity.
2. *Implementing the matching method*. The analyst uses a predetermined rule to match individuals that are similar to each other. One rule can be, for example, selecting the "nearest-neighbor." Using this approach, the analyst pairs the most similar TB participant and non-participant.
3. *Assessing the quality of the matched samples*. The analyst studies whether the comparison group is statistically similar to the treatment group on observable characteristics.
4. *Analysis of the outcome and estimation of the treatment effect*. The analyst uses regression analyses to compare the outcomes for the TB participants and the matched comparison group.

In this report we use the PSM strategy for our primary analysis and use additional matching strategies to assess the robustness of our results to the matching method used. The first robustness check uses a one-to-one Mahalanobis nearest-neighbor matching (MDM) method. The second uses a coarsened exact matching (CEM) method. We discuss the econometrics of propensity score matching here. For a detailed description of the Mahalanobis matching method, see Rubin (1980) and Stuart (2010). For a detailed description of the coarsened exact matching method, see Iacus et al (2012).

In the body of the text, we present difference-in-differences results that use the control group constructed using PSM. These estimates tend to be more conservative – that is, less likely to reject the null hypothesis. This choice is benign since results are similar across models.

Propensity score matching

The propensity score matching method defines the distance between participant i and non-participant j as follows:

$$D_{ij} = |e_i - e_j|$$

where e_i is the propensity score for individual *i* and e_j is the score for person *j*. A propensity score is, in the case of this report, the predicted probability that subject *i* will enroll into the TB Program, irrespective of whether that individual entered training. This prediction is based on observable characteristics like age, education, and occupation. If two people are identical on all the observable characteristics used in the probability model, then those two people are given the same propensity score. The distance between them is zero.

We use a logistic regression model to calculate the probability that individuals enroll in the TB Program. The dependent variable is binary – equal to one when the person is a TB participant, and zero otherwise. We included the following independent variables in the logistic regression:

- Age and age squared. We include the age and the squared age of all individuals at the date of the UI claim we use to define cohort membership.
- Indicator for whether the person experienced a dip in earnings in the year before claiming UI benefits (i.e., Ashenfelter dip). We set this variable equal to one when the individual earned more in the fifth quarter prior to the UI claim that determines their cohort membership than they earned in the second quarter prior to that UI claim.
- Size of the Ashenfelter dip in percent terms. This measures how much less the person earned in the second quarter prior to their UI claim that determines their cohort membership than in the fifth quarter prior to that claim.
- Occupation before the unemployment spell. We include 24 dummy variables for occupation prior to the UI claim that determines the individuals' cohort membership. We exclude the category "occupation unknown."
- Number of transitions from unemployment to employment in the three years prior to opening the UI claim that determines study cohort.
- Previous earnings for each of the 12 quarters before opening a UI claim. If they did not earn anything in a quarter, their earnings are listed as zero.
- Formal education level at the time of opening a UI claim. We include nine dummy variables that indicate an individual's formal educational status prior to the UI claim that determines their cohort membership.
- Workforce development area (WDA) where the individual claimed UI benefits. We include 14 dummy variables that describe an individuals' location in Washington state.
- Ethnicity. We use seven dummy variables that capture individuals' self-reported ethnicity.
- Veteran status. We use a dummy variable equal to one for individuals that served in the United States military.
- Low income status. We use a dummy equal to one when an individual is at, or below, the threshold established in Engrossed Substitute House Bill 1906 (ESHB 1906). The threshold is set at 130 percent of the state minimum wage rate in an individual's base year – that is, the year used to calculate the maximum unemployment benefit amount for each UI claimant. See Aviles et al (2015) for more details.

- Disability status.[9] We use a dummy variable equal to one for individuals that are classified as having a disability.

For each matching method, we consider only individuals of the same gender who claimed UI in the same calendar year. We fitted a logistic regression for each cohort. Then, using those fitted models, we calculated each individual's propensity score. Afterwards, we matched TB participants with non-participants by using a one-to-one nearest-neighbor matching method without replacement and without calipers. The strategy minimizes D_{ij}, the absolute distance between the propensity score of TB participant *i* with non-participant *j* in the same cohort.[10]

Mahalanobis distance matching

In PSM, the logistic regression aggregates lots of information about individuals into a single score, a unidimensional measure of differences between people. The Mahalanobis method chooses the closest TB participants and non-participants using the "Mahalanobis distance metric." This differs from the PSM approach in that it calculates a multidimensional distance between individuals' characteristics. The main limitation of this matching method is that it does not work well when the number of covariates used to match subjects is large (see Stuart 2010, 7).

Coarsened exact matching

The coarsened exact matching method has attractive properties. It is computationally efficient, capable of including many covariates, does not require imputation for missing data, and tends to improve the multivariate balance between treatment and control groups (Iacus et al, 2012). The main limitation of this method is that it tends to discard numerous subjects in the treatment group, decreasing the sample size available for analysis and constraining the type of estimate that can be obtained (King and Nielsen 2019, 3, n. 2). In this study, more than half of the TB participants in each cohort were dropped from the data after coarsened exact matching.

Analyzing the denied applicants

We also created a fourth control group by studying UI claimants that applied to the TB Program each year but whose application was rejected by program adjudicators. These subjects share many

---

9 We used the same independent variables for the coarsened exact matching method. For the Mahalanobis matching method we use a subset of these variables, based on the recommendation by Stuart (2010), who suggests that this specific matching technique works well with fewer than 10 covariates. These are age, veteran status, an indicator for whether they are a low-income individual prior to claiming, disability status, the Ashenfelter dip indicator variable, the Ashenfelter dip in percent terms, the median of earnings in twelfth through second quarters preceding the unemployment insurance claim, and the number of transitions from unemployment to employment in the three years prior to opening the UI claim that determines study cohort.

10 Though rare, analysts may find that two or more subjects in the comparison pool are equally distanced from a treated subject. The chance to have these ties increases as (1) the size of comparison pool increases and (2) the number of variables observations matched on decreases. For this study, we used R package "MatchIt" version 3.0.2 for matching and confirmed that when the search algorithm finds a tie, it randomly selects one subject to be in the control group. As such, the matching algorithm can slightly give in different results in each run (this is no longer the case with the updated version of the package MatchIt version 4). To examine the size of this impact, we reran the matching with four arbitrarily selected cohorts (2002, 2004, 2010, and 2013) and compared the matching and DID results. We confirmed that with PS matching, the difference generated by the random selection is very small. The largest change is still less than 0.2 percent of the total number of the matched subjects. In turn, the impact on the DID estimates is quantitatively negligible. It does not affect the inference we conduct or the results we report.

observable and unobservable traits (e.g., motivation to train) with TB participants.[11] The fact that all four methods yield results with roughly the same basic pattern give us confidence in the program effect estimates.

Matching methods for causal inference

The primary difficulty in measuring the TB Program effects is in constructing comparison groups that are truly similar to the control group. This is accomplished in an experimental setting because the randomized assignment ensures that whether someone receives treatment does not depend on their characteristics. Here, however, people choose to enroll in the TB Program. As such, whether someone receives treatment does depend on their characteristics. We can try to control for the differences between the *observed* characteristics, like age and gender, between the control and treatment groups in our study. This is precisely what the matching methods accomplish. However, we cannot be sure that the comparison groups we construct are similar along dimensions we do not observe. It is possible that, on average, people who enroll in the TB Program are different along an unobservable dimension, like motivation to train or interest in learning. These potential differences would bias our results if they are correlated with the decision to train *and with* earnings, or the likelihood of employment. By constructing four control groups from four different methods, we try to account for these potential differences between the comparison groups.[12]

Econometric strategy part two: comparing treatment and control groups

We apply a difference-in-differences (DID) econometric strategy to estimate the effect of the TB Program on participants' outcomes. The DID method compares outcomes for the treatment and control group. Since we have created four separate control groups, we fit four DID models for each outcome we study.

The DID method provides intent-to-treat (ITT) estimates, which measure the average effect of *participation in the TB Program* on outcomes, irrespective of the number of TB participants that trained.[13] Green and Gerber (2012, 139) write that ITT estimates are "commonly used to describe the effectiveness of a program when the main concern is the extent to which the program changed outcomes." They ask, "[r]egardless of whether the program treated a large or small proportion of its intended targets, did it change the average outcome?"

For each cohort, we fit a:

[11] Future research can focus on the denied group in greater detail. A more detailed analysis could compare people that appeal and fail (the control group), to people that appeal and are successful (a narrowing of focus for the treatment group). We can match on observables *within* this group of appealers to create a cleaner identification strategy. However, the external validity of this approach – the degree to which we can generalize the results to the general population – is very limited. As such, we leave such an analysis for future research.[12] The individual fixed effects we include in our regression analysis further control for unobservable, time-invariant cofounders that may bias our results.

[12] The individual fixed effects we include in our regression analysis further control for unobservable, time-invariant cofounders that may bias our results.

[13] About 85 percent of each cohort's TB participants enroll in training, while the rest are removed from the program when they do not enroll in training programs. Those who are removed, while they successfully applied for the program, are never given additional UI benefits.

1. DID model that compares the outcomes for TB participants and the control group constructed using **the propensity score matching method.**
2. DID model that compares the outcomes for TB participants and the control group constructed using **the Mahalonobis matching method**.
3. DID model that compares the outcomes for TB participants and the control group constructed using **the coarsened exact matching method**.
4. DID model that compares the outcomes for TB participants to outcomes for a control group comprising individuals who are denied from the program.

As such, we fit a total of 180 models to study the TB Program's average net impact. That is, we have four types of models for 15 cohorts studying 3 outcomes.

The DID model

This approach compares the difference in average trends in earnings and the likelihood of employment experienced by TB participants and the comparison group. The treatment effect is the difference in the trend after the program begins, conditional on the difference in trends before the program begins.[14] If we had only two periods of data (one year of data before and one year of data after the TB Program), our estimate of the effect of the TB Program would be the difference between the average earnings for participants and non-participants *before the program* subtracted from the difference between the average earnings for participants and non-participants *after the program* – a difference between two differences. With more than two follow-on years of data, we use regression analyses to construct an analogous estimate of the program effects.

To construct our ITT estimates, we use two different regression model specifications. We use all three years of data before each participant opens a UI claim, and all follow-on data, to estimate these models. The first model specification allows us to obtain an ITT estimate of the *yearly average effect* of the program on the outcome of interest. This estimate will tell us, for example, how much more earnings, in contrast to the control group, TB participants are making on average in each follow-on year. The specification for this model is:

$$Y_{i,t} = a_i + b_t + \delta D_{i,t} + \varepsilon_{i,t}$$

where Y_{it} is the outcome of interest for individual *i* at time *t*, a_i and b_t are individual and time fixed effects, respectively, and D_{it} is an indicator that equals 1 after follow-on year 0 if the individual *i* is a TB participant. That is, D_{it} is only equal to one for TB participants, and only after they enroll in the TB Program. The DID estimate is δ. Obtaining a δ of, for example, minus \$100 would tell us that, on average, TB participants earn \$100 less than people in the comparison group in each follow-on year.

---

[14] The DID methodology relies on an important assumption known as "parallel trends." The assumption is that either all unobserved factors are time-invariant at the group level or that all time-varying factors are group invariant (see Wing *et al.* 2018, 457). For this study, this means that there is no factor that influences outcomes for the TB group over time but does not affect the control group over time (and vice-versa). Basically, the parallel trends assumption refers to the idea that TB participants would have experienced the same change in outcomes as non-participants if they had not enrolled into the program. Our matching methods make this assumption more likely to hold.

The second specification allows us to estimate how the effect of the program changes over time. This specification provides individual ITT estimates for each follow-on year. This model allows us to assess whether the program effects decrease or increase over time. As we will show in future chapters, estimates obtained from this model specification show that the program has a negative impact on participants' earnings and time employed during the first three to four years after program enrollment. Then, participants tend to catch up to the earnings and employment levels of the control group five to six years after enrollment into the program. The specification is the following:

$$Y_{i,t} = a_i + b_t + \sum_{m=1}^{3} D_{i,t-m}\lambda_m + D_{i,t}\gamma + \sum_{s=1}^{S} D_{i,t+s}\delta_s + \varepsilon_{i,t}$$

where, as before, Y_{it} is the outcome of interest for individual *i* at time *t*, and a_i and b_t are individual and time fixed effects, respectively. In this specification, γ captures the effect of the program the year of enrollment (follow-on year 0). The λ_m coefficients evaluate whether TB participants and non-participants have similar trends in outcomes *before* entering the UI system (years $t-3$ through $t-1$) and the δ_s coefficients measure the impact of the program on participants' outcomes s periods after enrollment.

In both specifications, identification of δ comes largely from the individual fixed effects. These capture all characteristics that are particular to individuals and do not change over time. The identifying variation for the TB Program's net impact, δ, comes from changes over time in (1) program participation status, and (2) the outcome variable for each person separately. The net impact measure is each person's impact, averaged. For the control group, program participation status does not change, but their outcomes may change over time. As such, δ measures the net impact of program participation on outcomes, controlling for idiosyncratic drivers of changes over time in the outcome, and for broad trends over time that the control and treatment group both experience. By selecting the control group with a matching approach, we more plausibly capture trends that both groups experience over time, increasing the accuracy of the δ measurement.

In the DID models, the key assumption is that conditional on the individual-level fixed effects, the comparison group selected by the matching process forms a good "counterfactual" for the treatment group. That is, if TB participants had instead *not* participated, their outcomes would look similar to the outcomes of their peers in the control group. This is referred to as the "parallel trends" assumption. Matching aids in this process by making the treatment and control groups more similar on average prior to treatment. Because of the matching process, the assumptions required for the DID estimates to be unbiased are more likely to hold.

Studying how the net impact of the TB Program on earnings varies with age and prior-year earnings

The regression specifications above allow us to understand the average program effect for the entire population. While these estimates are very informative, they mask differences in the TB Program's impact across demographic groups. It could be the case that the average young person benefits more from the TB Program than the average old person does. It could be that wealthy people stand to benefit less from the TB Program than economically disadvantaged people. The TB estimates we recover from the above regressions do not permit us to study whether this is the case. We can augment the first regression model to study differences in the average ITT effect of the TB Program on earnings by an individuals' observable characteristics. Consider a simple model that permits the analysis of differences in the TB Program's net impact by age:

$$Y_{i,t} = a_i + b_t + \delta_1 D_{it} + \delta_2 D_{i,t} age_i + \varepsilon_{i,t}$$

where the variables have the same definitions as above, and age_i is a person's age at the time of their initial UI claim, measured in years. This regression will allow us to estimate the ITT effect of the TB Program on earnings for people of all ages. For instance, in the 2008 cohort, there are individuals of ages 19 to 72. This regression lets us calculate 53 different ITT estimates: one for people age 19, one for people age 20, and so on. These age-specific ITT estimates describe how the outcome variable, $Y_{i,t}$, changes when someone of a particular age receives treatment (D_{it} changes from zero to one). In other words, they are the derivative of $Y_{i,t}$ with respect to D_{it}. In this model, that is:

$$\frac{\partial Y_{i,t}}{\partial D_{i,t}} = \delta_1 + \delta_2 age_i$$

Now, if older people benefit more from the TB Program than younger people, the estimate of δ_2 will be positive. If younger people benefit more, the estimate of δ_2 will be negative. The relationship between age and the ITT is linear in this model by construction: the average 19-year-olds' ITT differs from the average 20-year-olds' by δ_2, from the average 21-year-olds' by $2\delta_2$, from the average 22-year-olds' by $3\delta_2$, and so on.[15] This artificial linearity is a model short coming – it likely oversimplifies the relationship between age and the TB Program's impact. If so, this oversimplification forces the estimate of δ_2 to take a value that may not accurately reflect the relationship between the ITT and age.

We can amend the model so that it more accurately captures the relationships in our data by including a quadratic term for age. This lets the ITT estimates differ by age in a more flexible and realistic way. This "simple quadratic" model of the ITT as a function of age that we estimate is:

$$Y_{i,t} = a_i + b_t + \delta_1 D_{it} + \delta_2 D_{i,t} age_i + \delta_3 D_{i,t} age_i^2 + \varepsilon_{i,t}$$

and it has the ITT:

$$\frac{\partial Y_{i,t}}{\partial D_{i,t}} = \delta_1 + \delta_2 age_i + \delta_3 age_i^2.$$

[15] Note that the weighted average of the ITTs for each age group (weighted by the number of people of that age) equals the ITT measured in the basic model. These models are consistent in that sense. This one simply allows us to understand how different demographic groups respond differently to TB Program participation.

We can study the relationship between other observable characteristics and the ITT by incorporating additional interaction terms. The "complex quadratic" model of differences in ITT estimates by demographic groups that we estimate is:

$$Y_{i,t} = a_i + b_t + \delta_1 D_{it} + \delta_2 D_{i,t} age_i + \delta_3 D_{i,t} age_i^2 + \delta_4 D_{i,t} E_i + \delta_5 D_{i,t}(NAICS_i) + \delta_6 D_{i,t} Gender_i + \varepsilon_{i,t}$$

where E_i is an individuals' earnings in the year prior to their initial claim, $NAICS_i$ is the industry they belonged to prior to their initial claim, and $Gender_i$ is their gender at the time of the initial claim. This model has the ITT:

$$\frac{\partial Y_{i,t}}{\partial D_{i,t}} = \delta_1 + \delta_2 age_i + \delta_3 age_i^2 + \delta_4 E_i + \delta_5 (NAICS_i) + \delta_6 Gender_i.$$

While the "simple quadratic" model above lets us study how the impact of the TB Program changes across age groups, this "complex quadratic" model lets us study differences *within* each age group. For instance, maybe economically-disadvantaged 19-year-olds benefit more from the TB Program than wealthy 19-year-olds. This more complex model allows us to test this hypothesis.

This "complex quadratic" model also gives us a more nuanced understanding of the ITT estimate differences across ages. In the "complex" model, the relationship between age and the amount that someone benefits from TB Program participation is conditioned on gender, industry, and earnings in the year before their UI claim. As such, the estimates from the "complex quadratic" model give a more accurate measure of the correlation between age and the impact of the TB Program on earnings.[16] The "complex quadratic" model, then, has two nice benefits: we can study how the TB Program affects different demographic groups, and we can understand the relationship between these demographic groups and the program's net impact more accurately.[17]

Once we've fit the "complex quadratic" model, we can predict the effect of the TB Program on each TB participant's earnings. These predictions will vary by all the dimensions we include in the model: age, gender, industry, and lagged earnings. If we observe two people that are identical along those dimensions, our prediction for the impact of the TB Program on their earnings will be the same. We call these individual-specific predictions, i.e., the derivative of $\hat{Y}_{i,t}$ with respect to D_{it}, "premiums" in earnings that are attributable to the TB Program. Since $D_{it} = 0$ for those who do not participate in the TB Program, this derivative will equal zero for all non-participants, meaning we can only use this method to predict premiums for TB participants. These premiums are:

$$premium_i \equiv \hat{\delta}_1 + \hat{\delta}_2 age_i + \hat{\delta}_3 age_i^2 + \hat{\delta}_4 E_i + \hat{\delta}_5 (NAICS_i) + \hat{\delta}_6 Gender_i$$

---

16 For example, since young people tend to earn less than their older peers, and disadvantaged people may benefit more from TB participation, the simple model likely incorrectly ascribes some of the variance in ITTs across income groups to age.

17 Note that, as before, the weighted average of the premiums is equal to the ITT result obtained from the simple DID model.

where the hat above the estimator conveys that the value is the sample-specific estimate of the population parameter. We can study the relationship between age and the TB premium using this model in two ways. First, we can assess the values of $\hat{\delta}_2$ and $\hat{\delta}_3$ obtained from the regressions above. Together, these tell us the relationship between age and the impact of the TB Program. Likewise, the coefficient $\hat{\delta}_4$ informs us about the relationship between earnings in the year prior to the UI claim, and the net impact of the TB Program.

Second, we can calculate the average ITT for age groups. We study three age groups: under 35, ages 35 to 46, and older than 46. We chose these age cutoffs so that roughly one third of TB participants falls into each category. We can measure whether the ITT for each group is different. We present results from both types of analyses in *Chapter 4*.

Studying how the net impact of the TB Program varies with program of study

In addition, we can use these premiums to assess the relationship between the course of study someone chooses, and how much they benefit from the TB Program. This allows us to understand whether specific courses of study result in systematically higher or lower premiums. For instance, it could be that studying business administration results in higher earnings in the future than studying theater arts. To analyze how the course of study that individuals chose influences their outcomes, we regress their two-digit Classification of Instructional Programs (CIP) code on premiums:

$$premium_i = \beta CIP_i + \nu_i$$

where CIP_i describes the course of study that individual i undertook, ν_i is a composite error term that includes uncertainty from two sources – the fact that the relationship between CIP_i and $premium_i$ is stochastic and the fact that $premium_i$ is measured with uncertainty – and the regression is run only for individuals that participate in the TB Program. There are 43 two-digit CIP codes, so we estimate 43 correlations between the course of study and the TB Program's net impact on earnings. Unfortunately, we do not have CIP data for the 2002 and 2003 cohorts, and so omit them from this analysis. We present these regression results in *Chapter 4*.

In this regression, unlike in the preceding analyses, the independent variable (what course to study) and the dependent variable (a function of TB enrollment) are determined at the same time. The choice of what course to study is made concurrently with the choice to enroll in the TB Program. As such, this regression cannot give estimates that can be interpreted as causal relationships. Though they are just correlations, they are still informative. They give insight into who among TB participants benefitted from the program and what courses they took.

Outcomes evaluated in the 2021 net impact study

We study the net impact of the TB Program on three outcomes in each follow-on year: employment, earnings, and course credits attempted.

To study the net impact of the program on employment, we construct a dependent variable equal to the number of quarters an individual is employed in each year. As such, this variable takes value zero, 25, 50, 75, or 100 percent. If an individual earns at least $100 in a quarter, we consider them to be employed in that quarter.

To study the net impact of the program on annual earnings, we sum the quarterly earnings for all four quarters in each follow-on year. If individuals file their UI claim in second quarter 2002, for instance, their first follow-on year of earnings is equal to the sum of their earnings in second, third and fourth quarters of 2002, plus first quarter 2003.

