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ABSTRACT

Outcome Mechanisms for Improved Employment and Earnings through Screened Job Training: Evidence from an RCT*

This study fills a gap in the literature on the outcome mechanisms in which successful training programs improve employment and earnings, such as raises on the job or longer job duration. The city of New Orleans implemented a job training program as an RCT for low-income workers. Individuals in the treatment group were more likely to work in the target industries and move out of low-skill industries. In the first 9 months after training, the treatment group experienced higher earnings with new employers and with existing employments. After 9 months, the effects were driven by higher probability of staying with an employer (with now-higher earnings). Findings encourage patience on the part of trainees and the government, as workers may not find their stable, target employment immediately. Government may also want to find ways to improve early connections with employers after training.

JEL Classification: J24, J38, J64, M53
Keywords: job training, RCT, disadvantaged workers

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1. Introduction

The U.S. labor market has become increasingly focused on knowledge-based industries, which require higher levels of technical skills. There is a resulting widening of the gap between the skills needed in the marketplace and those available from existing workers, with companies unable to fill vacancies and grow at the rate in which they would like. As a result, the wage disparity between high- and low-skilled workers has grown (Acemoglu 2002). Many workers with few or obsolete skills face diminishing demand, and worsening labor outcomes. These forces have combined to increase the polarization of job quality and income, leading to widening inequality (Autor and Dorn 2013; Autor, Katz, and Kearney 2006; Goos and Manning 2007; Goos, Manning, and Salomons 2014).

Providing effective pathways for unemployed and underemployed workers into sustainable and sustaining employment is a critical policy issue to combat both poverty and inequality. One policy solution for this challenge is to increase access to training programs that increase the human capital of these workers, better-situating them in the workforce landscape with in-demand skills. There are several types of programs aimed at workforce development through increasing human capital, including formal post-secondary education (four-year and two-year college), vocational and career education, apprenticeships, job training programs, and on-the-job training. These differ in the funder, the provider, and the format of the training. These types of programs have shown promising returns in some settings, albeit not in every instance. Publicly-subsidized training programs are typically medium-length (often between one week and one year) and have training provided by various entities, including community colleges, private training organizations, employers, and the government itself. Such programs have existed in the United States for several decades and have a robust research base. The existing literature has found that the programs can be effective, but are not always so (Van Horn, Edwards, and Greene 2015; Fortson et al. 2017).

Two of the issues that hurt the effectiveness of training programs are high attrition and mismatches between skills provided and local labor demand. To that end, the New Orleans Office of Workforce Development (OWD) designed and implemented *Career Pathways*, funded by a US Department of Labor (DOL) Workforce Innovation Fund (WIF) grant. WIF grants are authorized under the Workforce Innovation and Opportunity Act (WIOA) passed in 2014. Career Pathways offered training for unemployed and underemployed individuals relevant for medium-skilled jobs in the advanced manufacturing, health care, and information technology (IT) sectors. OWD purposefully chose the industries and developed the curriculum in coordination with local firms, critical features of successful programs referred to as demand-driven training. Further, to combat program attrition, potential trainees were screened by various profiling tools before they were allowed entry into the randomization pool and thus into training. For evaluation purposes, the program was implemented as a randomized controlled trial (RCT).

In this paper, we examine new and important aspects of the impact of the Career Pathways program. In Baird et al. (2019), we evaluated the Career Pathways program and found it to lead to increased earnings, especially for those unemployed at entry into the program. We add to those findings and to the overall literature on effectiveness of job training programs in several ways. First, while we are examining the same intervention as Baird et al. (2019), we obtained more data than used in that report, expanding from their analysis of 13 training cohorts to 20 in our paper, as well as examining post-training outcomes for an additional 6 months. Additionally, the program allowed for persons randomized into a control group to enter later randomization pools, resulting in some individuals moving from the control group to the treatment group in later quarters. We account for these individuals in a different manner than in Baird et al. (2019), eliminating the risk of bias from the selection of motivated repeaters out of the control group.

Most importantly, this paper differs from Baird et al. (2019) by evaluating the outcome mechanisms (potential pathways for increased earnings and employment) by which these WIF training programs are and are not effective, including evaluating whether the observed treatment effect arises from differences in wage changes in current jobs, transitions in and out of higher paying jobs, job retention patterns, and/or placement into target industries. These factors are not examined by Baird et al. (2019) or in the literature on the effectiveness of job training programs overall. Understanding the outcome mechanisms is important, as it helps to shed light on how trainees are and are not benefiting, which can be used for developing the appropriate post-training supports (in areas where they are not succeeding), and in building best practices. These results can also help illustrate areas in which program administrators and evaluators should be patient, if some outcome mechanisms take several months to arise or stabilize. The results also can be used for recruitment efforts, for trainees, trainers, and target employers. Altogether, understanding these heretofore ignored outcome mechanisms of job training programs provide rich nuance into how successful programs can benefit workers.

We find that the training is effective in terms of increasing earnings (average treatment effect (ATE) estimate using an intent-to-treat (ITT) design of \$436/quarter for about a 12 percent increase over the control group; the local average treatment effect (LATE) estimate using an instrumental variable estimator for those that completed training is \$1,054/quarter for about a 30 percent increase over the control group. The effectiveness of the program is significantly higher for workers who enter the program with no job, a group of particular interest. For them, the ATE and LATE estimates are \$1,450 and \$2,501, respectively. This group also has positive and significant estimates for the impact of the program on the probability of being employed.

For outcome mechanisms, treated individuals have a longer average duration in higher-paying jobs and a higher probability of employment after training, with these effects again strongest for those entering training with no employment. Additionally, in the short term (first nine months), treatment effects arise both from starting with a new employer that provides higher earnings and from pay-raises by current employers. Medium-term effects are instead driven by higher probability of staying with an employer. Finally, the treatment group individuals are likely to find employment in target industries related to their training area, and less likely to be working in low skill, low wage industries such as food services and drinking places. This suggests that the trained workers are developing new human capital. There is an increase in this effect over time. These findings suggest that the program is effective and is reaching the most disadvantaged workers where other programs have struggled. Our findings also encourage patience for trainees finding employment in target industry and finding stable employment with the same firm.

The paper proceeds as follows. Section 2 describes the prior literature regarding job training programs for adults. Section 3 discusses the context and the training program as well as the data sources used in this analysis. Section 4 describes the methodology used in the paper, while section 5 presents the results of our analyses. Section 6 concludes.

2. Prior Literature

The literature on evaluations of job training programs has generally been positive, with gains to employment and earnings for participants (Council of Economic Advisers 2019; Roder, Clymer, and Wyckoff 2008). However, we note below some differences between estimated training impact for programs aimed at dislocated versus overall adult population workers.

