Evaluation of a New Job Training Program: Code Louisville

Christopher R. Bollinger & Kenneth R. Troske[*]

DRAFT: June 20, 2023

Abstract:

In this paper we estimate the returns to a new and novel job training program, called Code Louisville, designed to provide participants with training in modern computer software development (coding). The program is open to adults in the Metropolitan Louisville area and is targeted towards individuals who would qualify for training through the Workforce Investment and Opportunity Act (WIOA) program. This program has many of the characteristics of sectoral training programs that tend to be small and run thought nongovernmental organization, but it also appear to produce higher returns than federal job training programs. Since Code Louisville is administered as part of WIOA in Louisville, KY through the local Workforce Development Board (WDB)—KentuckianaWorks, this evaluation provides evidence on whether sectoral programs continue to provide higher returns when they are part of the federal job training system. In addition, since the primary training in Code Louisville is offered online, we provide evidence of the success of less expensive online training programs. A final feature of our analysis is we use a new, but fairly widely available, source of data to construct comparison samples for our matching. We find that the Code Louisville program produces positive impacts on labor market outcomes for participants that are somewhat larger than traditional WIOA training program. However, the size and timing of benefits differs by gender and educational attainment prior to entering the program. While we find the returns to be smaller than those seen in other sectoral programs, the cost of administering the online Code Louisville appears to be substantially less than other programs. Our findings thus provide evidence supporting the growth and expansion of the program.

[*]Affiliation: Department of Economic, University of Kentucky. Support for our initial evaluation for the Code Louisville program came from the Department of Labor. We thank the staff of KentuckianaWorks, especially Michael Gritton, Brian Luerman and Sarah Ehresman for providing their data and helping us understand the Code Louisville program, as well as for comments on the paper. We also thank the staff at KYSTATS for all their help accessing the data and with the matching and analysis in the paper, especially Jessica Cunningham, Kris Stevens, Kari Whitt, Angie Tombari and Matt Berry. Finally, we thank Peter Mueser for comments on the paper and Nicholas Graff for helpful research assistance. All remaining errors are ours.

I. Introduction

Estimates of the returns to participating in a federal job training program such as the Workforce Innovation and Opportunity Act (WIOA) program, or its predecessor the Workforce Innovation Act (WIA) program, or the Trade Adjustment Act (TAA) program suggest that these programs provide positive, but modest increased earnings and employment (see Heinrich, et al, 2013, Schochet et al., 2012, Hyman, 2018 and Andersson et al., 2022). In contrast, recent research examining training programs that focus on training workers to work in a specific sector or occupation and that are often financed by private or nongovernmental organizations—commonly referred to as sectoral training programs—tend to show much larger returns (see Baird, et al., 2022 and Katz, et al., 2022).[1]

Obvious questions arising from these studies is what differences between federal training programs and sectoral training programs might account for the estimated differences in returns and is it possible to increase the scale of private sector sectoral training programs such that participants continue to experience these higher returns.

In this paper we begin to address these questions by estimating the returns to a job training program, called Code Louisville, that has many of the characteristics of a sectoral training program but was originally funded through the Department of Labor's Workforce Innovation Fund (WIF) and is administered through the Louisville, KY local Workforce Development Board (WDB)—KentuckianaWorks. Code Louisville is designed to provide participants with training in modern computer software development (coding). The program is also motivated by a perceived need for software development talent in the Louisville Metropolitan area. Code Louisville is open to adults in the Metropolitan Louisville area.

Launched in 2015, Code Louisville is a departure from traditional WIOA training programs in that it uses online software to conduct training rather than the more common classroom experience. The advantage of the online approach is believed to be lower cost and a more flexible time commitment: participants can work at their own pace at times convenient for them, rather than attending classes at times determined by the program that may conflict with other commitments. A second key difference is the inclusion of a mentoring program in which participants are assigned to small mentoring groups of approximately twelve participants that are led by volunteer mentors who are experienced software development workers in the Louisville Metropolitan area. The program also includes job placement services that are more hands-on than the typical job placement services offered through WIOA and more

[1] Baird et al. (2022) estimate the returns to the Career Pathways program which is part of WIOA.

closely resemble the job placement services available through sectoral training programs (see Katz et al., 2022).

Another unique aspect of our study is that we evaluate the Code Louisville programs by matching data on Code Louisville participants to records that are part of Kentucky Center for Statistics' (KYSTAT) Kentucky Longitudinal Data System (KLDS). These data contain demographic, earnings and residential information for most people who live in Kentucky by combining data from a variety of state administrative databases, including data from all public schools in Kentucky, the state Unemployment Insurance system as well as driver's license information. Given the large population we must draw from, we are able to create comparison samples that are nearly identical to our treatment samples on observable characteristics. The KYSTAT's administrative data system is similar to administrative data created in numerous other states (Bloom-Weltman, 2019). We believe we are one of the first researchers to use these data to conduct this type of analysis, so one contribution of our paper is demonstrating whether and how these data can be used in evaluating government training programs.

Our main findings are that the Code Louisville program impacts labor market outcomes for both men and women but in different ways. Initially after enrolling in the program men show very little gains in earnings, but, by the end of the data—three years post enrollment—men experience quarterly earnings gains of between 5-10% per quarter relative to individuals in the comparison sample. However, men experience significant gains in employment of approximately 5 percentage points within one year of enrolling in the program that persist through the end of the data. In contrast, women in the program experience a 5% gain in quarterly earnings relative to the control groups within one year of enrolling in the program, which grows to approximately a 15% increase in quarterly earnings by three years after enrollment. However, women experience a smaller 3 percentage point increase in employment by three years after enrollment. These benefits primarily accrue to people who actually complete at least one module in the program. We also find that the gains tend to primarily accrue to individuals with the most education—a bachelor's degree or higher. We find smaller and often statistically insignificant estimated impact for individuals with less education. Individuals for whom we do not know their education level show very little increase in earnings as a result of participating in the program, but males in this group do experience approximately a 5 percentage point increase in employment. These estimated effects are comparable, or slightly better than previous evaluations of federal job training program (Heinrich et al., 2013; Card et al., 2018; Andersson et al., 2022), but below estimates from previous evaluations of sectoral training programs (Baird, et al., 2022 and Katz, et al., 2022). One factor accounting for the smaller impacts may be that the training in the Code Louisville program is delivered in an online format while training in the other programs is primarily in-person. However, the fact that the cost of the Code

Louisville program is substantially below the cost of other WIOA programs (See Bollinger and Troske, 2019) suggests that the Code Louisville program may be a more efficient way to provide this type of training to interested participants. We believe this evidence is strong enough to suggest that federal officials consider expanding the Code Louisville program to other locations. We also believe that our success in creating matched control groups using data on the population of people living in an area suggests that other researchers should consider using similar data when available.

The rest of our paper is as follows. In the next section we provide a brief review of the recent literature examining the returns to federal job training programs and sectoral job training programs. In section III we provide a brief history of and more details about the Code Louisville program. In section IV we discuss the data used in our analysis in more detail and in section V we discuss our empirical methodology. We discuss our results in section VI and present our conclusions in section VII.

II. Literature Review

Evaluating the effectiveness of job training programs has a long history. Early efforts at evaluating the effectiveness of job training programs culminated in the seminal paper by Robert LaLonde (LaLonde, 1986) which argued for the need to use experimental methods to produce rigorous evaluations of training programs and led to a significant world-wide increase in program evaluation studies. One resulting study is Orr, *et al.*, (1996) which discusses the results from the random assignment evaluation of the Job Training Partnership Act (JTPA) program. Orr et al. find small but statistically significant impacts of job training on earnings for adult disadvantaged workers on the order of a 10% increase in earnings for women and a 5.6% increase in earnings for men.

Recent evaluations of the Workforce Innovation Act (WIA) program—the successor to the JTPA program—are Fortson et al. (2017), Heinrich et al. (2008, 2013), and Andersson et al. (2022). Fortson et al. (2017) discuss the results from a federally funded randomized control trail (RCT) evaluation of WIA and do not find that training programs produce a statistically significant increase in earnings above that received through core and intensive services. However, this finding is likely due to the lack of power to estimate differences in the programs as opposed to training providing no benefit. In contrast non-experimental studies of WIA by Heinrich et al. (2008, 2013) and Andersson et al. (2022) find that longer term training programs provide incremental returns of approximately 25% for women and 15% for men who are part of the WIA adult worker program. The adult worker program focuses on workers who are economically disadvantaged and face significant barriers to work. Both studies also report that workers in the dislocated worker program experience no statistically significant increase in earnings. This finding is important to our study because the individuals participating in the Code Louisville have characteristics that are similar to WIOA dislocated workers.

Katz et al. (2022) summarizes and compares the results from four RCT evaluations of sectoral training programs. All of these programs focus on providing training to low-income adults, similar to the Adult program in WIOA, across three different sectors: advanced manufacturing, health care, and information technology (IT). These programs produce large earnings gains of 12%-34% that persist over time. Most of the earnings gains appear to come from providing skills that allow participants to move into occupations that pay above-average wages.

Baird, et al. (2022) discuss the results from an evaluation of a training program in New Orleans called Career Pathways. This program is like the Code Louisville program in that both were funded through the Department of Labor's Workforce Innovation Fund, both screened workers on the likelihood that they would complete and benefit from the training program and both provide training for workers to work in the IT sector. One difference between these programs is that the Career Pathways program also provides training for jobs in the advanced manufacturing and healthcare sectors. Baird, et al. (2022) report that participants in this program experienced an average increase in earnings of about 12%. A large part of the gain comes from workers who are unemployed at the start of the training obtaining employment after training, but also from workers moving into in the targeted sectors.

Two recent evaluations of the Federal Trade Adjustment Act (TAA) program, a program that provides training to workers who lost their jobs because of competition from imports, provide somewhat contradictory results. In a careful nonexperimental evaluation of TAA, Schochet et al. (2012) find that the program has no significant impact on participant future earnings. In contrast, Hyman (2018) uses the random assignment of applicants to case workers who differ in their propensity to approve an application to the program to estimate the impact of training on workers and finds workers who participate in training experience about a $10,000 increase in annual earnings in the ten years after participation relative to what they would have expected to earn absent training.

Card, Kluve, and Weber (2018) provide a meta-analysis of 97 studies of 199 active labor market policies from around the world. They find that job search assistance programs have the highest initial impact, but this impact is relatively short term and fades over time. Job training programs appear to have smaller initial impacts, but these impacts are more long-lasting. They find that conclusions are not particularly sensitive to whether an RCT design was used or whether the evaluation was conducted using quasi-experimental statistical approaches. This is important to the validity of our study. Heckman and

Smith (1999) provide some guidance in this and note that it is crucial to control for pre-program status well in advance of the entry to the program.

Another relevant paper is Bollinger and Troske (2019), which is our initial evaluation of the Code Louisville program produced for the Department of Labor. In this evaluation we compared the outcomes of Code Louisville participants with the outcomes of two other WIOA training programs conducted by KentuckianaWorks, which trained workers for jobs in the advanced manufacturing sector, as well as all other WIOA participants who had individual training accounts (ITA). The two WIOA training programs were the Certified Production Technician (CPT) and Manufacturing and Employment Training Connection (M-TEC) programs. ITA participants typically sought two-year associate degrees in medical fields or truck driving training. There are two shortcomings with this report. First, we only had at most four quarters of earnings and employment information after entry for participants in the Code Louisville program, which previous research suggests is too short a time period to see impacts of these programs (i.e., Heinrich et al., 2013 and Card, Kluve and Weber, 2018). Second, participants in the Code Louisville program tended to be much more advantaged and more educated than participants in other WIOA programs, making it difficult to find comparable individuals in these other programs. We address both shortcomings in the present study, using a longer follow-up period and a much larger comparison pool (see below for details).

Research on the importance of computer literacy and usage is also relevant to this analysis. Krueger (1993) is often cited as the earliest evidence that workers who use computers tend to have higher earnings. Other authors such as Doms, Dunne, and Troske (1997), Autor, Katz, and Krueger (1998) and Green (1999) have provided evidence showing that the use of more advanced technology is associated with higher wages. However, DiNardo and Pischke (1997) cautioned against drawing strong causal conclusions showing that higher paid, more productive, workers tended to use computers along with other managerial tools. Similarly, Entorf and Kramarz (1996) and Doms, Dunne, and Troske (1997) present evidence showing that workers who use computers tend to be more highly paid prior to starting to use a computer. Hamilton (1997) found that more direct measures of computing skills, such as use of specific software and knowledge of specific programming languages, was associated with higher wages. Borghans and Weel (2004) showed that while there is a premium for specific computer skills, the premium does not differ with the level. Similarly, Vakhitova (2006) finds that Microsoft certification only provides returns for those who obtain certification beyond basic competency.

Online learning has been growing rapidly, and the literature examining the effectiveness of online learning has grown rapidly as well. The Means, *et al.* (2010) meta-analysis identified over a thousand papers written on the topic in the prior decade. In their analysis of 50 of these papers, they find that, overall, online courses appear to do slightly better for learning outcomes than traditional face-to-face instruction (it should be noted that of 50 studies included in their analysis only 11 showed statistically significant positive gains; in contrast only 3 showed that online was statistically worse). Perhaps most important for this study, they find that blended approaches, where the online course is combined with some face-to-face instruction, had the largest gains compared to the traditional classroom settings (of the 11 significant studies, 10 of them were the blended approach).

We believe our current paper contributes to the existing literature in several ways. First, we evaluate a unique, new sectoral training program that is at least partially financed with federal dollars and whose participants were required to be eligible for WIOA training. Our study also informs the literature on the efficacy of blended online training compared to traditional face-to-face learning of most training programs. However, we emphasize that our study is not designed to assess learning outcomes. Finally, we use novel data to form our matched comparison samples used in our estimation.

III. The Code-Louisville Program

The Code Louisville (CL) program is designed to provide participants with training in modern computer software development (programming or coding). The program is also motivated by a perceived need for software development talent in the Louisville Metropolitan area. The program is open to individuals 18 years old or older who have at least a high school diploma or GED and who live in Jefferson, Bullitt, Henry, Oldham, Shelby, Spencer, and Trimble counties in Kentucky as well as Clark, Crawford, Floyd, Washington, Harrison, and Scott counties in Southern Indiana. The program is administered by KentuckianaWorks and is targeted towards individuals who would qualify for training through WIOA.