To study the net impact of the program on annual course credits attempted, we construct an indicator variable equal to one when the UI claimant attempted to earn at least one credit in their first follow-on year, and equal to zero if they did not attempt to earn any credits in their first follow-on year. Since academic calendars on semester systems and quarter systems do not necessarily line up nicely with fiscal quarters, we use a period of 365 days to define the follow-on year instead of quarters. So, this indicator variable is equal to one when an individual attempted to earn a credit within 365 days of claiming UI benefits.

Chapter 3: Descriptive statistics

We present three descriptive analyses in this chapter. First, we study the characteristics of the 2002 to 2016 TB Program participants, summarize the characteristics of each comparison group, and analyze the differences between the treatment group and comparison groups. We present these basic descriptive statistics in *Figures 3-1* and *3-2*.

Second, we study how the TB participants differ over time. The early cohorts are characterized by highly skilled individuals, typically men, who seek training for a short period of time and then return to their previous employer.[18] The later participants have lower education and skill levels, typically work in different industries, and typically do not often return to their original employer. These later cohorts tend to have a more equal number of men and women. Roughly half of the individuals in the two early cohorts are employed in the aerospace engineering industry, while about two percent of the people in the later cohorts are. We present a descriptive analysis of the differences between cohorts in *Figures 3-3* and *3-4*. The differences between the results we find for the 2002 and 2003 cohorts (positive average program effects) and the cohorts from 2006 to 2016 (negative average program effects) are partially explained by differences in who seeks training.

Third, we describe what kinds of classes TB Program participants enrolled in. We study two-digit Classification of Instructional Program (CIP) codes, which describe the course of study. In total, there are 43 different types of courses that TB participants pursued. We provide the top 20 most popular courses of study, and the number of people that pursued them in *Figure 3-5*.

Basic summary statistics

In *Figure 3-1*, we provide basic information about each cohort, including:

1. Total number of TB participants in each cohort.
2. Number of TB participants that remained for analysis after we prepared the data by discarding outliers and individuals with missing data. We identify outliers as anyone younger than 17 or older than 80, and anyone with quarterly earnings above \$50,000. For reference, the top 99th percentile for quarterly earnings is around \$34,000 for all years.
3. Sum of the number of treatment and control group subjects constructed using PSM. We present this information in the column titled "Subjects in dataset."
4. Number of years of data available in the pre-unemployment period.
5. Number of follow-on years of data available.

[18] See Aviles et al (2015), *Figure 1-2* for summary statistics on returning to the employer of record for the early cohorts. A larger percent of the TB Program participants returns to their employer of record in the 2002 and 2003 cohorts than in later cohorts.

Figure 3-1. Number of TB participants and years of data for the 2002 through 2016 cohorts
Washington state, 2002 through 2016
Source: Employment Security Department/LMEA

Cohort	TB participants (total)	Analyzed TB participants (no outliers/missing data)	Subjects in dataset	Years (pre-unemployment period)	Years (follow-on period)
2002	3,619	3,583	7,166	3	17
2003	2,509	2,469	4,938	3	16
2004	1,176	1,154	2,308	3	15
2005	1,152	1,139	2,278	3	14
2006	1,254	1,217	2,434	3	13
2007	1,000	932	1,864	3	12
2008	1,919	1,860	3,720	3	11
2009	4,577	4,477	8,954	3	10
2010	3,061	2,966	5,932	3	9
2011	2,409	2,343	4,686	3	8
2012	2,218	2,185	4,370	3	7
2013	1,806	1,778	3,556	3	6
2014	1,751	1,726	3,452	3	5
2015	1,373	1,366	2,732	3	4
2016	981	969	1,938	3	3

In *Figure 3-2*, we present pre-unemployment information on earnings, employment, education, gender, race, ethnicity, common industries, and the number of people (N) in each group. We also compare the characteristics of different subgroups in our data in this figure. In the second column (titled "Treated"), we present information for all individuals who participated in the TB Program from 2002 to 2016. In the third column ("Untreated"), we present the same information for all individuals who did not participate in the TB Program. A naïve comparison between this group and the control gives us a biased estimate of TB Program effects since the groups are very dissimilar. The control group is twice as likely to have been unemployed in the three years prior to the UI claim that establishes their cohort. They are younger on average and differ drastically in terms of educational attainment and gender composition. We use bold font in the third column to denote values that are statistically different from the treated groups' values at the 95 percent confidence level. In fact, all rows are bold. To conduct valid causal inference, we control for these differences using our matching approach. In addition, time-invariant characteristics are also controlled for using an individual fixed effects approach in the DID models.

In the fourth column ("PSM"), we present the summary statistics for the group of untreated individuals that are most similar to the treatment group in their propensity scores. In the fifth column ("P-value"), we present the p-values from the test that the treated (column two) and PSM group (column four) are different. A p-value lower than 0.05 means that the two groups are different along that specific dimension.

Figure 3-2. Summary Statistics
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Variable	Treated	Untreat	PSM	P-value	MDM	Denied	CEM	All
Annual earnings[19] [20]	$48,216	**$37,355**	$48,066	0.45	$47,624	$42,944	$36,498	$43,965
Percent of time unemployed	11.2%	**22.4%**	**13.2%**	< 0.01	11.4%	14.4%	9.9%	12.2%
Age at claim	40.6	**39.1**	40.6	0.92	40.5	39.3	38.5	39.8
Women	49.4%	**37.8%**	49.4%	*	49.4%	52.8%	63.2%	53.3%
Disabled	2.8%	**2.3%**	2.6%	0.26	2.8%	3.0%	0.0%	2.1%
Veteran	16.0%	**10.8%**	**14.5%**	< 0.01	16.0%	14.6%	4.6%	12.3 %
Education								
No formal education	1.8%	**7.1%**	1.8%	0.60	1.8%	1.5%	0.7%	1.5%
Elementary-high school	35.6%	**45.6%**	35.1%	0.15	35.6%	33.7%	53.3%	39.6%
Some college	27.1%	**17.9%**	27.8%	0.06	27.1%	28.4%	22.2%	26.1%
College complete or graduate	30.8%	**24.1%**	30.9%	0.75	30.8%	31.8%	22.5%	29.0%
GED	4.4%	**4.9%**	4.1%	0.07	4.4%	4.4%	1.1%	3.5%
Ethnicity								
White	71.0%	**70.0%**	70.7%	0.5	74.6%	66.3%	88.6%	75.4%
Hispanic	7.1%	**12.1%**	7.0%	0.91	7.0%	7.5%	3.9%	6.4%
Black	6.0%	**5.2%**	5.9%	0.65	5.0%	9.0%	1.7%	5.2%
Other	9.8%	**8.3%**	10.0%	0.38	8.4%	11.3%	4.9%	8.5%
Industry								
Construction	5.7%	**17.7%**	**10.0%**	< 0.01	14.0%	5.8%	10.0%	10.5%
Manufacturing	28.6%	**13.8%**	**19.1%**	< 0.01	15.9%	23.0%	19.6%	18.8%
Retail trade	7.6%	**9.3%**	**8.5%**	< 0.01	8.1%	9.6%	10.4%	9.1%
Public administration	7.8%	**4.2%**	**7.0%**	< 0.01	8.2%	7.2%	5.5%	6.8%
N	30,164	4,476,7	30,164	*	30,164	19,971	26,990	104,461

Notes: t-test is performed for the comparison pool and PSM control. In the Ethnicity section, the option "unknown/no answer" is omitted.
Bold denotes p < 0.05.
*Not reported since they are exactly matched on gender for PSM.

Excluding industry, which we will discuss in more depth later, 13 of the 15 characteristics we compare are statistically similar for the treated and PSM groups. The PSM approach does a particularly good job of selecting untreated individuals that have similar wages, are of a similar age, have similar education levels, and report identifying as similar ethnicities. Though members of the PSM group are more likely to have been unemployed compared to the TB participants, they are substantially less likely to have been unemployed compared to the entire untreated group (third column).

[19] The earnings and employment values are summaries of individuals' experiences over the three years prior to entering a specific cohort in this study. For instance, the percent of time unemployed gives a description of the annual percent of time individuals in each group remained unemployed *prior to* the unemployment spell that we analyze in this paper.

[20] Conditional on employment.

Most of the differences documented in column five are in the industry that employs individuals. The fixed effects strategy we adopt in our DID model controls for these industry-composition differences. In addition, the fact that the denied comparison group is similar to the treatment group by industry, and the results are robust to using the denied comparison group, gives us confidence that this challenge does not invalidate our results.

In columns six through eight, we present the summary statistics for the control groups we construct from our alternative methods – Mahalonobis distance matching (MDM), those who appeal their denial unsuccessfully (denied), and coarsened exact matching (CEM). In column nine ("All Control"), we present that summary statistics for all individuals selected by any matching method. All the groups differ slightly.

Using each as a comparison group makes the parallel trends assumption in our difference-in-difference analysis likely to hold for different reasons. In each model, time-invariant individual-specific characteristics are modeled by the individual fixed effects, and trends that may differ over time across the two groups are modeled by the matching process. Using a different matched cohort (different matching process) captures slightly different group-specific trends that may exist. Assessing the suite of results collectively provides a robust estimate of the TB Program's causal effect on earnings, employment, and training.

Differences over time

In *Figure 3-3*, we present summary statistics describing differences in TB participants across cohorts for all variables except industry. We give a deeper analysis of differences across cohorts by industry in *Figure 3-4*. In both tables, we pool the data into six groups. The first group (in column two) benefits from participating in the TB Program. These are the 2002 and 2003 cohorts. The TB Program has mixed effects for the second group (in column three), but in general the net impact is negative. The second group comprises the 2004 to 2007 cohorts. The third through sixth groups are unambiguously disadvantaged by participating in the training benefits. We observe them during the recession (2008 to 2009), recovery (2010 to 2012), and expansion (2013 to 2016) periods respectively.[21] In column seven, we present the pooled summary statistics for the 2004 to 2016 cohorts.

In column eight of the table in *Figure 3-3*, we present how the p-values for the test that the 2002 to 2003 cohorts differ from the others (2004 to 2016). The 2002 to 2003 cohorts are different from the rest in all observable aspects. They enjoy higher wages, are more likely to have a job, are older, have a more male-skewed gender composition, are more frequently white, are less likely to be disabled, and are more likely to be veterans. Their educations differ in all categories but the proportion of the population that has between one and twelve years of education. In contrast, the characteristics of individuals who participate in the TB Program from 2004 to 2016 are qualitatively similar.

---

[21] There is suggestive evidence that the 2005 cohort *may* have positive lifetime benefits from participating in the TB Program (though they have a negative lifetime impact in the follow-on years we observe). However, the 2004, 2006, and 2007 cohorts are disadvantaged by participation. An alternative grouping would combine the 2007 to 2009 cohorts. The disadvantage of this alternative grouping is that it combines non-recession (2007) and recession (2008 to 2009) years. Either grouping yields the same key takeaway: the 2002 to 2003 cohorts are different from those that follow.

Figure 3-3. TB participants' summary statistics
Washington state, 2002 through 2016 TB participants
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Variable	2002–2003	2004–2007	2008–2009	2010–2012	2013–2016	2004–2016	P-value
Annual earnings conditional on	$58,902	$49,261	$47,157	$42,909	$44,278	**$45,529**	< 0.01
Percent of time	5.2%	7.5%	8.6%	14.5%	18.9%	**12.8%**	< 0.01
Age at claim	41.6	42.5	40.3	39.3	39.9	**40.3**	< 0.01
Women	42.6%	55.5%	48.4%	52.3%	49.4%	**51.2%**	< 0.01
Disability	0.9%	2.4%	2.0%	3.2%	5.2%	**3.3%**	< 0.01
Veteran	18.1%	14.7%	11.2%	15.1%	21.2%	**15.5%**	< 0.01
Education							
No formal education	0.1%	1.4%	2.7%	2.3%	2.2%	**2.2%**	< 0.01
Elementary to high school	36.6%	36.5%	36.2%	34.8%	34.4%	35.4%	= 0.08
Some college	38.7%	29.3%	22.4%	23.5%	23.2%	**24.2%**	< 0.01
College complete or	20.7%	28.4%	33.8%	35.0%	34.5%	**33.4%**	< 0.01
GED	3.7%	4.1%	4.7%	4.1%	5.4%	**4.6%**	< 0.01
Ethnicity							
White	73.2%	70.6%	73.9%	71.6%	65.0%	**70.4%**	< 0.01
Hispanic	3.3%	7.4%	6.7%	8.1%	9.8%	**8.0%**	< 0.01
Black	3.8%	5.9%	4.8%	6.6%	9.1%	**6.6%**	< 0.01
Other	12.6%	10.8%	8.9%	7.9%	9.4%	**9.1%**	< 0.01
N	6,052	4,442	6,337	7,494	5,839	24,112	-

Notes: t-test is performed to compare the 2002 to 2003 cohorts and the later cohorts. In the ethnicity section, the option "unknown/no answer" is omitted.
Bold denotes p < 0.05.

Possibly the most important difference between participants in 2002 to 2003 and those who participate later is in the industry in which they worked prior to participating. We report these differences in *Figure 3-4* at three different levels. In the top portion of the table, we select the top three industries (2-digit NAICS codes) for each cohort and present the proportion of individuals employed in that sector. In this portion, "-" does not represent zero percent or missing data, but rather "not top-3." Within an industry, people may have many different professions. For instance, people in the aerospace industry may be engineers, project managers, human resources staff, janitors, secretaries, or may have some other occupation.

The key takeaway from the top portion of *Figure 3-4* is that the 2002 to 2003 cohorts are different from the later cohorts. The most common industry types are given in rows two, three, and four. Roughly 65 percent of the TB participants in 2002 to 2003 are employed in manufacturing. In contrast, in the later years, fewer than 20 percent of participants are engaged in manufacturing. After manufacturing, the next most common industry for the 2002 to 2003 cohorts is "administrative and waste services" (roughly four percent) and then "transportation and warehousing" (roughly 4 percent). Most of the participants in the early cohorts work in manufacturing. In later cohorts, there is more balance. The most common industry is also manufacturing (with about 20 percent). Then it is "public administration" (roughly 10 percent), followed by "retail and trade" (roughly 9 percent).

We can learn more about the manufacturers in the 2002 and 2003 cohorts by studying the 3-digit and 4-digit NAICS codes. We present this information in the lower two portions of *Figure 3-4*. We present *unconditional* percentages in the table's lower portions. For example, when we report that 45.3 percent of the 2002 to 2003 cohort is employed in transportation equipment manufacturing, we do not mean 45.3 percent of manufacturers are engaged in this specific type of manufacturing, but 45.3 percent of all TB participants in 2002 to 2003 are engaged in this specific type of manufacturing.

Almost half of the 2002 to 2003 TB cohorts are from the high-tech manufacturing industry groups of aerospace product and parts manufacturing, and semiconductor and electronic component manufacturing. Less than 3 percent of the later cohorts engage in these activities.

As discussed in *Chapter 1*, it appears that a huge layoff by the aircraft production company Boeing, combined with the legal change for the TB Program in 2002, largely explains the unusual composition of the TB participants group in 2002 and 2003. Participants in these two early cohorts could be productively employed as aerospace engineers, temporarily leave their jobs due to the financial hardship of the employer, and then return to work with the same employer after the hardship passed. This contrasts with participation in later years when individuals did not regularly return to work with the same employer. See Aviles et al (2015) for deeper discussion on heterogeneous program effects for those who return to work with the same employer.

Figure 3-4. TB participants' percentage of employment by industry (2-, 3-, and 4-digit NAICS)
Washington state, 2002 through 2016 TB participants
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Industry	2002–2003	2004–2007	2008–2009	2010–2012	2013–2016	2004–2016
2-digit NAICS						
Manufacturing	65.3%	24.8%	25.6%	13.5%	16.0%	19.4%
Administrative and waste services	4.6%	*	*	*	*	*
Transportation and warehousing	4.0%	7.8%	*	*	*	*
Finance and insurance	*	8.9%	8.6%	*	*	*
Construction	*	*	9.8%	*	*	*
Public administration	*	*	*	10.5%	16.3%	9.5%
Healthcare and social assistance	*	*	*	10.2%	*	*
Retail trade	*	*	*	*	9.6%	8.7%
3-digit NAICS – types of manufacturing						
Transportation equipment manufacturing (336)	45.3%	3.5%	6.0%	1.6%	4.6%	3.8%
Computer and electronic product manufacturing (334)	6.2%	2.7%	2.2%	0.6%	0.6%	1.4%
4-digit NAICS – types of equipment manufacturing						
Aerospace product and parts manufacturing (3364)	44.9%	2.6%	2.8%	0.8%	3.6%	2.3%
Semiconductor and electronic component manufacturing (3344)	4.0%	0.8%	1.0%	0.1%	0.2%	0.5%
Alumina and aluminum production (3313)	3.2%	0.1%	0.2%	0.01%	1.9%	0.5%

*Indicates the industry is not one of the three most common industries.

Courses of study

We present the top 20 most common courses of study for the 2004 to 2016 cohorts in *Figure 3-5*. The most common choice is to study healthcare topics. In our data, 4,846 people chose to study healthcare topics. The top five most popular choices are rounded out by business, information sciences, engineering, and mechanics. In *Chapter 4*, we study how these choices are correlated with the TB Program's net impact on earnings.

Figure 3-5. Courses of study sought by TB participants
Washington state, 2004 to 2013 TB participants
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016; ERDC

Course of study	Number of TB participants
Health professions and related programs	4,486
Business, management, marketing, and support services	3,582
Computer and information sciences and support services	2,371
Engineering technologies and related fields	1,037
Mechanic and repair technologies	902
Precision production	604
Homeland security, law enforcement, firefighting, and related protective services	370
Legal professions and studies	321
Education	284
Personal and culinary services	253
Construction trades	251
Transportation and materials moving	192
Natural resources and conservation	154
Communications technologies and support services	141
Agriculture, agricultural operations, and related sciences	126
Visual and performing arts	111
Parks, recreation, leisure, and fitness studies	54
Social sciences	41
Science technologies and technician	35
Library arts and sciences, general studies and humanities	23

Chapter 4: Net impact of the TB Program on earnings

We present our estimates of the TB Program's impact on participants' earnings in *Figures 4-1* through 4-*3*. These estimates are the difference-in-differences model results using the PSM group for comparison. For each cohort, we provide estimates of program effects for all available follow-on years. In addition to reporting the results in tables, we provide results for three representative cohorts – 2002, 2006, and 2014 – in a plot in *Figure 4-4*. This information is redundant since the results are provided in the tables, but the plots may make it easier to under the results visually.

After describing these average net impacts for each cohort, we study differences within cohorts in three ways. We study the TB Program's net impact on earnings for individuals coming from the aerospace manufacturing industry (results reported in *Figure 4-5*) and for individuals of different ages (results reported in *Figures 4-6* through *4-10*). We report the TB Program's net impact by age and earnings in the year prior to the UI claim of interest in *Figure 4-11*. We present the underlying regression results in *Figure 4-12*.

We also studied the correlation between the course of study that an individual chose and the TB Program's net impacts on their earnings. We report these results in *Figures 4-13*.

Net impact of the TB Program on earnings for all cohorts

When we compare TB participants' earnings to control group members' earnings for all cohorts in all years, we see that TB participants earn $3,621 less on average *per year*. The earnings for the participants in all cohorts tend to drop in the first few follow-on years while they train. Then, they catch back up to their peers who did not participate. They initially forego earnings, and never surpass their peers in the wages they earn *enough* to break even. As such, the total long-run effect of the program on their earnings is negative. Because they participated in the TB Program, they earned $3,621 less in income in *each* of the observed follow-on years.

This aggregated result hides differences in the program's effect for each cohort. In the remainder of this section, we analyze the program effects for each cohort.

Net impact of the TB Program on earnings for the 2002 and 2003 cohorts

In *Figure 4-1*, we present the results for the 2002 and 2003 cohorts. We report results that are statistically significant at the 95 percent level in bold. We present the bootstrap standard errors below the estimates.[22]

[22] Bootstrap standard errors are derived from the data, as opposed to being analytically constructed based on the regression design employed. To calculate bootstrap standard errors, the statistician uses an algorithm that (1) resamples from the dataset with replacement to construct a "bootstrap analog" dataset; (2) fits the model on the bootstrap analog data and stores the results; (3) repeats the first two steps a large number of times, say 100; and (4) calculates the standard deviation of the estimate analogs.

The results in *Figure 4-1* tell a clear story. Participants in these years made a large investment in their first four follow-on years in the form of forgone earnings. For the 2002 cohort, this investment is foregoing $12,184 in the first year, $22,747 in the second, $12,555 in the third, and $3,627 in the fourth. Then, starting in year four, they begin to enjoy greater earnings than their peers. The annual earnings boost they enjoy from having participated in the TB Program increases until the ninth follow-on year. In the middle of year 10, the amount they earn have accumulated so much that they are greater than the size of the initial investment. The value of their investment switches from being negative to being net positive. We have data on another six follow-on years, they continue to earn a large amount more than their peers each year. Over the course of the 16 years that we observe for this cohort, they net $63,934 because of their participation in the TB Program.

The story is largely the same for the participants in the 2003 cohort. They forego $37,530 in earnings in the first four follow-on years.[23] Then, over the next decade they enjoy annual earnings boosts. In year nine, they've earned enough so that their investment has paid off. Over the 15 years that we observe their earnings, they net $41,318 from their participation in the TB Program.

If we only observed earnings for participants in 2002 and 2003, we would conclude that the program has a large and positive effect on earnings. We would conclude that the program is working as intended for the typical participant. However, *Figures 4-2* and *4-3* tell a different story for the more recent cohorts.

Figure 4-1. TB Program net impact on earnings by follow-on year (measured in 2016 dollars); Washington state, 2002 through 2003 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year																
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**
2002	**($12,184)**	**($22,747)**	**($12,555)**	**($3,627)**	**$2,782**	**$5,732**	**$6,512**	**$8,754**	**$9,714**	**$10,880**	**$10,576**	**$11,742**	**$10,987**	**$10,560**	**$9,369**	**$8,889**	**$8,550**
Bootstrapped standard error	586	740	747	754	765	864	908	886	942	962	974	1,061	1,150	1,139	1,120	1,186	1,250
2003	**($13,576)**	**($18,391)**	**($5,563)**	$700	**$3,652**	**$5,151**	**$7,232**	**$8,309**	**$8,425**	**$8,035**	**$8,610**	**$7,946**	**$7,030**	**$5,907**	**$4,723**	**$3,828**	
Standard errors	705	914	946	1,010	1,008	980	1,018	1,075	1,187	1,229	1,233	1,251	1,310	1,364	1,309	1,378	

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

[23] Note that we treat statistically insignificant estimates as zeroes in calculating net impacts.

