The Labor Market Returns to Customized Job Training

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Abstract

Economic theory dating back to Becker (1962) predicts that employers and workers should share the cost of job training in specific, non-transferable skills, leaving workers or the government to fund general skills training. Customized job training (CJT) programs, which exist in most U.S. states, defy this logic by using public subsidies to teach workers a range of skills, including those that are firm-specific. Are governments mistakenly subsidizing training that firms would pay for on their own, or does CJT generate benefits that justify public investment? I answer these questions using unique hand-collected data from Tennessee on firms' grant applications and trainees' enrollment linked with rich administrative data on education, earnings, and public assistance. I exploit the fact that there is quasi-random rationing among equally comparable firms and equally eligible prospective trainees. The estimates show that enrolling in a CJT program, typically lasting about four months, increases earnings by 3% per quarter over five years, comparable to the return from one additional year of work experience. To explain these findings, I classify the skills taught in each program by mapping program descriptions in firms' grant applications to O*NET detailed work activities. Despite the fact that CJT programs primarily produce transferable skills across industries and occupations, benefits to the government through higher individual income tax revenues more than offset training costs, yielding an exceptionally high marginal value of public funds relative to other job training programs.

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1 Introduction

Many state-subsidized job training programs defy Becker's classic logic that employers and workers should both finance training workers in firm-specific skills, leaving workers or the government to fund general skills training that is portable across firms (Becker, 1962). The U.S. federal government's $4.3 billion annual spending on training emphasizes fairly general skills, yet nearly every state government subsidizes Customized Job Training (CJT) (Department of Labor, 2025). By design, CJT gives firms full discretion over curriculum, participant selection, and delivery. Firms typically enroll their incumbent employees, conduct training in their own facilities, and use proprietary machines and other factors of production that are not available to outside employers. For instance, an enrollee might learn to control robots or machine tools that are used only at one automaker. At least on the surface, much of CJT training appears partly if not largely firm-specific. While CJT may be an efficient way to raise productivity, create and retain jobs, and boost worker earnings, it is not obvious that taxpayers derive sufficient, presumably indirect, benefits to justify public funding. For instance, firms might conduct the same training if left to fund it on their own.

In this paper, I estimate the causal impacts of CJT using a novel and comprehensive dataset on CJT in Tennessee. I hand-collected and digitized firm grant applications and contracts from the entire state from 2016 to 2024. I linked these data to program cost information and individual training records, thereby opening the "black box" of firm-designed and subsidized training. The resulting dataset is further merged with Tennessee's longitudinal records on education, quarterly earnings and benefits, and public assistance participation.

In addition, I gathered the description of each CJT program from its firm's applications and applied natural language processing (NLP) to measure the transferability and specificity of skills. Specifically, each description is mapped to detailed work activity statements from the U.S. Department of Labor's O*NET database, enabling me to characterize the program's provision of general, industry-specific, occupation-specific, and firm-specific skills. I validate the NLP-based classifications by conducting a statewide employer survey in which firms reviewed and categorized training text into the same four skill groups.

Identifying the causal effect of CJT is challenging because workers and firms select into training. Plant managers may allocate training slots to workers whom they observe to be most productive or already on an upward learning trajectory. Workers with stronger motivation or greater learning ability may join training, while others do not. Firms that are more productive or on a steeper productivity trajectory—perhaps because they have recently invented new technology or methods—may be more likely to apply to run a CJT program.

I mitigate worker and firm selection by making full use of the rich, longitudinal data built for this study. For example, Unemployment Insurance (UI) records provide firms' employment levels and industry classifications over time, which are used to determine eligibility for CJT grants, as well as workers' earnings levels and trajectories, which I use to match treated workers with equally eligible prospective trainees. However, one might still worry about selection on unobserved worker or firm characteristics.

Fortunately, several constraints on Tennessee's CJT programs generate rationing that provides variation that I can exploit via plausible causal empirical strategies. Specifically, each of Tennessee's nine regional workforce boards typically allocates funds to eligible firms on a first-come, first-served basis. Furthermore, the timing of when regions receive funding, if any, is rather arbitrary. As a result, a more promising program might remain unfunded while a less promising program is funded because its firm applied earlier in the year. Moreover, Tennessee imposes a cap of \$25,000 per grant application on state funding for CJT. Each year, there is a state funding cap of \$50,000 per firm. Once this cap is reached, the firm must wait a full year before reapplying for state CJT funding. As a result, a firm with a promising program may not get funding at a particular time if the firm is in a "fallow" year or the region has no CJT monies that year. Even if a firm receives a CJT grant, funding caps may force it to allocate training arbitrarily among workers who are equally promising or prospective.

These constraints imply that some feasible training opportunities are implemented while others are not, even though they would be equally productive. If training varies among equally propitious workers within a firm, then a difference-in-differences (DID) model is appropriate. By comparing changes in outcomes between treated and untreated co-workers within the same firm, this model eliminates the selection on the firm. Moreover, individual fixed effects absorb persistent worker heterogeneity, time fixed effects capture aggregate shocks, and industry-by-year fixed effects account for sector-specific dynamics. The key identifying assumptions are that no unobserved shocks systematically coincide with training enrollment beyond what these controls capture, and individuals are not selected into treatment based on potential outcomes. Although this design holds firm conditions constant, a remaining concern is that managers may selectively assign training to employees based on expected performance, potentially biasing the estimates.

Similarly, the arbitrary variation in funding cycles and amounts, the first-come, first-served allocation rules across firms and regions, and fallow years following funding imply that possibly equally productive CJT programs do and do not get funded. If the training varies among equally propitious workers across applying firms, then a DID model is also appropriate. By comparing changes in outcomes between treated and untreated workers at firms that do and do not receive CJT grants, this model alleviates the concern of selection on the worker.

To address the concern that the individual fixed effects are not enough to account for the endogeneity of individual training, I implement a difference-in-instrumental-variables (DID-IV) strategy that further exploits the quasi-random allocation of CJT grants to firms. By using firm-level CJT grant receipt as an instrument for individual-level CJT enrollment, an access-to-treatment instrument, I compare treated workers with untreated workers at applying firms that do not get the grant. The reason that these untreated workers were not trained is no longer a within-firm selection issue, but an access issue, further alleviating the selection on the worker concern. Another key identifying assumption is that grant receipt affects worker earnings only through its effect on training participation. For all estimation strategies, I account for differences in observed characteristics between treated and control workers using inverse probability weights.

Across all strategies, I find that enrollment in CJT significantly and persistently raises worker earnings when skill production matches local labor market demand. My preferred strategy, the DID-IV, shows that enrolling in CJT increases earnings by $406 per quarter (3%) relative to a similar, untreated worker in a firm that applied for the grant but did not receive it. The estimated effects are comparable in magnitude to early-career returns to a year of work experience (Deming, 2023). Customized job training produces earnings gains that are comparable to or exceed the most successful U.S. government and sectoral training programs.[1]

Next, I examine heterogeneity in enrollment effects to assess how training operates across different contexts. I find that the enrollment effects of CJT on earnings differ systematically across observed firm characteristics but not worker characteristics. Using my preferred approach, earnings gains are concentrated in small firms with fewer than 50 employees and in the manufacturing and wholesale sectors. Earning gains following enrollment are $825 per quarter (6%) in small firms, $355 per quarter (2%) in the manufacturing sector, and $1,503 per quarter (9%) in the wholesale sector. These patterns are consistent with macroeconomic evidence that smaller firms tend to provide fewer training opportunities across countries and have the most to gain from training subsidies (Ma et al., 2025). By worker characteristics, earning effects are similar across males and females as well as across Hispanic, non-Hispanic Black, and non-Hispanic White individuals. Effects are also similar for new and incumbent workers at the firm.

My data on training content linked with individual outcomes allows me to investigate whether CJT programs are increasing the supply of transferable human capital or merely function as cash transfers to firms. Using the NLP-based skill classification, I find that

[1] See Black et al. (2023) and Card et al. (2018) for reviews and meta-analyses of the job training literature.

CJT programs emphasize partially transferable skills. On average, 87% of CJT training is transferable or partially transferable: 3% of content is general, 41% occupation-specific, 43% industry-specific, and 13% firm-specific. The skill composition remains stable over time and across industries. The relatively large share of transferable and partially transferable training aligns with the economic rationale for public subsidies, which aim to address underinvestment in transferable skills. The statewide employer survey corroborates this pattern, indicating that 83% of training is transferable or partially transferable.

The effectiveness of CJT varies with the transferability of skills taught. To understand how the returns to CJT vary differentially with skill specificity, I estimate the enrollment effect of CJT for the individuals whose training program was ranked in the top 25% of each skill category. I find that the effectiveness of the CJT program is predominantly driven by industry-specific skills. Enrolling in programs with high industry-specific scores increases earnings by $1,367 per quarter (9%). This pattern is consistent with standard search models in which earnings reflect workers' outside options, implying that investments in transferable or partially transferable skills generate larger earnings gains than firm-specific skills.

I next examine where these returns are realized—whether workers benefit by remaining with their training firm or by applying new skills elsewhere. Customized job training modestly strengthens worker retention while also generating transferable skills that benefit those who change employers. Four years after training, treated workers are 10% more likely to remain with their incumbent firm and 11% more likely to remain in their incumbent industry. Using the DID-IV strategy, I find no significant earnings effects for workers who stay with their training firm or industry. Descriptive estimates show that firm leavers experience quarterly earnings gains of $323 (2%) and industry leavers gain $475 (3%). Together, these results suggest that CJT develops skills with meaningful transferability, yet trained workers remain more attached to their employers. This coexistence of skill transferability and retention is consistent with post-Beckerian models in which labor market frictions allow firms to invest in partially transferable training.

Customized job training generates earnings gains that exceed program costs, making it cost-effective relative to its scale of funding. Combining the analysis of CJT effects on earnings and program costs, I construct cost-benefit analyses for the program. I apply the marginal value of public funds (MVPF) framework of Hendren and Sprung-Keyser (2020) and traditional back-of-the-envelope cost-benefit calculations. The MVPF is the ratio of the participant's after-tax willingness to pay (WTP) for the program to net program costs. The positive effects of CJT training result in a positive WTP, and these earnings gains lead to tax revenue for the government that exceeds the program's cost, resulting in a negligible net cost. Thus, the MVPF of CJT programs is infinite. Traditional back-of-the-envelope cost-benefit calculations indicate that these programs pay for themselves within four quarters. Together, the cost-benefit analyses show that these programs are an efficient use of government funds.

Broadly, I find that enrolling in CJT generates substantial and persistent earning gains. These gains are comparable to or larger than a wide range of job training programs in different settings and countries, including apprenticeships and sectoral training (Clark and Fahr, 2001; Fersterer et al., 2008; Maguire et al., 2010; Katz et al., 2022; Bollinger and Troske, 2025).[2] Moreover, a cost–benefit analysis shows that CJT yields an infinite MVPF within five years of enrollment, whereas other job training and adult-focused policies yield MVPFs between 0.44 and 2.0.[3] These findings imply that CJT programs deploy public funds more efficiently than other evaluated job training and adult-focused policies. By subsidizing employer-designed training that directly aligns skill supply with firm demand, governments may address coordination failures in the private provision of workforce development. The results highlight how firms, when subsidized, invest in skill bundles that balance private and shared returns—challenging the view that public support for firm-led training necessarily finances purely specific human capital.

This paper makes three main contributions to the broader economics literature. First, this paper provides the first causal evidence on whether subsidized employer-designed and administered job training programs that align skill supply with the local labor demand of specific firms raise worker earnings. I combine novel data on firm applications, firm contracts, and individual training records with Tennessee's longitudinal data system. To identify the causal effect of enrollment in CJT, I exploit plausibly exogenous variation in grant receipt. This approach moves beyond descriptive and self-reported analyses to provide the first causal evidence on the earnings returns to state-subsidized, employer-designed training programs.

Despite the prevalence of these firm training programs, the small existing literature on these programs relies almost entirely on firm or worker surveys. Hollenbeck (2008) surveys firms nationwide and documents that roughly half report earnings increases following incumbent worker training. Holzer et al. (1993) analyze Michigan's grant program with a 32% response rate and reports short-lived increases in training hours without describing earnings following training. Van Horn and Fichtner (2003) study New Jersey's Workforce Development Partnership using firm and worker surveys linked to administrative earnings records for on-the-job training participants only. They note a 10.6% rise in average earnings two

[2] Sectoral employment programs train individuals for employment in specific industry sectors that are considered to have to have strong current local labor demand and prospects for career growth. Targeted sectors typically include health care, information technology (IT), and manufacturing.

[3] For example, Hendren and Sprung-Keyser (2020) calculates MVPFs for a large selection of job training policies to be between 0.44 and 1.48. The CJT MVPF estimate is well above estimates reported in Hendren and Sprung-Keyser (2020) for a variety of policy interventions targeted at adults, ranging from 0.5 to 2.0.

years after training, but these estimates are based on differences in inflation-adjusted means without a control group or addressing selection, making them purely descriptive.

A large, related literature finds that firm-sponsored training typically generates positive returns, but estimates are difficult to interpret causally and vary widely across settings (Black et al., 2023; Ma et al., 2024).[4] In contrast, U.S. government-sponsored training and employment programs—targeting youth, disadvantaged adults, and dislocated workers—have a mixed record, with few cases of persistent employment and earnings gains.[5] Unlike CJT, these programs often fail to match training content to local labor market demand (Deming et al., 2023).

Programs that are more closely tied to local labor markets include apprenticeships, internships, and sectoral training programs. Evaluations of apprenticeships and internships report mixed effects (Wolter and Ryan, 2011; Klein and Weiss, 2011; Margaryan et al., 2022). Sectoral initiatives often report larger gains, but the evidence typically comes from small, resource-intensive pilots in which estimated impacts may partly reflect bundled wraparound services in addition to training (Maguire et al., 2010; Katz et al., 2022; Bollinger and Troske, 2025).

Second, this paper advances the firm training literature by providing new evidence on the content of firm-designed training. I build on this research by constructing a novel dataset on firm training content. Using this dataset and natural language processing techniques, I identify discrete tasks within program descriptions and classify content along four dimensions of skill transferability: general, industry-, occupation-, and firm-specific.

The literature lacks substantial evidence about the content of employer-designed training programs. Van Horn and Fichtner (2003) provide anecdotal evidence of training content through surveys, stating 70% of firms offer occupational training. Dillon et al. (2025) scrape publicly available incumbent worker training descriptions from six states and use text from grant applications to map program content into four broad occupational categories (professional, administrative/sales, service, and production) with an LLM.

Two features distinguish my work from prior research that measures the content of employer-designed training programs. First, I observe employer-submitted training plans for all funded CJT grants and link them to worker-level administrative records. Publicly available training plans may not be fully representative, and they may omit important details because of privacy concerns. My use of a comprehensive set of training plans that are

[4] Ma et al. (2024) document 128 different estimates of firm-sponsored training around the world with a median earnings gain of 3.8% following training.

[5] See, for example, Ashenfelter (1978); Ashenfelter and Card (1984); LaLonde (1986); Heckman et al. (1998); Greenberg et al. (2003); Hollenbeck (2009); Heinrich et al. (2013); Fortson et al. (2017); Card et al. (2018); Hyman (2018); Mastri et al. (2018); Andersson et al. (2022).

not publicly available sidesteps these potential concerns. Second, Dillon et al. (2025) use the text from the training plans to assign programs to four broad occupational groupings. My administrative data allows me to directly observe detailed occupation codes for all workers.

Third, this paper provides the first empirical evidence on the labor-market returns to the transferability of skills, bridging long-standing theoretical debates on whether workers capture these returns and recent empirical efforts to measure training content. I extend this literature by directly measuring the full spectrum of skill transferability within employer-led programs. Using 2,087 detailed work activities (DWAs) from O*NET, I classify each CJT program's content along four dimensions: general, industry-, occupation-, and firm-specific. Linking these measures to administrative earnings data, I estimate the first causal returns to different levels of skill transferability. This approach moves beyond inferring specificity from occupational aggregates by quantifying how the content and transferability of employer-provided training shape worker earnings.