Code Louisville is a departure from traditional WIOA training programs in that it uses online software to conduct the training rather than the more common classroom training. The advantage of the online approach is believed to be lower cost and a more flexible time commitment: participants can work at their own pace at times convenient for them, rather than attending classes at times determined by the program that may conflict with other commitments.

A second key difference is the mentoring program. Participants are assigned to small mentoring groups of approximately twelve participants, led by volunteer mentors who provide support during the program. The mentors are experienced software development workers in the Louisville Metropolitan area. Mentors typically meet with the students as a group on a weekly or bi-weekly basis and respond to online discussions and emails throughout the week. The mentoring program is seen as bringing accountability, guidance, and support to the learning process.

The program also includes job placement services and assistance in a variety of forms. Job readiness workshops and one-on-one meetings are key components of the job placement services that are tailored to the tech industry. In focus groups, these were often cited as crucial in helping with job placement. Additionally, social mixers are conducted and designed to provide participants with networking opportunities. The job placement services work with local employers to identify potential matches and assisted participants with resume writing and other job search support. The program's job placement services are more hands-on than the typical job placement services offered through KentuckianaWorks.

The program used the TreeHouse system for the online course content, including video lectures and reference material.[3] The system also provides the ability to track participant progress and provide feedback on assignments. A second online element is SLACK, which provides participants an online interface between other participants in the mentoring group and their mentors. SLACK allows for online discussions and responses by mentors, other participants, or program administrators to participant questions.

Following the nomenclature used by Code Louisville, we use the term "cohort" to mean a group of participants who started a training subject in a particular month and year. The program groups individuals into cohorts and then, within cohorts, into mentor groups. Each training module lasts 12 weeks. Participants are told to expect to spend 15 hours per week on coursework. Again, following terms used by Code Louisville, we call the subject in which a participant is receiving training a "track." Within each cohort, different participants may be learning different material depending on the track in which they are enrolled.

Cohorts were formed in May, July, and September of 2015, and then in January, May and September of subsequent years through 2019. In 2019 an August and an October cohort were formed, and then in 2020 January and March cohorts were formed. Our data end with the March 2020 cohort. As we discuss below, this cutoff was chosen based on the availability of post-participation earnings data.

Every effort is made to accommodate all registrants in each cohort. In some cases, registrants who were contacted failed to follow up and start the program. This rate significantly increased in 2016 when Code Louisville added prerequisites to the program. Participants are required to complete some

[3] In late 2020 KentuckianaWorks switched to the PluralSight system to provide course content.

basic computing modules that is designed to test participants' ability to learn in an online environment and their ability to devote sufficient time to the program. In addition, KentuckianaWorks gives priority to people who met WIOA priority of service requirements, applicants receiving public assistance, who are low-income, or who are basic skill deficient (U.S. Department of Labor, 2010).

The program allows participants to complete several tracks (topics) which provide training in different aspects of software development. Nearly all participants begin with the Front End Web Development track which teaches participants how to write and produce HTML, CSS and JavaScript for a website or Web Application. There are two exceptions to this typical starting path. Two cohorts (May and September of 2016) started with a track teaching Fullstack JavaScript which combines some of the front-end web development skills (HTML, CSS) with "back end" web development (e.g., management of underlying databases for a web page). The second exception was a small group of people who had already demonstrated some knowledge of the material from Front End Web Development and began the program with another available track.

To be considered a successful completion, participants must attend at least 9 of the 12 weekly meetings with the mentors and must complete all of the online curriculum assigned for that module within the twelve-week period. Additionally, they must attend at least two events (conferences for example) where technology is the main subject of the event and where they will meet new people in the technology field. Finally, each participant must submit a final project within the twelve-week window, which demonstrates the skills developed and would be suitable as a "portfolio" element in their job search.

After completing the initial track, participants are able—but not required—to pursue training in up to seven additional tracks: PHP Development, Rails Development, iOS Development, Android Development, .Net Development, Python and Full Stack JavaScript (or Front End Web Development for those starting in May and September 2016). Different tracks are offered in different time periods depending on both student demand and feedback from industry. Each of these tracks allows participants to further develop software design skills.

After completion of at least one track, participants are eligible for job placement assistance through the Code Louisville program and its partnerships with local companies seeking software developers.

Originally, when the program was funded through DOL's Workforce Innovation Fund the broad target population for the study were individuals eligible for either WIOA Dislocated Worker or Adult training programs. Later, the Code Louisville programs focuses on job seekers who are interested in pursuing job training or educational programs and not simply programs designed to help them find a job.

Code Louisville participants are recruited through advertisements in American Job-Centers, on television, radio and in print media. Counselors at the job centers may also recommend this program to individuals seeking job training. Since this program is not designed to replace any large-scale program, it is likely that this type of recruiting will continue as the program expands. This recruitment strategy is one reason why we have chosen to estimate the effect on treatment on the treated as these estimates reflect the impact of the program on people who choose to participate in the program.

KentuckianaWorks started the Code Louisville program in early 2015. The first cohort was formed and began training in May of 2015. While Code Louisville has done some limited advertising for the program, a speech by then President Obama while visiting Louisville in the spring of 2015 in which he mentioned the program, and word of mouth seem to have generated substantial interest in the program during the last four years. Typically, the program has been oversubscribed, with Code Louisville maintaining a wait list of up to 1,000 individuals. An initial constraint on the program was the number of volunteer mentors. However, as the program has continued, more mentors have been drawn from previous participants in the program, allowing the program to grow over time.

IV. Data

The Code Louisville program collects basic information on participants including name, address, and social security number. Code Louisville also tracked and provided start dates (the date the module was started), all modules started, and whether a participant completed a module. For our analysis KentuckianaWorks provided the participant data to KYSTATS who maintain the Kentucky Longitudinal Data System (KLDS).[4] The KLDS includes administrative data from a variety of sources. For our purposes KLDS contains individual records from all K-12 public schools, all non-proprietary post-secondary institutions in Kentucky, employer, wage and claims data from the unemployment insurance (UI) system, birth and death records from Vital Statistics, and driver's license data.[5] KYSTATS linked the Code Louisville to the KLDS using social security number and person name. It is from the KLDS data that we obtain our data on a participant's age, gender, race (White, Black, Asian, Other, Two or more, unknown), education (high school degree or GED, attend some college, associate degree or certificate,

[4] Due to data restrictions, employees with KYSTATS did the actual matching and ran all the regressions following instructions that we provided.
[5] KYSTATS has education data starting in 2008, so education information will be missing for anyone who completed schooling or left school prior to this date or attended school in another state or country.

bachelor's degree, master's degree or more, unknown education), quarterly employment status and earnings, and county of residence.[6, 7]

The initial data set provided by Code Louisville contained 2005 individuals who enrolled in the program. We restrict analysis to individuals who were between age 20 and 63 at the time of enrollment (start date of their first module), who lived in Jefferson, Bullitt, Oldham and Shelby counties when they enrolled in the program, and who had at least one quarter of nonzero earnings in the year prior to enrollment. This reduced the sample of participants to 1222 individuals, which is 60.9% of the original sample.[8]

The sample of potential comparison individuals consisted of people whose most recent residence, as of 2021, was Jefferson, Bullitt, Oldham and Shelby counties who were between 20 and 63 years old between 2013 and 2020. Participants in the Code Louisville program are excluded from the sample of potential comparison individuals.[9]

Matching occurs in two steps. First, treated individuals are exactly matched to individuals in the control sample based on gender, race and education.[10] This results in 1,212 treated individuals matched to approximately 4 million individuals in the control sample. Once this match is done, the quarter/year the treated individual starts the program is set as the zero quarter for both the treated and matched individuals We calculate the earnings in the year prior to treatment as the sum of total quarterly UI earnings in the four quarters prior to the zero quarter. Next, the initially matched sample is refined by matching treated individuals to the comparison individuals based on a match on age and also matching to all individuals whose four quarters of prior earnings was within 2% of the prior earnings of a treated individual.[11] This resulted in 1,108 individuals in the treated sample and 192,690 individuals in the comparison sample. We

[6] Race categories also include Hawaiian and other Pacific Islander, and Alaskan native or American Indian. However, since there were so few Code Louisville participants in these race categories, we had to drop them from the analysis for disclosure reasons. We also do not include people of these races in the potential comparison sample.
[7] While KentuckianaWorks collects much of this demographic data for participants, we limit ourselves to information available from the KLDS to ensure that we have comparable data for participants and our controls.
[8] We dropped 327 individuals because their age was missing or not between 20 and 63 or who had no UI wage information. We dropped an additional 456 individuals because they either had missing residency information or did not live in the Kentucky portion of the Louisville MSA.
[9] In order to improve the speed of the matching algorithm we also drop anyone in the potential comparison sample with quarterly earnings above $200,000. This number is the maximum value of quarterly earnings across all the participants, plus one standard deviation of the distribution of quarterly earnings across all participants rounded up to the nearest $25,000 increment.
[10] Matching is done with replacement so the same control individual could be matched to multiple treatment individuals.
[11] Because KLDS does not use birth month when determining age, age is calculated as calendar year minus birth year and is treated as a continuous variable. Age is matched through subclasses where each subclass contains of at most two different ages. 87.8% of subclasses contain two ages.

then assign weights for individuals in the comparison sample, where the weights are the inverse of the number of matches for person *i*, in the treatment group. The sum of these weights will equal the number of people in the treatment sample. We then repeat this matching process for our completers sample—that is Code Louisville participants who complete at least one module. This produces a treatment group of 635 individuals and a control group with 110,605 individuals.[12]

Table 1 provides basic summary statistics for the treatment groups and the weighted control groups. By construction, the weighted averages of the control group are identical for the exact match variables of gender, race and education. The weighted average of age is also identical between the two groups because we also employed an exact match on age. There are small difference in average earnings in the year prior to enrollment between the treated and weighted comparison sample because this is the only variable where we did not match exactly, but the differences are small (less than 2% given our matching strategy).

Table 1 shows that we do not know education for 37.7% of the enrolled group and 38.6% of the completers. There are two potential reasons for unknown education. The first is that the individual completed their education in Kentucky prior to 2008, which is the first year that KYSTATS has education data. This impacts both the treatment and control groups similarly so missing education should proxy for similar background characteristics. The second, and less common, reason is that an individual may have attended a private school or was educated outside of Kentucky, so no record exists in the state databases. Again, this should have a similar impact on the treatment and control groups. While a main source for race information is also the school data, we can obtain race from other sources in the KLDS, so race is missing less often than education.

While slight differences between the enrolled treatment group and the completed treatment group exist, none are large. The enrolled group has a slightly larger percent with some college (28.5%) than the completers (24.8%). Completers are also more likely to have a bachelor's degree (20.4% compared to 18.9%) or a master's degree (8.4% compared to 7.3%) suggesting more educated participants are more likely to complete at least one module of the program.[13]

In Figures 1 and 2 and we provide evidence on the evolution of earnings and employment for

[12] We experimented with using propensity score matching with a caliper, nearest neighbor matching and coarsened exact matching on all four quarters or earnings prior to the zero quarter. However, all these other techniques produced lower quality of matches.

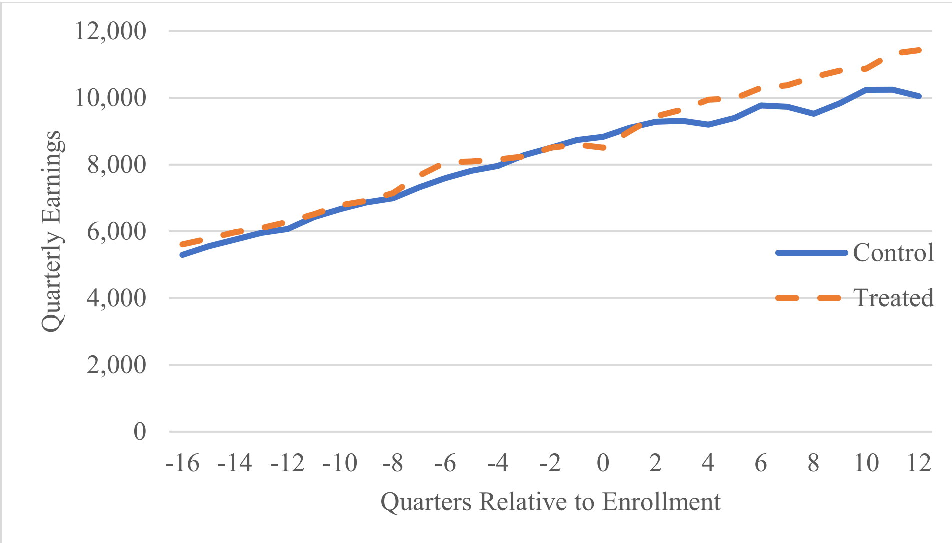
[13] Given that postsecondary education data began in 2008, the data is skewed towards younger people. The increased percentage in education by completers could suggest that completers are younger as opposed to more educated. We do not believe this is the case as the average age for both the enrolled and the completed treatment groups is 33.8 and the treatment groups should be similarly impacted by missing educational data.