Net impact of the TB Program on earnings for the 2004 and 2005 cohorts

Figure 4-2 reports the same earnings impacts for the 2004 and 2005 cohorts. Participants in the 2004 cohort forego $39,670 on average while training over the course of four years. In the fifth year, they catch up to their peers. At this point, they are done training and back in the labor force, earning similar wages to the control group members that never trained. For follow-on years four through 14, we cannot reject the null hypothesis that the control group and treatment group earn the same amount on average. The life-time value of participating in the TB Program for the 2004 cohort is negative $39,670.

The 2005 cohort does better. They forego $38,325 while training and catch up to their peers in follow-on year five. In follow-on year 7 (2012) they enjoy their first annual earnings boost. Between 2012 and 2019, they earned $31,436 more than the control group members on average. It is possible that they net a positive amount in the future, though, as of this study, they lost $6,889 because of their participation in the TB Program.

Figure 4-2. TB Program net impact on earnings by follow-on year (measured in 2016 dollars), Washington state, 2004 through 2005 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year														
Cohort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
2004	**($11,317)**	**($17,337)**	**($8,262)**	**($2,754)**	($556)	$236	$281	$902	$1,544	$2,041	$2,295	$1,953	$1,894	$1,069	$2,199
Bootstrapped standard error	912	1,073	1,183	1,314	1,342	1,501	1,651	1,661	1,656	1,655	1,772	1,828	1,857	1,825	1,807
2005	**($11,135)**	**($16,063)**	**($8,066)**	**($2,971)**	$552	$1,139	$2,271	**$4,124**	**$4,417**	**$3,989**	**$4,531**	**$4,816**	**$4,839**	**$4,720**	
Bootstrapped standard error	914	1,170	1,206	1,207	1,250	1,318	1,331	1,372	1,436	1,526	1,586	1,617	1,543	1,591	

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations

Net impact of the TB Program on earnings for the 2006 through 2016 cohorts

The cohorts in the most recent decade fare poorly (*Figures 4-3* and *4-4*). Only one out of ten of the cohorts enjoys any annual earnings boosts at all. This is the 2009 cohort that earns more than the control group in follow-on years seven, eight, and nine. In these follow-on years, the 2009 cohort earns $1,880 more than their peers in the control group, on average. Even for this cohort, the average loss of earnings because of participation in the TB Program is $34,627. If they continue to earn an average of $1,880 more than their peers each year, they will see their first net gain because of program participation in 2029.

For the 2006 to 2008 cohorts, there is strong evidence that the program has a large negative effect on participant earnings. It is possible that, like the 2004 cohort, these cohorts will never earn more than their peers in the control group.

For the 2010 to 2013 cohorts, the participants' earnings catch up to the control group members' earnings on average in follow-on year five. These cohorts forewent an average of $47,652 while training. There is insufficient data to determine whether they will ever have higher earnings than the control group. For the 2010 cohort, we observe eight follow-on years; they do not earn more than their control-group peers in any of them.

For the 2014 to 2016 cohorts, there is insufficient data to determine whether they will enjoy a net positive boost to their earnings because of their participation in the TB Program. We only observe their "lock-in" follow-on years. The 2016 cohort does forego an unusually large amount of income during their first to follow-on years. Overall, it is hard to predict whether these cohorts will ever earn more than their peers in the control group. They may only catch up to their peers, never surpassing their earnings and never regaining their foregone earnings.

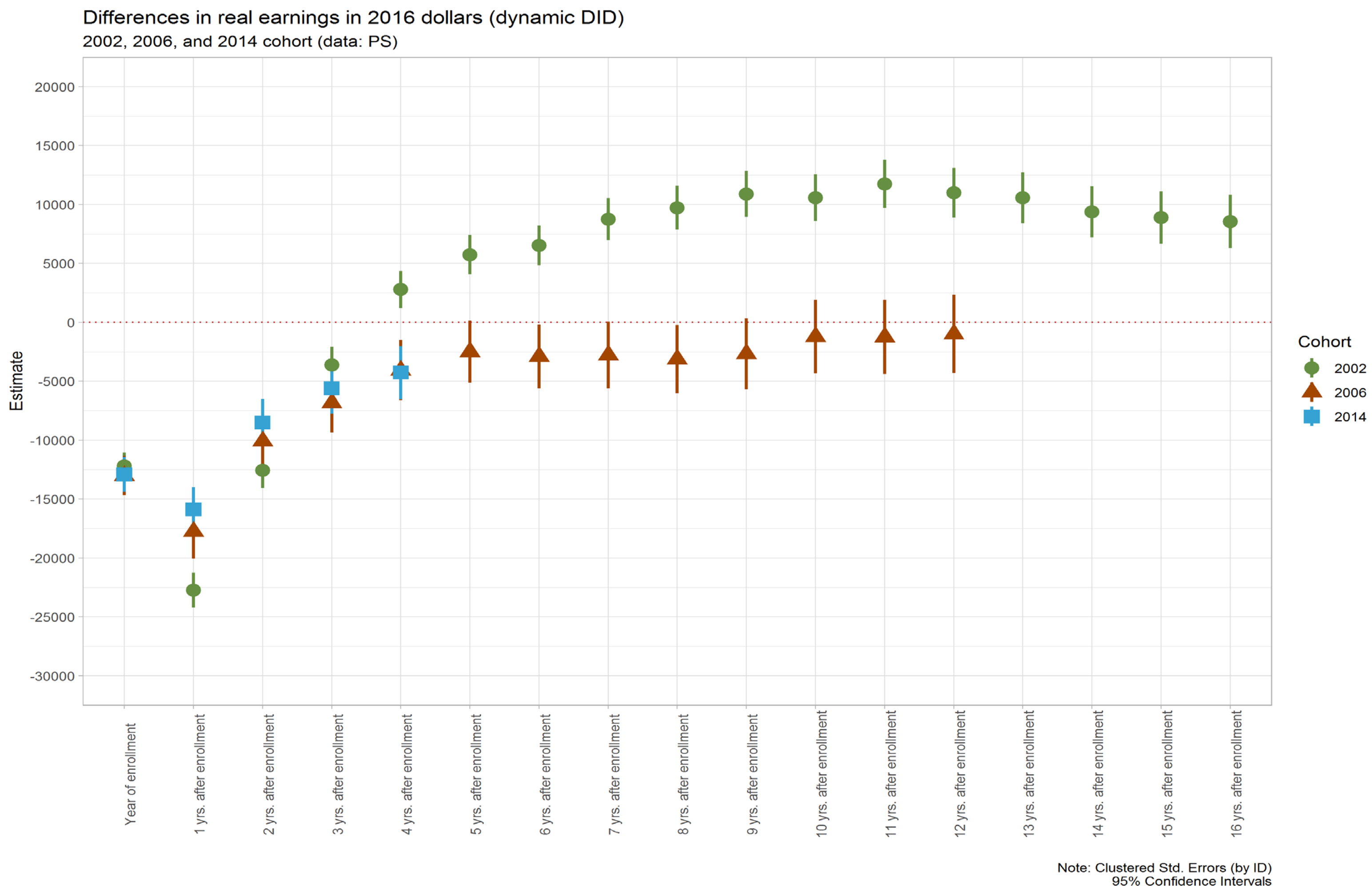
Did the 1,919 members of the 2008 cohort who lost $62,000 in earnings on average – for instance – make a wise choice to participate? Why are individuals participating in the program if their earnings are reduced by doing so? One possibility is that individuals incorrectly expect to earn more than their peers because of their participation in the TB Program. Afterall, the 2002 and 2003 cohorts witnessed large gains because of their participation. It is reasonable to expect that individuals incorrectly judged the quality of their investment. A second reason may be that individuals use the training to change careers. It is possible that individuals correctly anticipate a loss of earnings by participating in the TB Program and are willing to accept that loss to change professions. It could be that their new occupation is one that has benefits that we do not measure here (such as reduced commute time, or improved office culture and comradery). In this case, from the individual's perspective, participating in the program is worthwhile. We lack the data required to test either of these hypotheses. Further research is needed to ascertain why individuals participate in the TB Program when it harms their long-term earnings outlook.

Figure 4-3. TB Program net impact on earnings by follow-on year (measured in 2016 dollars)
Washington state, 2006 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	Follow-on year												
	0	1	2	3	4	5	6	7	8	9	10	11	12
2006	**($13,018)**	**($17,750)**	**($10,099)**	**($6,831)**	**($4,074)**	**($2,500)**	**($2,902)**	**($2,789)**	**($3,128)**	($2,676)	($1,216)	($1,246)	($987)
Bootstrapped standard error	742	1,112	1,250	1,277	1,238	1,248	1,274	1,378	1,428	1,404	1,493	1,534	1,581
2007	**($11,972)**	**($18,411)**	**($9,975)**	**($6,208)**	**($3,980)**	**($4,030)**	**($3,107)**	($1,791)	($907)	($779)	($505)	($20)	
Bootstrapped standard error	987	1,259	1,330	1,312	1,366	1,302	1,332	1,456	1,510	1,610	1,706	1,719	
2008	**($12,817)**	**($19,521)**	**($13,111)**	**($7,089)**	**($3,668)**	**($3,096)**	**($2,464)**	($1,067)	($674)	($986)	($695)		
Bootstrapped standard error	598	782	764	792	774	840	872	919	1,062	1,136	1,121		
2009	**($10,577)**	**($16,601)**	**($8,591)**	**($3,347)**	**($1,679)**	($430)	$957	**$1,487**	**$1,864**	**$2,290**			
Bootstrapped standard error	419	505	624	601	601	589	658	709	762	777			
2010	**($12,969)**	**($17,371)**	**($10,657)**	**($5,567)**	**($3,029)**	**($2,454)**	($1,353)	($726)	($149)				
Bootstrapped standard error	498	616	714	720	793	846	950	1,020	1,020				
2011	**($13,898)**	**($18,140)**	**($10,733)**	**($5,364)**	**($3,683)**	**($2,319)**	($904)	$259					
Bootstrapped standard error	680	847	881	942	997	990	1,018	990					
2012	**($13,065)**	**($17,281)**	**($9,081)**	**($4,540)**	**($2,348)**	($1,134)	($189)						
Bootstrapped standard error	702	849	912	974	1,098	1,069	1,136						
2013	**($11,804)**	**($14,010)**	**($6,600)**	**($3,420)**	**($2,276)**	($268)							
Bootstrapped standard error	784	968	1,064	1,008	1,137	1,277							
2014	**($12,912)**	**($15,886)**	**($8,511)**	**($5,601)**	**($4,270)**								
Bootstrapped standard error	690	910	916	950	944								
2015	**($13,121)**	**($14,695)**	**($8,211)**	**($4,340)**									
Bootstrapped standard error	926	1,192	1,262	1,257									
2016	**($17,053)**	**($17,689)**	**($8,983)**										
Bootstrapped standard error	1,089	1,343	1,416										

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations

Figure 4-4. TB Program net impact on earnings (measured in 2016 dollars)
Washington state, 2002, 2006, and 2014 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016



The 2002 and 2003 cohorts have many aerospace-industry workers compared to later years, and the early aerospace-industry workers have positive TB Program net impacts

We have documented two major ways in which the two early cohorts differ from the following 14 cohorts: the TB Program had a positive effect on the early participants' earnings, and a disproportionally large percent of the two early cohorts are from the aerospace industry. On average, the 2002 and 2003 cohorts benefit from the TB Program, but the later cohorts lose earnings by participating. In the 2002 and 2003 cohorts, 44.9 percent of the people we study (treatment and PSM control group combined) come from the aerospace industry prior to their UI spell. On average, only 2.4 percent of the other cohorts come from the aerospace industry (*Figure 3-4*).

Out of the 60,328 participants and PSM control group members, 4,593 people are employed in the aerospace manufacturing industry, potentially as aerospace engineers. In the 2002 and 2003 cohorts, we study 12,104 people total; 3,658 of them are employed in the aerospace industry. As such, 80 percent of all the aerospace employees are in the first two cohorts. The remaining 935 aerospace employees are spread out across the remaining cohorts (comprising 48,224 TB participants and control group members).

To assess whether aerospace industry employees benefit more from the TB Program than people from other industries, we can fit our DID model on two populations separately: aerospace employees and everyone else. In doing this, we get two distinct treatment effects: the ITT effect for aerospace employees and the ITT effect for everyone else. We fit this model on two groups:

1) The 2002 and 2003 cohorts.
2) The 2004 to 2016 cohorts.

When we fit this model on the 2002 and 2003 cohorts' data, we see that aerospace employees benefit more from the program. Their annual earnings increase from participating in the TB Program is $7,714. Non-aerospace employees in these early cohorts also benefit from participating in the TB Program, earning an annual increase of $1,486 because of their participation. The aerospace industry workers benefit far more than employees from other industries. For the early cohorts, then, the fact that 44.9 percent of the members come from the aerospace industry workers, and the fact that the TB Program increased these workers' annual earnings to a large degree, partially explains the positive average ITTs reported in *Figure 4-1*. We report these estimates and their bootstrapped standard errors in *Figure 4-5*.

When we fit the model on the 2004 to 2016 cohorts, we find a somewhat different result. The difference between the net impact of the TB Program on earnings for the two groups is not significant at the 95 percent level. As such, we cannot conclude that aerospace employees benefit more than employees from other industries in the later cohorts.

There are three differences between the early two cohorts and the later 14 cohorts. The second two observations help to explain the first:

1) The early two cohorts have positive ITT estimates while the later 14 cohorts have negative ITT estimates.
2) The early cohorts have a large percent of people from the aerospace industry, but the later cohorts do not.

3) The TB participants from the aerospace industry in the early cohorts benefited far more than participants from non-aerospace industries, but in later cohorts, the TB Program has a similar net impact on earnings for the two groups.

Figure 4-5. TB Program net impact on earnings for pooled sample by aerospace product and parts manufacturing (NAICS 3364) vs. the non-aerospace (measured in 2016 dollars)
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	Aerospace (A)	Non-Aerospace (B)	Difference (A – B)
Net impact (2002-03)	**7,714**	**1,486**	**6,228**
Bootstrapped standard error (2002 to 2003)	1,527	612	**1,594**
Net impact (2004 to 2016)	-2,210	**-5,444**	3,234
Bootstrapped standard error (2004 to 2016)	2,630	284	2,595

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Younger people benefit from the TB Program on average

Younger people have higher lifetime earnings, on average, because of their participation in the TB Program. Middle aged and older participants, on average, earn less over their lifetime than their peers because of their participation in the TB Program.

We present two types of evidence to support these findings. First, we report the distributions of the "premiums" – the predicted net impact of the TB Program on annual earnings for each individual, resulting from the "complex quadratic" model described in *Chapter 2* – for people under the age of 35, between the ages of 35 and 46, and older than 46. Second, we present the predictions as a scatterplot. After discussing these results, we give some additional comments on the regressions that underlay them.

In the densities plotted in *Figures 4-6* through *4-9*, we present pooled results by age group. In *Figure 4-6*, we plot three densities using the first six years of follow-on data for the 2004 to 2013 cohorts – i.e., the cohorts that have at least six years of follow-on data and which are harmed, on average, by their participation in the TB Program. We limit our analysis to six years of follow-on data to make the cohorts more comparable with each other. In *Figures 4-7* through *4-9*, we plot densities for specific cohorts. The x-axis has annualized premiums from the TB Program. Positive predicted annualized premiums mean that over time, on average, that individual benefited from the TB Program. Negative predicted premiums mean that over time, on average, that person lost money because they participated in the TB Program. For example, a predicted premium of $500 means that that person, on average over the timeframe we study, gained $500 dollars per year because they participated in the TB Program. The time horizon is six years *Figures 4-6* and *4-9*, 18 years in *Figure 4-7*, and 14 years in *Figure 4-8*. The y-axis depicts the percent of TB participants in the relevant data that have the corresponding predicted premium value. The density plots capture the predictions for all individuals in the relevant sample – pooled for *Figure 4-6*, and individual cohorts for *Figures 4-7* through *4-9* –that participated in the TB Program.

In *Figure 4-6*, the green density gives the distribution of TB premiums for people under the age of 36, the orange density gives the distribution of TB premiums for people ages 36 to 46, and the blue density gives the distribution for people over the age of 46. For all three groups, there are some individuals that benefit because of their participation in the TB Program, and some individuals who are worse off because of their participation. For the young group, more people benefit than are harmed. For the older group, more people are harmed than benefit. The averages of the distributions give the age-group-specific ITT estimates. These tell us how, if someone in that age category participated in the TB Program, we would expect their annual earnings to change. The average benefit that accrues to young people because of the TB Program is $239/year ($p < 0.05$). The average for the middle-aged group is -$8,477/year ($p < 0.05$). The average for the older group is -$16,508/year ($p < 0.05$). The average for all individuals, given by the black vertical line on the plot, is -$7,817 ($p < 0.05$). While there are older individuals that benefit from TB participation, on average per year, the individuals in this group lost out on $16,508 over the six years. It is possible that older individuals experience the relative loss in earnings from the lock-in period, then retire shortly afterwards, never enjoying any returns on their investment.

In *Figure 4-7*, we plot the same densities but with 2002 cohort data alone. The ITT for the whole cohort is positive (*Figures 4-1* and *4-4*), which is plotted here by the vertical line intersecting the x-axis at $3,763 ($p < 0.05$). The average ITT for both the young group and the middle-aged group are positive ($16,258 and $6,882 respectively, $p < 0.05$). The average ITT, however, is negative for the older group in 2002 (-$12,826, $p < 0.05$). Even in the 2002 cohort, which benefited overall from participating in the TB Program, the older participants tend to lose money over their lifetimes, relative to their peers who do not train, because of the TB Program.

In *Figures 4-8* and *4-9*, we present the same densities for 2006 and 2014, chosen because they are representative of the distributions for the later cohorts. The ITT estimates for the entire population are negative for both cohorts (-$5,325 and -$8,366 respectively, $p < 0.05$). For the younger group in 2006, the ITT estimate is positive ($9,303, $p < 0.05$). The younger group has a negative ITT estimate in 2014, likely because we observe few years of working after training for this cohort.

In *Figure 4-10*, we present corroborating evidence. Here, as in *Figure 4-6*, we plot data for the 2004 to 2013 cohorts and limit the number of follow-on years to six. The x-axis represents an individual's age, and the y-axis represents their predicted annualized premium (note that previously, premium was on the x-axis). We color code the age groups in the same way: the youngest group's data are plotted in green, the middle-aged group's data are plotted in orange, and the older group's data are plotted in blue. For each group, we provide a line of best fit, and the corresponding 95 percent confidence interval for it.

This plot highlights the negative relationship between age and the net impact of the TB Program on a person's earnings. On average, individuals younger than 28 benefit from the TB Program. On average, individuals older than 28 lose money compared to their peers because of the TB Program. They are worse off because of it. No one over the age of 66 that participated in the TB Program had a positive predicted annualized premium (the oldest person in the study was age 79 at their initial claim). Everyone under the age of 20 (the youngest people in the study were age 19 at their initial UI claim) had a positive predicted annualized premium.

Poorer people benefit from the TB Program on average

Across cohorts, within each of the three age groups we study, there are some people that we predict benefit from participating in the TB Program. This is evident in *Figures 4-6* through *4-9*. Part of each distribution, regardless of the age category, falls to the right of zero. For instance, in *Figure 4-6* about 10 percent of people older than 46 are predicted to have a positive predicted impact from the TB Program. This is true for 24 percent of people ages 36 to 46, and 54 percent of people under the age of 36.

There are also people in each age category that we predict lose money, compared to their peers, because they participate in the TB Program. While the typical young person benefits from the TB Program, some do not. In *Figure 4-6*, we see that about half of the young group have a positive premium, and half have a negative premium.

This pattern – some people in each age category benefit and some are harmed by TB Program participation – exists for all cohorts. Which older people benefit? Which younger people are harmed by participating in the TB Program?

In *Figure 4-11*, we plot the relationship between the TB premium and the amount an individual earned in the year prior to their UI claim. As in *Figure 4-10*, we plot the premium on the y-axis. We plot the amount of income a person earned in the year prior to their TB participation on the x-axis. We present separate scatterplots for each age group, using the same color distinctions as before: the younger group's data are plotted in green, the middle-aged group's data are plotted in orange, and the older group's data are plotted in blue. For each group, we also plot the line of best fit and corresponding 95 percent confidence interval.

There is a strong negative correlation between the amount earned in the year prior to the UI claim and the TB premium. We predict that, the lower your income, the more you stand to benefit from participation in the TB Program. There are two possible explanations for this relationship. The first is that the amount of earnings that people forgo during training is lower for poorer individuals. As such, they can break even on their investments with more modest increase in annual earnings. The second is that training may be particularly effective for poorer people.

We also plot three vertical lines, one at each place where best-fit lines cross zero. These are significant points on the plot. When the best fit line is positive for a group, the predicted impact of the TB Program on that group's earnings is positive. For the youngest group, the average ITT for people who earn less than $40,577 is positive. About half of the younger group earns less than this amount. That's why about half of the purple distribution plotted in *Figure 4-6* lays above zero. Younger, poorer people tend to benefit from the TB Program. On the other hand, the members of the younger group that are harmed by the TB Program tend to be wealthy. They forgo large amounts of earnings during the lock-in period and may have lower marginal returns to education.

Even in the two older cohorts, there are people that benefit from the TB Program – these people tend to have lower earnings in the year prior to their UI claim. Middle-aged people that earned less than $30,029 in the year prior to their UI claim tend to benefit from the TB Program. Roughly 23 percent of people (1,423 out of 6,178) in the middle-aged group earn less than this amount. For this group of 1,423 people, the expected net impact of the TB Program is positive. Older people that earned less than $17,858 tend to benefit from the TB Program. Roughly 9 percent of people (555 out of 6,420) in the older group earn less than this amount. For this group of 555 older people, the expected net impact of the TB Program is positive.