Economic theory emphasizes the role of skill specificity in the returns to workers: Becker (1962) distinguishes general from firm-specific training; Acemoglu and Pischke (1998, 1999a,b) conclude that wage compression and labor-market frictions can make even general training behave as specific; and Lazear (2009) highlights how the portability of skills depends on the thickness of local markets.

Empirical studies have largely inferred skill specificity indirectly—from worker mobility or tenure (Neal, 1995; Loewenstein and Spletzer, 1999; Gathmann and Schönberg, 2010)—or through the returns to apprenticeships and CTE programs emphasizing occupation-specific training (Malamud and Pop-Eleches, 2010; Jepsen et al., 2014; Golsteyn and Stenberg, 2017; Hanushek et al., 2017; Carruthers and Sanford, 2018).

An emerging literature classifies training content to better understand the specificity and transferability of skills. I extend this work by developing a continuous and multidimensional measure of skill transferability and by estimating the returns to each skill type. Eggenberger et al. (2018) construct an occupation-specificity index for Swiss apprenticeships from 181 curriculum-based skills and show that graduates who remain in their trained occupation earn more. Similarly, Albanese and Aliberti (2024) classify job training programs in Italy as general or industry-specific based on administrative training codes that proxy for the industry in which skills are applied, finding that workers in more competitive labor markets receive less general training, but without examining worker earnings. These measures are broad proxies for skill type and thus are unlikely to be able to fully distinguish between various types of relevant skills. For example, occupationally framed training can embed general, industry-, and firm-specific skills, so estimates attributed to occupation-specificity may partly reflect those components. I address this by directly measuring all four dimensions jointly and estimating how each skill dimension contributes to earnings. Additionally, as discussed above, Dillon et al. (2025) use training text to measure occupational categories, but they do not characterize the skill transferability of the programs they examine.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 outlines the data sources and construction of the analysis sample. Section 4 details the empirical strategy and identifying assumptions. Section 5 reports the baseline effects of enrolling in customized job training on labor-market outcomes, and Section 6 examines heterogeneous effects. Section 7 introduces the measurement of skill specificity and presents the corresponding results on earnings returns by skill type. Section 8 analyzes worker mobility. Section 9 uses the estimates to conduct a cost-benefit analysis. Section 10 concludes.

2 Institutional Setting

Governments invest heavily in job training even though most programs find limited and short-lived effects on worker earnings. These investments persist partly for economic development reasons. They also reflect market failures: firms may underinvest in transferable skills because they cannot capture the full return, while firm-specific skills can reinforce monopsony power. Customized job training programs embody this tension by subsidizing firm-designed training to align skill production with employer demand. Whether such subsidies expand the supply of portable skills or simply underwrite firm-specific human capital remains an open question.

2.1 Program attributes

The U.S. Department of Labor invests $4.3 billion annually in job training, yet most programs fail to generate sustained employment and earnings gains for workers (Department of Labor, 2025; Card et al., 2018). These programs are often standardized and disconnected from the specific demands of local employers (Deming et al., 2023). Governments nonetheless continue to allocate substantial resources to training. One motivation is that technological change shifts demand for skills in ways private markets may not address (Goldin and Katz, 2009; Acemoglu and Autor, 2011). Another is that states use training as a tool for local economic development and firm recruitment (Slattery and Zidar, 2020; Lester et al., 2025).

Customized job training programs emerged to address this gap by subsidizing training that firms themselves design to meet their workforce needs. North Carolina launched the first customized training program in 1958, even before establishing its community college system (Duscha and Graves, 1999). Today, nearly every U.S. state operates a customized training initiative, and states are regularly recognized by business magazines for the strength of these programs.[6] Unlike other training initiatives, CJT is not organized around the attainment of formal credentials or the provision of wraparound services; its defining feature is full employer discretion, with firms choosing whom to train, what skills to emphasize, who provides the training, and the content, delivery, and duration of instruction.

Firms access CJT by applying for grants through their state's department of labor or workforce agency. These agencies administer funds drawn from federal Workforce Innovation and Opportunity Act (WIOA) allocations, and many states supplement the federal funds with state appropriations. Applications must specify the type of training, describe the training need and program, identify the number of workers to be trained and the training provider, and state the total estimated cost and requested funding. States vary in how they allocate funds across applications: some rely on competitive or discretionary review, while others award grants on a first-come, first-served basis. Approved firms then enter into contracts that set reimbursement terms and reporting requirements. Agencies monitor compliance by reviewing reimbursement documentation and conducting worksite visits to verify that training takes place.

Customized job training applications fall into two categories that determine who is trained and how costs are shared: on-the-job training (OJT) and incumbent worker training (IWT). On-the-job training subsidizes the hiring and training of new workers, typically those employed for six months or less. For an OJT program to be made, the majority of potential trainees need to be new workers. Employers design a training plan and receive reimbursement for up to 50% of earnings during the training period, with discretion to raise the subsidy to 75% for small firms or workers with barriers to employment. This structure supports skill acquisition while also reducing the risk associated with hiring individuals who might not otherwise have been employed. Incumbent worker training subsidizes training for existing employees who have been employed at the firm for more than six months by reimbursing non-earnings costs such as trainer salaries, curriculum, or instructional materials. The same majority rule holds for incumbent workers participating in IWT. Employers must cover a fixed share of training costs that rises with firm size: at least 10% for firms under 50 employees, 25% for firms with 51–100 employees, and 50% for firms with more than 100 employees. When demand exceeds available funds, WIOA requires priority for low-income individuals, public assistance recipients, and workers with barriers to employment. Federal law also limits how WIOA funds may be used. Local workforce boards may allocate no more than 20% of their funding to IWT. Governors may separately reserve up to 15% of total state WIOA

[6] Magazines such as *Business Facilities* and *Area Development*, which cover corporate site selection and economic development, publish annual rankings of states' customized training programs.

funds for discretionary training uses.

Tennessee implements this framework through a grant-based system administered by the Department of Labor and Workforce Development in partnership with nine regional workforce boards. Firms complete a brief pre-application verifying eligibility—being current on state taxes, employing at least five workers, and operating in a targeted industry designated by the regional workforce board. Eligible firms then submit a full application, prepared with assistance from regional business service representatives and reviewed at the regional level. Grants are awarded on a first-come, first-served basis at the regional level, given that the region was allocated CJT monies for that year.[7] Tennessee also imposes programmatic limits, including caps of $25,000 per training grant and $50,000 per firm per year, along with a one-year waiting period once the $50,000 cap is reached. Once approved and contingent on available funds, firms sign a contract and begin training. Employers submit monthly reimbursement requests documenting training completion and expenses. For example, one Tennessee manufacturer used a $25,000 IWT grant to train six workers in advanced injection molding, with the firm covering earnings costs while the grant reimbursed training expenses.

2.2 Conceptual basis for empirical analysis

State investment in CJT programs reflects both imperfections in private human capital formation and the broader objectives of regional economic development. Workers often face barriers to investing in their own skills due to credit constraints, risk aversion, or limited information about the training valued by employers and its expected returns (Becker, 1964; Altonji, 1993; Belley and Lochner, 2007; Caliendo et al., 2022; Patnaik et al., 2022). In imperfect labor markets, monopsony power further discourages worker investment: when employees cannot fully capture the returns to higher productivity, they underinvest in skill acquisition. On the other hand, firms invest only if expected returns exceed costs. Firms will weigh the productivity gains of their workers following training against the higher earnings they must pay to retain their workers.[8] Small and young firms encounter similar financing frictions as workers that restrict their ability to fund training internally (Banerjee and Duflo, 2014; Kerr and Nanda, 2009). States therefore have an incentive to intervene—both to correct underinvestment in training and to pursue place-based development goals. Consistent with this view, states frequently use targeted subsidies and incentive programs to attract firms and stimulate job creation (Slattery and Zidar, 2020; Lester et al., 2025).

The CJT model seeks to address these market failures by leveraging firms' private infor-

[7] Discussions with regional workforce directors indicate that the funding cycle is rather arbitrary. The exact date when funding comes to the region is not routine if it happens at all in that year.

[8] Part of this decision includes the firms' liquidity constraints, which they will weigh in their decision.

mation about relevant skills while offsetting part of the training cost through public funds. Yet it also embodies competing forces. Firms tend to underinvest in transferable skills that benefit other employers, creating poaching externalities that reduce the private return to training (Stevens, 1994; Acemoglu and Pischke, 1999a). Conversely, when training is highly firm-specific, workers become dependent on a single employer's technology or processes, strengthening monopsony power (Loewenstein and Spletzer, 1999; Lazear, 2009; Cavounidis and Lang, 2020). The optimal skill mix likely combines both transferable and firm-specific components, but it is unclear whether firms achieve this balance when supported by public subsidies. Whether CJT expands the supply of portable skills or primarily underwrites firm-specific content is ultimately an empirical question that motivates the analysis below.

3 Data

The potential for research on CJT has long been constrained by limited data. Prior studies of these programs have relied largely on firm and worker surveys rather than administrative data (Holzer et al., 1993; Van Horn and Fichtner, 2003; Hollenbeck, 2008). Assembling comprehensive data on training is challenging because it involves multiple actors: firms apply for grants and design programs, state and local governments administer funding and oversight, and workers participate in the training itself. To move beyond these limitations, I compile the first comprehensive dataset on CJT programs by hand-collecting and digitizing firm applications, contracts, and training records, and linking them to administrative data on education, earnings, benefits, and public assistance from Tennessee's longitudinal data system.

3.1 Training records

The core of this analysis rests on an original, comprehensive dataset combining firm- and worker-level administrative records for all publicly subsidized CJT programs in Tennessee from 2016 through 2024. The firm-level data draws on pre-application forms, full applications, and contracts for OJT and IWT grants administered by the Tennessee Department of Labor and Workforce Development and regional workforce boards. Pre-applications, available for all firms from 2016 through the second quarter of 2024, contain firm and occupation identifiers, brief training descriptions, and eligibility checks such as employing at least five workers and being current on state taxes. These records capture both funded and unfunded applicants, enabling comparison across the entire applicant pool.

Firms that meet eligibility requirements are invited to submit a full application. For these firms, I hand-collected and digitized the corresponding applications and contracts from 2017 through 2025. State agencies are not required to retain applications beyond five years, so records prior to 2019 generally were unavailable. I collected all applications that could be obtained, including any surviving pre-2019 records. As a result, the dataset reflects the most comprehensive coverage that can be assembled. These documents record where the training will take place, how many hours it will last, the competencies workers are expected to obtain, how those competencies relate to firm productivity, the budget breakdown between the firm and the state, and the intended outcomes of the training. I analyze the text of these training descriptions separately in Section 7 to characterize the skill composition and transferability of training content. Contracts and reimbursement records further document training completion, expenditures, and oversight, including site visits and compliance monitoring.

Table 1 and Figure 1 summarize the sample of firms that applied for CJT grants. I observe 1,291 firms that received CJT funding and 499 firms whose applications were denied, implying an acceptance rate of roughly 87%. Funded firms are larger on average, employing nearly 400 workers, whereas denied applicants employ about 100 workers. More than one-third of funded and denied firms are concentrated in manufacturing, reflecting the state's industrial base. As shown in Figure 1, applications increased steadily through 2019, declined sharply during the COVID-19 pandemic, and began to recover by 2023. Smaller firms with fewer than 50 employees are disproportionately represented among awardees.

To contextualize the scale of public investment, I supplement firm-level data with administrative expenditure records from the Tennessee Department of Labor and Workforce Development covering 2018 through 2024. These records allow benchmarking of firm-reported budgets against regional totals and provide the most complete accounting of CJT spending available. Figure A.1 plots total government expenditures by year and training type. Spending peaked in 2019 at nearly $4 million—driven primarily by IWT—fell sharply in 2020 with the onset of the COVID-19 pandemic, and stabilized around $2.5 million annually by 2022. The predominance of IWT funding underscores the state's emphasis on supporting training for incumbent workers, while OJT funding remained smaller.

The individual-level component of the dataset identifies the workers who participated in training, the occupation for which they were trained, the timing and duration of participation, and whether they completed the program. In collaboration with the Tennessee Department of Labor and Workforce Development, I extracted and cleaned these records to enable linkage with the state's longitudinal data system. Enrollment trends in CJT mirror firm participation, rising steadily through 2019, contracting sharply during the pandemic, and rebounding in later years (Figure 1a). Table 2 summarizes the characteristics of worker training. The average program lasts 121 days—roughly two-thirds of a U.S. school year—and the completion rate is 69%, substantially higher than national averages for U.S. workforce programs.[9] Training is heavily concentrated in production occupations, which account for more than 40% of all trainees and nearly 75% of OJT trainees. Incumbent worker training programs cover a broader range of occupations than OJT programs, including production; architecture and engineering; management; and installation, maintenance, and repair.

3.2 Employment and earnings records

To measure labor market outcomes, I worked with the Tennessee Department of Labor and Workforce Development and the Office of Evidence and Impact to link the individual and firm training records with Unemployment Insurance (UI) records maintained by the Tennessee Department of Labor and Workforce Development. State personnel created crosswalks from the training records to the UI records using individual social security numbers and firm federal employer identification numbers. The UI records include all employees in firms that are subject to unemployment insurance taxes. These administrative records provide quarterly information on employment status, earnings, and job mobility both before and after training.

With these data, I track earnings trajectories for each participant, measure employment retention, and identify movements between firms. Because the records span 2005 through 2024, I observe long pre-training baselines and multiple years of post-training outcomes. The employer identifiers also make it possible to assess whether trainees remain with the training firm or transition to other firms, a key distinction for evaluating the portability of skills. For comparability across time, I deflate all earnings to 2024 quarter two using the Consumer Price Index and topcode earnings at the 99th percentile.

Table 3 shows that CJT trainees are established workers rather than new labor-market entrants. The average participant has a strong prior attachment to employment, with positive earnings in more than 90% of pre-enrollment quarters. Average pre-training earnings are approximately $12,000 per quarter—above the federal poverty threshold for a single-person household ($15,650 annually in 2024) but below Tennessee's per-capita income of $64,908 in the same year. Differences across program types reflect their distinct roles in the labor market. On-the-job training participants show weaker prior attachment, earning in 81% of pre-enrollment quarters with baseline earnings around $6,000 per quarter. Incumbent worker training participants have stronger attachment—positive earnings in 92% of quarters and average pre-training earnings of about $13,000 per quarter. These contrasts underscore the programs' complementary functions: OJT serves as a flexible entry or new-hire subsidy,

[9] For example, the national Job Corps completion rate under WIOA definitions is approximately 38%, with only the best centers reaching ∼65%. The WIOA core programs report credential attainment rates of ∼72% for adults and ∼62% for youth in Program Year 2023 (U.S. Department of Labor, 2023). This is also above the six-year college completion rate of 62% (Causey et al., 2022).

while IWT offsets the cost of skill upgrading for incumbent workers.

In addition to employment and earnings, I observe related datasets. Quarterly unemployment insurance benefits records report individual eligibility and take-up, allowing me to track unemployment spells and benefit receipt. Federal and state program participation records capture whether individuals engaged in services such as Trade Adjustment Assistance (TAA), Workforce Innovation and Opportunity Act (WIOA) training, Wagner-Peyser services, and Adult Basic Education (ABE). These records also include participation in the Supplemental Nutrition Assistance Program (SNAP), which I use to measure reliance on safety net programs. Together, these datasets make it possible to distinguish the effects of CJT from participation in other common training and public assistance programs.

Participation in other workforce and public assistance programs is limited among CJT trainees prior to training, underscoring the distinct role that employer-led training plays within the broader landscape of active labor market policies. Table 3 shows that only a small share of participants engaged with other publicly funded training or reemployment services before enrolling in CJT: approximately 2% engaged with Wagner–Peyser and nearly 0% participated in ABE, TAA, or WIOA.