Table 1: Summary Statistics for the Matched Sample of Enrolled Individuals and Individuals Completing at Least One Module

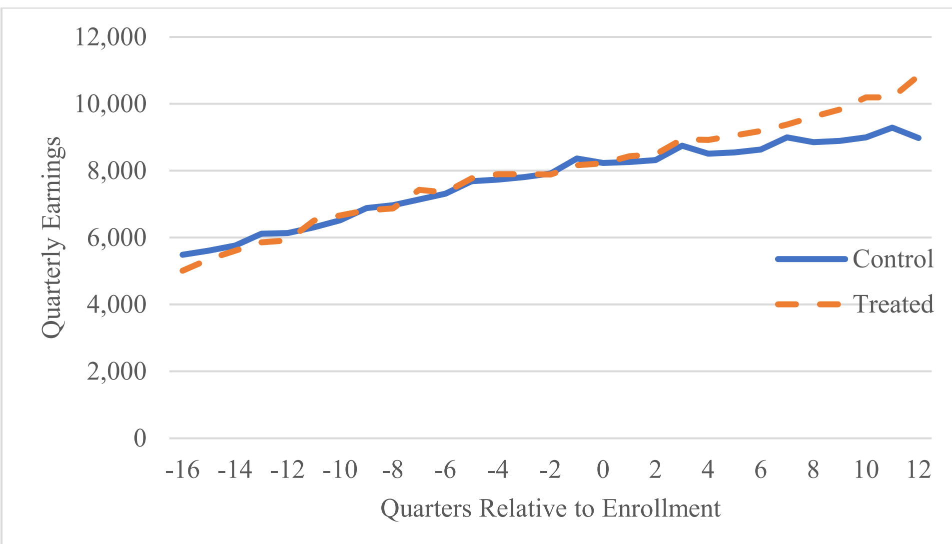
	Enrolled			Completed		
	Treatment	Weighted Control	Std. Difference	Treatment	Weighted Control	Std. Difference
Female	0.333	0.333	0.000	0.324	0.324	0.000
Asian	0.020	0.020	0.000	0.024	0.024	0.000
Black	0.138	0.138	0.000	0.107	0.107	0.000
Other race	0.013	0.013	0.000	0.014	0.014	0.000
Two or more races	0.011	0.011	0.000	0.005	0.005	0.000
Unknown race	0.078	0.078	0.000	0.087	0.087	0.000
White	0.741	0.741	0.000	0.764	0.764	0.000
High school or less	0.046	0.046	0.000	0.041	0.041	0.000
Some college, no degree	0.264	0.264	0.000	0.236	0.236	0.000
Associates or certificate	0.042	0.042	0.000	0.041	0.041	0.000
Bachelors degree	0.192	0.192	0.000	0.209	0.209	0.000
Masters or higher	0.079	0.079	0.000	0.091	0.091	0.000
Unknown education	0.375	0.375	0.000	0.381	0.381	0.000
Age	33.8	33.8	0.002	33.8	33.8	0.002
UI earnings 1 year prior to Enrollment	30011.67	30115.62	-0.004	31497.35	31539.47	-0.002
UI Earnings 2 years prior to Enrollment	27736.51	27309.76	0.016	28465.23	28583.94	-0.004
UI Earnings 3 years prior to Enrollment	24596.51	23930.62	0.026	24610.76	24842.71	-0.009
Employment 1 quarter after Enrollment	0.873	0.867	0.018	0.871	0.872	-0.004
Employment 1 year after Enrollment	0.776	0.763	0.031	0.787	0.774	0.033
Employment 2 years after Enrollment	0.765	0.724	0.097	0.781	0.734	0.114
Employment 3 years after Enrollment	0.741	0.704	0.084	0.772	0.715	0.136
UI Earnings 1 year after enrollment	34723.61	33802.38	0.032	36171.77	35274.14	0.031
UI Earnings 2 years after enrollment	38035.36	35240.82	0.090	39858.22	36749.45	0.099
UI Earnings 3 years after enrollment	38228.83	34772.88	0.103	41270.09	36374.55	0.145
Sample Size	1,108	192,690		635	110,605	

Figure 1: Quarterly Earnings by Quarter Since Starting Program, All Enrolled Participants

Panel A-Men



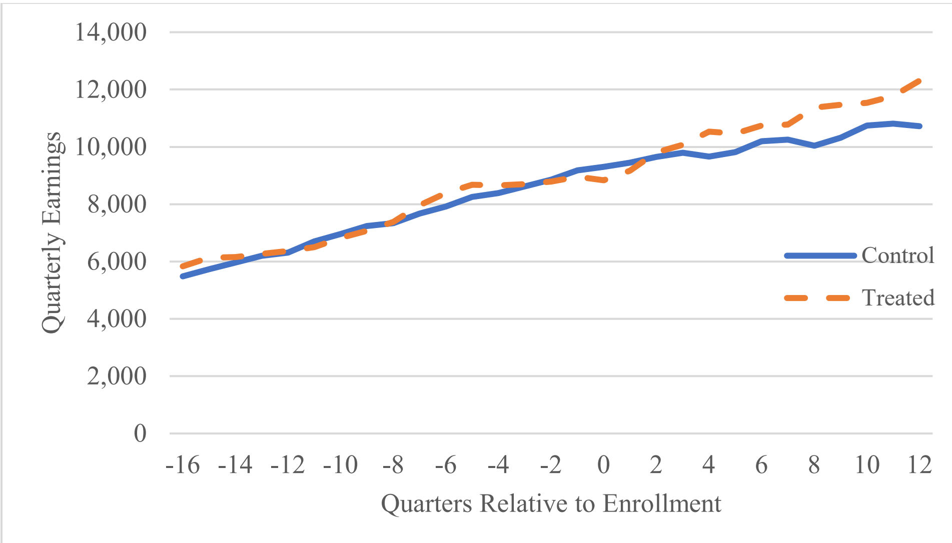
Panel B-Women



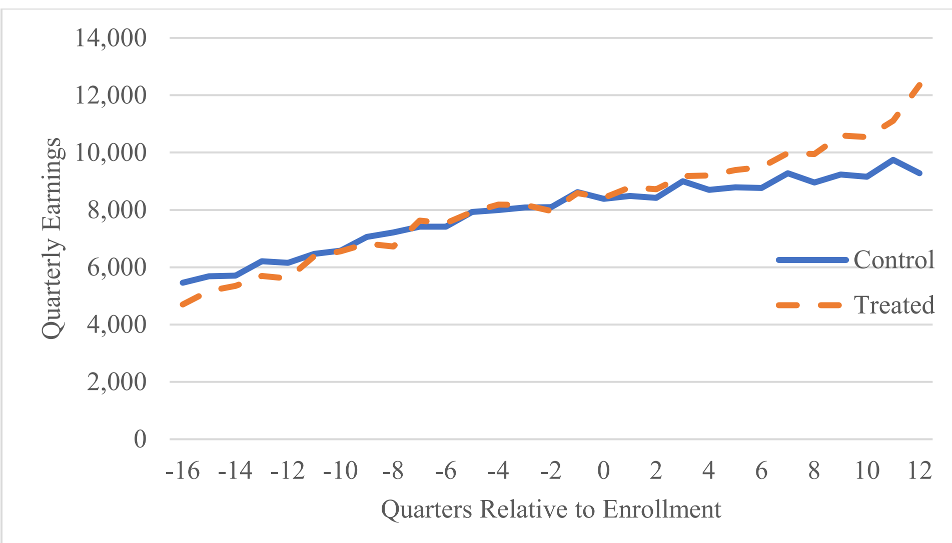
Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [*https://kystats.ky.gov/.*](https://kystats.ky.gov/)

Figure 2: Quarterly Earnings by Quarter Since Starting Program, Participants Completing At Least One Module

Panel A-Men



Panel B-Women



Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [*https://kystats.ky.gov/.*](https://kystats.ky.gov/)

both control and treatment groups by gender. Figure 1 graphs earnings by quarter relative to enrollment for both those who enrolled in the Code Louisville Program and the matched control group and Figure 2 plots the same information for our completers sample—individuals who completed at least one module and their matched control group. In these figures we see that for both men and women earnings rise steadily throughout the study period. Earnings growth for the control group appear to flatten during the last five quarters of the post treatment period while earnings for the treated sample continue to grow. Importantly we see that in both level and trends there is little difference between treatment and control groups prior to the quarter of entry.

In Figures 3 and 4 we plot the employment rates by gender for the enrolled and completers groups. Employment appears to rise relatively steadily for both groups until the treatment period when it begins to fall. This upward trend is the result of sample restrictions requiring some earnings in the four quarters prior to enrollment for matching purposes. Again, we note that pre-trends are nearly identical, particularly for men. Women in the treatment group have slightly lower employment rates than the control group, particularly 12 to 16 quarters prior to enrollment. However, the overall trends are quite similar, particularly in the last four quarters prior to enrollment.

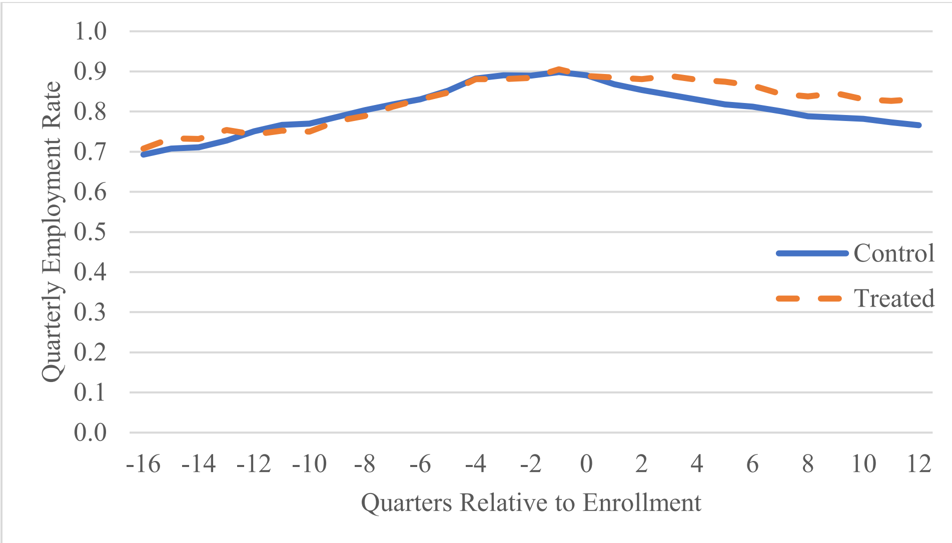
Table 1 and Figures 1-4 demonstrate the advantage of using these large longitudinal state datasets for evaluating programs. Using fairly simple matching it is possible to form large comparison samples that have observable characteristics that are nearly identical to almost any treatment sample.

Table 2 presents summary statistics for our treated enrollment sample (column 1 from Table 1), for KentuckianaWorks WIOA participants with an ITA or who participate in the M-TEC or CPT training programs, and for individuals in the Louisville MSA.[14] To ensure the samples are comparable we restrict the sample of individuals in the Louisville MSA to people between 20 and 64 years old. Comparing the enrolled treatment sample with other WIOA training participants and individuals in the Louisville MSA shows that participants in the Code Louisville program are different from participants in other WIOA programs and often different that the average individuals of similar ages in the Louisville MSA labor force. Women are only 33% of the participants yet are 52% of the Louisville MSA labor force. Comparing the Code Louisville data to data from the other training programs administered by KentuckianaWorks, women comprised 62% of the ITA participants, 30% of the CPT participants and 21% of the M-TEC participants. The gender distribution is similar to the two industrial training programs, but markedly different from the educational training program.

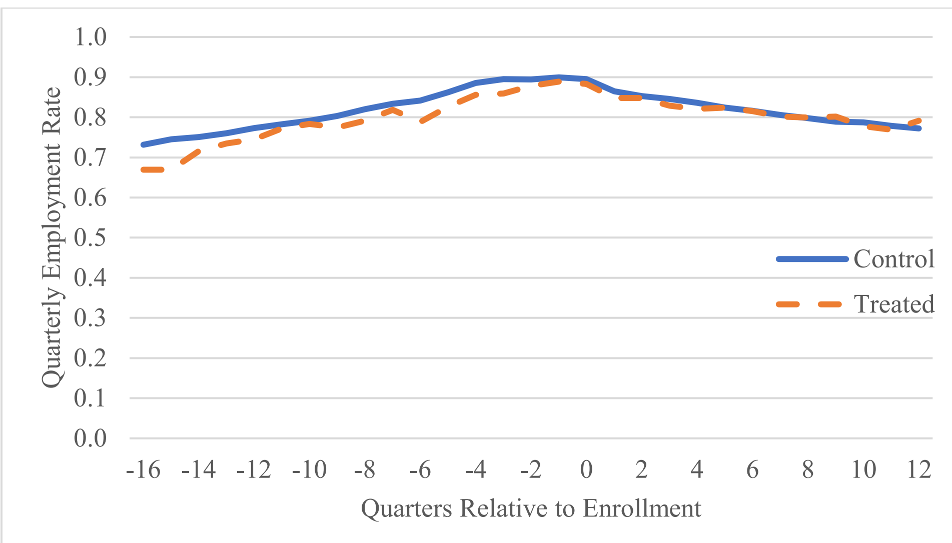
[14] Statistics for individuals in the Louisville MSA are obtained from the 2015 American Community Sample five-year panel.

Figure 3: Rates of Employment by Quarter Since Starting Program, All Enrolled Participants

Panel A-Men



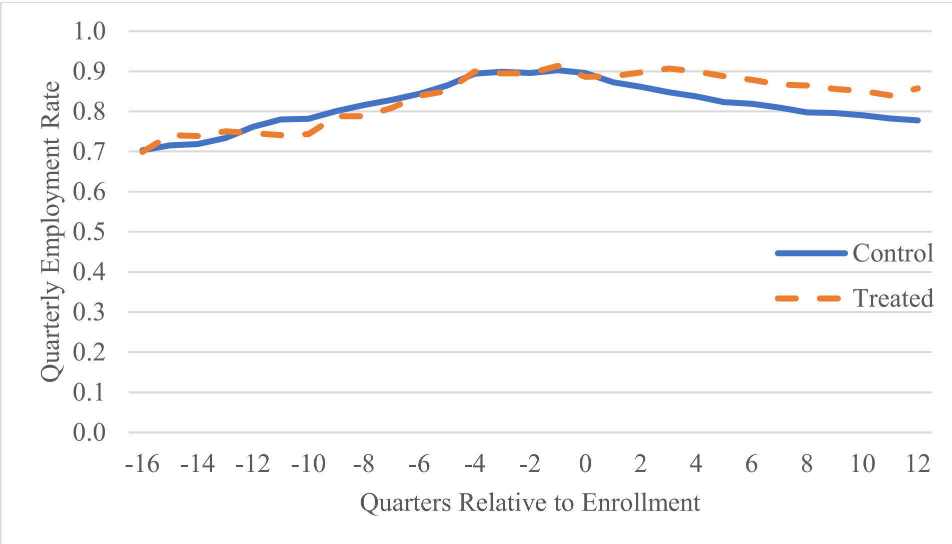
Panel B-Women



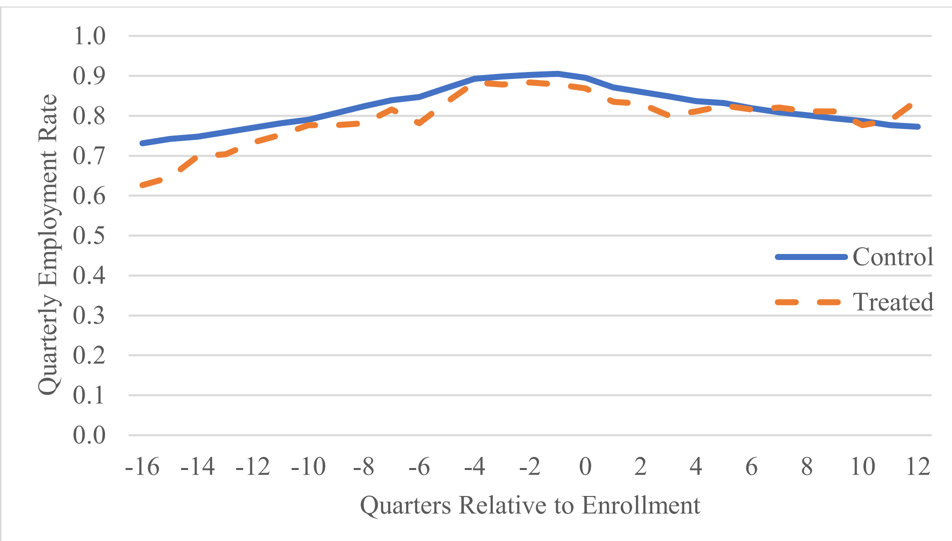
Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [https://kystats.ky.gov/.](https://kystats.ky.gov/)