Figure 4-6. Distribution of the TB Program's predicted net impact on earnings post training by age group (measured in 2016 dollars)
Washington state, 2004 through 2013 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

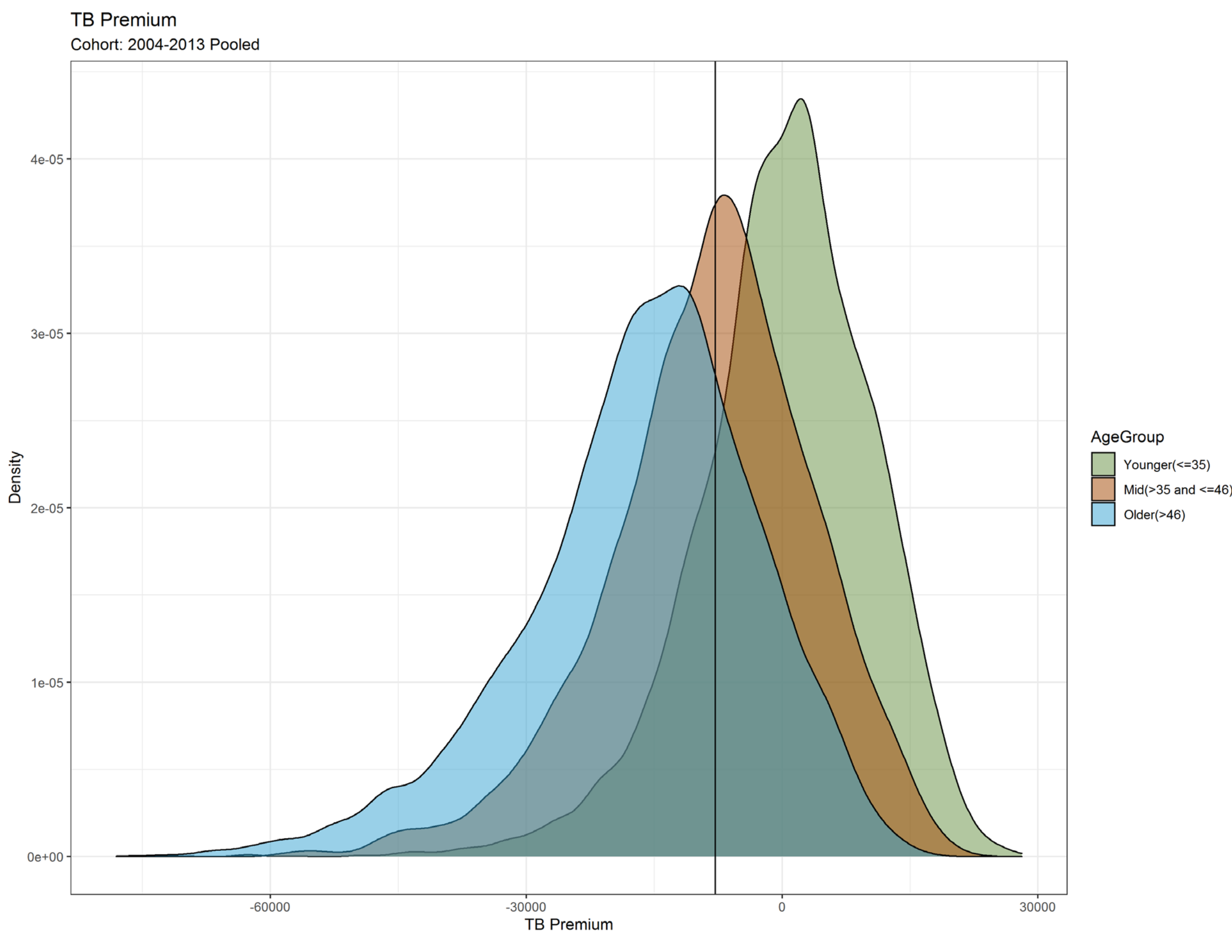


Figure 4-7. Distribution of the TB program's predicted net impact on earnings post training by age group (measured in 2016 dollars)
Washington state, 2002 cohort
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

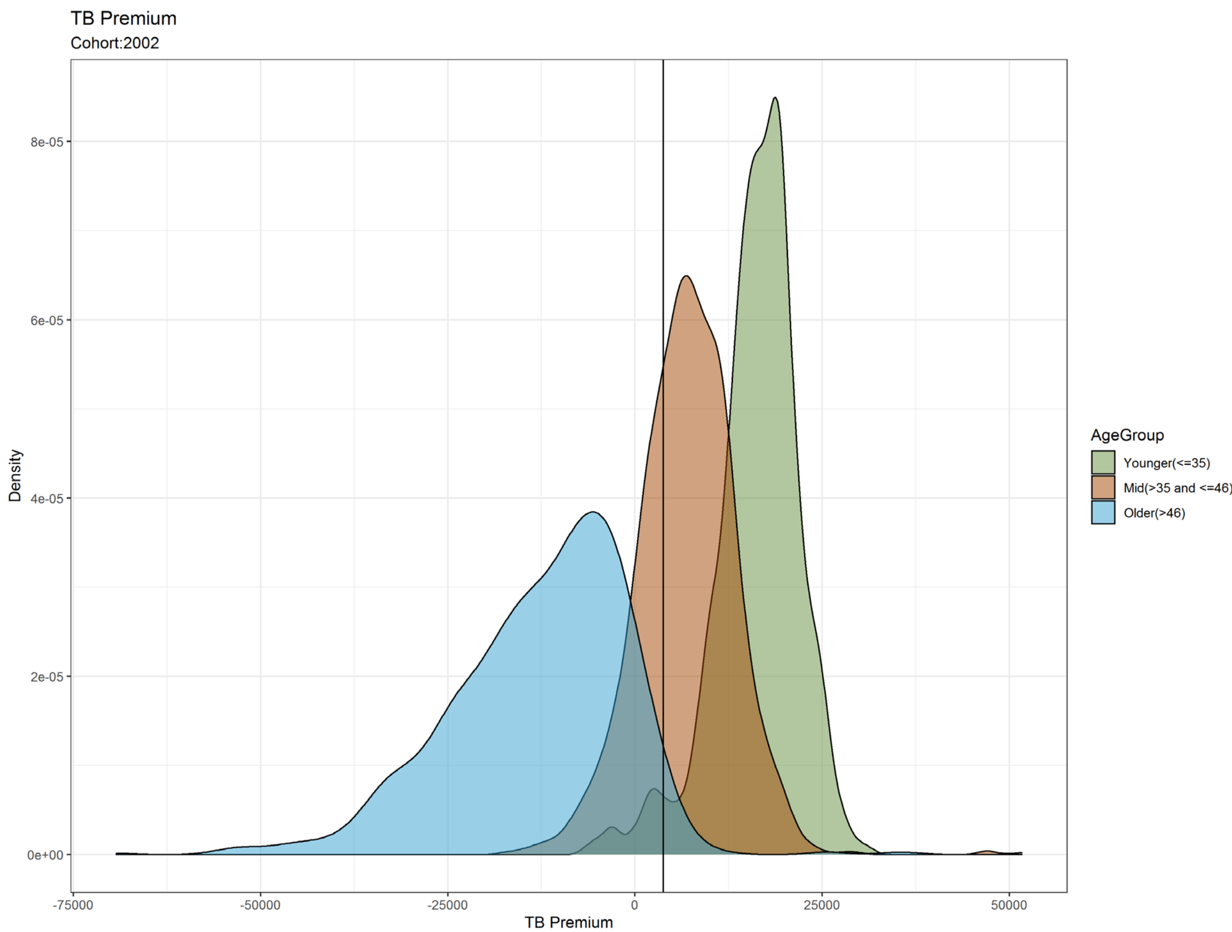


Figure 4-8. Distribution of the TB program's predicted net impact on earnings post training by age group (measured in 2016 dollars)
Washington state, 2006 cohort
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

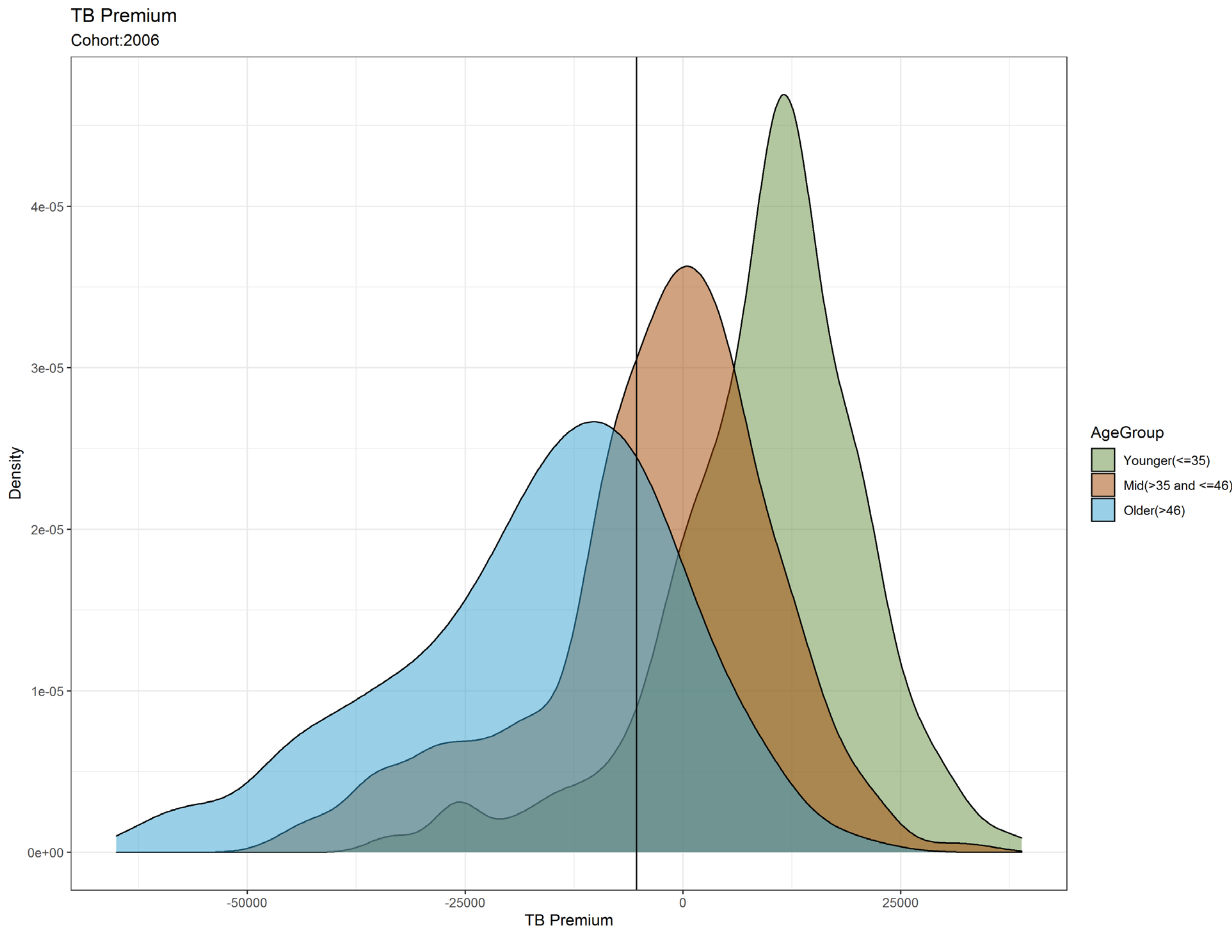


Figure 4-9. Distribution of the TB Program's predicted net impact on earnings post training by age group (measured in 2016 dollars)
Washington state, 2014 cohort
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

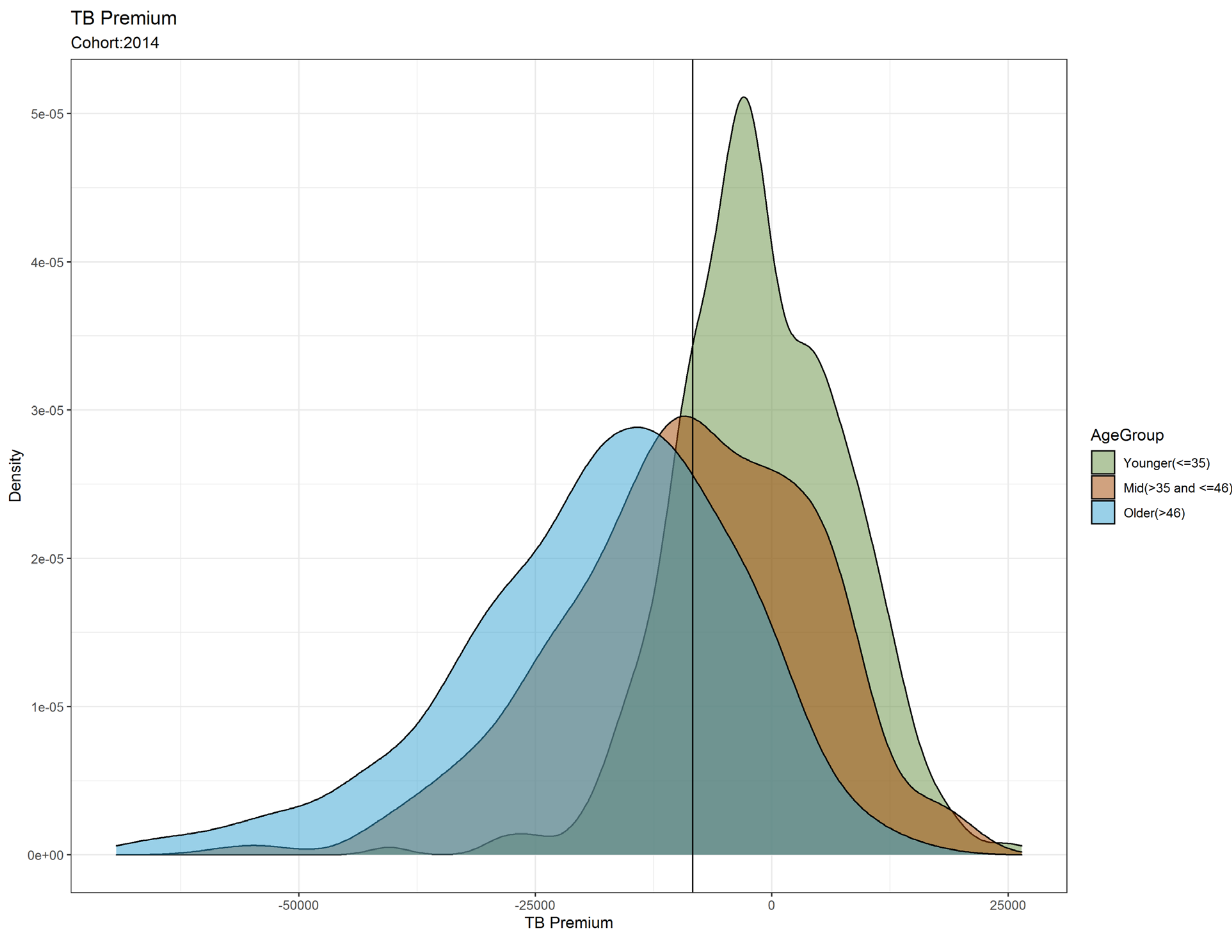


Figure 4-10. The TB Program's predicted net impact on earnings post training by age group, with lines of best fit for each group (measured in 2016 dollars)
Washington state, 2004 through 2013 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

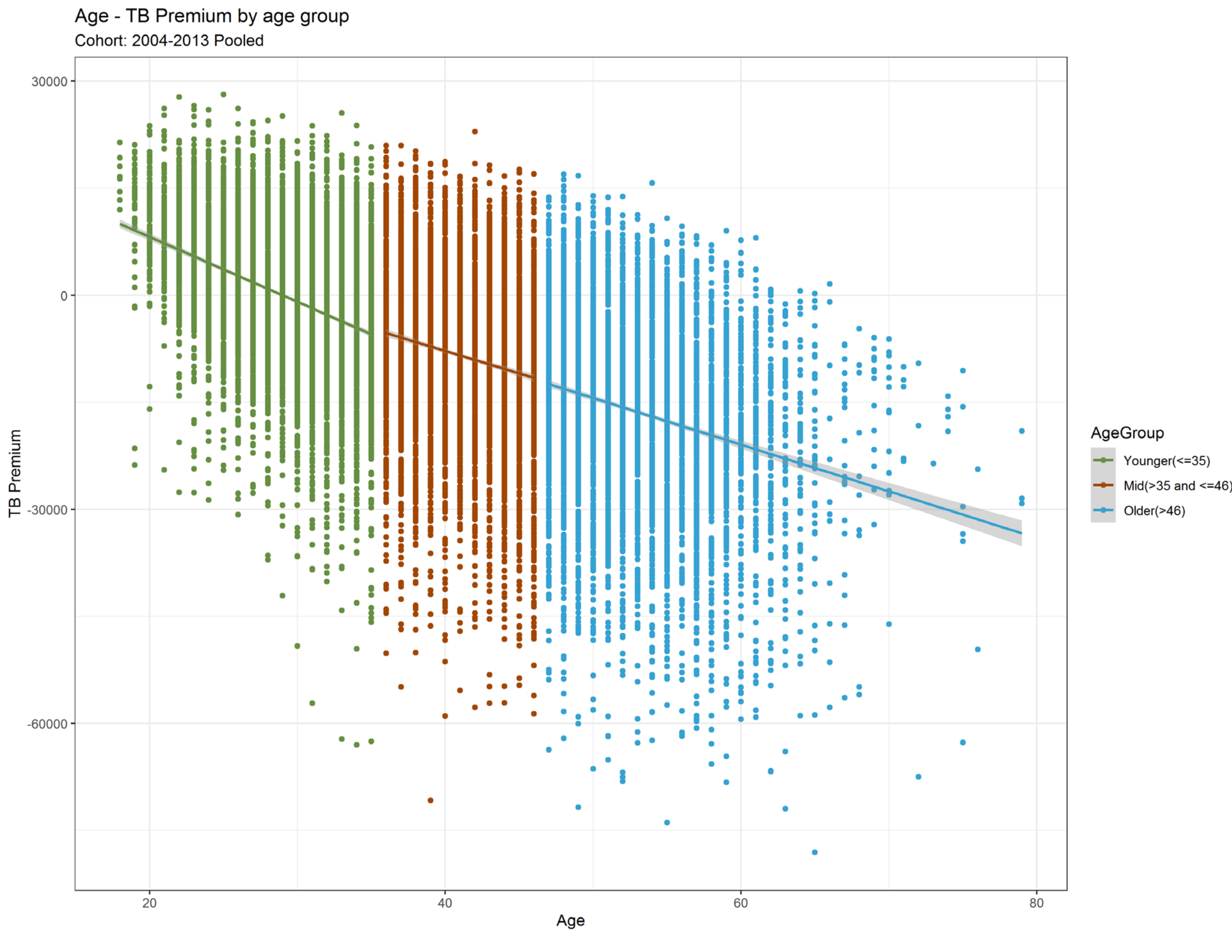
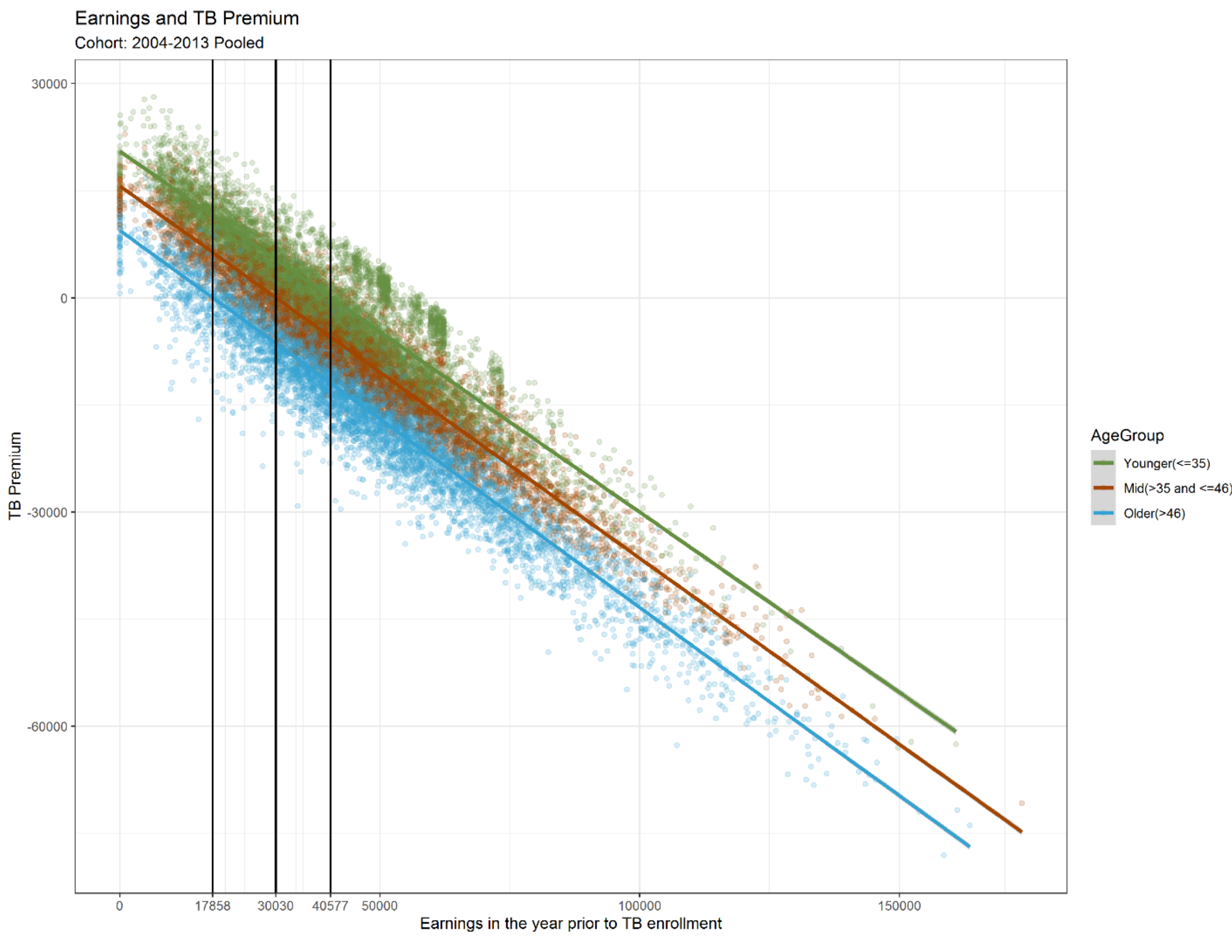


Figure 4-11. How the TB Program predicted net impact on earnings post training varies with observable characteristics, with lines of best fit for each age group (measured in 2016 dollars)
Washington state, 2004 through 2013 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016



These results are based on the DID regressions with interactions, described in *Chapter 2*. We present the results of those regressions in *Figure 4-12* for the "representative" cohorts we study – 2002, 2006, and 2014 – excluding the industry fixed effect estimates. Age and earnings in the year prior to the UI claim jump out as key explanatory variables. They have strong correlations with the impact of the TB Program on a person's annual earnings. The other parameters we control for in these regressions have weaker and sporadic relationships. Being male is positively correlated with TB premiums for the 2006 cohort, but not the 2002 or 2014 cohorts.