Records from the Tennessee Department of Labor and Workforce Development also report UI benefit eligibility and SNAP benefit receipt. Roughly 2% of trainees received UI benefits prior to enrollment, and 0% participated in SNAP. Although federal guidelines encourage states and regional boards to prioritize low-income or displaced workers when allocating training funds, the Tennessee CJT program primarily serves employed adults with steady earnings histories. These low rates of prior program participation indicate that CJT trainees are not simply shifting from existing public training or safety-net programs. Because the administrative data link CJT participation to these federal and state programs, I can verify that prior training and assistance participation are rare and unlikely to drive the estimated effects of CJT.

3.3 School enrollment and achievement

To capture educational outcomes, I worked with the Tennessee Office of Evidence and Impact to link training records to data from the Tennessee Department of Education, the Tennessee Department of Higher Education, and the Tennessee Independent Colleges and Universities Association. These records track students from kindergarten through postsecondary education, providing detailed information on enrollment, persistence, and completion.

The secondary education records span 2005 through 2022 and include attendance, grade-level test scores, high school enrollment, and graduation information. Demographic characteristics are derived from the linked K–12 administrative records, which provide nearly complete coverage of individuals who attended public schools in Tennessee. These records yield high-quality information on race, ethnicity, and gender, ensuring consistency across cohorts and over time. Table 3 shows that the average CJT participant is about 36 years old and predominantly male and non-Hispanic White. On-the-job training participants are younger on average than IWT participants, and a larger share of non-Hispanic Black workers participate in OJT than in IWT. These demographic differences align with program design: OJT targets new or recently hired workers earlier in their careers, whereas IWT primarily supports training for established employees in more senior or technical roles.

For higher education, the data span 2005 through 2024 and provide semester-by-semester information on enrollment, field of study, credits attempted and earned, and degree completion across all public and private institutions in Tennessee. Table 3 shows that only 19% of CJT participants have ever enrolled in college, where postsecondary enrollment is defined as attendance at any public or private two- or four-year institution in the state. This exceptionally low rate highlights that CJT participants are a relatively low-skilled segment of the workforce, with far less formal education than the typical U.S. worker. For comparison, about 61% of recent U.S. high school graduates enroll in college. Postsecondary data are missing for 28% of individuals in the sample because their age implies they would have completed high school before 2005, the first year covered by the higher-education records. Even under the conservative assumption that all of these older individuals pursued college prior to 2005, the implied enrollment rate would still fall well below the national average.

Two limitations arise when working with state administrative data. First, individuals who migrate out of Tennessee are no longer observed in the UI records, leading to attrition from the sample. Tennessee's outmigration rate is relatively low—about 71% of individuals born in the state still resided there as of 2012 (Aisch et al., 2014)—which helps limit this concern. Second, missing employment or earnings observations may reflect either outmigration or non-employment, and the two cannot be distinguished in the data.

3.4 Sample construction

The analysis sample is restricted to working adults employed at firms that applied for or received CJT grants. I focus on individuals between the ages of 18 and 64. Once workers are identified as employed at the time their firm applied for or received a training grant, I retain all available UI observations for those individuals. Treated individuals are defined as those observed enrolling in a CJT program. To ensure sufficient pre-treatment and post-treatment earnings history, I restrict the sample to workers with at least four quarters of non-consecutive, positive earnings prior to CJT enrollment and at least one quarter post CJT enrollment at any firm observed in Tennessee's UI system.

The resulting dataset forms worker-level panels spanning eight pre-treatment quarters and twenty post-treatment quarters, producing an unbalanced, stacked panel with a long baseline and follow-up window suitable for estimating dynamic effects.[10] Quarters immediately surrounding enrollment are excluded in cases where evidence of an Ashenfelter's dip appears. The final sample includes 6,661 distinct treated workers.

4 Empirical strategy

To estimate the causal effect of enrolling in a CJT program on worker earnings, I use two estimation strategies that impose different identifying assumptions: a difference-in-differences (DID) model and a difference-in-instrumental-variables (DID-IV) model. The DID design compares changes in outcomes for treated workers before and after training to changes over the same period for comparable untreated workers. The DID-IV design exploits plausibly exogenous variation in firm CJT applications and awards, instrumenting for individual participation with firm-level receipt of a training grant.

4.1 Difference-in-differences

The DID estimating equation is

$$Y_{it} = \alpha + \beta CJT_{it} + \theta X_{it} + \gamma_i + \tau_t + \psi_{s(i,t=0)t} + \varepsilon_{it}, \tag{1}$$

where Y_{it} denotes quarterly earnings, CJT_{it} is a binary indicator equal to one in the enrollment quarter and thereafter, X_{it} includes age, age squared, county unemployment rates, and county income per capita, γ_i are individual fixed effects, τ_t are year-by-quarter fixed effects, and $\psi_{s(i,t=0)t}$ are industry-by-year fixed effects based on the worker's industry at the time of treatment.

Year-by-quarter fixed effects (τ_t) absorb common time shocks, such as macroeconomic or seasonal factors affecting all workers in a given period, while industry-by-year fixed effects ($\psi_{s(i,t=0)t}$) flexibly control for sector-specific fluctuations in labor demand or productivity.[11] Identification comes from the difference in earnings differences between treated and control workers within the same period, after accounting for permanent individual heterogeneity and aggregate or industry shocks. The coefficient β thus captures the average causal effect of CJT

[10] Here "stacked" means that each individual is assigned a relative-time variable in which $t = 0$ denotes the quarter of treatment (or pseudo-treatment for control units). Thus, even though treatment occurs in different calendar quarters across individuals, the stacked panel aligns them relative to treatment timing.

[11] I add firm-by-year fixed effects to absorb shocks common to firms within the same funding cycle as a robustness check.

participation on treated workers' earnings (ATT), conditional on identifying assumptions.

A central identifying assumption in a DID framework is that, in the absence of training, average earnings for treated and control workers would have evolved in parallel. Two potential sources of bias warrant particular attention. The first is differential pre-treatment trends between the treated and control groups. To assess this, I estimate event study models that interact treatment with relative time since enrollment, testing for parallel pre-trends and capturing dynamic treatment effects. The second is correlated shocks: if individuals who enroll in CJT simultaneously experience unobserved shocks that affect earnings or employment, estimates may be biased. A well-known example in the training literature is Ashenfelter's dip (Ashenfelter, 1978), in which outcomes decline just prior to enrollment. To address this concern, I exclude the quarters immediately surrounding enrollment that show evidence of an Ashenfelter's dip.

The DID design also relies on SUTVA, which requires that a worker's potential outcomes depend only on their own treatment status. In this setting, SUTVA could be violated if training indirectly affects untreated workers within or across firms. Within firms, positive spillovers may arise if untrained workers benefit from more productive trained coworkers, leading to an underestimate of β. Conversely, crowd out effects could occur if firms reallocate resources or supervisory attention toward trainees at the expense of others, generating an overestimate of β. Across firms, potential interference could stem from the state's allocation of complementary resources: if non-awarded firms receive alternative support, control earnings would rise, biasing estimates downward, whereas if the state directs additional funding or attention toward awarded firms, control firms could be adversely affected, biasing estimates upward.

In addition to parallel trends, the DID framework relies on several additional assumptions. The first is that unobserved traits, such as worker ability and preferences, are stable over time. If motivation or productivity evolve in ways correlated with training, estimates of β could be biased. A second assumption is that treated and control workers do not differ in their underlying earnings growth as they age and gain experience. If treated workers were on steeper upward trajectories relative to control workers even in the absence of training, the DID estimate would overstate the true effect. Finally, the design assumes that pre-training occupational choices are separable from training investment decisions. If workers move into lower-paying roles to become eligible for training, pre-treatment earnings would be mechanically depressed, biasing β upward.

Under these assumptions, β in Equation (1) captures the average treatment effect on the treated (ATT): the mean change in earnings for CJT participants relative to what would have occurred absent training.

While the DID specification in Equation (1) defines the estimating framework, credible identification also requires constructing appropriate comparison groups for treated workers. I consider two complementary control groups that differ in the proximity of comparison: a within-firm control group composed of untreated employees at the same firm during the quarter prior to training, and an across-firm control group consisting of workers employed at firms that applied for but did not receive CJT grants. Each approach offers distinct advantages and addresses potential sources of bias in different ways.

The within-firm comparison is appealing because treated and control workers operate in the same firm environment, industry, and local labor market. This ensures exposure to identical firm-level shocks and managerial practices. However, selection into training within a firm may not be random. Supervisors could choose employees with higher motivation or better prospects, or workers might self-select into training opportunities. Such selection would bias estimates if these unobserved differences correlate with post-training earnings growth. The quasi-random rationing generated by the CJT funding caps may force credit-constrained firms to allocate training arbitrarily among equally promising or prospective workers.

The across-firm comparison mitigates within-firm selection by exploiting variation at the firm level. Here, treatment assignment depends on whether a firm's CJT application was awarded funding. Employees at non-awarded applicant firms serve as controls for those at awarded firms. Importantly, CJT grants are generally allocated on a first-come, first-served basis within each funding cycle, subject to available appropriations rather than a formal scoring or ranking system. This, in combination with arbitrary variation in regional CJT budgets and fallow years, implies that, among firms submitting comparable applications within the same cycle, award receipt is plausibly exogenous to unobserved firm or worker characteristics. The main difference between treated and control firms is therefore the receipt of the grant itself, which generates credible variation in training participation.

To further improve comparability between treated and control workers, I reweight the sample using average treatment-on-the-treated inverse probability weights (ATT-IPW). Let $p(X_i) = \Pr(\text{treated} \mid X_i)$ denote the estimated probability of treatment. The weights are

$$w_i = \begin{cases} 1, & \text{if treated} \\ \dfrac{p(X_i)}{1 - p(X_i)}, & \text{if untreated.} \end{cases}$$

This reweighting aligns the covariate distribution of untreated workers with that of treated workers, producing a control group that more closely approximates the counterfactual earnings trajectory of treated workers. The key concern is not simply that treated and control workers differ in observed characteristics, but that these level differences may be correlated with exposure to different trends or shocks. If, for example, higher-earning workers are also employed in firms with steeper earnings growth or more cyclically sensitive industries, unweighted comparisons could confound treatment effects with underlying differences in trajectories. Propensity weighting mitigates this concern by ensuring that treated and control workers are balanced on covariates predictive of both treatment and outcome dynamics. Under WIOA, individuals who are low-income or receiving public assistance may receive priority for training, so I estimate $p(X_i)$ using pre-treatment earnings, public assistance participation, and demographic characteristics.

Tables 4 and 5 summarize observed characteristics for treated and control workers before and after applying the ATT-IPW weights. Prior to weighting, treated individuals were slightly older, had higher pre-enrollment earnings, and were marginally less likely to participate in priority programs than control workers. After weighting, none of these differences remain statistically significant except for age and earnings, which are not economically different. Mean values of age, gender, education, race, pre-training earnings, and employment stability are nearly identical across groups, and standardized mean differences fall below conventional thresholds for imbalance.

4.2 Difference-in-IV approach

As previously discussed, selection into training within a firm raises concerns that CJT_{it} may be endogenous. To address this, I instrument firm-level training grant receipt for CJT_{it}. Consider the following two-stage estimator:

$$Y_{it} = \beta CJT_{it} + \theta_2 X_{it} + \gamma_i + \tau_t + \psi_{s(i,t=0)t} + \varepsilon_{it}, \tag{2}$$

$$CJT_{it} = \alpha FirmCJTAward_{f(i,t=0)t} + \theta_1 X_{it} + \gamma_i + \tau_t + \psi_{s(i,t=0)t} + \nu_{it} \tag{3}$$

where Y_{it} is individual quarterly earnings, CJT_{it} is a binary indicator equal to one in the enrollment quarter and thereafter, $FirmCJTAward_{f(i,t=0)t}$ is a binary indicator equal to one in the quarter that an individual's firm is awarded a CJT training grant and thereafter, X_{it} includes age, age squared, local unemployment rates, and county-level income-per-capita, γ_i are individual fixed effects, τ_t are year-by-quarter fixed effects, and $\psi_{s(i,t=0)t}$ are fixed effects for the industry that individual i worked in when treatment occurred $s(i, t = 0)$ interacted with the calendar year (i.e., industry-by-year fixed effects).

The DID-IV strategy addresses selection within the firm by using firm-level training grant awards as an instrument for individual training enrollment, i.e., an "access to treatment" instrument. This approach requires an additional identifying assumption, the exclusion restriction, which says that firm grant receipt affects worker earnings only through training participation, not through other channels such as contemporaneous changes in firm demand or productivity unrelated to training. A violation of this rule in my setting would mean that firm receipt of a grant would affect treated workers' earnings through a different pathway than the training itself. For example, a violation of the exclusion restriction would occur if treated firms raised the earnings of all workers at the same time as training began. However, given the relatively modest size of the subsidy, it is unlikely that firms would raise earnings for all workers. Moreover, because firms submit applications that are reviewed and processed sequentially, a marginal worker's access to CJT depends not on individual characteristics but on the timing of the firm's application and the availability of state funds at that moment. This mechanical allocation mechanism limits the role of unobserved worker or firm factors in determining access.

For the DID-IV design, I combine the within-firm and across-firm control groups into a single control group, so that firm grant receipt does not perfectly predict nor fail to predict individual training.

Without propensity weights, the coefficient β provides an empirical estimate of the average effect of CJT training for individuals who comply with $FirmCJTAward_{f(i,t=0)t}$ (i.e., the local average treatment effect or LATE). With propensity weights, the resulting estimate of β is a weighted-LATE, specifically a treatment-on-the-treated-LATE (TT-LATE). The TT-LATE is not equal to the ATT unless some design-based features are present, such as one-sided noncompliance (Aronow and Carnegie, 2013; Słoczyński et al., 2022). Since I am using an access to treatment instrument, which means there are no always-takers, this is a setting with one-sided noncompliance. Thus, ATT-IPW weighting means that the coefficient β provides an empirical estimate of the ATT of enrolling in CJT.

5 Results

5.1 Event-study estimates

To understand the dynamic treatment effects of CJT over time and to assess the validity of the identifying assumptions, I begin by estimating event-study models that interact treatment status with relative time to enrollment. These models serve two purposes. First, they allow a direct evaluation of pre-treatment trends, providing a test of the parallel trends assumption. Second, they trace the evolution of treatment effects over time, showing how earnings respond to training and whether those effects persist.

Figure 2 summarizes the estimated dynamic effects across the DID specifications: within-firm and across applying firms. In all cases, the event-time coefficients are normalized to zero two quarters before enrollment.

Figure 2a focuses on the within-firm comparison, contrasting treated workers with untreated coworkers in the same firm. There are no differential pre-trends before training.[12] The estimate in the quarter immediately preceding training dips slightly below zero, a pattern consistent with the Ashenfelter's dip often observed in evaluations of training programs. Following enrollment, earnings rise sharply and remain elevated for at least five years.

Figure 2b turns to the across applying firm comparison, comparing treated workers to workers at firms that applied for but were not awarded CJT grants. Again, there are no differential pre-trends before training. Earnings increase at the time of training and remain persistently higher over the subsequent five years, indicating that the treatment effects are not short-lived.

5.2 DID and DID-IV estimates

Table 6 reports the estimated effects of CJT enrollment on quarterly earnings using eight quarters of pre-treatment and twenty quarters of post-treatment observations. Each column corresponds to a different specification or control group. All standard errors are clustered at the level of treatment assignment: by individual for the DID within-firm comparisons and by firm for the DID across-firm and DID-IV specifications. The number of individuals and total earnings observations are reported in the table.

The first two columns present conventional DID estimates. In column (1), treated workers are compared to untreated coworkers within the same firm. Earnings rise by an average of \$809 per quarter following training, roughly 5% of pre-training earnings. In column (2), treated workers are compared to employees at firms that applied for but did not receive CJT grants. The estimated gain of \$637 per quarter (4%) is of a similar magnitude. The close alignment of these two estimates across control groups provides reassurance that the results are not driven by idiosyncratic firm-level shocks or unobserved differences between treated and control firms.