Figure 4: Rates of Employment by Quarter Since Starting Program, Participants Completing At Least One Module

Panel A-Men



Panel B-Women



Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [https://kystats.ky.gov/.](https://kystats.ky.gov/)

Table 2: Summary Statistics for the Matched Sample of Enrolled Individuals in Code Louisville, Individuals in Other KentuckianaWorks WIOA Training Programs and Individuals in the Louisville MSA

	Matched Enrolled Treatment Sample	Other KetuckianaWorks WIOA Training Programs[a]			Louisville MSA[b]
		ITA	M-TEC	CPT	
Female	0.33	0.62	0.21	0.30	0.52
White	0.74	0.54	0.30	0.36	0.71
Black	0.14	0.43	0.65	0.61	0.23
Asian	0.02	0.00	0.04	0.01	0.02
Other race	0.01	0.03	0.01	0.09	0.04
Two or more races	0.01	N/A	N/A	N/A	N/A
Unknown race	0.08	N/A	N/A	N/A	N/A
High school degree or GED	0.05	0.61	0.57	0.53	0.40
Some college, no degree	0.26	0.16	0.20	0.17	0.20
Associates or certificate	0.04	0.07	0.05	0.07	0.08
Bachelors degree	0.19	0.06	0.08	0.09	0.19
Masters or higher	0.08	0.00	0.00	0.00	0.14
Unknown education	0.38	0.01	0.06	0.02	0.00
Age	33.84	33.5	37.4	37.1	37.3
Earnings[c]	$30,012	N/A	N/A	N/A	$64,713

Notes: a-Data taken from Tables 9-15 in Bollinger & Troske (2019). Data originally provided by KentuckianaWork and are for participants in the program between 2015 and 2018. b-Data from 2015 American Community Survey (ACS) five-year sample. We restrict the sample from the ACS to individuals between 20-64 years old. c-Earnings for the Code Louisville treatment sample are from the year prior to enrollment.

Missing data means that comparison to other WIAO programs at KentuckianaWorks or to statistics on the Louisville MSA is challenging. In making comparisons, we are going to assume race and education data are missing at random, and thus compare percentages for those whom race is measured. For example, those coded as white comprise 74% of the participants in Code Louisville data. Since race is missing for 8% of the treated sample, if we assume race is missing at random this means that 80% of the treated sample is white which is above the 71% of the population between 20 and 64 in the Louisville MSA that is white. Once we adjust for missing values blacks make up 15% of the Code Louisville participants, which is below the 23% of the Louisville MSA population that is black. It is also the case that individuals of other races are underrepresented in the Code Louisville program. We also see differences in race when we compare our Code Louisville sample with data on the other WIOA training programs. For example, whites comprise only 54% of individuals with an ITA and 30% and 36% of participants in the M-TEC and CPT programs, respectively. In addition, blacks comprise 43% of the ITA program, 61% of the CPT program and 65% of the M-TEC program, which is well above their representation in the Code Louisville program.[15]

Unfortunately, education is the least well measured of the variables we use, with 38% of the sample missing education. Only 3% of our sample had a high school degree or GED. If we adjust this number for missing education, this is still only 5% of the treated group. In the Louisville Metropolitan Area workforce 40% have a high school education or a GED. Similarly, in the ITA program over 61% of participants have a high school degree, in the CPT program over 53% of participants have a high school education, and in the M-TEC over 57% have a high school diploma.

Comparing educational attainment, we see that, once we adjust for missing values, 42% of Code Louisville participants have a bachelor's degree or higher which is slightly higher than the 33% of the Louisville MSA work force that has at least a bachelor's degree. Associate degrees make up 7% of participants after adjusting for missing values which is similar to the 8% of Louisville MSA workers who have an associate's degree. One large difference in education is between the 46% of participants that have "some college" but no degree compared to the 20% of workers in the Louisville MSA. The three other training programs have much lower educational attainment. Only 6% of the ITA participants have a bachelor's degree or more as do 9% and 7% of CPT and M-TEC participants, respectively. While 16% of ITA, 17% of CPT and 20% of M-TEC have some college, this is below share for the Code Louisville program.

---

[15] It should be noted that missing race information is common for these other programs as well.

This Code Louisville group is slightly younger, at 33 years on average, than the typical worker in the MSA (even restricting the MSA sample to people between the ages of 20 and 64). This is not surprising since younger workers are more likely to invest in training and education. The age of Code Louisville participants is comparable to the ITA program participants who are 33 years old on average and just slightly younger than the CPT and M-TEC programs participants who are 38 and 37 years old on average, respectively.

Average earnings for 20-64 year old labor force participants in the metropolitan area are $64,713 while Code Louisville participants had average earnings of $30,012 in the year prior to enrollment. There are several possible explanations for this difference. One factor may be that the average age of Code Louisville participants is lower. While we don't know the exact distribution of education because of the large percentage of people with missing education, some groups with lower earnings, such as those with "some college" are overrepresented in the Code Louisville sample, and this could also account for the lower earning. Finally, people with higher earnings are less likely to seek out training programs.

Table 2 makes clear that participants in the Code Louisville program are more advantaged than the typical participant in a WIOA program, but are somewhat less advantaged than the typical worker in the MSA.

V. Methodology

Our goal is to estimate the average effect of treatment on the treated (ATT) over time for participants in the Code Louisville programs using the matched data. Programs such as Code Louisville are voluntary, and individuals select into them based on their interests and expectations. ATT measures the impact of the training on the type of individual who chooses that training. A typical key assumption when matched data is that, conditional on observable characteristics used for matching, there is no selection into treatment (Conditional Independence). If the conditional independence assumption holds then Figure 1 and 2 below comparing earnings and employment post entry for our treatment and comparison samples show the basic ATT estimates. Unfortunately, we have limited conditioning variables (age, education, race and gender) for the matching so we choose to estimate regression models to control for any remaining differences between individuals in the treatment and control sample. In particular, we used observed earnings for three years prior to enrollment for all individuals, and in our preferred model, individual fixed effects to further address the choice to enter training. In some models, instead of fixed effects, we condition on race and educational status.

We estimate models using the panel data on earnings, log earnings or employment—our three outcome measures—on samples containing individuals in both the treatment and control groups (the matched samples). We estimated weighted regressions using the weights described above. Conditioning on past earnings or employment as well as including a person-specific fixed effect, which we do in our preferred model, should capture any remaining differences between the treatment and control samples that are fixed over time. Equation (1) shows our preferred model:

$$y_{it} = \alpha_i + \delta X_{it} + \sum_{s=-4}^{12} \beta_s D_{st} T_i + \sum_{s=-4}^{12} \gamma_s D_{st} + \sum_{r=2012}^{2020} \sum_{q=1}^{4} \theta_{rq} D_{rq} . \quad (1)$$

The variable y_{it} represents our three outcome measures, earnings in a quarter t, log earnings in quarter t or employment in quarter t (which equals 1 if someone is employed in the quarter).[16] T_i =1 for individuals in the treatment group and 0 for individuals in the control group. The variable D_{st} = 1 if s=t. This is a series of 17 dummy variables indicating each of the quarters starting four quarters prior to enrollment, the enrollment quarter (t=0) and then the 12 quarters after enrollment. Other controls include a cubic in age (X_{it}) and a variable for calendar year-quarter (D_{rq}). The coefficients α_i are individual fixed effects. The coefficients β_s, measure the relative earnings of the treatment group compared to the control group and is our estimate of the effect of treatment on the treated in the quarter. The quarterly dummies for calendar time control for macro-economic effects such as overall wage growth or recessionary pressures. We include these controls because our matching requires treatment and matched individuals to live in the same four counties, so essentially these controls capture any cohort effects.[17] This specification closely resembles the specification used in prior evaluations of job training programs such as Heinrich et al. (2013) and Andersson et al. (2022).

We also estimate several additional models to examine the robustness of our estimates from our preferred model. We start with equation 2:

$$y_{it} = \alpha + \phi T_i + \sum_{s=-4}^{16} \beta_s D_{st} T_i + \sum_{s=-4}^{16} \gamma_s D_{st} + \sum_{r=2012}^{2020} \sum_{q=1}^{4} \theta_{rq} D_{rq} \quad (2)$$

---

[16] When discussing our results for brevity we will focus on models where log earnings or employment are the outcome measures. Results for all outcomes are included in the online appendix (https://gattonweb.uky.edu/faculty/Troske/Working%20papers/Online%20Appendix%20for%20Code%20Louisville%20Paper.pdf).
[17] County of residence is determined by the most recent residence as of 2021.

where we include the treatment dummy separately from the interaction with D_{st} to capture possible differences in the outcome variable in quarters 16 to 5 prior to enrollment. We also estimate equation 2 dropping the main T_i variable but keep the interaction between T_i and D_{st}.

As another robustness check we estimate equation (3):

$$y_{it} = \alpha + \delta X_{it} + \phi T_i + \sum_{s=-4}^{16} \beta_s D_{st} T_i + \sum_{s=-4}^{16} \gamma_s D_{st} + \sum_{r=2012}^{2020} \sum_{q=1}^{4} \theta_{rq} D_{rq} \qquad (3)$$

where X_i includes the cubic in age as well as dummy variables for the six race groups (with Asian being the excluded group) and dummy variables for the six education groups (with unknown education being the excluded group). We again also estimate equation (3) dropping T_i but keep the interaction between T_i and D_{st}.

As we showed above, participants in the Code Louisville program tend to be more educated than nonparticipants. One possibility is that the return to the program varies by education level. In order to test whether effects are heterogeneous with respect to education we also estimate equation (4):

$$y_{it} = \alpha_i + \sum_{s=-4}^{16} \beta_s D_{st} T_i + \sum_{s=-4}^{16} \gamma_s D_{st} + \sum_{s=1}^{16} \varphi_s ED_i D_{st} T_i + \sum_{r=2012}^{2020} \sum_{q=1}^{4} \theta_{rq} D_{rq} \qquad (4)$$

where we add an interaction between the educational attainment of an individual using our six education classifications (ED_i), the training dummy and number of quarters since treatment.

In order to allow heterogenous treatment effects by gender, we estimate all of our models separately for men and women. In addition, we also estimate separate models for all participants who start the program and those who complete at least one of modules.

VI. Main Results

Estimates for all Enrollees

Our main specification provides a series of coefficients, β_s, tracing out the quarterly impact of enrollment or completion on the three main outcomes measures. As such, the most straightforward way to present our estimates is in graphs. We provide full sets of coefficients and standard errors from our estimation of equation 1 in the online Appendix Tables 1 and 2(https://gattonweb.uky.edu/faculty/Troske/Working%20papers/Online%20Appendix%20for%20Code%20Louisville%20Paper.pdf). For our initial results the treatment group includes everyone who started the program regardless of whether they complete any modules. For our results where earnings or log earnings are the dependent variable our analysis excludes quarters with zero earnings. We initially focus on the results from estimating equation (1) since this controls for time invariant characteristics like race and education before discussing our results from estimating the other specifications.[18] To reduce the number of figures in the main body of the paper we focus on the coefficients where log earnings or employment is the outcome variable. Figures showing the coefficients where earnings is the outcome are included in the online appendix (https://gattonweb.uky.edu/faculty/Troske/Working%20papers/Online%20Appendix%20for%20Code%20Louisville%20Paper.pdf).

Figure 5 presents our main results showing the estimated effect of treatment on log earnings for men and women. In each graph the blue line represents the coefficient estimates, while the two dashed grey lines provide the upper and lower 95% confidence interval of coefficient estimates.

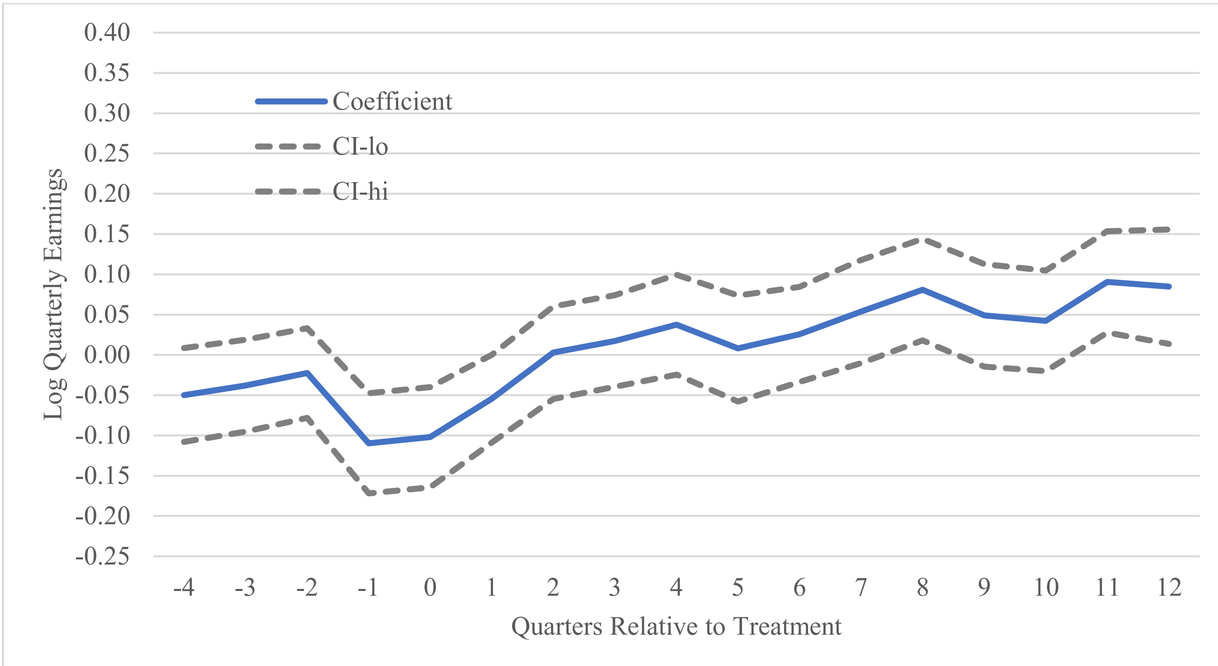
For both men and women there is a decline in log earnings relative to the comparison group in the quarter prior to enrollment and the quarter of enrollment. Beginning with the first quarter post enrollment, log earnings relative to the control group begin to rise. For men, by three quarters past enrollment, log earnings are above parity with the control group while for women this occurs within two quarters. Both men's and women's earnings continue to rise relative to the control group, although the rise is most pronounced for women. By the last four quarters in our data, treated women earn over 15% more on average per quarter than their control counterparts and this difference is statistically significant. For men, the difference is less pronounced with the gain being less than 10% at the end of the data and only statistically significant in the 8th quarter and 12th quarter past enrollment.