Figure 4-12. How the TB Program predicted net impact on earnings post training varies with observable characteristics
Washington state; 2002, 2006 and 2014 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	2002	2006	2014
Treatment variable	($5,530)	**$38,346**	**$23,117**
Standard error	8,374.93	9,755.00	5,632.91
Age interactions			
Age interaction	**$2,071**	($59)	$222
Standard error	368.52	413.69	242.62
Age squared interaction	**($40)**	**($9)**	**($8)**
Standard error	4.36	4.721	2.93
Education interactions			
Some college	($2,122)	($2,647)	**($2,919)**
Standard error	1,370.59	1,791.97	1,206.36
High school education or	($2,045)	($1,238)	**($2,650)**
Standard error	1,373.89	1,652.75	1,068.59
GED	**($5,201)**	($3,118)	($221)
Standard error	2,617.59	3,172.99	1,677.50
No formal education	$2,249	$5	($146)
Standard error	6,723.22	3,203.01	2,558.99
Lagged income interaction			
Lagged earnings	**-0.15**	**-0.54**	**-0.48**
Standard error	0.02	0.04	0.03
Gender interaction			
Male	$1,362	**$4,589**	$1,513
Standard error	964.9	1,434.98	967.24

Notes: Bold denotes $p < 0.05$.
The interaction is with the TB Program indicator variable. The interpretation of the coefficient estimates is that the premium increases (or decreases) by that amount with a change in the independent variable. The excluded education category is "college educated" so that the education interaction results describe the correlation between that education level and the TB premium, relative to the college educated. An example of how to interpret the coefficients: people with a GED education have a premium that is $5,200 less than the premium for college educated individuals.

Programs of study that increase younger people's lifetime earnings

We report the results from the following regression, for the whole population and for people younger than 36, in columns two and three of *Figure 4-13:*

$$premium_i = \beta CIP_i + \nu_i.$$

We present regression coefficients and standard errors for the top 20 most popular courses of study. The coefficients give the average net impact of the TB Program on annual earnings for people that pursued that course of study. The weighted average of the coefficients is equal to the population's annual ITT estimate. Since the average TB participant in the 2004 to 2013 cohorts lost a large amount of money per year because of the TB Program, the coefficient estimates are negative. People who took courses in protective services, healthcare, and education fared *relatively* well. The average net impact of the TB Program on earnings is still negative for these participants, but it is relatively high compared to other TB participants.

We also fit the regression using data from people under the age of 36. This group benefitted from the TB Program. As such, some of their coefficients are positive and some are negative. For instance, young people who studied to be mechanics have higher premiums, but those who studied business management have lower premiums. Young people who took healthcare courses benefitted from the TB Program, on average. However, young people that studied information services have a negative average premium. Those who took classes in education also had positive ITT estimates on average.

Figure 4-13. How the ITT on earnings post training varies with course of study sought by TB participants
Washington state, 2004 through 2013 TB participants
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016; ERDC

Course of study	Full sample	Youths
Health professions and related programs	**($5,851)**	**$745**
Standard error	196.02	237.118
Business, management, marketing, and support services	**($9,929)**	**($1,006)**
Standard error	227.99	319.326
Computer and information sciences and support services	**($10,346)**	**($615)**
Standard error	280.23	358.32
Engineering technologies and related fields	**($8,615)**	**($1,435)**
Standard error	423.73	529.93
Mechanic and repair technologies	**($7,406)**	**$1,509**
Standard error	454.34	524.35
Precision production	**($6,989)**	$755
Standard error	555.22	650.769
Homeland security, law enforcement, firefighting, and related protective services	**($3,955)**	($196)
Standard error	709.38	656.082
Legal professions and studies	**($7,872)**	$1,379
Standard error	761.6	995.405

Course of study	Full sample	Youths
Education	**($5,149)**	**$2,175**
Standard error	809.7	972.728
Personal and culinary services	**($6,554)**	($692)
Standard error	857.87	1,024.83
Construction trades	**($11,812)**	($1,248)
Standard error	861.28	1,086.32
Transportation and materials moving	**($7,357)**	**$2,741**
Standard error	984.76	1,242.79
Natural resources and conservation	**($7,494)**	($387)
Standard error	1,099.56	1,369.49
Communications technologies and support services	**($5,987)**	$1,944
Standard error	1,149.13	1,449.33
Agriculture, agricultural operations, and related sciences	**($8,170)**	$3,641
Standard error	1,215.61	1,562.86
Visual and performing arts	**($9,641)**	**($4,532)**
Standard error	1,295.15	1,464.05
Parks, recreation, leisure, and fitness studies	**($7,568)**	$3,192
Standard error	1,856.88	2,485.58
Social sciences	**($7,447)**	$1,411
Standard error	2,131.03	2,485.58
Science technologies and technician	**($5,494)**	$32
Standard error	2,306.46	2,485.58
Library arts and sciences, general studies and humanities	**($5,958)**	$2,437
Standard error	2,845.22	4,183.86

Chapter 5: Net impact of the TB Program on employment

While TB participants train, they are less likely than their peers to be employed. Over time, TB participants catch up with, and surpass, their peers in their likelihood of being employed. We present these results in *Figures 5-1* through *5-3*. These estimates are the difference-in-differences model results using the PSM group for comparison. For each cohort, we provide estimates of program effects for all available follow-on years. Coupled with the results presented in *Chapter 4*, these findings suggest that the TB Program participants are more likely to take jobs with lower wages after the lock-in period ends.

Net impact of the TB Program on employment for all cohorts

When we compare the percent of time employed for TB participants to control group members' percent of time employed in all years, we see that participants are 4 percent less likely to work than the control group. This is because there are large reductions in employment while training. Later, TB participants become slightly more likely to be employed than members of the control group but overall, in all follow-on years, TB participants are slightly less likely to work than control group members. This is not a negative effect of the program *per se*, as a reduction in time employed while training is to be expected. The negative estimate for all follow-on years reflects the large decrease in employment during the lock-in period and the modest gains in the subsequent follow-on years.

Net impact of the TB Program on employment for the 2002 and 2003 cohorts

In *Figure 5-1*, we present the results for the TB net impact on employment the 2002 and 2003 cohorts. We report results that are statistically significant at the 95 percent level in bold. We present the bootstrap standard errors below the estimates.

The TB participants in the 2002 cohort were less likely to be employed than their peers in the first three follow-on years. During the program and in the first follow-on year, respectively, they were 23.3 percent and 34.1 percent less likely to work than the control group members. Starting in follow-on year three, the TB participants became more likely to be employed than their peers in the control group. In follow-on year three, the TB participants were 6.9 percent more likely than their peers to work. In follow-on years four through 16, they are consistently about 10 percent more likely to be employed than the control group members.

The story is the similar for the 2003 cohort. They are initially less likely to be employed while they seek training. Then, starting in follow-on year three, they start to have a higher likelihood of being employed than their peers in the control group. Compared to the control group, in each of the follow-on years from four to 15, they are 7.6 percent more likely to work on average. This is a smaller net benefit than the 2002 cohort enjoys, but it is still economically meaningful.

The TB participants in the 2002 and 2003 cohorts are (1) more likely to be employed than the control group members after the initial "lock-in" years, *and* (2) earn more than the control group members after the initial "lock-in" years.

Figure 5-1. TB Program net impact on employment by follow-on year
Washington state, 2002 and 2003 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	**-23.30%**	**-34.10%**	**-2.10%**	**6.90%**	**9.30%**	**9.80%**	**9.70%**	**9.90%**	**10.70%**	**10.70%**	**10.50%**	**10.50%**	**10.80%**	**10.90%**	**9.90%**	**9.60%**	**8.50%**
Bootstrapped standard error	0.60%	0.90%	0.90%	1.00%	0.90%	1.00%	1.00%	1.10%	1.00%	1.10%	1.10%	1.20%	1.20%	1.20%	1.20%	1.10%	1.10%
2003	**-22.90%**	**-19.40%**	0.70%	**6.00%**	**8.30%**	**8.40%**	**9.20%**	**9.20%**	**8.50%**	**8.80%**	**8.30%**	**7.20%**	**6.90%**	**6.40%**	**5.50%**	**4.20%**	
Bootstrapped standard error	0.80%	1.10%	1.20%	1.30%	1.30%	1.20%	1.20%	1.20%	1.20%	1.20%	1.20%	1.20%	1.30%	1.40%	1.40%	1.50%	

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations

Net impact of the TB Program on employment for the 2004 and 2005 cohorts

The 2004 and 2005 cohorts have similar outcomes to the 2003 cohort. For these cohorts, the initial "lock-in" effect makes participants less likely to work than the control group members. Then, in follow-on years four through 13, they are roughly seven percent more likely to work than the control group members. For the 2003 to 2005 cohorts, the initial training investment pays off. They are more likely than their peers to work starting in the third or fourth follow-on year, and from then on out (as far as our data show).

However, the TB participants' earnings in the 2004 and 2005 cohorts never surpass the control group members' earnings (*Figure 4-2*). Combined with the evidence in *Figure 5-2*, these findings suggest that these TB participants opt to take lower paying jobs after the lock-in period. This supports the hypothesis that people use the training to make an occupational shift. They may gain accreditation or skills in training that qualify them to take entry-level jobs in a new industry. If this industry is an "in demand" industry while the one they left due to layoffs is not "in-demand," then this occupational shift is in accordance with TB Program goals.[24]

Figure 5-2. TB Program net impact on employment by follow-on year
Washington state, 2004 and 2005 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year														
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**
2004	**-27.10%**	**-21.00%**	-3.20%	**3.40%**	**6.20%**	**6.70%**	**6.70%**	**6.70%**	**6.50%**	**7.40%**	**7.40%**	**6.70%**	**6.30%**	**6.10%**	**6.80%**
Bootstrapped standard error	1.30%	1.70%	1.70%	1.70%	1.70%	1.80%	2.00%	2.00%	2.20%	2.00%	2.40%	2.30%	2.30%	2.20%	2.20%
2005	**-29.70%**	**-20.50%**	-1.20%	3.00%	**5.70%**	**5.70%**	**7.20%**	**7.60%**	**6.90%**	**5.90%**	**6.60%**	**6.90%**	**7.10%**	**7.00%**	
Bootstrapped standard error	1.20%	1.70%	1.60%	1.60%	1.70%	1.60%	1.60%	1.70%	1.70%	2.00%	2.00%	2.00%	2.00%	2.00%	

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

24 An alternative explanation is that members of the control group gain on-the-job training and skills while members of the TB group gain training credits and skills. It could be that, from employers' perspectives, these types of training are comparable.

Net impact of the TB Program on employment for the 2006 through 2016 cohorts

The 2006 to 2016 cohorts have mixed results (*Figure 5-3*). For instance, the 2006 cohort has a lock-in effect, then a positive effect in follow-on years four to seven, then no effect thereafter. In contrast, the 2007 cohort has a lock-in effect, then no effect in follow-on years three to seven, then a positive effect thereafter. These opposing patterns in back-to-back years make it challenging to draw a broad conclusion about the program effects for cohorts in 2006 to 2016. The one clear take-away is that all cohorts experience a lock-in effect in the first several years. Some enjoy small premiums in certain follow-on years. While not always significant at the 95 percent confidence level, the point estimates of the TB Program's net impact on employment probability are consistently positive after follow-on year four.

Overall, these cohorts are less likely to be employed than their peers across the observed follow-on years. The premiums they enjoy in later years are modest, but the lock-in effects are large. As such, the modest benefits of the program do not offset the large investment.

This evidence of modest benefits in later follow-on years corroborates the findings in the Aviles et al. (2015) analysis. In that study, they hypothesize that TB participants use the training opportunity to transition occupations. They gain new skills that let them take entry level jobs in occupations they may prefer to work in, or which may be in greater demand.

Figure 5-3. TB Program net impact on percent of time employed by follow-on year
Washington state, 2006 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

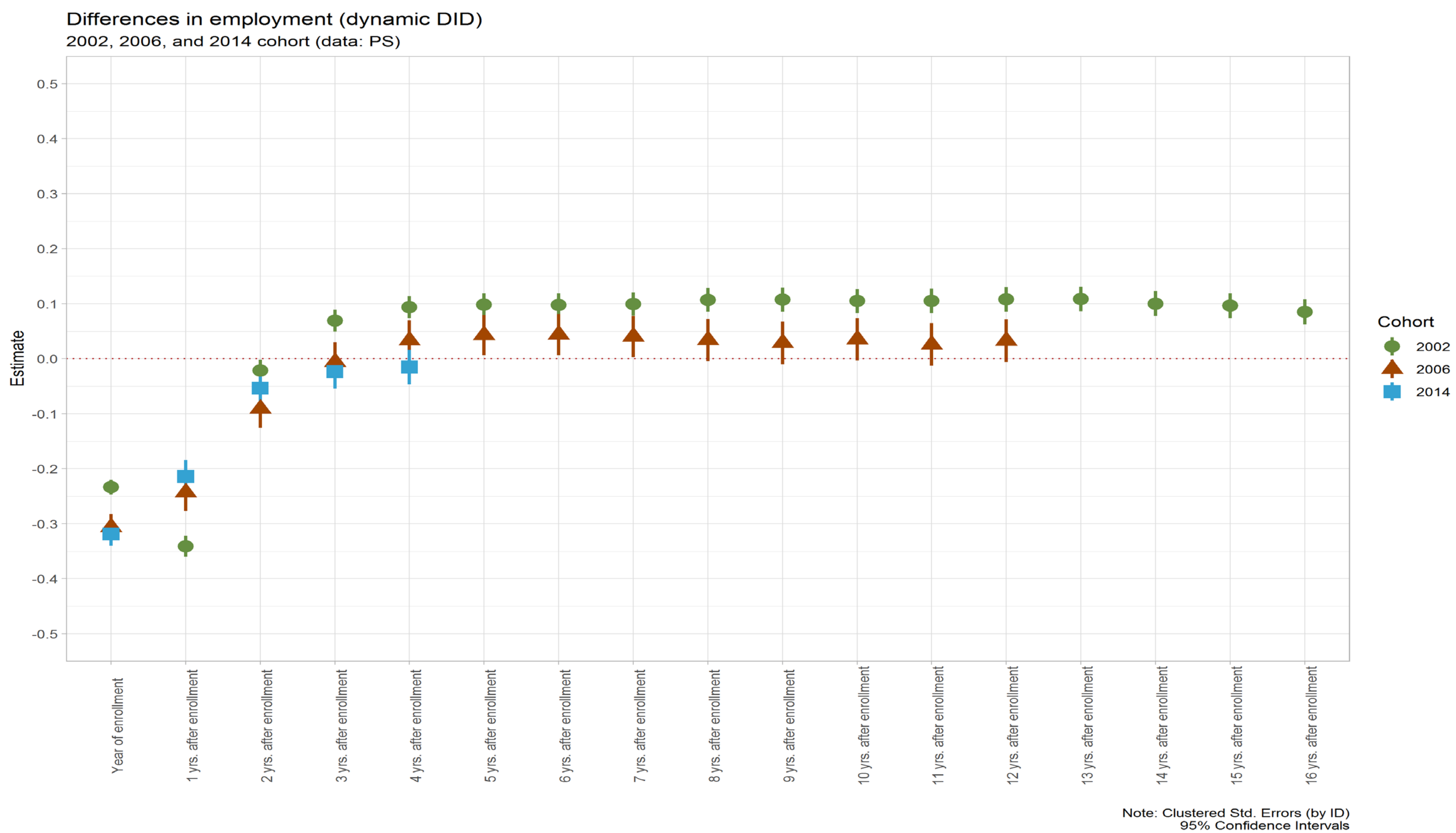
	Follow-on year												
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**
2006	**-30.60%**	**-24.30%**	**-9.10%**	**-0.60%**	**3.30%**	**4.20%**	**4.30%**	**4.00%**	3.30%	2.80%	3.50%	2.50%	3.20%
Bootstrapped standard error	1.30%	1.70%	1.90%	1.80%	1.70%	1.70%	1.70%	1.80%	1.80%	1.80%	1.80%	1.90%	2.00%
2007	**-31.20%**	**-30.50%**	**-12.10%**	-2.80%	0.60%	0.30%	1.20%	3.10%	**5.00%**	**4.90%**	**4.70%**	**6.10%**	
Bootstrapped standard error	1.20%	1.80%	1.90%	2.00%	2.00%	1.90%	2.00%	2.30%	2.30%	2.30%	2.20%	2.30%	
2008	**-27.40%**	**-38.30%**	**-16.90%**	-2.30%	0.70%	0.90%	1.60%	2.10%	1.60%	0.60%	0.70%		
Bootstrapped standard error	0.90%	1.30%	1.20%	1.30%	1.40%	1.30%	1.30%	1.40%	1.50%	1.50%	1.50%		
2009	**-26.70%**	**-34.70%**	**-11.90%**	0.80%	**1.80%**	**2.60%**	**3.80%**	**3.70%**	**3.50%**	**3.40%**			
Bootstrapped standard error	0.60%	0.80%	0.90%	0.90%	0.80%	0.90%	0.9	0.90%	0.90%	0.90%			
2010	**-30.60%**	**-35.80%**	**-12.30%**	-1.50%	1.30%	1.80%	**2.70%**	2.50%	2.50%				
Bootstrapped standard error	0.80%	1.20%	1.10%	1.10%	1.20%	1.20%	1.30%	1.40%	1.30%				
2011	**-31.00%**	**-34.10%**	**-9.30%**	0.70%	2.10%	**2.90%**	**3.70%**	**3.70%**					
Bootstrapped standard error	1.00%	1.20%	1.20%	1.20%	1.40%	1.40%	1.40%	1.40%					

	Follow-on year												
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**
2012	**-32.50%**	**-31.00%**	**-7.80%**	0.40%	2.20%	**3.00%**	**2.90%**						
Bootstrapped standard error	0.90%	1.10%	1.20%	1.30%	1.30%	1.40%	1.40%						
2013	**-29.20%**	**-22.50%**	**-5.00%**	0.07%	2.00%	**4.20%**							
Bootstrapped standard error	1.00%	1.30%	1.60%	1.50%	1.60%	1.70%							
2014	**-31.90%**	**-21.40%**	**-5.30%**	-2.30%	-1.50%								
Bootstrapped standard error	1.10%	1.40%	1.50%	1.60%	1.50%								
2015	**-31.60%**	**-22.90%**	**-6.80%**	-1.50%									
Bootstrapped standard error	1.20%	1.60%	1.70%	1.90%									
2016	**-34.30%**	**-26.00%**	**-6.40%**										
Bootstrapped standard error	1.30%	1.80%	1.90%										

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

In addition to reporting the results in tables, we provide results for three representative cohorts – 2002, 2006 and 2014 – in a plot in *Figure 5-4*. This information is redundant since the results are provided in the tables, but the plots may make it easier to under the results visually.

Figure 5-4. TB Program net impact on employment
Washington state, 2002, 2006 and 2014 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016



Chapter 6: Net impact of the TB Program on training

The TB Program has a clear and positive effect on the probability that participants train in the first three years. On average, in the first year, TB participants are 64 percent more likely to train than control group members. Some control group members train, but not many. Some who sign up for the TB Program do not train, but then they lose eligibility for the additional UI benefits and are removed from the program. In general, the program successfully increases the chances that people seek new skills and knowledge by attempting to earn training course credits.

We present these results in *Figures 6-1* through 6-*3*. These estimates are the difference-in-differences model results using the PSM group for comparison. For each cohort, we provide estimates of program effects for all available follow-on years.

Net impact of the TB Program on course credits attempted for all years

The lifetime effect of the TB Program is to increase the likelihood of training by 17.6 percent annually. Most of the effect is in the early "lock-in" years and the effect diminishes over time. For a few cohorts, though, the TB Program increases the likelihood of training later too. In all, the TB Program accomplishes its goal of encouraging participants to gain additional training. The skills and accreditation they acquire, while they do not increase participants' earnings potential, may help them shift from a contracting sector to a growing sector. This occupational change may be valuable to the participants, though we lack the data to test this hypothesis.

Net impact of the TB Program on course credits attempted for the 2002 and 2003 cohorts

The TB Program has a large, positive, and persistent effect on course credits attempted for the 2002 and 2003 cohorts (*Figure 6-1*). In the first eight follow-on years for both cohorts, there is a positive and statistically significant program effect. For the 2002 cohort, there are positive effects in follow-on years 12 through 15. For the 2003 cohort, there are significant program effects in follow-on years 14 and 15. Note that we do not have data to understand the program effects in 2002 or 2003.

