

Further Education During Unemployment^{*†}

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Abstract

Evidence on the effectiveness of retraining U.S. unemployed workers primarily comes from evaluations of training programs, which represent one narrow avenue for skill acquisition. We use high-quality records from Ohio and a matching method to estimate the effects of retraining, broadly defined as enrollment in postsecondary institutions. Our simple method bridges two strands of the dynamic treatment effect literature that estimate the treatment-now-versus-later and treatment-versus-no-treatment effects. We find that enrollees experience earnings gains of six percent three to four years after enrolling, after depressed earnings during the first two years. The earnings effects are driven by industry-switchers, particularly to healthcare.

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[†]The Ohio Longitudinal Data Archive is a project of the Ohio Education Research Center (oerc.osu.edu) and provides researchers with centralized access to administrative data. The OLDA is managed by The Ohio State University's Center for Human Resource Research (chrr.osu.edu) in collaboration with Ohio's state workforce and education agencies (olda.ohio.gov), with those agencies providing oversight and funding. For information on OLDA sponsors, see <https://chrr.osu.edu/projects/ohio-longitudinal-data-archive>.

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1 Introduction

The U.S. labor market has become increasingly polarized in recent decades, as high- and low-skilled jobs grow at the expense of middle-skilled jobs that traditionally employ workers with moderate levels of education (Autor, Katz and Kearney, 2006, 2008). These trends continued through the Great Recession, resulting in disproportionately high unemployment among workers without a college degree (Katz, 2010; Hoynes, Miller and Schaller, 2012). A long line of research shows that job displacement, especially during economic downturns, is associated with large and persistent earnings losses, adverse health outcomes, and negative impacts on the children of the unemployed (Jacobson, LaLonde and Sullivan, 1993; Couch and Placzek, 2010; Krolkowski, 2018; Sullivan and von Wachter, 2009; Davis and von Wachter, 2011; Oreopoulos, Page and Stevens, 2008; Stevens and Schaller, 2011). To mitigate these social and economic costs, economists and policymakers across the political spectrum have advocated for new skill acquisition through further education. At the peak of the Great Recession, for example, the U.S. Departments of Labor and Education created the website opportunity.gov and encouraged state governments to contact unemployment insurance (UI) claimants and inform them of resources (e.g., federal financial aid) and institutions (e.g., community colleges) for reskilling.

Much of our knowledge about the effects of further education for unemployed workers in the U.S. comes from evaluations of government-sponsored training programs (e.g., the Workforce Investment Act program, or WIA, now replaced by the Workforce Innovation and Opportunity Act, or WIOA), even though these programs only constitute one narrow avenue through which unemployed workers upgrade their skills. In reality, many more workers enroll directly in a local postsecondary institution such as a community college. For instance, in the fall of 2017, 4.5 million nontraditional (i.e., 25 years old or above) undergraduate students were enrolled nationwide, compared to 1.6 million participants in the largest U.S. training program in 2016-17 (WIOA Adult and Dislocated Worker programs), of which only a minority received training services (Snyder, de Brey and Dillow, 2019b; Social Policy Research Associates, 2018). While a large literature analyzes the effects of community college education (see Kane and Rouse, 1999 and Belfield and Bailey, 2011 for reviews), few studies focus on unemployed workers, a policy relevant group that differs from other community college attendees. Unemployed workers tend to be older and more experienced, but they have different opportunity costs, face different labor market barriers, and therefore may see different returns to further education. The only studies that directly examine unemployed workers enrolled in community col-

leges are Jacobson, Lalonde and Sullivan (2005*a,b*), which focus on long-tenured Washington state workers laid off in the early 1990s. This leaves a nearly twenty-year research void on this important topic.

Our study seeks to fill this void. We estimate the labor market effects of retraining among unemployed workers, where retraining is broadly defined as enrollment in a postsecondary institution.¹ We link together high quality administrative data from the state of Ohio, which include UI claims, quarterly wage records, course enrollment and credential data from all in-state public higher education institutions (including community colleges and technical centers), and WIA records. By following unemployed workers who filed a UI claim between 2004 and 2011, we observe that indeed the majority of retraining does not occur within the context of a narrowly defined training program: in our data, nearly 88,000 workers enroll in public postsecondary institutions following a layoff, compared with 27,000 workers who retrain through WIA.

We also tackle methodological issues along the way of our empirical inquiry. To estimate the effects of retraining, we use a matching method that compares the labor market outcomes of unemployed workers who pursue further education (enrollees) versus observably similar workers who do not (matched non-enrollees) within two years after layoff. Because workers enroll at different times, a standard matching estimand will only identify the effect of enrolling now versus potentially enrolling later (e.g., Sianesi, 2004), but not the effect of enrolling versus not enrolling that we are interested in. We show that, with a testable additional assumption regarding the selection into training that is consistent with Ashenfelter and Card (1985) and Heckman and Robb (1985*a,b*), we can identify a lower bound of the latter effect with a simple modification of the standard estimand. The estimated lower bound appears to be tight in our empirical context.

Our matching specification is informed by the large literature that uses selection-on-observables designs to evaluate training programs in both the U.S. and international contexts (see McCall, Smith and Wunsch, 2016 for a comprehensive review). Moreover, to support our specification, we have conducted our own validation analysis in the spirit of LaLonde (1986), using data from the National Job Training Partnership Act Study (NJS) (details can be found in our previous working paper Leung and Pei, 2020). This analysis, which builds on the influential work by Heckman, Ichimura and Todd (1997), Heckman et al. (1998), and Heckman and Smith (1999), evaluates the ability of various models and specifications (including those based on machine-learning) to recover a causal effect. We find that when we have a sample of workers recently attached to the labor market and incorporate detailed earnings histories linearly into the covariate

¹We use the word “retrain” in accordance with its meaning by the Cambridge Dictionary: to learn new skills so you can do a different job, or to teach someone a new skill so that they can do a different job. Workers can “retrain” irrespective of their educational background.

set, conventional (logit-based) propensity score matching performs well, indicating the plausibility of the underlying conditional independence assumption.

We graphically present the average earnings trajectories of enrollees and matched non-enrollees in the five years before and four years after enrollment. The trajectories reveal little difference in earnings pre-enrollment, followed by temporarily depressed earnings of enrollees while they are in school (the “lock-in” effect), and sustained positive effects thereafter. Overall, we estimate that the lower bound earnings effect among enrollees is \$348 per quarter, or about six percent, in the third and fourth years after enrolling. A decomposition of this earnings gain reveals that retraining affects earnings mostly at the extensive margin. While the magnitudes of enrollment effects are heterogeneous across various subgroups, we consistently observe positive earnings gains four years after enrolling. Following an early subset of workers for a longer period, we find that the retraining effect persists and widens to 13 percent at the end of a ten-year horizon.

Another advantage of our study relative to existing training program evaluations is our ability to look into the “black box” of retraining. That is, we observe the courses taken and credentials received by enrollees in our sample, which allows us to explore the types of training underlying our estimates. A simple accounting suggests that the enrollment effects are driven by workers who train and subsequently find employment in new industries post-layoff, particularly the healthcare sector.

This paper makes the following contributions. First, it bridges two largely separate strands of empirical literature on training programs and on community colleges by studying the policy-relevant unemployed worker population that intersects with both. As mentioned above, the U.S. training literature focuses mainly on evaluating government sponsored programs, which finds mixed results.² In the community college literature, recent studies that use administrative earnings and transcript data show that associate degrees yield earnings gains of about 18 to 26 percent relative to no degree, mixed effects of other credentials, and positive effects of healthcare-related programs (see review by Belfield and Bailey, 2017a). As noted above, Jacobson, Lalonde and Sullivan (2005a,b) are the only studies that look at the community college effects on unemployed workers. Their preferred regression model suggests that enrollment increases earnings between six and eight percent (their main earnings effect finding of nine to thirteen percent is for one year of full-time

²For WIA, recent experimental and non-experimental evaluations find zero to long-lasting negative effects of training for dislocated workers (Heinrich, Mueser and Troske, 2008; Andersson et al., 2013; McConnell et al., 2016; Fortson et al., 2017). For the Trade Adjustment Assistance (TAA) program, which provides training to workers affected by trade, one non-experimental evaluation finds initially large negative effects that fade to zero over a four-year period, while another study utilizing quasi-random variation on TAA petition approvals finds positive effects, though the two studies present estimates of different quantities that are not directly comparable (Schochet et al., 2012; Hyman, 2022).

enrollment, and we scale it based on the average course load in their data), but their estimates are sensitive to the model used.³

Second, we contribute methodologically to the dynamic treatment effect literature by connecting the studies that estimate the treatment-now-versus-later and treatment-versus-no-treatment effects of training. In particular, by modifying the treatment-now-versus-later estimand per Sianesi (2004), we can use "static" propensity score matching to bound the treatment-versus-no-treatment effects on the treated that are typically identified with dynamic estimands as in Lechner (2009) and Lechner and Miquel (2010). Our simple estimator can be implemented with off-the-shelf software commands and avoids the inferential challenges their dynamic counterparts encounter.

Finally, this paper sheds light on the effects of retraining for a recent period, which includes the Great Recession and covers a wide range of economic conditions, and in a Rust Belt state characterized by movement away from declining manufacturing industries. Recency of data is important: labor market trends such as the rise in automation and trade in past decades (Autor, Dorn and Hanson, 2013; Autor et al., 2014; Acemoglu and Restrepo, 2020) may have impacted training effects particularly in former manufacturing centers, which makes our estimates more informative for current policy-making relative to those by Jacobson, Lalonde and Sullivan (2005*a,b*) from Washington state in the early 1990s. The economic boom and bust in our sample period allow us to speak to the literature examining the dependence of educational returns on labor market conditions. Consistent with previous studies (Lechner and Wunsch, 2009; Kahn, 2010; Oreopoulos, von Wachter and Heisz, 2012), we find retraining leads to larger average earnings gains for those enrolled during the Great Recession, who sought jobs afterwards in a thawing labor market.

2 Institutional Background

2.1 Unemployment Insurance

In this paper, we identify unemployed workers as those who claim unemployment insurance. To be eligible for UI, workers must have lost a job through no fault of their own and have sufficient earnings and work weeks prior to job loss. In Ohio, workers must have worked at least 20 weeks and have an average weekly

³Most of the community college research using similar administrative earnings data rely on a fixed effects model. In our setting, we find differential pre-trends between enrollees and non-enrollees that would render estimates from fixed effects models biased, similar to Jacobson, Lalonde and Sullivan (2005*a*). We also find that fixed effects specifications yield biased estimates in our validation exercise (see Leung and Pei, 2020).

wage of about \$200 per week in the one-year period that begins five calendar quarters prior to job loss. As a result, our study population consists of UI claimants previously attached to the labor force.

While workers generally need to actively search for jobs and be available to work in order to continue receiving benefit payments, they can pursue “approved training” opportunities without losing UI eligibility. States vary in their definitions of approved training, though it generally includes vocationally-oriented or basic education training. According to NASWA (2010), the Ohio unemployment agency automatically approves all training through workforce programs and has 7,000 courses listed as approved. It also approves academic courses that do not lead to a specific occupation on a case-by-case basis.

2.2 Postsecondary Institutions

Our study focuses on the impact of classroom training, which can take place in different settings. First, workers may choose to attend community colleges and enroll in courses that may lead to an associate degree or sub-associate credentials such as certificates. Alternatively, workers may enroll at technical centers. Technical centers typically offer occupation-specific programs that may lead to a state license or other credentials. Examples include state license for practical nursing or professional certification in welding.

The cost of attendance varies by institution and program. According to the Integrated Postsecondary Education Data System (IPEDS), the average tuition and fees across Ohio institutions were approximately \$6,400 per year in 2010. However, since unemployed workers are often financially constrained, they are likely to be eligible for and rely on several forms of financial assistance. First, workers may be eligible for federal financial aid such as Pell grants, subsidized loans, and tuition tax credits.⁴ Second, they may obtain training funding from workforce programs like WIA or TAA. In WIA, eligible participants may receive an Individual Training Account (ITA) voucher that can be used towards approved training (fewer than 20 percent of WIA participants receive training services, while the others only receive “core” or “intensive” services such as assistance in job search, placement, employment and career planning). The TAA program provides tuition assistance to workers affected by import competition. To understand the relative sizes of the various sources of financial assistance, Barnow and Smith (2016) report that in 2014, Pell Grants for those who pursued vocational education totaled \$8.2 billion. In contrast, the expenditures for the WIA Dislocated Worker program and TAA were only \$1.2 and \$0.3 billion, respectively.

⁴Although the amount of federal aid typically depends on income from about two years prior, UI claimants may qualify for simplified needs tests or automatic zero expected family contribution starting in 2009.

3 Data, Analysis Sample, and Descriptive Statistics

Our analysis primarily draws on several administrative data sources from Ohio: 1) UI claim records, 2) student records from public postsecondary institutions, including community colleges and technical centers, 3) quarterly wage records, and 4) WIA participant records.

For our analysis of labor market effects, we study workers who file an eligible UI claim between 2004 and the third quarter of 2011. To focus on workers who seek further education after unemployment, we exclude those who enroll at any point within two years prior to layoff.⁵ Our analysis sample contains 1.9 million claims, coming from 1.3 million unique individuals (see Appendix A for details on sample construction and data elements). The UI records contain the claim date, demographics (gender, race, number of dependents, age, and zip code), and prior job information (industry and occupation).⁶

Our schooling data cover all public postsecondary institutions in Ohio. The Higher Education Information system (HEI) records contain enrollment information for 37 public two- and four-year institutions, though we focus on those who first enroll in a two-year institution in this analysis. In the HEI data, we observe terms enrolled, courses taken, and degrees or other credentials obtained (i.e., graduate or professional, bachelors, associate, or less than two-year awards). We also have student records from 53 publicly funded technical centers through the Ohio Technical Centers (OTC) database. In the OTC data, the courses offered range from one-day courses to certificate programs that last several years. We observe the dates of enrollment in courses as well as any credentials obtained. Despite the expansive coverage of the HEI and OTC data, we do not know whether a worker enrolls in a private institution. While we acknowledge this to be a limitation of our paper as our “non-enrollees” may in fact enroll in a program we do not observe, many studies in the training literature also suffer from similar issues. For example, a survey of trainees in the WIA Gold Standard Evaluation reveals that 28 percent of the training was not funded by WIA (Fortson et al., 2017).⁷ In comparison, the share of observed non-enrollees in our data enrolling in a private institution is likely to be smaller. According to IPEDS, among students 25 or older at two-year or lower institutions in the fall of 2007, less than 10 percent were enrolled in private institutions, of which 94 percent were in for-

⁵This restriction eliminates roughly 147,000 claims. The excluded claimants are younger with lower tenure and lower pre-layoff earnings.

⁶The number of dependents is recorded because the maximum UI benefit amount is higher when a claimant has more dependents. However, only workers with high enough prior earnings receive the maximum UI amount. Since many workers’ UI benefits do not change with dependents, this measure likely understates the true number of dependents.

⁷Specifically, Fortson et al. (2017) find that while 43 percent of workers assigned to the “full WIA” experiment arm self-reported to have trained, only 31 percent in the experiment arm received WIA funding. This implies that $(43 - 31)/43 = 28$ percent of the trainees sought training outside WIA.

profit institutions. The unobserved private school enrollment is likely to lead us to underestimate the effect of training if there is private enrollment in our matched comparison sample—Cellini and Turner (2019) show that attending for-profit institutions leads to a positive, but statistically insignificant, earnings gain.

We construct our main outcome variables using quarterly earnings data from the state’s UI system (we discuss how out-of-state earnings may impact our estimates in Appendix A). In addition to earnings, we also observe, through the third quarter of 2017, number of weeks worked from 2003 and industry for each private sector employer from 1995 (unlike the UI claim records, the quarterly wage data contain no information on occupation). The long earnings history allows us to observe at least three years of pre-layoff earnings. We also use this data to construct measures of pre-layoff job tenure and outcomes like industry switching.

Finally, we observe whether a worker in our analysis sample is in the WIA Standardized Record Data, which cover participants of WIA Adult, Youth, and Dislocated Worker programs. We use information on the dates of WIA training to identify the subset of enrollees observed in the HEI and OTC data who received WIA training services. Next we state our definition of an enrollee and provide descriptive statistics on enrollee demographics, the timing of enrollment, and enrollment characteristics.

Definition of an enrollee We define an enrollee as a worker who enrolls in a community college or a technical center within two years of layoff.⁸ The two-year window is motivated by the fact that UI benefits were available for up to 99 weeks during the Great Recession, which our analysis period covers. We choose to have a consistent definition of treatment by using the same two-year window throughout the entire sample, even though UI is only available for 26 weeks under normal economic conditions. One may be concerned as to whether workers receiving 26 weeks of UI are still unemployed two years after layoff, but as we show in Section 5, enrollees on average have continuously depressed earnings between layoff and enrollment regardless how soon they enroll.

Who enrolls? Panel A of Table 1 presents descriptive statistics for our UI claimant sample by enrollment status. Of the 1.9 million claims in our data, 71,745, or 4 percent, are followed by enrollment in a public postsecondary (two-year or less) institution. While women make up only 34 percent of UI claimants, they are better represented among enrollees, at 44 percent. Compared with 13 percent among non-enrollees, African Americans make up 18 percent of the enrollee population. In terms of prior job characteristics,

⁸Since we observe enrollment term rather than date of enrollment in the HEI data, we approximate enrollment terms winter, spring, summer, and fall to the first, second, third, and fourth calendar quarters, respectively. Since enrollment typically occurs in the fall and spring terms, and those terms are likely to begin earlier than the corresponding fourth and second calendar quarters, we are likely to report an enrollment start date that is on average later than when workers actually begin schooling. In 2013, all Ohio public colleges switched to the semester system, which eliminates the corresponding winter quarter.

enrollees are less likely to have worked in manufacturing and transportation, have lower job tenure, are younger, and have lower prior earnings (all earnings are expressed in 2012 dollars).

When do workers enroll, and what are their enrollment characteristics? Table 2 shows that workers take time to enroll—on average 3.7 quarters after layoff—and the mean enrollment length is 4.5 terms. Enrollees in our UI claimant sample mostly attend community colleges (87 percent). 90 percent of enrolled workers take at least one occupational course, and the average proportion of occupational courses is 60 percent, where courses are classified as occupational based on their Classification of Instructional Program (CIP) code following the taxonomy by the National Center of Education Statistics. The overall credential receipt rate within four years of enrollment (including sub-baccalaureate awards, licenses, and industry credentials) is 26 percent. The majority of degrees or credentials obtained are associate degrees or lower: sub-associate credentials account for 58 percent of total credential receipts, associate degrees for 40 percent, and bachelor’s and graduate degrees make up the remaining 2 percent (while we focus on workers who begin enrollment in a community college or technical center, some eventually go on to four-year institutions).

4 Identification, Estimation, and Empirical Implementation

The empirical challenge in estimating labor market effects of enrollment stems from the differences in the characteristics of workers who do and do not enroll that relate to their future earnings potential. Following a long line of research in training program evaluation as reviewed by McCall, Smith and Wunsch (2016), we adopt a selection-on-observables research design to measure the causal effects of retraining. Because enrollment timing relative to layoff varies across workers, we rely on a dynamic framework for partial identification, with our main proposition providing a treatment effect lower bound. We discuss the underlying assumptions and define the treatment effect parameters of interest in Section 4.1 below.

Before we proceed, we highlight several rationales for why matching may “work” in our context, given the (often justified) skepticism towards it. First, our sample construction helps to mitigate the “Ashenfelter’s dip” problem, a major challenge in training program evaluation. The Ashenfelter’s dip refers to the phenomenon in most evaluations of U.S. training programs wherein trainees experience (on average) an earnings dip prior to training, while non-trainees do not. This is likely because the decision to retrain is often a reaction to transitory shocks (Ashenfelter, 1978). A prime example of such shocks is the loss of employment. By starting from the set of recently unemployed workers in Ohio, we shut down job loss as a

channel that triggers training, as both enrollees and non-enrollees in our sample have experienced it.

Second, in our matching specification we take advantage of the information available on past labor market histories from our rich administrative data, which makes the selection-on-observables assumption plausible as recent studies have argued. As Andersson et al. (2013) succinctly state, “[m]otivated workers, and high ability workers, should do persistently well in the labor market; if so, conditioning on earlier labor market outcomes will remove any selection bias that results from motivation and ability also helping to determine training receipt.” Andersson et al. (2013) also find that adding firm fixed effects does not change causal estimates relative to specifications which incorporate detailed labor market histories. Similarly, in extensive empirical Monte Carlo simulations based on German administrative data, Lechner and Wunsch (2013) find that including variables on firm characteristics, industry- and occupation- specific experience, health, program compliance, desired job characteristics, and detailed regional information does not further reduce bias relative to only using basic demographics and labor market histories. Finally, Caliendo, Mahlstedt and Mitnik (2017) find that controlling for typically unobserved non-cognitive traits adds little beyond past labor market histories.

Third, our matching specification is guided by a validation exercise in the spirit of LaLonde (1986), Heckman, Ichimura and Todd (1997), and Heckman et al. (1998). As described in detail in a previous version of this paper (Leung and Pei, 2020), we use data from the NJS to assess whether more flexible machine-learning-based specifications offer better performance than a conventional (logit) propensity score model where the terms enter linearly. We find that conventional propensity score matching methods perform competitively even without the use of machine learning algorithms, and is able to recover a causal effect in subsamples that more closely resemble our Ohio study population—namely workers with previous labor market attachment, for whom past earnings are likely to be predictive of future prospects.

Fourth, the specification informed by our validation study leads to high quality of matching. In Section 4.4, we show overlapping support in the propensity score distributions (except for outliers that amount to one percent of the enrollee sample) and covariate balance across enrollees and matched non-enrollees.

Finally, we believe matching to be better suited than other empirical methods for our setting. In Appendix C, we explore the possibility of adopting alternative research designs—which may identify different causal parameters than matching—including fixed effects models, the use of a distance instrument, and a regression discontinuity design based on layoff timing. We discuss why we cannot use them for our analysis.

Despite these reasons in favor of using matching for our analysis, doubts may still linger over the validity

of the selection-on-observables assumption. Chief among them is the possibility that even within a matched pair with the same labor market history, the non-enrollee chooses not to enroll because she expects to be recalled to her previous employer or has a job offer in hand to start in the future (Sianesi, 2004; Fredriksson and Johansson, 2008). As we show next, our main identification result provides a lower bound on a dynamic treatment effect parameter, and the presence of such “forward-looking” workers may further bias our estimate downward against us finding an enrollment effect.

4.1 Parameters of Interest, Identification and Estimation

The main assumption underlying our method is the conditional independence assumption (CIA), or “unconfoundedness”. For expositional purposes, we begin with the simple *static* case where enrollment decision takes place at one point in time for all laid-off workers. Let D denote whether a worker enrolls in school, with $D = 1$ if the worker enrolls and $D = 0$ if she does not. Let $Y(\cdot)$ denote the potential post-enrollment earnings of the worker: $Y(1)$ is the worker’s potential future earnings if she enrolls, and $Y(0)$ is her potential future earnings if she does not enroll. The observed outcome is $Y = Y(1)D + Y(0)(1 - D)$.

The static unconfoundedness assumption standard in the matching literature is

$$Y(0) \perp\!\!\!\perp D | \mathbf{X} \tag{1}$$

where \mathbf{X} is a vector of observed covariates realized at or prior to enrollment. Together with a common support condition—the propensity score $p(\mathbf{X}) \equiv \Pr(D = 1 | \mathbf{X})$ is less than one (almost) everywhere on the support of \mathbf{X} —CIA implies the identification of the treatment effect on the treated (TOT) parameter via propensity score matching:

$$E[Y|D = 1] - E[E[Y|D = 0, p(\mathbf{X})]|D = 1] = \underbrace{E[Y(1) - Y(0)|D = 1]}_{\text{TOT}}. \tag{2}$$

In our study, the “treatment” (enrollment) is allowed to occur over a period of two years, a complication that necessitates a *dynamic* variant of the framework above. An important consideration in the dynamic setting is the treatment effect parameter of interest. Several earlier studies (Sianesi, 2004; Fredriksson and Johansson, 2008; Biewen et al., 2014) rely on the dynamic counterpart of the conditional independence assumption and use an estimand similar to that in equation (2) to identify the treatment effect of enrolling in one period versus not enrolling in that period but possibly later in the two-year window (the “treatment-now-versus-later” effect). This is different from the dynamic version of the TOT parameter we are interested

in, which is the effect of enrolling versus not enrolling during the two-year window (the “treatment-versus-no-treatment” effect). As McCall, Smith and Wunsch (2016) point out, the “treatment-versus-no-treatment” parameter may be more useful than the “treatment-now-versus-later” parameter, as the former can be more easily incorporated into a cost-benefit analysis.⁹

In this paper, we modify the method used by studies that estimate the “treatment-now-versus-later” effect. This modified method preserves the simplicity of *static* propensity score matching and can partially identify the *dynamic* TOT parameter under an additional testable assumption. In the remainder of this subsection, we state our assumptions and identification results. We discuss the connection between our method and alternatives in the dynamic treatment effect literature in the next subsection.

For ease of exposition, we focus on a two-period case here and leave the more general multi-period case to Appendix B (we have an enrollment window of eight quarters in our empirical setting). In the two-period case, D equals one if a worker starts training either in the first or second period post-layoff. We use the binary variable D_1 to denote whether a worker begins enrollment in period 1, and D_2 to denote whether she begins enrollment in period 2. It follows that $D = D_1 + D_2$ with two implications. First, $D = 0$ implies that $D_1 = 0$ and $D_2 = 0$ (a worker who never enrolls does not start training in either period). Second, at most one of D_1 and D_2 can equal one, meaning that $D_1 = 1$ implies $D_2 = 0$ (a worker who already enrolls in period 1 is no longer “at risk” for starting enrollment in period 2), and vice versa (a worker who starts enrollment in period 2 cannot have enrolled in period 1).

We use \mathbf{X}^1 and \mathbf{X}^2 to denote the vectors of conditioning covariates available at the beginning of the first and second period and before the realization of D_1 and D_2 , respectively. The superscript notation, consistent with Abbring and Heckman (2007), indicates that the covariates incorporate cumulative information up to the beginning of a time period before the training decision for that period. More concretely, we think of \mathbf{X}^1 as incorporating the relevant covariates available at the beginning of period 1 and before the realization of D_1 ; \mathbf{X}^2 includes all the variables in \mathbf{X}^1 , and also additional variables realized after D_1 but prior to D_2 .¹⁰ Denoting earnings of period t by Y_t , which realizes at the end of the period, the additional variables in \mathbf{X}^2 but not \mathbf{X}^1 typically include Y_1 . In summary, the sequence of variable realizations is:

$$\mathbf{X}^1 \text{ (realized at layoff)} \rightarrow D_1 \rightarrow Y_1 \rightarrow \mathbf{X}^2 \rightarrow D_2 \rightarrow Y_2 \rightarrow Y_3 \rightarrow Y_4 \rightarrow \dots$$

⁹The “now-versus-later” effect may be the more relevant parameter such as in the analysis of the earnings impact of job displacement (Krolkowski, 2018).

¹⁰The conditioning set notation in Lechner (2009) and Lechner and Miquel (2010) is similar: our \mathbf{X}^1 and \mathbf{X}^2 correspond to their X_0 and \underline{X}_1 .

Note that because training can only take place during period 1 or 2 in the two-period case, it is not necessary to include the conditioning set \mathbf{X}^t and treatment D_t when $t > 2$ in the sequence above.

Finally, we use $Y_t(1)$ and $Y_t(0)$ to denote the potential earnings at the end of period t . Our main identification result uses the following observation rule: for a worker who begins enrollment in period s ($s = 1, 2$), the observed post-treatment ($t \geq s$) outcome is $Y_t = Y_t(1)$; for a worker who enrolls in neither period, $Y_t = Y_t(0)$ for all t . Missing from the observation rule is the relation between observed and potential *pre*-treatment outcomes among the treated population. While it is tempting to write $Y_t = Y_t(0)$ for a period- s enrollee when $t < s$, doing so requires an additional assumption of no anticipation. No-anticipation is not needed for our main identification result, but we do invoke it to derive testable implications of our assumptions—more details below and in Appendix B.1.

Here are the assumptions for our main identification result. The first is a dynamic CIA assumption

Assumption 1. *a) $Y_t(0) \perp\!\!\!\perp D_1 | \mathbf{X}^1$ for $t \geq 1$ and b) $Y_t(0) \perp\!\!\!\perp D_2 | \mathbf{X}^2, D_1 = 0$ for $t \geq 2$.*

Assumption 1 and variations thereof are standard in the literature—they are referred to as “sequential randomization” (e.g., Robins, 1997) or “weak dynamic conditional independence” (e.g., Lechner, 2009). In our context, it says that within the “at-risk” set of workers who have not yet enrolled and are therefore able to begin enrollment at the start of period s , $s = 1, 2$, the potential future outcome $Y_t(0)$ is independent of a worker’s enrollment decision conditional on the information available. Following an argument analogous to the static case, Assumption 1 allows for the identification of the training effect for period-2 enrollees when conditioning on \mathbf{X}^2 and $D_1 = 0$ (i.e., the effect of D_2). By simply conditioning on \mathbf{X}^1 , it also allows for the identification of the effect of enrolling in period 1 versus not enrolling in period 1 but possibly in period 2 (i.e., the effect of D_1) with the estimand:

$$E[Y_t | D_1 = 1] - E[E[Y_t | D_1 = 0, \mathbf{X}^1] | D_1 = 1] \quad (3)$$

for $t \geq 1$. However, it is more complex to identify the TOT parameter of training versus no-training (i.e., the effect of D) for period-1 enrollees. We invoke an additional assumption to bound it from below:

Assumption 2. $E[Y_t(0) | D_1 = 0, D_2 = 1, \mathbf{X}^1] \leq E[Y_t(0) | D_1 = 0, D_2 = 0, \mathbf{X}^1]$ for $t \geq 1$.

Assumption 2 says that among workers who have the same observable characteristics at the beginning of period 1, those who begin enrollment in period 2 have lower average potential future earnings absent training than their never-enrolled counterparts. It reflects the idea that workers who select into training tend to have

lower opportunity costs in doing so. As we discuss in Appendix B.1, under additional conditions that are not substantively more restrictive, this assumption is consistent with models of selection into training by Ashenfelter and Card (1985) and Heckman and Robb (1985*a,b*) (henceforth, AC and HR, respectively). The AC and HR frameworks also lead to testable implications: To the extent that the earnings process has positive serial dependence, the observed period-1 earnings Y_1 among period-1 non-enrollees can proxy for their future potential outcome $Y_t(0)$ (as mentioned above, this test requires an additional no-anticipation assumption, but we point out in Appendix B.1 that AC and HR's specifications of the earnings process implicitly maintains this assumption). Thus, we can test Assumption 2 by comparing the Y_1 of period-2 enrollees and never-enrolled workers with similar \mathbf{X}^1 . The test can be implemented via propensity score matching, and we present evidence in support of Assumption 2 in Section 5.1.

Assumption 2 allows us to bound the TOT effect for period-1 enrollees from below with a simple modification of the treatment-now-versus-later estimand in (3):

$$E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, \mathbf{X}^1]|D_1 = 1]. \quad (4)$$

The only difference between (3) and (4) is the comparison group: the comparison group in (3) consists of workers who did not enroll in period 1 ($D_1 = 0$), while the comparison group in (4) consists of workers who did not enroll in either period ($D = 0$). Intuitively, it is impossible to identify the TOT with the treatment-now-versus-later estimand (3) because we do not observe $Y_t(0)$ of the later-enrollees (i.e., workers in its comparison group who enroll in period 2). By going from (3) to (4), we replace the later-enrollees by non-enrollees with similar \mathbf{X}^1 , for whom $Y_t(0)$ is the observed Y_t . Assumption 2 says that these non-enrollees have a higher average $Y_t(0)$, allowing (4) to provide a lower bound for the TOT parameter.

Directly implementing estimand (4) is subject to the curse of dimensionality when \mathbf{X}^1 contains many covariates. The use of a propensity score is the usual remedy for overcoming this challenge. However, we are not willing to make a strong CIA assumption for the estimand (i.e., $Y_t(0) \perp\!\!\!\perp D_2|D_1 = 0, \mathbf{X}^1$), and, therefore, we do not have a standard propensity score theorem at our proposal. Fortunately, as we show in Appendix B.2, propensity score matching preserves inequality.¹¹

Formally, define the propensity scores $p_1(\mathbf{X}^1) \equiv \Pr(D_1 = 1|D_2 = 0, \mathbf{X}^1)$ and $p_2(\mathbf{X}^2) \equiv \Pr(D_2 = 1|D_1 = 0, \mathbf{X}^2)$, and our main identification result is

¹¹By similar reasoning, propensity score matching also preserves inequality in the simple static setting. Specifically, if selection into treatment is negative conditioning on covariates: $E[Y_t(0)|D = 0, \mathbf{X}] \geq E[Y_t(0)|D = 1, \mathbf{X}]$, then selection into treatment is also negative conditioning on the propensity score: $E[Y_t(0)|D = 0, p(\mathbf{X})] \geq E[Y_t(0)|D = 1, p(\mathbf{X})]$. This is a useful result, as it can help sign the population bias when propensity score matching.

Proposition 1. Under Assumptions 1 and 2 and provided that $p_1(\mathbf{X}^1), p_2(\mathbf{X}^2) < 1$,

(a):

$$E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, p_1(\mathbf{X}^1)]|D_1 = 1] \leq \underbrace{E[Y_t(1) - Y_t(0)|D_1 = 1]}_{TOT \text{ for } D_1=1: \delta_{1t}} \text{ for } t \geq 1 \quad (5)$$

$$E[Y_t|D_2 = 1] - E[E[Y_t|D = 0, p_2(\mathbf{X}^2)]|D_2 = 1] = \underbrace{E[Y_t(1) - Y_t(0)|D_2 = 1]}_{TOT \text{ for } D_2=1: \delta_{2t}} \text{ for } t \geq 2; \quad (6)$$

(b):

$$\sum_{s=1}^2 \{E[Y_t|D_s = 1] - E[E[Y_t|D = 0, p_s(\mathbf{X}^s)]|D_s = 1]\} \Pr(D_s = 1|D = 1) \leq \underbrace{E[Y_t(1) - Y_t(0)|D = 1]}_{Overall TOT: \delta_t} \text{ for } t \geq 2.$$

All proofs are in Appendix B. Part (a) of Proposition 1 consists of identification results for δ_{1t} and δ_{2t} , the TOTs at time t among period-1 and period-2 enrollees, respectively. Part (b) aggregates across the two enrollee populations and provides a lower bound for δ_t , the overall TOT parameter at time t . For expositional ease, t , our time index here, refers to time relative to layoff. But we can also define δ with an alternative time index, such as time relative to the beginning of enrollment. Concretely, to obtain the overall TOT τ quarters after enrollment, we take an average of $\delta_{1\tau}$ and $\delta_{2(\tau+1)}$ weighted by the shares of period-1 and period-2 enrollees. Since enrollment is our treatment variable of interest, we use this alternative time indexing in most of our empirical analyses, which is consistent with the convention in event studies.

The identification results in Proposition 1(a) lead to standard propensity score matching estimators for the lower bound of δ_{1t} and for the value of δ_{2t} . We can compute their corresponding asymptotic variances by following Abadie and Imbens (2016). To estimate the lower bound of the overall TOT δ_t , we simply take an average of the two estimators weighted by the shares of period-1 and period-2 enrollees and compute its asymptotic variance accordingly (details in Appendix B.2).

A natural question that arises is how conservative the lower bound from Proposition 1(b) is. It is easy to see that the tightness of our bound depends crucially on the share of later-enrollees, i.e., $\Pr(D_2 = 1|D_1 = 0, p_1(\mathbf{X}^1))$. When this share is zero, the lower bound estimand point identifies δ_{1t} . Intuitively, if no one enrolls in period 2, the treatment-now-versus-later effect for $D_1 = 1$ becomes the treatment effect of enrolling in period 1 versus not enrolling in either period. The bound is likely to stay informative when this share of later-enrollees is small, which is indeed the case in our empirical context.

More concretely, we can empirically assess the tightness of the bound in two ways. First, we can also construct an upper bound of δ_{1t} (and therefore δ_t) via propensity score matching. The construction uses the

non-negativity of earnings, allowing us to bound the $Y_t(0)$ of later-enrollees by zero from below. We can easily estimate this upper bound and apply standard inference procedures (details in Appendix B.2). The second way is to recognize that δ_{1t} is actually point identified under Assumption 1, per results by Lechner (2009) and Lechner and Miquel (2010). While the associated estimator from Lechner's identification result is more complex to implement and comes with inferential challenges, we can generate an alternative point estimate and compare to our estimated lower bound. In the next section, we discuss the connection of our paper to Lechner's and the broader dynamic treatment effect literature.