Columns (3) and (4) report results from the DID-IV specification, which instruments individual CJT participation with firm-level grant receipt. The first stage shows a strong and statistically significant relationship between firm grant receipt and worker participation conditional on observed covariates, with an F-statistic well above conventional thresholds, confirming instrument relevance. The second stage, shown in column (4), identifies the

[12] Understated standard errors in the pre-period may be more likely than a slight pre-trend, since the specification treats each worker's enrollment as a separate event even when multiple workers are treated simultaneously.

average treatment effect on the treated under the ATT-IPW weighting scheme. The point estimate implies an increase in quarterly earnings of \$406, or approximately 3% of pre-enrollment earnings.

The estimated earnings gains from CJT are comparable to early-career returns to a year of work experience, which range from 3 to 10% depending on worker skill (Deming, 2023). Importantly, these gains are realized after relatively short training programs that last roughly one-third of a year, suggesting that even modest investments in skill development can generate sizable and persistent returns when training is aligned with firm demand.

Figure 3 compares the estimated CJT effects to those from the existing literature on adult-focused education and job training. The CJT effect is in line with the largest published impacts of U.S. government training programs (Card et al., 2018; Heinrich et al., 2013; Andersson et al., 2022). The magnitude and persistence of the CJT effects align more closely with the most successful sectoral training interventions (Katz et al., 2022) and with the returns to sub-baccalaureate credentials such as community college diplomas and certificates (Jepsen et al., 2014; Bahr et al., 2015; Xu and Trimble, 2016).

Two caveats are worth noting. First, the cross-study comparison is based on raw earnings effects rather than normalized percentage changes, which may affect relative magnitudes. Second, several of the external estimates do not report standard errors, so their corresponding point estimates are plotted without confidence intervals.

6 Heterogeneous effects

6.1 Worker characteristics

Earnings gains extend across worker characteristics. Table 7 reports the effects of CJT on earnings by demographic characteristics, including sex, race, and ethnicity. Each column reports results for different specifications and samples. Panels A and B report results by sex, and Panels C, D, and E report results by race and ethnicity. I find CJT generates broad-based earnings improvements across demographic groups, with no single demographic group driving the results.

The distinction between new and incumbent workers is particularly informative. Recall that new workers with less than six months of tenure at the incumbent firm participate in OJT programs, while incumbent workers with more than six months of tenure enroll in IWT programs. Table 8 shows that enrollment in OJT raises quarterly earnings by \$476 relative to untreated new workers and enrollment in IWT raises quarterly earnings by \$699 relative to untreated incumbent workers. These effects are not statistically different from one another, highlighting that the returns to CJT training are similar across new and incumbent workers. The event study estimates in Figures A.2 and A.3 provide the dynamic counterpart to these average effects, tracing earnings trajectories in within-firm and across-firm. There is no evidence of differential pre-trends, and earnings rise at treatment with effects persisting for at least 5 years.

6.2 Firm characteristics

Earnings gains extend across firm characteristics. Table 9 reports the effects of CJT on earnings by industry sector of the firm. Enrolling in CJT increases quarterly earnings by $355 per quarter (2%) in the manufacturing sector and $1,503 per quarter (9%) in the wholesale sector. Training supported through CJT in administration and construction sectors, although imprecise, have point estimates that are large and consistent with the main findings.

Table 10 reports the effects of CJT on earnings by firm size. Firm size matters for CJT earnings gains; working at a small firm with fewer than 50 employees and enrolling in CJT generates an additional $824 per quarter (5%) relative to similar untreated workers using my preferred specification. Customized job training in medium and large firms does not generate consistent significant earning effects across specifications.

7 The returns to skill specificity

7.1 Dimensions of skill specificity

In practice, job training is a combination of transferable and specific skills. A central challenge in the firm training literature is measuring skill specificity. The CJT applications and contracts provide detailed curricula and course descriptions that specify training objectives, instructional content, and competencies to be obtained. To classify these skills, I develop a NLP protocol that maps each training description to a collection of transferability and specificity metrics, and I validate the NLP protocol with a statewide employer survey.

I identify four dimensions of specificity, reflecting the varying scope of training generalization. First, a training teaches firm-specific skills when its content is unlikely to be applicable to tasks performed at other firms. Second, a training teaches occupation-specific skills when its content is unlikely to be transferable to other major occupation groups, defined as the first two digits of the six-digit O*NET occupation code. Third, a training teaches industry-specific skills when its content is unlikely to be transferable to other industrial sectors, defined as the first two digits of the six-digit NAICS industry code. Fourth, a training teaches general skills when its content is likely to be broadly applicable to many different industrial, occupational, and firm contexts.

7.2 An NLP classification of CJT training

The NLP classification of skills centers on detailed work activity (DWA) statements from the U.S. Department of Labor's O*NET database. O*NET organizes information hierarchically: at the base, there are 1,016 occupations in 23 major occupation groups. These occupations are described by 17,536 unique "task statements." These tasks are mapped to 2,087 DWAs. Each occupation is connected to a set of DWAs, and DWAs may be shared across occupations. The DWAs are further grouped into 332 "intermediate work activities," 55 "generalized work activities," and 44 "basic and cross-functional skills." An example of a DWA is "design electromechanical equipment or systems." This DWA is associated with nine occupations, such as mechatronics engineers, aerospace engineers, and automotive engineers. All nine of these occupations are in the same major occupation group: architecture and engineering occupations.

The first step to associating each training description with the four measures of skill specificity is to link the training description to a collection of DWAs. Each training description was compared to all 2,087 DWAs using OpenAI's GPT-4o mini, a large language model (LLM), guided by structured annotation criteria. I asked the LLM to answer two yes/no questions for each training description-DWA pair: (i) is the training *intended* to improve performance in the DWA, and (ii) is the training *likely* to improve performance in the DWA given the described training content. A training was said to be "likely to improve performance in a DWA" if the described content reasonably implied that the activity would be developed, even if not explicitly stated (e.g., training which included "reading blueprints" likely improves the DWA "interpret technical drawings"). To obtain the collection of DWAs associated with a training description, I kept all DWAs that received a "yes" answer to either of the two questions posed to the LLM.

In the second step, I compute measures of skill transferability by type. For full details on the creation of the four measures of skill specificity and transferability, see Appendix B.1.

To compute the firm-specificity, each training description is evaluated by the LLM for references to firm-specific skills, competencies, processes, or activities. The model assigns a score on a five-point Likert scale, where 1 indicates no firm-specific elements, and 5 indicates entirely firm-specific content. The resulting score was normalized to land between 0 and 1.

The second measure is occupation-specificity. Using O*NET's DWA-to-major-occupation-group crosswalk, I identify the set of occupation groups linked to each training description (with multiplicity). Occupation-specificity is high when the associated DWAs appear in relatively few occupation groups and low when they are shared across many groups.

The third measure is industry-specificity. I combine the O*NET crosswalk with the U.S. Bureau of Labor Statistics' Occupational Employment and Wage Statistics (OEWS) data, which reports the distribution of occupations across industry sectors. This mapping provides empirical weights connecting DWAs to industries via the occupation mapping. Industry-specificity is high when a training's DWAs are concentrated in few sectors and low when they span many sectors.

Finally, I compute a generality score as the ratio of matched DWAs to all possible DWAs. A higher score indicates that a program produces broadly applicable human capital, while a lower score reflects a more narrowly transferable training program.

To validate these measures, I conducted an employer survey that asked participating firms to assess the transferability of skills taught in their programs. Specifically, firms identified whether the competencies acquired would be useful only at their firm, within the occupation of the worker being trained, within their industry, or across a wide range of employers. See Appendix C.5 for details.

7.3 The composition of CJT programs by skill type

Figure 4 presents the distribution of skill types across CJT programs from the NLP analysis. The results show that CJT produces skills that are 87% transferable or partially transferable and 13% non-transferable. The relatively small share of firm-specific training is consistent with the rationale for public subsidies, since the benefits of training are likely to extend beyond the firm that provides it.

Customized job training emphasizes occupation- and industry-specific competencies, while general and firm-specific content make up only a small share. On average, 3% of training is general, 41% occupation-specific, 43% industry-specific, and 13% firm-specific, with standard deviations of 5%, 8%, 4%, and 28%, respectively.

Figure 5 presents word clouds generated from the NLP. Each word cloud presents the most common words in the top 100 training descriptions ranked by their skill specificity. General training emphasizes broad, transferable workplace competencies such as safety, understanding, and workplace quality. Industry-specific training highlights technical skills such as maintenance, machine operation, and welding. Occupation-specific training reflects specialized functions such as care, wound management, and patient education. Firm-specific training centers on equipment, operations, and maintenance practices.

Figures A.4a and A.4b further demonstrate that these distributions are stable over time and across industries, suggesting that CJT programs consistently produce skills with a high degree of transferability.

7.4 The impact of CJT on earnings by skill type

The returns to CJT vary by the degree of skill transferability. Table 11 reports enrollment effects for trainees whose programs rank in the top quartile of each skill category (general, industry-specific, occupation-specific, and firm-specific) based on the NLP measures developed in Section 7.2. Enrolling in a CJT program focused on producing industry-specific skills increases worker earnings by $1,367 per quarter (9%) relative to untreated workers using my preferred specification. Although imprecisely estimated, the returns to highly occupation- and firm-specific CJT programs are in line with the overall effect of CJT enrollment.

The magnitude of the gains from programs focused on industry-specific skills is comparable to the well-documented 10% return to an additional year of schooling (Card, 1999; Bhuller et al., 2017; Gunderson and Oreopolous, 2020). However, the effects from CJT are generated from training that is typically completed within a third of a year. Moreover, these estimates are comparable to or larger than the average effect of sectoral training documented by Katz et al. (2022) and Bollinger and Troske (2025).

While Becker's classic framework drew a sharp line between general and specific human capital, the evidence here suggests that skill types may matter jointly rather than in isolation. This perspective is reinforced by the argument that labor market frictions can blur the boundary between general and specific training (Acemoglu and Pischke, 1998, 1999a,b), and the skill-weights approach, which emphasizes how combinations of competencies enhance worker productivity (Lazear, 2009). Viewed through this lens, the estimates are consistent with the idea that skills are produced in bundles and may act as complements to one another.

8 Worker mobility

The earnings effects of CJT may arise through worker mobility. First, training may strengthen workers' attachment to their incumbent employer or industry by raising firm-specific productivity and creating incentives for employers to retain trained workers. Second, training may generate portable skills that enable workers to transition to new firms or industries and capture earnings gains elsewhere.

Table 12 presents estimates from an OLS regression of treatment on a binary variable, which is one if an individual remains incumbent at the firm (or in the industry) four years post enrollment. Treated workers are 10 percentage points (pp) more likely to remain with their training firm and 11 pp more likely to remain in the same industry relative to controls.

To put these magnitudes in context, the median employee tenure in the U.S. was 3.9 years as of January 2024. The fact that CJT participants exhibit higher retention than this national benchmark may underscore how training program content can bind workers to their incumbent employer. On the one hand, such retention may reflect productivity gains that employers value most internally, allowing workers to realize earnings increases through promotions, firm-specific pay policies, or industry-specific career ladders. On the other hand, it may indicate that CJT generates human capital that is less transferable across employers; this is consistent with the NLP analysis showing that most of CJT training content is industry- and occupation-specific, which is partially transferable. Despite providing transferable skills, CJT enrollees have a higher propensity to remain at their firm.

A complementary channel points to the portability of skills across employers. Distinguishing between workers who stay and those who leave, as can be seen in Table 13, shows that the earnings effects are muted for stayers but positive for leavers. Those who exit their training firm or industry experience earnings gains of $323 to $475 per quarter, indicating that CJT leavers are moving in ways that raise their earnings. These results suggest that CJT generates competencies that transfer across firms and industries, enabling workers to leverage training into higher-paying jobs elsewhere. This interpretation is also consistent with the NLP analysis that programs emphasize industry- and occupation-specific skills, which are portable across employers even if they remain rooted in particular sectors.

9 Costs and benefits of customized job training

I combine my estimates with program costs to conduct cost-benefit analyses by applying an MVPF framework and traditional back-of-the-envelope cost-benefit calculations.

9.1 Willingness to pay

Since the individual does not pay for their own training, the WTP for the program is calculated as the present value of the benefit over 5 years with a discount factor of 3% per year. I calculate the benefit over 5 years because that is where my window ends, but one could argue that the earnings gains could be persistent over the life cycle. The persistence of CJT impacts contrasts sharply with the modest and short-lived effects found in most evaluations of broadly targeted federal training programs (LaLonde, 1986; Card and Sullivan, 1988; Heckman et al., 1999; Hotz et al., 2006; Greenberg et al., 2003). Even the Trade Adjustment Assistance program studied by Hyman (2018) generated substantial cumulative earnings gains of roughly $50,000 over ten years, but its annual effects started to decline by the end of the third year.

Willingness to pay calculations for all estimates are presented in Table 14. For example, the quarterly earnings effect estimate for enrollment in CJT is $405.53 per quarter using the

DID-IV model. This is an annual earnings gain of \$1,622.12, and discounting this at 3% over 5 years gives a WTP of

$$WTP = \sum_{i=1}^{5} \frac{1622.12}{(1+0.03)^t} = 1622.12 \times \frac{1-(1+0.03)^{-5}}{0.03} \approx \$7,428.83.$$

This is a sizable earnings benefit relative to the cost of the program; it is more than five times the average program cost.

9.2 Program cost

Upfront program costs are modest by the standards of workforce training and higher education subsidies. Table 14 shows that the upfront public cost per trainee in a CJT program is \$1,102.25. This was calculated empirically using the cost data provided by the state. To put this in perspective, federal spending on WIOA in 2020 averaged roughly \$2,300 per person trained, with only about \$0.5 billion of the \$4 billion program budget actually recorded as training expenditures (see Deming et al. (2023)). By comparison, Pell Grants cost the federal government about \$4,090 per student per year, reaching 6.4 million students annually (College Board, 2023).

Since the program has positive effects, there is an increase in the federal income tax revenue that will be recouped by the government over five years. This cost calculation does not account for indirect cost reductions resulting from decreased public assistance or employment service participation following CJT enrollment. Table 6 gives the average pre-enrollment quarterly earnings of CJT enrollees as \$15,909.45, which is \$63,637.80 per year. For a single individual, the 2024 standard deduction on federal income tax was \$14,600, meaning that the average taxable income for CJT enrollees is \$49,037.80. This corresponds to a 2024 marginal tax rate of 22% on additional earnings. Since the WTP is the same as the discounted increased earnings gain over 5 years, we can see that the government will recoup 22% of the WTP as federal income tax, decreasing the public cost per training by the same amount. For my preferred estimate, the WTP was \$7,428.83, which implies an increase in federal income tax revenue of \$1,634.34. Subtracting this from the upfront cost per trainee leaves us with a net cost per trainee of -\$532.09. I provide 95% confidence intervals in square brackets below each calculation.

For back-of-the-envelope cost-benefit calculations, I calculate the number of quarters after training until the program gains exceed the program cost (ignoring any federal income tax revenues). In the calculation of the MVPF, any training cost that the firm incurs was ignored because these are not public dollars. However, for back-of-the-envelope cost-benefit calculations, it makes sense to consider this cost when measuring the number of quarters to pay off. The private cost of CJT training programs depends on the firm's size. The average firm in my sample has more than 100 employees, which implies that the firm matches 50% of the government subsidy. I estimate the private cost of training using this rule, which suggests the firms pay an average of $526.13 per CJT trainee. Therefore, the average total cost per trainee is $1,551.38 with the inclusion of the private cost. Even with the inclusion of private cost, CJT is substantially less expensive on a per-participant basis than other programs.