Figure 6-1. TB Program net impact on course credits attempted by follow-on year
Washington state, 2002 through 2003 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year																
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**
2002	N/A	N/A	**18.30%**	**6.60%**	**3.90%**	**3.10%**	**2.90%**	**1.70%**	**1.30%**	1.10%	0.40%	0.30%	**0.70%**	**0.50%**	**0.80%**	**0.60%**	0.02%
Bootstrapped standard error	N/A	N/A	0.60%	0.60%	0.60%	0.50%	0.40%	0.40%	0.40%	0.30%	0.30%	0.30%	0.30%	0.20%	0.20%	0.20%	7.00%
2003	N/A	**46.90%**	**17.60%**	**6.00%**	**3.10%**	**3.50%**	**1.90%**	**1.80%**	**1.30%**	0.70%	0.80%	0.60%	0.50%	0.40%	**0.70%**	**0.30%**	
Bootstrapped standard error	N/A	1.10%	0.80%	0.70%	0.70%	0.60%	0.60%	0.50%	0.50%	0.50%	0.40%	0.40%	0.40%	0.30%	0.30%	0.10%	

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Net impact of the TB Program on course credits attempted for the 2004 and 2005 cohorts

During the program, the 2004 TB participants are 63.7 percent more likely to seek training, and the 2005 TB participants are 64.8 percent more likely (*Figure 6-2*). The "lock"-in effect persists in the first follow-on year but diminishes slightly. In follow-on year 2, the TB participants are still more likely to train than the control group, but only by 14.1 and 10.5 percent for the two cohorts respectively. For the 2005 cohort, the program effect becomes negative starting in year four. It becomes negative starting in follow-on year seven for the 2004 cohort. Thereafter, for both cohorts, the TB Program decreases the likelihood that participants seek training. The TB participants are less likely to train than the control group after the lock-in period ends. However, because the positive effect is so large during the lock-in period, the TB Program still increases the lifetime probability that participants in the 2004 and 2005 cohorts attempt training.

Figure 6-2. TB Program net impact on course credits attempted by follow-on year
Washington state, 2004 through 2005 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year													
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**10**	**11**	**12**	**13**	**14**
2004	**63.70%**	**39.60%**	**14.10%**	**3.90%**	1.40%	-1.10%	-1.60%	**-3.60%**	**-5.10%**	**-4.50%**	**-4.10%**	**-4.70%**	**-3.90%**	**-5.20%**
Bootstrapped standard error	1.60%	1.90%	1.80%	1.50%	1.20%	1.20%	1.30%	1.20%	1.10%	1.00%	1.00%	1.10%	1.00%	0.90%
2005	**64.80%**	**37.60%**	**10.50%**	-2.10%	**-6.40%**	**-5.90%**	**-7.20%**	**-9.40%**	**-8.40%**	**-9.80%**	**-10.10%**	**-9.20%**	**-9.60%**	
Bootstrapped standard error	1.60%	1.90%	1.70%	1.60%	1.60%	1.40%	1.30%	1.30%	1.20%	1.30%	1.20%	1.20%	1.10%	

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Net impact of the TB Program on course credits attempted for the 2006 through 2016 cohorts

As seen in *Figure 6-3*, the results in the 2006 through 2016 cohorts follow the same pattern as the 2005 results reliably. The lock-in effect is large: the participants are much more likely to seek training than the control group members during the program and in the first two follow-on years. Starting in follow-on year four or five, the TB Program decreases the likelihood that participants seek training. Thereafter, TB participants are less likely to seek training than the control group members. However, because the lock-in effects are so big, the net effect of the program is still positive over the course of the follow-on years we observe.

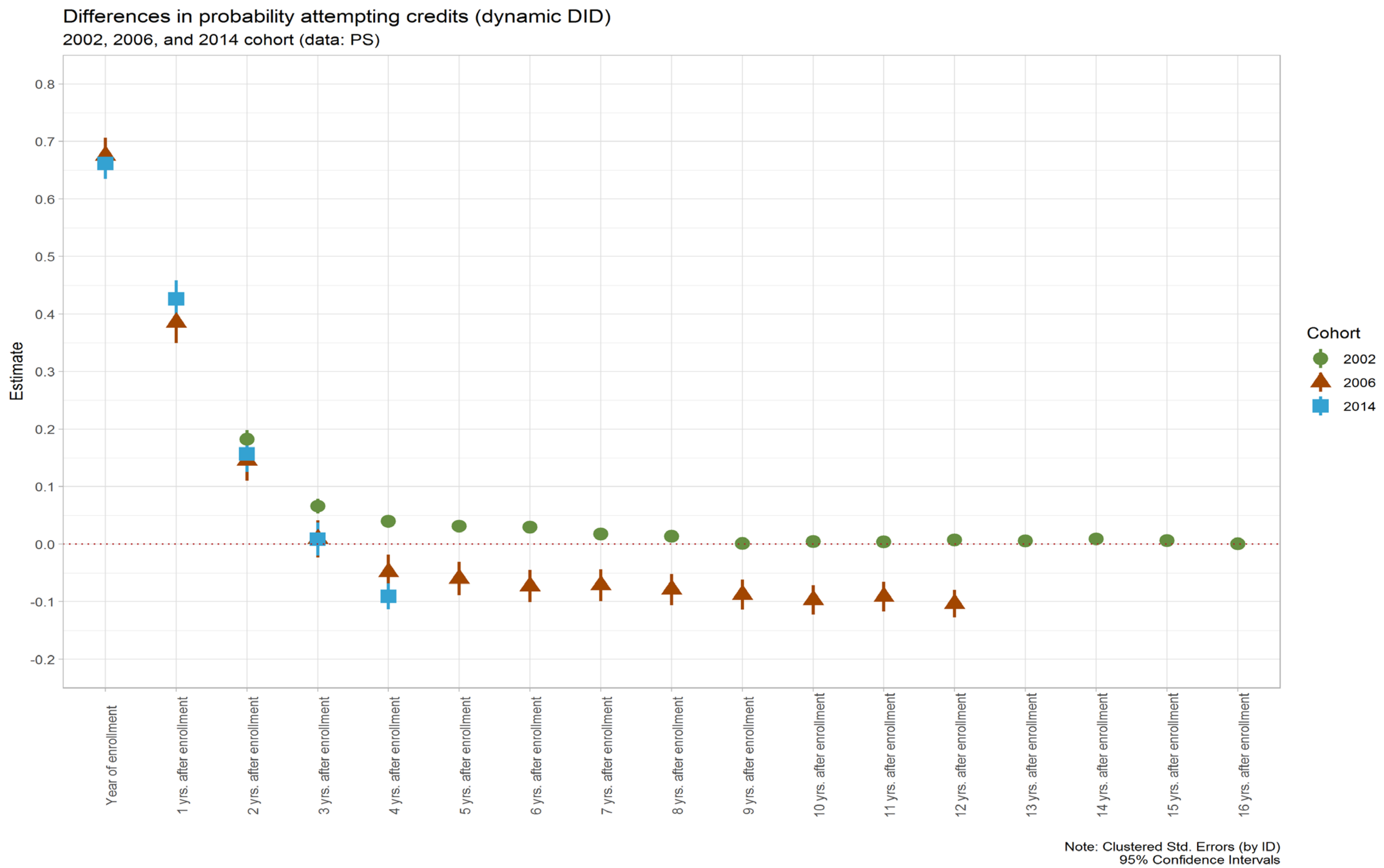
Figure 6-3. TB Program net impact on course credits attempted by follow-on year
Washington state, 2006 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year												
Cohort	0	1	2	3	4	5	6	7	8	9	10	11	12
2006	**67.50%**	**38.50%**	**14.50%**	0.90%	**-4.80%**	**-6.00%**	**-7.30%**	**-7.10%**	**-7.90%**	**-8.80%**	**-9.70%**	**-9.10%**	**-10.40%**
Bootstrapped standard error	1.50%	1.80%	1.80%	1.70%	1.70%	1.60%	1.50%	1.60%	1.60%	1.50%	1.50%	1.50%	1.30%
2007	**67.80%**	**39.50%**	**16.80%**	0.70%	**-5.00%**	**-5.40%**	**-6.40%**	**-8.40%**	**-8.40%**	**-8.70%**	**-9.10%**	**-10.00%**	
Bootstrapped standard error	1.60%	2.00%	2.10%	1.90%	1.80%	1.50%	1.50%	1.50%	1.40%	1.50%	1.50%	1.40%	
2008	**70.40%**	**55.30%**	**27.60%**	**4.70%**	-0.4	**-3.00%**	**-4.60%**	**-4.20%**	**-5.00%**	**-5.30%**	**-5.20%**		
Bootstrapped standard error	1.30%	1.40%	1.40%	1.30%	1.30%	1.10%	1.20%	1.00%	1.00%	1.00%	0.90%		
2009	**64.80%**	**50.10%**	**20.10%**	1.40%	**-3.10%**	**-5.90%**	**-6.70%**	**-7.30%**	**-7.10%**	**-9.00%**			
Bootstrapped standard error	0.90%	1.00%	1.10%	0.90%	0.90%	0.80%	0.90%	0.80%	0.80%	0.70%			
2010	**65.50%**	**51.40%**	**23.70%**	**5.00%**	-0.50%	**-2.00%**	**-3.20%**	**-4.60%**	**-6.20%**				
Bootstrapped standard error	1.10%	1.00%	1.10%	1.10%	1.00%	1.00%	1.00%	0.90%	0.80%				
2011	**64.80%**	**47.30%**	**17.90%**	2.00%	-1.80%	**-3.90%**	**-7.00%**	**-8.60%**					
Bootstrapped standard error	1.20%	1.40%	1.50%	1.40%	1.20%	1.10%	1.00%	1.00%					
2012	**65.40%**	**46.00%**	**16.30%**	1.80%	**-2.30%**	**-5.10%**	**-9.10%**						
Bootstrapped standard error	1.30%	1.40%	1.40%	1.30%	1.20%	1.20%	0.90%						
2013	**67.10%**	**44.70%**	**15.70%**	**3.20%**	-1.80%	**-9.40%**							
Bootstrapped standard error	1.50%	1.80%	1.70%	1.60%	1.40%	1.30%							
2014	**66.20%**	**42.60%**	**15.70%**	0.90%	**-9.10%**								
Bootstrapped standard error	1.60%	1.70%	1.70%	1.50%	1.20%								
2015	**66.20%**	**41.90%**	**14.80%**	**-7.50%**									
Bootstrapped standard error	1.50%	1.90%	1.60%	1.20%									
2016	**68.40%**	**46.10%**	**4.70%**										
Bootstrapped standard error	1.90%	2.10%	2.00%										

Notes: t-test is performed for the comparison pool and PSM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

In addition to reporting the results in tables, we provide results for three representative cohorts – 2002, 2006 and 2014 – in a plot in *Figure 6-4*. This information is redundant since the results are provided in the tables, but the plots may make it easier to under the results visually.

Figure 6-4. TB Program net impact on employment
Washington state, 2002, 2006 and 2014 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016



References

Andersson, F., Holzer, H. J., Lane, J. I., Rosenblum, D., & Smith, J. (2013). Does federally funded job training work? Nonexperimental estimates of WIA training impacts using longitudinal data on workers and firms (No. w19446). *National Bureau of Economic Research.*

Aviles, Gustavo, Cassie Bordelon, Andrew Greenwell, Linda Pham, Adrian Sinkler, and Ernie Stromsdorfer. 2015. Net Impact Study of Annual Earnings for the Training Benefits Program 2002 through 2012, *Washington State Employment Security Department.*

Barnow, B. S., & Smith, J. (2015). Employment and training programs. In *Economics of Means-Tested Transfer Programs in the United States, Volume 2* (pp. 127-234). University of Chicago Press.

Doerr, A., Fitzenberger, B., Kruppe, T., Paul, M., & Strittmatter, A. (2017). Employment and earnings effects of awarding training vouchers in Germany. *ILR Review*, 70(3), 767-812.

Employment Security Department. 2020. *Employer Tax Handbook: Jan. 1 – Dec. 31, 2020*. Olympia, WA: n.p.

Green, D. P. and Gerber, A. S. 2012. Field Experiments: Design, Analysis, and Interpretation, New York, NY: W. W. Norton & Company.

Heinrich, C., Mueser, P. R., Troske, K. R., Jeon, K. S., & Kahvecioglu, D. (2009). New estimates of public employment and training program net impacts: A nonexperimental evaluation of the Workforce Investment Act program. *Unpublished Manuscript.*

Hollenbeck, K. 2009. Return on Investment Analysis of a Selected Set of Workforce System Programs in Indiana. *Report submitted to the Indiana Chamber of Commerce Foundation, Indianapolis, Indiana.*

Iacus, S. M., King, G., and Porro, G. 2012. Causal Inference without Balance Checking: Coarsened Exact Matching, *Political Analysis*, 20 (1): 1-24.

King, G. and Nielsen, R. 2019. Why Propensity Scores Should Not Be Used for Matching, *Political Analysis*, 27 (4): 435-454.

LaLonde, R. J. (2003). Employment and training programs. In *Means-tested transfer programs in the United States* (pp. 517-586). University of Chicago Press.

Rubin, D. B. 1980. Bias Reduction Using Mahalanobis-Metric Matching, *Biometrics*, 36 (2): 293-298.

Stuart, E. A. 2010. Matching methods for causal inference: A review and a look forward, *Statistical Science*, 25 (1): 1-21.

Wing, C., Simon, K., and Bello-Gomez, R. A. 2018. Designing Difference in Differences Studies: Best Practices for Public Health Policy Research, *Annual Review of Public Health*, 39: 453-469.

Appendices

Appendix 1: Difference-in-differences results for earnings using alternative matching methods

We present the dynamic difference-in-differences (DID) model results for earnings for each cohort using the Mahalanobis (MDM) matching control group (*Appendix figure A1-1*), the coarsened exact matching (CEM) control group (*Appendix figure A1-2*), and the denied applicants control group (*Appendix figure A1-3*) in this appendix. These results mirror those presented in *Chapter 4*, which obtain by using the propensity score matching (PSM) comparison group to estimate the dynamic DID model for earnings:

$$Y_{i,t} = a_i + b_t + \sum_{m=1}^{3} D_{i,t-m}\lambda_m + D_{i,t}\gamma + \sum_{s=1}^{S} D_{i,t+s}\delta_s + \varepsilon_{i,t}.$$

Here, $Y_{i,t}$ represents the earnings for person i in period t. See *Chapter 2* for a complete description of the equation and how to interpret the coefficient estimates. The results presented in this appendix are obtained from the same regression specification, using the three alternative comparison groups instead of the PSM comparison group.

The results are largely consistent across models. We study whether each cohorts' average TB participant breaks even, and if so, when. We compare this to the results from the DID model using the control group selected by the PSM method and presented in the main body of the text. As such, we make 45 comparisons in this appendix: one for each of the three models used as robustness checks, for each of the fifteen cohorts. Across these 45 comparisons, we find that the three alternative models provide corroborating evidence in 40 instances (89 percent).

The MDM results for earnings always agree with the PSM modeling results. The discrepancies in results when using the CEM and denied comparison groups are as follows:

1) The denied control group DID results suggest that the 2002 and 2003 cohorts did not benefit from the TB Program but, like the later cohorts, suffered long-term earnings losses because of their TB participation. As we discuss below, however, these results should be interpreted cautiously. The earnings pre-trends for the 2002 and 2003 treatment and denied groups are not parallel, and so nothing conclusive can be deduced from these robustness-check results.
2) Similarly, the CEM control group DID results suggest that the 2002 and 2003 cohorts did not benefit from the TB Program but, like the later cohorts, suffered long-term earnings losses because of their TB participation.
3) The CEM control group DID results suggest that the 2004 cohort benefitted from the TB Program, earning premiums in follow-on years six through 12 and 14. These premiums sum to $45,914. The 2004 cohort, according to this analysis, forwent $26,490 during their lock-in training period. They broke even in follow-on year 10, according to the CEM DID results.

The results for the 2005 through 2016 cohorts are consistent across models. Regardless of the comparison group we study, the DID model results show that the average TB participant earned substantially less than their peers during their training period and failed to enjoy premiums sufficient to break even on their investment. Their lifetime earnings are lower than their peers who chose not to participate in the TB Program.

We offer some words of caution: the parallel-trends assumption may not hold for the denied control group and the treatment group. This assumption is required for the DID results to have a causal interpretation. If the assumption does not hold, the control group does not provide a useful counterfactual outcome for the treatment group. One way to assess whether the parallel trends assumption is likely to hold is to analyze the differences in outcomes before the treatment is provided. In the equation above, the estimates of λ_1 and λ_2 provide this information. These correspond to the effect of the TB Program on earnings one year and two years before the program took effect. If the parallel trend assumption does hold, the estimates of these parameters should be indistinguishable from each other (and from zero). If the estimates of λ_1 and λ_2 are significantly different from zero, we cannot attribute differences observed in the average outcomes between the two groups to the TB Program alone. For the PSM DID results, these estimates are indistinguishable from zero. For the denied group, however, they are statistically different from zero at the 95 percent confidence level.

We report estimates of λ_1 and λ_2 for the PSM and denied control groups, for each cohort, in *Appendix figure A1-4*. For the PSM group, there is only one estimate that is statistically different from zero at the 95 percent confidence level. This is likely a false positive since we use a 95 percent confidence level for our statistical tests and have 30 tests.[25] In general, the PSM group has parallel pre-trends for earnings. It seems that the propensity score matching does a good job of constructing a useful control group for causal inference.[26]

On the other hand, all the pre-trends for the denied control group are significant for the 2002 to 2009 cohorts, and one of the two is significant for the 2013 cohort. This is systematic evidence that the denied control group does not give a reliable causal estimate of the program's net impact, particularly for the early cohorts. As such, the robustness checks provided by the denied group for earnings should be considered with caution. They disagree with the PSM results for the 2002 and 2003 cohorts, but this apparent lack of robustness may be a spurious correlation for the denied control group results.[27]

[25] With a 95 percent confidence level, the chance of a false positive is 5 percent. With 20 tests, then, the statistician expects one false positive. With 30, the statistician expects 1.5 false positives.

[26] The pre-trends are similarly parallel for the MDM and CEM control group analyses.

[27] Note that the program changed in 2009 because of Engrossed Substitute House Bill 1906 (ESHB 1906). This bill expanded eligibility to individuals whose hourly wage is less than 130 percent of the state's minimum wage. This likely increased the proportion of applicants with low incomes and changed the composition of the denied group. In fact, the denied group had higher average earnings prior to their UI spell from 2002 through 2009, and similar earnings prior to their UI spell after 2009. This is consistent with the larger portion of low-income applicants after 2009 changing the composition of the denied group.

Appendix figure A1-1. TB Program net impact on earnings by follow-on year (measured in 2016 dollars) – MDM control group
Washington state, 2002 through 2003 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2006

	Follow-on year																
Cohort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	**($14,324)**	**($25,119)**	**($14,120)**	**($4,199)**	**$3,079**	**$6,098**	**$6,589**	**$9,594**	**$11,531**	**$12,626**	**$12,008**	**$13,242**	**$12,843**	**$12,165**	**$10,655**	**$9,981**	**$9,585**
Bootstrapped standard error	575	771	870	789	815	855	943	911	914	964	1,018	1,043	1,128	1,175	1,223	1,160	1,159
2003	**($14,537)**	**($18,381)**	**($4,028)**	**$2,009**	**$4,094**	**$5,913**	**$8,607**	**$9,940**	**$10,647**	**$10,381**	**$11,001**	**$10,181**	**$9,440**	**$8,983**	**$7,867**	**$6,423**	
Bootstrapped standard error	602	814	871	952	975	985	1,018	1,063	1,102	1,125	1,139	1,197	1,185	1,248	1,329	1,346	
2004	**($101,542)**	**($16,037)**	**($6,921)**	**($1,809)**	$729	$1,788	$2,788	**$3,213**	**$4,062**	**$4,281**	**$4,213**	**$4,042**	**$3,841**	$3,099	$2,991		
Bootstrapped standard error	914	1,220	1,306	1,307	1,275	1,307	1,443	1,523	1,500	1,493	1,563	1,612	1,627	1,666	1,715		
2005	**($11,994)**	**($16,059)**	**($7,110)**	**($2,572)**	$181	$1,596	$2,581	**$4,609**	**$5,535**	**$5,403**	**$5,462**	**$5,683**	**$5,279**	**$4,888**			
Bootstrapped standard error	1,024	1,197	1,247	1,272	1,341	1,393	1,416	1,431	1,467	1,375	1,482	1,584	1,610	1,711			
2006	**($13,471)**	**($18,147)**	**($11,294)**	**($7,964)**	**($4,965)**	**($2,699)**	($2,439)	($1,940)	($2,042)	($1,713)	($293)	$29	$1,079				
Bootstrapped standard error	803	1,065	1,164	1,228	1,328	1,356	1,393	1,375	1,335	1,369	1,352	1,294	1,335				
2007	**($13,565)**	**($16,985)**	**($8,877)**	**($5,350)**	($2,249)	($2,404)	($2,090)	($1,108)	$299	$1,106	$2,613	$2,921					
Bootstrapped standard error	1,033	1,218	1,292	1,186	1,321	1,399	1,386	1,535	1,601	1,751	1,713	1,851					
2008	**($12,620)**	**($18,540)**	**($11,944)**	**($5,826)**	**($2,712)**	($1,847)	($1,648)	($525)	$351	$423	$664						
Bootstrapped standard error	615	781	872	897	922	1,056	1,042	1,151	1,227	1,236	1,333						
2009	**($10,688)**	**($16,312)**	**($8,451)**	**($3,322)**	**($1,351)**	($281)	$884	**$1,564**	**$1,999**	$1,779							
Bootstrapped standard error	421	537	561	576	603	629	705	745	791	955							
2010	**($12,856)**	**($16,627)**	**($9,292)**	**($4,366)**	**($2,110)**	($1,126)	($409)	$50	$894								

Cohort	Follow-on year 0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Bootstrapped standard error	498	635	668	728	728	762	804	831	802								
2011	**($13,531)**	**($17,075)**	**($9,181)**	**($4,104)**	**($2,033)**	($489)	$938	$1,967									
Bootstrapped standard error	616	773	797	895	977	1,029	1,035	1,096									
2012	**($12,480)**	**($14,952)**	**($7,075)**	**($2,037)**	($685)	$396	$1,295										
Bootstrapped standard error	576	765	857	936	985	1,011	1,055										
2013	**($11,877)**	**($12,786)**	**($5,990)**	**($2,372)**	($473)	$1,046											
Bootstrapped standard error	690	1,049	1,095	1,133	1,177	1,161											
2014	**($11,847)**	**($13,555)**	**($6,973)**	**($4,640)**	**($3,133)**												
Bootstrapped standard error	675	998	1,033	1,184	1,218												
2015	**($12,858)**	**($14,208)**	**($6,761)**	**($3,045)**													
Bootstrapped standard error	848	1,143	1,249	1,287													
2016	**($16,147)**	**($18,025)**	**($10,054)**														
Bootstrapped standard error	1,134	1,368	1,543														