4.2 Relation to the Dynamic Treatment Effect Literature

This is certainly not the first paper to consider identification of treatment effects in a dynamic context. As mentioned above, previous studies (e.g., Sianesi, 2004) have estimated treatment-now-versus-later effects. But many other studies aim to estimate alternative causal parameters. We review relevant research in this latter category below with a particular focus on Lechner's work, providing more context for our method.¹²

In a series of influential studies (e.g., Robins, 1986, 1997; Gill and Robins, 2001), James Robins extends the static potential outcomes framework to consider identification of dynamic treatment effects. Specifically, Robins studies identification of potential outcomes under alternative treatment sequences. These treatment sequences are related to but distinct from our treatment variable defined in Section 4.1: Whereas our treatment variable is whether a worker begins training, each element in Robins's sequence corresponds to whether a worker receives training in a given period. Under a sequential-randomization assumption and a no-anticipation assumption, the distributions of potential outcomes under alternative treatment sequences are identified in the Robins framework. In Appendix B.4.1, we formally discuss Robins's assumptions and results by adapting the excellent summary in Abbring and Heckman (2007) to our two-period setting.

Two important studies by Lechner (2009) and Lechner and Miquel (2010) extend the work by Robins. They focus on the identification of average effects such as the TOTs and propose estimators based on sequential propensity score matching (Lechner and Miquel, 2010) and sequential inverse probability weighting (Lechner, 2009). The estimators offer an advantage over those by Robins as they require no functional form assumptions for potential outcomes.

¹²A recent study by van den Berg and Vikström (2022) analyzes dynamic treatment effects on earnings in a setting where training eligibility hinges on a worker remaining unemployed. The closest connection of van den Berg and Vikström (2022) to our paper is their treatment effect parameter: they are also interested in the TOT effect of training versus not training. Since workers in our setting are not subject to the eligibility criterion they consider, our methodology is more closely related to the work by Lechner.

A remarkable implication of the elegant results by Lechner (2009) and Lechner and Miquel (2010) is that δ_{1t} , the TOT for period-1 enrollees from Section 4.1, is point identified under dynamic CIA (Assumption 1). Consequently, the aggregate TOT parameter δ_t is also identified under dynamic CIA. Using our notation, the estimand that identifies δ_{1t} takes the form:¹³

$$E[Y_t|D_1 = 1] - E\left[E[E[Y_t|p_2(\mathbf{X}^2), \tilde{p}_1(\mathbf{X}^1), D = 0]|\tilde{p}_1(\mathbf{X}^1), D_1 = 0]|D_1 = 1\right]. \quad (7)$$

In (7), the propensity score for $D_1 = 1$ is defined as $\tilde{p}_1(\mathbf{X}^1) \equiv \Pr(D_1 = 1|\mathbf{X}^1)$, which differs from $p_1(\mathbf{X}^1)$ by not conditioning on $D_2 = 0$, i.e., period-2 enrollees are in the at-risk set when computing $\tilde{p}_1(\mathbf{X}^1)$.

We now provide an intuitive account of Lechner's identification result and how it relates to ours. For ease of understanding, we focus on identification results with estimands that directly match on covariates—that is, replace the propensity scores $p_1(\mathbf{X}^1)$, $\tilde{p}_1(\mathbf{X}^1)$, and $p_2(\mathbf{X}^2)$ in Proposition 1 and (7) by the covariates \mathbf{X}^1 , \mathbf{X}^1 , and \mathbf{X}^2 , respectively. Constructing the Lechner estimand entails two steps in a two-period setting. The first step is equivalent to the treatment-now-versus-later matching of Sianesi (2004), for which the matched control set consists of period-1 non-enrollees ($D_1 = 0$) with similar \mathbf{X}^1 . This matched control set can be broken down into two groups of workers: i. those who enroll later ($D_2 = 1$) and ii. the “never-enrollees” ($D_2 = 0$). The second step updates the matched control set: it replaces group i by their never-enrolled counterparts with similar \mathbf{X}^2 (since \mathbf{X}^2 contains all the covariates in \mathbf{X}^1 , matching on \mathbf{X}^2 is equivalent to matching on both \mathbf{X}^1 and \mathbf{X}^2).

To make sense of these two steps, first note that Assumption 1(a) ensures that the average $Y_t(0)$ in the matched control set in step one is the average $Y_t(0)$ for the $D_1 = 1$ population. The problem is that $Y_t(0)$ is not observed for group i, so we need to replace it with observable quantities. This is what step two accomplishes. Under Assumption 1(b), the average Y_t among group i's replacements is equal to the average $Y_t(0)$ of group i, which implies that the average Y_t of the updated matched control set at the end of step two identifies $E[Y_t(0)|D_1 = 1]$.

To compare our method with Lechner's, it is useful to recast our estimand construction also as a two-step process. The first step is identical to Lechner's, resulting in a matched control set consisting of groups i and ii. In the second step, we also replace group i by their never-enrolled counterparts. But unlike Lechner, these replacements share similar \mathbf{X}^1 with group i, rather than \mathbf{X}^2 . Under our Assumption 2, the observed mean Y_t

¹³Like Robins, Lechner (2009) and Lechner and Miquel (2010) define potential outcomes for various treatment sequences. Using their potential outcome notation (reviewed in Appendix B.4.1), the second term in the estimand (7) identifies the mean potential outcome $E[Y_t^{00}|D_1 = 1]$ in the counterfactual scenario where period-1 enrollees do not enroll in either period.

among these replacements is a lower bound for the mean $Y_t(0)$ of group i. Because we only use covariates \mathbf{X}^1 , we can merge the two steps and directly look for matches with similar \mathbf{X}^1 among the never-enrollees.

The point identification of the Lechner estimand is quite appealing, but its implementation is complex especially with many periods. Specifically, an S -period setting requires S matching steps and involves the estimation of S propensity scores. For the two-period case, implementing the estimand (7) involves matching twice: first matching on the estimated propensity score $\tilde{p}_1(\mathbf{X}^1)$, and then matching on the estimated propensity score vector $(p_2(\mathbf{X}^2), \tilde{p}_1(\mathbf{X}^1))$. We have eight periods for our Ohio analysis, as we allow for an enrollment window of eight quarters post-layoff. As such, we need to perform matching eight times, and each time, the corresponding propensity score vector increasing in length by one. Lechner and Miquel (2010) sensibly use Mahalanobis matching when matching on multiple propensity scores in their four-period analysis, but concerns with the curse of dimensionality become more relevant with more periods, thereby limiting the general applicability of the estimator.

Furthermore, the complexity with the Lechner and Miquel (2010) estimator creates inferential challenges. As with all propensity score matching papers at the time, sampling variation in estimating the propensity score was ignored when computing analytical standard errors. With the advent of Abadie and Imbens (2016), it became standard to account for the uncertainty in propensity score estimation. Thus, should we choose to rely on Lechner and Miquel (2010), extending Abadie and Imbens (2016) to the complex case of Mahalanobis matching on a vector of estimated propensity scores seems warranted, but doing so would distract from the substantive analysis of this paper. Similarly, further investigation on inference is also needed for the inverse propensity weighting (IPW) estimator of Lechner (2009), which we discuss in more detail in Appendix B.4.2.

For these reasons, we rely on the partial identification result in Proposition 1 and the corresponding matching estimator to generate our main estimates. Its advantage is simplicity, as the estimation and inference procedures can be implemented using off-the-shelf Stata commands. Its disadvantage is the loss of point identification, but we do not view it as a threat to the empirical substance of our analysis. As mentioned above, we can also construct an upper bound, and together, the two bounds indicate an informative range for the TOT effect. Moreover, we can produce the TOT point estimate by following Lechner and Miquel (2010), which is only slightly above our estimated lower bound. We report these estimates in Section 5.1.

Finally, while this section focuses on research that assumes selection on observables, another strand of the dynamic treatment effect literature explicitly models unobserved heterogeneity. Examples include

Abbring and van den Berg (2003, 2004), Heckman and Navarro (2007), and Ba et al. (2017). We provide an overview of these studies in Appendix B.4.3.

4.3 Matching Specification

This section describes our matching specification. As discussed above, the specification is informed by our own validation exercise, described in Leung and Pei (2020). In particular, we find that in another training context, where detailed earnings histories exist for a sample of recently employed workers, modelling the propensity score as a simple logit where the terms enter linearly performs well against more flexible models. However, the validation exercise sample is quite small relative to the Ohio sample. Since we have a large sample, we use both exact and propensity score matching to ensure that our matched comparison group is as similar as possible to the enrollees.

As described in Section 3, we define enrollees as those who start school within eight quarters after filing a UI claim. Proposition 1 (more precisely, its many-period generalization, Proposition 3) suggests that we can estimate the TOT lower bound separately for those who enroll in the first through eighth quarter after layoff and aggregate them to obtain a lower bound for the overall TOT. The Proposition also states that, for all eight enrollee cohorts, we should construct the corresponding matched comparison group by drawing from the pool of workers who do not enroll within the two-year post-layoff period.

We perform exact matching along three dimensions. First, we require that enrollees be exactly matched to non-enrollees laid off in the same quarter. This is motivated by the fact that economic conditions and policies varied widely over our study period. Workers laid off in 2004 may differ from those starting unemployment at the peak of the Great Recession and face dramatically different labor market landscapes.¹⁴ Furthermore, policies enacted to help workers overcome challenges during the Great Recession, such as UI extensions and information campaigns about resources for retraining, may have influenced workers' decisions to enroll (Barr and Turner, 2015, 2016). By comparing workers that were laid off around the same time, we attempt to control for the influence of time-varying labor market conditions and policies.

Second, following the training evaluation literature, we require that workers be exactly matched on gender, as decisions to enroll may differ between men and women. As argued by Heckman and Smith

¹⁴Although Heckman et al. (1998) stress the importance of matching workers from the same local labor market, evidence from Michalopoulos, Bloom and Hill (2004) and Mueser, Troske and Gorislavsky (2007) suggests that this is less important when comparison groups are drawn from a single state. Since our data come from (moderately sized) Ohio, temporal rather than geographic variation capture most of the variation in labor market conditions within our sample. That said, we also include the unemployment rate in the month and county of layoff in the propensity score model, which accounts for geographic differences.

(1999), men’s decisions to enroll may be more heavily influenced by economic prospects, while women’s decisions may depend more on family responsibilities. The different motivations for enrolling may translate into different training effects across gender.

Finally, we exactly match enrollees and non-enrollees based on whether they were working in manufacturing at layoff. The manufacturing sector is of particular interest to policymakers, as it has been in rapid decline over the past decades (Autor, Dorn and Hanson, 2013), particularly in “rust belt” states like Ohio.

Within the (exactly matched) layoff quarter, gender, and manufacturing cells for each of the eight enrollment timing cohorts, we estimate a separate propensity score model. As we demonstrate in the validation study in Leung and Pei (2020), it is crucial to include pre-enrollment earnings, and we use three years of pre-layoff quarterly earnings as (linear) inputs into the model.¹⁵ In addition to pre-layoff earnings, we also include quarterly earnings between layoff and enrollment per our identification result (we illustrate the importance of incorporating these earnings with empirical evidence in Section 5.1). Finally, we include in our propensity score model demographic and prior job characteristics to the extent that our data allow it.¹⁶ Specifically, we include race indicators (white, African American, other, or unknown), pre-layoff sector indicators (construction, wholesale trade, administrative support and waste management, healthcare and social assistance, accommodation and food services, retail trade, transportation, and other), job tenure categories (less than one year, one to six years, and more than six years), age indicators (age below 19, each year from age 19 and 59, and older than 59), whether a worker has a dependent at the time of UI claim, and county unemployment rate during the month of layoff.

We use the estimated propensity score to pair each enrollee with her nearest neighbor from the comparison sample, where each comparison worker may be matched to more than one enrollee (i.e., matching with replacement). Choosing a larger number of neighbors may further reduce variance in the estimated treatment effect, but the quality of the match may deteriorate as we allow for larger propensity score differences between the enrollee and comparison workers. Since we have a large enough sample size to attain precise estimates and are more concerned with bias, we use only one neighbor in our main specification. We do, however, explore the sensitivity of our results with respect to the number of neighbors in Section 5.1.

After each logit propensity score regression, we estimate the (cell-specific) TOT with the average differ-

¹⁵While the literature also favors including indicators for zero earnings in each quarter, we do not include these to minimize the number of perfect prediction and overlap problems within the exact-matching-cells. However, as discussed later, we find that the inclusion of these indicators do not meaningfully change the estimates in a robustness check.

¹⁶Out of the 980 propensity score models, 27 do not converge to a solution with the full set of covariates. In these cases, we estimate the model by eliminating one covariate (i.e., one industry dummy, demographic variable, or quarter of earnings).

ence in outcomes between each enrollee and its match, and the standard error is computed following Abadie and Imbens (2016). We then aggregate the TOTs across the enrollment-timing \times layoff-quarter \times gender \times sector cells to obtain the overall treatment effect, where the weights are proportional to the number of enrollees in each cell.

Given that we match workers along the dimensions discussed above, the question of residual differences between enrollees and matched non-enrollees remains. We argue that these remaining factors that compel workers to enroll are unlikely to be related to future earnings potential, conditional on the covariates used for matching. The empirical literature documents several potential sources of variation. First, research has shown that distance to a community college affects whether a worker ultimately enrolls (Card, 1995). While we do not find distance to be a strong instrument (Appendix C), it is indeed negatively associated with enrollment. Second, information and nudges appear to influence students' decision-making. For example, in our context, Barr and Turner (2016) show that a letter targeted to UI claimants informing them of financial aid resources affects enrollment. Relatedly, complexity in UI rules (such as what constitutes as "approved training" that allow workers to continue receiving benefits) may introduce additional variation in whether workers decide to pursue training opportunities. Third, in the context of training programs, there may be variation in who can access training resources based on requirements set by local job centers (Fortson et al., 2017). Finally, there has been evidence of capacity constraints in community colleges and programs within community colleges (e.g., Grosz, 2020), which may generate exogenous variation in who can enroll.

4.4 Matching Design Quality Check: Overlapping Support and Covariate Balance

As emphasized by Smith and Todd (2005), we need to assess the validity of the overlapping support assumption. We first point out that some observations indeed appear to violate this assumption, but they are a tiny fraction of the sample. Specifically, when we use the trimming threshold from the algorithm by Imbens and Rubin (2015) (p. 367-368), we find that only 842 observations out of the nearly 72,000 enrollees are dropped—841 observations are dropped due to perfect prediction within exact matching cells, and one observation has a propensity score that is too close to 1.¹⁷

We show the overlap of the enrollee and non-enrollee distributions in Appendix Figure A.1. Because many of the propensity scores are close to zero, for ease of visual inspection, we overlay the histograms of

¹⁷The remaining difference in the enrollee sample size between Panels A and B of Table 1 is due to the elimination of the winter quarter of 2013 in the HEI data mentioned in Section 3. We do not estimate the propensity score and enrollment effect for (OTC only) enrollees who start in that quarter, which eliminates an additional 24 enrollees.

the log odds ratio (*lor*) of the two groups, which is a monotone transformation of the propensity score (i.e. $lor \equiv \log \frac{p(x)}{1-p(x)}$). In the top row, we plot the estimated log odds ratio distributions for workers who enroll one, four and eight quarters post layoff, and overlay the corresponding distributions for the non-enrollees. We see that the *lor* distributions have little support on the positive range, indicating that the propensity score is below 0.5 for the vast majority of observations, and not surprisingly enrollees tend to have a higher propensity to pursue further education. We show the *frequency* plots of *lor* for the two groups in the bottom row, which are more relevant for assessing overlapping support. Because the number of observations in the non-enrollee group is far larger than that in the enrollee group, there appears to be sufficient overlap even in the higher range of the *lor*.

To compare covariate values across samples, we follow the literature (e.g., Imbens, 2015) and report in Panel A of Table 1 the normalized differences between enrollees and non-enrollees for each covariate. That is, for each covariate X , we report $(\bar{X}_E - \bar{X}_N)/\sqrt{(S_{X,E}^2 + S_{X,N}^2)/2}$, where \bar{X}_E and \bar{X}_N are the respective sample means of X among enrollees and non-enrollees, and $S_{X,E}$ and $S_{X,N}$ are the corresponding sample standard deviations. The denominator can be interpreted as an average standard deviation (ASD), allowing the normalized difference to be interpreted in percentage terms of the ASD. The covariate that exhibits by far the largest difference is age: Enrollees are younger by 58 percent of ASD. The contrasts are less stark for the other 21 covariates: Nine have a normalized difference below 5 percent of ASD, two between 5 and 10 percent, eight between 10 and 20 percent, and two just above the 20 percent threshold of what Rosenbaum and Rubin (1985) consider a large difference (Imbens, 2015 suggests a higher rule of thumb threshold of 30 percent). In Panel B of Table 1, we show the average characteristics of the *matched* enrollee and non-enrollee samples. Indeed, covariates are balanced across the two groups: normalized differences are very small, and all t -tests fail to reject equality despite the large sample size. We proceed to present our main empirical results on the effects of retraining in the next section, where we also provide additional graphical evidence in support of balance in pre-enrollment earnings trajectories.

5 Empirical Results: Effects of Further Education During Unemployment

5.1 Overall Effects

We begin by graphically presenting the average earnings of the full sample of enrollees and their matched non-enrollees. In Figure 1, the solid line shows the average earnings of enrollees over time, from 20 quarters

before until 16 quarters after enrollment begins. The dashed line shows the earnings averaged across each enrollee's nearest neighbor in the non-enrollee sample (see Appendix Figure A.2 for a comparison of earnings trajectories for enrollees and unmatched non-enrollees). Prior to enrollment, both enrollees and their closest comparison workers have similar earnings trajectories, increasing in the period from five years prior to enrollment to approximately two years prior, before dropping to 50 percent of the peak by the time of enrollment. The seemingly slow decline in earnings is due to the fact that we are averaging the earnings of workers who enroll at different times post-layoff, and not because of a drawn-out earnings reduction process for all workers. The close alignment of the pre-enrollment trajectories signifies high match quality and is not mechanically guaranteed just because the earnings enter the logit model for propensity score estimation (see Figure 2 of Leung and Pei, 2020). After enrollment, the two lines begin to diverge. Enrollees have lower earnings for approximately two years before surpassing their comparison group. This "lock-in" effect, which may come about because enrollees are more constrained in their ability to search for jobs and work while in school, is consistent with the finding in Heinrich et al. (2013) for the WIA Dislocated Worker program.¹⁸ The gains appear to grow after the lock-in period while the earnings of non-enrollees flatten. In the third and fourth years post-enrollment, enrollees earn \$348 more per quarter than non-enrollees as reported in Table 3, a gain of about six percent. It is notable that even at more than four years after layoff, both enrollees and non-enrollees do not catch up (on average) to their pre-layoff earnings.

As discussed in Section 4.1, these estimates are lower bounds of the enrollment effect, under the assumption that those who enroll in later periods have lower counterfactual earnings than those who never enroll (Assumption 2 and its many-period generalization Assumption 4). We first present evidence in support of this assumption using the test proposed in Section 4.1 and Appendix B.1. Specifically, for an enrollment quarter $s = 1, \dots, 7$ and a later-enrollee cohort enrolling $l > s$ quarters later ($1 \leq l \leq 8 - s$), the test compares their earnings during each of the l quarters (*before* the later-enrollees enroll) against non-enrollees with similar characteristics at the beginning of quarter s . We should find lower earnings among later-enrollees under Assumptions 2 and 4 if these interim earnings positively correlate with future potential earnings $Y_t(0)$. We implement the test using propensity score matching and present the earnings differences between later-enrollees and matched non-enrollees for all seven values of l (we aggregate across s for concise presentation)

¹⁸Although the mean (median) enrollment duration is 4.5 (4) quarters in the sample, the average duration between first and last enrollment quarters is closer to 6 quarters due to the fact that workers are not always "continuously" enrolled. Furthermore, it may take some time post-training for earnings to recover: Fortson et al. (2017) note that there were about two quarters between when WIA trainees completed training and began post-training employment.

in Appendix Table A.1. Indeed, all earnings differences are negative, lending credibility to the assumption underlying the lower bound result.

Second, we show that the lower bound is informative in our setting. As discussed in Section 4.2, results from Lechner (2009) and Lechner and Miquel (2010) imply the point identification of the TOT. We implement the sequential propensity score matching estimator of Lechner and Miquel (2010) and find that enrollees earn \$351 more than non-enrollees in the third and fourth years post-enrollment. Our lower bound estimate of \$348 is remarkably close and its 95 percent confidence interval comfortably contains \$351. We also construct an upper bound using Proposition 4 in Appendix B.3. Its estimate is \$488, or nine percent; together, the two bounds pinpoint an informative range for the earnings effects. The tightness of the bounds results from the small number of later-enrollees relative to non-enrollees, so replacing the later-enrollees' potential earnings with other values will not have a major impact. The fact that the point-identified estimate is much closer to our lower bound suggests that the counterfactual earnings of the later-enrollees are better approximated by the earnings of their non-enrollee replacements than by zero.

Because our lower bound is very close to the point estimate of the TOT effect, we refer to the lower bound simply as “the effect” in the remainder of this paper for brevity. Next, we conduct several sensitivity checks on our average effect estimates. While the set of non-enrollees in Figure 1 is selected by using 12 quarters of pre-layoff earnings in the propensity score formulation, Panels A and B of Appendix Figure A.3 show that earnings patterns are robust to alternative matching specifications that include fewer quarters of earnings (one pre-enrollment quarter and four pre-enrollment quarters, respectively). However, when we match without earnings between layoff and enrollment in Panel C, the patterns are quite different: the enrollees in this panel have much lower earnings than their matched comparison group heading into enrollment.¹⁹ Appendix Figure A.3 highlights the importance of controlling for the most recent information just before enrollment. In contrast, including dummies for zero earnings (on top of the full set of quarterly earnings) in the propensity score model does not appear to affect estimates substantially—effects for the third and fourth year post-enrollment remains at six percent (\$339 per quarter, Panel A of Appendix Figure A.4). We also explore the sensitivity of our findings to using more neighbors. We show in Panel A of Appendix Figure A.5 the estimated treatment effect against the number of neighbors used.²⁰ The estimated treatment

¹⁹The estimates of enrollment effects in the third and fourth year after enrollment are \$378 (seven percent), \$366 (seven percent), and -\$219 (-4 percent) for Appendix Figure A.3 Panels A, B, and C, respectively.

²⁰For this exercise, we only use the cell with the largest number of claims (male non-manufacturing workers laid off in the first quarter of 2009). We focus on this subsample to ease the significant computational burden of running the matching analysis 25 times with the full sample.

effect tends to shift upward as we increase the number of neighbors, and the variance is reduced by up to six percent as we show in Panel B of the same figure. An estimate using the full sample with five neighbors follows this general pattern, but since our sample size allows for precise estimates, we use the lowest bias one-neighbor specification. Finally, we probe the robustness of our results to dropping counties that border another state, as workers from these counties are more likely to be employed in another state post-layoff (which we would not be able to observe). We find that the omission of these counties do not change our effect estimates much—in the third and fourth year post-enrollment, enrollees earn \$324 more per quarter (six percent, Panel B of Appendix Figure A.4).

The two panels of Appendix Figure A.6 plot the probability of having positive earnings and weeks worked per quarter, respectively. Prior to enrollment, we see a similar pattern of a rise and drop in employment for both groups. After enrollment begins, enrollees are less likely to be employed initially but eventually overtake the comparison group at approximately the two-year mark. A natural question that arises is whether or not the gain in employment can explain all of the gains in earnings, or whether enrollment increases both employment and wage rates. One way to answer this question is to note that in Panel B of Appendix Figure A.6, non-enrollees work for 7.3 weeks and earn \$5354 per quarter on average three to four years after enrollment, implying a weekly wage of approximately \$730. Enrollees, on the other hand, are employed 7.9 weeks and have a weekly wage of approximately \$722. Although the differences in weekly wage rates are not causal effects, this indicates that increased wage rates are not driving the enrollment effects.²¹ Another way to see this is to examine the earnings distributions of enrollees and matched non-enrollees, shown in Appendix Figure A.7. While the top panels of the figure document similar earnings distributions before enrollment, the bottom panels show that the distributions start to diverge eight quarters after enrollment and further widens at the end of quarter 16, where the gains are concentrated at the extensive margin. Therefore, we conclude training mainly affects employment and likely has minimal effect on wage rates four years after enrolling.

²¹Since Appendix Figure A.6 shows a small gap in weeks worked between enrollees and non-enrollees in the pre-period, the weeks "effect" may be overstated. To see how this affects our conclusion, consider the following decomposition of the earnings effect: $\Delta\text{Earnings} = \Delta\text{Weeks} \times \frac{\text{Earnings}_N}{\text{Weeks}_N} + \Delta \frac{\text{Earnings}}{\text{Weeks}} \times \text{Weeks}_E$ where Earnings and Weeks are average quarterly earnings and average weeks worked in a quarter, respectively, for enrollees (E) and non-enrollees (N), and Δ denotes the difference between enrollees and non-enrollees. If we adjust the weeks "effect" downward by 0.22 weeks (the gap in the pre-period), the first term of this decomposition still accounts for 70 percent of the total earnings effect.

5.2 Effects By Subgroup

We now present the enrollment effects by subgroup. For subgroups that have exactly matched participants (i.e., enrollment timing, layoff quarter, gender, and sector), we estimate the enrollment effect by restricting to enrollees within the subgroup and examine the difference in outcomes compared to their matched non-enrollees. For subgroups that are not exactly matched (age, tenure, and race groups), we first restrict the analysis sample to the subgroup and then re-match enrollees to non-enrollees using the estimated propensity score described in Section 4.3.²² That is, we do not implement another matching procedure with re-estimated propensity scores conditional on the subgroup, which is computationally expensive and could bring in workers not included in the set of matched non-enrollees in Section 5.1, resulting in inconsistencies across samples.

A potential challenge is that the nearest neighbor for an enrollee in the subgroup analysis may differ from that in the full sample analysis and may be a worse match. But as we show in Appendix Table A.2, our method still achieves balance in most of the 28 subgroups, as measured by estimates of TOT on earnings one to two years and three to four years pre-enrollment, respectively. After adjusting for multiple hypotheses testing with the Holm method, pre-enrollment earnings from three subgroups (workers under 40, workers with job tenures greater than six years, and Black workers) remain statistically unbalanced at the 5 percent level.²³ But even for these three groups, the imbalance goes away in the most recent year immediately preceding enrollment as seen in Appendix Figures A.12, A.13, and A.14. This is reassuring as the most recent pre-enrollment earnings are possibly most predictive of future potential outcomes. To alleviate any remaining concerns with this imbalance, which manifests in seemingly parallel trajectories between enrollee and matched non-enrollees before earnings start to decline, we also report estimates using difference-in-differences matching in Table 4. These estimates are obtained by first differencing future earnings with (symmetrically timed) pre-enrollment earnings within person, and then comparing the resulting differences between enrollees and matched non-enrollees.²⁴ The reported standard errors in Table 4

²²We cannot simply compare enrollees within a certain subgroup to their original matched comparison groups because this compares the outcomes of enrollees within a subgroup to non-enrollees that are potentially not in the subgroup. A simple way to see this is to consider a randomized experiment: if a population is randomly assigned to a treatment or control group with a coin flip, the propensity to be treated is 50 percent for the entire population. If we wanted to estimate the treatment effect for women only, we cannot simply compare the outcomes of treated women with the entire control population, even though their propensity scores are the same at 0.5.

²³Note that, because the nearest neighbor may change from the full sample to a particular subgroup, balance in the full sample and one subgroup does not imply balance in the complement of that subgroup.

²⁴The differencing step does not materially affect estimates. This is consistent with Chabé-Ferret (2017), who finds that when conditioning on many periods of pre-treatment earnings as we do here, the bias of matching with or without differencing is similar.

are from testing whether the average pairwise difference between enrollees and matched non-enrollees is different from zero, and we abstract away from the sampling variation in forming these matched pairs. This abstraction is inconsequential for the full sample where we also have available the standard errors that account for the sampling variation in forming matched pairs per Abadie and Imbens (2016): compared with their counterparts in Table 3, the two standard errors in columns (1) and (6) of the first row in Table 4 are only slightly larger.

For each of these subgroups, the effects may vary due to differences in “types” and intensity of schooling (e.g., types of courses taken, duration of enrollment, and whether a credential was obtained), differences in enrollee composition, or other factors such as labor market conditions. While we cannot tease out the exact reasons for why we find different effects for some subgroups, we discuss in turn likely explanations.

By Enrollment Timing As discussed in Section 3, workers typically do not enroll immediately after layoff, and some take longer to go back to school than others. In Appendix Figure A.8, we show the impacts of enrollment by the quarter in which workers go back to school within two years after layoff. Examining the pre-enrollment period, we see a sharp drop in earnings that is similar across all eight panels, each corresponding to enrolling in a certain quarter (one through eight) since job loss. By comparing the matched non-enrollees’ earnings trajectories in each graph, it is clear that the groups differ in the extent to which their earnings recover post-enrollment, with larger earnings gains for those who enroll later relative to layoff. As reported in Table 4, we find that workers who enroll later have a smaller “lock-in” effect in the first two years after enrolling, and larger gains from enrollment (up to 12 percent) in the third and fourth years.

There are several potential explanations for these patterns. First, it is possible that the larger lock-in effects are due to a higher “treatment dosage” for those who enroll earlier. Consistent with the lock-in effects, those who enroll one quarter after layoff have a mean enrollment duration of 4.7 quarters, while those who enroll eight quarters after layoff are only enrolled on average for 4.2 quarters (and the pattern is monotonic for those who enroll in the second through seventh quarters). It is also true that these longer durations of enrollment correspond to higher rates of credential receipt: For those who enroll within the first quarter post-layoff, about 27 percent receive a credential, while for those who enroll in the eighth quarter after layoff, the credentialing rate is about 20 percent (the pattern is roughly monotonic in between, though it peaks for those who enroll in the second quarter). However, we also see that earlier enrollees have lower post-enrollment gains, which seems inconsistent with the fact that they have more “intensive” schooling

(though it is possible that we simply have not looked at a long-enough post-period to observe the full gains).

Another explanation, however, is consistent with the discussion in Heckman, LaLonde and Smith (1999) that training is a form of job search, and enrollees tend to be workers who are not yet reemployed. Correspondingly, those who enroll later have relatively long spells of unemployment. In other words, earnings patterns seem to reflect selection into who enrolls earlier or later. Appendix Figure A.8 shows that later enrollees tend to be those who have earnings that have been depressed for a while before enrolling, and their counterfactual earnings without school tend to be flatter than earlier enrollees. The lack of lock-in for later enrollees likely reflects their low opportunity cost of schooling. Therefore, while we find larger enrollment effects for those who enroll later, we note that this result does not imply that it would be more advantageous for workers to delay enrollment. The exercise may simply be comparing across different types of workers.

By Layoff Year Since our study period covers the Great Recession, there is considerable variation in the labor market conditions at the times of enrollment and reemployment. Prior studies have shown that training programs are more effective when the unemployment rate is high at program entry and low when training ends (Lechner and Wunsch, 2009; Kluge, 2010; Card, Kluge and Weber, 2018). In particular, poor labor market conditions at program entry are associated with smaller lock-in effects due to lower opportunity costs of training, as well as larger gains at the end of training. Outside of the training literature, there is also evidence that graduating college during an economic downturn is associated with persistent earnings losses (Kahn, 2010; Oreopoulos, von Wachter and Heisz, 2012).

In Appendix Figure A.9, we present the earnings of enrollees relative to matched non-enrollees separately by year of layoff. Consistent with the literature, we find that the effects of training do appear to be larger among workers laid off when unemployment rates peaked during the Great Recession: Table 4 shows that workers laid off in 2010 and early 2011 have earnings gains of 8 to 12 percent in the third and fourth years post-enrollment, while the cohorts laid off prior to the recession in 2006 to 2007 (who would be entering the labor market as unemployment was rising rapidly) have gains that are indistinguishable from zero (the effects for those laid off in 2008 and 2009 fall somewhere in between these extremes).

Of course, there are differences in the composition of workers who enter unemployment in different years—for example, workers who lost their jobs at the peak of the recession had higher earnings than workers laid off earlier. To explore this, we conduct an exercise similar to those by Lechner and Wunsch (2009) and Heinrich and Mueser (2014), where we reweight each year’s estimates so that the enrollee composition

matches those laid off in 2004 based on observable characteristics. Specifically, for enrollees laid off in year m , we estimate the counterfactual average earnings impact (conditioning on $D = 1$ is omitted for brevity):

$$\begin{aligned} & \int E[Y_t(1) - Y_t(0)|\mathbf{X}, \text{year} = m] dF(\mathbf{X}|\text{year} = 2004) \\ &= \int E[Y_t(1) - Y_t(0)|\mathbf{X}, \text{year} = m] \underbrace{\frac{\Pr(\text{year} = 2004|\mathbf{X})}{\Pr(\text{year} = m|\mathbf{X})} \frac{\Pr(\text{year} = m)}{\Pr(\text{year} = 2004)}}_{\text{weight}} dF(\mathbf{X}|\text{year} = m). \end{aligned}$$

To estimate the weights, we restrict the sample to only enrollees laid off in 2004 and year m . We predict whether an observation is from the 2004 cohort via logit using observable characteristics (gender, industry, age, race, presences of dependents, pre-layoff wages, and quarter of layoff), and the predictions give us the first term of the weights. For the second term of the weights, we simply need the proportion of observations in each year. We then calculate the reweighted treatment effect for enrollees laid off in year m via the usual nearest neighbor matching, but with each treatment-control pair multiplied by its estimated weight.

We present the results in Appendix Table A.3 and find that changing composition of the layoff cohorts over time play a minimal role in the larger treatment effects of 2010 and 2011, though it does appear to moderately dampen the effect for 2009.²⁵ The finding that composition does not explain much of the effect variation over time echoes Lechner and Wunsch (2009). It is also broadly similar to Heinrich and Mueser (2014), which finds that training programs targeting displaced workers were more beneficial in 2008-2009 relative to 2007 (though composition explains more of the temporal variation in that context than ours).

We conclude from this exercise that layoff timing plays an important role in determining the earnings gains. The flatness of the earnings trajectories of those laid off in 2006 and 2007 likely results from the lack of job opportunities when the workers completed their training during the Great Recession. In comparison, earnings grew much more quickly for later cohorts that faced a thawing labor market.

By Gender Appendix Figure A.10 shows the enrollment effects by gender. In contrast to Jacobson, Lalonde and Sullivan (2005a), we find that the gains to enrollment are larger for men in the four years post enrollment, though we note that women spend more time in school, at an average of 5.1 quarters over four years versus 4.1 quarters for men. This means that women have a longer “lock-in” period relative to men, and a longer follow-up period may reveal larger effects for women. In the third and fourth years af-

²⁵The small discrepancies in the number of observations between Appendix Table A.3 and Table 4 are due to the fact that we require “overlap” between the 2004 enrollees and other year’s enrollees. Practically, this means that when enrollees have characteristics that perfectly predict their unemployment year (i.e., they are so different from the 2004 enrollees that they cannot be used for reweighting), we drop them from the analysis.

ter enrollment, we find that the earnings gains of enrollment are nine percent for men and four percent for women. In Section 5.6, we discuss the different types of courses and credentials obtained by women and men, and the subsequent differences in industries of employment.

By Manufacturing vs. Non-manufacturing Due to the decline of the manufacturing sector and the large earnings losses suffered by unemployed workers therein, the effects of retraining among this group have attracted particular policy interests (Couch and Placzek, 2010). Appendix Figure A.11 shows separately how further schooling impacts manufacturing and non-manufacturing workers. Consistent with previous studies on the earnings impact of displacement, manufacturing workers, who make up 29 percent of our sample, appear to have higher average pre-layoff earnings than non-manufacturing workers and experience a correspondingly larger drop in earnings post-layoff. We find that in the third and fourth years post-enrollment, manufacturing workers see a two percent earnings gain on average, smaller than the nine percent for non-manufacturing workers (Table 4). However, there is reason to believe that the effect will increase further for manufacturing workers in the future. The long lock-in period, which largely contributes to the muting of the third to fourth year earnings effect, partly reflects the substantially higher (14 percentage points) likelihood of enrollees from manufacturing to switch to a different industry than matched non-enrollees (we discuss industry switching in more detail in Section 5.6). Since switchers forgo industry specific skills, it may take longer for the human capital investment to pay off. Furthermore, we see in Panel A of Appendix Figure A.11 that the earnings trajectory of the non-enrollees is quite flat for those who previously worked in manufacturing, allowing for the possibility of higher longer-run enrollment effects.