Figure 6 presents results where employment is the dependent variable, with a 1 indicating someone is employed (has reported earnings in a quarter). These figures show that treated women have higher employment rates than women in the control group, but this difference is insignificant in the three quarters leading up to enrollment and in the quarter of enrollment. For men, the employment gain relative to the control group is immediate and rapid; by three quarters post enrollment, employment is 5 percentage points higher, on average, among treated men than their control group and employment remains between 5 and 6 percentage points higher for the remaining two years of the study. In contrast, women see much smaller gains in employment. The treated group's initial 2 percentage point advantage in employment rises to approximately 4 percentage points by the end of the data but remains largely statistically insignificant.

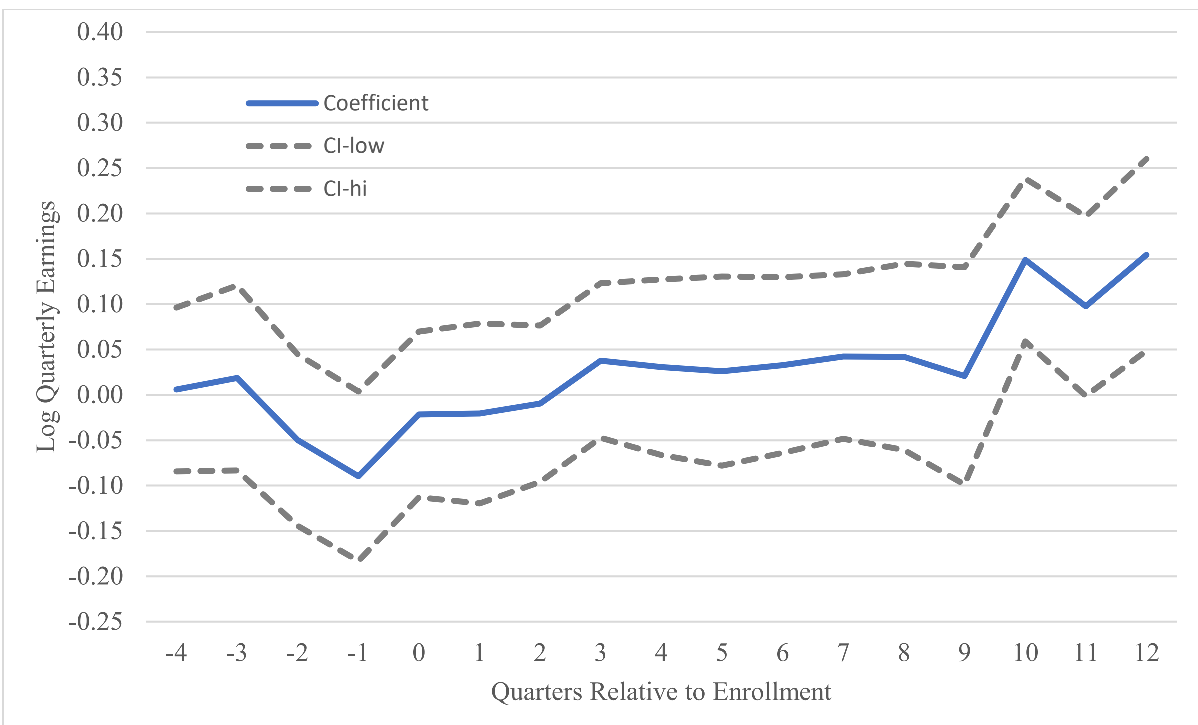
[18] While technically education is time variant, only a small portion of the sample had any changes in the degrees attained during our sample period.

Figure 5: Effect of Treatment on Log Quarterly Earnings by Quarter, Estimation of Equation 1, All Enrolled Participants

Panel A-Men



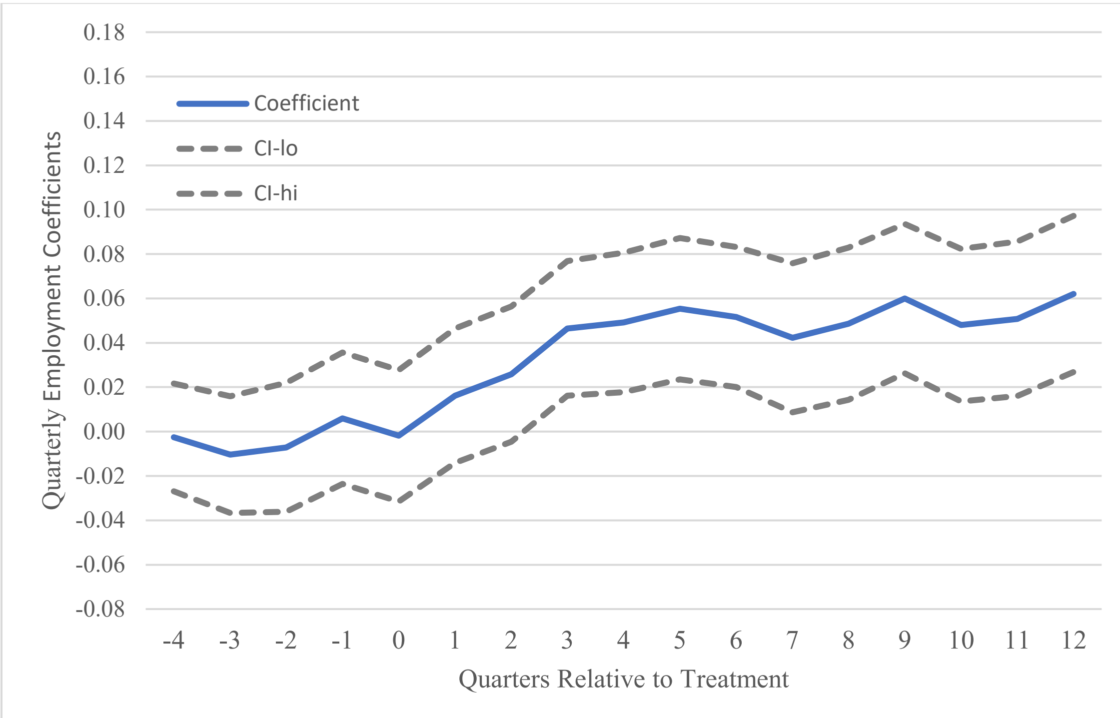
Panel B-Women



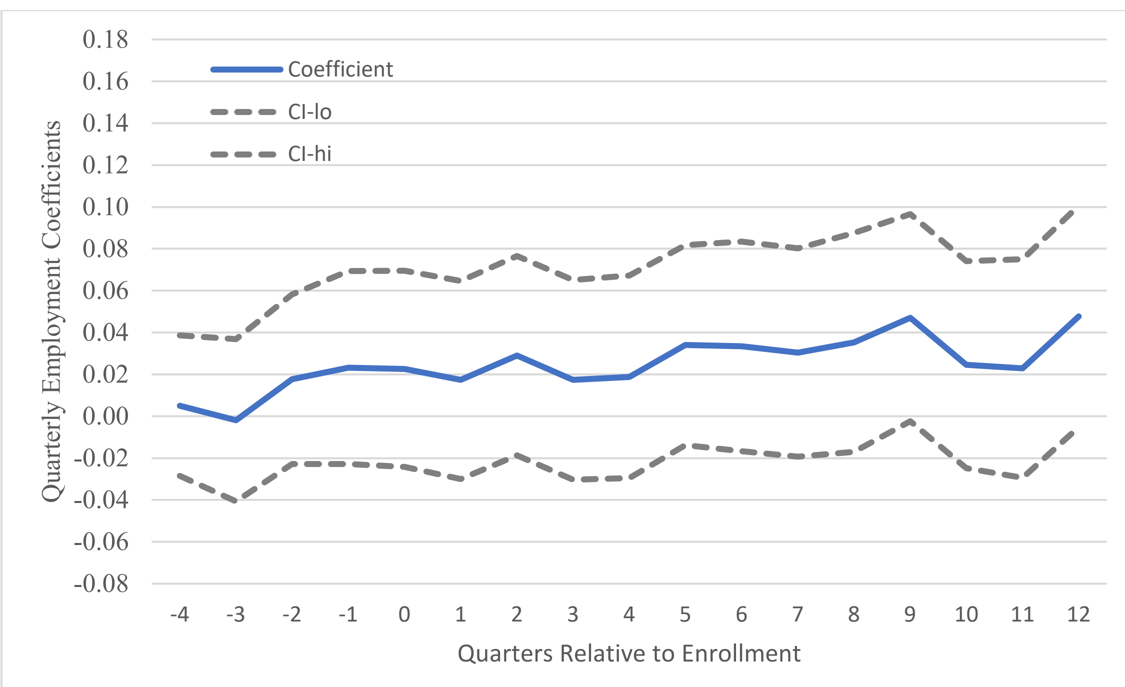
Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: https://kystats.ky.gov/.

Figure 6: Effect of Treatment on Employment by Quarter, Estimation of Equation 1, All Enrolled Participants

Panel A-Men



Panel B-Women



Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [https://kystats.ky.gov/.](https://kystats.ky.gov/)

Combining our results on earnings and employment we see that for men in the treatment group the gain is largely in employment, although men do experience modest gains in log earnings and the increase in earnings occurs later in the period. For women, we do not see significant gains in employment, but we see fairly large gains in log earnings among the treatment sample soon after enrollment and these gains continue to grow through the end of the data.

Estimates for Completers

Our second analysis focuses on the subsample of individuals who enroll in the program and who complete at least one module of the program. Figures 7 and 8 present the coefficient estimates for men and women, for each of the two outcomes, log earnings and employment, from estimating equation 1 only including data for completers and their matched control group.[19] The results, while similar to the results for all participants, tend to be larger in magnitude but also have larger standard errors due to smaller sample sizes.

Figure 7 again shows that men have smaller gains, on average, in log-earnings than women especially after enrolling in the program. Men's log earnings gains do reach 5% by the last year of the study but are not statistically significant. Women's log earnings again start growing strongly soon after enrollment and this growth continues till the end of the data, with average log earnings gains reaching 20%. As with the enrolled group, men's employment (Figure 8) rises quickly to a nearly 8 percentage point gain, on average, relative to the control group by the end of the first year after enrollment and remains above the control group, and statistically significantly, through the remainder of the study period. Women are much slower to show gains in the probability of being employed but by the last two quarters of the data women's employment rises sharply to over 10 percentage points higher than the control group. However, large standard errors make most of these differences statistically insignificant.

While we focus our attention on the enrolled group, focusing on intent to treat, as most job training studies do, it is clear that the differences in employment outcomes are largely due to the completers. The basic findings that men gain in employment early and earnings late, while women gain more in earnings and these gains occur early in the period, but they experience smaller and later gains in in employment, are all larger among the groups that completes at least one module of the program.

Checks on Robustness

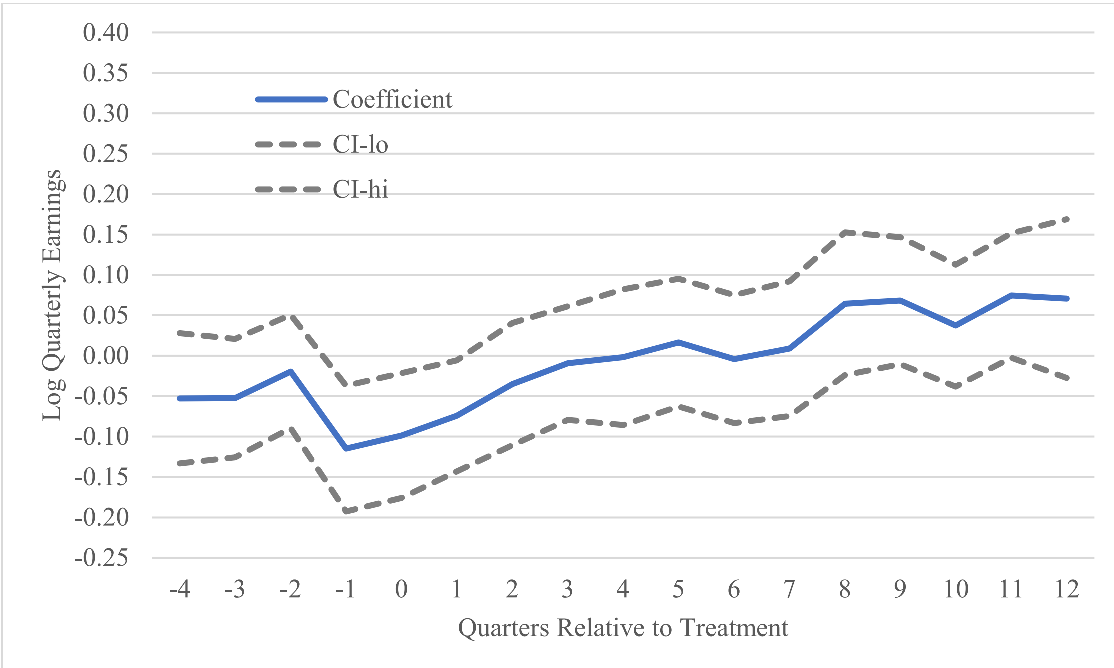
In order to examine the robustness of our results we estimate equations 2 and 3 both including