Notes: t-test is performed for the comparison pool and MDM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A1-2. TB Program net impact on earnings by follow-on year (measured in 2016 dollars) – CEM control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	**($12,614)**	**($18,359)**	**($8,852)**	**($3,605)**	($2,144)	$140	$1,891	$1,971	$1,482	$1,496	$3,186	**$3,612**	$3,178	$2,565	$1,971	$2,006	$1,820
Bootstrapped standard error	733	1,047	1,004	1,152	1,344	1,510	1,481	1,423	1,595	1,683	1,758	1,782	1,813	1,854	1,863	1,838	2,009
2003	**($11,873)**	**($15,266)**	**($5,748)**	($2,081)	$222	$1,424	$2,343	$2,671	$1,940	$2,939	$2,792	$1,742	$2,353	$1,847	$833	$1,256	
Bootstrapped standard error	807	1,484	1,506	1,529	1,610	1,650	1,665	1,722	1,753	1753	1777	1,786	1,861	2,039	1,978	2,048	
2004	**($9,914)**	**($12,456)**	**($4,120)**	($73)	$2,072	$3,054	**$4,121**	**$4,875**	**$6,693**	**$6,795**	**$6,136**	**$5,983**	**$5,748**	$4,513	**$5,563**		
Bootstrapped standard error	1,068	1,582	1,621	1,634	1,739	1,800	1,941	1,948	2,015	2,090	2,150	2,190	2,219	2,311	2,461		
2005	**($9,915)**	**($11,450)**	**($5,140)**	($2,874)	$236	$1,313	$3,287	**$4,017**	**$4,628**	**$4,575**	**$4,792**	**$4,990**	$4,384	$4,908			
Bootstrapped standard error	1,082	1,313	1,414	1,581	1,561	1,639	1,842	1,883	1,994	2,200	2,137	2,229	2,454	2,595			
2006	**($12,689)**	**($14,854)**	**($7,524)**	**($4,441)**	($2,841)	($1,014)	($809)	($480)	($146)	$1,707	$760	($572)	($413)				
Bootstrapped standard error	1,024	1,739	1,761	1,856	1,772	1,856	1,989	2,088	2,245	2,373	2,455	2,601	2,649				
2007	**($10,976)**	**($10,935)**	**($5,078)**	($1,806)	$301	$724	$2,622	$2,760	$2,841	$4,235	$4,102	$3,243					
Bootstrapped standard error	1,742	2,306	2,206	2,487	2,524	2,792	3,112	3,078	3,130	3,170	3,268	3,169					
2008	**($11,569)**	**($18,621)**	**($10,442)**	**($3,816)**	($1,522)	$109	$569	$1,974	$2,421	$1,388	$1,243						
Bootstrapped standard error	693	987	1,136	1,320	1,371	1,526	1,675	1,633	1,661	1,760	1,923						
2009	**($10,314)**	**($15,861)**	**($8,129)**	**($3,184)**	**($1,917)**	($1,057)	$499	$1,614	$1,990	**$2,643**							
Bootstrapped standard error	500	757	747	793	902	970	1,040	1,074	1,166	1,266							
2010	**($12,278)**	**($14,896)**	**($8,446)**	**($3,570)**	($2,193)	($1,267)	($363)	$577	$1,141								

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Bootstrapped standard error	682	895	1,003	1,120	1,151	1,211	1,253	1,320	1,385								
2011	**($11,372)**	**($13,826)**	**($5,589)**	($2,010)	$191	$1,357	$2,267	$2,564									
Bootstrapped standard error	868	1,260	1,294	1,376	1,392	1,419	1,468	1,482									
2012	**($12,135)**	**($14,288)**	**($7,632)**	**($3,005)**	($785)	$598	$350										
Bootstrapped standard error	850	1,087	1,225	1,301	1,447	1,511	1,751										
2013	**($10,497)**	**($10,112)**	($2,526)	$1,424	$2,424	$3,561											
Bootstrapped standard error	940	1,451	1,494	1,658	1,837	2,002											
2014	**($11,098)**	**($11,683)**	**($5,044)**	($1,054)	$642												
Bootstrapped standard error	1,221	1,732	1,904	1,948	2,035												
2015	**($11,683)**	**($10,930)**	**($4,195)**	($1,198)													
Bootstrapped standard error	1,278	1,703	1,897	2,108													
2016	**($15,835)**	**($16,733)**	**($9,069)**														
Bootstrapped standard error	2,125	2,617	2,662														

Notes: t-test is performed for the comparison pool and CEM control.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A1-3. TB Program net impact on earnings by follow-on year (measured in 2016 dollars) – Denied control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year																
Cohort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	**($5,448)**	**($12,565)**	**($8,272)**	**($3,391)**	($1,054)	$365	$121	$546	$1,173	$1,913	$1,381	**$2,210**	$1,632	$1,488	$527	($111)	($1,263)
Bootstrapped standard error	482	719	768	750	748	766	819	904	998	1,064	1,019	1,034	1,113	1,037	1,018	1,063	1,203
2003	**($7,163)**	**($11,384)**	**($4,463)**	**($1,957)**	($751)	($91)	$621	$874	$674	$286	$397	$185	($1,158)	($1,972)	($2,377)	**($3,314)**	
Bootstrapped standard error	611	876	1,001	972	1,015	1,023	1,069	1,167	1,159	1,263	1,368	1,364	1,401	1,438	1,490	1,490	
2004	**($5,537)**	**($10,848)**	**($6,087)**	**($3,067)**	**($2,335)**	**($2,605)**	($2,496)	($2,725)	($2,284)	($2,008)	($1,802)	($2,716)	**($3,301)**	**($4,264)**	**($4,269)**		
Bootstrapped standard error	816	1,037	1,073	1,240	1,196	1,223	1,322	1,453	1,451	1,565	1,606	1,646	1,645	1,661	1,685		
2005	**($5,830)**	**($11,857)**	**($7,236)**	**($5,112)**	**($3,639)**	**($2,898)**	($2,483)	($1,967)	($1,799)	($1,895)	($1,462)	($1,233)	($1,540)	($2,036)			
Bootstrapped standard error	818	1,136	1,106	1,177	1,205	1,263	1,365	1,401	1,489	1,442	1,596	1,618	1,624	1,595			
2006	**($7,757)**	**($15,163)**	**($12,218)**	**($11,541)**	**($9,391)**	**($8,222)**	**($8,320)**	**($9,105)**	**($9,039)**	**($9,586)**	**($9,079)**	**($9,310)**	**($9,351)**				
Bootstrapped standard error	785	1,077	1,115	1,056	1,123	1,182	1,066	1,062	1,161	1,296	1,456	1,426	1,415				
2007	**($6,900)**	**($14,632)**	**($10,801)**	**($8,754)**	**($7,477)**	**($7,451)**	**($7,507)**	**($7,263)**	**($7,588)**	**($7,125)**	**($6,787)**	**($7,020)**					
Bootstrapped standard error	866	1,161	1,204	1,225	1,230	1,298	1,445	1,421	1,481	1,489	1,506	1,519					
2008	**($5,354)**	**($11,440)**	**($8,063)**	**($4,525)**	**($3,253)**	**($3,514)**	**($3,654)**	**($3,188)**	**($3,840)**	**($4,095)**	**($3,933)**						
Bootstrapped standard error	576	711	894	961	1,011	1,057	1,114	1,150	1,239	1,322	1,327						
2009	**($2,471)**	**($6,031)**	**($3,007)**	**($1,234)**	($1,261)	($1,461)	($884)	($384)	($748)	($863)							
Bootstrapped standard error	538	684	684	757	849	882	946	980	1,024	1,006							
2010	**($3,142)**	**($5,728)**	**($3,187)**	($673)	($148)	($529)	($462)	($1,457)	($1,210)								

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Bootstrapped standard error	543	801	876	862	964	1,039	1,052	1,057	1,142								
2011	**($1,926)**	**($3,782)**	($1,321)	$663	$1,397	$1,519	$1,114	$1,649									
Bootstrapped standard error	709	1,118	1,144	1,109	1,316	1,373	1,384	1,319									
2012	**($1,942)**	**($3,857)**	($746)	$826	$1,757	$1,781	$1,681										
Bootstrapped standard error	670	948	982	964	998	1,192	1,262										
2013	**($2,605)**	**($5,600)**	**($3,167)**	($1,793)	($872)	($217)											
Bootstrapped standard error	763	1,193	1,243	1,381	1,429	1,493											
2014	($1,293)	**($3,842)**	($1,559)	$600	$637												
Bootstrapped standard error	828	1,239	1,317	1,299	1,386												
2015	($1,225)	**($3,939)**	($910)	$128													
Bootstrapped standard error	1,085	1,414	1,490	1,605													
2016	**($5,343)**	**($9,581)**	**($5,856)**														
Bootstrapped standard error	1,037	1,330	1,614														

Notes: t-test is performed for the comparison pool and the denied control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A1-4. Pre-trends analysis for the PSM and denied control groups
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	PSM – λ_1	PSM – λ_2	Denied – λ_1	Denied – λ_2
2002	45	-55	**3,657**	**2,395**
Bootstrapped standard error	486	457	544	458
2003	-254	-217	**2,912**	**1,503**
Bootstrapped standard error	632	477	640	484
2004	16	410	**2,963**	**3,455**
Bootstrapped standard error	888	623	946	794
2005	-94	138	**2,785**	**2,468**
Bootstrapped Std. Error	821	634	918	731
2006	112	-28	**3,801**	**2,528**
Bootstrapped standard error	892	700	1,048	820
2007	-179	169	**3,857**	**3,478**
Bootstrapped standard error	973	778	997	744
2008	-819	283	**5,919**	**3,890**
Bootstrapped standard error	645	487	680	525
2009	88	369	**3,458**	**2,275**
Bootstrapped standard error	438	376	525	410
2010	-919	-832	-120	-131
Bootstrapped standard error	530	426	680	558
2011	-584	-662	1,266	773
Bootstrapped standard error	577	462	750	567
2012	178	101	75	-413
Bootstrapped standard error	730	595	790	687
2013	497	383	-812	**-1,731**
Bootstrapped standard error	888	736	1,046	857
2014	**-1,983**	-1,226	-703	-566
Bootstrapped standard error	802	725	1,082	798
2015	519	-100	200	-669
Bootstrapped standard error	856	729	1,366	949
2016	104	513	2,212	1,714
Bootstrapped standard error	1,321	989	1,471	1,095

Notes: t-test is performed for the comparison pool and the denied control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix 2: Difference-in-differences results for employment using alternative matching methods

We present the dynamic difference-in-differences (DID) model results for employment outcomes for each cohort using the Mahalanobis (MDM) matching control group (*Appendix figure A2-1*), the coarsened exact matching (CEM) control group (*Appendix figure A2-2*), and the denied applicants control group (*Appendix figure A2-3*) in this appendix. These results mirror those presented in *Chapter* 5, which are obtained by using the propensity score matching (PSM) comparison group to estimate the dynamic DID model for earnings:

$$Y_{i,t} = a_i + b_t + \sum_{m=1}^{3} D_{i,t-m}\lambda_m + D_{i,t}\gamma + \sum_{s=1}^{S} D_{i,t+s}\delta_s + \varepsilon_{i,t}.$$

Here, $Y_{i,t}$ represents the employment outcomes for person i in period t. See *Chapter 2* for a complete description of the equation and how to interpret the coefficient estimates. The results presented in this appendix are obtained from the same regression specification using the three alternative comparison groups instead of the PSM comparison group.

The results are largely consistent across models. We study whether cohorts break even in each of the analyses, and if so, when. We compare this to the results from the DID model using the control group selected by PSM and presented in the main body of the text. As such, we make 45 comparisons in this appendix: one for each of the three models used as robustness checks, for each of the fifteen cohorts. As in the previous appendix, across these 45 comparisons, we find that the three robustness models provide corroborating evidence in 40 instances (89 percent).

As for the earnings outcomes, the MDM and PSM model results agree for all cohorts' instances for employment outcomes. The discrepancies between the PSM, CEM, and denied group results are as follows:

1) The denied control group DID results suggest that the 2007, 2009, and 2012 cohorts benefited from the TB Program in their chances of employment. Overall, including the lock-in years, these cohorts were more likely than their peers to be employed in the observed follow-on years.
2) Similarly, the CEM control group DID results suggest that the 2007 cohort benefited from the TB Program in their chances of employment. Conversely, the CEM control group DID results suggest that the 2003 cohort did not benefit from the TB Program but were less likely to be employed over the course of their lives because of it.

Appendix figure A2-1. TB Program net impact on percent of time employed by follow-on year – MDM control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year																
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**
2002	**-27.27%**	**-38.19%**	**-4.86%**	**4.51%**	**7.80%**	**9.52%**	**9.15%**	**9.47%**	**10.54%**	**10.38%**	**10.16%**	**10.57%**	**11.37%**	**11.00%**	**10.07%**	**9.61%**	**8.97%**
Bootstrapped standard error	0.72%	1.03%	1.12%	1.02%	0.97%	0.97%	1.00%	0.97%	0.94%	0.99%	1.00%	1.06%	1.06%	1.07%	1.11%	1.06%	1.03%
2003	**-24.51%**	**-19.27%**	**2.97%**	**7.57%**	**8.62%**	**9.62%**	**11.23%**	**11.43%**	**11.07%**	**11.75%**	**11.68%**	**10.99%**	**9.46%**	**8.81%**	**8.38%**	**6.91%**	
Bootstrapped standard error	0.89%	1.12%	1.15%	1.14%	1.20%	1.28%	1.27%	1.26%	1.36%	1.41%	1.31%	1.34%	1.32%	1.39%	1.41%	1.41%	
2004	**-27.33%**	**-20.62%**	-2.91%	3.15%	**5.23%**	**5.80%**	**5.31%**	**5.61%**	**4.90%**	**5.52%**	**5.82%**	**5.49%**	**6.37%**	**6.07%**	**4.72%**		
Bootstrapped standard error	1.50%	1.99%	1.94%	1.92%	1.83%	1.74%	1.73%	1.99%	1.88%	1.92%	2.02%	1.90%	1.97%	1.93%	1.93%		
2005	**-30.36%**	**-18.94%**	-0.44	**4.21%**	**7.31%**	**6.19%**	**5.88%**	**7.31%**	**7.05%**	**6.15%**	**6.78%**	**8.41%**	**7.53%**	**5.86%**			
Bootstrapped standard error	1.35%	1.91%	1.90%	1.80%	1.98%	1.84%	1.91%	1.97%	2.11%	1.89%	1.85%	1.99%	2.05%	2.00%			
2006	**-32.03%**	**-25.37%**	**-10.64%**	**-4.21%**	0.21%	1.81%	1.85%	1.01%	0.49%	0.82%	1.29%	2.86%	2.88%				
Bootstrapped standard error	1.15%	1.60%	1.64%	1.66%	1.76%	1.75%	1.73%	1.68%	1.78%	1.74%	1.81%	1.69%	1.74%				
2007	**-33.15%**	**-29.10%**	**-12.93%**	**-2.74%**	1.34%	2.04%	1.23%	3.27%	**5.07%**	**5.42%**	**6.95%**	**9.09%**					
Bootstrapped standard error	1.21%	1.88%	1.88%	1.83%	1.94%	2.22%	2.17%	2.13%	1.98%	2.08%	1.97%	2.28%					
2008	**-28.09%**	**-37.74%**	**-16.76%**	-2.27%	0.74%	1.53%	1.56%	1.59%	2.34%	2.39%	3.08%						
Bootstrapped standard error	0.88%	1.15%	1.37%	1.41%	1.42%	1.60%	1.49%	1.51%	1.54%	1.57%	1.66%						
2009	**-26.98%**	**-34.50%**	**-11.43%**	1.40%	**3.03%**	**3.53%**	**3.51%**	**3.65%**	**3.65%**	**3.55%**							
Bootstrapped standard error	0.67%	0.90%	0.93%	0.93%	0.92%	0.95%	1.00%	1.00%	1.00%	0.99%							
2010	**-29.98%**	**-33.89%**	**-9.96%**	0.63%	**3.55%**	**4.42%**	**4.48%**	**5.00%**	**4.98%**								
Bootstrapped standard error	0.87%	1.20%	1.21%	1.22%	1.14%	1.19%	1.08%	1.18%	1.26%								
2011	**-29.37%**	**-32.34%**	**-7.52%**	**3.34%**	**4.55%**	**5.73%**	**5.70%**	**5.95%**									

Cohort	Follow-on year 0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Bootstrapped standard error	0.94%	1.23%	1.21%	1.24%	1.35%	1.31%	1.31%	1.38%									
2012	**-29.39%**	**-27.73%**	**-4.75%**	**3.23%**	**3.94%**	**4.14%**	**5.35%**										
Bootstrapped standard error	0.92%	1.30%	1.47%	1.42%	1.40%	1.53%	1.46%										
2013	**-28.05%**	**-20.66%**	-2.66%	2.81%	**4.18%**	**4.91%**											
Bootstrapped standard error	1.07%	1.59%	1.62%	1.72%	1.80%	1.76%											
2014	**-29.14%**	**-18.19%**	**-3.23%**	0.49%	1.40%												
Bootstrapped standard error	1.13%	1.32%	1.57%	1.54%	1.51%												
2015	**-30.27%**	**-22.16%**	**-4.23%**	1.78%													
Bootstrapped standard error	1.20%	1.70%	1.84%	2.00%													
2016	**-30.68%**	**-25.21%**	**-5.60%**														
Bootstrapped standard error	1.43%	1.89%	2.08%														

Notes: t-test is performed for the comparison pool and the MDM control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A2-2. TB Program net impact on percent of time employed by follow-on year – CEM control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year																
Cohort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	**-28.81%**	**-32.03%**	-1.85%	**7.61%**	**7.23%**	**7.76%**	**9.10%**	**8.74%**	**7.89%**	**7.60%**	**8.71%**	**8.22%**	**7.37%**	**6.31%**	**5.99%**	**6.48%**	**5.54%**
Bootstrapped standard error	1.33%	1.81%	1.90%	2.24%	2.37%	2.63%	2.59%	2.40%	2.38%	2.41%	2.51%	2.63%	2.63%	2.54%	2.42%	2.37%	2.35%
2003	**-28.03%**	**-22.76%**	-0.21%	3.16%	**5.26%**	**6.69%**	**8.14%**	5.39%	5.57%	**7.26%**	**5.74%**	4.60%	4.01%	3.62%	1.67%	3.83%	
Bootstrapped standard error	1.30%	2.43%	2.37%	2.55%	2.53%	2.48%	2.61%	2.79%	2.95%	2.69%	2.61%	2.48%	2.52%	2.52%	2.69%	2.69%	
2004	**-28.75%**	**-20.33%**	-0.79%	**6.72%**	**8.98%**	**9.15%**	**10.47%**	**11.35%**	**13.76%**	**12.44%**	**10.09%**	**9.37%**	**11.00%**	**9.78%**	**9.50%**		
Bootstrapped standard error	2.21%	3.18%	2.84%	2.70%	3.09%	3.45%	3.55%	3.36%	3.51%	3.47%	3.66%	3.47%	3.58%	3.60%	3.46%		
2005	**-28.88%**	**-11.53%**	3.18%	4.87%	**6.83%**	**6.80%**	**9.93%**	**10.73%**	**11.69%**	**11.97%**	**11.79%**	**11.58%**	**11.20%**	**9.92%**			
Bootstrapped standard error	1.84%	2.57%	2.51%	2.63%	2.61%	2.96%	3.23%	3.32%	3.47%	3.41%	3.40%	3.46%	3.55%	3.50%			
2006	**-33.24%**	**-22.10%**	-3.85%	0.85%	4.54%	6.17%	6.57%	6.08%	6.07%	**7.38%**	2.50%	1.62%	2.59%				
Bootstrapped standard error	2.13%	3.08%	3.10%	2.92%	3.37%	3.35%	3.49%	3.41%	3.56%	3.75%	3.54%	3.37%	3.30%				
2007	**-28.45%**	**-18.90%**	-2.39%	3.52%	6.38%	9.15%	9.19%	**10.02%**	**11.65%**	**13.34%**	**11.10%**	**12.59%**					
Bootstrapped standard error	2.80%	3.82%	3.74%	4.48%	4.62%	4.87%	5.14%	4.74%	4.52%	4.61%	4.76%	4.73%					
2008	**-27.93%**	**-40.31%**	**-15.81%**	0.06%	3.40%	4.52%	**4.85%**	**5.10%**	4.54%	3.55%	3.61%						
Bootstrapped standard error	1.37%	2.19%	2.43%	2.37%	2.46%	2.54%	2.45%	2.39%	2.59%	2.57%	2.73%						
2009	**-27.21%**	**-37.97%**	**-12.91%**	0.22%	0.37%	1.21%	**3.33%**	**4.22%**	**3.97%**	**4.04%**							
Bootstrapped standard error	0.91%	1.42%	1.44%	1.41%	1.63%	1.60%	1.56%	1.52%	1.63%	1.76%							
2010	**-32.12%**	**-36.10%**	**-10.81%**	-0.53%	1.88%	2.58%	3.41%	**4.57%**	**4.57%**								
Bootstrapped standard error	1.44%	1.79%	1.84%	1.98%	1.88%	1.94%	2.00%	1.94%	2.07%								
2011	**-30.82%**	**-34.51%**	**-7.36%**	-0.09%	3.09%	3.85%	3.97%	4.37%									

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Bootstrapped standard error	1.63%	2.32%	2.47%	2.37%	2.36%	2.41%	2.46%	2.35%									
2012	**-32.75%**	**-31.21%**	**-8.74%**	-0.81%	1.86%	1.97%	1.35%										
Bootstrapped standard error	1.65%	2.43%	2.32%	2.25%	2.31%	2.46%	2.61%										
2013	**-27.85%**	**-21.28%**	0.20%	**7.59%**	**6.50%**	5.45%											
Bootstrapped standard error	1.76%	2.77%	2.66%	2.73%	2.83%	3.02%											
2014	**-31.76%**	**-17.86%**	-0.77%	5.32%	4.85%												
Bootstrapped standard error	1.92%	3.29%	3.28%	3.34%	3.29%												
2015	**-34.44%**	**-21.12%**	-5.85%	-0.62%													
Bootstrapped standard error	2.50%	3.42%	3.61%	3.74%													
2016	**-37.08%**	**-30.40%**	**-8.65%**														
Bootstrapped standard error	2.57%	4.33%	4.32%														