Other Subgroups The final rows of Table 4, and the accompanying earnings trajectory plots in Appendix Figures A.12-A.14, show the enrollment effects for several other subgroups of interest. We find statistically significant positive effects across workers of different age and some racial groups. There do not seem to be large differences in enrollment effects for older (age 40 and above) workers relative to younger workers and the effects are of a similar magnitude as in Jacobson, Lalonde and Sullivan (2005*b*). Although Jacobson, Lalonde and Sullivan (2005*a,b*) focus on long-tenured workers who have worked three or more years before layoff, we find that the shortest-tenured group with less than one year of tenure enjoys the largest gains at ten percent. Finally, while there is no consensus on how the effects to community college vary by race (Belfield and Bailey, 2011) and estimates are not available in Jacobson, Lalonde and Sullivan (2005*a,b*), we

find larger effects for whites than for African Americans.²⁶

Lastly, another interesting subgroup to examine consists of those with previous college experience. While we do not observe prior college for most workers in our analysis sample, the higher education data does go back far enough for us to observe recent college experience (i.e., within the previous seven years) for those who were laid off 2007 and later. For this analysis, we focus on those who are age 25 or younger, as this lookback period would capture their prime college attendance years (i.e., ages 18-25).²⁷ These restrictions eliminate 94 percent of sample, though within this subsample, the proportion of those who enroll after layoff is higher than in the full sample (nine percent vs. four percent). Among these enrollees, 70 percent had no prior college. Since these are much smaller samples, we re-estimate the propensity scores for each subsample (enrollees who have or did not have prior college), and require only exact matches on gender and claim year (rather than quarter and manufacturing vs. not manufacturing as in the main analysis); all other matching variables remain the same, with the addition of claim quarter and sector in the propensity score model. The results are presented in Appendix Figure A.15. We find that those without prior college experience larger effects of about 12 percent while those with recent prior college experience a marginally statistically significant gain (at the ten percent level) of about three percent in the third and fourth years after enrolling. One interpretation of this finding is decreasing marginal benefits: Among young unemployed workers, additional training is less beneficial if one already has some college coursework. However, we caution that this is a narrow subgroup of workers and the results may not generalize to older or higher tenure workers who may have been out of school for a longer period of time.

5.3 Effects By School Type and Quality

We now turn to how earnings gains vary by the type of school attended. Our main enrollee sample consists of those whose first observed institution are community colleges and technical centers. We now separately analyze the enrollment effects for each. First, we note that technical center enrollees differ from community college enrollees in terms of both pre-layoff and enrollment characteristics. As shown in Appendix Table A.4, technical centers tend to enroll many more former manufacturing workers (50 percent of technical center enrollees versus 25 percent of community college enrollees), and those who have higher tenures and

²⁶One possible explanation for this is discrimination in the labor market. For example, Bertrand and Mullainathan (2004) find that African American job applicants have lower returns (as measured by callbacks) to resume quality than white job applicants.

²⁷While one may be concerned with the validity of conditional independence for younger workers with a limited earnings history, we note that our sample only contains workers who have had substantial enough earnings to be eligible for UI.

pre-layoff earnings. Technical center enrollees also tend to be enrolled for slightly longer periods and are much more likely to obtain a (sub-associate) credential.

To estimate the effects of community college (technical center) attendance, we restrict the sample to enrollees whose first institution is a community college (technical center), and non-enrollees from the main analysis sample. We then implement the same matching specification as our main analysis. Appendix Figure A.16 shows the earnings of enrollees in the two types of institutions and their matched non-enrollees.

We find that the patterns of earnings are quite different for community college and technical center attendees. The “lock-in” earnings loss for technical center enrollees is much more substantial than that of community college enrollees, of about -\$990 (22 percent) vs. -\$283 (six percent) for the first two years post-enrollment. This may reflect the relatively more “intensive” nature of the courses offered at technical centers; although it is difficult to compare to those offered at community colleges, the fact that many more enrollees gain a credential from a technical center (in approximately the same duration of enrollment) does indicate higher coursework intensity. Turning to the third and fourth years post-enrollment, the earnings gain for technical center enrollees is also larger, at \$516 (ten percent) vs. \$336 (six percent).

Since the majority of our sample are community college enrollees, it is worth examining whether different community colleges can generate different earnings gains. We ask in this subsequent analysis whether “school quality” plays a role in determining the earnings effect. Although quality is multidimensional, we focus on three measures: the instructional spending per student, the proportion of students graduating within eight years after entry, and median long-run (ten-year) earnings of those who enroll in an institution. The first measure is calculated using information from IPEDS and the latter two are based on the College Scorecard developed by the U.S. Department of Education. We break the sample of community college enrollees into two groups by whether they are above or below the median (within our sample) in terms of their institution’s instructional spending and match them to non-enrollees. We do the same for those who are above or below the median in terms of the institution’s completion rate and long-run earnings. Appendix Figure A.17 shows the results of this analysis. We find that school quality matters for earnings effects. For those enrolled in schools with low (below median) instructional expenditures, the third and fourth year effect is \$300 (six percent) while the analogous numbers for those attending schools with high (above median) expenditures is \$419 (seven percent). Similar patterns emerge when we measure quality using institutional completion rates and median earnings: For those attending institutions with high completion rates and earnings, the third and fourth year earnings effect is \$471 (nine percent) and \$443 (eight percent), respectively, versus \$379 (seven

percent) and \$291 (five percent), respectively, for institutions with low completion rates and earnings.

5.4 Enrollment Effects For Workforce Investment Act Participants

As discussed in the introduction, much of the evidence on the earnings gain associated with retraining in the U.S. are from evaluations of training programs, such as those funded by WIA. While our study population consists of a broader set of workers who seek retraining, this section focuses on the WIA trainees in our sample to better connect to the literature on government sponsored training programs. Specifically, we provide descriptive statistics for the subset of workers who have trained under WIA and estimate their enrollment effects.

For this analysis, we restrict our sample of enrollees to workers who are also observed to have been enrolled in WIA training within two years of layoff (we refer to this group as “WIA enrollees”). We identify 8,771 WIA trainees within our enrollee sample of almost 71,000 workers.²⁸ WIA enrollees differ in characteristics from our overall enrollee sample. As shown in Appendix Table A.5, more than half of WIA enrollees are formerly manufacturing workers, compared with 29 percent for the main analysis sample. They are also older and have higher earnings prior to layoff. In terms of schooling characteristics, WIA enrollees are more likely to enroll in technical centers (45 percent versus 15 percent for the main sample), and are enrolled longer (5.2 versus 4.5 quarters). Finally, WIA enrollees are more likely to obtain a credential (58 percent versus 26 percent), mostly at the associate and sub-associate levels.

Using this sample of WIA enrollees and the non-enrollees from our main analysis sample, we implement the matching estimator discussed in Section 4.3 with some adjustments for the relatively small number of WIA enrollees. Instead of exactly matching on claim quarter, gender, and sector, we simply require enrollees be matched with non-enrollees who were laid off within the same year and are of the same gender (we drop 459 enrollees that violate our overlap thresholds), and add claim quarter and sector dummies to the propensity score model.

Figure 2 shows the earnings of WIA enrollees and their matched non-enrollees. Compared with Figure 1 for all enrollees, we see that WIA enrollees have higher average pre-layoff earnings and experience a larger drop at layoff. After enrollment, WIA enrollees endure a statistically significantly larger lock-in

²⁸We observe an additional 18,070 WIA trainees in our analysis sample who do not enroll in a public two-year postsecondary institution, which occurs because WIA training may take place at employers, private/online schools, or other institutions not captured in our data. In Fortson et al. (2017), 24 percent of a nationally representative sample of WIA trainees assigned to the “full WIA” arm of the WIA Gold Standard Evaluation enrolled in two-year community colleges; the analogous number in our setting is 19 percent.

effect (-33 percent relative to non-enrollees in the first two years post-enrollment), catch up to matched non-enrollees more slowly, and see smaller effects (four percent; statistically significant at the five percent level) in years three and four. Not surprisingly, these earnings patterns for WIA enrollees are similar to those for manufacturing workers as depicted in Panel A of Appendix Figure A.11, given the large proportion of manufacturing workers among WIA enrollees. And analogous to our discussion of manufacturing workers in Section 5.2, the trajectories indicate that WIA effects may increase further into the future.

Our results are broadly consistent with a recent randomized evaluation of WIA by Fortson et al. (2017). Their experiment compares the outcomes of randomly selected workers who were offered the full array of WIA services (including training services if eligible) versus those offered only non-training WIA services (e.g., either “core” or both “core” and “intensive” services, which include informational tools, job search assistance, and placement services). Fortson et al. (2017) find no statistically significant difference in earnings between the two groups 12 quarters after randomization, the last time period observed, though the difference in actual training receipt in their study is only about 9 percentage points between the treatment and control groups.²⁹ However, the point estimates of Fortson et al. (2017) document similar dynamics as Figure 2, in that the earnings of the treatment group catch up to the control group at about two years after randomization.

5.5 Long-Run Effects

While our data do not span a long enough period to allow for the estimation of longer run enrollment effects for the entire analysis sample, we can examine whether the earnings gains are likely to persist by following a subsample of early enrollees further out. For this analysis, we restrict our attention to workers who enroll no later than 2007Q3 (15,833 enrollees) and compare their earnings to their matched non-enrollees for ten years post-enrollment. Since all workers in this subsample were laid off before the Great Recession, they differ from our main analysis sample in that their earnings (and “short-run” enrollment effects) are somewhat depressed in the four-year follow-up period as shown in Table 4. We see this pattern again in Figure 3, where the twin vertical dashed lines denote the two follow-up periods for our main effect estimates. Despite the dip in earnings that begins around the second to third year after enrollment, the effects appear to increase over time as the labor market rebounds, ultimately resulting in a 13 percent gain in the tenth year after enrollment (point estimate \$706, standard error \$73). We find that enrollees are five percentage points more

²⁹As Fortson et al. (2017) report a local average treatment effect, it is not directly comparable to our estimates of the treatment-on-the-treated effect.

likely to be employed at the end of the ten-year follow-up period, and conditional on employment, enrollees earn six percent more (the extensive margin explains 58 percent of the overall earnings gain in a formal decomposition exercise). Our estimates are higher than Jacobson, Lalonde and Sullivan (2005a)'s preferred extrapolated long-run effects of retraining from the 1990s (six to eight percent as mentioned in Section 1), which the authors suggest may be downwardly biased due to differential pre-trends. Our estimates are qualitatively consistent with the long-run positive effects of TAA for high quality (long duration) training by Hyman (2022), although it is hard to draw quantitative comparisons because Hyman (2022) estimates intent-to-treat effects and the TAA treatment includes both benefit payments and training.

5.6 What Does Schooling Do?

In this section, we explore the mechanisms underlying our main estimates. We first present results from simple decomposition analyses showing that the positive earnings effects are primarily driven by increased employment in certain industries, especially healthcare. We then analyze the course and credential data and document that the “excess” employment in these industries can be largely accounted for by the number of workers taking related courses.

We start our analysis by exploring the extent to which enrollees are more likely to leave their pre-layoff (two-digit) industry. We focus on the broadest categorization of industry to highlight the contrast between pre-layoff and post-layoff jobs. The right (left) side of Figure 4 Panel A shows the probability that an enrollee (matched non-enrollee) is employed in their pre-layoff industry, not employed, or employed in a different industry, over the four-year follow-up period. Consistent with Appendix Figure A.6, enrollees are less likely than non-enrollees to be employed immediately after enrollment, and Figure 4 shows that this difference is almost entirely explained by the differential probability of re-employment within the pre-layoff industry, as the two groups are about equally likely to switch industries in quarter zero. Over time, the probability of working in a different industry increases more quickly for enrollees: by the end of the four-year follow-up period, they are nine percentage points more likely to work in a different industry. On net, enrollees are seven percentage points more likely to be employed in quarter 16 after enrollment.

Panel B of Figure 4 decomposes the average earnings of enrollees and non-enrollees in each quarter of the follow-up period from Figure 1 into components contributed by industry stayers and industry switchers

(following Autor et al., 2014). Specifically, each group's quarterly earnings are decomposed as

$$E[\text{Earnings}] = \underbrace{\Pr(\text{Empl. in Same Ind.}) \cdot E[\text{Earnings} | \text{Empl. in Same Ind.}]}_{(i)} + \underbrace{\Pr(\text{Empl. in Diff. Ind.}) \cdot E[\text{Earnings} | \text{Empl. in Diff. Ind.}]}_{(ii)} \quad (8)$$

where “Empl. in Same Ind.” and “Empl. in Diff. Ind” denote employment in the pre-layoff and non-pre-layoff industry, respectively. The gray lines in the figure represent components (i) and (ii) in equation (8) for enrollees (solid) and non-enrollees (dashed), and each pair sums up to their respective black lines. Consistent with Panel A, the earnings difference in quarter zero is driven by a higher contribution from industry stayers (component i) among non-enrollees. Over time, however, industry switchers among enrollees (component ii) out-contribute their non-enrollee counterparts. By quarter 16, industry switching entirely explains the overall earnings gain: the difference in component (ii) between enrollees and non-enrollees is 103 percent of the overall earnings gain, while the difference in component (i) is -3 percent.

Appendix Figure A.18 shows which industries drive the enrollment effects by plotting the number of enrollees and matched non-enrollees employed in each sector (or not employed at all) in quarter 16. Among women (Panel A), enrollees are much more likely to work in healthcare. Among men (Panel B), enrollees are also more likely to work in healthcare (though to a lesser extent) and construction. Both male and female enrollees are less likely to work in manufacturing. To connect these findings to Figure 4, Appendix Figure A.19 shows that industry switchers make up the bulk of the increased employment in healthcare. This pattern of industry switching driving the effects of enrollment is consistent with Carruthers and Sanford (2018), who study the effects of attending sub-associate level institutions in Tennessee.

Given that increased employment in healthcare and construction explains most of the gains associated with schooling, we now provide evidence that the courses taken and credentials received by individuals in these sectors are related to their work. We link courses or credential subjects to industries using a mapping of academic subjects to occupations from the National Center of Education Statistics and the joint distribution of occupations and industries from the National Employment Matrix by the Bureau of Labor Statistics. Specifically, we define an academic subject as associated with an industry if the subject prepares a worker for an occupation where more than a quarter of the occupation is employed in a single two-digit industry (see Appendix A.2 for details). For community colleges, where individuals typically take multiple courses, we associate an enrollee's coursework with the modal industry across her courses.

Figure 5 shows the fraction of enrollees in each post-layoff industry who have taken courses that are linked to that industry. We see a large fraction of women (Panel A) employed in healthcare having taken related courses. The analogous plot for men (Panel B) shows that meaningful fractions of enrollees employed in construction, healthcare, and manufacturing have taken related courses. Strikingly, for both men and women, the number of healthcare or construction course-takers mirrors the enrollee-matched-non-enrollee employment difference in the sector. Turning to the analogous results for industry-related credentials, Figure 6 shows that for both men and women, the “excess” healthcare employment match those with health-related credentials, but the same is not true for construction.³⁰ The finding that the healthcare sector is a key driver of employment growth among those who retrain is consistent with empirical evidence from other studies. Grosz (2020) shows that one specific health program (associate’s degree in nursing) leads to substantial earnings gains relative to not enrolling at all. Others find evidence that earnings gains associated with credentials in health appear larger than those for non-health subjects (e.g., Bohn, McConville and Gibson, 2016; Stevens, Kurlaender and Grosz, 2019). The lack of credentials in construction suggests that the mechanism through which schooling improves labor market prospects may differ by industry. While training may increase access to jobs that require licenses or certification in healthcare, the mechanism is more likely to be industry-specific skill-building in construction.

5.7 Enrollment Effects for Completers (and Related Issues)

An interesting question that remains is whether the enrollment effects differ by the nature of the schooling spell (i.e., whether someone ultimately obtains a credential, or “completes”). One way to answer this question would be to make assumptions about the earnings processes and selection into completion. This is the prevailing method in the literature for estimating credential effects, and used, for example, by Jepsen, Troske and Coomes (2014) and Stevens, Kurlaender and Grosz (2019). These papers estimate the effects of different types of community college degrees by assuming that conditional on an enrollee fixed effect, there is no more selection into completion of different types of credentials. We have run the same fixed effects regression, but we find that, in our sample, there are large differences in earnings patterns (particularly in the transitory components) among completer and non-completer enrollees that would violate the identifying

³⁰We find that 73 percent of those who have taken healthcare courses and are employed in healthcare receive a credential, compared to the overall credential rate of 26 percent. The credential data suggests that it is not a specific occupation within healthcare that is driving the result: of the approximately 3,500 enrollees employed in healthcare with credentials, 26 percent received a credential in Licensed Practical Nursing, 14 percent in Nursing Assistance, and 12 percent in Registered Nursing.

assumptions of a fixed effects model. This may not be surprising given that we also find differential trends in earnings leading up to enrollment among all UI claimants when we control only for individual fixed effects and displacement effects, as discussed in Appendix C.

Alternatively, we can construct formal bounds for the average enrollment effect among completers with additional assumptions. In the spirit of Assumption 2, it might be reasonable to assume that completers have lower potential earnings than non-completers, conditional on information available at enrollment. We state our formal assumptions and describe the construction of bounds in Appendix D.³¹ However, the estimated bounds are not informative: We find that the completer enrollment effect (three to four post-enrollment) lies between -\$1036 and \$5262, a range that cannot rule out zero or even a sizable negative effect. As discussed in Appendix D, this lack of information can be largely attributed to the relatively low fraction of completers.

While the bounds are not informative, we provide suggestive evidence that the post-enrollment earnings growth is driven mostly by completers. We decompose enrollee earnings into parts contributed by completers and non-completers and present the results in Appendix Figure A.20.³² We see completers contribute disproportionately to earnings growth. Between quarter 0 and 16, enrollees gained \$2,566 in earnings over the four years, with completers (which make up 26 percent of enrollees) contributing \$982 and non-completers (which make up 74 percent of enrollees) contributing \$1,585. In other words, completers' gain is more than three quarters larger than that of non-completers, and if the completer proportion were increased to 50 percent, the average enrollee earnings gain would increase to \$2,959.

6 Cost-Benefit Analysis of Further Education

In light of the estimated average earnings impacts, can we justify the investment in further education? In this section, we provide back-of-the-envelope calculations on the private and social returns to retraining an additional worker. The private return compares the net present value of the stream of *after-tax* earnings impacts of retraining against the out-of-pocket education expenses an enrollee has to pay upfront. The social return compares the net present value of the stream of *pre-tax* earnings impacts of retraining against the overall cost of enrolling an additional unemployed worker.

³¹While we focus on the enrollment effect of completers, these results could also apply to bounding the enrollment effect among those who choose a particular course of study.

³²Specifically, this graph plots components (i) and (ii) in the following decomposition for enrollees: $E[\text{Earnings}] = \Pr(\text{Completer}) \cdot E[\text{Earnings} | \text{Completer}] + \Pr(\text{Non-completer}) \cdot E[\text{Earnings} | \text{Non-completer}]$.

(i) (ii)

While we rely on our estimates of earnings impacts to calculate the returns, we need further assumptions regarding other inputs, such as years of work life remaining and tax rates. First, we assume that an average enrollee has 30 years remaining in her work life just prior to retraining. Given that an average enrollee in our sample is about 35 years old at layoff (Table 1) and that it takes slightly less than a year from layoff to enrollment (Table 2), our assumption implies that the enrollee will stop working at around 66, which is consistent with Jacobson, Lalonde and Sullivan (2005*b*) and just under the normal retirement age of 67 faced by most of our enrolled workers. Second, we use the ten-year post-enrollment real earnings impacts graphed in Figure 3 for workers who began schooling before the fall quarter of 2007 and assume that the earnings gains from year 11 until year 30 will stay at the tenth year level. The earnings impacts increase monotonically from -\$2,327 in the first year to \$2,915 in the ninth year before dipping slightly to \$2,824 in the tenth year (the tenth year impact does not differ statistically significantly from the ninth year impact). Our returns measures will be biased downward if the gains keep getting higher beyond the tenth year, but they will overstate the benefit of further education if gains eventually fall. We return to the latter possibility at the end of this section. Third, we use a real interest rate of two percent to discount future earnings when calculating the net present value. This is based on the fact that the daily Treasury real long-term rates, as calculated from the yields of outstanding long-term Treasure Inflation-Protected Securities (TIPS), averaged to 1.98 percent between 2009 and 2010, the two years during which enrollment in our sample peaked. Our real interest rate is lower than the four percent used by Jacobson, Lalonde and Sullivan (2005*b*) due to the different time period we examine.³³ Finally, we follow Jacobson, Lalonde and Sullivan (2005*b*) and assume that workers pay 25 percent of their earnings in various taxes. This average tax rate may be on the higher end given the lower federal income tax rates relative to those in the 1990s, which will imply a conservative private return estimate. The tax rate does not enter the social return calculations.

To compute the private cost of further education, we rely on IPEDS and the Digest of Education Statistics by NCES. To estimate the yearly out-of-pocket expenses for a typical enrollee, we first subtract the expected amount of grant from the sum of annual tuition and book costs during the 2010-2011 academic year. While IPEDS reports tuition and book costs directly, we need to calculate the expected amount of grant. To do this, we multiply together the reported average grant amount among students receiving a

³³Jacobson, Lalonde and Sullivan (2005*b*) study workers who enrolled in the 1990s when the long-term Treasury bill yields were around seven percent and inflation rates around three percent. TIPS were not introduced until the late 1990s, and the published Treasury real long-term rates became available in early 2000, at which point they hovered just above four percent—consistent with Jacobson, Lalonde and Sullivan (2005*b*).

grant and the fraction of students receiving a grant. The first measure in the product is an estimate of $E[\text{grant amount}|\text{receiving grant}]$, which is \$4,194 for community colleges and \$4,559 for technical centers, and the second measure an estimate of $\text{Pr}(\text{receiving grant})$, which is 73 percent for community colleges and 74 percent for technical colleges. To the extent that UI claimants are more likely to qualify for and receive larger grants, we overstate out-of-pocket costs and underestimate private returns. We estimate out-of-pocket expenses separately for the two types of institutions (community colleges and technical centers), and we take an average across the two types weighted by the proportion of enrollees attending each.³⁴

To estimate the social investment on an additional enrollee, which encapsulates both the enrollee's expenses and government subsidies, we follow Rouse (1998) and proxy it with the variable cost per full-time-equivalent (FTE) student. Like Rouse (1998), who estimates the cost of educating a student in an associate degree program, we obtain our variable cost by excluding from the total expenditure any fixed costs (spending on administration, public service, operation and maintenance) and research outlays (which is not important for training enrollees in our sample and accounts for less than 0.1 percent of the total expenditure). We compute the cost measures using information for the 2010-2011 academic year from Snyder and Dillow (2013), and the annual social investment per FTE student is \$8,084 in a community college.³⁵ Expenditure information is not available for technical centers in Snyder and Dillow (2013). To be conservative, we proxy it with a higher measure of \$14,122, which is the variable cost for an FTE student at a four-year institution.³⁶ Lastly, since the average length of enrollment is 4.5 terms, we multiply the yearly cost measures by $4.5/2 = 2.25$, assuming two academic terms per year. There could be more than two terms per year, and enrollees in our sample might not have enrolled full time, both of which imply an overestimate of cost and a conservative estimate of the return.

Putting all the numbers together, the net present value of the average post-tax earnings impacts is \$37,056 against an out-of-pocket cost of \$6,121, and the resulting private net benefit an enrollee accrues is \$30,936. The net present value of the average pre-tax earnings impacts is \$49,408 against a social investment of \$20,217, and the resulting social net benefit an enrollee accrues is \$29,192. Another way to interpret these

³⁴Unlike two- and four-year institutions that report cost information for each academic year, most technical centers report cost statistics by program, and we use the average costs corresponding to the largest program offered.

³⁵This figure represents the national average and is not Ohio-specific. Snyder and Dillow (2013) only break down the total expenditure into detailed categories at the national level, meaning that we cannot conduct the Rouse (1998) exercise for Ohio. That said, the reported total expenditure per two-year FTE student is \$12,398 nationally and \$12,346 in Ohio. Given the small difference, the national average is likely to serve as a good substitute.

³⁶To estimate the cost of educating a two-year college student in a four-year institution, Rouse (1998) adjusts the variable cost of four-year colleges because it is cheaper to add a two-year student than an upper class undergraduate or graduate student. We arrive at the dollar figure by adopting the same adjustment for the cost of educating a student enrolled in a technical center.

quantities is that the worker gets \$6.05 for every dollar invested, and the society gets \$2.44. These ratios are high compared to those reported by Jacobson, Lalonde and Sullivan (2005*b*), whose private and social benefit-to-cost ratio estimates are in the range of \$1.69-\$4.52 and \$1.20-2.61, respectively (their Table 5A columns 1-4). The smaller 2 percent real interest rate we use is largely responsible for the differences—using a 4 percent real interest rate to discount future earnings as in Jacobson, Lalonde and Sullivan (2005*b*), our private and social benefit-to-cost ratios become \$4.37 and \$1.76, respectively.

Our estimates imply a private internal annual rate of return (IRR) of 15 percent and a social IRR of 8 percent. Jacobson, Lalonde and Sullivan (2005*b*) do not report the private IRR, so we compare our estimate against those by Heckman, Lochner and Todd (2006). Our private IRR is somewhat higher than those reported by Heckman, Lochner and Todd (2006) for a comparable education level, which come from applying generalized Mincer regressions to the Decennial Census data. After accounting for tuition and taxes, Heckman, Lochner and Todd (2006) find private IRRs between 8 and 14 percent for two more years of study among those with 12 years of education (their Table 4).³⁷ Our social IRR is comparable to the preferred estimates of 4 to 14 percent by Jacobson, Lalonde and Sullivan (2005*b*) that pass a specification check (their Table 5B column 2).

We have mentioned reasons that these estimates may be conservative (e.g., the approximations of tax rate and the use of investment per FTE student to measure training cost). Additionally, the “long-run” earnings estimates used for this calculation comes from an early cohort of enrollees who reentered labor market at the height of the Great Recession and likely saw lower earnings gains compared to later cohorts, as seen in Appendix Figure A.9. Finally, our implicit assumption that out-of-pocket expenses are paid in cash is also likely to underestimate the private IRR.³⁸ For example, if the average out-of-pocket expenses are financed entirely with unsubsidized Stafford loan at the 6.8 percent nominal rate during our sample enrollment period, the private IRR increases to 20.4 percent.³⁹ This higher IRR reflects the fact that when training returns exceed borrowing costs, it is optimal to cover education expenses with loans.

³⁷Our sample is arguably more comparable to this population than the population going from 14 to 16 years of education, as the latter consists of many who obtain a Bachelor’s degree. Our higher private IRR of 15 percent may be driven by the different time period examined. Heckman, Lochner and Todd (2006) show their IRRs increased by 6 to 17 percentage points between 1950 and 1990, and further gains are likely going into the 2000s. Another potential reason for our higher IRR is that our sample of UI claimants with previous labor force attachment is more positively selected.

³⁸According to IPEDS data for the 2010-11 academic year, 51 percent of Ohio students at community colleges and 39 percent at technical centers took out loans. The average loan amounts were \$4,500 and \$5,548, respectively. Following Rouse (1998) and Jacobson, Lalonde and Sullivan (2005*b*), loans (and grants) to students do not affect the social IRR calculation (the corresponding investment is calculated from the institutions’ perspective, which is agnostic about how tuition and related expenses are paid).

³⁹We assume a two percent annual inflation rate as the average was 1.92 percent in the time span of our post-enrollment data.

Of course, by using average measures that include *ex post* earnings impacts in our calculations, we ignore both uncertainty and heterogeneity. When making enrollment decisions, workers are also unsure how much they will make post-training. Individuals also have different abilities leading to different labor market returns and capacity to pay back loans. Similarly, workers incur different non-monetary training costs we do not account for—these include psychic costs or long hours spent studying, which Carneiro, Hansen and Heckman (2003), Cunha, Heckman and Navarro (2005, 2007), and Cunha and Heckman (2007, 2008) show can be substantial. We acknowledge that accounting for uncertainty and heterogeneity tends to lower the return estimates (Heckman, Lochner and Todd, 2006), but we abstract away from them to facilitate comparisons with existing estimates. Similarly, we follow Rouse (1998), Jacobson, Lalonde and Sullivan (2005b), and Heckman, Lochner and Todd (2006) and abstract away from general equilibrium effects.⁴⁰

With these abstractions, the only assumption we invoke that may substantially overstate the benefit of further education is the persistence of training effects over a twenty-year horizon (between year ten and year thirty). While this extrapolation is consistent with Jacobson, Lalonde and Sullivan (2005b), it may not serve as a good approximation. For example, many of our enrollees might have lost jobs again in the COVID-19 pandemic, including those working in healthcare due to the postponement of routine and elective procedures (Scott, 2020), and the earnings gain might have decreased substantially as a result. One way to interpret the numbers without the twenty-year extrapolation is to ask how long it takes to break even on the educational investment. The impacts graphed in Figure 3 suggest private (social) investment breaks even at 8 (14) years after enrollment begins.

7 Conclusion

In this paper, we estimate the effects of retraining unemployed workers. Linking together high quality administrative records, we follow the population of Ohio UI claimants between 2004 and 2011 who enroll in public postsecondary institutions. Adopting a matching method that bridges two strands of the dynamic treatment effect literature, we find that enrollees experience a “lock-in” effect of depressed earnings immediately after enrolling but see an average earnings gain of \$348 per quarter (about six percent) over the third

⁴⁰For example, sending many more unemployed claimants back to school to prepare for a new career in healthcare will shift the supply of new healthcare workers, which will in turn depress wages and crowd out other workers (McCall, Smith and Wunsch, 2016 refer to these as relative skill price and displacement effects, respectively). The general equilibrium effect could also be positive—as a referee suggests, having a more educated workforce may improve health, reduce crime, and strengthen democracy (see Lochner, 2011 for a nice review).

and fourth years post-enrollment. We find that much of the gain is driven by enrollees who take courses and are subsequently employed in health-related fields. A longer-run follow-up of an early subsample suggests that the gains persist and widen over a ten-year period.

With our earnings impact estimates, we conduct a cost-benefit analysis from the private and social perspectives. We find that the out-of-pocket investment from an average worker breaks even 8 years after enrolling whereas the social investment (including both private investment by the worker and government subsidies) breaks even in 14 years. Assuming an average worker stops working around the normal retirement age as in Jacobson, Lalonde and Sullivan (2005*b*), the private benefit is \$6.05 for every dollar of her out-of-pocket investment in schooling, and the social benefit is \$2.44 for every dollar of total educational investment. The implied private and social IRRs are 15 percent and 8 percent, respectively, which are in the range of estimates by Heckman, Lochner and Todd (2006) and Jacobson, Lalonde and Sullivan (2005*b*).

Braxton and Taska (2023) show that technological change that leads to new skill requirements is a major driver of earnings declines after job loss. This suggests that policies that encourage and enable unemployed workers to retrain, including efforts to expand community college access, can therefore be beneficial in the long run. Specifically, policies that target UI recipients to ease their transition to enrollment may be effective based on existing evidence. Barr and Turner (2015) show that UI benefit extensions during the Great Recession induced a significant increase in enrollment among the unemployed. Using the same temporal variation in benefit extensions, we replicate their analysis in Appendix E and find a similar effect within Ohio: for every 10-week increase in UI benefits, there is a ten percent increase in enrollment, or an additional 1,200 enrollees per year. Combined with our estimates of positive private and social returns to enrollment, this indicates that UI policies may have social benefits beyond what is typically considered.

While we find positive returns to enrollment on average, we caution that our cost-benefit analysis abstracts from the uncertainty and heterogeneity in enrollment effects. Indeed, an open question remains as to whether gains vary across training types. Although we find that the retraining effects are driven by course-taking and subsequent employment in healthcare and construction, evidence that can guide policymakers and unemployed workers on the most effective type of training is a fruitful avenue for future research.

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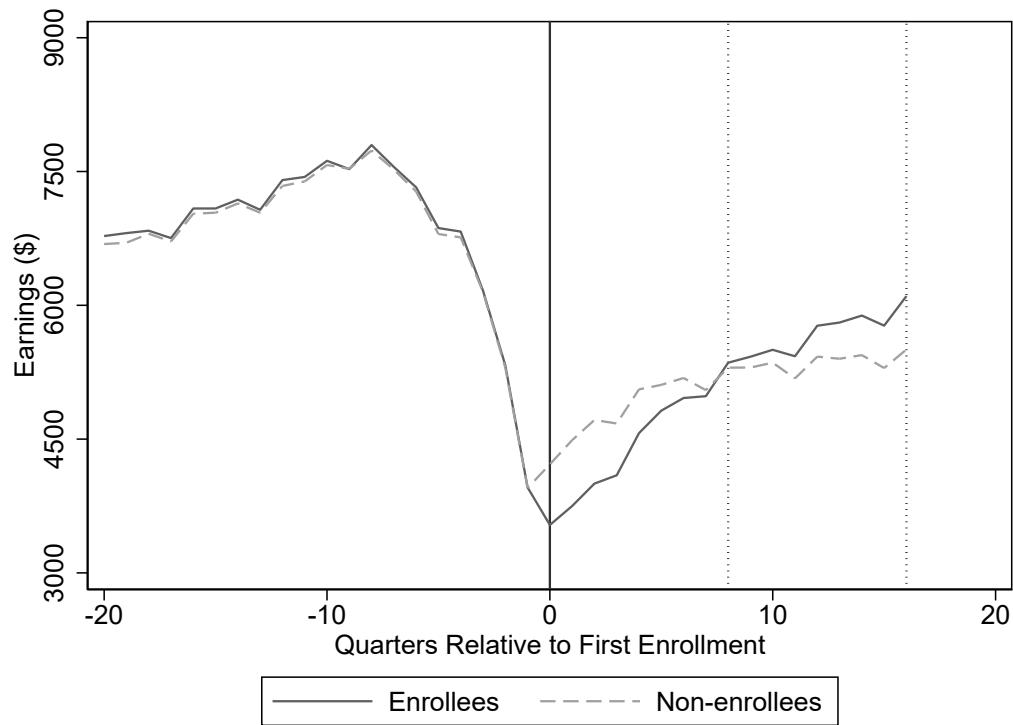
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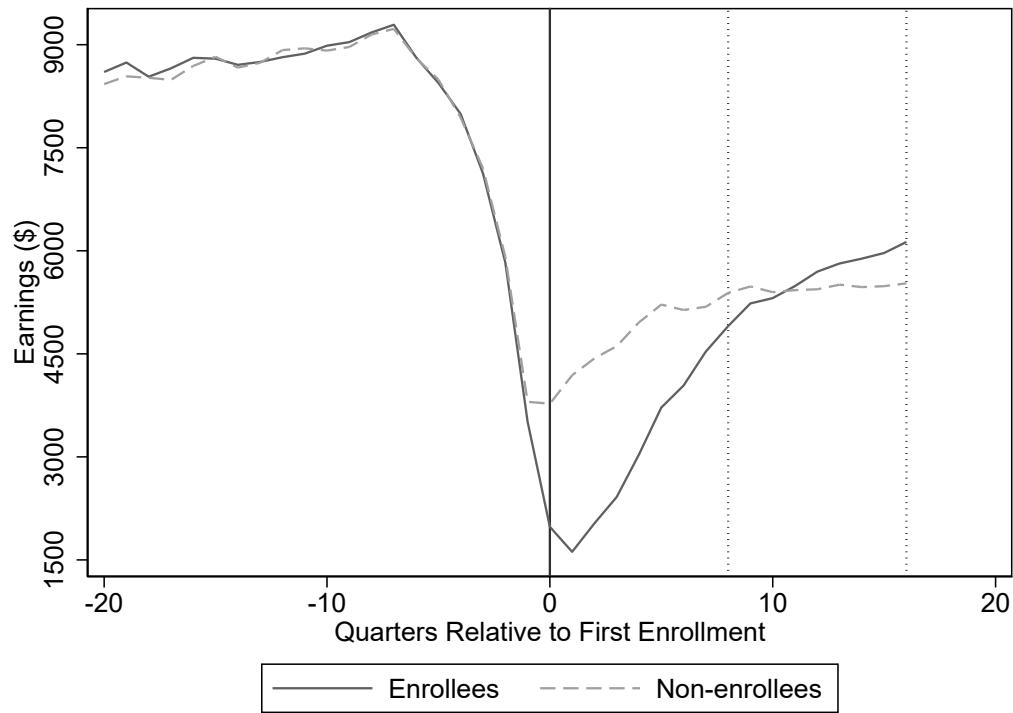
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Figure 1: Earnings of Enrollees and Matched Non-enrollees



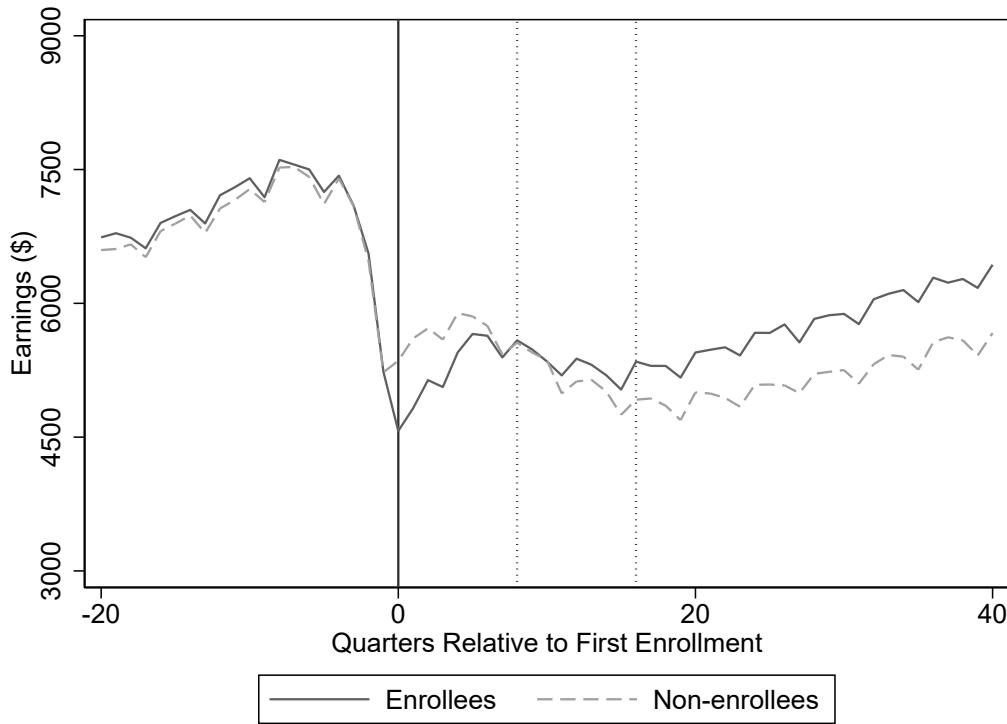
Notes: This figure plots the average quarterly earnings of enrollee and matched non-enrollee UI claimants. The solid vertical line denotes the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. $N = 141,758$, corresponding to 136,074 unique individuals.