---

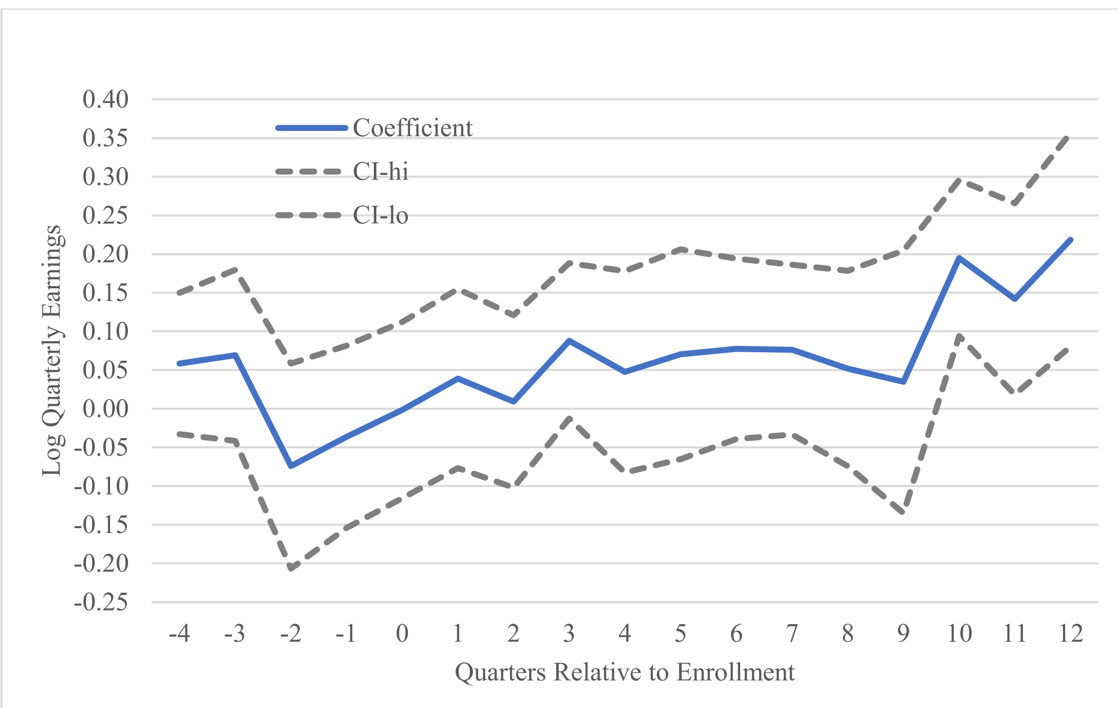
[19] Results where earnings is the outcome variable are include in the online appendix (https://gattonweb.uky.edu/faculty/Troske/Working%20papers/Online%20Appendix%20for%20Code%20Louisville%20Paper.pdf).

Figure 7: Effect of Treatment on Log Quarterly Earnings by Quarter, Estimation of Equation 1, Participants Completing At Least One Module

Panel A-Men



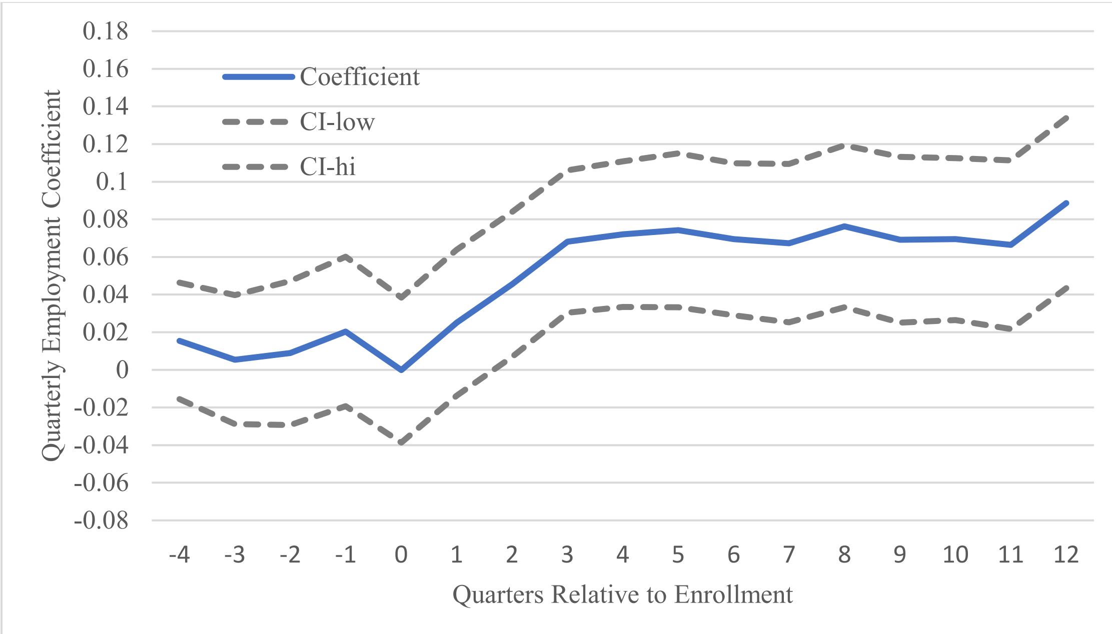
Panel B-Women



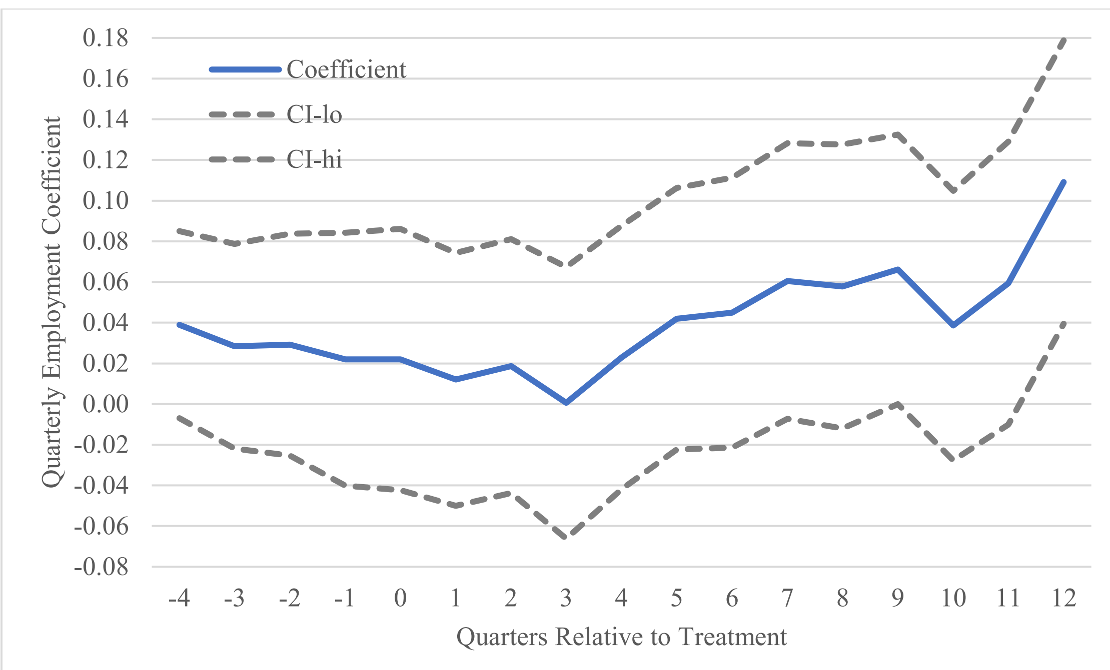
Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: https://kystats.ky.gov/.

Figure 8: Effect of Treatment on Employment by Quarter, Estimation of Equation 1, Participants Completing At Least One Module

Panel A-Men



Panel B-Women



Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: https://kystats.ky.gov/.

and then dropping the treatment dummy, T_i. These specifications allow us to examine more carefully whether our matching fully accounts for differences in the groups, and whether remaining differences might impact the estimates from our main model specification. It should be noted that the main specification should absorb systematic, time invariant differences in the treatment and control groups, but will not capture time varying systematic difference between the control and treatment samples.

Figure 9 presents results for men and women where log earnings is the dependent variable and Figure 10 presents results where employment is the dependent variable.[20] We include the plot of the coefficient estimates from equation 1 for comparison. In all estimates the treatment groups include all participants, regardless of whether they completed a module.

Examining these figures, we can see that the estimated impacts over time are quite similar across the different model specifications. However, for the models where log earnings is the dependent variable, the estimates for the regressions that include the treatment dummy lie below the estimates that do not include the treatment dummy with the estimates from our preferred model lying in-between. This ordering of the estimates suggests that there are some systematic differences in prior log earnings between the treatment and control group that remaining after matching. However, over time the differences between the estimates from the models decline, suggesting that by the end of the period the impact of any initial differences has disappeared.

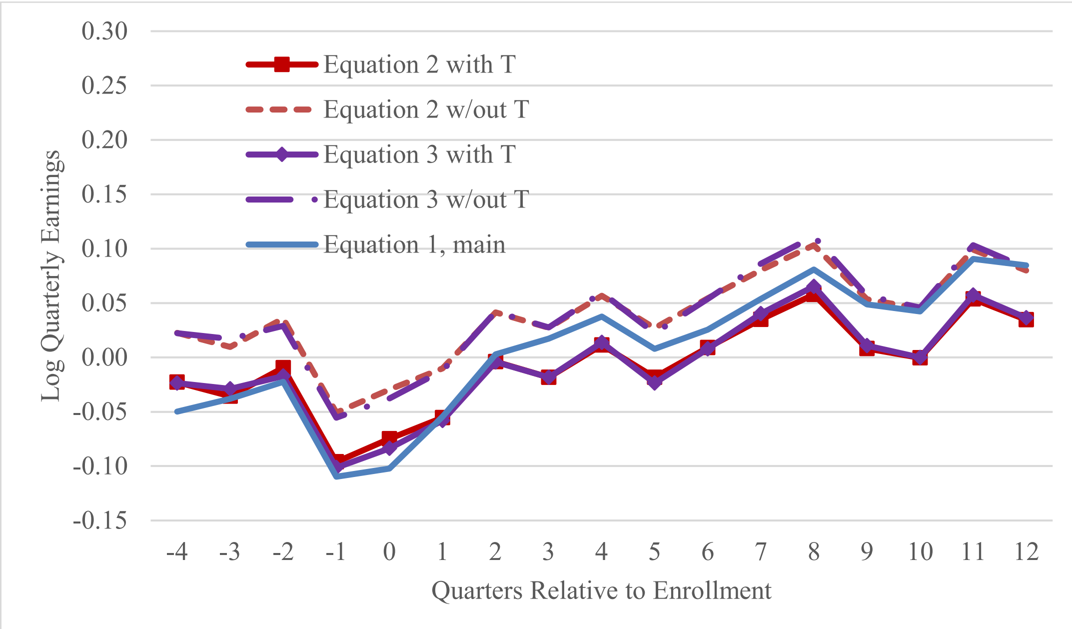
The estimates for men where employment is the outcome are nearly identical across all the models. For women we see that the estimated impact for the models including the person specific fixed effect or the treatment dummy are nearly identical but lie above the estimates dropping the treatment dummy. Similar to the men, the relative positions suggest that there are remaining differences in employment between the treatment and control sample, but these differences are being captured either by the treatment dummy or the person specific fixed effect.

Despite some differences, the parallel coefficients on treatment and the similarities in the estimates imply that our conclusions are robust and lend support to our preference for the model that includes person specific fixed effects. In addition, all of these models support our main conclusions--the largest gains from treatment for men occur in employment, not wages, while largest gains for women are in wages, not employment. Further, post-treatment trends in outcomes are seen in all specifications.

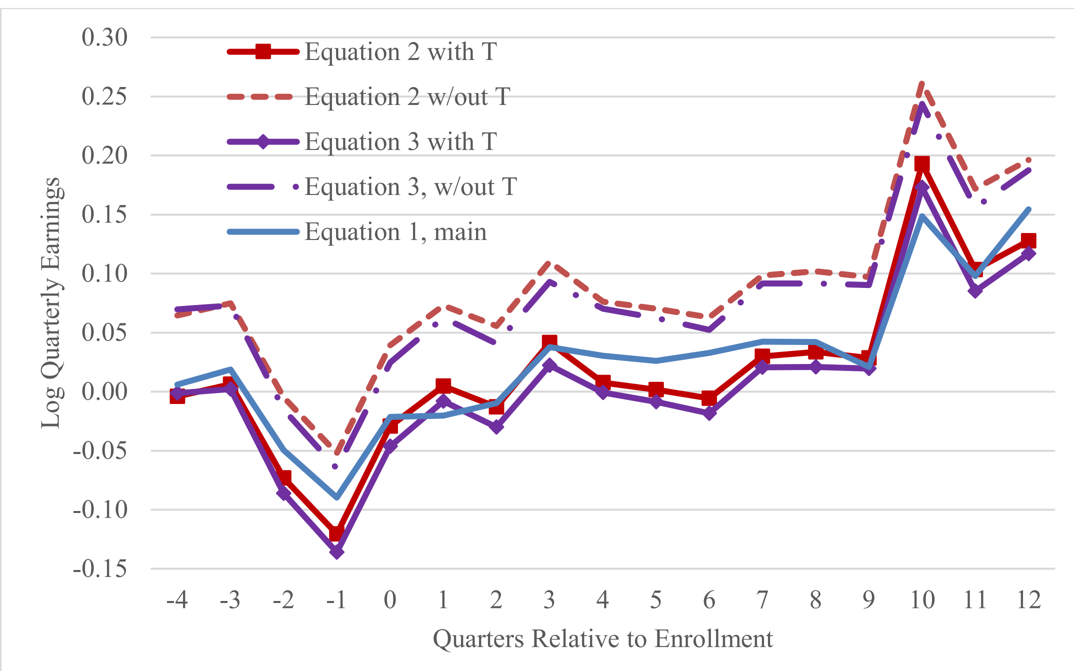
[20] All the coefficient estimates and standard errors for all our regressions are presented in online Appendix Tables 3 and 4. Appendix Figure 3 presents the graphs where earnings is the outcome (https://gattonweb.uky.edu/faculty/Troske/Working%20papers/Online%20Appendix%20for%20Code%20Louisville%20Paper.pdf).