The U.S. Department of Labor funds innovative training programs through the Workforce Innovation Fund (WIF), authorized by the Workforce Innovation and Opportunity Act (WIOA) signed into law in 2014 and its predecessor, the Workforce Investment Act (WIA). WIF programs are of two types: Adult programs and Dislocated Workers programs. Individuals may qualify for the Dislocated Workers program in several ways, including but not limited to being laid off (either by facility closing or otherwise) while being unlikely to return to work in that industry, being self-employed but without significant work opportunities, and or having relied on the income of another family member who no longer provides such income (Department of Labor 2017). Adult programs make no such requirement. Although our study evaluates an intervention in the Adult program and not the Dislocated Workers program, we include a subgroup analysis of a comparable population of workers by investigating the impact of the program for those without a job at entry into training. Therefore, we review the literature for both Adult and Dislocated Worker training programs.

The most comprehensive study of training under the WIA–funded programs is reported in Fortson et al. (2017), a follow-up on McConnell et al. (2016). Fortson et al. report on an RCT design similar to our study, but did so across 28 workforce investment boards' training programs. They found statistically significant increases of over $600 a quarter in earnings from treatment assignment after training. However, these results only are evident in the survey responses of earnings; using administrative data, there is a smaller, positive, and not statistically significant effect on earnings from training assignment. They also found that providing training was not cost effective, which they explain as partially due to a substantial fraction of members of the control group finding alternative ways to fund participation in the same training programs (participation rate of about 40 percent in job training, compared to around 50 percent of the treatment group. In our study, while control persons could seek training on their own (including other programs from the same training providers), they were not eligible for participating in the WIF training program cohorts even if they paid for it themselves. However, Fortson and colleagues found that providing intensive services without training was cost effective. They further found no positive returns for individuals assigned to training in the Dislocated Worker program.

Earlier evaluations of WIA also provide a useful benchmark for the present program, although they have often relied on quasi-experimental methodology. Heinrich et al. (2013) evaluate WIA training programs using quasi-experimental design methods in twelve states for up to four years after the start of the programs. They find moderately-sized, positive, and statistically significant treatment effects on employment and earnings. For example, they estimate quarterly earnings increases of $591 for women and $419 for men (around a 25 percent and 15 percent increase over base earnings for women and men, respectively) for the WIA Adult program. Employment rates see an average of around six percentage-point increases. However, they find no returns in the WIA Dislocated Worker programs, available to those that had been laid off. Thus, those potentially most in need of the program benefitted the least. These results are similar to Andersson et al. (2013), who in investigating a different set of WIA programs find similar effects: for the adult classification, earnings effects are around a $300-$600 increase in quarterly earnings overall in the first three years after training, with the estimated increase for the third year being $1,300-$1,700, while the increase in employment rates is around two percentage points. However, as in Heinrich et al. (2013), they find a worse outcome for trainees in the Dislocated Workers program, finding significant average earnings losses over the first three years (although with gains in the third year). While not a WIA program, Hollenbeck (2012) finds a postsecondary CTE training program yielding around a $1,500 increase in quarterly earnings. Andersson et al. (2013) note that it remains a puzzle why the Dislocated Worker program tends to be less successful in generating increased income and employment, compared to the Adult program, especially considering the services provided are very similar and the characteristics of the trainees do not differ nearly as much as their impact estimates.

One characteristic of job training programs that has been shown to be effective is having it be demand-driven: training programs that are highly integrated with the local labor market and private sector. In fact, it is one of the changes made in the transition from WIA to WIOA training programs (Fortson et al. 2017, p.5). Demand-driven strategies consist of implementing services and activities—including job training—that focus on the needs of specific sectors. In other words, these training programs are based on market-aware strategies that identify sectors that have unmet needs for workers and provide training to low-income workers to acquire the needed skills to fill the available positions (Roder, Clymer, and Wyckoff 2008). The results obtained in demand-driven training programs are encouraging. For example, Maguire et al. (2010) evaluated the employment and earning impacts of three demand-driven training programs and found gains not only from working more hours but also from higher wages. Van Horn, Edwards, and Greene (2015) provide a more in-depth review of this literature.

This paper contributes to the research in two important ways. First, it adds to research on the effectiveness of job training programs using the rigorous methodology of an RCT, and with a special attention to if and how those most disadvantaged upon entry benefit. Most of the literature on the returns to training programs in the last few decades, as described above, have relied on quasi-experimental methods, with the primary exception being the Fortson et al. (2017) evaluation. Nonexperimental methods to evaluate social programs can be problematic and fragile, as discussed in LaLonde (1986). RCTs do have their limitations: they are at times "black boxes," showing whether an intervention was successful without necessarily shedding light on why or how it succeeded, limiting replication (see List and Rasul 2011, Heckman and Smith 1995, and Heckman 2000 for a discussion of these limitations). However, this is no more true for RCTs than it is for quasi-experimental methods, and experimental estimates continue to be the gold standard in estimating causal effects.

Our second contribution is to take a closer look into the outcome mechanisms through which the program works. We examine the possible ways through which earnings can increase, and how that changes over time following training. Additionally, we look at placement into the target industries, as well as the effect of training on job duration post-training, and how that varies by type of job and employment history. The placement into the target industry sheds light onto the question of whether the participants are developing skills that are currently demanded in the local labor market. This is typically not studied; however, Fortson et al. (2017) looked at this in 28 WIA programs, and found the treatment group were not more likely to work in the target occupations.

3. Context

3.1. Description of the Career Pathways program

The Career Pathways training program recruited over 600 individuals and offered training to over 300 individuals across 25 cohorts in one of three training pathways (advanced manufacturing, health care, and information technology) occurring between 2016 and 2018. OWD recruited potential candidates using several methods, including local One-Stop Centers where individuals came for help with employment and public programs such as Unemployment Insurance and SNAP, fixed tablet stations placed in targeted local hotspots throughout the city, targeted outreach through paid advertising and online marketing campaigns (e.g., Facebook, Craigslist, Twitter, and radio ads), and existing community partnerships.

Interested candidates for training were then screened for inclusion in the study. The screening process differed slightly across cohorts, but overall addressed the goal of identifying candidates more likely to persist in the training program, through literacy and numeracy readiness, as well as dependability and availability. The screening process typically included four components: (1) attendance at a mandatory orientation, (2) drug testing (and at times a criminal background check), (3) completion of a relevant assignment or test to determine basic literacy and numeracy (such as the Test of Adult Basic Education, or TABE) and (4) completion of a structured and scored 45-minute interview with OWD about their work readiness.

OWD used these four tools to gauge candidates' interest in the career pathways and likelihood for successfully completing the training program. Those deemed likely to succeed were placed into the randomization pool, while those unlikely to succeed were directed to other government programs and benefits. While selection of trainees is always a consideration of any training program, this rigorous level of determination and selection is uncommon. In Fortson et al.'s 2017 review of 28 programs, they found that sites used assessment tools, including the TABE, but did so for information on how to support the trainee and not as a selection criteria. Fortson et al. recommend that these kinds of assessment tools be used to select trainees in much the way that Career Pathways did, which may be related to the better findings in this study.

Candidates approved for training after the screening were invited to attend an additional meeting, wherein they were informed that in order to qualify for training, they had to sign a consent form in which they agreed to be part of the study whether or not they were randomly assigned to the treatment status. For those that consented, half were randomly selected to be invited to the training program, and half were not. Randomization was done using random numbers within strata defined by gender, annual income in the prior year being above or below $5,000, employment status at that time, and age being 35 years or older. Training typically started within a week after randomization so as to minimize attrition at this stage.