The back-of-the-envelope cost-benefit calculations do not account for changes in productivity during training. A limitation of this analysis is that I do not observe hours spent in training and how training impacts productivity. Therefore, it is unclear how much indirect cost is incurred by the firm in the form of lost productivity during training. On the other hand, I also cannot observe the gains in productivity during and after training beyond proxying for productivity using earnings.

9.3 MVPF results

Using the estimates of WTP and costs, I calculate the MVPF for CJT within 5 years and find that it is an efficient use of government funds. Table 14 reports MVPF results across specifications. In my preferred specification, the MVPF of CJT is infinite. The 95% confidence interval for this MVPF is [207.93, inf].

The intuition for this result is straightforward. Participants experience sizable earnings gains, which in turn generate additional federal income tax revenue. These revenues are large enough to offset the direct program costs, producing negative net costs. In the unified framework of Hendren and Sprung-Keyser (2020), a policy with positive WTP and negative net costs yields an infinite MVPF. Provided that all participants have positive WTP, this constitutes a Pareto improvement.[13]

Even when excluding tax revenues, CJT remains highly cost-effective. The MVPF is 6.74, with a 95% confidence interval of [4.45, 9.03]. This implies that every dollar of state expenditure generates nearly seven dollars in direct benefits to participants, apart from any fiscal externalities through federal taxes. Relative to other job training policies that have been studied in the MVPF literature, including sectoral training, CJT has the highest average MVPF.

I also examine heterogeneity across worker types. For new workers, the MVPF (including tax revenues) is 9.23, with a 95% confidence interval of [3.23, 27.26]. For incumbent workers, where the combination of large earnings gains and fiscal offsets is strongest, the MVPF is

[13] Hendren and Sprung-Keyser (2020) note that such cases in the tax literature are associated with "Laffer effects," where infinite MVPFs raise total revenue, and produce this effect.

infinite.

From a policy perspective, the relevant question is how the returns to CJT compare to alternative uses of public funds. To benchmark CJT, Figure 6 reproduces the canonical plot from Hendren and Sprung-Keyser (2020) with the MVPF estimates from this analysis superimposed. In this figure, the horizontal axis reports the average age of beneficiaries, and the vertical axis reports the estimated MVPF. As emphasized by Hendren and Sprung-Keyser (2020), this plot provides a unified metric of programs' "bang for the buck," enabling direct welfare comparisons across domains.

Restricting attention to policies targeting adults highlights a stark contrast. Most job training programs for adults deliver modest returns: MVPFs range from –0.23 to 1.48, with an average of 0.44. Other adult-oriented interventions—such as college subsidies, health insurance expansions, food stamps, tax credits, and unemployment insurance—tend to cluster near one, rarely exceeding it. Against this backdrop, the MVPF estimates for CJT fall in the set of policies with infinite MVPFs, joining a small group of adult college interventions that more than recoup their fiscal costs.

Placing CJT within the Hendren–Sprung-Keyser framework illustrates two key points. First, CJT substantially outperforms historical adult job training programs, suggesting that its firm-designed structure addresses limitations that have historically constrained training efficiency. Second, CJT compares favorably even to adult-targeted policies outside of the training domain, delivering fiscal efficiency on par with the select few interventions that generate infinite MVPFs. In short, CJT emerges as an unusually effective deployment of public resources within the adult policy landscape.

9.4 Back-of-the-envelope cost-benefit calculations

As a complement to the MVPF framework, I also present traditional back-of-the-envelope cost-benefit calculations, which provide a simple benchmark for assessing program cost recovery. The earnings gains from CJT offset the combined public and private costs of training within 4.08 quarters, with a 95% confidence interval of [3.05, 6.18]. Thus, program expenditures are recouped in just over one year under my preferred estimate.

I also examine heterogeneity across worker types. For new workers, the payback period, inclusive of both public and private costs, is 6.27 quarters, with a 95% confidence interval of [2.87, 7.71]. This longer horizon reflects their lower initial earnings and the slower accumulation of benefits. For incumbent workers, the payback period, again including both public and private costs, is 1.92 quarters, with a 95% confidence interval of [1.41, 3.02]. These results underscore that while both groups ultimately generate sufficient earnings gains to cover training costs, the timing of cost recovery differs across worker types.

10 Conclusion

This paper provides new causal evidence on the earnings returns to subsidized employer-designed job training. Using novel training data linked with administrative data from Tennessee that merge firm applications, curricula, and program costs, I show that participation in CJT increases workers' quarterly earnings by $406 (3%), with effects persisting for at least five years. These gains are comparable to early-career returns to an additional year of work experience and exceed those of most evaluated job training programs. CJT is highly cost-effective: benefits exceed expenditures in a year, and the implied marginal value of public funds is infinite.

By opening the "black box" of firm-designed training and using NLP and employer validation, I find that CJT programs primarily teach industry- and occupation-specific skills that are partially transferable across firms. The results show that 87% of training content is transferable or partially transferable, and only 13% is firm-specific. Workers trained in programs emphasizing industry-specific content experience the largest gains. This distribution is consistent with the rationale for public subsidies, since much of the benefit from training extends beyond the firm that provides it and generates shared returns to both workers and employers. These findings challenge standard models predicting that firms avoid providing transferable training, suggesting instead that firms willingly invest in partially transferable skills under labor-market frictions that permit shared gains.

The NLP framework developed here also opens new directions for research. Quantifying the mix of transferable and non-transferable skills provides a scalable way to link the content of training to both worker and firm outcomes. This approach can be applied to a wide range of settings—including vocational curricula, apprenticeships, career and technical education, and employer–education partnerships—to understand how skill composition varies across programs, industries, and local labor markets. Even in long-standing systems such as apprenticeships and CTE, we know little about the share of transferable versus non-transferable skills or how this ratio shapes workers' long-term earnings, mobility, and career resilience, or firms' productivity and retention. Applying this framework across contexts could help identify which forms of training produce the highest social returns and clarify the boundary between private and public investment in skill formation.

Looking ahead, an important next step is to examine how CJT affects firm productivity, how skill specificity shapes productivity and resilience, and whether such programs serve as a form of insurance against economic shocks, helping workers and firms recover more quickly after disruptions. Together, these questions can deepen our understanding of how public and private investments jointly build human capital, impact productivity, and affect economic stability.

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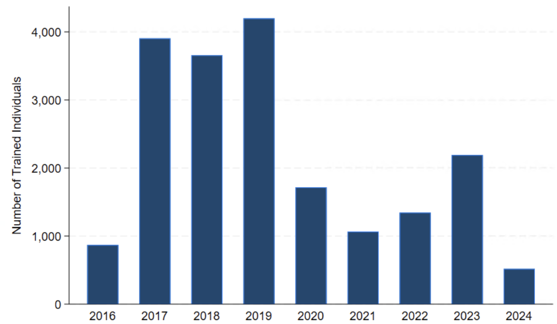
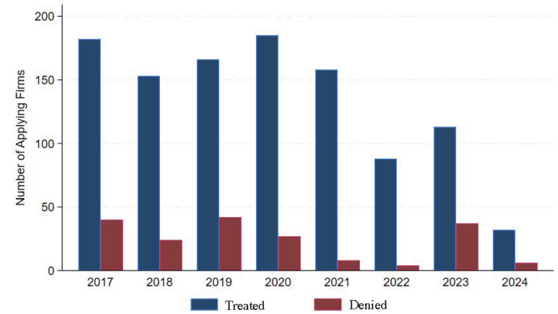
Figures

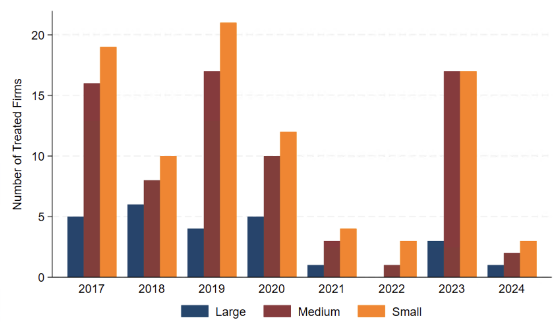
Figure 1: CJT enrollment and applications



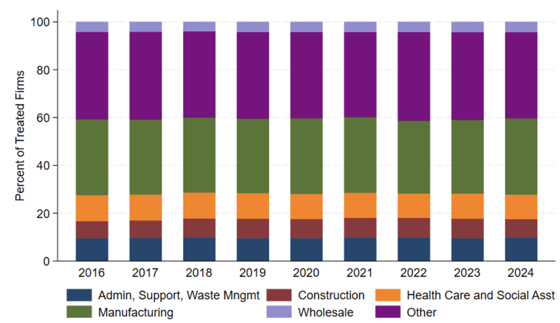
(a) CJT individual enrollment



(b) CJT firm applications



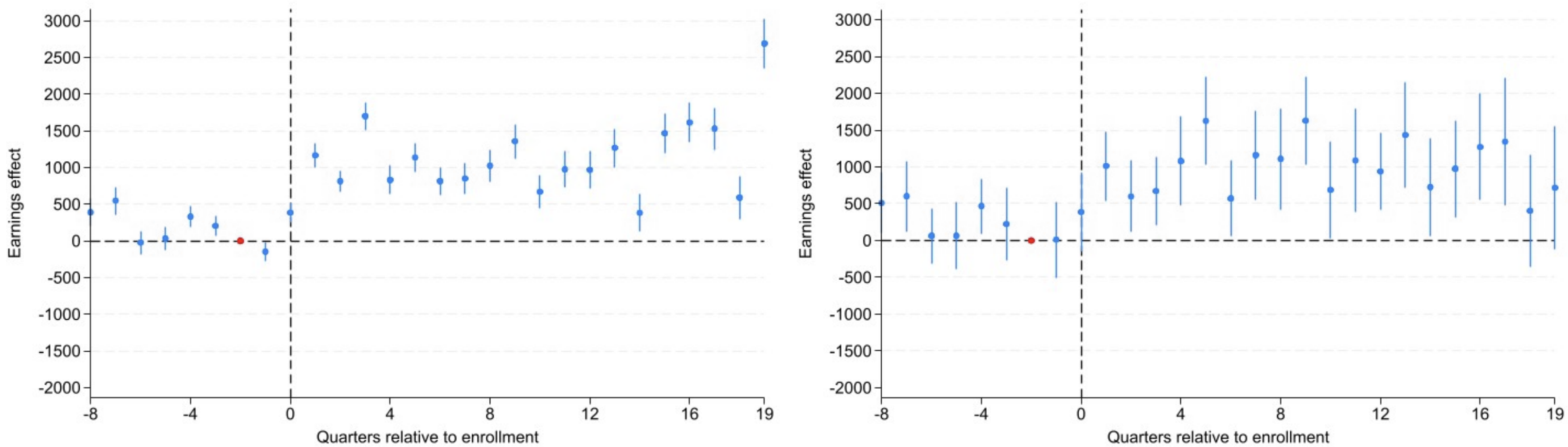
(c) CJT firm size composition



(d) CJT firm industry composition

Notes: This figure plots enrollment data from individual training records maintained by the Tennessee Department of Labor and Workforce Development. Firm applications are drawn from pre-application records maintained by the Tennessee Department of Labor and Workforce Development. Subfigure (a) plots annual counts of individual participants over time. Subfigure (b) plots firm applications and awards over time. Subfigure (c) plots treated firm composition by size. Small firms have fewer than 50 employees, medium firms have between 50 and 249 employees, and large firms have 250 or more employees. Subfigure (d) plots treated firm composition by industry for industries representing more than 5% of the observations.

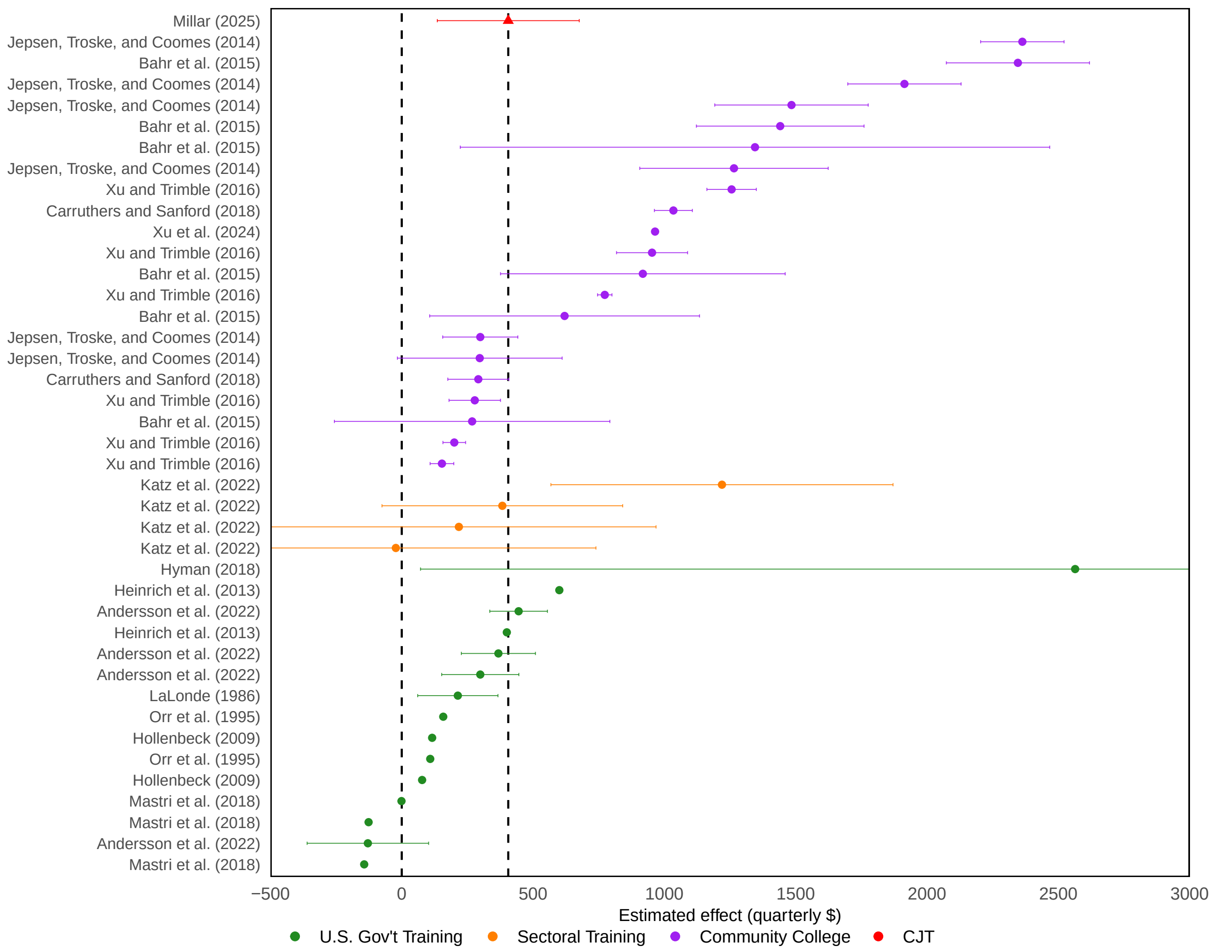
Figure 2: CJT event studies



(a) Within **(b) Across Applying**

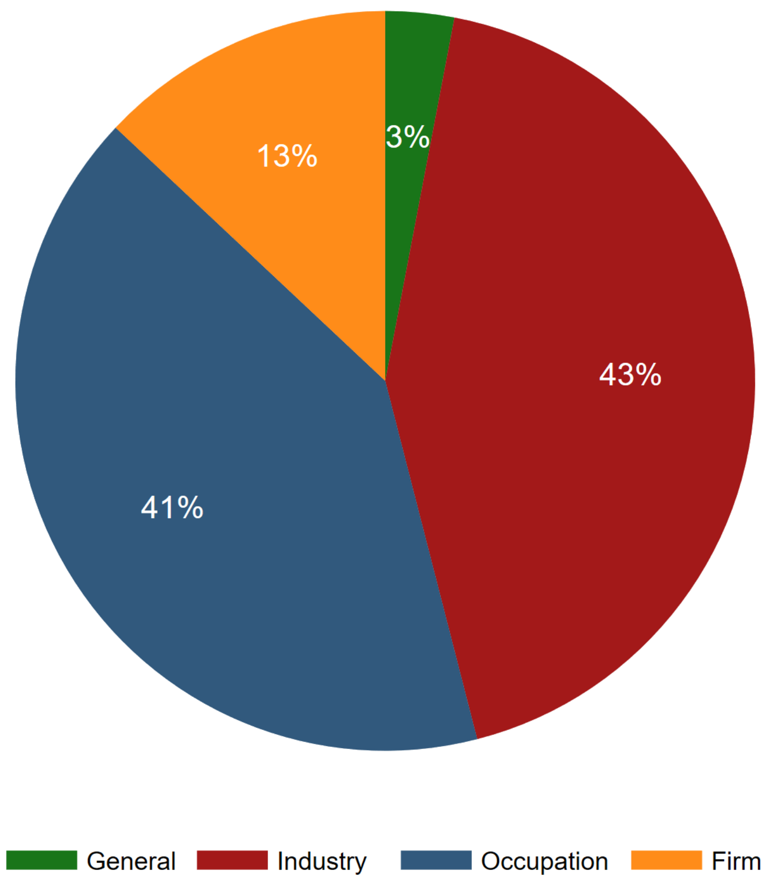
Notes: These figures present the coefficients from event study regressions estimating the earnings relative to training enrollment, with leads and lags in event time. Specifications include individual, time, and industry-by-year fixed effects. Subfigure (a) compares training enrollees to their untreated coworkers. Subfigure (b) compares training enrollees to workers at firms who applied for training grants but did not receive them. All event study regressions are propensity weighted using ATT-IPW weights. 95% confidence intervals are shown. All figures were created by the author using administrative data from the state of Tennessee.