Figure 2: Earnings of Enrollees Who Participated in WIA and Matched Non-enrollees



Notes: This graph shows the average quarterly earnings of enrollees who received WIA training services, and their matched non-enrollees. The solid vertical line denotes the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. $N = 16,624$ UI claims, corresponding to 16,487 unique individuals.

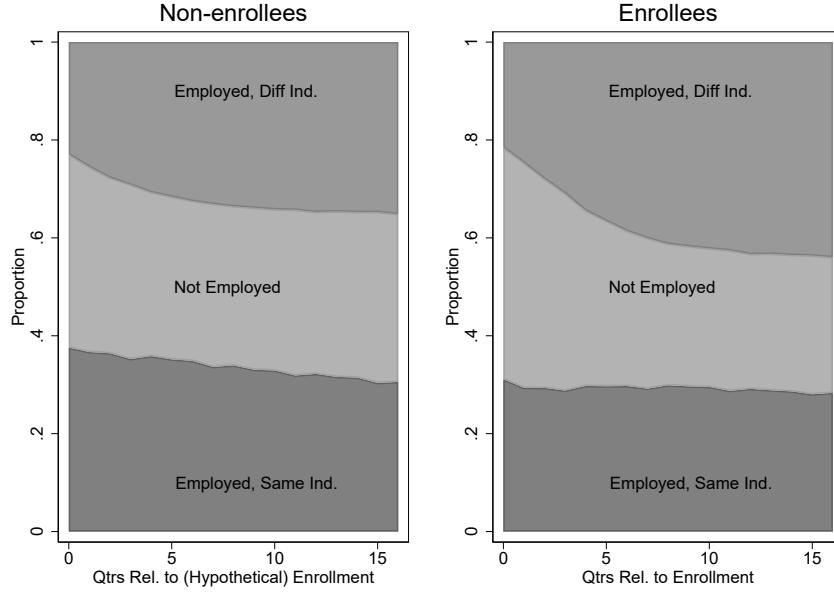
Figure 3: Long-run Earnings of Enrollees and Matched Non-enrollees



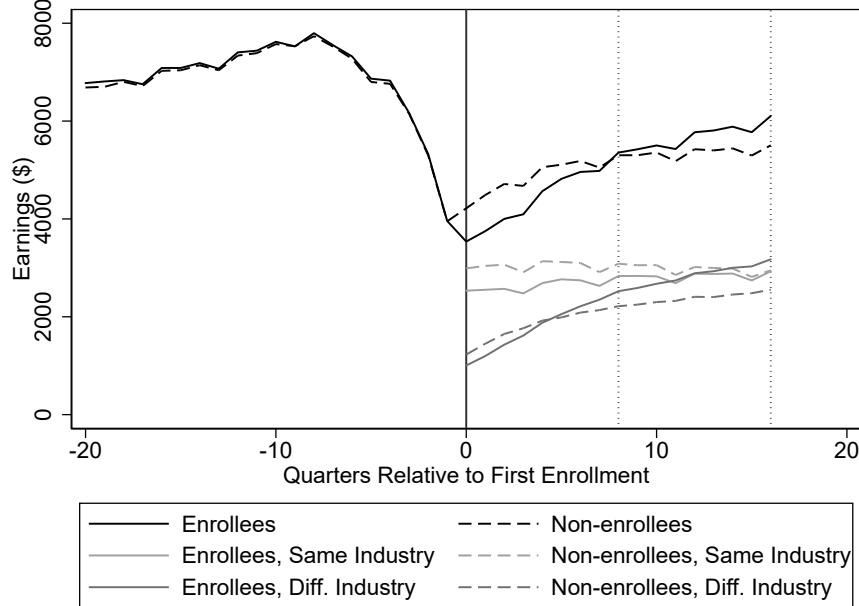
Notes: This graph shows the average quarterly earnings of UI claimants who enrolled from 2004 through the third quarter of 2007 and their matched non-enrollees. The solid vertical line denotes the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. $N = 31,666$ UI claims, corresponding to 30,973 unique individuals.

Figure 4: Industry Switching Among Enrollees and Matched Non-enrollees

(A) Probability of Switching Industries Over Time



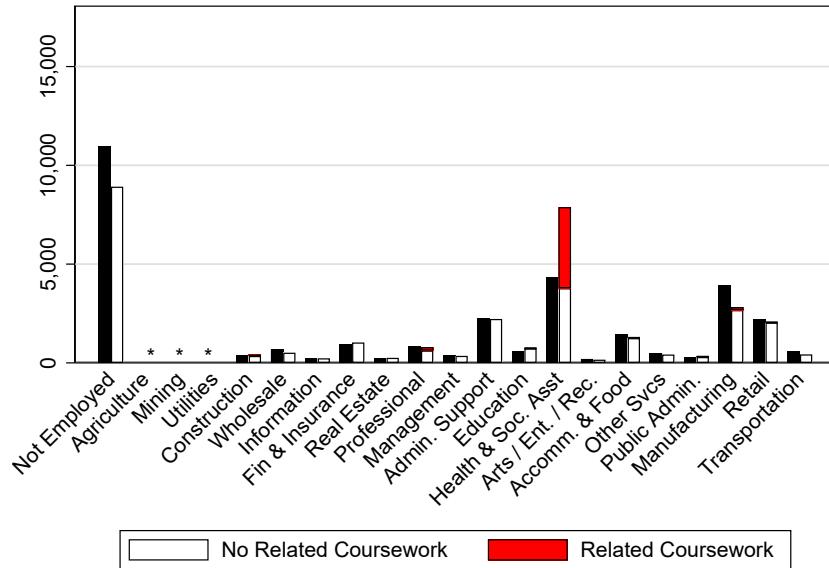
(B) Decomposition of Earnings: the Role of Industry Switching



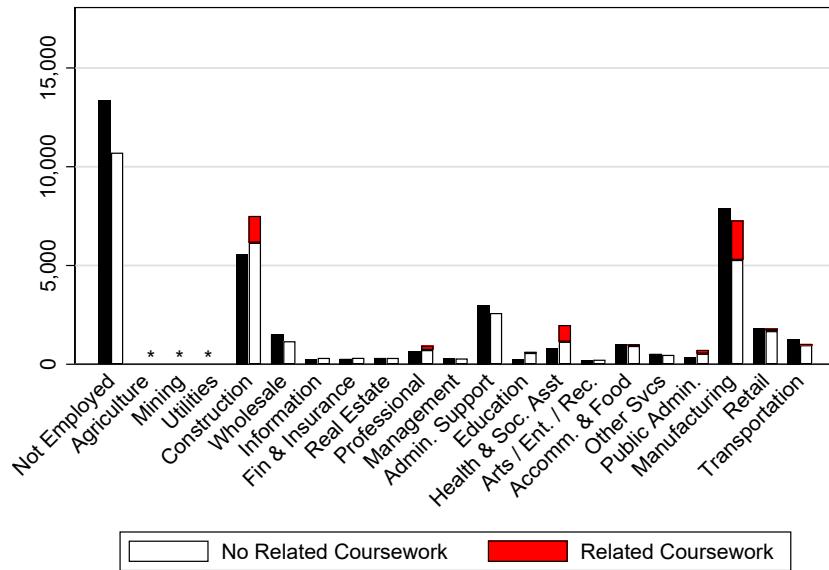
Notes: The figures in Panel A plot the probability of employment in the same (pre-layoff) two-digit industry, employment in a different two-digit industry, and non-employment over time for enrollees and matched non-enrollee UI claimants. Panel B plots the average quarterly earnings of enrollee and matched non-enrollee UI claimants (black solid and dashed lines). The gray lines disaggregate the post-enrollment earnings into two components: average quarterly earnings from the pre-layoff (“same”) industry and from a different industry, each scaled by the probability of employment in either the same or different industries. The solid (dashed) gray lines sum up to the solid (dashed) black lines. $N = 141,758$, corresponding to 136,074 unique individuals.

Figure 5: Industry-Related Course-Taking, by Industry of Employment at 16th Quarter Post-Enrollment

(A) Women

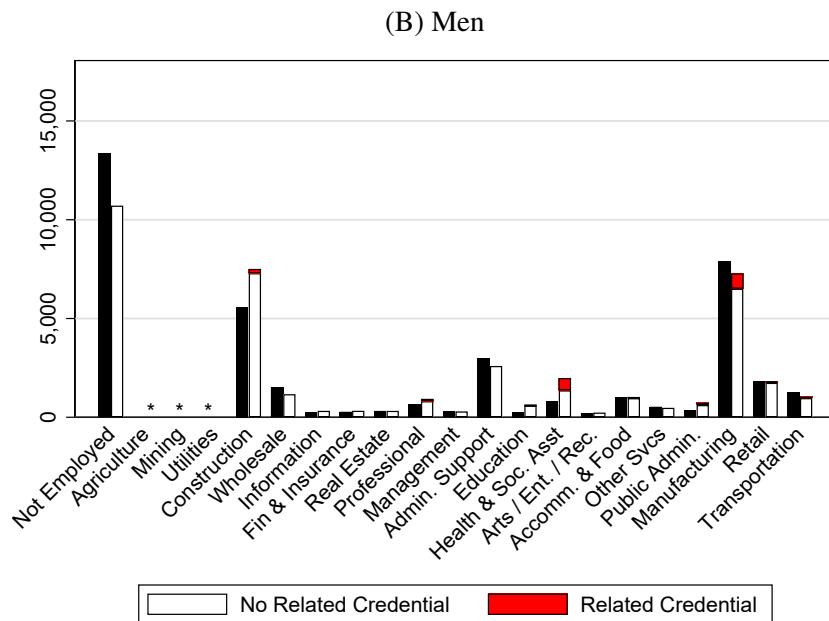
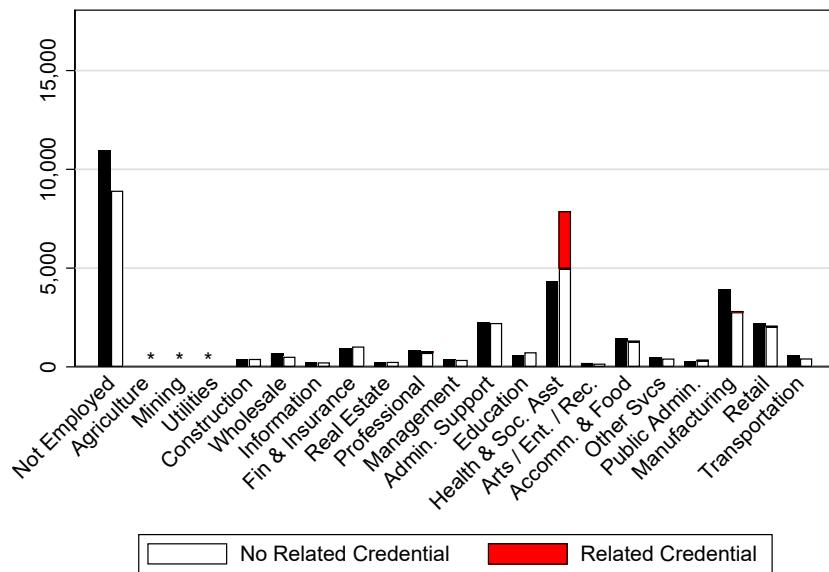


(B) Men



Notes: In each plot, the right side of each pair of bars shows the number of enrollees who take or do not take sector-related coursework. Courses are defined as sector-related if the course prepares a worker for an occupation where a large proportion of the occupation is concentrated in one industry (see text for details). The left bar shows the number of matched non-enrollees in each sector. Agriculture, Mining, and Utilities sectors have fewer than 200 workers in each enrollee/non-enrollee cell and are not plotted. If there are 10 or fewer enrollees in a sector who take sector-related coursework, it is not plotted.

Figure 6: Industry-Related Credential Receipt, by Industry of Employment at 16th Quarter Post-Enrollment
 (A) Women



Notes: In each plot, the right side of each pair of bars shows the number of enrollees who obtain or do not obtain sector-related credentials. Credentials are defined as sector-related if the credential prepares a worker for an occupation where a large proportion of the occupation is concentrated in one industry (see text for details). The left bar shows the number of matched non-enrollees in each sector. Agriculture, Mining, and Utilities sectors have fewer than 200 workers in each enrollee/non-enrollee cell and are not plotted. If there are 10 or fewer enrollees in a sector who have sector-related credentials, it is not plotted.

Table 1: (A) Descriptive Characteristics of Enrollees and Non-enrollees

	Enrollees		Non-enrollees		Norm. Diff.
	Mean	SD	Mean	SD	
Female	0.44	0.50	0.34	0.47	0.22
Race					
White	0.75	0.43	0.81	0.40	-0.13
Black	0.18	0.38	0.13	0.33	0.14
Other	0.02	0.15	0.02	0.15	0.00
Unknown	0.05	0.22	0.05	0.21	0.02
Prior Industry					
Manufacturing	0.29	0.46	0.32	0.47	-0.06
Construction	0.16	0.37	0.16	0.37	0.00
Admin. Support & Waste	0.12	0.32	0.11	0.31	0.02
Healthcare and Social Assistance	0.09	0.28	0.06	0.23	0.11
Retail Trade	0.08	0.27	0.07	0.26	0.02
Accommodation and Food Services	0.04	0.20	0.04	0.19	0.02
Wholesale Trade	0.04	0.19	0.04	0.20	-0.03
Transportation	0.03	0.18	0.05	0.21	-0.05
Tenure at Recent Employer					
<=1 year	0.35	0.48	0.27	0.45	0.16
>1 years to <=3 years	0.28	0.45	0.26	0.44	0.06
>3 years to <=6 years	0.16	0.37	0.18	0.38	-0.04
>6 years	0.21	0.41	0.29	0.45	-0.19
Age	35.75	10.83	42.32	11.81	-0.58
Cty Unempl. Rate at Layoff (%)	8.24	2.69	7.87	2.65	0.14
Earnings					
1 year before layoff	32476	21009	36801	26949	-0.18
2 years before layoff	29869	22449	35030	28315	-0.20
3 years before layoff	28488	23563	34088	28720	-0.21
Observations	71,745		1,793,437		

Notes: “Enrollees” (“Non-enrollees”) are UI claimants who enroll (do not enroll) in a public post-secondary institution in Ohio within two years of filing a UI claim. “SD” denotes standard deviation. “Norm. Diff.” is the normalized difference defined in Section 4.4. All earnings are expressed in 2012 dollars.

Table 1: (B) Descriptive Characteristics of Enrollees and Matched Non-enrollees

	Matched					
	Enrollees		Non-enrollees		Norm. Diff.	t-stat
	Mean	SD	Mean	SD		
Female	0.44	0.50	0.44	0.50	-	-
Race						
White	0.75	0.43	0.75	0.43	0.00	0.04
Black	0.18	0.38	0.18	0.38	0.00	-0.59
Other	0.02	0.15	0.02	0.15	0.00	0.23
Unknown	0.05	0.22	0.05	0.22	0.00	0.81
Prior Industry						
Manufacturing	0.29	0.45	0.29	0.45	-	-
Construction	0.16	0.37	0.16	0.37	0.00	0.45
Admin. Support & Waste	0.12	0.32	0.12	0.32	0.00	0.45
Healthcare and Social Assistance	0.09	0.28	0.09	0.28	0.00	-0.85
Retail Trade	0.08	0.27	0.08	0.27	0.00	0.79
Accommodation and Food Services	0.04	0.20	0.04	0.20	0.00	0.28
Wholesale Trade	0.04	0.19	0.04	0.19	0.00	-0.46
Transportation	0.04	0.18	0.04	0.19	0.00	-0.62
Tenure at Recent Employer						
<=1 year	0.35	0.48	0.34	0.47	0.01	0.94
>1 years to <=3 years	0.28	0.45	0.29	0.45	-0.01	-1.56
>3 years to <=6 years	0.16	0.37	0.16	0.36	0.01	1.73
>6 years	0.21	0.41	0.21	0.41	0.00	-0.93
Age	35.75	10.82	35.70	10.83	0.00	0.82
Cty Unempl. Rate at Layoff (%)	8.25	2.69	8.25	2.72	0.00	-0.45
Earnings						
1 year before layoff	32491	20981	32329	26612	0.01	1.28
2 years before layoff	29882	22436	29748	27984	0.01	0.99
3 years before layoff	28499	23566	28338	28270	0.01	1.16
Observations	70,879		70,879			

Notes: “Enrollees” are UI claimants who enroll in a public post-secondary institution in Ohio within two years of filing a UI claim, and “Matched Non-enrollees” are their matched comparison group. “SD” denotes standard deviation. “Norm. Diff.” is the normalized difference defined in Section 4.4. “t-stat” is the t-statistic corresponding to the difference in means between enrollees and matched non-enrollees.

Table 2: Enrollment Characteristics

Time from job loss to enrollment (quarters)	3.7
Terms/Quarters Enrolled	4.5
Type of Institution Attended (%)	
Technical Center	15.3
Community College	87.2
Types of Courses	
Taken at least one occupational course (%)	89.8
Avg proportion of courses occupational	0.6
Credential (%)	
Graduate / Professional	0.1
Bachelors	0.6
Associate	10.3
Less Than Associate	14.8
Observations	70,879

Notes: Type of Institution Attended, Terms/Quarters Enrolled, Types of Courses, and Credential are calculated within four years of first enrollment. Enrollees may attend more than one type of institution over the four-year period. “Less than Associate” credentials include less than two-year awards from HEI and any credential from OTC.

Table 3: Enrollment Effect Estimates

	TOT Estimates (1)	Matched Non-enrollee Mean (2)
Post-Enrollment Quarterly Earnings		
1-2 Yrs Post-Enrollment	-381.22 (20.68)	4947.45
3-4 Yrs Post-Enrollment	348.11 (23.32)	5363.62
Pre-Layoff Quarterly Earnings		
1 Year Before Layoff	40.66 (23.63)	8082.16
2 Years Before Layoff	33.36 (24.59)	7437.06
Observations	141,758	70,879

Notes: Column (1) shows the estimated effect of enrollment on earnings in the time period denoted by the row headings. Column (2) shows the mean earnings in the matched non-enrollee sample. Abadie and Imbens (2016) standard errors are in parentheses.

Table 4: Enrollment Effect Estimates By Subgroup

Subgroup	Quarterly Earnings, 1-2 Yrs Post-Enrollment				Quarterly Earnings, 3-4 Yrs Post-Enrollment				No. of Enrollees (11)
	Simple Diff. TOT Estimate (1)	Diff-Diff TOT Estimate (2)	Nonent. Mean (3)	Pct. Ch. DD (4)	Simple Diff. TOT Estimate (6)	Diff-Diff TOT Estimate (7)	Nonent. Mean (8)	Pct. Ch. DD (9)	
	4947	-421 (25)	-8%	-9%	348 (29)	306 (33)	5364	6%	
Quarters From Layoff to Enrollment									
1	-942 (64)	-930 (68)	5567	-17%	17 (71)	5 (79)	5883	0%	0% 11934
2	-657 (57)	-604 (61)	5080	-13%	210 (69)	254 (77)	5561	4%	5% 13563
3	-330 (60)	-389 (63)	4706	-7%	386 (67)	265 (80)	5249	7%	5% 11736
4	-274 (67)	-388 (66)	4842	-6%	437 (75)	336 (89)	5230	8%	6% 9488
5	-134 (79)	-190 (70)	4730	-3%	540 (87)	488 (99)	5100	11%	10% 7595
6	12 (82)	-191 (68)	4781	0%	533 (90)	368 (102)	5169	10%	7% 6577
7	53 (92)	58 (76)	4628	1%	564 (105)	604 (121)	5026	11%	12% 5516
8	57 (109)	11 (90)	4759	1%	501 (115)	537 (127)	5112	10%	10% 4470
Layoff Year									
2004	-421 (84)	-433 (78)	5667	-7%	-8%	205 (90)	105 (100)	5345	4% 6017
2005	-40 (104)	-208 (87)	5902	-1%	-4%	388 (101)	244 (117)	4985	8% 4632
2006	-311 (83)	-447 (82)	5270	-6%	-8%	127 (88)	-42 (101)	4926	3% -1% 6747
2007	-568 (73)	-573 (73)	4562	-12%	-13%	104 (82)	75 (98)	4871	2% 2% 7480
2008	-587 (57)	-590 (57)	4519	-13%	-13%	385 (66)	369 (75)	5251	7% 7% 13897
2009	-475 (53)	-469 (53)	4896	-10%	-10%	347 (60)	392 (69)	5754	6% 7% 17108
2010	-184 (73)	-244 (69)	4913	-4%	-5%	470 (85)	438 (93)	5665	8% 8% 9623
2011	20 (88)	-79 (89)	4781	0%	-2%	782 (103)	668 (117)	5458	14% 12% 5375
Male	-153 (38)	-244 (36)	5778	-3%	-4%	595 (42)	501 (48)	6229	10% 8% 39831
Female	-675 (31)	-649 (33)	3882	-17%	-17%	31 (36)	55 (42)	4253	1% 1% 31048
Manufacturing	-993 (52)	-1073 (53)	5888	-17%	-18%	137 (56)	25 (65)	6213	2% 0% 20626
Non-manuf.	-130 (29)	-154 (27)	4561	-3%	-3%	435 (33)	421 (38)	5015	9% 8% 50253
Age <40	-382 (28)	-449 (25)	4664	-8%	-10%	334 (32)	220 (34)	5186	6% 4% 46356
Age >=40	-373 (49)	-402 (49)	5444	-7%	-7%	346 (55)	431 (63)	5691	6% 8% 25364
Tenure									
<=1 Year	-124 (37)	-213 (35)	4122	-3%	-5%	455 (42)	395 (48)	4505	10% 9% 24777
1-6 Years	-574 (37)	-507 (35)	4964	-12%	-10%	252 (42)	285 (46)	5424	5% 5% 31951
>6 Years	-406 (66)	-678 (65)	6265	-6%	-11%	468 (72)	220 (78)	6521	7% 3% 14992
White	-435 (30)	-470 (29)	5250	-8%	-9%	383 (34)	350 (39)	5682	7% 6% 53905
Black	-171 (48)	-226 (48)	3713	-5%	-6%	255 (53)	62 (62)	4052	6% 2% 12646

Notes: Columns (1) and (2) show the estimated effect of enrollment on quarterly earnings in the two years after first enrollment, computed by taking the difference between enrollees and matched non-enrollees, and by matched difference-in-differences, respectively, for each subgroup denoted by the row headings. Columns (6) and (7) show the analogous results for the third and fourth years after enrolling. Columns (3) and (8) show the mean quarterly earnings of the matched non-enrollees. Columns (4), (5), (9), and (10) express the columns (1), (2), (6), and (7) as percentages of the matched non-enrollee mean. Standard errors for the mean pairwise difference between enrollees and matched-non-enrollees are reported in parentheses.

Appendix (For Online Publication Only)

A Data Sources and Sample Construction: Additional Details

A.1 Construction of Analysis Sample and Variables

Our unemployment insurance (UI) claims sample consists of eligible, regular UI claims (i.e., claims that are filed when a worker is first unemployed). To create the sample, we first construct UI spells by grouping together regular claims and all associated extension claims (e.g., benefits under the Extended Benefit, Emergency Unemployment Compensation, and Trade Readjustment Allowance programs), which have the same benefit year beginning date. We then check for cases where an individual has two overlapping UI spells (i.e., one spell begins before the previous one ends), which we further group together as part of the same UI spell. These steps help ensure that the beginning of each spell corresponds to when a worker is first laid off, rather than a continuation of an ongoing unemployment spell. We drop observations that have missing gender, age under 16 or over 100, and non-Ohio zip codes. For enrollees in our sample, we only use the UI spell that is closest to (but before) enrollment. Non-enrollees are allowed to have multiple UI spells in the sample.

Several of our key variables come from the quarterly wage database that is part of the Ohio UI system. Our main outcome measures are quarterly earnings. Although the earnings data we receive are topcoded, the censoring points are sufficiently high so as to be not very relevant for our sample, as is evident from the summary statistics of Table 1: prior to 2009, earnings above \$99,999 were censored at \$99,999; for all quarters starting in 2009, the top one percent of earnings have been topcoded to the average of the top one percent. The wage data also contain employer pseudo-IDs and industry, which allow us to construct job tenure measures. Since these employer pseudo-IDs are not in the UI claims data, we first search in the wage data for an employer that matches the industry reported in the UI claims data in the five quarters before layoff, starting with the most recent quarter. Once we have identified the employer that matches the industry in the claims data, we define tenure as the number of quarters since the first time a worker is observed to have worked for the employer in the wage data. We then create three tenure categories: less than one year, one to six years, and more than six years (since we only observe wages starting in 1995Q2, the maximum tenure for the earliest claims in the sample is 8.75 years). If we do not find an employer with a matching industry or if the industry is missing in the claims data, we report the tenure at the most recent employer in the wage data. Only a small fraction of the wage data have missing industry (0.5 percent).

Since we only observe earnings within Ohio, it is possible that we are understating the earnings of workers who work out-of-state. To gauge the extent of this issue, we examine the out-migration rates of workers who claim UI in the Survey of Income and Program Participation (SIPP). In the 2004 (2008) SIPP panels, there were 139 (261) individuals who reported receiving UI benefits while residing in Ohio at some point during the four years of the panel. Of these individuals, 8 (4) individuals moved out-of-state, and 3 (2) found jobs in their new destination in the 2004 (2008) panels. Although the samples of UI recipients are small in the SIPP, these rates of out-migration are similar to the overall yearly migration rates for Ohio found using IRS Statistics of Income data (2005-2016) and the American Community Survey (2010-2016), which is about two percent. To get a sense for how out-migration may affect our main estimates, consider the extreme scenario where two percent of the non-enrollee control group have no in-state earnings due to out-migration and that *none* of the enrollee treatment group has migrated. Then replacing the zero earnings of the bottom two percent of the control group with their mean earnings will increase the average control earnings by about \$107 (two percent times \$5,364 as reported in Table 3) three to four years post-enrollment, resulting in a treatment effect estimate of \$241 per quarter. Even in this very conservative scenario, there is a positive enrollment effect of more than four percent.

A.2 Enrollment, Course, and Credential Data

Our enrollment data come from two sources: 1) The Higher Education Information (HEI) data, which cover two- and four- year public colleges, and 2) the Ohio Technical Center (OTC) data, which contain information on technical centers (the OTC data also include training that takes place in correctional facilities and high schools, though these account for less than one percent of our enrollee sample). The HEI data, available from the summer of 1999 to the spring of 2017, are all reported at the person-institution-term level so we can observe enrollment, courses taken, and credentials/degrees obtained in every term for each individual. Terms are “Winter”, “Spring”, “Summer”, and “Autumn”, which we map to the first, second, third, and fourth quarters of the calendar year in our analysis. Starting in 2013, “Winter” terms were eliminated as all public colleges moved to a semester system. The OTC data, spanning years between 2002 and 2017, contain start and end dates for each course, and credentials are often associated with specific courses.

Each course and credential has a Classification of Instructional Programs (CIP) code that denotes the subject area. In the HEI data, 0.3 percent of courses taken by enrollees in our sample are missing CIP codes (none of the credentials are missing CIP codes). In the OTC data, although we see that 10 percent of courses

in our enrollee sample have missing CIP codes, there is another variable containing an internal subject code that we can use to fill in most of the missing CIP codes. That is, for courses that have missing CIP codes but non-missing internal subject codes, we fill in the most common CIP code associated with that internal subject code. This procedure effectively reduces the percentage of OTC courses with missing CIP codes to 0.7 percent. Also, the HEI course information for 2006 appears to be incomplete relative to the enrollment data: 17 percent of the enrollee-by-term observations in 2006 are not associated with courses. This missing data issue only affects results in Section 5.6 and may slightly undercount the number of enrollees that take coursework related to specific industries or areas.

To categorize courses and credentials to specific (two-digit) industries, we create a mapping of CIP codes to industries using the data from the National Center of Education Statistics (NCES) and the Bureau of Labor Statistics (BLS). The NCES provides a list of occupations that each CIP subject prepares for, while the National Employment Matrix (2018) from the BLS has data on the share of workers of a particular occupation in a specific industry (National Center for Education Statistics, 2011; Bureau of Labor Statistics, 2019). To create this mapping, we consider only “occupational” (as opposed to “academic”) CIP codes, as defined by the NCES. We also remove “post-secondary teacher” occupations (SOC 25-1000) because many subjects list educators of that subject as a possible occupation, and these tend to be categorized as post-secondary teachers, most of whom work in the Educational Services sector. Since we believe most workers who train in a particular subject are not aiming to teach in the area, we do not want to false attribute a subject to the education sector. We then order the occupations within CIP subject by the total employment of the occupation according to the National Employment Matrix, and map each CIP code to the most populous occupation. We further map a CIP code to a two-digit NAICS industry if, for the occupation the CIP maps to, more than a quarter of the employment is within that industry. We use the largest industry if there are multiple industries that meet this criterion. We designate a CIP code as not associated with an industry if 1) the largest industry accounts for less than 25 percent of the occupation the CIP maps to, 2) the CIP is not associated with any occupation, or 3) the mapped “industry” is self-employment (i.e., more workers in the mapped occupation are self-employed than working in any particular sector). An example of a course not mapped to an industry is Human Resources Management (CIP 52.1001): although it prepares a worker for the Human Resource Specialist occupation, human resource specialists work in many different industries and cannot be assigned to a specific one.

Courses and credentials in the OTC data are mapped to industries via their CIP code. When a worker

takes more than one course, we use the course with the most course hours (and randomly pick a course with the same largest number of course hours). In the HEI data, where each enrollee typically takes more than one course, we first map each of her courses to an industry and then assign to her coursework the modal industry over all her courses (for this assignment, we require that she take at least three courses in that industry). When we observe a worker earn more than one credential, we map her credentials to the modal (non-missing) industry over all her credentials, and in the case of ties, we keep the industry mapped from the highest credential. When a worker is observed in both OTC and HEI data, and the industries of her coursework do not match, we use the one in the OTC data (because courses in OTC are more easily matched to an industry as there are fewer courses); if the industries of the credential do not match, we use the one in the HEI data (because institutions in the HEI data tend to confer higher credentials).

A.3 Workforce Investment Act Data

To analyze the enrollment effects for workers who trained under the Workforce Investment Act (WIA) program, our analysis sample has been merged with the WIA administrative program data (from the WIA Standardized Record Data system). The data contain quarterly snapshots of WIA participants and exiters between 2006Q1 and 2015Q4. The earliest snapshot (2006Q1) contains participants who exited the program starting in 2004Q1. The analysis sample in Section 5.4 contains only WIA participants who received job training from WIA. We observe the month of WIA registration, the beginning and end months of WIA training, program funding stream (e.g., adult, dislocated worker, or youth), and type of training (e.g., on-the-job, skill upgrading, entrepreneurial skills). We say that an enrollee from our main analysis sample is a trainee in the WIA program if we observe her starting WIA training within 24 months after the UI claim date. We only observe WIA participation for workers in our main analysis sample of UI claims.

B Identification Results: Details, Proofs, and Generalizations

In this section, we provide additional details to Sections 4.1 and 4.2. In Section B.1, we connect Assumption 2 to models of training decisions by Ashenfelter and Card (1985) and Heckman and Robb (1985a). We supply proofs and expand on the partial identification results in Section B.2. Section B.3 generalizes our assumptions and identification results in Section 4.1 from two periods to an arbitrary number of periods, S . In Section B.4, we fill in the details of Section 4.2 on Robins (Section B.4.1), Lechner (Section B.4.2), and

the literature modeling unobserved heterogeneity (Section B.4.3).

B.1 Discussion of Assumption 2

In this section, we discuss Assumption 2 further and show that it is consistent with models of training participation by Ashenfelter and Card (1985) and Heckman and Robb (1985*a,b*) (henceforth AC and HR, respectively). Without loss of generality, we think of \mathbf{X}^s as a vector of past earnings leading up to D_s , for $s = 1, 2$. Specifically, the conditioning set \mathbf{X}^1 in Assumption 2 consists of earnings up to K periods before layoff: $\mathbf{X}^1 = (Y_{-K}, Y_{-(K-1)}, \dots, Y_0)$. While we also include in our matching specification other demographic and labor market indicators, such as gender, race, age, industry, layoff timing, we can think of inequality in Assumption 2 as holding within each cell defined by these variables, so that the inequality also holds when these variables are incorporated into \mathbf{X}^1 .

We now show that, under mild additional conditions, Assumption 2 is consistent with a model of training participation by AC. Since Assumption 2 concerns the training decision among non-enrollees in period 1 ($D_1 = 0$), we focus on this group. The AC model (p. 653) reflects the observation that training participants tend to have lower earnings just before participation than non-participants. In our context, the formal statement of the AC rule governing the selection into training is that $D_2 = 1$ if and only if $Y_{2-k} + v < \bar{y}$, where k is a positive integer, \bar{y} is a constant, and v is a random variable. AC assume v to be completely idiosyncratic such that it is independent of any component of the earnings process; in our setting, this translates to v being independent of the joint distribution of potential outcomes in any period. Since we also allow training to begin in more than one period, a feature not in AC, we need to strengthen the independence of v by incorporating D_1 . This leads to our first additional condition:

$$(D_1, \{Y_t(0)\}_{t \in \mathbb{Z}}) \perp\!\!\!\perp v. \quad (\text{A1})$$

We do not view condition (A1) as substantively more restrictive than the AC condition of $\{Y_t(0)\}_{t \in \mathbb{Z}} \perp\!\!\!\perp v$.

We can think of all workers as drawing v from the same distribution regardless of the value of D_1 , but only those with $D_1 = 0$ will decide the value of D_2 based on their v draw.

Our second additional condition is a strengthened version of Assumption 1(a):

$$\{Y_t(0)\}_{t \in \mathbb{Z}} \perp\!\!\!\perp D_1 | \mathbf{X}^1. \quad (\text{A2})$$

While (A2) requires the conditional independence between D_1 and the joint distribution of potential outcomes across time, it is also not substantively more restrictive than its marginal counterpart of Assumption

1(a). If we believe in the randomized assignment of D_1 conditional on \mathbf{X}^1 , then (A2) ought to be true.

Finally, our third additional condition is no-anticipation:

$$\mathbf{X}^1 = \{Y_{-K}(0), Y_{-(K-1)}(0), \dots, Y_0(0)\}, \text{ and } Y_1 = Y_1(0) \text{ when } D_1 = 0. \quad (\text{A3})$$

We show in Appendix B.4.1 below that (A3) is equivalent to the typical no-anticipation assumption from the dynamic treatment effect literature (see Abbring and Heckman, 2007 for a nice exposition and discussion on the no-anticipation assumption), which relates potential outcomes under different treatment sequences. In both parts of (A3), the potential outcomes for a not-yet enrolled worker are not affected by whether she would pursue training subsequently. As a result, the first part of (A3) says that the baseline covariate vector $\mathbf{X}^1 = (Y_{-K}, Y_{-(K-1)}, \dots, Y_0)$ coincide with the vector of potential outcomes under no-treatment. And the second part says that period-1 outcomes among period-1 non-enrollees does not depend on their potential treatment status in period 2. As we argue below, the no-anticipation assumption is implicitly maintained in the AC and HR frameworks, and we simply make them explicit here. We now state and prove the following Lemma before proceeding to show consistency of Assumption 2 and the AC selection rule.