The training was typically structured as in-classroom instruction, for 20 hours across five days a week for a total of two months. In almost every cohort, individuals were then offered an additional two months of training to build on the first round. The training program represented a coordinated effort between the OWD and the training providers, which included a local community college, established professional training providers, and local firms, including a health network and Goodwill. OWD engaged with potential employers through Trade Advisory Committees, meeting quarterly, to obtain an understanding of their workforce requirements and hiring needs and to develop the training. At the end of each 2-month round of training, candidates in most cohorts were invited to test for at least one industry-based credential in the form of a certification. The medium-length duration of the training served both as a challenge (skills desired may take significantly longer to develop) and an opportunity (workers were more likely to be able to afford the opportunity cost of working less during a shorter training), and was chosen to strike a balance between these two competing issues.

3.2. Data

Our data come from two sources. First, in order to enter a randomization pool, individuals had to fill out a brief baseline survey with questions about their demographics and current employment and earnings. They also provided their social security numbers (SSN) and consented to their employment outcomes from the state of Louisiana. This enabled the second data source, which contains employment, earnings, and industry records merged on SSN from the Louisiana Workforce Commission (LWC) for each individual for each employers they worked that was reported to the state for each quarter between and including the first quarter of 2014 and the first quarter of 2019, for 21 total consecutive quarters of data for each individual. Most of these quarters were before the start of training, although the exact number that were before and after depends on which cohort an individual entered into.

The two data sources provide somewhat difference perspectives on employment and earnings history. The baseline survey question about current work status at time of randomization does not exclude self-employment or employment in jobs not covered by the Louisiana unemployment insurance system, such as farm labor, work in the informal labor market, or "under-the-table" employment not reported to the state, and is with respect to the moment, and not employment over the prior quarter. Similarly, the survey question about annual income does not exclude earnings from these same jobs. Meanwhile, the LWC data only allows us to observe work status in covered jobs at any point during the quarter of program application (and several quarters prior), but not at the moment of program application, as the baseline survey allows. However, the baseline survey does not contain data further back from the moment of the survey. We use both sources of information to measure the prior labor history of applicants, use the survey for control variables, and use the LWC data to measure the outcomes.

We define a cohort as a group of individuals entering the same randomization pool for a specific training program together at the same time. There were 25 training cohorts across the Career Pathways program. The first was a pilot cohort and not randomized, and so is not included in the analysis. We also drop the next two cohorts, given large changes in aspects of Career Pathways (in particular, in how they are recruited and screened) between these first two cohorts and the subsequent 22 cohorts. The last two cohorts are not included in this study because we were unable to acquire data on their post-training outcomes. This leaves us with 20 cohorts on which to evaluate the effectiveness of the program. OWD allowed for individuals that were randomized into treatment to re-enter later randomization cohorts. Some of these thus were later randomized into treatment. For the purposes of this paper, we define treatment status according to their first treatment assignment, even if a control individual was later randomized into treatment for a subsequent cohort. As we discuss in Section 3, this controls for non-random selection biases.

Table 1 provides information on the number of cohorts, individuals, and observations for different pathways in the analytic sample. Overall, 197 people were assigned to be in the treatment group, and 192 in the control group. This excludes individuals in later cohorts that were in an earlier cohort's control group, as discussed. However, given treatment assignment in every cohort is random, these excluded observations from later cohorts will be randomly taken out of both the treatment and control, ending with balance both in terms of numbers of people in each group and demographics of each. About half of the individuals were in the advanced manufacturing pathway, and a quarter in each of health care and information technology. Overall, there are almost 2,000 post-training quarterly observations in our analytic sample.

Table 1: Training Pathways Counts of People and Person-Quarters

Pathway	Area	Cohorts	Number of individuals assigned treatment	Number of individuals assigned control	Post-training quarterly observations
Advanced manufacturing	Electrical	5	77	67	876
	Pipefitting	1	5	6	22
	Welding	1	11	8	57
Health care	Medical billing and coding	4	32	32	176
	Patient access representative	1	19	20	195
Information technology	Information technology	8	53	59	575
Total		20	197	192	1,901

We also have measures of whether individuals attended at least one training class and whether they completed that first round of training (Table 2). 18 percent of individuals assigned to training (35 people) did not show up to even one class. There is also non-compliance with initial treatment assignment among the control group. This happens as individuals in the control group (a) return into a later cohort, (b) are randomized into treatment, and (c) attend at least once and/or complete. Here, the level of non-compliance also is around 18 percent. For our main outcomes, we estimate the LATE model using two different measures of participation – whether the individual attended at least one training class and whether they completed training. For additional outcomes, we only present LATE estimates using the indicator of whether the individual completed model because it is the more relevant measure related to participation in the program. Note that 79 percent of those that attended at least one class completed the first round of training.

Table 2: Treatment Compliance

		Attended Training 1		Completed Training 1		
		0	1	0	1	Total
Treatment Assignment	0	166	26	170	22	192
	1	35	162	71	126	197
	Total	201	188	241	148	389

Note: Attended training refers to whether the individual attended at least one class in the first round (2 month) training. Completed training refers to whether the individual completed the first round of training.

A further complication is that not all SSNs in the baseline surveys were matched onto LWC records. There could be three reasons for this: they might never have worked in the state of Louisiana between January 1st, 2014 and March 31st, 2019, they might only have worked in unreported jobs, or there may have been an error in the SSN from the baseline survey (either in what was provided or in the digitization of it). We strongly suspect that most if not all cases are due to the last scenario of incorrect SSNs.[1] This accounts for 28 individuals that are not included in the analysis above the 389 we include. Although there is near-zero difference in probability by treatment status and no statistical relationship, we generate and use non-response weights for the

[1] The LWC data covers a period of over five years in which we found no matches, and the set of individuals with no match in LWC did not change across waves of LWC data acquisitions. That is, there were no additional individuals not observed at all with the addition of a new year of data. Also, 33% of respondents who were not in the LWC data reported in their baseline survey at time of randomization that they had partial or full employment in the month prior to the randomization, and 51% of the same group mentioned that they had personal income over $5,000 in the last year.

remaining subjects to account for these individuals.

Table 3 presents the baseline characteristics of the sample we use for this analysis, as well as the employment and earnings from the LWC data prior to training and after training. We find that there is no statistical difference between the treatment and control group on any of the baseline dimensions, and that the effect sizes are quite small. Thus, while we will covariate-adjust for all of these variables in the regression to increase precision, they are not necessary to correct for imbalances between treatment and control groups. The table also reveals attributes of the sample—that they are predominantly Black, of various ages, and have diverse labor backgrounds, with almost half not holding a job at the time of the interview and nearly half having a prior year's income below $5,000. The average quarterly earnings (not conditioned on working) was around $3,500, implying an average annual earnings of around $14,000.