Figure 3: Literature comparison: Returns to education and job training



Notes: This figure compares my preferred estimate of CJT earnings effects to published estimates from prior studies of adult training and education programs. Repeated paper references on the y-axis indicate estimated effects for distinct subpopulations and/or programs. Some external estimates lack reported standard errors; those results are plotted without confidence intervals. The customized job training estimate is derived from Tennessee administrative data from 2016 to 2024.

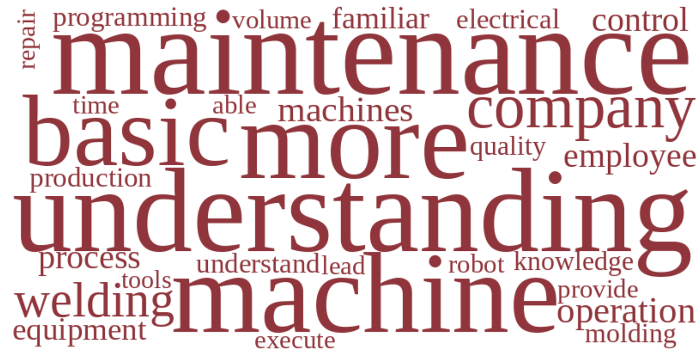
Figure 4: NLP CJT pie chart share of skills



Notes: This figure shows the distribution of training program content across four skill categories (general, occupation-specific, industry-specific, and firm-specific) for CJT training programs. Percentages are generated from natural language processing of training program descriptions. Data were pulled from hand-collected and digitized firm grant applications in Tennessee.

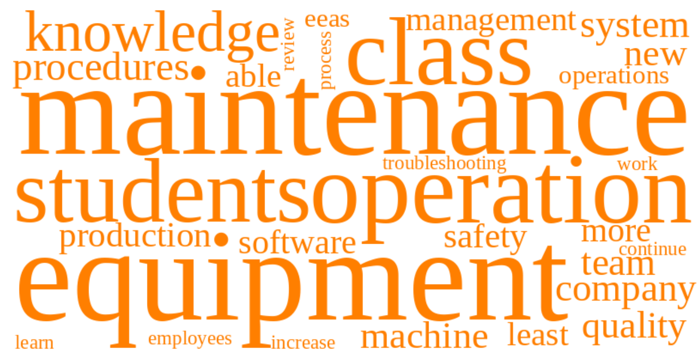
Figure 5: NLP word clouds by skill type

(a) General



(b) Industry-specific

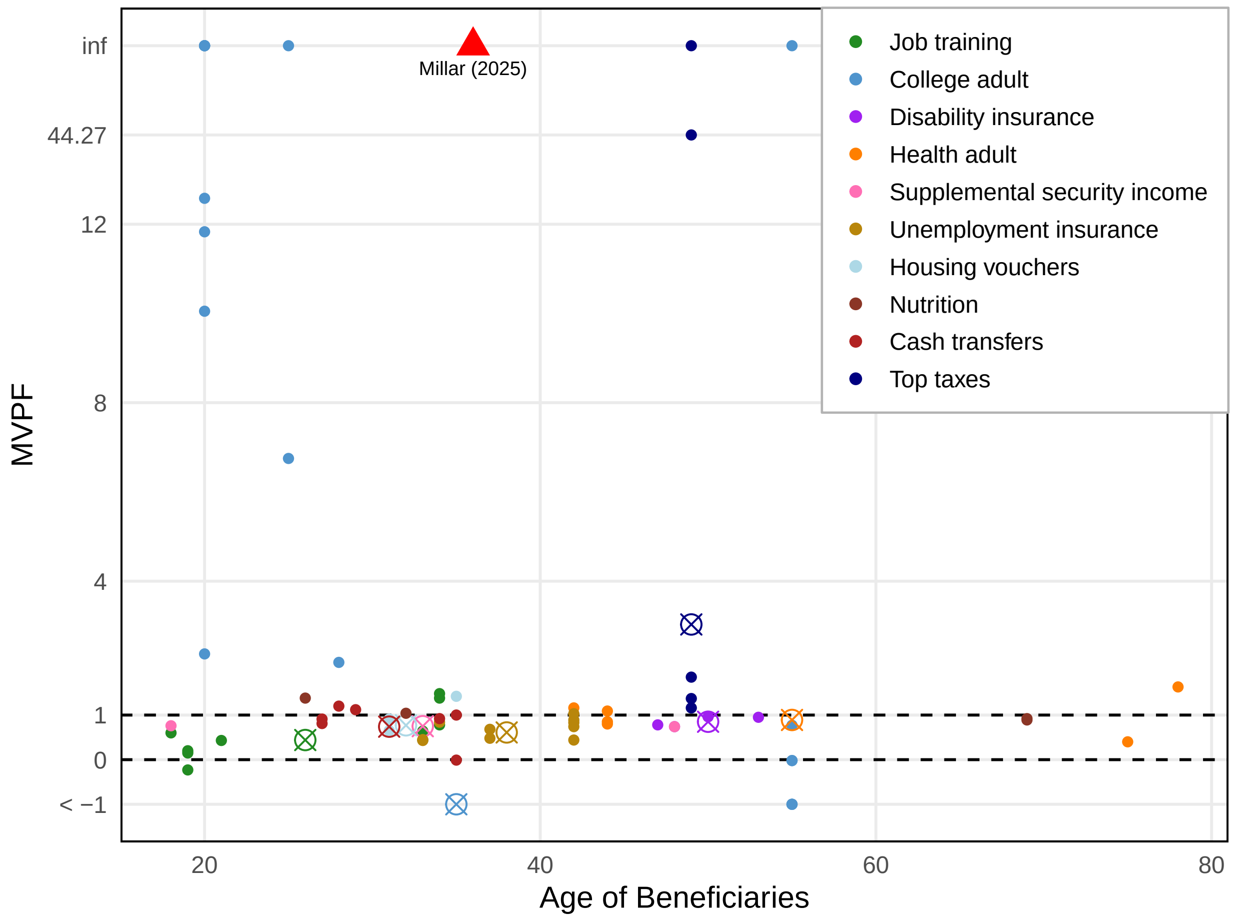
(c) Occupation-specific



(d) Firm-specific

Notes: This figure presents word clouds generated from natural language processing of training program descriptions. Data were pulled from hand-collected and digitized firm grant applications in Tennessee. Subfigures (a)–(d) plot the most common words, excluding the syncategorematic words (e.g., 'the,' 'a,' 'of,' 'some,' 'all'), in the top quartile general, industry-specific, occupation-specific, and firm-specific (respectively) programs as classified by the NLP analysis.

Figure 6: MVPF estimates across programs



Notes: This figure plots the marginal value of public funds (MVPF) estimates for CJT alongside other programs targeting adults following the unified framework in Hendren and Sprung-Keyser (2020). The horizontal axis reports beneficiary age, and the vertical axis reports the estimated MVPF. Customized job training estimates are derived from Tennessee administrative data and cost records. Comparison values for other programs (e.g., job training, higher education, disability insurance, housing vouchers, and transfers) are taken from published estimates reported in Hendren and Sprung-Keyser (2020). Circles denote program-specific estimates; category averages are shown as circles with superimposed X's. Values above 1 imply that benefits exceed costs. Values at or below 1 indicate that program costs exceed estimated benefits. "inf" indicates an infinite MVPF where programs yield estimated benefits but negligible net cost.

Tables

Table 1: Firm descriptive statistics

	Applying Firms				Non-applying firms			
	Accepted	Denied	Difference	p-value	Accepted	Non-applying	Difference	p-value
Panel A: Employment								
Average employees pre-training	396.46	101.11	295.35	0.00	396.46	14.06	382.40	0.00
	(1,472.45)	(326.28)	(16.85)		(1,472.45)	(86.68)	(1.00)	
Average earnings pre-training	\$10,327.74	\$11,857.15	-\$1,529.40	0.00	\$10,327.74	\$11,020.37	-\$692.63	0.00
	(6,261.87)	(5,892.41)	(78.76)		(6,261.87)	(9,843.38)	(56.24)	
Panel B: Industry								
Manufacturing	31.37%	33.07%	-1.70%		31.37%	5.32%	26.05%	
Health Care and Social Assistance	10.59%	15.32%	-4.73%		10.59%	11.18%	-0.59%	
Admin, Support, and Waste Management	9.65%	3.16%	6.49%		9.65%	6.63%	3.02%	
Construction	7.83%	12.95%	-5.12%		7.83%	9.24%	-1.41%	
Wholesale	4.17%	8.40%	-4.23%		4.17%	9.65%	-5.48%	
Other	36.39%	27.10%			36.39%	57.98%		
N (distinct firm observations)	1,291	499			1,291	216,605		

Notes: This table presents variable means and standard deviations for firms that applied for CJT grants and those that did not apply. Columns 1 through 4 compare treated firms to firms that applied for CJT but were denied. Columns 5 through 8 compare treated firms to firms that did not apply for CJT. Employment and industry data are drawn from administrative Unemployment Insurance records and application files from the Tennessee Department of Labor and Workforce Development. Firm industry composition is shown for the five largest industries among treated firms. Industries are presented at the 2-digit NAICS code level.

Table 2: Individual training summary statistics

	Treatment Groups		
	CJT	OJT	IWT
Panel A: Training characteristics			
Days Enrolled in Job Training	121.44	70.07	130.54
	(101.66)	(48.96)	(105.73)
Successful Completion	0.69	0.75	0.68
Unsuccessful Completion	0.03	0.20	0.00
Other	0.28	0.05	0.32
Panel B: Occupation Codes			
Production	40.71%	73.06%	36.27%
Architecture and Engineering	15.68%	2.24%	17.80%
Management	9.66%	1.66%	10.69%
Installation, Maintenance, and Repair	9.19%	1.54%	10.76%
Construction and Extraction	4.56%	3.92%	4.49%
Business and Financial Operations	3.93%	0.27%	4.76%
Office and Administrative Support	3.06%	1.76%	3.34%
Transportation and Material Moving	3.04%	7.45%	2.13%
Life, Physical, and Social Science	1.85%	0.41%	2.25%
Healthcare Support	1.80%	1.65%	1.39%
Healthcare Practitioners and Technical	1.42%	2.39%	1.05%
Arts, Design, Entertainment, Sports, and Media	1.21%	0.05%	1.33%
Community and Social Service	1.03%	0.05%	1.07%
Other occupations	2.59%	3.55%	2.67%
Panel C: Industry Codes			
Manufacturing	65.19%	54.49%	67.06%
Wholesale Trade	6.63%	5.10%	6.91%
Construction	5.80%	4.34%	6.02%
Health Care and Social Assistance	5.34%	5.96%	5.20%
Admin, Support, and Waste Management	5.08%	16.97%	3.00%
Transportation and Warehousing	2.97%	1.38%	3.24%
Mining	1.57%	0.67%	1.75%
Retail Trade	1.46%	3.48%	1.15%
Professional Scientific and Technical Services	1.40%	1.10%	1.46%
Finance and Insurance	1.21%	0.19%	1.39%
Other industries	3.35%	6.32%	2.82%

Notes: This table presents variable means and standard deviations for individuals who enroll in CJT. Variable means are also presented for OJT and IWT enrollees. In Panel A, ‘Other’ includes dropped out of activity, system closed, voided, or unknown. ‘Other’ typically indicates system-closed, which means that the individual or company did not report back to the Tennessee Department of Labor and Workforce Development within 90 days of the last point of contact. Occupations are presented at the 2-digit O*NET code level. Industries are presented at the 2-digit NAICS code level.

Table 3: Individual descriptive statistics

	CJT	OJT	IWT
Panel A: Demographics			
Age at training start	36.343	29.710	36.386
	(10.796)	(8.799)	(0.280)
Female	0.279	0.408	0.022
	(0.449)	(0.492)	(0.146)
Hispanic	0.022	0.020	0.022
	(0.145)	(0.138)	(0.146)
Non-Hispanic Black	0.105	0.344	0.105
	(0.409)	(0.475)	(0.306)
Non-Hispanic White	0.788	0.589	0.787
	(0.409)	(0.492)	(0.409)
Panel B: Education			
Postsecondary enrollment	0.191	0.209	0.191
	(0.393)	(0.407)	(0.393)
Postsecondary credits earned	29.146	17.695	29.132
	(62.930)	(46.094)	(62.926)
Obtained 2-year degree	0.016	0.010	0.016
	(0.125)	(0.100)	(0.125)
Obtained 4-year degree	0.027	0.013	0.027
	(0.162)	(0.115)	(0.162)
Obtained certificate	0.030	0.046	0.030
	(0.171)	(0.211)	(0.170)
Missing postsecondary data	0.283	0.352	0.283
	(0.450)	(0.478)	(0.451)
Panel C: Public assistance participation			
Supplemental Nutrition Assistance Program participation	0.000	0.001	0.000
	(0.012)	(0.037)	(0.012)
Unemployment Insurance benefit eligible	0.015	0.081	0.015
	(0.122)	(0.273)	(0.120)
Panel D: Employment and Earnings			
Pre-enrollment proportion of positive earnings quarters	0.90	0.81	0.92
Quarterly earnings (pre-enrollment)	12,142.37	6,373.69	13,007.93
	(10,729.33)	(4,660.27)	(11,094.54)
# of quarters at training firm (post-enrollment)	7.25	7.26	11.25
	(6.53)	(6.53)	(6.84)
Adult Education and Development enrollment	0.001	0.003	0.001
	(0.024)	(0.052)	(0.025)
Trade Adjustment Assistance enrollment	0.000	-	0.000
	(0.012)	-	(0.012)
Workforce Innovation and Opporutnity Act enrollment	0.001	0.055	0.001
	(0.022)	(0.229)	(0.033)
Wagner-Peyser enrollment	0.022	0.164	0.021
	(0.148)	(0.371)	(0.142)
N (distinct year-quarter observations)	509,390	77,777	434,449

Notes: This table presents variable means and standard deviations for individual trainees prior to treatment. Demographic characteristics were generated from Tennessee K-12, postsecondary, and Unemployment Insurance records. “Missing postsecondary data” is an indicator for an individual who graduated from high school prior to 2005.