Lemma 1. *Under Assumptions (A1), (A2), and (A3),*

(a)

$$\{v, \{Y_t(0)\}_{t \in \mathbb{Z}}\} \perp\!\!\!\perp D_1 | \mathbf{X}^1; \quad (\text{A4})$$

(b)

$$Y_t(0) \perp\!\!\!\perp D_1 | Y_j(0) + v, \mathbf{X}^1, \text{ for } t \geq 1 \text{ and } j = 1, 2. \quad (\text{A5})$$

Proof. For part (a), first notice that (A1) implies

$$v \perp\!\!\!\perp D_1 | \{Y_t(0)\}_{t \in \mathbb{Z}} \quad (\text{A6})$$

because

$$\begin{aligned} & \Pr(D_1 = 1 | v \leq v_0, \{Y_t(0)\}_{t \in \mathbb{Z}} \leq \{y_{0t}\}_{t \in \mathbb{Z}}) \\ &= \frac{\Pr(D_1 = 1, \{Y_t(0)\}_{t \in \mathbb{Z}} \leq \{y_{0t}\}_{t \in \mathbb{Z}} | v \leq v_0)}{\Pr(\{Y_t(0)\}_{t \in \mathbb{Z}} \leq \{y_{0t}\}_{t \in \mathbb{Z}} | v \leq v_0)} \\ &= \frac{\Pr(D_1 = 1, \{Y_t(0)\}_{t \in \mathbb{Z}} \leq \{y_{0t}\}_{t \in \mathbb{Z}})}{\Pr(\{Y_t(0)\}_{t \in \mathbb{Z}} \leq \{y_{0t}\}_{t \in \mathbb{Z}})} \\ &= \Pr(D_1 = 1 | \{Y_t(0)\}_{t \in \mathbb{Z}} \leq \{y_{0t}\}_{t \in \mathbb{Z}}) \end{aligned}$$

for any real number v_0 and sequence $\{y_{0t}\}_{t \in \mathbb{Z}}$. By (A3), $\mathbf{X}^1 \in \{Y_t(0)\}_{t \in \mathbb{Z}}$, and therefore

$$\Pr(D_1 = 1 | v, \{Y_t(0)\}_{t \in \mathbb{Z}}, \mathbf{X}^1) = \Pr(D_1 = 1 | \{Y_t(0)\}_{t \in \mathbb{Z}}, \mathbf{X}^1) = \Pr(D_1 = 1 | \mathbf{X}^1),$$

where the last equality follows from the CIA condition (A2). Condition (A4) follows.

For part (b), we first establish that for $t \geq 1$ and $j = 1, 2$,

$$\{Y_t(0), Y_j(0) + v\} \perp\!\!\!\perp D_1 | \mathbf{X}^1. \quad (\text{A7})$$

To see this, note that (A4) implies

$$\{Y_t(0), Y_j(0), v\} \perp\!\!\!\perp D_1 | \mathbf{X}^1,$$

and since v is independent to everything else,

$$\begin{aligned} & \Pr(Y_t(0) \leq y_{0t}, Y_j(0) + v \leq w | D_1, \mathbf{X}^1) \\ &= \int \Pr(Y_t(0) \leq y_{0t}, Y_j(0) \leq w - v_0 | D_1, \mathbf{X}^1) f_v(v_0) dv_0 \\ &= \int \Pr(Y_t(0) \leq y_{0t}, Y_j(0) \leq w - v_0 | \mathbf{X}^1) f_v(v_0) dv_0 \\ &= \Pr(Y_t(0) \leq y_{0t}, Y_j(0) + v \leq w | \mathbf{X}^1), \end{aligned}$$

which proves (A7).¹ Finally, we can follow the steps that proves (A6) to derive condition (A5) from (A7). \square

As indicated by the free parameter k , the AC decision rule is agnostic as to which period's earnings are used to determine D_2 . We show that our Assumption 2 is consistent with the AC model for any value of $k \geq 1$. We consider two cases. The first case is $k \in [2, K+2]$, implying $Y_{2-k} \in \mathbf{X}^1$. It follows that

$$\begin{aligned} & E[Y_t(0) | D_1 = 0, D_2 = 1, \mathbf{X}^1] \\ &= E[Y_t(0) | D_1 = 0, v \leq \bar{y} - Y_{2-k}, \mathbf{X}^1] \\ &= E[Y_t(0) | D_1 = 0, v > \bar{y} - Y_{2-k}, \mathbf{X}^1] \\ &= E[Y_t(0) | D_1 = 0, D_2 = 0, \mathbf{X}^1], \end{aligned}$$

where the second equality is a consequence of Lemma 1(a). In this case, Assumption 2 holds with equality.

The second case of $k \notin [2, K+2]$ is more interesting. In this case, Y_{2-k} is not in the conditioning set \mathbf{X}^1 . We go through the most relevant subcase of $k = 1$ here, and the same argument applies in the cases where $k > K+2$.² Following HR, AC assume that the conditional expectation of potential earnings absent training

¹Consistent with the joint normality assumption below, v is assumed to be continuously distributed with density f_v .

²We include three years of pre-layoff earnings in our propensity score estimation. It is much more likely that the most recent

in any period is linear in the training selection variable, which will be the case if the potential earnings process and v are joint normal. We impose this joint normality in this section. Under these assumptions,

$$\begin{aligned} E[Y_t(0)|D_1 = 0, Y_1 + v, \mathbf{X}^1] \\ = E[Y_t(0)|D_1 = 0, Y_1(0) + v, \mathbf{X}^1] \\ = E[Y_t(0)|Y_1(0) + v, \mathbf{X}^1] \\ = \alpha_t^{AC} + \{Y_1(0) + v\} \beta_t^{AC} + \mathbf{X}^1 \gamma_t^{AC} \end{aligned}$$

where α_t^{AC} , β_t^{AC} , and γ_t^{AC} are coefficients in the linear conditional expectation function, the first equality follows from no-anticipation, and the second equality follows from Lemma 1(b). Since AC parameterize the earnings process, we can further determine the sign of β_t^{AC} . Specifically, in the AC framework, the potential earnings process is:³

$$Y_{it}(0) = \omega_i + \lambda_t + \varepsilon_{it},$$

where ω_i is the individual random effect, λ_t the economy-wide time fixed effect, and ε_{it} the transitory error which is assumed to follow an AR(1) process with coefficient ρ . Defining $\text{var}(\omega_i) \equiv \sigma_\omega^2$, $\text{var}(\varepsilon_{it}) = \sigma_\varepsilon^2$, and $\text{var}(v) = \sigma_v^2$, it turns out that β_t^{AC} has a closed-form expression for any $K \geq 0$ (so that there is at least one variable in \mathbf{X}^1) and for any $t \geq 1$:

$$\beta_t^{AC} = \frac{\rho^{t-1}(1+\rho)(1-\rho^2)\sigma_\varepsilon^4 + (1-\rho^2)\{1+(K+1-K\rho)\rho^{t-1}\}\sigma_\varepsilon^2\sigma_\omega^2}{(1+\rho)(1-\rho^2)\sigma_\varepsilon^4 + (K+2-K\rho)(1-\rho^2)\sigma_\varepsilon^2\sigma_\omega^2 + \{K+1-(K-1)\rho\}\sigma_\omega^2\sigma_v^2 + (1+\rho)\sigma_\varepsilon^2\sigma_v^2}. \quad (\text{A8})$$

For a stationary process with positive serial dependence, ρ lies between zero and one (AC estimate ρ to be between 0.73 and 0.8), in which case $\beta_t^{AC} > 0$.

(and post-layoff) earnings Y_1 determines the training decision than earnings from more than three years ago, as reflected in the discussion of “temporal alignment” by Calónico and Smith (2017).

³Note that the AC formulation implies no-anticipation: the potential outcome $Y_{it}(0)$ is the same regardless of whether the worker would enrollee in period $t+1$, and it is equal to the actual outcome for a worker not yet enrolled as of period t . The same reasoning applies to the HR framework below.

With these results, we can show that Assumption 2 holds:

$$\begin{aligned}
& E[Y_t(0)|D_1 = 0, D_2 = 1, \mathbf{X}^1] \\
&= E[Y_t(0)|D_1 = 0, Y_1 + v < \bar{y}, \mathbf{X}^1] \\
&= E[Y_t(0)|D_1 = 0, Y_1(0) + v < \bar{y}, \mathbf{X}^1] \\
&= E[Y_t(0)|Y_1(0) + v < \bar{y}, \mathbf{X}^1] \\
&\stackrel{(i)}{=} \alpha_t^{\text{AC}} + E[Y_1(0) + v|Y_1(0) + v < \bar{y}] \beta_t^{\text{AC}} + \mathbf{X}^1 \gamma_t^{\text{AC}} \\
&\leq \alpha_t^{\text{AC}} + E[Y_1(0) + v|Y_1(0) + v \geq \bar{y}] \beta_t^{\text{AC}} + \mathbf{X}^1 \gamma_t^{\text{AC}} \\
&= E[Y_t(0)|Y_1(0) + v \geq \bar{y}, \mathbf{X}^1] \\
&= E[Y_t(0)|D_1 = 0, D_2 = 0, \mathbf{X}^1],
\end{aligned}$$

where equality (i) follows from the law of iterated expectations.

The HR training participation rule differs slightly from that by AC. It is derived from an economic model of a risk-neutral worker maximizing expected earnings. Assuming perfect foresight, HR show that a worker decides to train if the present value of the earnings gain exceeds the sum of the direct training cost and the opportunity cost of foregone earnings during training. Adapted to our two-period setting, the HR rule states that among period-1 non-enrollees, $D_2 = 1$ if and only if $Y_2(0) + v < \alpha/r$, where v is the direct cost of training, r the market interest rate, and α the earnings effect of training.⁴ Consistent with subcases in HR, we think of the treatment effect α as constant across the population for simplicity.⁵

Since interest rate r is also a constant, the randomness in training decisions come from $Y_2(0)$ and v . HR's parameterization of the potential earnings process is

$$Y_{it}(0) = \mathbf{W}_{it}\boldsymbol{\varsigma} + U_{it}$$

for individual i in time t , where \mathbf{W} is a vector of observables. In Heckman and Robb (1985b) (p. 181), the error term is further specified as $U_{it} = \omega_i + \varepsilon_{it}$, where ε_{it} follows an AR(1) process. Thus, the HR and AC earnings process coincide when \mathbf{W}_{it} consists of the time fixed effect, a case we focus on here. For v , the simplest assumption we can impose is that it is independent to U_{it} , a case HR consider.⁶ With it, the HR

⁴ v corresponds to $-S_i$ on p. 244 of Heckman and Robb (1985a), where the authors refer to S_i as the training subsidy.

⁵HR start from a case of constant treatment effect; even in the presence of heterogeneous treatment effect, HR consider the case where workers may not know their individual effect *ex ante*, but base their enrollment decision on the population average effect (p. 181, Heckman and Robb, 1985b).

⁶HR potentially allow nonzero correlation between v and $Y_t(0)$ through observables. In the AC framework, this could be modeled as writing v as the sum of a linear function of the individual fixed effect ω plus an error term that is independent to everything else. By following similar arguments as in the rest of this section, we can show that Assumption 2 holds if v is

decision rule simply replaces Y_{2-k} in the AC decision rule with $Y_2(0)$. Further imposing condition (A1) and joint normality, we can follow the reasoning above to derive Assumption 2:

$$\begin{aligned}
& E[Y_t(0)|D_1 = 0, D_2 = 1, \mathbf{X}^1] \\
&= \alpha_t^{\text{HR}} + E[Y_2(0) + v|Y_2(0) + v < \alpha/r] \beta_t^{\text{HR}} + \mathbf{X}^1 \gamma_t^{\text{HR}} \\
&\leq \alpha_t^{\text{HR}} + E[Y_2(0) + v|Y_2(0) + v \geq \alpha/r] \beta_t^{\text{HR}} + \mathbf{X}^1 \gamma_t^{\text{HR}} \\
&= E[Y_t(0)|D_1 = 0, D_2 = 0, \mathbf{X}^1].
\end{aligned} \tag{A9}$$

α_t^{HR} , β_t^{HR} , and γ_t^{HR} are coefficients in the linear conditional expectation function of $Y_t(0)$ in terms of $Y_2(0) + v$ and \mathbf{X}^1 , where the closed-form expression for β_t^{HR} is:

$$\beta_t^{\text{HR}} = \begin{cases} \frac{(1-\rho^2)(1+\rho)\rho\sigma_e^4 + (1-\rho^2)\{1+(K+1-K\rho)\rho\}\sigma_e^2\sigma_\omega^2}{(1+\rho)(1-\rho^4)\sigma_e^4 + \{K+2-(K-2)\rho+K\rho^2(1-\rho)\}(1-\rho^2)\sigma_e^2\sigma_\omega^2 + \{K+1-(K-1)\rho\}\sigma_\omega^2\sigma_v^2 + (1+\rho)\sigma_e^2\sigma_v^2} & \text{if } t = 1 \\ \frac{\rho^{t-2}(1+\rho)(1-\rho^4)\sigma_e^4 + (1-\rho^2)\{1+\rho + \{K+1-(K-1)\rho+K\rho^2(1-\rho)\}\rho^{t-2}\}\sigma_e^2\sigma_\omega^2}{(1+\rho)(1-\rho^4)\sigma_e^4 + \{K+2-(K-2)\rho+K\rho^2(1-\rho)\}(1-\rho^2)\sigma_e^2\sigma_\omega^2 + \{K+1-(K-1)\rho\}\sigma_\omega^2\sigma_v^2 + (1+\rho)\sigma_e^2\sigma_v^2} & \text{if } t \geq 2 \end{cases} \tag{A10}$$

for any $K \geq 0$, and inequality (A9) holds because $\beta_t^{\text{HR}} > 0$ when $0 < \rho < 1$.

Finally, we note that under both the AC and HR frameworks above,

$$E[Y_1|D_1 = 0, D_2 = 1, \mathbf{X}^1] \leq E[Y_1|D_1 = 0, D_2 = 0, \mathbf{X}^1]. \tag{A11}$$

Inequality (A11) amounts to an empirical test of Assumption 2. It states that among period-1 non-enrollees with the same \mathbf{X}^1 , those who enroll in period 2 have lower average earnings in period 1.

We can generalize the two-period results here to an arbitrary number of periods, S . In particular, the testable implication in the S -period case is: Comparing workers who have similar X^s ($1 < s \leq S$), those who enroll l periods later ($1 \leq l \leq S-s$) have lower average earnings than their never-enrolled counterparts in all l interim periods. We conduct these tests in Section 5.1 and show evidence consistent with the training participation rules by Ashenfelter and Card (1985) and Heckman and Robb (1985a,b) as well as the S -period generalization of Assumption 2, which we state as Assumption 4 in Appendix B.3 below.

B.2 Details on Partial Identification Results

Construction of Upper Bounds via Propensity Score Matching

Proposition 2 below constructs the upper bounds for the TOT parameters via propensity score matching:

positively correlated with potential earnings. This will be the case, for example, if high ability individuals have higher potential earnings and incur lower training cost. Training cost could be lower for them because training takes less time or because they are better at finding training subsidies.

Proposition 2. Under Assumption 1 and provided that $p_1(\mathbf{X}^1), p_2(\mathbf{X}^2) < 1$ and $Y_t(0) \geq 0$ for $t \geq 1$,

(a): For $t \geq 1$,

$$\begin{aligned} & E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, p_1(\mathbf{X}^1)]\Pr(D_2 = 0|D_1 = 0, p_1(\mathbf{X}^1))|D_1 = 1] \\ & \geq E[Y_t(1) - Y_t(0)|D_1 = 1]; \end{aligned} \quad (\text{A12})$$

(b): For $t \geq 2$,

$$\begin{aligned} & \sum_{s=1}^2 \{E[Y_t|D_s = 1] - E[E[Y_t|D = 0, p_s(\mathbf{X}^s)]\Pr(D_2 = 0|D_s = 0, p_s(\mathbf{X}^s))|D_s = 1]\}\Pr(D^s = 1|D = 1) \\ & \geq E[Y_t(1) - Y_t(0)|D = 1]. \end{aligned}$$

Proposition 2(a) bounds δ_{1t} from above. Proposition 2(b) aggregates the upper bound on δ_{1t} from part (a) and the δ_{2t} from equation (6) to arrive an upper bound on the overall TOT parameter δ_t .

Proof of Proposition 1

We first state and prove two Lemmas. Lemma 2 has a similar form to part (a) of Proposition 1, except the estimand conditions on the covariates directly, as opposed to the propensity scores:

Lemma 2. Under Assumptions 1 and 2 and provided that $p_1(\mathbf{X}^1), p_2(\mathbf{X}^2) < 1$,

$$E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, \mathbf{X}^1]|D_1 = 1] \leq E[Y_t(1) - Y_t(0)|D_1 = 1] \quad (\text{A13})$$

$$E[Y_t|D_2 = 1] - E[E[Y_t|D = 0, \mathbf{X}^2]|D_2 = 1] = E[Y_t(1) - Y_t(0)|D_2 = 1]. \quad (\text{A14})$$

Proof. Equation (A14) directly follows from Assumption 1 and the overlap condition $p_2(\mathbf{X}^2) < 1$, and the proof is similar to that of the static identification result (2).

To prove equation (A13), first notice that

$$\begin{aligned} & E[Y_t(0)|D_1 = 1, \mathbf{X}^1] \\ & \stackrel{(1)}{=} E[Y_t(0)|D_1 = 0, \mathbf{X}^1] \\ & = \sum_{d_2=0,1} E[Y_t(0)|D_1 = 0, D_2 = d_2, \mathbf{X}^1] \Pr(D_2 = d_2|D_1 = 0, \mathbf{X}^1) \\ & \leq E[Y_t(0)|D_1 = 0, D_2 = 0, \mathbf{X}^1] \\ & = E[Y_t(0)|D = 0, \mathbf{X}^1] = E[Y_t|D = 0, \mathbf{X}^1], \end{aligned} \quad (\text{A15})$$

where equality (i) holds because of Assumption 1. Our desired result follows:

$$\begin{aligned}
& E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, \mathbf{X}^1]|D_1 = 1] \\
& \leq E[Y_t(1)|D_1 = 1] - E[E[Y_t(0)|D_1 = 1, \mathbf{X}^1]|D_1 = 1] \\
& = E[Y_t(1) - Y_t(0)|D_1 = 1].
\end{aligned}$$

□

Lemma 3 is a variant of a general property of the propensity score—conditional on the propensity score, any function of covariate inputs of the propensity score is balanced across treatment and control.

Lemma 3. *Let $g(\mathbf{X}^1)$ be a function of \mathbf{X}^1 ,*

$$g(\mathbf{X}^1) \perp\!\!\!\perp D_1 | D_2 = 0, p_1(\mathbf{X}^1).$$

Proof. Note that

$$\begin{aligned}
& \Pr(D_1 = 1 | g(\mathbf{X}^1), D_2 = 0, p_1(\mathbf{X}^1)) \\
& = E[D_1 | g(\mathbf{X}^1), D_2 = 0, p_1(\mathbf{X}^1)] \\
& = E[E[D_1 | \mathbf{X}^1, D_2 = 0] | g(\mathbf{X}^1), D_2 = 0, p_1(\mathbf{X}^1)] \\
& = E[p^1(\mathbf{X}^1) | g(\mathbf{X}^1), D_2 = 0, p_1(\mathbf{X}^1)] \\
& = p_1(\mathbf{X}^1),
\end{aligned}$$

and by similar reasoning

$$\Pr(D_1 = 1 | D_2 = 0, p_1(\mathbf{X}^1)) = p_1(\mathbf{X}^1).$$

The independence result follows. □

Now we prove Proposition 1.

Proof. The proof of equation (6) is analogous to that of the static propensity score matching result per Rosenbaum and Rubin (1983), so we omit it here.

To prove inequality (5), we start with the propensity score analog of equation (A15) in the proof of

Lemma 2

$$\begin{aligned}
& E[Y_t(0)|D_1 = 1, p_1(\mathbf{X}^1)] \\
& \stackrel{(i)}{=} E[E[Y_t(0)|D_1 = 1, \mathbf{X}^1]|D_1 = 1, p_1(\mathbf{X}^1)] \\
& \stackrel{(ii)}{\leq} E[E[Y_t(0)|D = 0, \mathbf{X}^1]|D_1 = 1, p_1(\mathbf{X}^1)] \\
& \stackrel{(iii)}{=} E[E[Y_t(0)|D = 0, \mathbf{X}^1]|D_1 = 1, D_2 = 0, p_1(\mathbf{X}^1)] \\
& \stackrel{(iv)}{=} E[E[Y_t(0)|D = 0, \mathbf{X}^1]|D_1 = 0, D_2 = 0, p_1(\mathbf{X}^1)] \\
& \stackrel{(v)}{=} E[Y_t|D = 0, p_1(\mathbf{X}^1)],
\end{aligned}$$

where equalities (i) and (v) follow the law of iterated expectations, inequality (ii) follows (A15), equality (iii) follows the fact that $D_1 = 1$ implies $D_2 = 0$, and equality (iv) follows Lemma 3 and the observation that the inner conditional expectation is just a function of \mathbf{X}^1 . The lower bound result in (5) easily follows:

$$\begin{aligned}
& E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, p_1(\mathbf{X}^1)]|D_1 = 1] \\
& \leq E[Y_t(1)|D_1 = 1] - E[E[Y_t(0)|D_1 = 1, p_1(\mathbf{X}^1)]|D_1 = 1] \\
& = E[Y_t(1) - Y_t(0)|D_1 = 1].
\end{aligned}$$

The lower bound result for the overall TOT in Part (b) of Proposition 1 is a simple consequence of part (a). Aggregating (5) and (6) with weights equal to the respective share of workers trained in period 1 and period 2 delivers the desired result. \square

Proof of Proposition 2

Proof. For part (a), notice that under Assumption 1 and provided $Y_t(0) \geq 0$,

$$\begin{aligned}
& E[Y_t(0)|D_1 = 1, p_1(\mathbf{X}^1)] \\
& = E[Y_t(0)|D_1 = 0, p_1(\mathbf{X}^1)] \\
& = \sum_{d_2=0,1} E[Y_t(0)|D_1 = 0, D_2 = d_2, p_1(\mathbf{X}^1)] \Pr(D_2 = d_2|D_1 = 0, p_1(\mathbf{X}^1)) \\
& \geq E[Y_t(0)|D = 0, p_1(\mathbf{X}^1)] \Pr(D_2 = 0|D_1 = 0, p_1(\mathbf{X}^1)).
\end{aligned}$$

Our desired result follows:

$$\begin{aligned}
& E[Y_t(1) - Y_t(0)|D_1 = 1] \\
& \leq E[Y_t(1)|D_1 = 1] - E[E[Y_t(0)|D = 0, p_1(\mathbf{X}^1)] \Pr(D_2 = 0|D_1 = 0, p_1(\mathbf{X}^1))|D_1 = 1] \\
& = E[Y_t|D_1 = 1] - E[E[Y_t|D = 0, p_1(\mathbf{X}^1)] \Pr(D_2 = 0|D_1 = 0, p_1(\mathbf{X}^1))|D_1 = 1].
\end{aligned}$$

For part (b), noting that $\Pr(D_2 = 0|D_2 = 0, P_2(\mathbf{X}^2)) = 1$, the period 2 quantity in the summand is simply the left hand side of equation (6). The result follows from aggregating inequality (A12) and equation (6) using weights equal to the respective share of workers trained in the two periods. \square

Estimating the Bounds

As mentioned in Section 4.1, the identification results of Proposition 1(a) lead to standard propensity score matching estimators of the lower bound for δ_{1t} and the value of δ_{2t} . We denote these estimators by $\hat{\delta}_{1t}^{\text{lb}}$ and $\hat{\delta}_{2t}$, respectively, whose asymptotic variances are v_{1t}^{lb} and v_{2t} , respectively. Following Abadie and Imbens (2016), the distributions of $\hat{\delta}_{1t}^{\text{lb}}$ and $\hat{\delta}_{2t}$ are asymptotically normal, v_{1t}^{lb} and v_{2t} can be consistently estimated, and inference can be conducted accordingly. Based on Proposition 1(b), the natural lower bound estimator, $\hat{\delta}_t^{\text{lb}}$, for the overall TOT parameter is simply $\hat{\delta}_t^{\text{lb}} = \pi_1 \hat{\delta}_{1t}^{\text{lb}} + \pi_2 \hat{\delta}_{2t}$, where π_1 and π_2 are the respective shares of workers beginning enrollment in period 1 and 2. For inference, we take the recommendation from Imbens and Rubin (2015) (p. 441) and treat π_1 and π_2 as non-random, in which case the variance of the asymptotically normally distributed $\hat{\delta}_{1t}^{\text{lb}}$ is $\pi_1^2 v_{1t}^{\text{lb}} + \pi_2^2 v_{2t}$ in large samples.

Based on Proposition 2, we can also estimate the upper bound for δ_{1t} , denoted by δ_{1t}^{ub} , via propensity score matching. With nearest neighbor matching, for example, the dependent variable for each matched observation in the $D = 0$ population is the product of Y_t and an estimate of $\Pr(D_2 = 0|D_1 = 0, p_1(\mathbf{X}^1))$. We can follow Heckman, Ichimura and Todd (1997) and estimate this conditional probability function at different values of the propensity score using a local linear regression. Under standard regularity conditions for nonparametric regressions and the correct specification of the propensity score model, the nonparametric estimate is consistent, and the sampling error can be ignored asymptotically when conducting inference using the Abadie and Imbens (2016) procedure. Finally, we can estimate the upper bound for δ_t with $\hat{\delta}_t^{\text{ub}} = \pi_1 \hat{\delta}_{1t}^{\text{ub}} + \pi_2 \hat{\delta}_{2t}$ and conduct inference similarly to that for $\hat{\delta}_t^{\text{lb}}$.

B.3 S -period Generalization

Now we generalize Assumptions 1 and 2 and the partial identification results from two periods to S periods.

First, define the period- s propensity score as

$$p_s(\mathbf{X}^s) \equiv \Pr(D_s = 1 | D_{s'} = 0 \text{ for all } s' \neq s, \mathbf{X}^s)$$

for $s = 1, \dots, S$.

Assumption 3. $Y_t(0) \perp\!\!\!\perp D_1 | \mathbf{X}^1$ for $t \geq 1$ and $Y_t(0) \perp\!\!\!\perp D_s | D_{s-1} = \dots = D_1 = 0, \mathbf{X}^s$ for $s = 2, \dots, S$ and $t \geq s$.

Assumption 4. $E[Y_t(0)|D_r = 1, \mathbf{X}^s] \leq E[Y_t(0)|D = 0, \mathbf{X}^s]$ for $r = s+1, \dots, S$, $t \geq r$, and $s = 1, \dots, S-1$.

We now state the generalized propositions for an arbitrary S . Their proofs are analogous to those of Propositions 1 and 2 but the notations are much more complex, so we omit them here. Propositions 3 and 4 construct lower and upper bounds of the TOT effects, respectively.

Proposition 3. Under Assumptions 3 and 4 and provided that $p_s(\mathbf{X}^s) < 1$ for all s ,

(a): for $s = 1, \dots, S-1$ and $t \geq s$,

$$E[Y_t|D_s = 1] - E[E[Y_t|D = 0, p_s(\mathbf{X}^s)]|D_s = 1] \leq E[Y_t(1) - Y_t(0)|D_s = 1],$$

and for $t \geq S$

$$E[Y_t|D_S = 1] - E[E[Y_t|D = 0, p_S(\mathbf{X}^S)]|D_S = 1] = E[Y_t(1) - Y_t(0)|D_S = 1];$$

(b): for $t \geq S$,

$$\sum_{s=1}^S \{E[Y_t|D_s = 1] - E[E[Y_t|D = 0, p_s(\mathbf{X}^s)]|D_s = 1]\} \Pr(D_s = 1 | D = 1) \leq E[Y_t(1) - Y_t(0)|D = 1].$$

Proposition 4. Under Assumption 3 and provided that $p_s(\mathbf{X}^s) < 1$ for all s and $Y_t(0) \geq 0$ for all t ,

(a): for $s = 1, \dots, S-1$ and $t \geq s$,

$$E[Y_t|D_s = 1] - E[E[Y_t|D = 0, p_s(\mathbf{X}^s)] \Pr(D = 0 | D_1 = \dots = D_s = 0, p_s(\mathbf{X}^s))|D_s = 1] \geq E[Y_t(1) - Y_t(0)|D_s = 1],$$

(b): for $t \geq S$,

$$\sum_{s=1}^S \{E[Y_t|D_s = 1] - E[E[Y_t|D = 0, p_s(\mathbf{X}^s)] \Pr(D = 0 | D_1 = \dots = D_s = 0, p_s(\mathbf{X}^s))|D_s = 1]\} \Pr(D_s = 1 | D = 1)$$

$$\geq E[Y_t(1) - Y_t(0)|D = 1].$$

B.4 Relation to the Dynamic Treatment Effect Literature: Details

B.4.1 Identification in the Robins Framework

In this section, we state the Robins identification results by adapting the excellent summary of Abbring and Heckman (2007). For ease of exposition, we focus on the two-period setting, but the results easily generalize to more periods. With two periods, there are four possible treatment sequences per Robins: trained in both periods, trained only in period 1, trained only in period 2, and did not receive training in either period. If we denote the training decision in period s by \tilde{D}_s , then a treatment sequence g is the concatenation of \tilde{D}_1 and \tilde{D}_2 and takes on the value of 11, 10, 01, or 00. We further denote the corresponding potential outcome for treatment sequence g at time $t \geq 1$ by Y_t^g , and the observation rule (or the consistency condition) is that $Y_t = Y_t^g$ when the actual treatment sequence is g .

There are two key assumptions in the Robins framework. The first is sequential randomization: for each treatment sequence $g = 11, 10, 01, 00$,

$$\tilde{D}_1 \perp\!\!\!\perp Y_t^g | \mathbf{X}^1 \text{ for } t \geq 1; \text{ and } \tilde{D}_2 \perp\!\!\!\perp Y_t^g | (\mathbf{X}^1, Y_1), \tilde{D}_1 \text{ for } t \geq 2. \quad (\text{A16})$$

It is easy to see that the assumption in (A16) is enveloped by our Assumption 1 (CIA): the two assumptions are equivalent if \mathbf{X}^2 , the period-2 conditioning set in the latter, takes the specific form of (\mathbf{X}^1, Y_1) . The second of Robins's assumption is no anticipation:

$$Y_1^{01} = Y_1^{00} \text{ and } Y_1^{11} = Y_1^{10}. \quad (\text{A17})$$

It states that potential outcomes in period 1 do not depend on the future treatment decision in period 2. As a side note, (A17) is equivalent to our no-anticipation assumption (A3) in Appendix B.1: $Y_1 = Y_1(0)$ when $D_1 = 0$. To see this, first note that $Y_1 = Y_1(0)$ for $D = 0$ (or $D_1 = D_2 = 0$) following our observation rule, so that the substantive part of our no-anticipation assumption is $Y_1 = Y_1(0)$ when $D_1 = 0$ but $D_2 = 1$. In the Robins framework, the $D_2 = 1$ population is subject to the $g = 01$ treatment sequence, and for them, $Y_1 = Y_1^{01} = Y_1^{00}$ where the second equality holds under (A17). Since our $D = 0$ population is subject to the $g = 00$ treatment sequence, our $Y_1(0)$ is the same as Robins's Y_1^{00} , and we have the desired result. Finally, we can apply this reasoning to show that the first part of (A3) is consistent with Robins by extending the definition of treatment sequences to cover times before the first period.

Under the assumptions in (A16) and (A17), the joint distributions of potential outcomes across time are identified for each counterfactual treatment sequence. For example, the potential outcome distributions

under $g = 00$ are identified as: for $t \geq 2$,

$$\Pr(Y_t^{00} = y_t, Y_1^{00} = y_1 | \mathbf{X}^1) = \Pr(Y_1 = y_1 | \tilde{D}_1 = 0, \mathbf{X}^1) \Pr(Y_t = y_t | \tilde{D}_1 = 0, \tilde{D}_2 = 0, Y_1 = y_1, \mathbf{X}^1), \quad (\text{A18})$$

and identifications for other g are similar. The proof of (A18) follows that in Section 3.2 of Abbring and Heckman (2007) and is omitted here. The estimand in (A18) is the sequential product of conditional outcome distributions using observations along the path of g .

Nonparametrically estimating these distributions is challenging, so Robins and various coauthors impose parametric restrictions on the relationship between potential outcomes, treatment, and confounders to obtain robust empirical results. For the structural nested model estimator (e.g., Robins, 1994), the restriction applies to the relationship between differences in potential outcomes (i.e., causal effects), treatment, and time-varying confounders in each period. For the marginal structural model estimator (e.g., Robins, 1998), the restriction applies to the relationship between levels of potential outcomes, summary measures of the treatment sequence (e.g., length of treatment exposure), and baseline confounders. Interested readers can consult Robins (2000) and references therein for a summary and comparison of his various estimators.

B.4.2 Practical Challenges with Adapting the Lechner (2009) IPW Estimator

Focusing on the two-period case, Lechner (2009) proposes a sequential IPW estimator for the counterfactual outcome of no training for workers enrolled in the first period. In his empirical analysis, Lechner (2009) reports standard errors obtained from five different methods and notes that they reassuringly lead to the same rejection decisions in most cases. However, Lechner (2009) does not choose a preferred standard error estimator and states that “it is beyond the scope of [his] article to investigate the issue of precise variance estimation of the IPW estimator in depth.”

It turns out that the IPW estimator takes a more complex form in the general S -period case. Formally, define the propensity scores $\tilde{p}_1 \equiv \Pr(D_1 = 1 | \mathbf{X}^1)$ and $\tilde{p}_s(\mathbf{X}^s) \equiv \Pr(D_s = 1 | \mathbf{X}^s, D_1 = D_2 = \dots = D_{s-1} = 0)$ for $s \geq 2$, which reflect the probability of treatment in period s among workers not yet treated conditional on observables up to s (note that $\tilde{p}_S(\mathbf{X}^S)$ coincide with $p_S(\mathbf{X}^S)$ defined in Section B.3). The IPW estimand is given by the following proposition.

Proposition 5. *Under Assumption 1 and provided that $\tilde{p}_s(\mathbf{X}^s) < 1$ for all s ,*

$$E[Y_t(0) | D_1 = 1] = \frac{1}{\Pr(D_1 = 1)} E \left[\frac{\tilde{p}_1(\mathbf{X}^1) \cdot Y_t \cdot 1_{[D=0]}}{\prod_{s=1}^S (1 - \tilde{p}_s(\mathbf{X}^s))} \right] \text{ for } t \geq 1. \quad (\text{A19})$$

We omit the proof of Proposition 5, as it simply extends that in Appendix B.1 of Lechner (2009).

According to Proposition 5, the denominator of the IPW estimator for the counterfactual Y_t mean among period-1 enrollees is the product of eight estimated propensity scores in our eight-period setting. In comparison, the denominator in Lechner’s two-period setting is the product of just two propensity scores. Because of this added complexity, we will need to reassess the sensitivity of different inferential methods. Furthermore, with eight periods, the subset of these methods based on generalized method of moments will involve many more parameters and moments, and is more likely to encounter numerical issues than in Lechner (2009). Given these challenges, we choose not to adapt Lechner (2009) to our analysis.

B.4.3 Studies that Model Unobserved Heterogeneity

An alternative strand of the dynamic treatment effect literature explicitly models the influence of unobserved heterogeneities. This strand typically models unobserved heterogeneities as “random effects”, which are assumed to be independent of observed covariates. In this section, we provide a brief overview of key studies in this strand.

Under no-anticipation and a conditional independence assumption (treatment is independent to outcome conditioning on both observed covariates and random effects), Abbring and van den Berg (2003, 2004) prove identification of dynamic treatment effects for duration outcomes in mixed proportional hazard models for both treatment and outcome timings. The word “mixed” refers to the model specification that a random effect enters into a proportional hazard model multiplicatively, which is crucial for identification. Heckman and Navarro (2007) unite the literature on dynamic treatment and on discrete choice—they consider semiparametric identification in a statistical framework where treatment in each period is determined by an index-crossing model. They allow anticipatory effects, and their identification relies on a large support condition that ensures sufficient variation of the index and helps to break treatment’s dependence on unobserved heterogeneity. Heckman and Navarro (2007) also propose an economically interpretable structural framework, and interested reader should consult Cunha, Heckman and Navarro (2007) and Heckman, Humphries and Veramendi (2016) for extensions. Ba et al. (2017) study labor market transitions among female adults recommended for classroom training in the NJS. They restrict to participants who enter the program during a nonemployment spell (following Eberwein, Ham and Lalonde, 1997), and they specify the joint likelihood of this initial nonemployment spell, timing of training participation, and subsequent employment and nonemployment spells. The likelihood is integrated over distributions of the random effects,

which are assumed to be discrete with finite supports as per Heckman and Singer (1984) and McCall (1996). Ba et al. (2017) estimate their complex model via simulated annealing.