Figure 9: Estimated Effects of Treatment on Log Quarterly Earnings by Quarter Across Different Model Specification, All Enrolled Participants

Panel A-Men



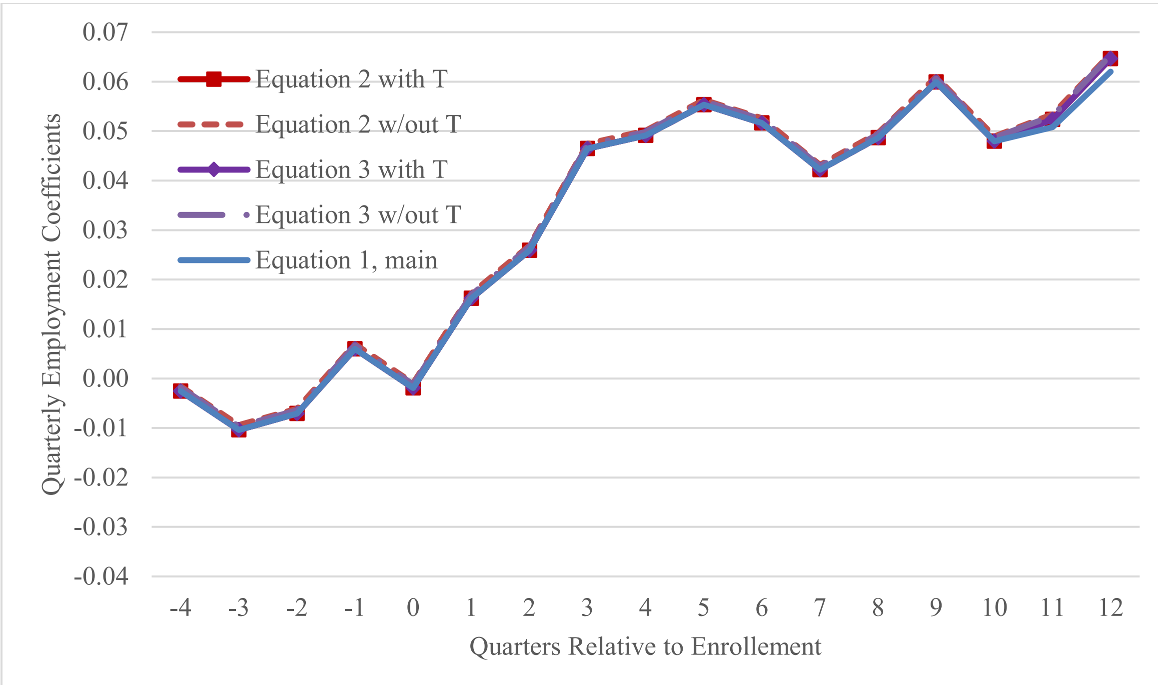
Panel B-Women



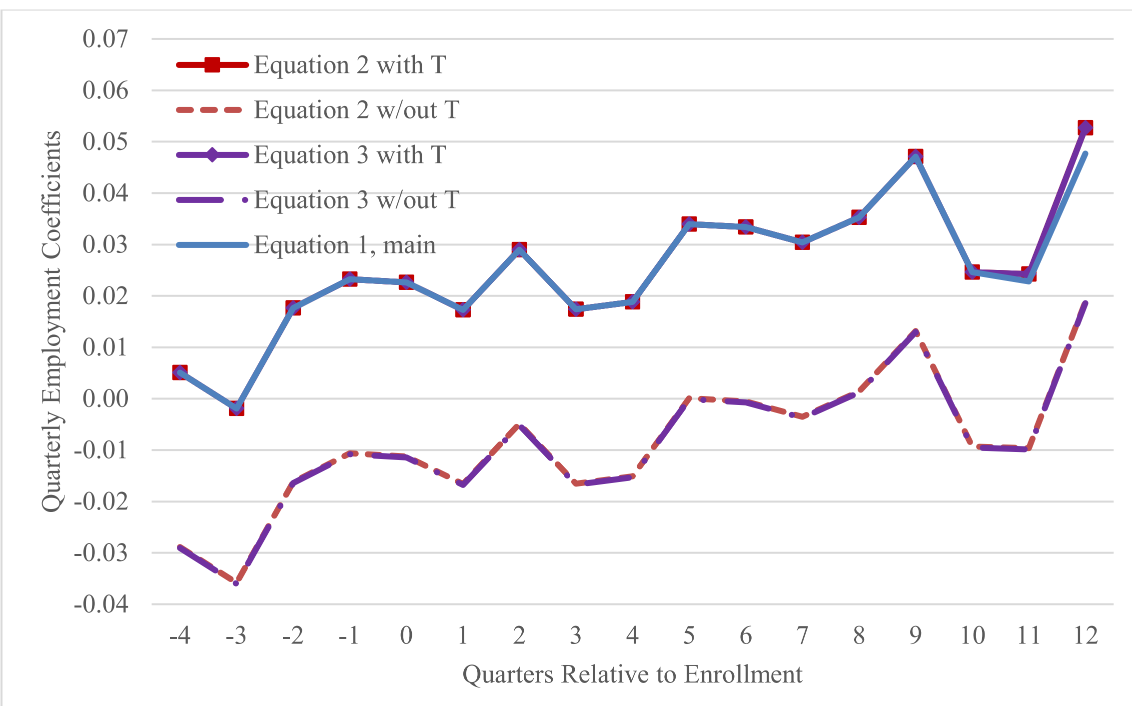
Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [*https://kystats.ky.gov/.*](https://kystats.ky.gov/)

Figure 10: Estimated Effects of Treatment on Employment by Quarter Across Different Model Specification, All Enrolled Participants

Panel A-Men



Panel B-Women



Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: [https://kystats.ky.gov/.](https://kystats.ky.gov/)

Heterogeneity by Education

Next, we present our results from estimating equation (4) in which we allow the quarterly impact of treatment to vary by the education of the participant prior to entry. One hypothesis is that the impact of the Code Louisville program may vary by educational attainment prior to starting the program, but a priori it is not clear whether the impact would be higher for those with more or less education.[21] Figures 11 and 12 present our results by education group and by gender for log-earnings and employment, respectively. Our separate estimates by education largely mirror our previous results. Looking initially at Figure 11, we continue to see the rise in earnings for men later in the period among all education groups except for individuals with missing education. The gains in log earnings are particularly large for those with a bachelor's degree and with just a high school degree, followed by those with a master's degree. All three of these groups experience gains in log earnings relative to the comparison groups of over 20% by four years after enrollment. We also see large gains in log earnings for women with a master's and bachelor's degree. The gain in earnings for other groups are much more erratic over time reflecting the fact that we have too few women in these groups to obtain an accurate estimate of the gains in earnings.

In Figure 10 we see the employment gain for men is largest among those with a bachelor's or some college, but individuals with unknown educational level also experience some of the largest gains in employment. Since individuals with unknown education are either older or individuals who migrated to Kentucky after completing their education, this may indicate that many people in this group are investing in new skills so that they can switch to a new industry or occupation. We note that while initially the employment gain to those with a high school degree or less is variable and even negative, by the last year of the data it rises dramatically, although the differences are not statistically significant.

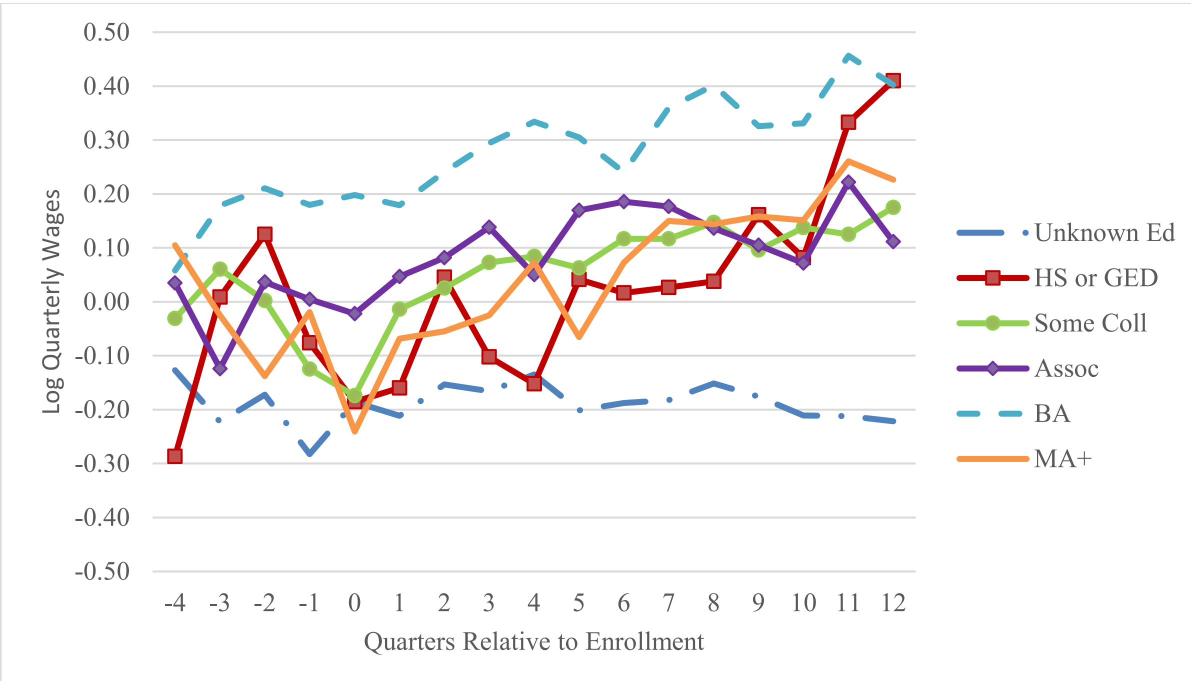
The changes in employment for women are more complicated. While for most educational groups, there is little employment effect among women, we see that woman with master's degrees have a dramatic increase in the probability of being employed. Conversely, women with an associate degree or a high school degree appear to experience a decline in employment relative to people in the control group. Although not presented, the positive impact is largely driven by completers, while the negative impact is driven by those who fail to complete the program.

---

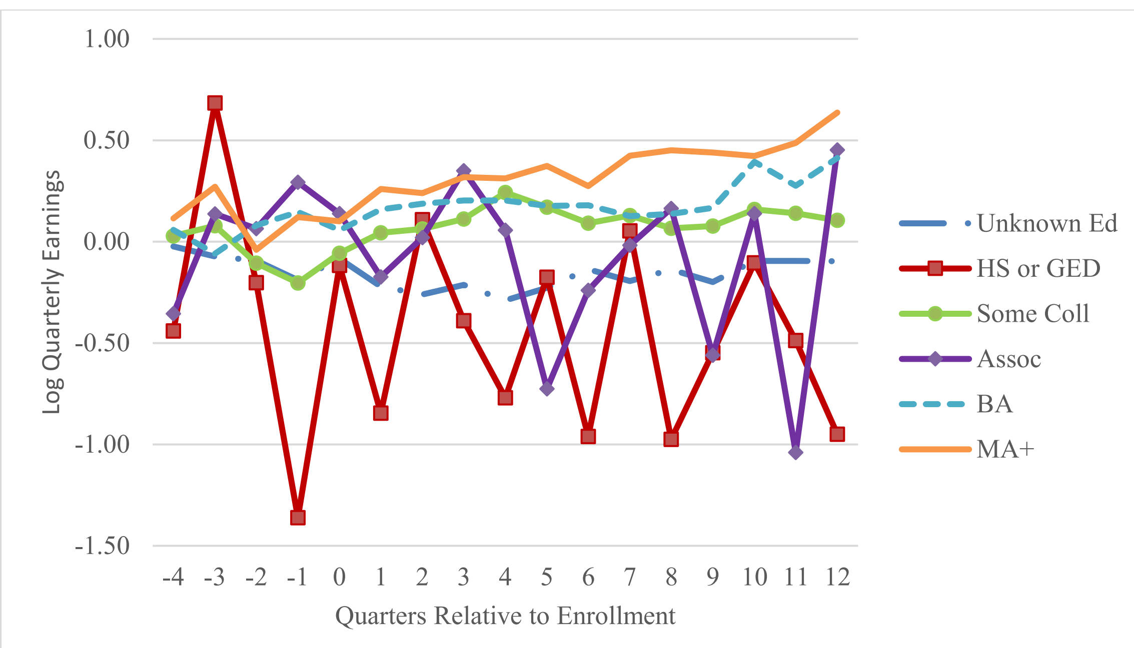
[21] Examining the coefficients on the education variables from our estimations of equation 3 (online Appendix Tables 7-10 https://gattonweb.uky.edu/faculty/Troske/Working%20papers/Online%20Appendix%20for%20Code%20Louisville%20Paper.pdf) shows no clear pattern of earnings or employment differences by education prior to entry. Generally, it is the case that individuals with more education have higher earnings and employment rates on average, but this is true for prime age workers. For younger workers, such as those in the code Louisville program, the pattern of earnings and employment by educational attainment is less clear.