Table 4: CJT covariate balance before propensity weighting

	Within Firm				Across Applying Firms			
	Control	Treat	Difference	p-value	Control	Treat	Difference	p-value
Panel A: Demographics								
Age at training start	33.252	36.343	3.092	0.000	35.846	36.343	0.498	0.001
	(10.623)	(10.796)	(0.137)		(11.251)	(10.796)	(0.150)	
Female	0.434	0.279	-0.155	0.000	0.488	0.279	-0.209	0.000
	(0.496)	(0.449)	(0.006)		(0.500)	(0.449)	(0.007)	
Hispanic	0.027	0.022	-0.005	0.013	0.027	0.022	-0.005	0.013
	(0.161)	(0.145)	(0.002)		(0.162)	(0.145)	(0.002)	
Non-Hispanic Black	0.242	0.105	-0.138	0.000	0.211	0.105	-0.107	0.000
	(0.429)	(0.306)	(0.005)		(0.408)	(0.306)	(0.005)	
Non-Hispanic White	0.665	0.788	0.123	0.000	0.690	0.788	0.097	0.000
	(0.473)	(0.409)	(0.006)		(0.462)	(0.409)	(0.006)	
Panel B: Education								
Postsecondary enroll	0.183	0.191	0.008	0.122	0.176	0.191	0.015	0.004
	(0.387)	(0.393)	(0.005)		(0.381)	(0.393)	(0.005)	
Postsecondary credits earned	30.052	29.146	-0.906	0.275	36.178	29.146	-7.032	0.000
	(64.404)	(62.930)	(0.830)		(70.364)	(62.930)	(0.931)	
Obtained 2-year degree	0.017	0.016	-0.001	0.405	0.014	0.016	0.001	0.410
	(0.130)	(0.125)	(0.002)		(0.119)	(0.125)	(0.002)	
Obtained 4-year degree	0.031	0.027	-0.004	0.085	0.035	0.027	-0.008	0.001
	(0.172)	(0.162)	(0.002)		(0.184)	(0.162)	(0.002)	
Obtained certificate	0.028	0.030	0.002	0.441	0.022	0.030	0.008	0.000
	(0.166)	(0.171)	(0.002)		(0.147)	(0.171)	(0.002)	
Missing postsecondary data	0.354	0.283	-0.071	0.000	0.334	0.283	-0.051	0.000
	(0.478)	(0.450)	(0.006)		(0.471)	(0.450)	(0.006)	
Panel C: Priority characteristics								
AED enrollment	0.001	0.001	-0.000	0.909	0.000	0.001	0.000	0.733
	(0.025)	(0.024)	(0.000)		(0.022)	(0.024)	(0.000)	
SNAP enrollment	0.000	0.000	-0.000	0.292	0.000	0.000	-0.000	0.282
	(0.020)	(0.012)	(0.000)		(0.021)	(0.012)	(0.000)	
TAA enrollment	0.000	0.000	0.000	0.320	-	-	-	-
	(0.007)	(0.012)	(0.000)		-	-	-	
WIOA enrollment	0.002	0.001	-0.001	0.091	0.001	0.001	0.000	0.908
	(0.147)	(0.035)	(0.001)		(0.034)	(0.035)	(0.000)	
WP enrollment	0.027	0.022	-0.004	0.041	0.017	0.022	0.005	0.003
	(0.161)	(0.148)	(0.002)		(0.130)	(0.148)	(0.002)	
UI eligible	0.034	0.015	-0.019	0.000	0.027	0.015	-0.012	0.000
	(0.181)	(0.122)	(0.002)		(0.162)	(0.122)	(0.002)	
Panel D: Earnings								
Earnings t = -1	$12,719.860	$16,668.980	$3,949.123	0.000	$14,493.940	$16,668.980	$2,175.042	0.000
	(8,347.492)	(8,215.695)	(107.676)		(9,199.836)	(8,215.695)	(121.660)	

Notes: This table reports covariate balance between CJT participants and control groups prior to inverse probability weighting. Columns compare treated individuals to controls within the same firm and across applying firms. Reported variables include demographics, education, and pre-enrollment characteristics. Differences and associated p-values are shown for each covariate. Standard errors are reported in parentheses. "—" indicates that one of the two groups had strictly zero values in that variable.

Table 5: CJT covariate balance after propensity weighting

	Within Firm				Across Applying Firms			
	Control	Treat	Difference	p-value	Control	Treat	Difference	p-value
Panel A: Demographics								
Age at start of training	35.006	36.343	1.337	0.000	36.179	36.343	0.164	0.279
	(10.986)	(10.796)	(0.144)		(11.158)	(10.796)	(0.151)	
Female	0.281	0.279	-0.002	0.773	0.281	0.279	-0.002	0.692
	(0.449)	(0.449)	(0.006)		(0.450)	(0.449)	(0.006)	
Hispanic	0.022	0.022	0.000	0.960	0.022	0.022	-0.000	0.946
	(0.146)	(0.145)	(0.002)		(0.146)	(0.145)	(0.002)	
Non-Hispanic Black	0.105	0.105	-0.000	0.989	0.106	0.105	-0.001	0.723
	(0.306)	(0.306)	(0.004)		(0.308)	(0.306)	(0.004)	
Non-Hispanic White	0.789	0.788	-0.001	0.833	0.788	0.787	-0.001	0.928
	(0.408)	(0.409)	(0.005)		(0.409)	(0.409)	(0.006)	
Panel B: Education								
Postsecondary enroll	0.185	0.191	0.007	0.207	0.183	0.191	0.008	0.145
	(0.388)	(0.393)	(0.005)		(0.387)	(0.393)	(0.006)	
Postsecondary credits earned	29.119	29.146	0.027	0.974	29.025	29.146	0.120	0.887
	(61.913)	(62.930)	(0.027)		(60.354)	(62.930)	(0.847)	
Obtained 2-year degree	0.015	0.016	0.001	0.704	0.015	0.016	0.001	0.486
	(0.122)	(0.125)	(0.002)		(0.120)	(0.125)	(0.002)	
Obtained 4-year degree	0.027	0.027	-0.000	0.818	0.028	0.027	-0.001	0.681
	(0.163)	(0.162)	(0.002)		(0.164)	(0.162)	(0.002)	
Obtained certificate	0.029	0.030	0.001	0.796	0.029	0.030	0.001	0.755
	(0.169)	(0.171)	(0.002)		(0.169)	(0.171)	(0.002)	
!Missing postsecondary data	0.248	0.282	-0.002	0.729	0.286	0.283	-0.003	0.587
	(0.451)	(0.450)	(0.006)		(0.452)	(0.450)	(0.006)	
Panel C: Priority characteristics								
AED enrollment	0.001	0.001	0.000	0.965	0.001	0.001	0.000	0.955
	(0.024)	(0.024)	(0.000)		(0.024)	(0.024)	(0.000)	
SNAP enrollment	0.000	0.000	0.000	0.979	0.000	0.000	-0.000	0.945
	(0.012)	(0.012)	(0.000)		(0.013)	(0.012)	(0.000)	
TAA enrollment	0.000	0.000	-0.000	0.939	-	-	-	-
	(0.013)	(0.012)	(0.000)		-	-	-	
WIOA enrollment	0.001	0.001	0.000	0.721	0.001	0.001	0.000	0.722
	(0.032)	(0.035)	(0.000)		(0.032)	(0.035)	(0.000)	
WP enrollment	0.022	0.022	0.000	0.886	0.022	0.022	0.001	0.809
	(0.147)	(0.148)	(0.002)		(0.146)	(0.148)	(0.002)	
UI eligible	0.015	0.015	0.000	0.979	0.015	0.015	-0.000	0.933
	(0.122)	(0.122)	(0.002)		(0.122)	(0.122)	(0.002)	
Panel D: Earnings								
Earnings t = -1	$16,981.400	$16,668.980	-$312.420	0.010	$16,844.690	$16,668.980	-$175.702	0.168
	(10,252.260)	(8,215.695)	(120.970)		(10,023.600)	(8,215.695)	(127.380)	

Notes: This table reports covariate balance between CJT participants and control groups after inverse probability weighting. Columns compare treated individuals to controls within the same firm and across applying firms. Reported variables include demographics, education, and pre-enrollment characteristics. Differences and associated p-values are shown for each covariate. Standard errors are reported in parentheses. "—" indicates that one of the two groups had strictly zero values in that variable.

Table 6: Returns to enrollment in CJT

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Post enrollment	809.41***	636.62***	0.59***	405.53***
	(58.69)	(142.06)	(0.02)	(137.88)
F-statistic			787.61	
Mean pre-enrollment	15,909.45	15,909.45		15,909.45
% mean pre-enrollment	5.09%	4.00%		2.55%
N (individual-quarters)	1,460,779	777,855		2,194,019
N (individuals)	65,688	38,461		98,426
R-squared	0.75	0.75		0.02
Individual FE	Yes	Yes		Yes
Year-by-quarter FE	Yes	Yes		Yes
Industry-by-year FE	Yes	Yes		Yes
Clustering	Individual	Firm		Firm

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings, pre-enrollment mean earnings, and the percentage earnings gains relative to the mean pre-enrollment earnings. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. The difference-in-differences (DID) "Within" specification compares treated workers to untreated workers in the same firm; standard errors are clustered at the individual level. The DID "Across Applying" specification compares treated workers to untreated workers at firms that applied but were not treated; standard errors are clustered at the firm level. The DID-IV "Within and Across" specification instruments for individual enrollment using firm-level application, and compares treated workers to untreated workers at treated firms and firms that applied but were not treated; standard errors are clustered at the firm level. Estimates rely on administrative data from Tennessee.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: Return to CJT enrollment by demographics

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Panel A: Males				
Post enrollment	824.03***	652.34***	0.58***	422.54***
	(72.25)	(174.45)	(0.02)	(165.03)
F-statistic			827.25	
% mean pre-enrollment	4.90%	3.88%		2.51%
N (individuals)	38,504	21,264		54,969
Panel B: Females				
Post enrollment	726.50***	610.29***	0.61***	374.18**
	(95.03)	(103.56)	(0.03)	(150.35)
F-statistic			408.43	
% mean pre-enrollment	5.36%	4.50%		2.76%
N (individuals)	27,638	17,563		43,347
Panel C: Hispanic				
Post enrollment	1,057.40***	953.59***	0.57***	682.53
	(357.04)	(320.49)	(0.03)	(580.81)
F-statistic			300.02	
% mean pre-enrollment	8.17%	7.37%		5.27%
N (individuals)	1,731	1,009		2,596
Panel D: Non-hispanic black				
Post enrollment	860.83***	855.49***	0.60***	495.03**
	(178.06)	(194.61)	(0.02)	(222.45)
F-statistic			642.40	
% mean pre-enrollment	6.29%	6.25%		3.62%
N (individuals)	15,112	7,483		21,900
Panel E: Non-hispanic white				
Post enrollment	798.48***	636.94***	0.59***	405.78***
	(67.12)	(145.46)	(0.02)	(157.11)
F-statistic			712.16	
% mean pre-enrollment	4.91%	3.92%		2.50%
N (individuals)	44,829	27,470		67,057

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings and the percentage earnings gains relative to the mean pre-enrollment earnings by sex, race, and ethnicity. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. Panel A reports estimates for males. Panel B reports estimates for females. Panel C reports estimates for Hispanics. Panel D reports estimates for non-Hispanic blacks. Panel E reports estimates for non-Hispanic whites. Estimates rely on administrative data from Tennessee.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: Returns to enrollment in CJT by new and incumbent workers

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Panel A: OJT				
Post enrollment	979.00***	643.69***	0.67***	475.53***
	(132.51)	(162.06)	(0.01)	(217.55)
F-statistic			3,435.66	
Mean pre-enrollment	7,137.09	7,137.09		7,137.09
% mean pre-enrollment	13.72%	9.02%		6.53%
N (individual-quarters)	246,114	129,575		307,199
N (individuals)	11,775	6,494		16,789
R-squared	0.59	0.62		0.04
Panel B: IWT				
Post enrollment	1,000.04***	723.49***	0.56***	699.45***
	(65.80)	(124.00)	(0.02)	(253.12)
F-statistic			621.28	
Mean pre-enrollment	15,808.95	15,808.95		15,808.95
% mean pre-enrollment	6.33%	4.58%		4.42%
N (individual-quarters)	855,208	494,511		1,210,193
N (individuals)	40,126	26,193		59,710
R-squared	0.71	0.72		0.02
Individual FE	Yes	Yes		Yes
Year-by-quarter FE	Yes	Yes		Yes
Industry-by-year FE	Yes	Yes		Yes
Clustering	Individual	Firm		Firm

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings, pre-enrollment mean earnings, and the percentage earnings gains relative to the mean pre-enrollment earnings. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. Panel A reports estimates for OJT enrollees. Panel B reports estimates for IWT enrollees. Estimates rely on administrative data from Tennessee.
* $p < 0.10$;   ** $p < 0.05$;   *** $p < 0.01$.

Table 9: Return to CJT enrollment by industry

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Panel A: Administration				
Post enrollment	520.87	726.23**	0.66***	597.95
	(402.28)	(339.25)	(0.05)	(497.84)
F-statistic			209.07	
% mean pre-enrollment	4.14%	5.77%		4.75%
N (individuals)	3,690	1,731		5,228
Panel B: Construction				
Post enrollment	376.20*	697.85**	0.75***	634.09
	(218.32)	(321.55)	(0.03)	(439.46)
F-statistic			663.43	
% mean pre-enrollment	2.73%	5.07%		4.61%
N (individuals)	1,874	1,711		3,216
Panel C: Health Care and Social Assistance				
Post enrollment	365.74*	222.19	0.69***	-305.05
	(191.36)	(232.28)	(0.06)	(419.13)
F-statistic			143.27	
% mean pre-enrollment	2.75%	1.67%		-2.30%
N (individuals)	13,607	9,225		22,395
Panel D: Manufacturing				
Post enrollment	828.37***	618.48***	0.54***	355.09*
	(72.78)	(191.56)	(0.02)	(215.76)
F-statistic			462.17	
% mean pre-enrollment	4.97%	3.71%		2.13%
N (individuals)	34,564	15,518		45,861
Panel E: Wholesale				
Post enrollment	1,569.59***	1,563.77***	0.61***	1,503.29**
	(276.36)	(570.11)	(0.07)	(666.60)
F-statistic			72.35	
% mean pre-enrollment	8.87%	8.84%		8.49%
N (individuals)	3,770	2,113		5,507

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings and the percentage earnings gains relative to the mean pre-enrollment earnings by firm industry. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. Panel A reports estimates for administration firms. Panel B reports estimates for construction firms. Panel C reports estimates for health care and social assistance firms. Panel D reports estimates for manufacturing firms. The industry is determined by the 2-digit NAICS code. Estimates rely on administrative data from Tennessee.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 10: Return to CJT enrollment by firm size

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Panel A: Small firms (1-49 employees)				
Post enrollment	926.43***	856.09***	0.70***	824.86***
	(111.39)	(263.63)	(0.01)	(287.48)
F-statistic			2,656.29	
% mean pre-enrollment	6.19%	5.72%		5.51%
N (individuals)	11,407	9,683		18,370
Panel B: Medium firms (50 - 249 employees)				
Post enrollment	752.51***	345.42*	0.54***	165.28
	(99.57)	(214.38)	(0.03)	(300.70)
F-statistic			369.69	
% mean pre-enrollment	4.65%	2.13%		1.02%
N (individuals)	18,978	12,125		28,394
Panel C: Large firms (≥250 employees)				
Post enrollment	689.96***	595.69***	0.44***	-321.04
	(140.73)	(187.33)	(0.04)	(559.51)
F-statistic			112.50	
% mean pre-enrollment	4.00%	3.43%		1.85%
N (individuals)	35,753	17,011		51,540

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings and the percentage earnings gains relative to the mean pre-enrollment earnings by firm size. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. Panel A reports estimates for firms with less than 50 employees. Panel B reports estimates for firms with 50 to 249 employees. Panel C reports estimates for firms with 250 or more employees. Estimates rely on administrative data from Tennessee.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 11: Return to CJT enrollment by skill type