C Alternative Identification Strategies

C.1 Jacobson, Lalonde and Sullivan (2005a) Fixed Effects Specifications

In this section, we consider models similar to those estimated in Jacobson, Lalonde and Sullivan (2005a) (hereafter, JLS), who use longitudinal earnings data to estimate the effects of attending community colleges for a population of UI claimants. To account for unobserved individual characteristics (that are either constant or evolving linearly), JLS estimate models of the form

$$Y_{it} = \beta E_{it} + \alpha_i + \omega_i t + \gamma_t + \delta_{it}(s_i, z_i) + \varepsilon_{it} \quad (\text{A20})$$

where Y_{it} is the earnings of individual i at time t , E_{it} is an indicator for whether the individual has started school as of time t , α_i and $\omega_i t$ are individual fixed effects and linear time trends, γ_t denote time fixed effects, and $\delta_{it}(s_i, z_i)$ are layoff effects that depend on layoff date s_i and fixed individual characteristics z_i .⁷ In the most basic specifications, $\delta_{it}(s_i, z_i) = \sum_{k=-12}^{24} D_{it}^k \delta_k$, where D_{it}^k are a full set of dummy variables for quarter relative to layoff: $D_{it}^k = 1$ if individual i had been laid off in quarter $t - k$. We also follow JLS and include a set of heterogeneous layoff effects that allow the earnings patterns relative to layoff to parametrically depend on demographic variables z_i , which include gender, race, whether a claimant lives in one of the ten largest counties, tenure, and ten-year age bins—see Jacobson, Lalonde and Sullivan (2005a) for the exact form of these heterogeneous layoff patterns.

To check for pre-trends in earnings before enrollment, we estimate models of the form:

$$Y_{it} = \sum_{k=-20}^{23} R_{it}^k \beta_k + \alpha_i + \omega_i t + \gamma_t + \delta_{it}(s_i, z_i) + \varepsilon_{it}$$

where Y_{it} is earnings for person i in quarter t , R_{it}^k is an indicator for beginning enrollment in quarter $t - k$ (and is equal to 0 for all non-enrollee observations). The regressions are estimated on a 5 percent random sample to speed up computation. We plot the estimated β_k from the regressions in Appendix Figure A.21.

The specification shown in Panel A of the figure includes individual fixed effects α_i , calendar quarter

⁷This is a simplification of Jacobson, Lalonde and Sullivan (2005a)'s model in that enrollment here is a binary state and we are estimating the average earnings effects after first enrolling. JLS also consider the incremental earnings effects of credits earned as well as earnings dynamics during and after school by replacing βE_{it} with $\phi_{it}(c_i, f_i, l_i, z_i)$ where c_i is credits earned, and f_i and l_i denote the enrollment entry and exit periods.

fixed effects γ_t , and quarter relative to layoff dummies; Panel B adds individual time trends $\alpha_i + \omega_i t$; Panel C adds heterogeneous layoff effects per the second term of equation (2) in Jacobson, Lalonde and Sullivan (2005a). We find that this “event study” specification check yields problematic pre-enrollment earnings differences between enrollees and non-enrollees, regardless of whether we account for individual trends. Therefore, we do not rely on these fixed effects models for our analysis.

C.2 Distance-Based Instrumental Variables

Another research design that has been used to estimate educational effects relies on the idea that, all else equal, students are more likely to enroll if they live close to a school (Card, 1995). Therefore, the distance to the nearest school can be used as an instrumental variable (IV) for enrollment. Since we observe the zip codes of UI claimants in our sample, we can adapt this design to our setting by computing the linear distance from a worker’s zip code to the nearest community college.⁸

Since the validity of the distance IV design hinges on the exogeneity of the distance measure, it is important to control for all other potential determinants of earnings that may also be correlated with distance to schools. In fact, we find workers who live close to community college to be different along observable dimensions from those who live farther away. Therefore, we control for a similar set of covariates as in our matching specification: 12 quarters of pre-layoff earnings, calendar quarter of layoff, prior industry (eight indicators), prior job tenure category (less than one year, one to six years, and more than six years), age indicators (age below 19, each year from age 19 and 59, and older than 59), whether a worker has a dependent, county unemployment rate during the month of layoff, and county fixed effects. As in our main analysis, we estimate models separately for men and women, and by whether or not they previously worked in manufacturing.

However, we do not find consistently strong first-stage relationships between distance and enrollment. Moreover, we find that the results are sensitive to transformations of the instrument (e.g., including a quadratic distance term), casting doubt on an IV framework in which the instrument is assumed to be independent to the reduced-form equation error term. We conclude that using an instrumental variables design to estimate enrollment effects is not desirable for our context.

⁸We use the nearest community college because nearly 90 percent of enrollment is in community colleges.

C.3 Timing of Layoff

One may conjecture that the timing of layoff may induce a discontinuous change in enrollment probability. That is, workers laid off just before the beginning of a semester may be much more likely to enroll than those laid off just after. This sudden decrease in probability may be leveraged to identify enrollment effects.

However, there do not appear to be salient discontinuities in Appendix Figure A.22, which plots fraction of enrollees by UI claim date. This should not be surprising given that schooling decisions may take time to materialize. In fact, the modal number of quarters between UI claim and enrollment is two as documented in the notes of Appendix Figure A.8. Therefore, implementing a regression discontinuity design in layoff date does not seem to be a viable path to estimating the effect of enrollment for unemployed workers.

D Enrollment Effects of Completers

In this section, we extend the potential outcomes framework in Section 4.1 and consider nonparametric partial identification of the enrollment effects for workers who ultimately obtain a credential (“completers”). For ease of exposition, we focus on the two-period setting, but our analysis easily extends to the case of an arbitrary number of periods.

First, we introduce additional notation. Let H be a binary variable indicating whether a worker is a completer if she enrolls. Here, we think of H as a type realized before enrollment. But H is only observed if a worker enrolls ($D = 1$): That is, we cannot observe in the non-enrollee sample who *would have been* a completer, which is a key distinction from the subgroup analysis in Section 5.2. We can capture the influence of H on the outcome by adding an argument to the potential outcome function: $Y_t(d, h)$ for $d, h = 0, 1$. Since a non-enrollee cannot complete a credential, we impose that $Y_t(0, 1) = Y_t(0, 0)$.

Next, we state our assumptions to partially identify the enrollment effect for compliers via matching.

Assumption 5. For $h = 0, 1$, $\{Y_t(0, h), H\} \perp\!\!\!\perp D_1 | \mathbf{X}^1$ for $t \geq 1$ and $\{Y_t(0, h), H\} \perp\!\!\!\perp D_2 | \mathbf{X}^2, D_1 = 0$ for $t \geq 2$.

Assumption 6. For $h = 0, 1$ and $t \geq 1$, $E[Y_t(0, h) | D_1 = 0, D_2 = 1, \mathbf{X}^1] \leq E[Y_t(0, h) | D_1 = 0, D_2 = 0, \mathbf{X}^1]$.

Assumption 7. For $s = 1, 2$ and $t \geq s$,

$$E[Y_t(0, 1) | D_s = 1, H = 1, \mathbf{X}^s] \leq E[Y_t(0, 0) | D_s = 1, H = 0, \mathbf{X}^s].$$

Assumption 5 extends Assumption 1. As we think of H as a pre-treatment type, it is reasonable to incorporate it into the conditional independence assumption. Assumption 6 is a restatement of Assumption

2 but uses the augmented potential outcome function. Assumption 7 pertains to the selection of compliers. It says that conditional on information available at enrollment, the completer enrollees have on average worse counterfactual outcomes than the non-completer enrollees. Although Assumption 7 does not have a direct counterpart in Section 4.1, it is in the same spirit as Assumption 7: Completers obtain a credential because of the dim labor market prospect they would face without it.

Now we construct the bounds for the enrollment effect of the compliers.

Proposition 6. *Under Assumptions 5-7 and provided that $Y_t(0,0) \geq 0$, for $s = 1, 2$ and $t \geq s$:*

$$E[Y_t|D_s = 1, H = 1] - E[E[Y_t|D = 0, \mathbf{X}^s]|D_s = 1, H = 1] \leq E[Y_t(1,1) - Y_t(0,1)|D_s = 1, H = 1] \quad (\text{A21})$$

$$E[Y_t|D_s = 1, H = 1] \geq E[Y_t(1,1) - Y_t(0,1)|D_s = 1, H = 1]. \quad (\text{A22})$$

Proof. For inequality (A21), we first prove the case of $s = 2$ and then $s = 1$. When $s = 2$, first notice that Assumption 5 implies that for $h = 0, 1$ and $t \geq 2$:

$$E[Y_t(0,h)|D_2 = 1, H = h, \mathbf{X}^2] = E[Y_t(0,h)|D = 0, H = h, \mathbf{X}^2]. \quad (\text{A23})$$

Combining Assumption 7 and (A23), we have

$$E[Y_t(0,1)|D = 0, H = 1, \mathbf{X}^2] \leq E[Y_t(0,0)|D = 0, H = 0, \mathbf{X}^2]. \quad (\text{A24})$$

It follows that

$$\begin{aligned} & E[Y_t|D = 0, \mathbf{X}^2] \\ &= \sum_{h=0,1} E[Y_t(0,h)|D = 0, H = h, \mathbf{X}^2] \Pr(H = h|D = 0, \mathbf{X}^2) \\ &\geq E[Y_t(0,1)|D = 0, H = 1, \mathbf{X}^2] \\ &= E[Y_t(0,1)|D_2 = 1, H = 1, \mathbf{X}^2] \end{aligned}$$

where the inequality uses (A24), and the last equality follows Assumption 3. Consequently,

$$\begin{aligned} & E[E[Y_t|D = 0, \mathbf{X}^2]|D_2 = 1, H = 1] \\ &\geq E[E[Y_t(0,1)|D_2 = 1, H = 1, \mathbf{X}^2]|D_2 = 1, H = 1] \\ &= E[Y_t(0,1)|D_2 = 1, H = 1], \end{aligned}$$

from which (A21) follows.

For the case of $s = 1$,

$$\begin{aligned} E[Y_t|D=0, \mathbf{X}^1] &= E[Y_t(0,1)|D=0, \mathbf{X}^1] \\ &\geq E[Y_t(0,1)|D_1=0, \mathbf{X}^1] \\ &= E[Y_t(0,1)|D_1=1, \mathbf{X}^1] \\ &\geq E[Y_t(0,1)|D_1=1, H=1, \mathbf{X}^1] \end{aligned}$$

where the first line follows from $Y_t(0,1) = Y_t(0,0)$, and the second, third, and fourth line from Assumption 6, 5, and 7, respectively. Inequality (A21) easily follows.

To bound the treatment effects from above, we need to bound $E[Y_t(0,1)|D_s=1, H=1, \mathbf{X}^s]$ from below. However, none of Assumptions 5-7 provide a direct way to do so with another population mean. That said, in the case of $s = 2$ for example, we could write

$$\begin{aligned} E[Y_t|D=0, \mathbf{X}^2] &= \sum_{h=0,1} E[Y_t(0,h)|D=0, H=h, \mathbf{X}^2] \Pr(H=h|D=0, \mathbf{X}^2) \\ &= \sum_{h=0,1} E[Y_t(0,h)|D=0, H=h, \mathbf{X}^2] \Pr(H=h|D_2=1, \mathbf{X}^2), \end{aligned}$$

and in principle bound $E[Y_t(0,1)|D_2=1, H=1, \mathbf{X}^2]$ from below using the average of the lowest $\Pr(H=1|D_2=1, \mathbf{X}^2)$ fraction of the $D=0, \mathbf{X}^2$ population. However, it is infeasible to implement this with nearest-neighbor matching used throughout the paper. We need to allow many more neighbors in order to estimate the probability $\Pr(H=1|D_2=1, \mathbf{X}^2)$, but the match quality will inevitably go down. Therefore, we choose to simply bound $E[Y_t(0,1)|D_s=1, H=1, \mathbf{X}^s]$ with zero, and inequality (A22) follows. \square

The propensity score counterpart to Proposition 6 turns out to require more than one propensity score. Intuitively, with both enrollment and completer status in the conditioning set, we will need the propensity to enroll as well as the propensity to be a completer. Therefore, in addition to the propensity score, $p_s(\mathbf{X}^s)$, from Section 4.1, we also need $e_s(\mathbf{X}^s) \equiv \Pr(H=1|D_s=1, \mathbf{X}^s)$ in the conditioning set (the proof of the partial identification result based on the propensity scores combines elements from the proofs of Propositions 1 and 6; it is tedious and omitted here). With two propensity scores, we will need to apply procedures tailored for multi-dimensional matching, so we opt to directly match on the covariates via Mahalanobis matching to save time from estimating the propensity scores.

While our upper bound construction in Proposition 6 simply relies on the non-negativity of potential

earnings of completers ($Y_t(0, 1)$), our lower bound construction is more involved. It entails drawing matches from the entire non-enrollee set, which consists of both completer and non-completer types. The observed earnings of these matches serve as an upper bound for the potential earnings of completers because we assume that completer types have, on average, lower potential earnings than non-completer types (Assumption 7). As a consequence, the lower bound becomes tighter as the number of completers increases. In fact, if all of our enrollees are completers, the lower bounds will coincide with those from Proposition 1 (and in this case, the enrollment effect for period-2 completers is point identified). Therefore, the lack of information in the estimated range for the completer enrollment effect from Section 5.7 can be largely attributed to the relatively low proportion of completers.

E Impact of UI Benefit Policies on Enrollment

In this section, we examine the role of UI benefit policies on enrollment decisions. Barr and Turner (2015) show that generous benefit durations induce more unemployed workers to pursue schooling. We replicate this finding for Ohio and discuss the implications for UI policy in light of our enrollment effect estimates. We note that while UI benefit durations affect enrollment, this policy variation cannot be readily used to estimate the effects of enrollment in our main analysis because benefit durations can directly impact unemployment and labor force participation (Rothstein, 2011).

E.1 Background and Summary of Barr and Turner (2015)

Under normal economic conditions, unemployed workers in Ohio (and in most other states) are eligible to receive 26 weeks of UI benefits, which replace 50 percent of past earnings up to a cap (the cap ranges from \$323 to \$524 per week during our sample period, depending on the year and number of dependents a worker has). The duration of benefits may be increased during economic downturns. As shown in Appendix Figure A.23, benefit durations varied substantially over our sample period in Ohio, reaching 99 weeks in 2009 and persisting for a few years afterward.⁹ The degree to which benefits were extended depended on the state unemployment rate and policies in place at a given time (see Rothstein, 2011 for details). Using the October Education Supplement of the Current Population Survey, Barr and Turner (2015) show that these changes in

⁹The narrow valleys are due to the failure to extend EUC08 legislation before a scheduled policy expiration. However, claimants were retroactively compensated after these lapses.

UI policy across states and over time increased unemployed workers' propensity to pursue post-secondary education. We use the same temporal variation to estimate the magnitude of the effect within Ohio.

One major difference between our analysis and Barr and Turner (2015)'s is our ability to observe the timing of enrollment (and unemployment), which allows us to relate enrollment to the UI policy in place at the time of the enrollment decision.¹⁰ This is important because, as discussed in Rothstein (2011), sudden changes in federal legislation during the Great Recession resulted in changing expectations on benefit duration over the unemployment spell. One way to see this is in Appendix Figure A.24. For this figure, we simulate the number of UI benefit weeks remaining for workers at various quarters of the unemployment spell using only variation in UI extension policies over time, and we plot the averages for different cohorts of claimants.¹¹ Before the recession, workers expected 26 weeks of benefits at the beginning of unemployment and would run out of benefits after two quarters. For cohorts laid off in 2008, workers began unemployment under the assumption that they were eligible for 26 weeks of benefits, but as new extensions began in June 2008, workers with relatively long spells of unemployment were eligible for the new extensions. Throughout 2009 and 2010, benefits were continually extended such that although workers used their benefits, the average number of UI benefit weeks remaining declined less quickly than would be expected mechanically with the passage of time—in fact, for the 2009 cohort, the remaining benefit weeks actually increased at times. To the extent that workers base enrollment decisions on expectations of UI remaining available, it is important to consider this policy variation over the course of unemployment.

E.2 Effect of UI Benefit Duration on Enrollment

Our analysis uses a 5 percent subset of our UI claims sample, which covers claimants from 2004-2011Q3.¹² For each claimant, we have a balanced panel of eight quarters starting in the first quarter after her claim. We are interested in the effect of the expected UI benefit duration on enrollment over the first two years of layoff. We estimate equations of the following form:

$$E_{it} = \beta P_{it} + p(UR_t; \rho) + \sum_{k=1}^8 \delta^k D_{it}^k + \lambda_t + \mathbf{X}_i \gamma + \varepsilon_{it}$$

¹⁰Barr and Turner (2015)'s main regressor is the UI duration in the August of the year enrollment is observed, under the assumption that workers make decisions to enroll at the start of the academic year.

¹¹This simulation assumes that all workers receive 26 weeks of regular benefits and is continuously unemployed starting on their claim date. The simulation code is lightly adapted from Rothstein (2011).

¹²One difference between our sample and Barr and Turner (2015)'s is that ours is a sample of *layoffs* while Barr and Turner (2015)'s is a sample of *unemployment spells*. The latter likely contains disproportionately more long-term unemployed workers.

where E_{it} is the enrollment indicator for worker i in quarter t , P_{it} is the UI potential benefit duration in quarter t , D_{it}^k are a set of indicators denoting the k th quarter since layoff, UR_t is the state unemployment rate at time t , and \mathbf{X}_i contain demographic and pre-layoff job characteristics. The main coefficient of interest is β , which is the effect of the benefit duration on enrollment. Since benefit durations are partially determined by state economic conditions, we include a quadratic function $p(UR_t; \rho)$ of the unemployment rate, following Barr and Turner (2015). We also flexibly control for time since layoff (δ^k), year and quarter-in-year effects (λ_t), and worker characteristics including gender, age category, race, whether a worker reports having dependents, past wage quintile, tenure at last employer (four categories), past industry (two-digit NAICS), and past occupation (two-digit SOC).

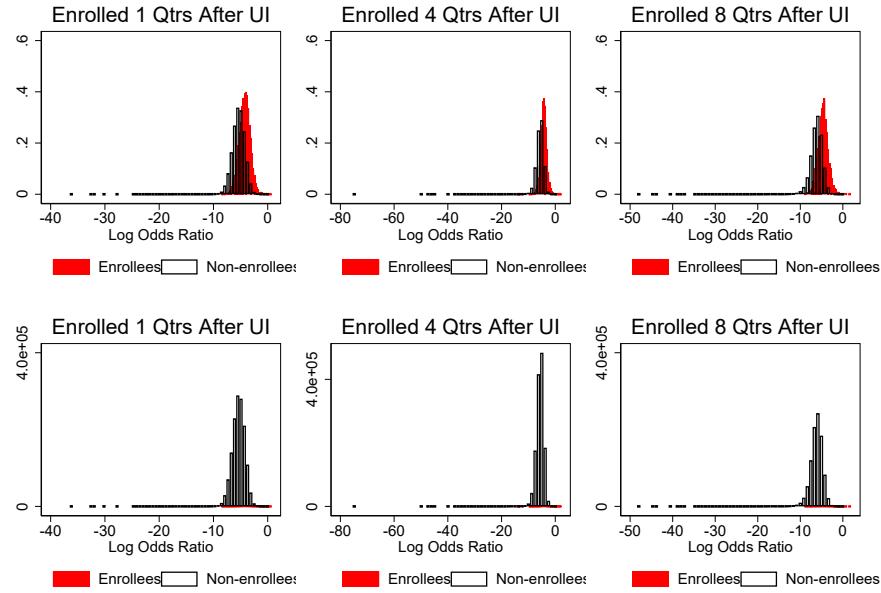
Appendix Table A.6 presents our regression results. The first column shows that a ten-week increase in potential benefit duration raises the probability of enrollment by 0.15 percentage points, or a 10 percent increase. Although this effect is smaller than the point estimate found in Barr and Turner (2015), it does fall inside their 95 percent confidence interval. As noted above, since potential durations were changing throughout the unemployment spell during the recession, the benefit duration presumed at the beginning of a worker's unemployment spell differs from the benefit duration actually experienced. In the second column of Appendix Table A.6, we show that the potential duration at the beginning of the spell has no impact on enrollment beyond the impact of the potential duration at the time of enrollment. The third column estimates the effect using an individual fixed effects model, utilizing only the variation in potential benefit expectations over time for each spell, and finds that the effect of potential duration is slightly smaller, though still statistically significant. Finally the fourth column only includes workers who are on their first unemployment spell, defined as those who have not yet experienced a full 13-week quarter of employment. Although the probability of enrollment is higher in this sample, the estimate is similar in percentage terms: a 10-week increase in potential benefit duration increases enrollment by 11 percent.

E.3 Policy Implications

These estimates indicate that a 10-week increase in UI potential durations induces approximately 1,200 more workers to enroll annually. Assuming that our estimated earnings gain of \$348 per quarter in the third and fourth year represent an estimate of the long-run effects, the increased enrollment would imply about \$1.7 million per year in earnings gain starting in the fifth year after enrollment (since the lock-in effects in the first two years roughly equal the positive earnings effects in the third and fourth years).

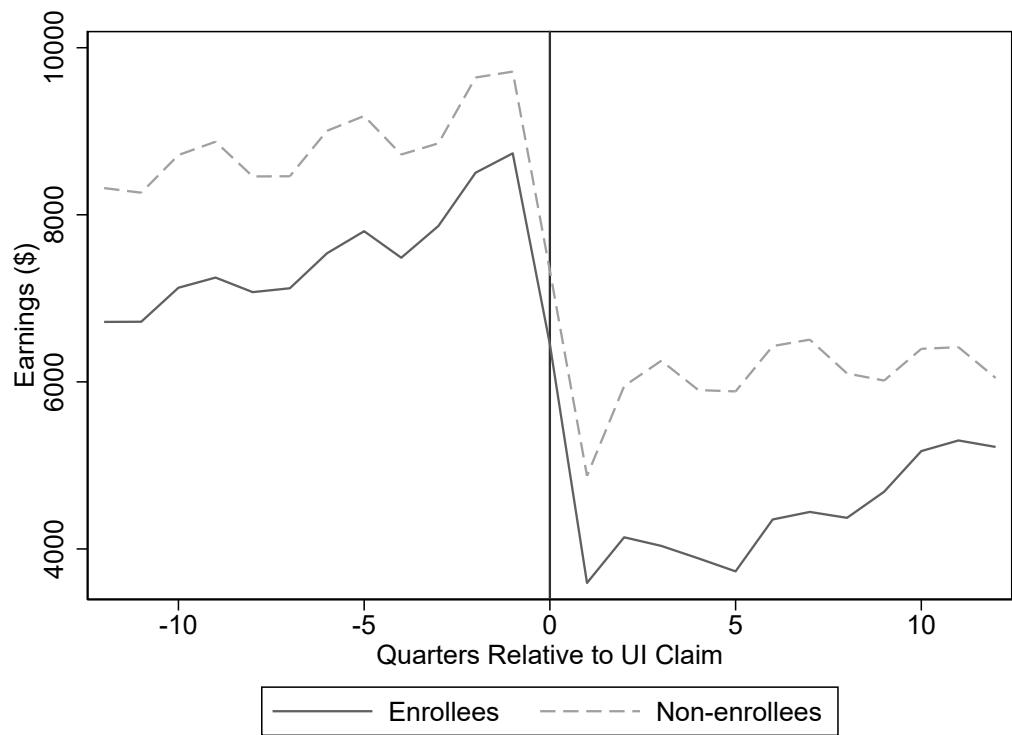
This finding also suggests that there is an externality associated with extending UI. As discussed in Schmieder and von Wachter (2016) and Lee et al. (2021), the overall impact of a policy on the government budget is a critical parameter in optimal policy-making. Therefore, estimates of the implied increases in tax revenues and government expenditures related to financial aid or tuition subsidies should be accounted for in UI policy analysis.

Figure A.1: Distributions of Log Odds Ratio



Notes: These figures show the distributions of the estimated propensity to enroll (expressed in log-odds ratios), for UI claimants who enroll in the first, fourth, and eighth quarters after layoff, and non-enrollees. The first row of figures show the distributions conditional on being an enrollee/non-enrollee; the second row shows the distributions unconditionally. There are $N = 1,432,293; 1,348,489; 959,731$ UI claims in the graphs in each column, corresponding to 1,045,644; 995,685; and 763,280 unique individuals.

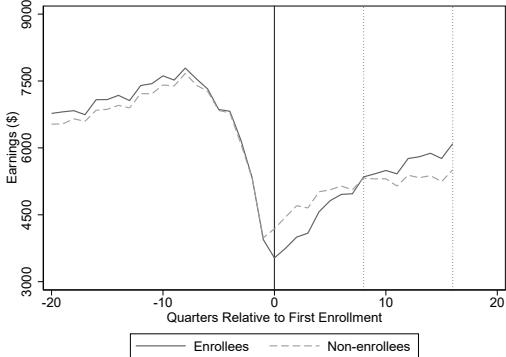
Figure A.2: Earnings of Enrollees and Non-enrollees



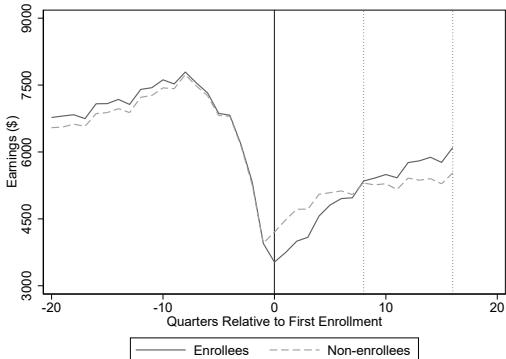
Notes: This figure plots the average quarterly earnings of enrollee and non-enrollee UI claimants for five percent of our analysis sample. The vertical line denotes the UI claim quarter. $N = 93,528$ UI claims (corresponding to 91,043 unique individuals).

Figure A.3: Earnings of Enrollees and Matched Non-enrollees Using Alternative Matching Specifications

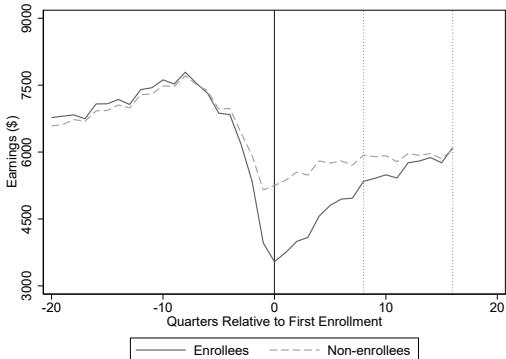
(A) Matched on One Quarter of Earnings Before Layoff and Earnings Between Layoff and Enrollment



(B) Matched on Four Quarters of Earnings Before Layoff and Earnings Between Layoff and Enrollment

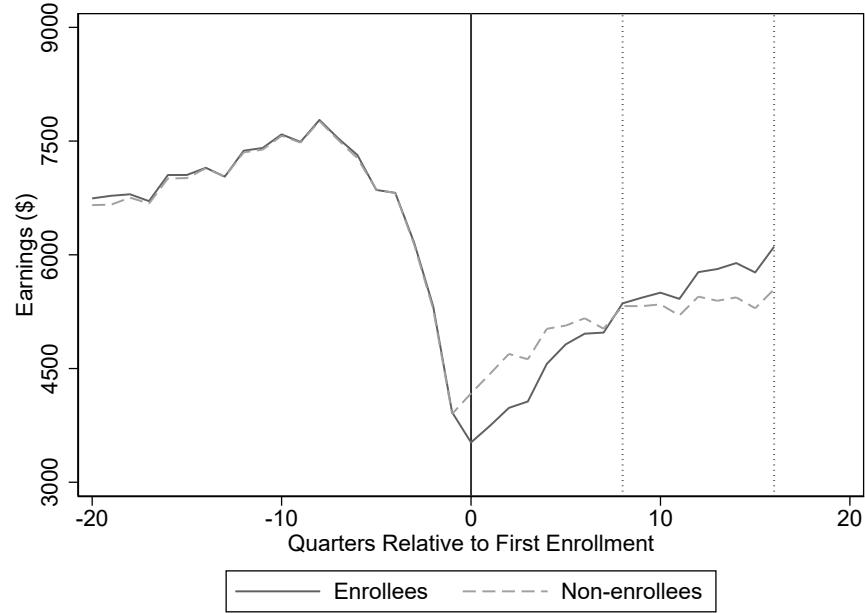


(C) Matched on Four Quarters of Earnings Before Layoff

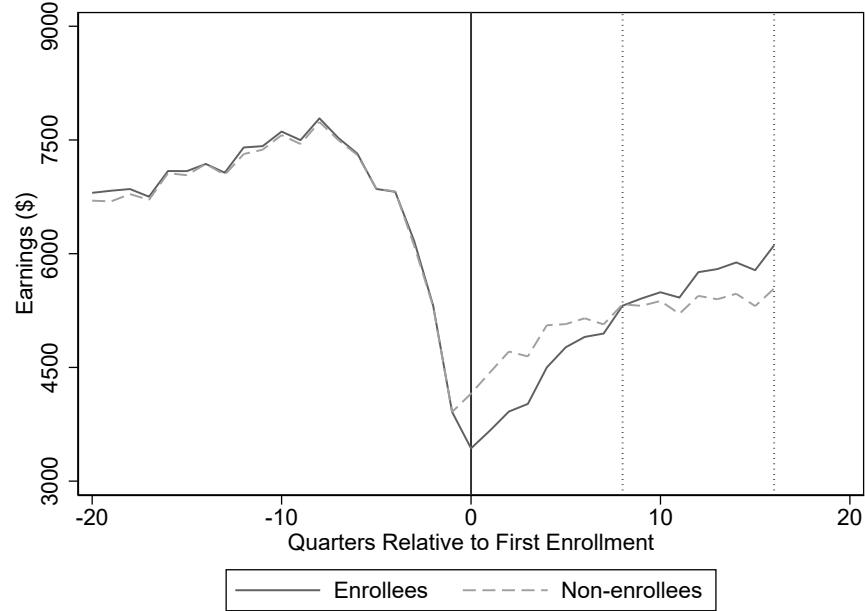


Notes: These figures plot the average quarterly earnings of enrollee and matched non-enrollee UI claimants, where the matching is done using all demographic variables described in Section 4.3 and the following alternative sets of earnings variables: one quarter of earnings before layoff and earnings between layoff and enrollment (Panel A), four quarters of earnings before layoff and earnings between layoff and enrollment (Panel B), and four quarters of earnings before layoff (Panel C). The solid vertical line denotes the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. These figures contain $N = 142,908$; $142,460$; and $141,958$ UI claims, respectively, corresponding to $137,360$; $136,741$; and $136,412$ unique individuals.

Figure A.4: Earnings of Enrollees and Matched Non-enrollees Using Alternative Specifications
 (A) Including Zero Earnings Indicators in Matching Specification

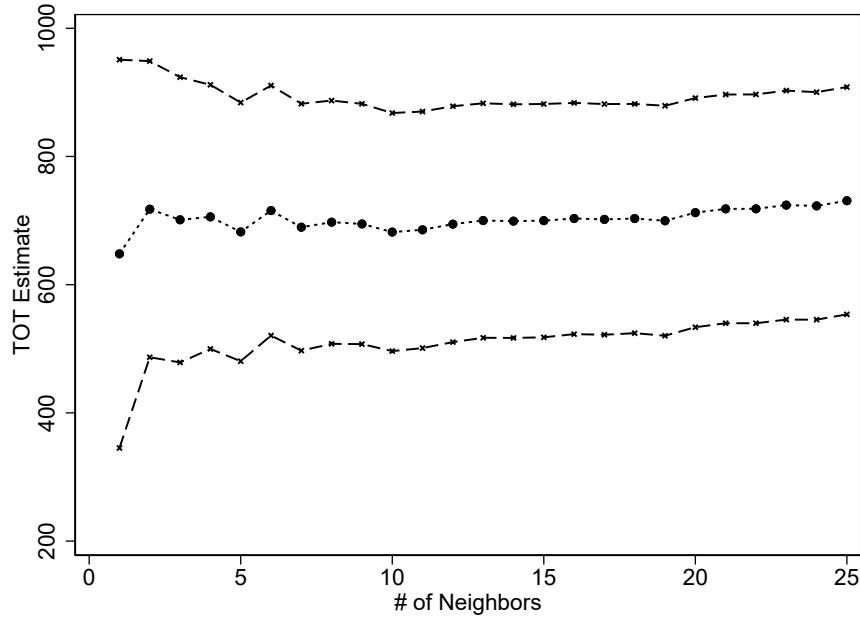


(B) Excluding Border Counties

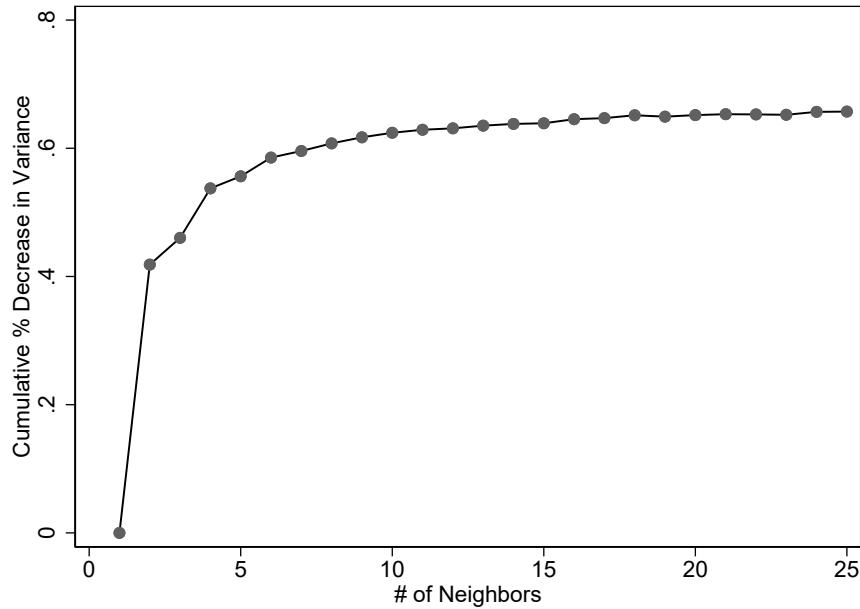


Notes: These figures plot the average quarterly earnings of enrollee and matched non-enrollee UI claimants, where the matching is done using all demographic variables described in Section 4.3 and a) including indicators for zero earnings in each quarter (Panel A), and b)excluding counties on the Ohio border (Panel B). The solid vertical line denotes the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. These figures contain $N = 139,550$ and $109,098$ UI claims, respectively, corresponding to 133,726 and 104,186 unique individuals.

Figure A.5: Estimated Enrollment Effects, By Number of Neighbors
 (A) Estimated Enrollment Effect



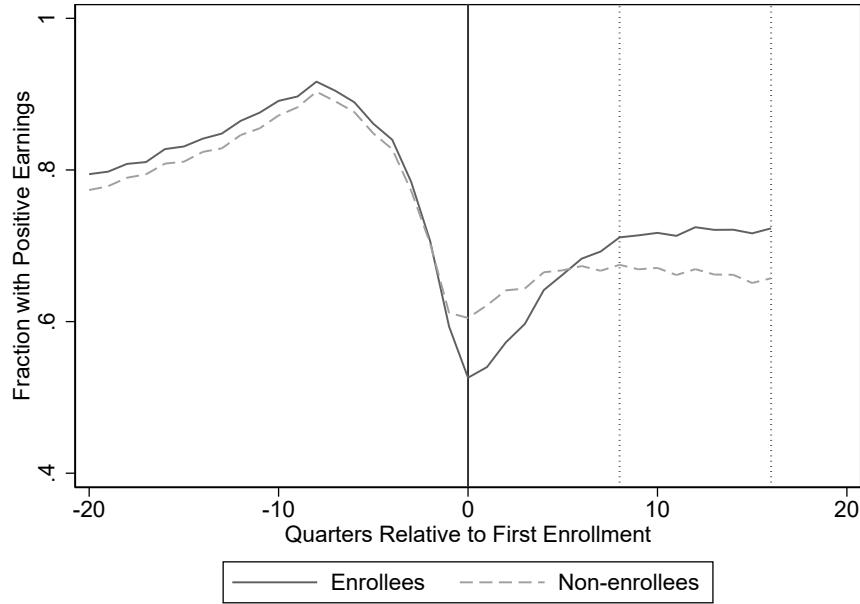
(B) Reduction in Variance



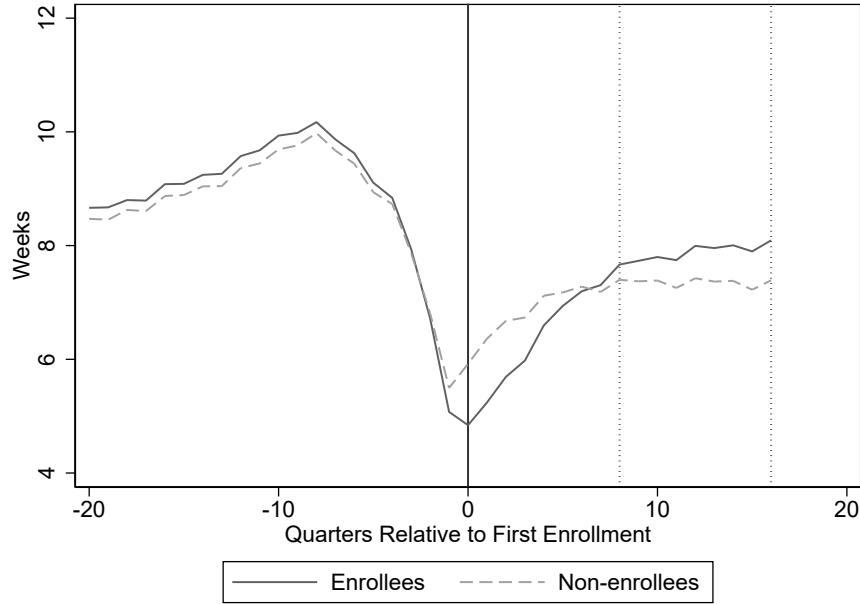
Notes: Panel A shows how the estimated enrollment effect (3-4 years after enrolling) varies by the number of matched neighbors for men who did not previously work in manufacturing and who filed a UI claim in the first quarter of 2009. Panel B shows the percent reduction in the variance of the estimated effect, relative to using one neighbor to match. $N = 54,685$ UI claims, corresponding to 54,685 unique individuals.