Table 3: Summary Statistics and Baseline Equivalence

	Control	Treatment	Difference	Std. Error of Diff.	Effect Size
Baseline characteristics					
Male	0.522	0.570	-0.048	0.164	0.096
Over 35 years old	0.535	0.584	-0.050	0.046	0.100
Working	0.517	0.504	0.013	0.051	0.026
Prior year's income>$5,000	0.620	0.638	-0.018	0.064	0.037
Black	0.886	0.921	-0.035	0.033	0.119
Age	39.066	40.160	-1.095	1.212	0.097
Proportion of prior quarters employed	0.614	0.625	-0.012	0.045	0.030
Average prior quarterly earnings	3815	3480	335	491	0.084
Outcomes					
Employed (post training period)	0.639	0.690	-0.051	0.048	0.108
Quarterly earnings (post training period)	3567	3909	-343	470	0.083

Note: Proportion of quarters employed and average quarterly earnings are based on LWC records for 0.5 years to 2.5 years prior to the start of training. Standard errors are clustered at the cohort by treatment status level. None of the associated p-values are significant at the 10 percent level. Effect sizes are the difference divided by the pooled standard deviation.

4. Methodology

Leveraging the RCT design, we evaluate the impact of being randomly assigned training on employment outcomes, while including several additional control variables to increase precision. Equation 1 presents the primary model used.

$$Y_{itc} = \alpha + \beta\ Treat_i + \lambda Y_{iB} + X_i\gamma + \psi_C + \phi_t + \varepsilon_{itc} \tag{1}$$

Y_{itc} is the dependent variable. It is either a binary variable indicating whether individual i in cohort c is employed in quarter t, or a continuous variable giving the dollar amount of their earnings from all reported jobs in quarter t (including zeros for quarters the individual had no reported earnings). For each individual, we use all quarters after the end of their cohort's training period through the final quarter available, leading to multiple records across time for individuals. $Treat_i$ (whether or not individual i was selected and invited for training) is the independent variable of interest (with independence ensured through randomization). The covariate Y_{iB} represents the average outcome for the period 0.5 to 2.5 years prior to randomization, or in other words, at baseline. More specifically, it represents the fraction of quarters with at least one job as reported to LWC (for the regressions of employment status) or average quarterly earnings (for the regressions of earnings) from 2.5 years to 0.5 years prior to the randomization.

We additionally control for the baseline characteristics that defined the strata used for randomization (the fully interacted set of variables for gender, baseline employment status, baseline annual income (less than or greater than \$5,000), and baseline age (younger or older than 35 years old), represented by X_i, and fixed effects for each cohort, represented by ψ_C. We also include time (year by quarter) fixed effects (ϕ_t). ε_{itc} is the random error term.

We estimate equation 1 using OLS regression for the ITT ATE model. As discussed in section 2, individuals randomized into the control group were allowed to enter subsequent randomizations and potentially enter into treatment. We only include their treatment assignment from their first cohort to obtain identification of the effects free from selection bias. Thus, there are people in the control group that are later treated in a subsequent cohort. Removing them from the control group would create a biased sample of control groups, as they are non-randomly deciding to re-enter later cohorts. They may be more motivated, which would yield an upwards bias on the treatment effect by removing more motivated control group members which later on have better labor outcomes. On the other hand, they may have worse employment opportunities after being in the control group than those that opt not to return, which would downward bias the treatment effect by excluding them. The ultimate direction of the bias would be unclear; instead, we keep them in the sample with their control group status.

We also estimate LATE models, for two definitions of treatment: attendance in at least one session of training, and completion of the first round of training. We do so using two-stage least square regressions. This allows us to account for the fact that there is non-compliance in both directions (treatment persons never attending, and control persons entering later treatment groups). These outcomes are endogenous to their labor market opportunities and unobserved motivation and ability. Thus, we use two-stage least squares and instrument for attendance and completion with initial treatment status.

We generate non-response weights to account for the 28 SSNs we were not able to match to the LWC records. We fit a logistic regression of non-missing (matched SSN) status onto the person's treatment status and the four randomization strata of baseline age, sex, income, and employment status. Using this fitted regression, we predict the probability of being a non-missing response and generate the regression weights as the inverse of this predicted probability.

To estimate effects differently for those that had a job before randomization versus those that did not, we use equation 1 but interact treatment with baseline work status. We additionally examine several other outcomes for the mechanisms, such as the probability of staying with a specific firm from one quarter to the next. Doing so, we again use equation 1, but with these other outcomes. We also examine what industries individuals end up working in, with the goal of examining if trainees are more likely to work in the target industries. To do so, we again use equation 1, but with the outcome of whether they work in a given 3-digit NAICS code; we separately estimate the regression for each of the 3-digit NAICS codes in which at least one worker is employed in our analytic sample. We control for the same covariates, including an indicator for them having worked in that given industry in the past (Y_{iB}). We then sort the coefficients by size, and report the largest three and smallest (most negative) three as a measure of the impact of training on employing industry.

To estimate the impact of training on job duration, we estimate a Cox proportional hazard model, with one observation per job per Study ID in the post-period. We evaluate each job that is held after the start of training for each individual. The model is represented by equation 2, where $h_{icj}(t)$ is the hazard for person i in cohort c of leaving a job t quarters after starting job j, $h_0(t)$ is the baseline hazard function, and $HadAtStart_{ij}$ is an indicator for individual i holding job j at the start of training. The other variables are as defined above.[2]

$$h_{icj}(t) = h_0(t)\exp(\beta Treat_i + X_i\gamma + \delta HadAtStart_{ij} + \psi_c) \tag{2}$$

[2] We also tested a model where instead of including a control for whether they had the job at the start, we dropped any job that they were already in at the end of training. While we do not present the results here, the estimates were very similar, and can be provided upon request.

Finally, we conduct analyses to investigate potential outcome mechanisms for treatment effects on earnings (including zeros for those without reported employment). We examine seven potential outcome mechanisms. For most of these analyses, the unit of observation is no longer person by quarter, but is expanded to person by quarter by employment with a firm, which is the raw unit of observation in the LWC data. In all cases, it is the sample and outcome that vary, and the model and methodology do not change and are in all cases OLS or 2SLS using the specification from equation 1.