Notes: t-test is performed for the comparison pool and the CEM control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A2-3. TB Program net impact on percent of time employed by follow-on year – Denied control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	**-4.66%**	**-16.71%**	-1.78%	**4.51%**	**4.74%**	**5.18%**	**5.85%**	**5.99%**	**6.00%**	**6.01%**	**5.22%**	**5.34%**	**5.17%**	**4.86%**	**3.86%**	**4.24%**	**3.72%**
Bootstrapped standard error	0.68%	0.99%	1.16%	1.03%	1.03%	1.01%	1.02%	1.11%	1.07%	1.09%	1.14%	1.15%	1.16%	1.16%	1.10%	1.08%	1.07%
2003	**-5.24%**	**-8.50%**	1.56%	**4.30%**	**6.06%**	**6.18%**	**6.88%**	**6.21%**	**5.78%**	**6.08%**	**5.92%**	**5.74%**	**4.62%**	**4.20%**	**4.04%**	**3.40%**	
Bootstrapped standard error	0.91%	1.33%	1.45%	1.26%	1.34%	1.33%	1.52%	1.58%	1.50%	1.47%	1.48%	1.51%	1.51%	1.53%	1.49%	1.50%	
2004	**-10.72%**	**-11.94%**	-0.10%	2.92%	**5.18%**	**4.77%**	**4.10%**	**3.81%**	3.37%	**4.47%**	**5.51%**	**4.62%**	**3.83%**	1.79%	1.71%		
Bootstrapped standard error	1.35%	1.97%	1.81%	1.77%	1.67%	1.70%	1.83%	1.93%	1.92%	2.13%	2.03%	1.96%	1.88%	1.88%	1.92%		
2005	**-12.57%**	**-10.82%**	0.57%	**4.57%**	**5.40%**	**6.39%**	**6.89%**	**6.04%**	**5.17%**	**4.81%**	**5.75%**	**6.94%**	**6.94%**	**5.68%**			
Bootstrapped standard error	1.27%	1.94%	1.89%	1.85%	2.02%	1.93%	1.92%	2.05%	2.10%	2.03%	2.22%	2.30%	2.26%	2.24%			
2006	**-14.73%**	**-15.82%**	**-5.32%**	-2.82%	1.67%	1.67%	1.35%	0.04%	-0.29%	-0.64%	-1.89%	-1.42%	-1.76%				
Bootstrapped standard error	1.44%	1.90%	1.78%	1.81%	1.93%	1.97%	1.72%	1.61%	1.71%	1.67%	1.81%	1.81%	1.87%				
2007	**-10.54%**	**-13.09%**	-3.13%	3.65%	**5.32%**	**4.64%**	**4.98%**	**6.76%**	**5.60%**	**5.88%**	**6.20%**	**6.35%**					
Bootstrapped standard error	1.23%	2.03%	2.07%	2.16%	2.10%	2.18%	2.19%	2.16%	2.06%	2.08%	2.08%	1.98%					
2008	**-5.71%**	**-14.15%**	**-4.19%**	**3.71%**	**4.24%**	2.08%	2.17%	1.27%	-0.52%	0.45%	1.04%						
Bootstrapped standard error	1.08%	1.20%	1.52%	1.56%	1.56%	1.57%	1.57%	1.62%	1.72%	1.86%	1.85%						
2009	**-3.92%**	**-8.78%**	**-2.70%**	**3.71%**	**3.22%**	2.19%	**3.25%**	**3.31%**	2.49%	**2.92%**							
Bootstrapped standard error	0.83%	0.98%	1.12%	1.24%	1.30%	1.28%	1.35%	1.36%	1.37%	1.33%							
2010	**-7.58%**	**-13.19%**	**-3.76%**	0.79%	1.61%	1.33%	1.51%	0.82%	0.03%								
Bootstrapped standard error	1.13%	1.35%	1.38%	1.33%	1.44%	1.61%	1.56%	1.52%	1.59%								
2011	**-5.19%**	**-6.61%**	1.77%	**4.31%**	3.76%	**4.00%**	2.37%	2.15%									
Bootstrapped standard error	1.15%	1.65%	1.81%	1.64%	1.84%	1.94%	1.93%	1.86%									

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2012	**-5.55%**	**-10.37%**	-0.57%	**3.29%**	**5.03%**	**5.00%**	**5.02%**										
Bootstrapped standard error	1.08%	1.77%	1.75%	1.53%	1.58%	1.71%	1.63%										
2013	**-8.44%**	**-10.31%**	-0.84%	0.57%	1.48%	2.01%											
Bootstrapped standard error	1.21%	1.85%	1.74%	1.72%	1.81%	1.88%											
2014	**-11.35%**	**-8.09%**	-0.09%	2.24%	1.53%												
Bootstrapped standard error	1.37%	1.89%	2.00%	1.91%	1.99%												
2015	**-12.57%**	**-14.09%**	-3.68%	-1.45%													
Bootstrapped standard error	1.58%	2.20%	2.26%	2.22%													
2016	**-15.20%**	**-17.91%**	**-4.57%**														
Bootstrapped standard error	1.38%	2.05%	2.30%														

Notes: t-test is performed for the comparison pool and the denied control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix 3: Difference-in-differences results for training credits attempted using alternative matching methods

We present the dynamic difference-in-differences (DID) model results for training credits attempted for each cohort using the Mahalanobis (MDM) matching control group (*Appendix figure A3-1*), the coarsened exact matching (CEM) control group (*Appendix figure A3-2*), and the denied applicants control group (*Appendix figure A3-3*) in this appendix. These results mirror those presented in *Chapter 6*, which are obtained by using the propensity score matching (PSM) comparison group to estimate the dynamic DID model for training credits attempted:

$$Y_{i,t} = a_i + b_t + \sum_{m=1}^{3} D_{i,t-m}\lambda_m + D_{i,t}\gamma + \sum_{s=1}^{S} D_{i,t+s}\delta_s + \varepsilon_{i,t}.$$

Here, $Y_{i,t}$ represents as an indicator variable equal to one when individual i attempted to earn training credits in period t. See *Chapter 2* for a complete description of the equation and how to interpret the coefficient estimates. The results presented in this appendix are obtained from the same regression specification, using the three alternative comparison groups instead of the PSM comparison group.

Note that, unlike for earnings and employment, most people in our sample do not attempt to earn training credits prior to their UI claim. They are employed and working full time, not seeking training. As such, the identification of the pre-trend parameters, λ_m, is based on the small, non-representative group of people that seek training before their UI claim of interest. Because we cannot verify whether the credits attempted pre-trends are truly parallel for the whole population, we have to rely to a greater extent on the assumptions that our matching model and difference-in-differences model are correctly specified in this context.

The results are completely consistent across models. We study whether cohorts break even in each of the analyses, and if so, when. We compare this to the results from the DID model using the control group selected by the PSM method and presented in the main body of the text. As such, we make 45 comparisons in this appendix: one for each of the three models used as robustness checks for each of the fifteen cohorts. Across these 45 comparisons, we find that the three robustness models provide corroborating evidence in all 45 instances.

Appendix figure A3-1. TB Program net impact on credits attempted by follow-on year – MDM control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	N/A	N/A	**18.89%**	**6.08%**	**3.85%**	**2.82%**	**2.48%**	**1.51%**	**1.14%**	0.50%	**0.92%**	**0.78%**	**0.84%**	0.39%	0.25%	**0.53%**	-0.06%
Bootstrapped standard error	N/A	N/A	0.67%	0.59%	0.65%	0.52%	0.51%	0.49%	0.44%	0.40%	0.35%	0.30%	0.29%	0.26%	0.30%	0.27%	0.08%
2003	N/A	**47.27%**	**18.19%**	**6.68%**	**3.36%**	**3.36%**	**2.03%**	1.05%	**1.01%**	0.16%	0.45%	0.45%	0.20%	0.36%	**0.81%**	0.24%	
Bootstrapped standard error	N/A	1.06%	0.98%	0.72%	0.68%	0.70%	0.57%	0.62%	0.47%	0.46%	0.38%	0.36%	0.32%	0.35%	0.30%	0.14%	
2004	**64.19%**	**41.05%**	**13.08%**	**3.01%**	0.84%	0.27%	-1.67%	**-3.88%**	**-4.88%**	**-4.68%**	**-5.82%**	**-5.38%**	**-5.49%**	**-4.77%**	**-5.65%**		
Bootstrapped standard error	1.58%	1.82%	1.45%	1.48%	1.39%	1.21%	1.24%	1.16%	1.01%	1.02%	1.00%	0.99%	0.89%	1.06%	0.83%		
2005	**65.06%**	**36.17%**	**10.01%**	-1.23%	**-6.06%**	**-6.32%**	**-6.67%**	**-9.22%**	**-9.13%**	**-9.04%**	**-9.92%**	**-10.10%**	**-8.78%**	**-9.83%**			
Bootstrapped standard error	1.56%	1.94%	1.94%	1.58%	1.40%	1.48%	1.25%	1.23%	1.18%	1.26%	1.23%	1.23%	1.34%	1.18%			
2006	**66.80%**	**39.11%**	**16.27%**	1.56%	-2.79%	**-3.86%**	**-6.41%**	**-5.26%**	**-5.92%**	**-6.82%**	**-7.56%**	**-7.15%**	**-8.22%**				
Bootstrapped standard error	1.38%	1.99%	2.02%	1.61%	1.42%	1.39%	1.50%	1.38%	1.34%	1.29%	1.22%	1.24%	1.21%				
2007	**68.24%**	**41.63%**	**19.21%**	1.29	**-3.54%**	**-6.01%**	**-6.22%**	**-7.83%**	**-9.12%**	**-9.66%**	**-10.09%**	**-10.09%**					
Bootstrapped standard error	1.83%	2.12%	2.02%	1.82%	1.70%	1.73%	1.66%	1.66%	1.65%	1.52%	1.46%	1.36%					
2008	**69.68%**	**53.87%**	**26.34%**	**4.19%**	-0.59%	**-2.42%**	**-4.46%**	**-4.95%**	**-5.81%**	**-5.48%**	**-5.81%**						
Bootstrapped standard error	1.09%	1.43%	1.54%	1.14%	1.12%	1.03%	1.02%	1.02%	0.92%	0.95%	0.86%						
2009	**63.44%**	**49.77%**	**20.04%**	1.07%	**-3.51%**	**-5.87%**	**-6.79%**	**-7.55%**	**-7.17%**	**-8.93%**							
Bootstrapped standard error	0.94%	1.11%	1.15%	1.00%	0.92%	0.79%	0.81%	0.78%	0.78%	0.72%							
2010	**66.45%**	**51.48%**	**24.04%**	**5.43%**	-0.27%	**-1.96%**	**-3.41%**	**-4.72%**	**-5.77%**								
Bootstrapped standard error	1.12%	1.32%	1.18%	1.01%	0.94%	0.99%	0.97%	0.92%	0.86%								
2011	**63.94%**	**47.59%**	**17.29%**	1.49%	**-2.65%**	**-4.31%**	**-7.47%**	**-9.05%**									
Bootstrapped standard error	1.14%	1.15%	1.20%	1.19%	1.22%	1.08%	1.08%	1.03%									

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2012	**65.77%**	**46.73%**	**16.02%**	1.78%	-2.01	**-5.17%**	**-9.43%**										
Bootstrapped standard error	1.16%	1.28%	1.37%	1.23%	1.24%	1.08%	1.08%										
2013	**65.80%**	**43.31%**	**14.51%**	2.14%	**-2.87%**	**-10.24%**											
Bootstrapped standard error	1.49%	1.91%	1.82%	1.73%	1.49%	1.16%											
2014	**66.16%**	**41.71%**	**14.08%**	0.81%	**-9.73%**												
Bootstrapped standard error	1.31%	1.59%	1.55%	1.42%	1.05%												
2015	**65.30%**	**42.17%**	**15.01%**	**-6.66%**													
Bootstrapped standard error	1.44%	1.86%	1.97%	1.37%													
2016	**66.15%**	**44.69%**	3.20%														
Bootstrapped standard error	1.67%	2.06%	1.97%														

Notes: t-test is performed for the comparison pool and the MDM control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A3-2. TB Program net impact on credits attempted by follow-on year – CEM control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Impact Study, 2002 through 2016

	Follow-on year																
Cohort	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**
2002	N/A	N/A	**18.10%**	**7.61%**	**5.06%**	**4.20%**	**3.86%**	**2.52%**	**2.59%**	**2.48%**	**1.65%**	1.08%	1.17%	**1.19%**	0.40%	0.62%	0.12%
Bootstrapped standard error	N/A	N/A	1.62%	1.52%	1.31%	1.35%	1.02%	1.02%	0.96%	0.94%	0.81%	0.74%	0.66%	0.53%	0.60%	0.67%	0.15%
2003	N/A	**43.34%**	**17.26%**	**6.03%**	2.71%	**2.93%**	2.05%	1.12%	1.17%	-0.13%	0.37%	0.14%	0.02%	0.76%	0.59%	0.52%	
Bootstrapped standard error	N/A	2.68%	1.93%	1.79%	1.47%	1.34	1.13%	0.95%	0.95%	0.90%	0.93%	0.93%	0.74%	0.68%	0.76%	0.44%	
2004	**61.21%**	**39.82%**	**13.68%**	4.02%	3.38%	-0.96%	-1.53%	-3.02%	**-4.87%**	**-5.51%**	**-5.74%**	**-6.11%**	**-5.91%**	**-4.34%**	**-4.96%**		
Bootstrapped standard error	2.83%	3.02%	2.87%	2.49%	2.40%	2.57%	2.30%	2.13%	2.07%	1.71%	1.75%	1.94%	2.04%	2.08%	1.70%		
2005	**66.20%**	**40.61%**	**15.86%**	3.47%	-1.68%	-2.08%	-4.79%	-4.73%	**-6.14%**	-4.88%	**-5.31%**	**-4.74%**	**-5.02%**	**-5.29%**			
Bootstrapped standard error	2.91%	3.25%	3.17%	2.83%	3.01%	2.79%	2.82%	2.78%	2.61%	2.51%	2.42%	2.33%	2.36%	2.14%			
2006	**61.41%**	**35.00%**	**10.94%**	3.22%	-2.46%	**-6.40%**	**-8.64%**	-4.63%	**-8.70%**	**-7.89%**	**-9.84%**	**-10.08%**	**-9.99%**				
Bootstrapped standard error	3.30%	3.71%	3.22%	3.23%	2.99%	2.89%	2.56%	2.75%	2.48%	2.23%	2.02%	2.19%	1.98%				
2007	**65.44%**	**40.61%**	**20.14%**	1.23%	**-7.68%**	**-7.24%**	-6.18%	**-7.09%**	**-10.42%**	**-9.42%**	**-9.93%**	**-10.42%**					
Bootstrapped standard error	3.81%	4.54%	5.04%	4.40%	3.72%	3.55%	3.67%	3.57%	3.95%	3.23%	3.41%	3.08%					
2008	**74.47%**	**61.50%**	**31.03%**	**6.52%**	3.47%	-1.26%	-1.98%	-0.48%	-0.56%	-1.25%	-1.58%						
Bootstrapped standard error	1.90%	2.47%	2.47%	1.87%	1.95%	1.85%	1.63%	1.66%	1.59%	1.65%	1.44%						
2009	**64.10%**	**52.31%**	**21.35%**	1.44%	-2.13%	**-3.68%**	**-5.35%**	**-5.20%**	**-4.98%**	**-7.48%**							
Bootstrapped standard error	1.50%	1.53%	1.64%	1.28%	1.30%	1.29%	1.13%	1.06%	1.04%	1.03%							
2010	**64.14%**	**50.65%**	**18.96%**	0.07%	**-3.04%**	**-3.66%**	**-4.03%**	**-4.07%**	**-6.19%**								
Bootstrapped standard error	1.60%	1.78%	1.68%	1.58%	1.54%	1.47%	1.35%	1.23%	1.07%								
2011	**65.54%**	**47.54%**	**14.58%**	1.73%	-3.26%	**-4.52%**	**-7.92%**	**-7.71%**									

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Bootstrapped standard error	1.90%	2.42%	2.38%	2.02%	1.97%	1.85%	1.75%	1.60%									
2012	**64.12%**	**47.70%**	**15.96%**	-0.30%	-3.89%	**-6.40%**	**-9.33%**										
Bootstrapped standard error	2.24%	2.81%	2.50%	2.25%	2.12%	1.88%	1.90%										
2013	**67.79%**	**45.82%**	**15.85%**	3.60%	-1.01%	**-9.16%**											
Bootstrapped standard error	2.34%	2.83%	3.05%	2.86%	2.58%	2.10%											
2014	**68.49%**	**48.32%**	**20.28%**	**5.84%**	**-6.90%**												
Bootstrapped standard error	2.93%	3.23%	3.33%	2.92%	2.30%												
2015	**63.54%**	**41.62%**	**12.24%**	**-7.98%**													
Bootstrapped standard error	3.18%	4.05%	3.89%	3.12%													
2016	**68.80%**	**47.51%**	6.88%														
Bootstrapped standard error	3.35%	4.33%	3.57%														

Notes: t-test is performed for the comparison pool and the CEM control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.

Appendix figure A3-3. TB Program net impact on credits attempted by follow-on year – denied control group
Washington state, 2002 through 2016 cohorts
Source: Employment Security Department/LMEA, Training Benefits Net Impact Study 2002 through 2016

	Follow-on year																
Cohort	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2002	N/A	N/A	**4.68%**	-0.57%	-0.22%	-0.61%	-0.15%	-0.83%	-0.54%	-0.14%	-0.20%	0.14%	-0.31%	0.04%	0.11%	0.09%	-0.09%
Bootstrapped standard error	N/A	N/A	0.90%	0.63%	0.67%	0.56%	0.60%	0.54%	0.48%	0.46%	0.43%	0.33%	0.38%	0.32%	0.34%	0.29%	0.09%
2003	N/A	**16.07%**	**3.23%**	-0.97%	**-1.97%**	-0.81%	**-1.65%**	-1.09%	-0.11%	0.09%	-0.54%	**-1.28%**	-0.66%	-0.53%	-0.30%	0.17%	
Bootstrapped standard error	N/A	1.50%	1.34%	1.03%	0.80%	0.78%	0.68%	0.72%	0.61%	0.52%	0.48%	0.48%	0.43%	0.37%	0.43%	0.16%	
2004	**12.14%**	**14.34%**	3.33%	-1.86%	-2.90%	-2.38%	-0.54%	**-2.85%**	**-2.82%**	-2.08%	-2.04%	-0.72%	-1.68%	-0.39%	-0.88%		
Bootstrapped standard error	1.99%	2.14%	1.95%	1.62%	1.68%	1.52%	1.45%	1.40%	1.26%	1.24%	1.30%	1.24%	1.22%	1.27%	1.10%		
2005	**13.32%**	**16.78%**	4.48%	0.93%	-0.92%	1.01%	2.00%	1.56%	1.38%	1.57%	1.88%	1.87%	2.65%	**3.20%**			
Bootstrapped standard error	1.92%	2.35%	2.39%	2.04%	2.03%	1.95%	1.84%	1.65%	1.72%	1.80%	1.71%	1.68%	1.70%	1.54%			
2006	**9.92%**	**13.88%**	**5.23%**	0.05%	-0.43%	-0.19%	-1.21%	-0.23%	0.20%	0.61%	-0.12%	1.11%	0.91%				
Bootstrapped standard error	1.87%	2.35%	2.48%	2.14%	1.79%	1.59%	1.62%	1.57%	1.64%	1.66%	1.51%	1.45%	1.34%				
2007	**12.40%**	**15.97%**	**5.46%**	-2.50%	-3.61%	-3.09%	-1.99%	-1.59%	-2.02%	-2.05%	-1.35%	-0.62%					
Bootstrapped standard error	2.30%	2.68%	2.65%	2.27%	1.93%	1.84%	1.84%	1.86%	1.70%	1.81%	1.64%	1.63%					
2008	**13.98%**	**16.56%**	**7.73%**	-1.89%	-0.70%	0.62%	0.00%	1.10%	0.21%	0.95%	**2.24%**						
Bootstrapped standard error	1.69%	1.87%	1.84%	1.69%	1.62%	1.37%	1.34%	1.24%	1.18%	1.12%	1.08%						
2009	**18.22%**	**14.04%**	**4.47%**	**-2.74%**	-1.75%	-1.79%	-1.75%	-1.59%	-1.03%	-1.31%							
Bootstrapped standard error	1.28%	1.70%	1.40%	1.27%	1.21%	0.99%	1.00%	0.97%	0.97%	0.84%							
2010	**24.78%**	**23.67%**	**11.46%**	**3.82%**	**3.91%**	**4.28%**	**4.09%**	**3.46%**	**4.41%**								
Bootstrapped standard error	1.74%	1.82%	1.81%	1.68%	1.67%	1.61%	1.58%	1.53%	1.38%								
2011	**19.00%**	**9.87%**	0.52%	-3.05%	-2.18%	-1.07%	-2.01%	-1.70%									
Bootstrapped standard error	1.91%	2.11%	2.23%	1.88%	1.68%	1.60%	1.53%	1.38%									

Cohort	Follow-on year																
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
2012	**18.81%**	**14.89%**	4.12%	0.80%	-1.12%	-1.23%	-2.30%										
Bootstrapped standard error	2.15%	2.11%	2.16%	1.85%	1.87%	1.69%	1.53%										
2013	**15.06%**	**18.28%**	**5.42%**	1.56%	1.10%	-1.40%											
Bootstrapped standard error	1.98%	2.40%	2.24%	2.10%	1.88%	1.60%											
2014	**18.73%**	**19.33%**	**9.59%**	**3.56%**	-0.30%												
Bootstrapped standard error	2.17%	2.41%	2.31%	1.74%	1.51%												
2015	**12.86%**	**19.26%**	**8.75%**	0.47%													
Bootstrapped standard error	2.66%	2.63%	2.57%	2.24%													
2016	**18.20%**	**25.33%**	**6.90%**														
Bootstrapped standard error	2.76%	2.80%	2.27%														

Notes: t-test is performed for the comparison pool and the denied control group.
Bold denotes $p < 0.05$.
Standard errors are calculated using a bootstrap algorithm with 100 iterations.