Figure 11: Estimated Effects of Treatment on Log Quarterly Earnings by Education and Quarter, All Enrolled Participants

Panel A-Men



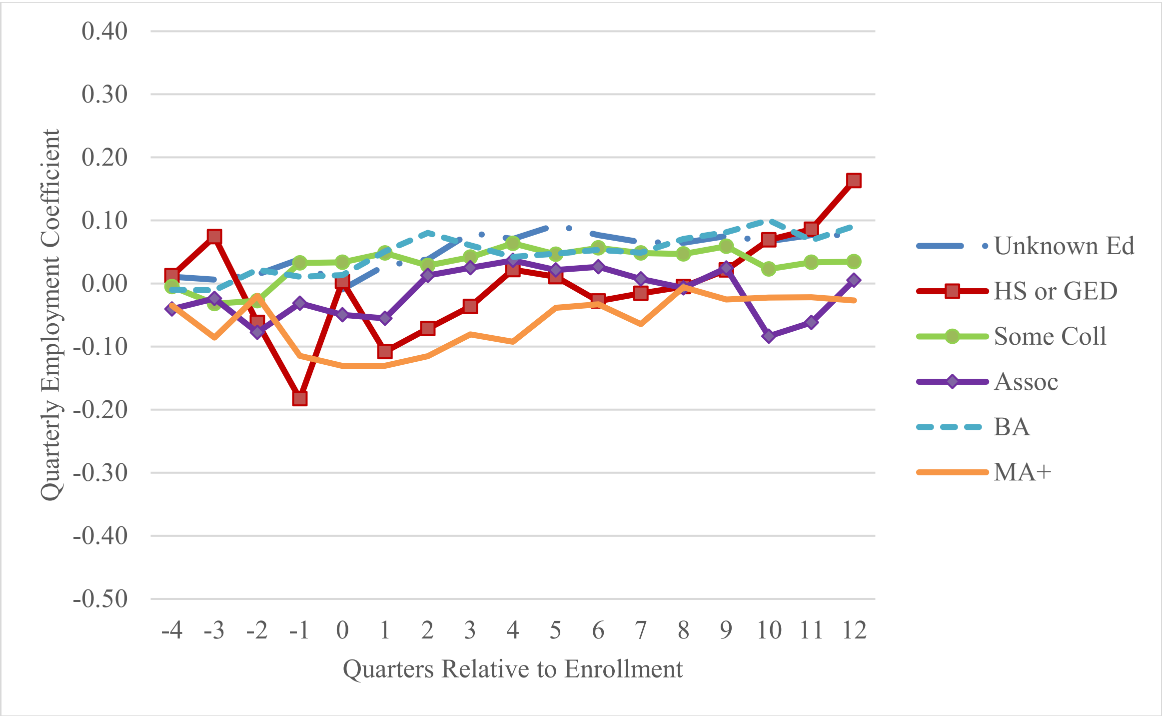
Panel B-Women



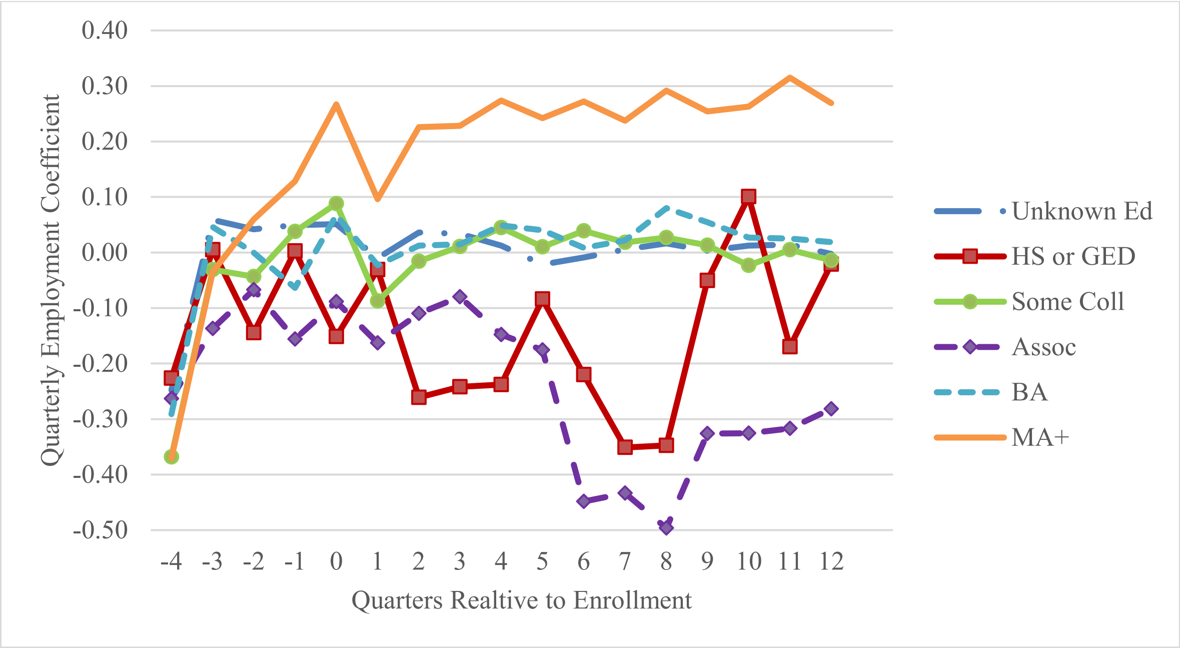
Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: *https://kystats.ky.gov/.*

Figure 12: Estimated Effects of Treatment on Employment by Education and Quarter, All Enrolled Participants

Panel A-Men



Panel B-Women



Source: Kentucky Longitudinal Data System, Kentucky Center for Statistics: https://kystats.ky.gov/.

Overall, these results show that there is substantial heterogeneity in the estimated impacts of the Code Louisville program with people with a bachelor's degree or more experiencing the biggest increases in earnings and employment and those with less education experiencing little measurable benefit. Likely the individuals with more education are those who are looking to change their career. For women, the Code Louisville program may also provide a new set of skills allowing them to re-renter the labor market after a period of inactivity.

Comparison with other job training programs

Comparing the estimated impacts of the Code Louisville program to estimated impact reported in recent evaluations of other job training programs we find they are broadly similar. For example, the recent evaluations of WIA by Heinrich et al. (2013) and Andersson et al. (2022) report similar gains in earnings that for participants in the WIA Adult Worker program that we find for participants in Code Louisville. Both studies find that earnings differences grow over time, reaching a maximum 2-3 years after entering the program. In addition, both the Heinrich et al. evaluation and the Card et al. (2018) meta-analysis of job training programs find that earnings gains are larger for women compared to men, something we also find (Andersson et al. does not find differential effects for men and women). One notable difference between our study and these previous studies in that we find much larger employment effects for men.

Comparing our estimated effects to evaluations of other sectoral training programs, we find that our estimated impacts are somewhat more modest than the estimated effects from these other programs. For example, Baird et al. (2022) reports earnings effect from treatment of around 12% and employment effects of around 14 percentage points, while Katz et al. (2022) report impacts on earnings of between 12% and 34%. One possible reason for the somewhat smaller impacts of the Code Louisville program is that in the other sectoral programs much of the training is in person, while in the Code Louisville program all the training occurs online. If online training is less effective than in-person training, then this could produce the smaller estimated impacts. However, when considering the potentially smaller impacts for the Code Louisville program it is important to consider differences in the cost of operating the programs. Analysis in Bollinger and Troske (2019) shows that the Code Louisville program is substantially less costly to operate on a per-participant basis than traditional WIOA training program. Given this, it may not be surprising that the benefits are smaller, but it also may be the case to the benefits per dollar spent are larger for the Code Louisville program.

V. Conclusions

While most recent evaluations of federal job training programs suggest that these programs produce positive labor market benefits for participants (Mueser and Troske, 2023) the impacts are fairly modest. One promising avenue for increasing the returns to training have been sectoral training programs which are designed to provide skills to participants that would enable them to obtain high paying jobs in growing sectors such as health care, advanced manufacturing and IT. However, many of these programs have been developed by non-governmental entities, leaving open the question of whether they can be implemented in federal job training programs such as WIOA.[22] In this paper we present evidence on the impact of the Code Louisville program, which is a program designed to provide participants with the skills necessary to obtain a job as a coder and is administered by the local WIOA Workforce Development Board in Louisville, KY.

Our estimates suggest that the Code Louisville program provides positive labor market benefits for both male and female participants, although the type and timing of benefits differ. Male participants see a fairly quick and large gain in employment relative to the control group of 5 percentage points within one year of enrolling in the program that persists through the end of the data but experience a fairly modest gain in quarterly earnings relative to the control group of between 5-10% by three years after enrollment. In contrast, women experience a fairly rapid gain in quarterly earnings of between 5-10% within one year of enrollment, which grows to between 10-15% by three years after enrollment. Women also experience around a 3 percentage point gain in employment relative to the control sample, but not until three years after enrollment. We further show that most of these increases accrue to participants who complete at least one module in the program. Participants who fail to complete any part of the program show insignificant benefits.

We also find that the biggest improvements in labor market outcomes occur for participants with the highest levels of education—a bachelor's degree or higher. Participants with less education typically do not show a statistically significant improvement in labor market outcomes, although participants with an unknown amount of education do show a significant increase in the probability of being employed relative to the control sample. While we do not advocate further restricting entry into the program, we think these results suggest it may be beneficial to advise those with lower educational attainment that other programs may be of more benefit.

We speculate that one reason the Code Louisville program produces somewhat smaller returns than other sectoral training programs is that most of the training in the Code Louisville program is

[22] See the Baird et al. (2022) for an example of a sectoral labor program implemented as part of WIOA.

provided online as opposed to the in-person training provided in the other sectoral training program. However, while the Code Louisville program does produce somewhat lower returns than these other programs, this training appears to be provided at a significantly lower cost that the training provided in other programs (Bollinger and Troske, 2019) suggesting that online training may be an efficient way to provide training for certain types of occupations and for participants who are able to participate in online training.

Given the declining funding going to federal job training programs (Mueser and Troske, 2023) it is imperative that we continue to explore new and novel ways to provide job training programs to people in need of these services. Our estimates of the impact of the Code Louisville program provides some initial evidence that this program is providing positive improvements in the labor market outcomes for program participants, which we believe justifies continuing the Code Louisville program and expanding it to other location in the country.

References

Andersson, F., Holzer, H. J., Lane, J. I. Rosenblum, D., and Smith, J. A., (2022). "Does Federally-Funded Job Training Work? Nonexperimental Estimates of WIA Training Impacts Using Longitudinal Data on Workers and Firms," Posted in advance of publication, *Journal of Human Resources*, (July), doi:10.3368/jhr.0816-8185R1.

Autor, D., Katz, L. and Krueger, A. (1998). "Computing Inequality: Have Computers Changed the Labor Market?" *Quarterly Journal of Economics* (Nov.) pp. 1169-1213.

Baird, M, D., Engberg, J., and Gutierrez, I, (2022). "RCT Evidence on Differential Impact of US Job Training Programmes by Pre-Training Employment Status," *Labour Economics* 75 (April), https://doi.org/10.1016/j.labeco.2022.102140.

Bloom-Weltman, J. (2019). "Statewide Longitudinal Data Systems (SLDS) Survey Analysis." NCES working paper 2020-157, Washington D.C. (October).

Bollinger, C. R., and Troske, K. R., (2019). "Evaluation of Code Louisville Training Program," Center for Business and Economic Research Report.

Borghans, L. and ter Weel, B. (2004). "Are Computer Skills the New Basic Skills? The Return to Computer, Writing and Math Skills in Britain." *Labour Economics*, Vol. 11, pp. 85-98.

Card, D., Kluve, J., and Weber, A.,(2018). "What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations," NBER Working Paper 21431, (July 2015), doi: 10.3386/w21431. Copy with same title is published in *Journal of the European Economic Association* 16 (June), 894-931.

DiNardo, J. and Pischke, J.-S. (1997). "The Computer Use Revisited: Have Pencils Changed the Wage Structure Too?" *Quarterly Journal of Economics*, Vol. 112, pp. 291-303.

Doms, M., Dunne, T. and Troske, K. (1997). "Workers, Wages, and Technology." *Quarterly Journal of Economics*, Vol.112, pp.253-290.

Green, F. (1999). "The Value of Skills." University of Kent, Studies in Economics, Working Paper Series, #9819.

Entorf, H. and Kramarz, F. (1996). "Does Unmeasured Ability Explain the Higher Wages of New Technology Workers?" *European Economic Review,* Vol. 41, pp.1489-1509.

Fortson K., Rotz D., Burkander P., Mastri A., Schochet P., Rosenberg L., McConnell S. and D'Amico, R. (2017). "Providing Public Workforce Services to Job Seekers: 30-Month Impact Findings on the WIA Adult and Dislocated Worker Programs," Social Policy Research Associates and Mathematica Policy Research, (May), https://www.mathematica.org/publications/providing-public-workforce-services-to-job-seekers-30-month-impact-findings-on-the-wia-adult.

Hamilton, B. (1997). "Returns to Computer Skills and Black-White Wage Differentials." John M. Olin School of Business, working paper.

Heckman J.J., Smith J.A. (1999). "The pre-programme earnings dip and the determinants of participation in a social programme: Implications for simple programme evaluation strategies." *Economic Journal* 109: 313–348. doi: 10.1111/1468-0297.00451

Heinrich C.J., Mueser P.R., Troske, K.R. (2008). "Workforce Investment Act Non-Experimental Net Impact Evaluation: Final report."

https://wdr.doleta.gov/research/FullText_Documents/Workforce%20Investment%20Act%20Non-Experimental%20Net%20Impact%20Evaluation%20-%20Final%20Report.pdf.

Heinrich, C.J., Mueser, P.R., Troske, K.R., Jeon, K.S., & Kahvecioglu, D.C. (2013). "Do Public Employment and Training Programs Work?" *IZA Journal of Labor Economics, 2*(1), 6.

Hyman, B. (2018). "Can Displaced Labor Be Retrained? Evidence from Quasi-Random Assignment to Trade Adjustment Assistance." Working Paper, University of Chicago, (November).

Katz, L. F., Roth, J., Hendra, R., Schaberg, K. (2022). "Why Do Sectoral Employment Programs Work? Lessons from WorkAdvance." *Journal of Labor Economics*, Vol 40 (S1), pp. S249-S291.

Krueger, A. (1993). "How Computers Have Changed the Wage Structure: Evidence from Microdata", 1984-1989. *Quarterly Journal of Economics,* Feb., Vol. 108 (1), pp. 33-60.

LaLonde, R. J., (1986). "Evaluating the Econometric Evaluations of Training Programs with Experimental Data," *American Economic Review* 76: 604–620.

Means B., Toyama Y., Murphy R., Bakia M. and Jones K. (2010). "Evaluation of Evidence-Based Practices in Online Learning: A Meta-Analysis and Review of Online Learning Studies" U.S. Department of Education, Office of Planning, Evaluation, and Policy Development, Policy and Program Studies Service, Center for Technology and Learning; Washington, D.C.

Mueser, P and Troske K., (2023). "Government-Supported Job Training in the U.S.: The Workforce Innovation and Opportunity Act Moving Forward," working paper.

Orr L. L., Bloom H. S., Bell S. H., Doolittle F., Lin W., Cave G. (1996). *Does Training for the Disadvantaged Work? Evidence from the National JTPA Study*. Urban Institute Press, Washington DC.

Schochet, Peter Z., Ronald D'Amico, Jillian Berk, Sarah Dolfin, and Nathan Wozny, (2012). *Estimated Impacts for Participants in the Trade Adjustment Assistance (TAA) Program Under the 2002 Amendments*, Final Report Prepared as Part of the Evaluation of the Trade Adjustment Assistance Program, Social Policy Research Associates and Mathematica Policy Research, Inc., (August).

U.S. Department of Labor, (2010). "Training and Employment Guidance Letter No. 07-10." Washington D.C. (August 19) https://www.dol.gov/agencies/eta/advisories/training-and-employment-guidance-letter-no-07-10.

Vakhitova, G. (2006). Market Issues of Microsoft Certification of IT Professionals. University of Kentucky, Doctoral Dissertation.