First, we estimate the probability of retaining a job with their existing firm. Insofar as they retain jobs more, this may lead to higher earnings. To do so, we limit the sample to jobs held in period t-1, and use as the outcome an indicator for still holding the job in period t. Second, we examine the probability of starting a new job. Starting new jobs, all else equal, may be related to increased earnings. Here, we use person-by-quarter units of observations, and use as the outcome an indicator for having work with a new firm in the quarter. Third, we examine the total number of jobs a person holds in a quarter. The more jobs that are held may lead to higher earnings through more total hours of work, but it may be associated with lower earnings if each one pays less and does not represent more hours. To do so, we tally the number of firms that a person works for (has non-zero earnings) for each quarter, and use the count as the outcome. Fourth, we examine the change in total earnings within a firm-employee relationship. We limit the sample to person/firm/quarter observations wherein the person worked for the same employer in the prior quarter. We then calculate the change in quarterly earnings. Note that this could happen through two ways: either an increase in their (direct or implied) hourly wage rate, or an increase in hours worked. We are unable to separate out these two. The outcome here is the change in earnings within-firm. Fifth, we examine the change in earnings between firms. We do so by limiting the sample to individual and quarters wherein the individual had their last quarter working for one firm (period t-1) and first quarter working for a different firm (period t). We calculate the total earnings in each quarter and use the change in these totals between the two quarters as the outcome. Sixth, we evaluate earnings when departing from a firm. We limit the sample to the final quarter in which the person worked at a firm (that is, they earned zero in the subsequent quarter from that firm), and use as the outcome the earnings from that firm in the quarter. Seventh, we examine earnings at new firms. Here we limit the sample to the first quarter in which the person worked at a firm (that is, they earned zero in the prior quarter from that firm), and use as the outcome the earnings from that firm in the first quarter.

For all analyses, we cluster the standard errors to reflect expected intraclass correlations. We cluster based on cohort by treatment status.

5. Results

5.1. Effects on Employment and Earnings

We first present the overall findings of the program on employment and earnings in Table 4. We find no significant effect on employment, although the results are positive and economically relevant. The LATE estimates for those that attended or completed the first round of training are larger than the ATE estimates, but also are not statistically significant. However, we do find a significant impact on earnings. The ATE estimate for quarterly earnings is $436 (marginally significant). The control group's average post-training earnings were $3,567 (Table 3), therefore this represents about a 12 percent increase in earnings for those assigned training. The effect becomes much larger in the LATE regressions, with the impact for completers ($1,054) representing approximately 30 percent increased earnings over the control group.

Table 4: Main Outcomes

	Employed			Quarterly earnings		
	ATE	LATE attended	LATE completed	ATE	LATE attended	LATE completed
Treatment	0.035	0.057	0.085	436.0*	705.1*	1,054.4**
	(0.026)	(0.039)	(0.054)	(256.7)	(389.1)	(532.0)
Avg. pre-randomization employment	0.252***	0.252***	0.254***			
	(0.068)	(0.065)	(0.066)			
Avg. pre-randomization earnings				0.338***	0.336***	0.330***
				(0.089)	(0.088)	(0.091)
Observations	1,901	1,901	1,901	1,901	1,901	1,901
R-squared	0.114	0.118	0.117	0.204	0.205	0.199
Control group mean	0.639	0.621	0.632	3567	3411	3462

Note: ATE is the average treatment effect, using an intent-to-treat design. LATE is the local average treatment effect for compliers in both the treatment and control group, using random treatment assignment as an instrumental variable. Regressions additionally control for fixed effects for cohort, year-by-quarter, and randomization strata (gender X age over 35 X baseline employment X baseline income over $5,000). Quarterly earnings are US dollars normalized to 2018. *p<.10, **p<.05, ***p<.01 with standard errors clustered by cohort/treatment assignment.

Table 5 presents the treatment effects by pathway. The largest treatment effects were for the five cohorts in health care, where there are significant employment and earning effects. Advanced manufacturing also had statistically significant earnings effects of a similar magnitude to the overall treatment effect. Information technology did not have statistically significant findings for either employment or earnings.

Table 5: Outcome Regressions by Pathway

	Employed		Quarterly earnings	
	ATE	LATE completed	ATE	LATE completed
Advanced manufacturing	-0.014	-0.030	369.0**	869.4**
	(0.028)	(0.070)	(176.4)	(438.3)
Health care	0.169***	0.216***	1,756.4***	2,279.7***
	(0.024)	(0.022)	(316.2)	(266.4)
Information technology	0.027	0.108	-363.6	-1,383.7
	(0.051)	(0.203)	(659.1)	(2,692.1)
Observations	1,901	1,901	1,901	1,901
R-squared	0.119	0.105	0.211	0.213

Note: ATE is the average treatment effect, using an intent-to-treat design. LATE is the local average treatment effect for compliers in both the treatment and control group, using random treatment assignment as an instrumental variable. Quarterly earnings are US dollars normalized to 2018. 95% confidence intervals are presented in brackets. The regressions additionally control for baseline outcomes (e.g., fraction of quarters employed between 0.5 and 2.5 years prior to randomization), gender, age, employment status at randomization, and annual income in the year prior to randomization, as well as cohort and time fixed effects. *p<.10, **p<.05, ***p<.01, with standard errors clustered by cohort/treatment assignment.

We next examine how the effects differ by the baseline employment status of the trainee. We do so by interacting treatment status with an indicator for having a job at randomization. Table 6 presents the results. Those that did not have a job before training, captured by the treatment variable, had large and statistically significant improvement for both employment probability and earnings, while those that had a job at the start did not have a statistically significant effect. For employment, those not having a job at entry into training have a ten-percentage point higher likelihood of employment, which in comparison to the around 57 percent rate in the relevant comparison group (not assigned treatment and do not have a job at baseline) that have a job post-training period, is sizeable. This means the program is helping most those we want to have the largest benefits. It also reinforces the puzzle described by Andersson et al. (2013) for why the WIF Dislocated Worker program has been less successful than the more general Adult population: the program evaluated here is within the Adult program, and the highest benefiters are for those who are more likely to be in similar circumstances as trainees in the Dislocated Worker program. While our individuals can be without a job for several reasons, and not just dislocation, the results are nonetheless encouraging, and it may be that screening in such a way as done in this program would benefit the Dislocated Worker training programs.

Examining earnings, we again find that trainees entering training without a job have large benefits with a treatment effect of $1,450 per quarter; given the average earnings for the control group of those with no job at baseline is $2,604, this represents a significant increase in earnings. The LATE completed estimate is more than a doubling of the comparison group's earnings.

Table 6: Outcome Regressions by Baseline Characteristic

	Employed		Quarterly earnings	
	ATE	LATE completed	ATE	LATE completed
Treat	0.102**	0.249**	1,450.4***	3,539.9***
	(0.044)	(0.101)	(385.5)	(1,211.9)
Treat X working at baseline	-0.134**	-0.322**	-2,017.5***	-4,775.9**
	(0.064)	(0.164)	(715.1)	(2,143.6)
Avg. pre-randomization employment	0.266***	0.279***		
	(0.070)	(0.106)		
Avg. pre-randomization earnings			0.350***	0.359***
			(0.087)	(0.075)
Observations	1,901	1,901	1,901	1,901
R-squared	0.118	0.083	0.218	0.140

Note: ATE is the average treatment effect, using an intent-to-treat design. LATE is the local average treatment effect for compliers in both the treatment and control group, using random treatment assignment as an instrumental variable. Quarterly earnings are US dollars normalized to 2018. 95% confidence intervals are presented in brackets The regressions additionally control for baseline outcomes (e.g., fraction of quarters employed between 0.5 and 2.5 years prior to randomization), gender, age, employment status at randomization, and annual income in the year prior to randomization, as well as cohort and time fixed effects. *p<.10, **p<.05, ***p<.01, with standard errors clustered by cohort/treatment assignment.