Figure A.6: Employment of Enrollees and Matched Non-enrollees

(A) Probability of Having Any Positive Earnings

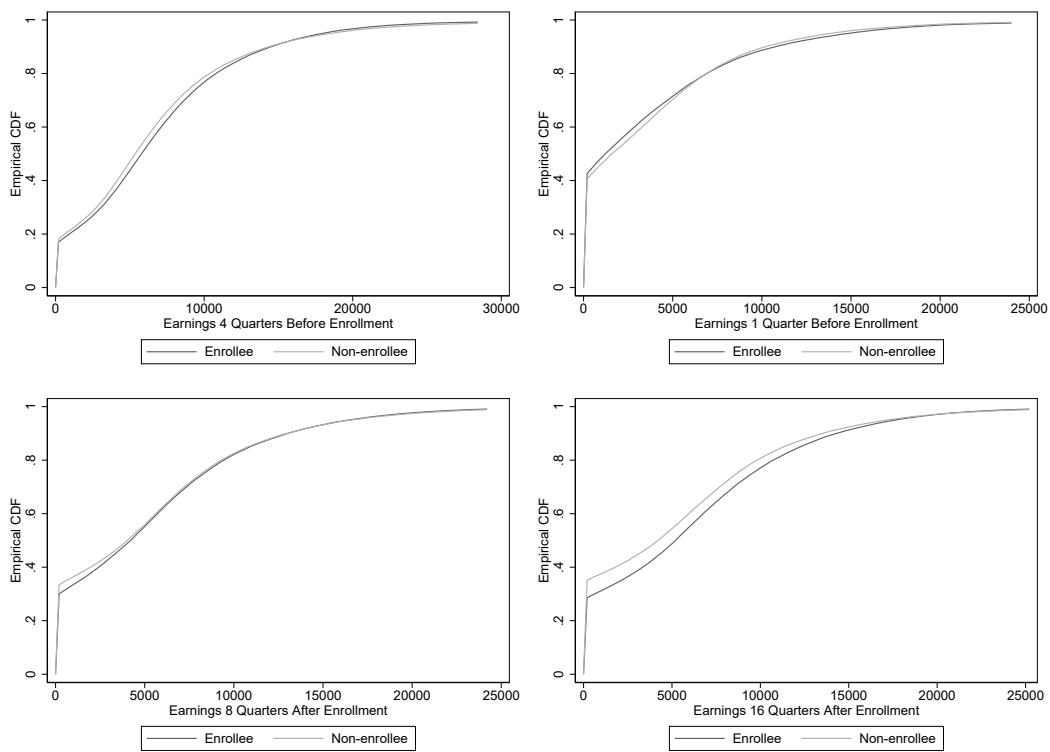


(B) Weeks Worked Per Quarter



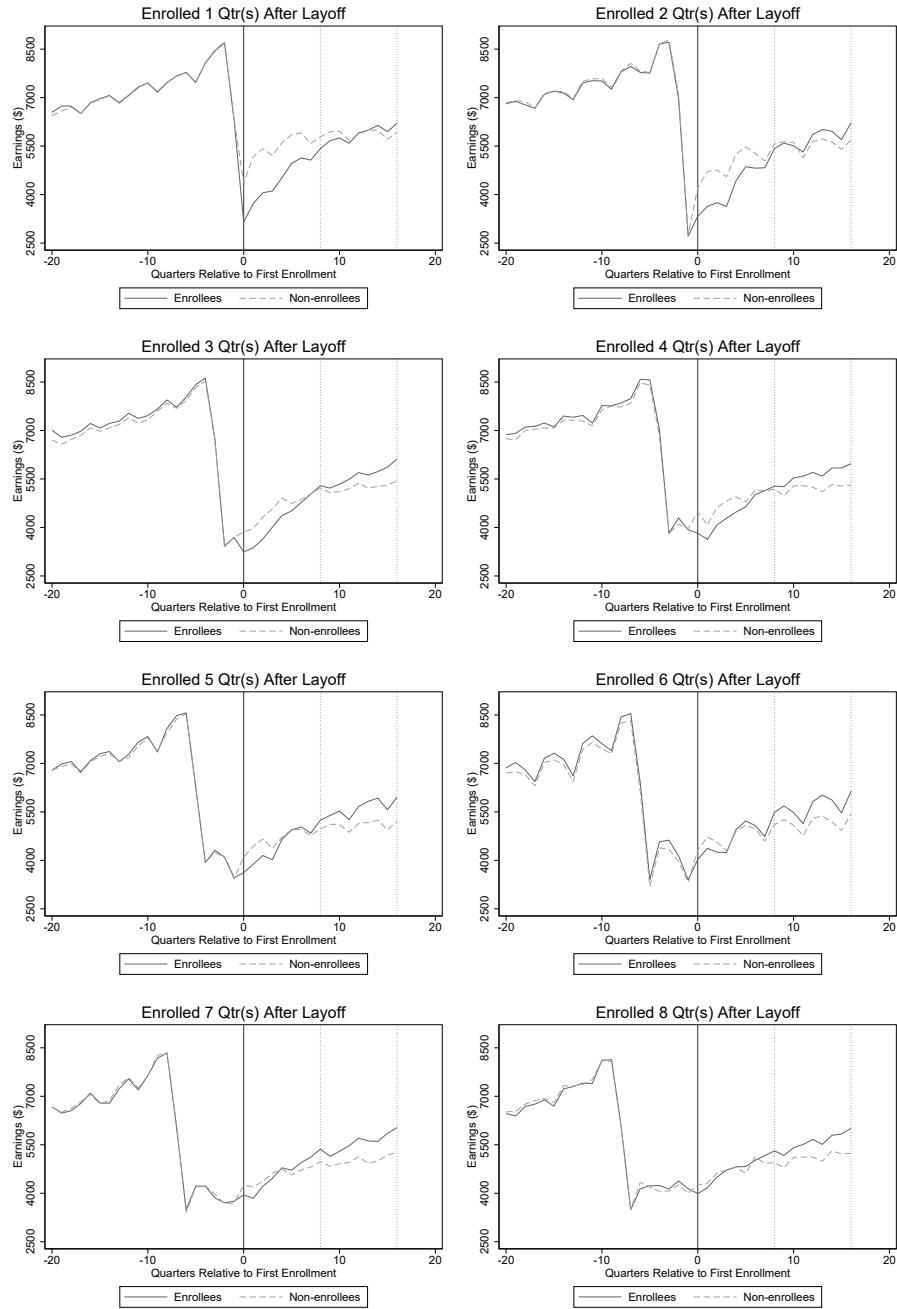
Notes: These figures plot the fraction of enrollee and matched non-enrollee UI claimants with positive earnings in each quarter (Panel A) and the quarterly average number of weeks worked (Panel B). The solid vertical line denotes the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. $N = 141,758$, corresponding to 136,074 unique individuals.

Figure A.7: Distributions of Enrollee and Matched Non-enrollee Earnings



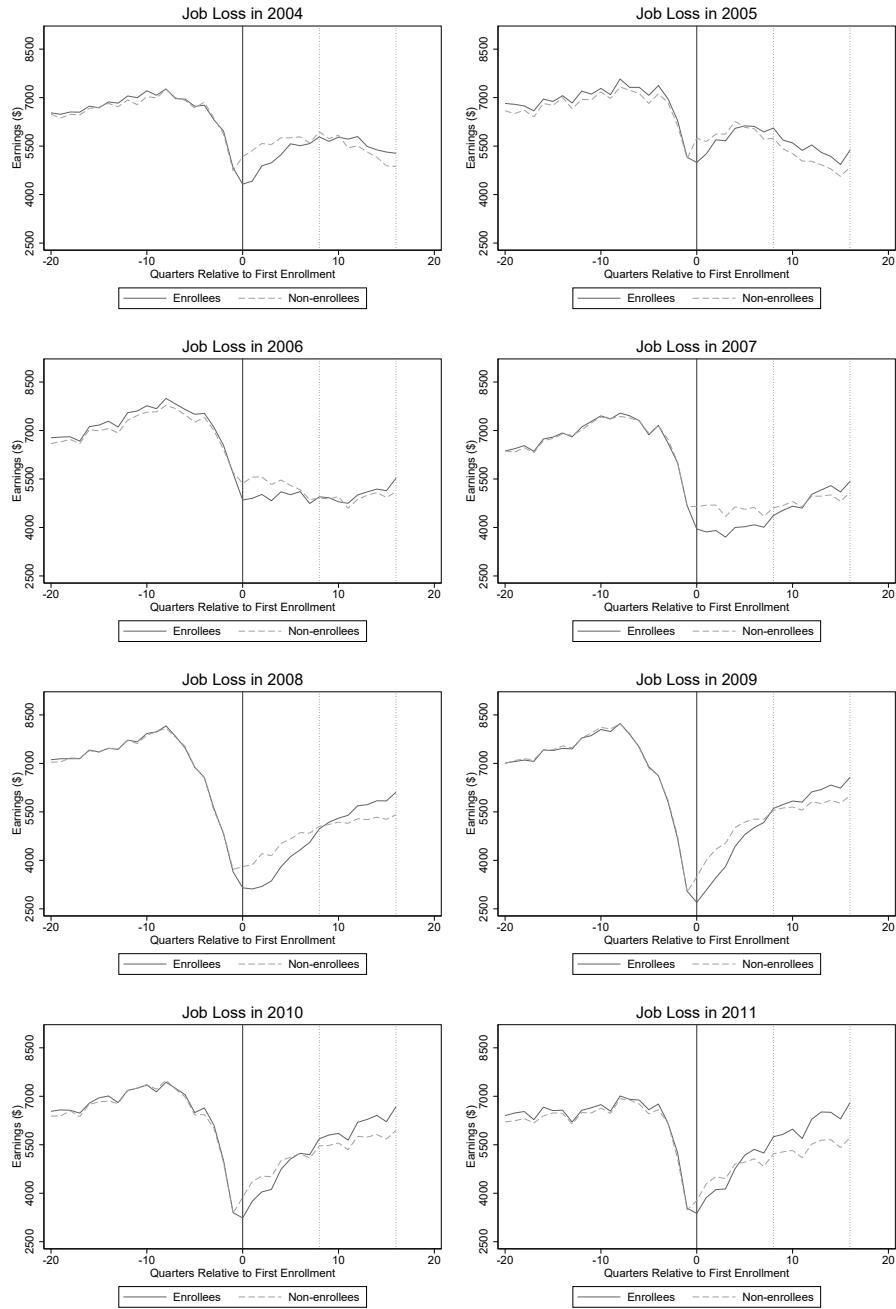
Notes: These figures show the empirical cumulative distribution functions of earnings for enrollees and matched non-enrollees four quarters before enrollment, one quarter before enrollment, eight quarters after enrollment, and 16 quarters after enrollment. $N = 141,758$, corresponding to 136,074 unique individuals, for each graph.

Figure A.8: Earnings of Enrollees and Matched Non-enrollees, By Enrollment Timing



Notes: Each graph shows the average quarterly earnings of UI claimants who enroll a certain number of quarters after filing a UI claim and their matched non-enrollees. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 23,868; 27,126; 23,472; 18,976; 15,190; 13,154; 11,032; 8,940$ UI claims in each graph, corresponding to 23,579; 26,750; 23,184; 18,769; 15,024; 13,024; 10,919; 8,866 unique individuals.

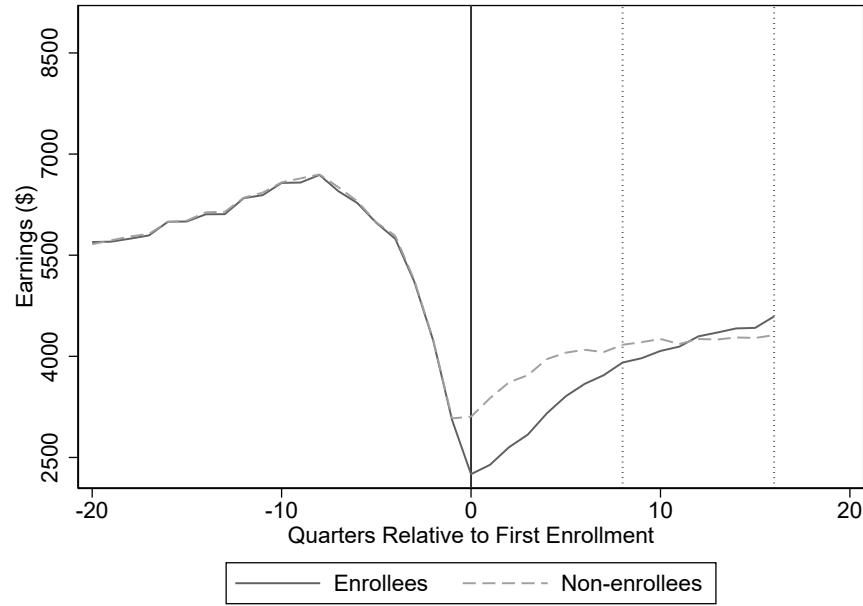
Figure A.9: Earnings of Enrollees and Matched Non-enrollees, By Year of Job Loss



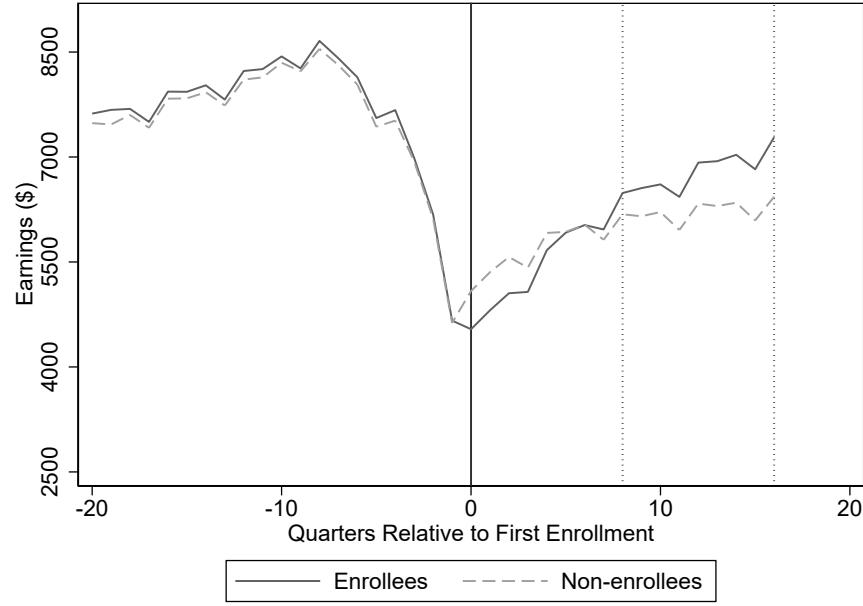
Notes: Each graph shows the average quarterly earnings of enrollees and matched non-enrollees who filed a UI claim in a specific year. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 12,034; 9,264; 13,494; 14,960; 27,794; 34,216; 19,246; 10,750$ UI claims in each graph, corresponding to 11,800; 9,096; 13,250; 14,632; 27,132; 33,358; 18,733; 10,514 unique individuals.

Figure A.10: Earnings of Enrollees and Matched Non-enrollees, By Gender

(A) Women

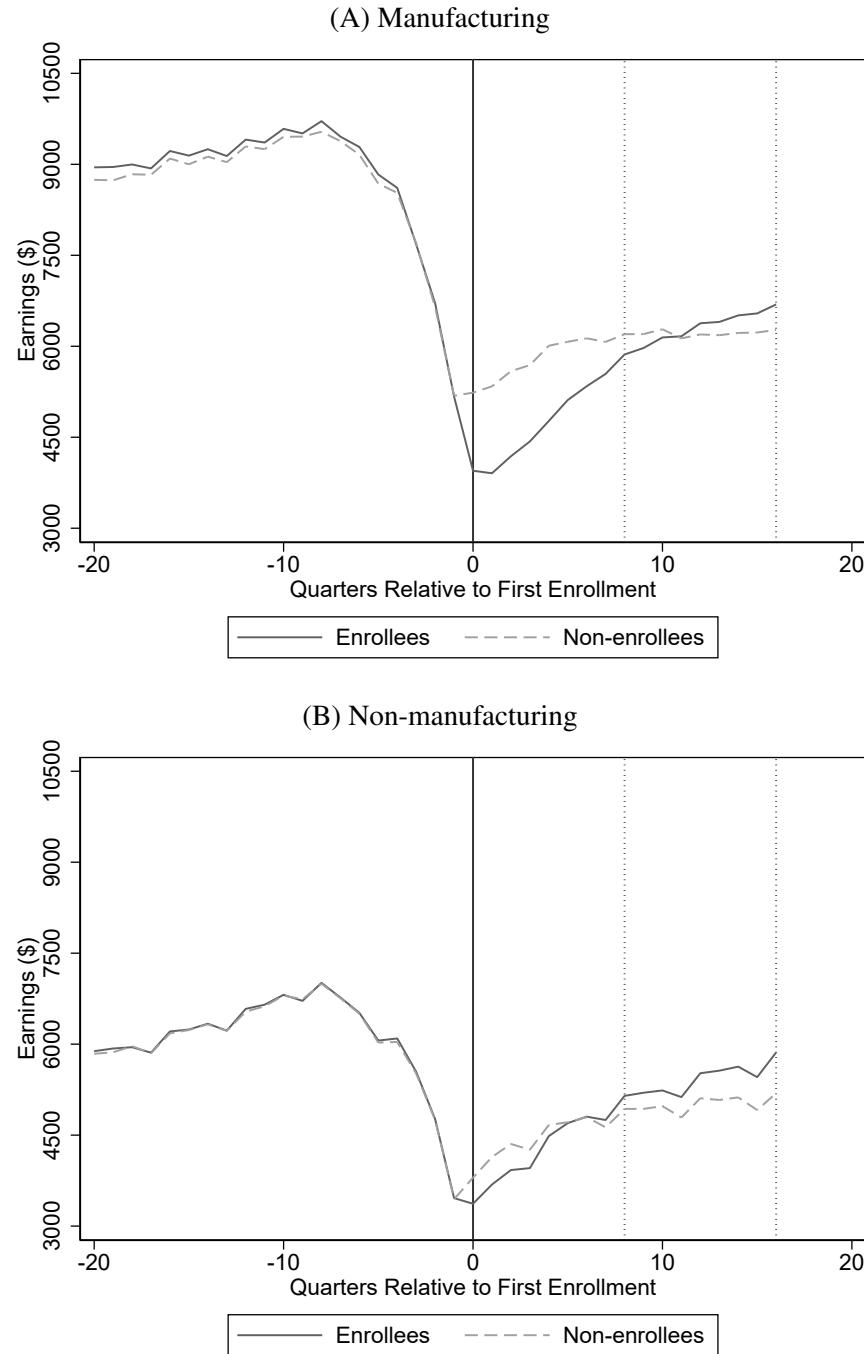


(B) Men



Notes: The upper (lower) graph shows the average quarterly earnings of female (male) enrollee and matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 62,096$ ($79,662$) UI claims in the upper (lower) graph, corresponding to 59,260 (76,814) unique individuals.

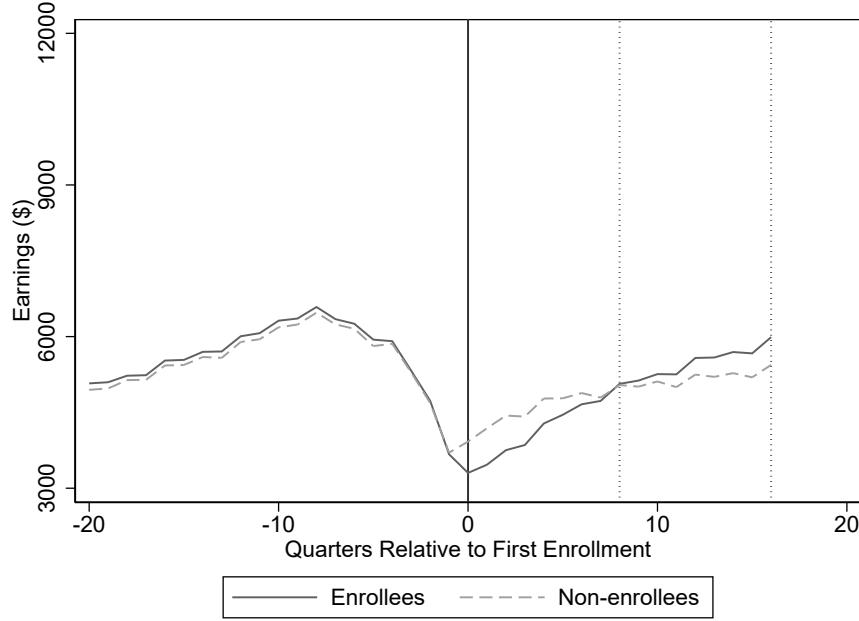
Figure A.11: Earnings of Enrollees and Matched Non-enrollees, Manufacturing vs. Non-manufacturing



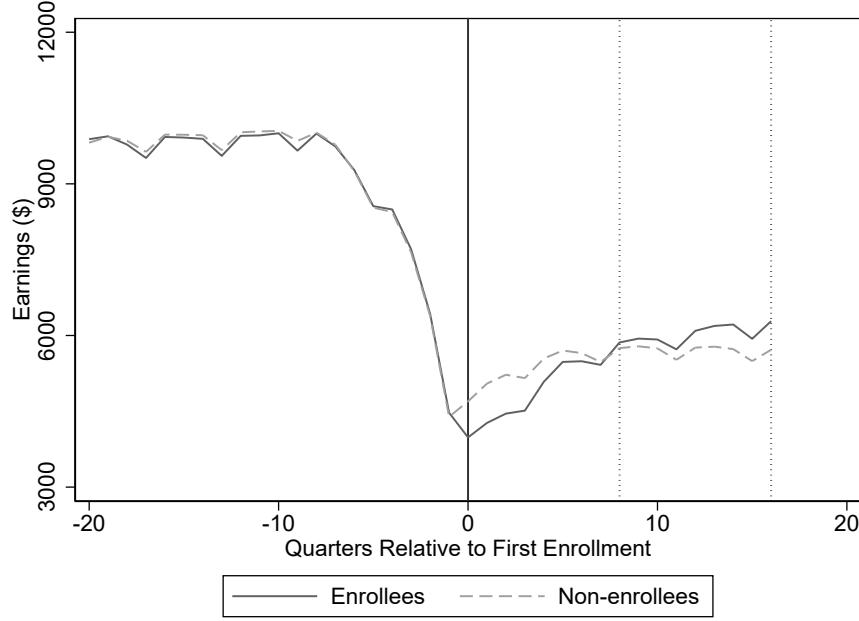
Notes: The upper (lower) graph shows the average quarterly earnings of enrollees who previously worked in a manufacturing (non-manufacturing) sector and matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 41,252$ ($100,506$) UI claims in the upper (lower) graph, corresponding to 39,514 (96,902) unique individuals.

Figure A.12: Earnings of Enrollees and Matched Non-enrollees, By Age

(A) Under Age 40



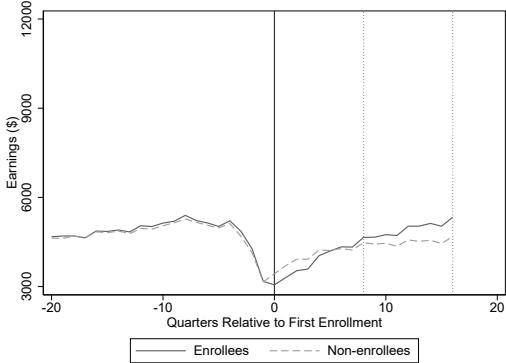
(B) Age 40 or Over



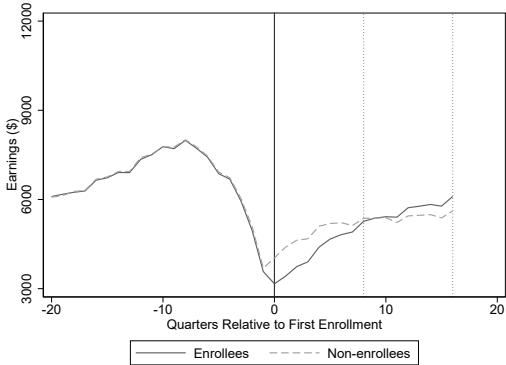
Notes: The upper (lower) graph shows the average quarterly earnings of enrollees under age 40 (age 40 or over) and matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 92,712$ ($50,728$) UI claims in the upper (lower) graph, corresponding to 88,477 (49,357) unique individuals.

Figure A.13: Earnings of Enrollees and Matched Non-enrollees, By Tenure

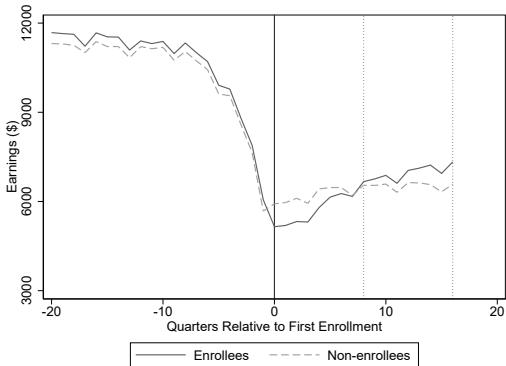
(A) Job Tenure of One Year or Less



(B) Job Tenure More Than One Year, Less Than Or Equal to Six Years



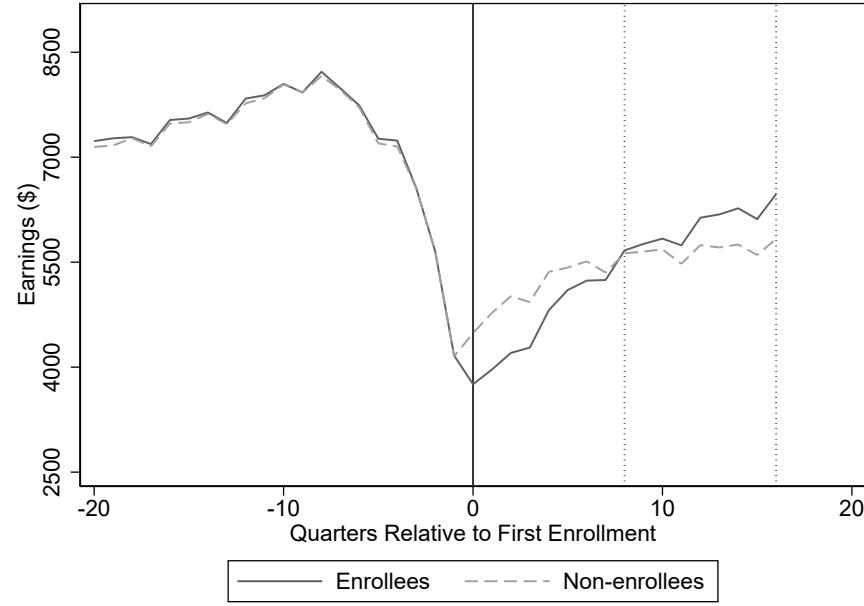
(C) Job Tenure More Than Six Years



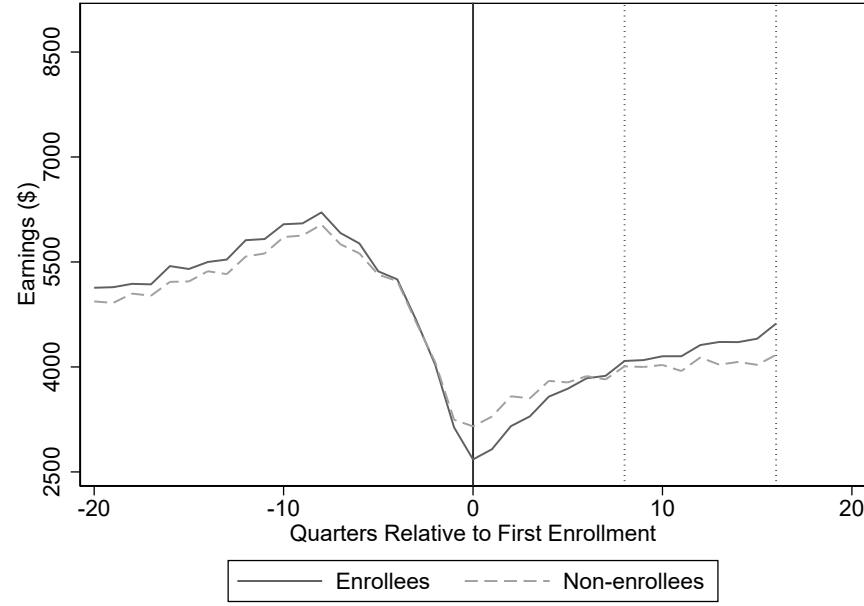
Notes: These graphs shows the average quarterly earnings of enrollees in different job tenure categories and matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 49,554$; $63,902$; and $29,984$ UI claims in the each graph, respectively, corresponding to $47,942$; $61,865$; and $29,026$ unique individuals.

Figure A.14: Earnings of Enrollees and Matched Non-enrollees, By Race

(A) White

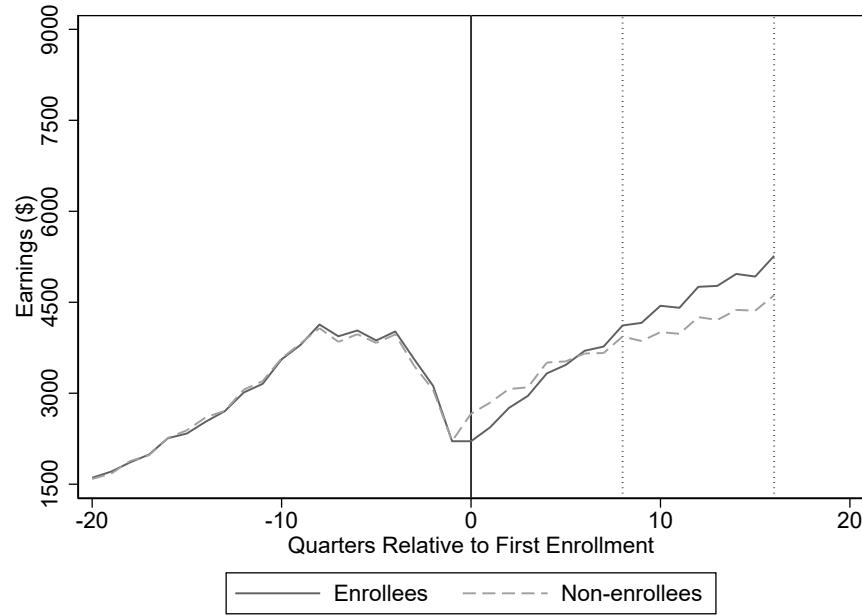


(B) Black

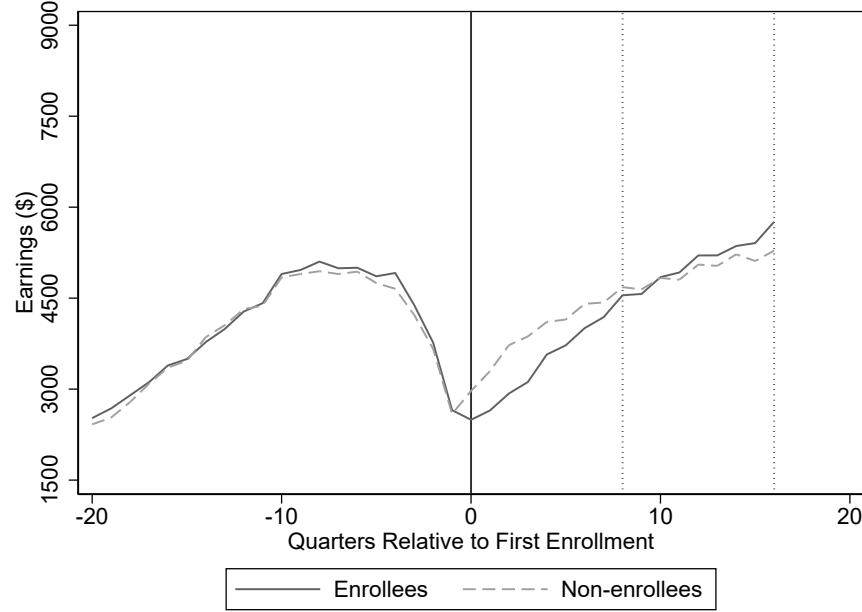


Notes: The upper (lower) graph shows the average quarterly earnings of white (Black) enrollees and matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 107,810$ ($25,292$) UI claims in the upper (lower) graph, corresponding to $103,514$ ($24,037$) unique individuals.

Figure A.15: Earnings of Enrollees and Matched Non-enrollees, By Prior College Experience
 (A) No Prior College



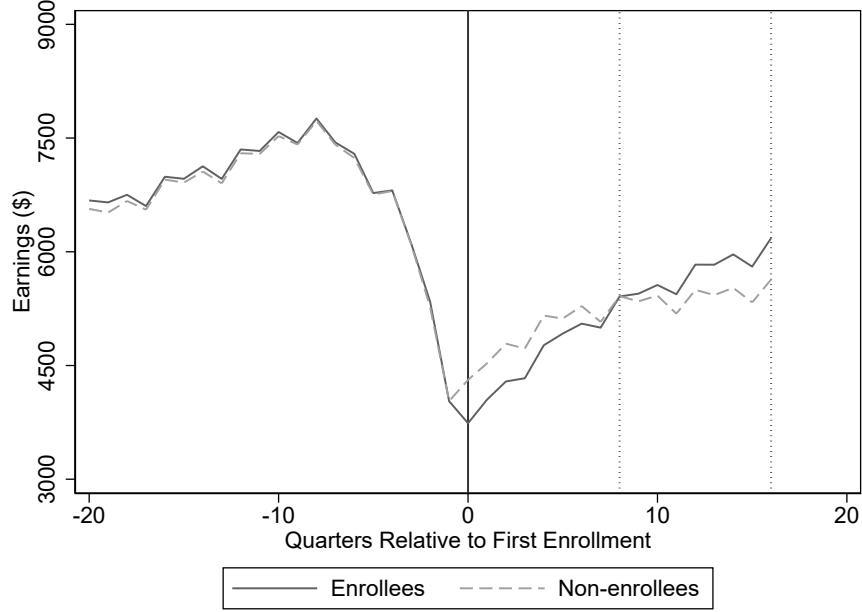
(B) With Prior College



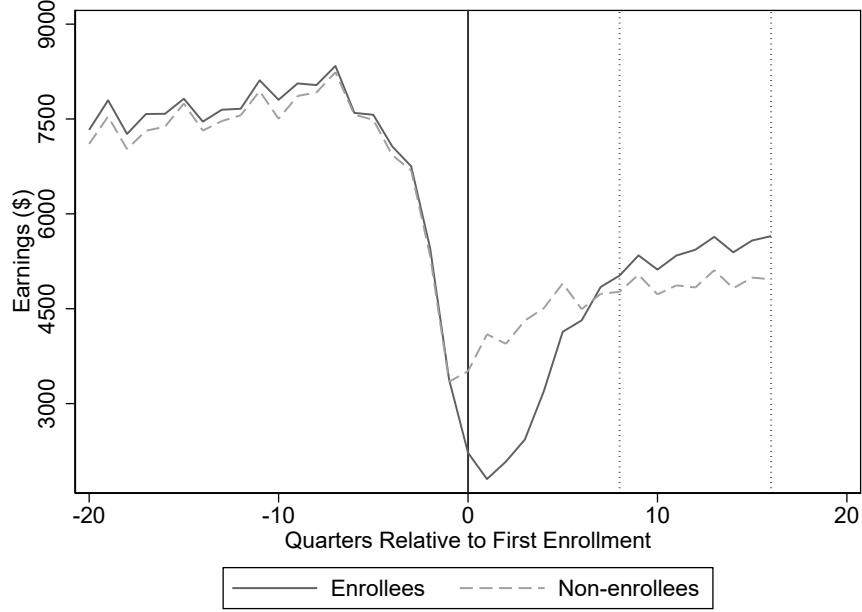
Notes: The upper (lower) graph shows the average quarterly earnings of enrollees with (without) prior college experience, and their matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 12,966$ ($5,212$) UI claims in the upper (lower) graph, corresponding to $12,421$ ($4,862$) unique individuals.