5.2. Effects on Job Duration

We next turn to the model which estimates the probability of ending a job in any given quarter based on a Cox proportional hazards model, for the ITT ATE model.[3] The first row of

[3] Note that we attempted to implement an instrumental variables estimator of these effects using the methodology of Martínez-Camblor et al. (2019). However, the procedure did not converge under the same specification as the ITT model, so we do not estimate the LATE estimates for this outcome.

Table 7 reports the hazard ratios when this estimator is applied to the entire sample. There is an overall goal of having longer job duration, such that we would want to see coefficients smaller than one (thus, a smaller probability of ending a job in a quarter). However, there is a tension--we do want workers to leave bad jobs and seek good jobs.

Overall, the coefficient is not statistically significant. However, when we again examine the heterogeneity by whether they held a job at baseline. We find that for individuals that did not hold a job at baseline, treatment leads to a statistically significant reduction in the risk of ending a job in any quarter, being only 80 percent as likely to end a job in any quarter as the control group.

With the desire to see if longer job duration is because they are lasting longer in *better* jobs longer, we test additional versions of the model. We take the average quarterly earnings they have in the job, and see if it falls above or below \$3,015.[4] We chose this threshold because it is the 2019 federal poverty line (FPL) for annual earnings for a single person household, divided by four to get to the quarterly FPL. We then repeat the Cox proportional hazard model for the subgroup of of study participants indicated in the table. We recognize that this approach has the limitation of conditioning on an endogenous outcome (being in a higher or lower paying job). However, we still believe the exercise is instructive and can shed light on the types of jobs wherein treated workers are more likely to persist compared to control workers in the same subgroup. We also include a column in Table 7 which reports the fraction of the observations in the sample that are for those assigned treatment, to give a sense of the magnitude of the potential imbalance due to conditioning on pay from job. Comparing rows 4 and 5 shows that there is a bit of an imbalance, with treated persons having a higher representation among the higher paying jobs (52%, compared to 48% for lower-paying jobs). However, the difference is small.

We examine the hazard ratios by job pay for the overall sample (rows 4 and 5). While not significant for either case, we find that the impact of treatment on job duration is more favorable for the higher-paying jobs. However, once we focus on the population of those without a job at baseline (rows 6 and 7), we find that treatment makes them more likely to leave lower-paying jobs (hazard ratio of 1.24) and much less likely to leave higher-paying jobs (hazard ratio of 0.586).

[4] One problem with this calculation of the average earnings is that it contains the end-point quarters of holding a job. Given we only have quarterly totals, an individual may only hold a job for a fraction of that period, leading to an underestimate of whether the job pays above FPL. However, this is treated symmetrically between treatment and control, and so only changes the interpretation, and not the validity of the estimates.

When we look at those with a job at baseline, the findings disappear. We take this as evidence that treatment is developing human capital that not only raises earnings, but gives them the skills to keep these jobs longer.

Table 7: Hazard Ratio from Cox Proportional Hazard Model for Ending a Job in a Quarter

Sample	Coefficient	Std. Error	Num. obs.	Fraction of obs. treated
All	0.902	0.067	759	0.497
No job at baseline	0.801**	0.073	340	0.468
Job at baseline	1.029	0.091	419	0.520
Jobs with mean earnings below $3,015	1.007	0.063	457	0.479
Jobs with mean earnings above $3,015	0.791	0.116	302	0.523
Jobs with mean earnings below $3,015, no job at baseline	1.242**	0.126	206	0.413
Jobs with mean earnings above $3,015, no job at baseline	0.586***	0.113	134	0.552
Jobs with mean earnings below $3,015, job at baseline	0.896	0.067	251	0.534
Jobs with mean earnings above $3,015, job at baseline	1.087	0.286	168	0.497

Note: Intent-to-treat design. Each cell is based on a separate regression. The regressions additionally control for gender, age, baseline employment status, and baseline income, as well as whether they had held that job at the end of training and cohort fixed effects. *p<.10, **p<.05, ***p<.01, with standard errors clustered by cohort/treatment assignment.

5.3. Effects on Industry of Employment

We next turn our attention to the industries that treated individuals are most (and least) likely to work in compared to the control group. While we estimated the difference between treatment and control for all industries, we only report the top and bottom three industries in terms of size of the coefficient, reported in Table 8. There, we show some evidence that the training is leading trainees to be more likely to be employed in the intended target industry, which we interpret as evidence that the training is developing human capital helpful for finding these jobs. Recall that both the treatment and control group are comprised of individual who self-selected into the training area, and presumably are interested in working in the target industries.

For advanced manufacturing, the top difference industry is specialty trade contractors, where the treatment group is five percentage points more likely to work. This industry is very related to the goals of the training. For health care, the effect of treatment is larger, as the treatment group is over 30 percentage points more likely to work in the hospital industry than the control group; the LATE estimate is almost 40 percentage points for those that attended at least one class. Also encouraging for the health care pathway are the negative treatment effects: in the health care pathway, the training assignment is causing them to be less likely to work in administrative and support services as well as the food services and dining places industry. Thus, they are moving out of jobs that are more likely to be low-skilled and lower paying and into a better career path. We see no such connection between target industry and later employment for the IT pathway, aligning with the null results on employment and earnings. Many industries employ people in information technology occupations so these findings should be interpreted with caution.

Table 8: Largest and Smallest Differences Between Treatment and Control Industry of Employment After End of Training Period (NAICS 3-Digit Codes)

		Cohort's target industry		
		Advanced manufacturing	Health care	Information technology
ATE	Top 3	Specialty Trade Contractors (4.9***)	Hospitals (32.2***)	Food Services and Drinking Places (5.8***)
		Professional, Scientific, and Technical Services (2.8***)	Social Assistance (2.9)	Justice, Public Order, and Safety Activities (4.3**)
		Food Services and Drinking Places (2.1)	Educational Services (2.4**)	Couriers and Messengers (3.8***)
	Bottom 3	Educational Services (-2.7***)	Administrative and Support Services (-5.9**)	Performing Arts, Spectator Sports, and Related Industries (-5.0***)
		Heavy and Civil Engineering Construction (-2.7***)	Food Services and Drinking Places (-5.8***)	Motion Picture and Sound Recording Industries (-4.2***)
		Personal and Laundry Services (-1.7***)	Furniture and Home Furnishings Stores (-4.3***)	Health and Personal Care Stores (-3.9***)
LATE Completed	Top 3	Specialty Trade Contractors (10.9***)	Hospitals (38.2***)	Food Services and Drinking Places (12.7***)
		Professional, Scientific, and Technical Services (4.7***)	Social Assistance (4.5*)	Building Material and Garden Equipment and Supplies Dealers (6.2***)
		General Merchandise Stores (3.0**)	Educational Services (2.8**)	Rental and Leasing Services (5.2**)
	Bottom 3	Educational Services (-4.3***)	Administrative and Support Services (-6.9**)	Educational Services (-12.9***)
		Heavy and Civil Engineering Construction (-4.1***)	Food Services and Drinking Places (-6.5***)	Administrative and Support Services (-7.4***)
		Food and Beverage Stores (-2.3***)	Furniture and Home Furnishings Stores (-5.1***)	Social Assistance (-4.8***)

Note: ATE is the average treatment effect, using an intent-to-treat design. LATE is the local average treatment effect for compliers in both the treatment and control group, using random treatment assignment as an instrumental variable. Each cell is based on a separate regression. The regressions additionally control for having worked in the industry before randomization, gender, age, baseline employment status, and baseline income, as well as cohort and time fixed effects. Treatment effects are in parentheses for the probability of working in that industry (on a scale from 0 to 100). *p<.10, **p<.05, ***p<.01 with standard errors clustered by cohort/treatment assignment.

For the health care pathway, which has both the largest employment and earning treatment effects as well as the highest placement into the target industries, we re-estimated the industry of employment regressions separately for the first three quarters post-training and for the fourth or later quarters post-training, to examine how the results might change depending on length of time elapsed since training. Focusing for example on the ATE effects, we find the difference in probability of working in the hospitals industry to be 26 percent in the first three quarters, but that increases to 69 percent in the fourth or later quarters after training. This is mirrored by the decreased probability of working in the food services and drinking places industry moving from -4 percent to -14 percent. Thus, the impact grows with more time elapsed.

5.4. Outcome Mechanisms of Effects on Earnings

Table 9 presents potential outcome mechanisms for the earnings treatment effect. We estimate the models for all post-training quarters, as well as separately for the first three quarters (approximately 60 percent of the observations) and the fourth or later quarters (the remaining 40 percent of observations). We present the overall earnings effect in the first row for ease of comparison. There, we find that there is not a difference in the ATE between the initial and later quarters, but that there is nearly a doubling of the LATE estimate from initial to later quarters.

We next present the results from seven potential outcome mechanisms for the treatment effects. The first three have to do with changes in employment status. First, we examine the probability that they are retained at a firm at which they worked the prior quarter. This is parallel to the hazard model analysis, but here uses linear regression. We find that there is no statistically significant overall effect, driven by no effect in the first three quarters. There is a large and significant increase in the probability of retaining jobs in the last three quarters however, with a 8 percentage point increase for the ATE and a 22 percentage point increase for LATE. Second, we examine the probability of starting with a new firm. Note that individuals can hold more than one job in the same quarter or even at the same time, and thus can retain existing firms and start at new firms. There, we find an overall decrease in the probability of working at a new firm in a given quarter. This reinforces the findings of small to no overall employment effects but longer job duration—treatment individuals are less likely to be transitioning jobs. This is particularly true in the later period, after the first three quarters. Finally, we also estimate the effect of treatment on total number of jobs held within a quarter. Again, there is no overall effect, although there is a marginally significant negative ATE effect in the later period. This suggests that training eventually reduces the need to hold multiple jobs.

Table 9: Outcome mechanisms for treatment effects

	All post-training quarters		First three post-training quarters		After three quarters post-training	
	ATE	LATE Completed	ATE	LATE Completed	ATE	LATE Completed
Overall effect on earnings	435.990*	1,054.361**	405.211	834.784*	457.849	1,450.683*
	(256.708)	(531.966)	(247.455)	(454.946)	(338.612)	(863.129)
Retaining existing firms	0.025	0.054	-0.011	-0.021	0.080***	0.220***
	(0.024)	(0.050)	(0.032)	(0.060)	(0.018)	(0.078)
Start new firms	-0.010**	-0.023*	-0.003	-0.006	-0.019***	-0.056**
	(0.004)	(0.012)	(0.004)	(0.009)	(0.006)	(0.027)
Number of firms	-0.012	-0.027	0.055	0.110	-0.096*	-0.283
	(0.050)	(0.113)	(0.056)	(0.111)	(0.054)	(0.174)
Change in earnings within-firm	323.188***	701.132***	616.951***	1,193.345***	13.746	35.681
	(87.499)	(204.771)	(139.313)	(285.610)	(170.953)	(419.833)
Change in earnings switch firm	310.112	743.600*	377.577	665.084	284.295	2,962.251
	(218.708)	(438.838)	(280.329)	(425.518)	(367.935)	(3,649.295)
Earnings at departed firm	306.947*	740.298*	399.428**	831.064**	91.786	364.316
	(161.529)	(384.847)	(178.092)	(372.493)	(403.934)	(1,349.218)
Earnings at new firm	438.648**	980.519***	420.791*	790.264*	232.417	833.332
	(173.823)	(373.942)	(236.382)	(422.518)	(275.715)	(904.542)

Note: ATE is the average treatment effect, using an intent-to-treat design. LATE is the local average treatment effect for compliers in both the treatment and control group, using random treatment assignment as an instrumental variable. First three quarters account for about 60% of observations, after first three for 40%. *p<.10, **p<.05, ***p<.01, with standard errors clustered by cohort/treatment assignment.

The final four rows of Table 9 present the outcome mechanisms related to changes in earnings directly. First, we examine the change in earnings within firm, due to wage increases and/or increases in hours worked. There, we find a large positive overall effect that is entirely driven by the increased pay in jobs during the first three quarters. The magnitudes are substantial too—the treatment group experience within-firm wage growth from quarter to quarter that is over $600 higher than the control group, and the LATE estimate for completers is double that. Second, we examine the change in earnings as workers change firms. There, we find positive but insignificant effects. Third, we examine the earnings in jobs that workers leave. We find that the treatment group are leaving higher paying jobs than the control group, but that this is concentrated in the first three quarters. However, this is mirrored by the effect shown in the last row of the treatment group also having higher earnings than the control group at new firms, with the effect of almost the same magnitude as their lost earnings at departed jobs. Again, this effect is primarily within the first three quarters. Together, these paint a story of early treatment effects being driven by, within the first three quarters after training, higher earnings increases within firm, which is carried over to new jobs that they hold. This is followed in the period after the first three quarters by the treatment effect of trainees holding their higher paying jobs with a higher probability. Table A1 in the appendix repeats the analysis, but focuses on the subgroup of those with no job at training. The findings are similar but tend to be larger in magnitude.

6. Conclusion

There is a persistent need to consider policies that would help disadvantaged persons struggling with employment. Among the many options are programs that work to develop the human capital of the individuals for skills that are in demand in the labor market. Job training programs have been shown to have promise, but there is still a need to understand how effective they are and what can help the segments of the population that are more vulnerable, including those without any job. In this paper, we evaluate a job training program in New Orleans, Career Pathways, aimed at disadvantaged populations unsatisfied with their employment situation, and research the outcome mechanisms through which the successful program works. We evaluate an RCT to understand how effective this 2-to-4 month training program was across 20 different training cohorts in advanced manufacturing, health care, and information technology. We find a meaningful intent to treat impact on earnings which represented over a 20 percent increase, or over $400 a quarter of a year. This impact more than doubled when we accounted for completion of training using a LATE estimator.