Figure A.16: Earnings of Enrollees and Matched Non-enrollees, By School Type

(A) Community College



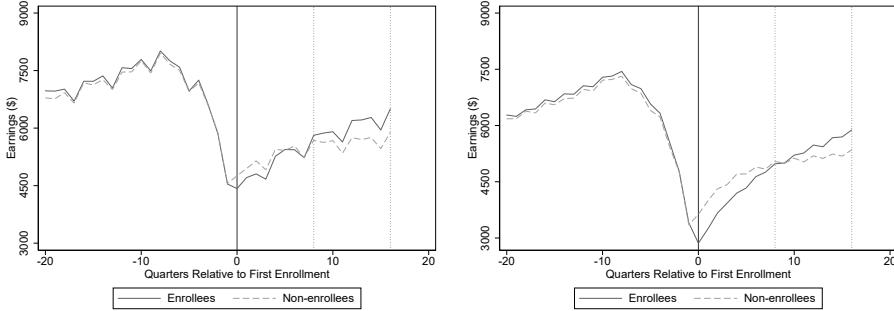
(B) Technical Center



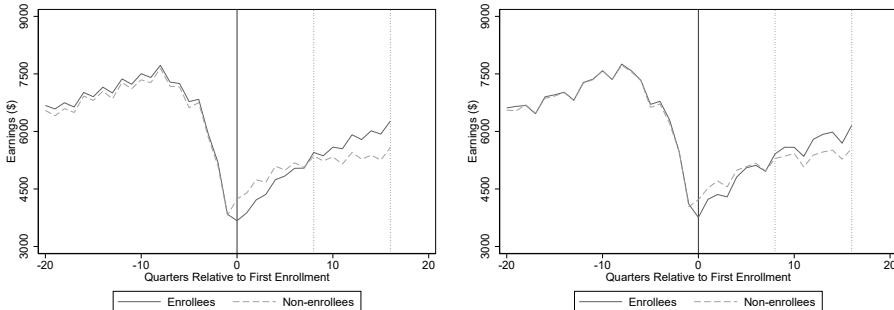
Notes: The upper (lower) graph shows the average quarterly earnings of community college (technical center) enrollees and matched non-enrollee UI claimants. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and 16 quarters after first enrollment. There are $N = 121,078$ ($15,932$) UI claims in the upper (lower) graph, corresponding to $116,691$ ($15,703$) unique individuals.

Figure A.17: Earnings of Enrollees and Matched Non-enrollees, By Institution Quality

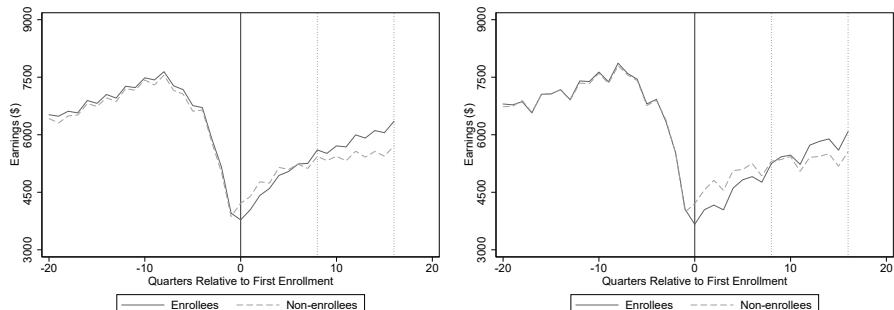
(A) Instructional Expenditures Per Student



(B) Institutional Completion Rates

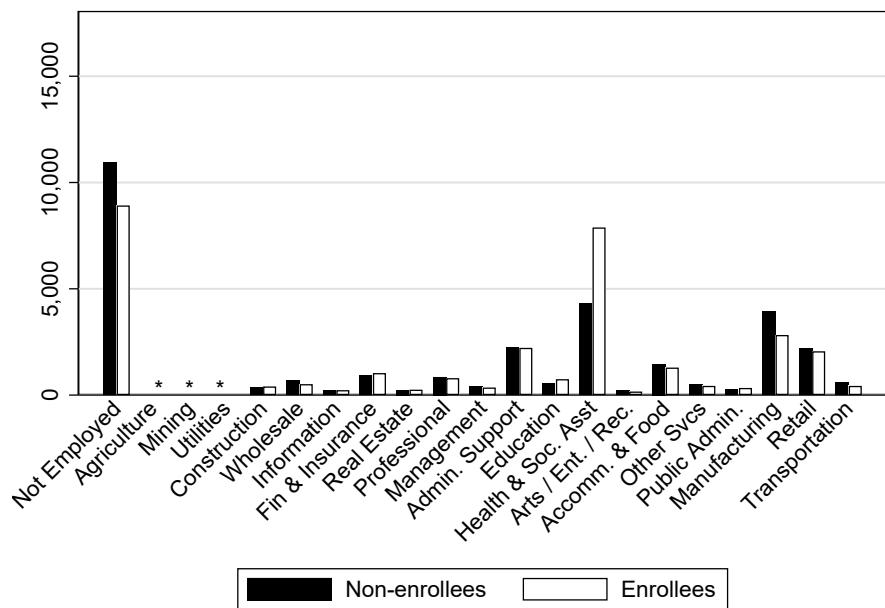


(C) Institutional Earnings

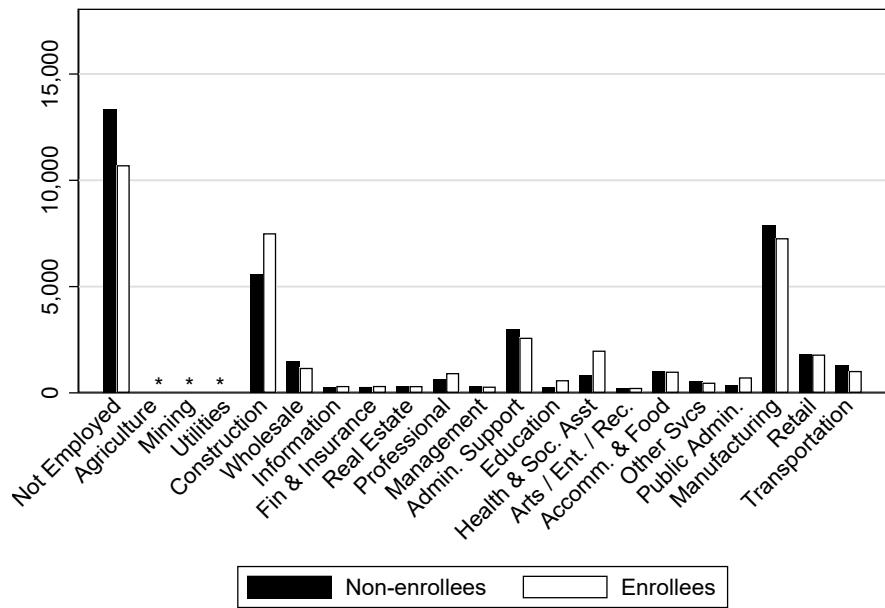


Notes: The left (right) graph of Panel A shows the average quarterly earnings of enrollees who attend community colleges that have above (below) median instructional expenditures per student, and matched non-enrollee UI claimants. The left (right) graph of Panel B shows analogous results for those who attend community colleges that have above (below) median institutional completion rates, and matched non-enrollee UI claimants. Institutional completion rate is defined as the proportion of entering students who graduated within eight years of entry. The left (right) graph of Panel C shows the analogous results for those who attend community colleges that have above (below) median institutional earnings. Institutional earnings is defined as the median earnings of students working and not enrolled 10 years after entry. The solid vertical lines denote the quarter of first enrollment, and the vertical dashed lines denote eight and sixteen quarters after first enrollment. There are $N = 63,348, 50,754, 60,488, 54,914, 57,504, 58,238$ UI claims in the upper left, upper right, middle left, middle right, lower left, and lower right graphs, respectively, corresponding to 61,742, 49,789, 59,129, 53,573, 56,261, 57,004 unique individuals.

Figure A.18: Industries of Enrollees and Matched Non-enrollees, 16th Quarter Post-Enrollment
 (A) Women



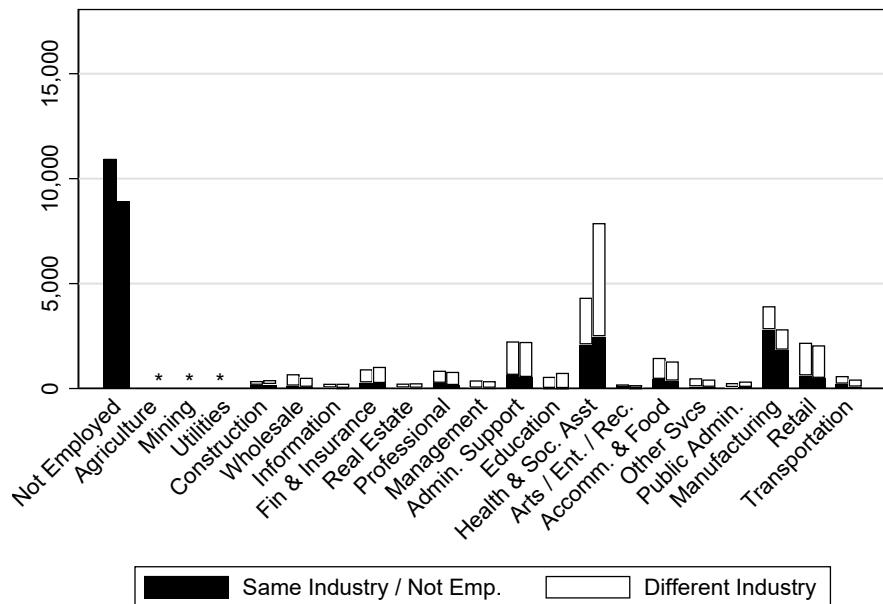
(B) Men



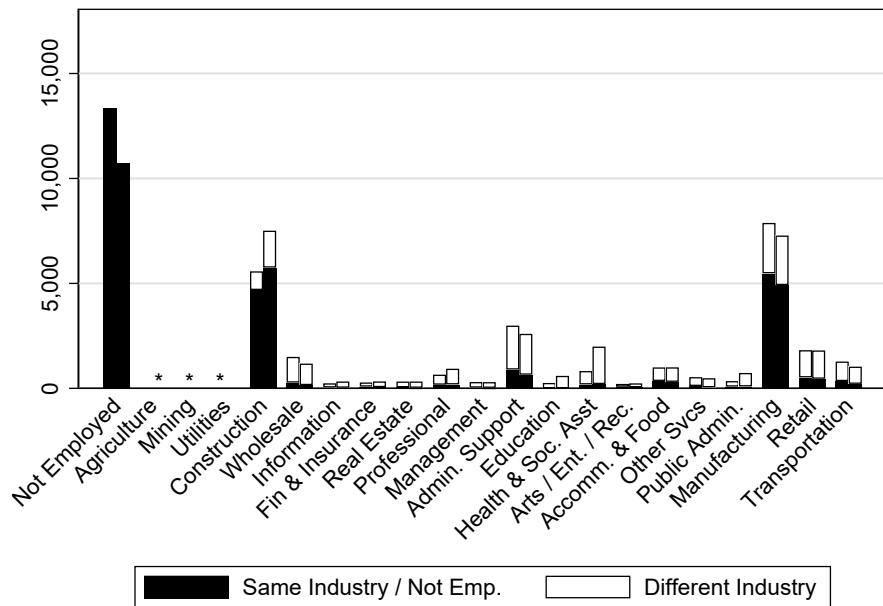
Notes: These figures plot the number of enrollee and matched non-enrollee UI claimants employed in each sector (two-digit NAICS) or not employed. Agriculture, Mining, and Utilities sectors have fewer than 200 workers in each enrollee/non-enrollee cell and are not plotted.

Figure A.19: Enrollees and Matched Non-enrollees Who Switched Industry, 16th Quarter Post-Enrollment

(A) Women

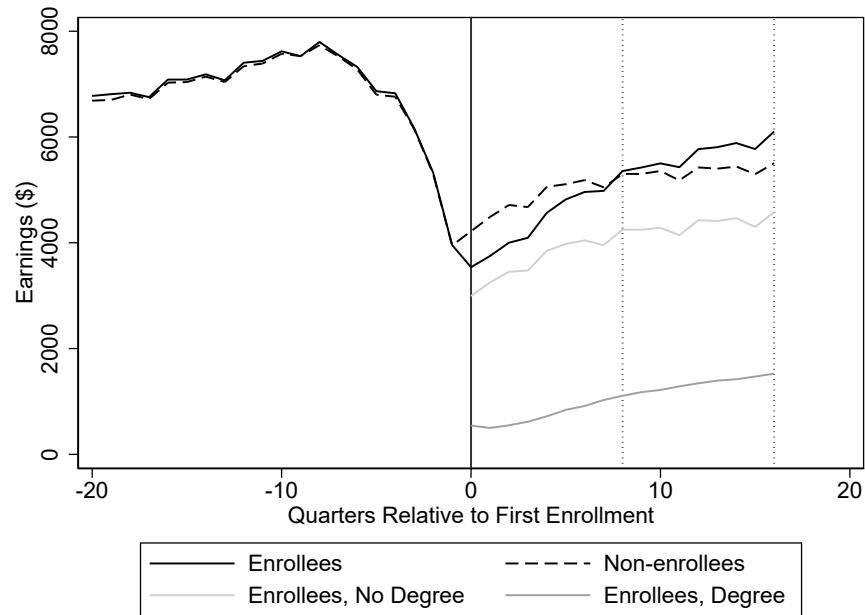


(B) Men



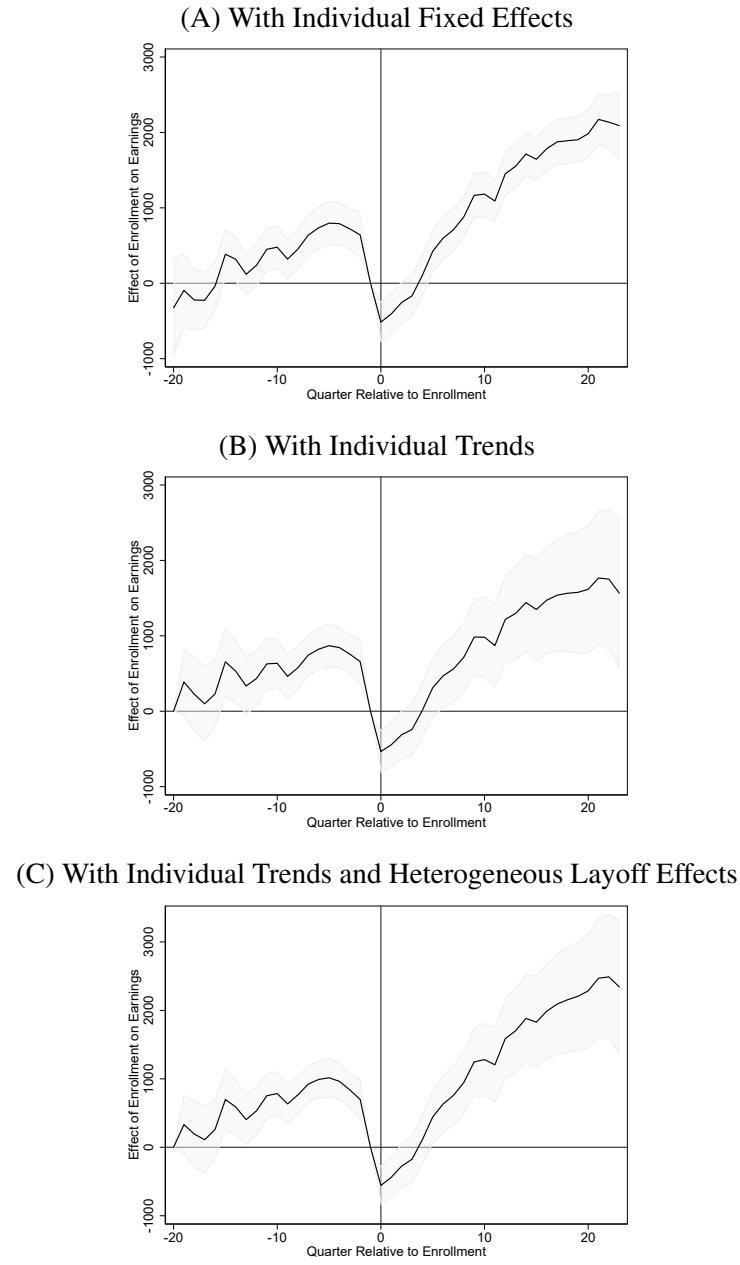
Notes: These figures plot the number of enrollees (right bar of each pair) and matched non-enrollees (left bar) that are employed in a different sector (two-digit NAICS) than their pre-layoff sector. Agriculture, Mining, and Utilities sectors have fewer than 200 workers in each enrollee/non-enrollee cell and are not plotted.

Figure A.20: Decomposition of Earnings Effect by Completers and Non-completers



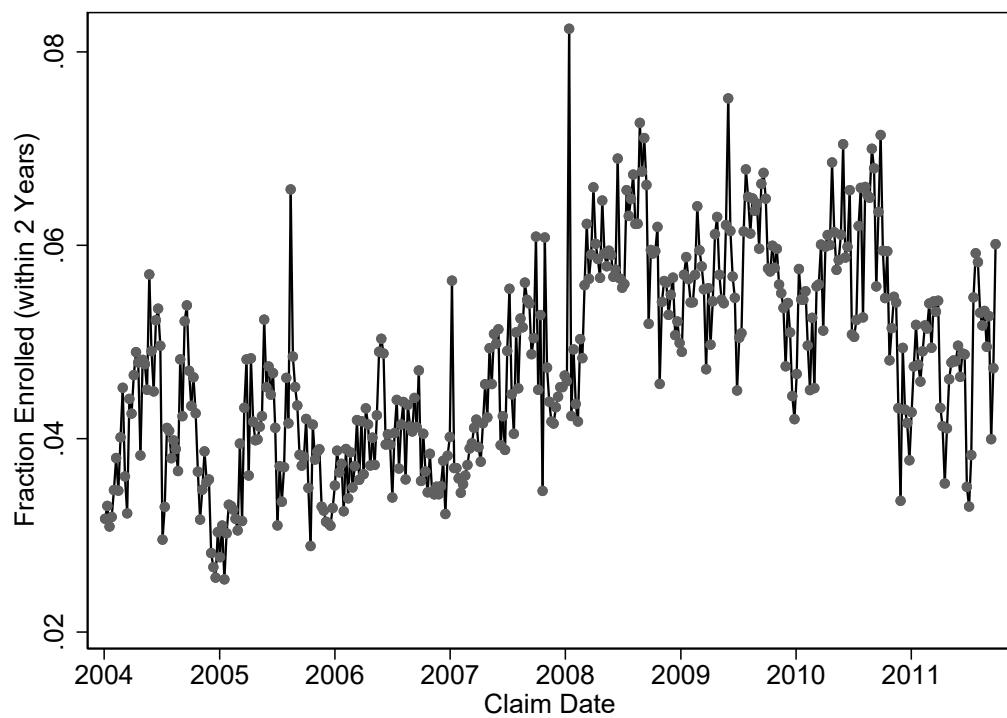
Notes: This figure plots the average quarterly earnings of enrollee and matched non-enrollee UI claimants (black solid and dashed lines). The gray lines disaggregate the post-enrollment earnings of enrollees into two components: average quarterly earnings of those who eventually obtain a credential and those who do not, each scaled by the proportion of obtaining a credential or not. The gray lines sum up to the solid black line. $N = 141,758$, corresponding to 136,074 unique individuals.

Figure A.21: “Event Study” Graphs for Fixed Effect Models (Ohio Sample)



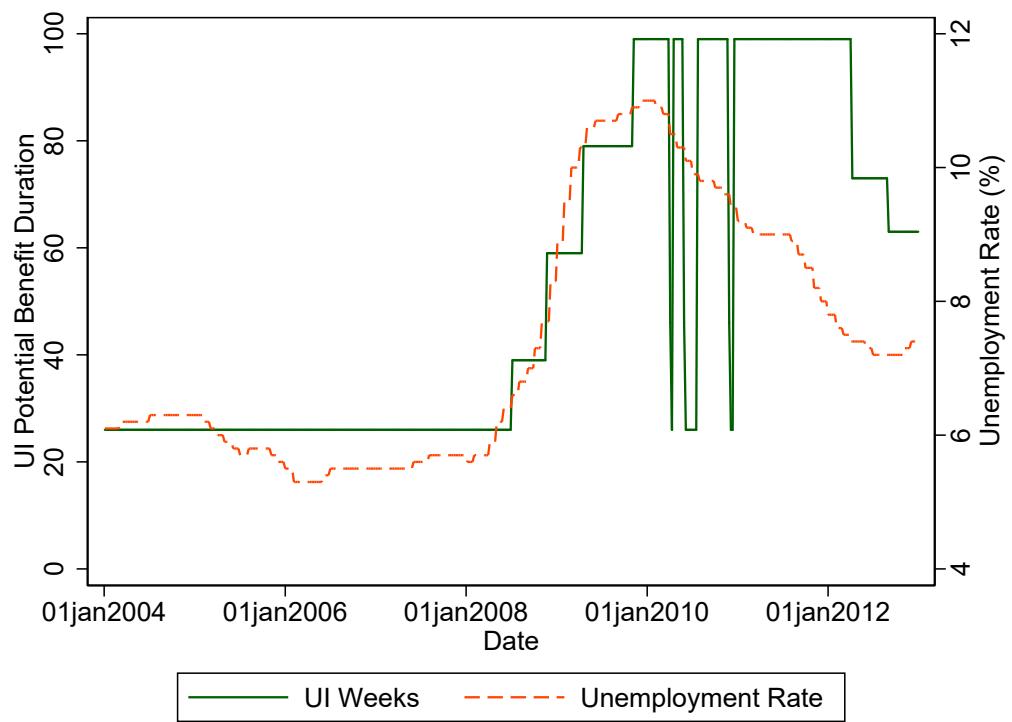
Notes: These figures show specification checks of fixed effect models using five percent of our main analysis sample. Each graph plots the estimated “effect” of enrollment on earnings. The model in Panel A includes individual fixed effects, quarter fixed effects, and indicators for time relative to layoff. The model in Panel B adds individual time trends. The model in Panel C adds heterogeneous layoff effects. Shaded regions are 95 percent confidence intervals, where standard errors are clustered at the UI claim level. $N = 93,528$ UI claims (corresponding to 91,043 unique individuals).

Figure A.22: Enrollment by UI Claim Date



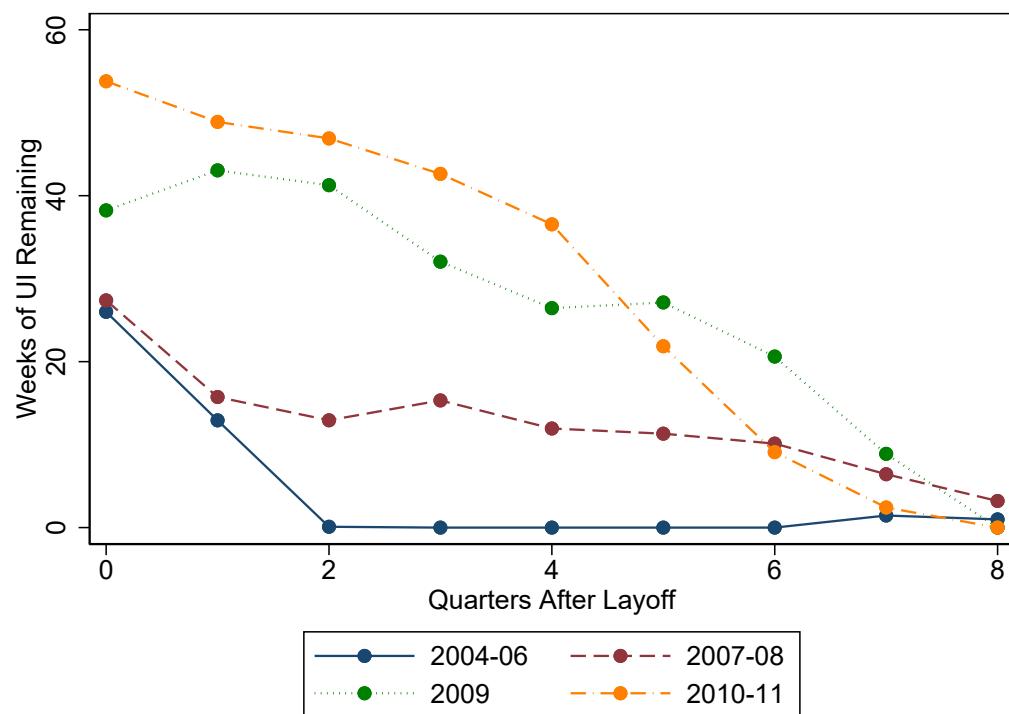
Notes: This figure plots the fraction of UI claimants who enroll within two years of claiming UI, by the date of the UI claim. $N = 1,994,777$, corresponding to 1,335,958 unique individuals.

Figure A.23: Ohio UI Extensions, 2004-2012



Notes: This figure shows the statutory UI benefit duration in weeks (left axis) and the unemployment rate (right axis) in Ohio.

Figure A.24: Simulated Weeks of UI Remaining to Workers of Different UI Claim Cohorts



Notes: This figure shows the simulated number of weeks of UI benefits remaining to various cohorts of UI claimants in five percent of our analysis sample. $N = 99,478$ UI claims (corresponding to 96,874 unique individuals).

Table A.1: Earnings Differences between Later-Enrollees and Matched Non-enrollees

Later-Enrollees That Enroll l Quarters Later	Earnings Difference q th Quarter Before Enrollment							No. of Later- Enrollees
	7	6	5	4	3	2	1	
1							-1169	59484
2						-865	-1393	45826
3					-532	-1049	-1469	34183
4				-624	-859	-1268	-1588	24640
5			-637	-851	-904	-1239	-1642	16971
6		-865	-1016	-1279	-1243	-1537	-1950	10269
7	-597	-1033	-1246	-1237	-1148	-1473	-1705	4611

Notes: This table presents a test of the assumption that later-enrollees have lower potential earnings than similar non-enrollees (Assumption 4, which generalizes Assumption 2). Each cell shows the difference in earnings between later-enrollees and matched never-enrollees. Since we define enrollment as enrolling in one of the eight quarters after layoff, later-enrollees can be categorized as enrolling in $l = 1, \dots, 7$, quarters later. That is, later-enrollees among period-1 non-enrollees ($D_1 = 0$) can enroll up to seven quarters later, later-enrollees among period-2 non-enrollees ($D_1 = D_2 = 0$) can enroll up to six quarters later, and so on. For each of these later-enrollee cohorts (denoted by rows), earnings differences between the later-enrollee and matched never-enrollees are presented. In particular, for later-enrollee cohort l , we report earnings difference for each of the l quarters before enrollment, and the columns denote the quarter $q = 1, \dots, l$ in which the comparison is made. For example, the upper right number shows that for later-enrollees that enroll one quarter later, earnings are \$1,169 lower than similar never-enrollees in the period before they enroll; the last row shows that average earnings for workers who enroll in 7 quarters (i.e. they are in the subset of period-1 non-enrollees who end up enrolling in quarter 8) are lower than their matched never-enrollees for each of the seven quarters before enrollment.

Table A.2: Pre-enrollment Earnings Differences, By Subgroup

Subgroup	Quarterly Earnings					No. of Enrollees	
	1-2 Yrs Pre-Enrollment		3-4 Yrs Pre-Enrollment		t-stat		
	TOT Estimate	(1)	TOT Estimate	(3)			
All	40 (25)	1.63	43 (30)	1.41	70879		
Quarters From Layoff to Enrollment							
1	-12 (70)	-0.18	12 (76)	0.16	11934		
2	-54 (61)	-0.88	-44 (71)	-0.62	13563		
3	59 (61)	0.96	121 (75)	1.61	11736		
4	114 (63)	1.81	100 (80)	1.25	9488		
5	56 (67)	0.83	51 (91)	0.56	7595		
6	202 (66)	3.06	165 (91)	1.81	6577		
7	-5 (76)	-0.06	-39 (106)	-0.37	5516		
8	46 (87)	0.53	-36 (112)	-0.32	4470		
Layoff Year							
2004	13 (78)	0.16	101 (91)	1.11	6017		
2005	168 (93)	1.82	144 (110)	1.31	4632		
2006	135 (86)	1.58	169 (97)	1.75	6747		
2007	4 (75)	0.06	29 (91)	0.32	7480		
2008	2 (55)	0.04	16 (70)	0.23	13897		
2009	-6 (50)	-0.13	-45 (64)	-0.70	17108		
2010	60 (69)	0.87	32 (84)	0.39	9623		
2011	98 (89)	1.11	113 (105)	1.08	5375		
Male	91 (36)	2.55	94 (44)	2.13	39831		
Female	-26 (32)	-0.81	-23 (39)	-0.60	31048		
Manufacturing	80 (50)	1.60	112 (58)	1.93	20626		
Non-manuf.	24 (28)	0.84	14 (35)	0.40	50253		
Age <40	67 (24)	2.78	114 (29)	3.92	46356		
Age >=40	29 (47)	0.61	-85 (58)	-1.47	25364		
Tenure							
<=1 Year	89 (33)	2.73	60 (43)	1.38	24777		
1-6 Years	-67 (34)	-1.96	-33 (42)	-0.79	31951		
>6 Years	273 (60)	4.53	248 (66)	3.74	14992		
White	35 (29)	1.21	32 (35)	0.92	53905		
Black	55 (46)	1.20	192 (57)	3.40	12646		

Notes: This table presents balance tests for enrollees and matched non-enrollees within subgroups. Columns (1) and (3) show the difference between enrollees and matched non-enrollees in the two years prior to enrollment and three to four years prior to enrollment, respectively. Standard errors (in parentheses) and t-statistics for the mean pairwise difference between enrollees and matched non-enrollees are reported.

Table A.3: Enrollment Effects by Year of Layoff: Original vs. Reweighted Estimates

Layoff Year	Quarterly Earnings, 1-2 Yrs Post-Enrollment			Quarterly Earnings, 3-4 Yrs Post-Enrollment			No. of Enrollees (7)	
	TOT Estimate (1)	Nonenr. Mean (2)	Pct. Ch. Diff. (3)	TOT Estimate (4)	Nonenr. Mean (5)	Pct. Ch. Diff. (6)		
<i>Original Estimates</i>								
2004	-421 (84)	5667	-7%	205 (90)	5345	4%	6017	
2005	-43 (104)	5903	-1%	388 (101)	4986	8%	4630	
2006	-307 (83)	5268	-6%	131 (88)	4924	3%	6743	
2007	-568 (73)	4562	-12%	105 (82)	4872	2%	7477	
2008	-584 (57)	4518	-13%	390 (66)	5250	7%	13885	
2009	-472 (53)	4895	-10%	351 (60)	5753	6%	17098	
2010	-183 (73)	4914	-4%	472 (85)	5667	8%	9616	
2011	23 (88)	4779	0%	788 (103)	5455	14%	5371	
<i>Reweighted Estimates (holding constant 2004 characteristics)</i>								
2004	-421 (84)	5667	-7%	205 (90)	5345	4%	6017	
2005	-253 (126)	5952	-4%	255 (121)	4934	5%	4630	
2006	-568 (99)	5234	-11%	-109 (106)	4885	-2%	6743	
2007	-631 (95)	4578	-14%	-13 (106)	4899	0%	7477	
2008	-469 (86)	4337	-11%	389 (101)	5012	8%	13885	
2009	-552 (74)	4867	-11%	176 (81)	5671	3%	17098	
2010	-261 (98)	4915	-5%	497 (111)	5626	9%	9616	
2011	-52 (167)	4875	-1%	759 (196)	5527	14%	5371	

Notes: This table shows the enrollment effects by year of layoff. The lower part of the table shows the enrollment effects where observations are reweighted to match the demographic composition of enrollees in the year 2004. Standard errors (in parentheses) for the mean pairwise difference between enrollees and matched non-enrollees are reported.

Table A.4: Characteristics of Community College and Technical Center Enrollee Subsamples

	Community College	Technical Center
<i>Demographic and Pre-Layoff Job Characteristics</i>		
Female	0.43	0.48
Race		
White	0.73	0.86
Black	0.19	0.08
Other	0.02	0.02
Unknown	0.05	0.04
Prior Industry		
Manufacturing	0.25	0.50
Construction	0.18	0.06
Admin. Support & Waste	0.12	0.09
Healthcare and Social Assistance	0.09	0.07
Retail Trade	0.08	0.07
Accommodation and Food Services	0.04	0.03
Wholesale Trade	0.04	0.04
Transportation	0.04	0.03
Tenure at Recent Employer		
<=1 year	0.36	0.27
>1 years to <=3 years	0.28	0.29
>3 years to <=6 years	0.16	0.16
>6 years	0.20	0.28
Age	35.27	38.91
Cty Unempl. Rate at Layoff	8.24	8.39
Earnings		
1 year before layoff	32371 [21278]	33458 [18810]
2 years before layoff	29592 [22814]	31849 [19582]
3 years before layoff	28126 [23835]	30768 [21121]
<i>Enrollment Characteristics</i>		
Time from job loss to enrollment (quarters)	3.8	3.2
Terms/Quarters Enrolled	4.5	4.7
Types of Courses		
Taken at least one occupational course	88%	99%
Avg proportion of courses occupational	0.59	1.0
Credential		
Bachelors	18%	73%
Associate	1%	0%
Less Than Associate	12%	1%
	5%	72%
Observations	60,261	7,944

Notes: This table presents descriptive characteristics for the subgroups of enrollees whose first institution is either a community college or a technical center. Type of Institution Attended, Terms/Quarters Enrolled, Types of Courses, and Credential are calculated within four years of first enrollment. “Less than Associate” credentials include less than two-year awards from HEI and any credential from OTC. Standard deviations are in brackets.

Table A.5: Characteristics of WIA Participant Subsample

Demographic and Pre-Layoff Job Characteristics	
Female	0.47
Race	
White	0.83
Black	0.12
Other	0.02
Unknown	0.04
Prior Industry	
Manufacturing	0.51
Construction	0.04
Admin. Support & Waste	0.08
Healthcare and Social Assistance	0.05
Retail Trade	0.06
Accommodation and Food Services	0.02
Wholesale Trade	0.04
Transportation	0.06
Tenure at Recent Employer	
<=1 year	0.25
>1 years to <=3 years	0.28
>3 years to <=6 years	0.17
>6 years	0.30
Age	40.65
Cty Unempl. Rate at Layoff	8.61
Earnings	
1 year before layoff	37669 [20773]
2 years before layoff	35878 [21252]
3 years before layoff	35213 [22484]
Enrollment Characteristics	
Time from job loss to enrollment (quarters)	3.2
Terms/Quarters Enrolled	5.2
Type of Institution Attended	
Technical Center	45%
Community College	61%
Types of Courses	
Taken at least one occupational course	98%
Avg proportion of courses occupational	0.8
Credential	58%
Bachelors	2%
Associate	16%
Less Than Associate	42%
Observations	8,312

Notes: This table presents descriptive characteristics for the subgroup of enrollees who received WIA training services. Type of Institution Attended, Terms/Quarters Enrolled, Types of Courses, and Credential are calculated within four years of first enrollment. Enrollees may attend more than one type of institution over the four-year period. “Less than Associate” credentials include less than two-year awards from HEI and any credential from OTC. Standard deviations are in brackets.

Table A.6: Effect of UI Potential Duration on Enrollment

	Dependent Variable: Enrolled			
	(1)	(2)	(3)	(4)
Benefit Duration (10 Weeks)	0.0015 (0.0002)	0.0015 (0.0002)	0.0010 (0.0002)	0.0022 (0.0003)
Benefit Duration at Layoff (10 Weeks)		-0.0003 (0.0004)		
UI Claim Fixed Effects			X	
Includes Only First Unemployment Spell				X
Dependent Variable Mean	0.0153	0.0153	0.0153	0.0207
Observations (UI Claim - Quarter)	793,448	793,448	793,448	365,664
Observations (UI Claims)	99,181	99,181	99,181	79,589

Notes: This table shows the estimated effect of UI benefit durations on enrollment over the eight quarters after filing a UI claim. Additional controls include: (all columns) indicators for quarters post layoff, year indicators, quarter-in-year indicators, quarterly state unemployment rate (quadratic); (all columns except (3)) female, 10-year age category, Black, Hispanic, indicator for having dependents, prior year wage quintile, tenure category, 2-digit prior industry, 2-digit prior occupation. Standard errors are clustered by individuals and in parentheses. There are 96,583 unique individuals in the regressions.