While the overall magnitudes of these effects are roughly in line with quasi-experimental estimates of other recent job training programs, one key finding with Career Pathways is that the individuals who benefited most were those with no job at entry of the program. This forms an interesting and important target population for these kind of job training programs, such that the local government could consider this type of program (with the screening and demand-driven features) for their Dislocated Workers programs as well, to see if such a model would improve the success of the training programs which have typically been found to be less successful (Andersson et al. 2013). That is, although we cannot definitively state why this program was successful for individuals who entered the program with no job while other similar programs found Dislocated Workers' training programs to have the least success, one reason may be because of the key element of screening individuals before admitting them to training. Specifically, the city verified that the individuals had sufficient literacy and numeracy through the Test of Adult Basic Education as well as three other screening exercises. This may have identified the key population of recently dislocated workers that are likely to benefit from this kind of training program, and this to our knowledge has not been used as a strict selection tool in the past (Fortson et al. 2017). Another reason may have been the demand-driven nature of the program. Prior evaluations of Dislocated Worker training programs have been for WIA programs that were created prior to the WIOA requirement to "emphasize sector-based strategies to promote employment in high-demand industries and occupations" (Fortson et al. 2017, p. 5).

There are several additional possible reasons for our finding that those without a job have the largest treatment effects. Some of the reasons are addressed in Fortson et al. (2017) as they discuss some of their differences for their findings for the Dislocated Workers programs. First, it may be a different population. Our study is not a Dislocated Workers program, but we examine within the Adult program persons coming in without a job. Fortson et al. 2017 show how persons enrolled in the Adult program tended to have lower earnings and past employment than those enrolled through the Dislocated Workers program, unlike our population of those without a job. Our subpopulation of non-workers may be excluded from the Dislocated Workers program for several reasons, including that they never worked or that they voluntarily left their prior job. Second, the setting in New Orleans may be unique and not comparable to the more national setting (across several states and metropolitan areas) in the other three studies. Something about the city of New Orleans and its population may have led to better program effects for workers without a job at entry into training. Third, our results may differ because of differences in the approaches, and the implications for how selection on unmeasured characteristics is controlled for. Although this would apply to Andersson et al. (2013) and Heinrich et al. (2013), our approach is largely comparable to Fortson et al. (2017) in using randomization. Fourth, the results may arise from differences in the actual training program. It may be that the connections with local industries were better than on average or better training providers were in place or other features of the training were for whatever reason more applicable to the population of those without a job. Fifth, it may be that the screening was effective in identifying individuals in the non-working group that are very well-suited for training, leading to our positive effects. It is not clear why we would find no effects for those entering with a job, either for this reason or any of the other listed reasons. More research and future programs would help disentangle what made the Career Pathways program so effective for this specific population, and whether that success would carry over to Dislocated Worker training programs.

We determine several important outcome mechanisms through which the program succeeds. Individuals in the training program were more likely to keep a job from quarter to quarter, resulting in longer job duration. Encouragingly, this effect was again driven by persons and jobs that we would be most interested in. Those without a job at program entry were the main drivers of the duration effect, and it was happening much more in higher paying jobs, while the population of workers without a job at the start of training were actually *more* likely to leave lower-paying jobs. Thus, we believe the training was equipping them with the skills that improved their likelihood of staying on these good jobs, where they had before had gaps in their employment. This is an important set of insights about how job training programs can benefit workers that has before been ignored in the literature.

We find strong evidence that individuals assigned to training in two of the three pathways (advanced manufacturing and health care) were more likely to end up working in the related target industries than their comparison workers in the control group. There is further evidence that this was at least partially driven by movement out of low-skill industries, suggesting that the development of human capital through the training program allowed them to move into better careers in the target industries. This stands in contrast to Fortson et al. (2017) who found no increased probability of working in the occupations for which they were trained. Further, this effect grew significantly over time for the health care industry, suggesting that trainers, training providers, and policy makers should be patient in waiting for the benefits of training to occur.

When we examine outcome mechanisms more carefully, an interesting pattern emerges whereby in the first 9 months after the end of training, treatment leads to higher earnings growth within employer and when transitioning from job to job. After 9 months, earnings growth stabilizes between the treatment and control groups, but the treatment group are more likely to retain their now-higher paying jobs. This is reinforced in examining employment within the target industry for the healthcare pathways, wherein the probability of being employed in the hospital industry increases from a 26 percentage point differential between treatment and control in the first 9 months to a 69 percent differential. Together, these outcome mechanism findings encourage patience on both the part of trainees and the government. The workers may not be able to find their stable, target employment immediately, but we find that they will eventually find it, after experiencing accelerated earnings growth early on while transitioning jobs. Additionally, the government may want to find ways to improve early connections with employers after training in the target industry, especially with jobs that may be more stable. However, overall we find the results of the analysis highly encouraging. Career Pathways was able to succeed with the population of high interest, those without jobs at start of training. And that success was accounted for along several dimensions, including higher employment rates, earnings, job duration in higher paying jobs, and eventual employment in higher skill industries from which they can make careers.

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Appendix

Table A1: Outcome mechanisms for those with no job at randomization

	All post-training quarters		First three post-training quarters		After three quarters post-training	
	ATE	LATE Completed	ATE	LATE Completed	ATE	LATE Completed
Overall effect on earnings	1,346.445***	3,347.783***	1,074.364***	2,211.065***	1,704.587***	5,902.824**
	(344.904)	(1,121.335)	(314.904)	(640.061)	(492.501)	(2,794.188)
Retaining existing firms	0.076**	0.202*	0.027	0.061	0.139***	0.446**
	(0.034)	(0.103)	(0.047)	(0.099)	(0.030)	(0.198)
Start new firms	-0.013	-0.037	-0.001	-0.003	-0.027***	-0.113*
	(0.009)	(0.031)	(0.010)	(0.024)	(0.008)	(0.066)
Number of firms	-0.125*	-0.355	-0.074	-0.172	-0.179**	-0.717
	(0.064)	(0.226)	(0.066)	(0.161)	(0.078)	(0.464)
Change in earnings within-firm	425.181***	1,091.627***	953.837***	2,231.529***	-91.465	-255.671
	(104.882)	(392.539)	(176.973)	(727.003)	(194.337)	(522.929)
Change in earnings switch firm	-44.743	-176.635	775.137	2,046.303	-611.699	-23,146.612
	(329.070)	(1,140.114)	(654.587)	(1,400.682)	(732.478)	(114,748.365)
Earnings at departed firm	632.086**	2,114.937**	686.502	1,942.082	448.794	2,646.516
	(298.245)	(1,046.886)	(497.345)	(1,204.042)	(582.677)	(3,866.828)
Earnings at new firm	811.132**	1,954.097**	807.920	1,719.512*	646.781	2,502.381
	(372.338)	(935.057)	(481.856)	(977.907)	(392.522)	(1,883.135)

Note: ATE is the average treatment effect, using an intent-to-treat design. LATE is the local average treatment effect for compliers in both the treatment and control group. First three quarters account for about 60% of observations, after first three for 40%. *p<.10, **p<.05, ***p<.01, with standard errors clustered by cohort/treatment assignment.