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Panel A: General skills				
Post enrollment	842.49***	772.42*	0.50***	-605.34
	(321.89)	(469.23)	(0.04)	(321.32)
F-statistic			149.94	
% mean pre-enrollment	5.01%	4.59%		3.59%
N (individuals)	59,652	32,417		91,791
Panel B: Industry-specific skills				
Post enrollment	1,384.14***	1,330.25***	0.48***	1,366.56***
	(243.86)	(248.14)	(0.06)	(405.07)
F-statistic			57.75	
% mean pre-enrollment	9.48%	9.11%		9.31%
N (individuals)	59,698	32,458		91,837
Panel C: Occupation-specfic skills				
Post enrollment	755.33***	744.51**	0.57***	407.58
	(276.08)	(322.54)	(0.60)	(293.65)
F-statistic			89.21	
% mean pre-enrollment	4.45%	4.38%		2.40%
N (individuals)	59,695	32,460		91,834
Panel D: Firm-specific skills				
Post enrollment	854.57***	781.09**	0.52***	345.62
	(280.16)	(366.84)	(0.04)	(292.58)
F-statistic			142.08	
% mean pre-enrollment	5.21%	4.73%		2.09%
N (individuals)	59,707	32,472		91,846

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings and the percentage earnings gains relative to the mean pre-enrollment earnings by training program skill composition. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. Panel A reports estimates for the top quartile of general skill programs. Panel B reports estimates for industry-specific programs. Panel C reports estimates for occupation-specific programs. Panel D reports estimates for firm-specific programs. The specificity of each training program is determined by natural language processing. Estimates rely on hand-collected and digitized firm application records linked to administrative data from Tennessee.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 12: Effect of CJT enrollment on worker mobility

	Stay at firm 4 years post enroll	Stay in industry 4 years post enroll
Panel A: CJT within and across		
CJT enrollment	0.104***	0.113***
	(0.005)	(0.006)
N (individuals)	100,012	100,012
Panel B: CJT within		
CJT enrollment	0.083***	0.086***
	(0.006)	(0.006)
N (individuals)	67,326	67,326
Panel C: CJT across		
CJT enrollment	0.142***	0.163***
	(0.006)	(0.006)
N (individuals)	39,347	39,347

Notes: This table reports the effect of CJT participation on the probability that workers remain with their firm or within their industry four years after enrollment. Treatment is defined as enrollment in a CJT program. Panel A pools the within- and across-firm control groups. Panel B presents within-firm estimates, where treated workers are compared to untreated workers in the same firm, with standard errors clustered at the individual level. Panel C presents across-firm estimates, where treated workers are compared to untreated workers at firms that applied but were not treated, with standard errors clustered at the firm level. Standard errors are in parentheses.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 13: Return to CJT enrollment by worker mobility

	Difference-in-Differences		Difference-in-IV	
	Within	Across Applying	Within and Across	
Quarterly earnings	ATT	ATT	First stage	ATT
Panel A: Stay at firm				
Post enrollment	827.73***	562.29***	0.52***	266.50
	(85.92)	(154.88)	(0.03)	(256.09)
F-statistic			282.56	
% mean pre-enrollment	4.89%	3.32%		1.58%
N (individuals)	18,286	9,010		24,978
Panel B: Leave firm				
Post enrollment	674.31***	509.57***	0.61***	323.07**
	(78.64)	(148.05)	(0.02)	(146.19)
F-statistic			1,278.38	
% mean pre-enrollment	4.39%	3.32%		2.10%
N (individuals)	47,932	29,846		73,443
Panel C: Stay in industry				
Post enrollment	815.38***	636.49***	0.55***	274.70
	(88.12)	(144.83)	(0.03)	(236.83)
F-statistic			329.84	
% mean pre-enrollment	4.81%	3.76%		1.62%
N (individuals)	24,951	12,360		34,300
Panel D: Leave industry				
Post enrollment	700.07***	589.93***	0.61***	474.86***
	(85.19)	(164.38)	(0.02)	(156.59)
F-statistic			1,516.82	
% mean pre-enrollment	4.66%	3.92%		3.16%
N (individuals)	41,250	26,481		64,089

Notes: This table presents regression estimates of the impact of CJT enrollment on quarterly earnings and the percentage earnings gains relative to the mean pre-enrollment earnings by worker mobility. Specifications include individual worker, year-by-quarter, and industry-by-year fixed effects, with ATT-inverse probability weights applied. Panel A reports estimates for individuals who remain at their firm 4 years after training. Panel B reports estimates for individuals who leave their firm within 4 years after training. Panel C reports estimates for individuals who remain in their industry 4 years after training. Panel D reports estimates for individuals who leave their industry within 4 years after training. Estimates rely on administrative data from Tennessee.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

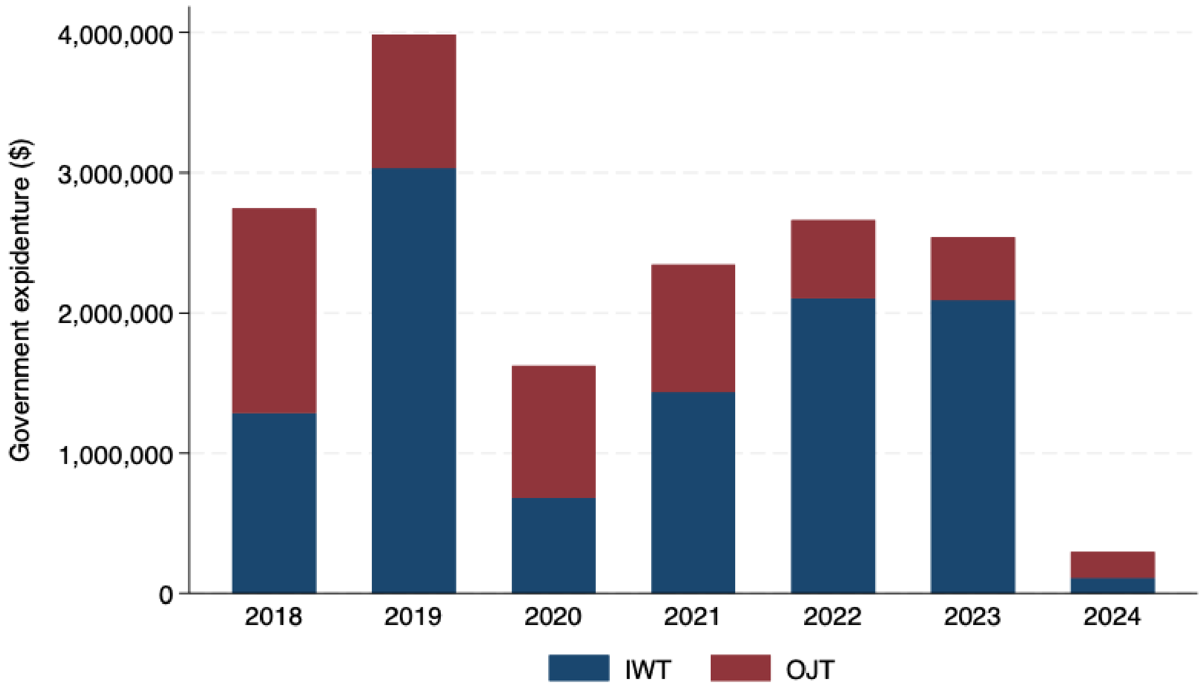
Table 14: MVPF and cost-benefit estimates

	Difference-in-Differences		Difference-in-IV
	Within ATT	Across Applying ATT	Within and Across ATT
Panel A: MVPF estimates			
Enrollment Effect	$809.41	$636.62	$405.53
	(58.69)	(142.06)	(137.88)
Willingness to pay	$14,827.44	$11,662.13	$7,428.83
	[$13,752.31, $15,902.58]	[$9,059.76, $14,264.51]	[$4,903.03, $9,954.63]
Public cost per trainee	$1,102.25	$1,102.25	$1,102.25
Public cost - federal income tax revenue	-$2,159.79	-$1,463.42	-$532.09
per trainee	[-$2,396.32, -$1,923.26]	[-$2,035.94, -$890.90]	[-$1,087.77, $23.58]
MVPF without income tax revenue	13.45	10.59	6.74
	[12.48, 14.43]	[8.22, 12.94]	[4.45, 9.03]
MVPF with income tax revenue	inf	inf	inf
	[inf, inf]	[inf, inf]	[207.93, inf]
Panel B: Back-of-the-envelope cost-benefit estimates			
Enrollment Effect	$809.41	$636.62	$405.53
	(58.69)	(142.06)	(137.88)
Public cost per trainee	$1,102.25	$1,102.25	$1,102.25
Estimated private cost per trainee	$551.13	$551.13	$551.13
Back-of-the-envelope quarters to payoff	1.36	1.73	2.72
based on public cost	[1.27, 1.47]	[1.42, 2.23]	[2.03, 4.12]
Back-of-the-envelope quarters to payoff	2.04	2.96	4.08
based on public and private cost	[1.91, 2.21]	[2.13, 3.35]	[3.05, 6.18]

Notes: This table reports estimates of the marginal value of public funds (MVPF) of CJT enrollment in the framework of Hendren and Sprung-Keyser (2020) as well as back-of-the-envelope cost-benefit calculations. The effect of enrolling in CJT is presented in the first row of Panels A and B for the three research designs. Willingness to pay is the net present value of 5 years of earnings gains discounted at 3% per year. The public cost of training is calculated empirically using annual public cost records provided by the Tennessee Department of Labor and Workforce Development. Federal income tax revenue is calculated as the marginal tax revenue from the willingness to pay. The marginal tax bracket was calculated using 2024 federal income tax brackets applied to annual mean pre-enrollment earnings for CJT trainees, less the 2024 individual federal standard deduction. The MVPF is the ratio of willingness to pay to cost per trainee. This MVPF does not account for indirect cost reductions resulting from decreased public assistance or employment service participation. When the cost is negligible (zero or negative), the MVPF becomes infinite. In Panel B, the estimated private cost per trainee is calculated using the 50% match requirement for firms with more than 100 employees (see Table 1 to see the average size of a treated firm). Back-of-the-envelope cost-benefit calculations are calculated as the ratio of the cost of training per individual to the earnings effect. Standard errors are in parentheses, and 95% confidence intervals are in brackets.

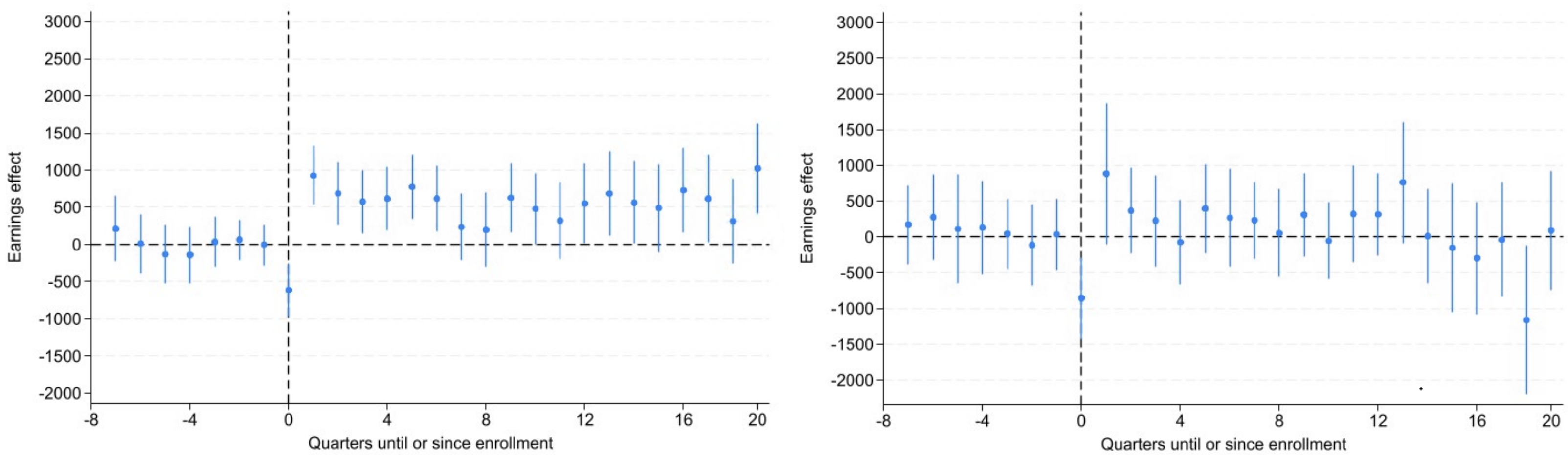
A Additional figures and tables

Figure A.1: Government expenditure by year



Notes: This figure plots expenditure for annual state outlays on CJT programs by type (on-the-job training and incumbent worker training) for 2018 Q1 to 2024 Q2. Data is provided by the Tennessee Department of Labor and Workforce Development. Values are expressed in 2024 nominal dollars. Records prior to 2018 are unavailable due to a system transition.

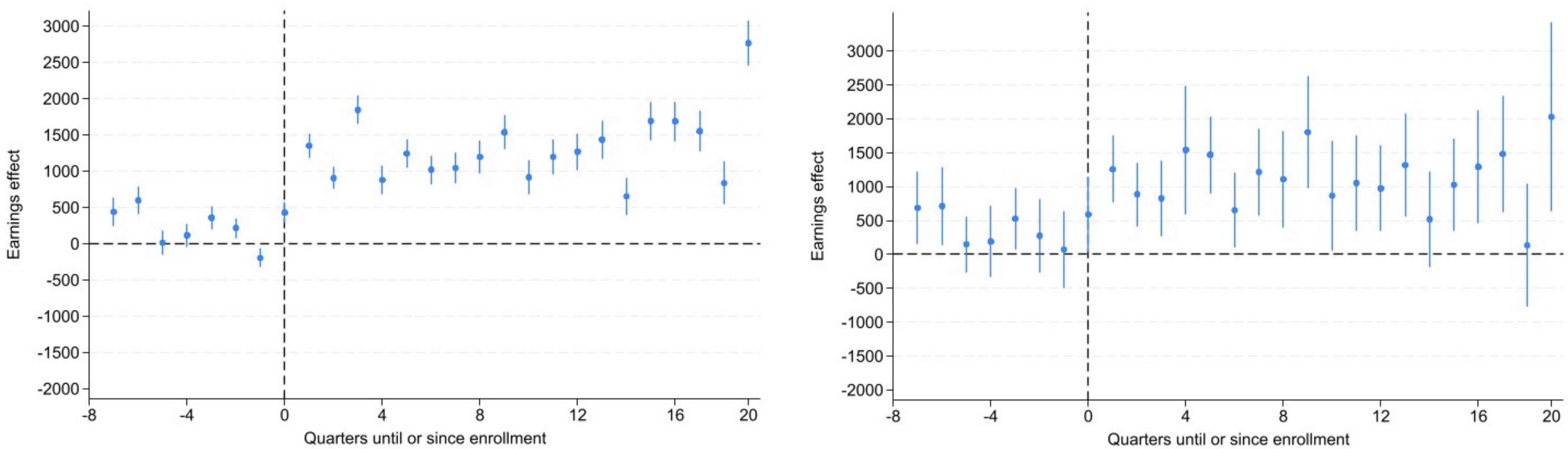
Figure A.2: Event studies for OJT



(a) Within **(b) Across Applying**

Notes: These figures present the coefficients from event study regressions estimating the earnings relative to OJT training enrollment, with leads and lags in event time. Specifications include individual, time, and industry-by-year fixed effects. Subfigure (a) compares training enrollees to their untreated coworkers. Subfigure (b) compares training enrollees to workers at firms who applied for training grants but did not receive them. All event study regressions are propensity weighted using ATT-IPW weights. 95% confidence intervals are shown. All figures were created by the author using administrative data from the state of Tennessee.

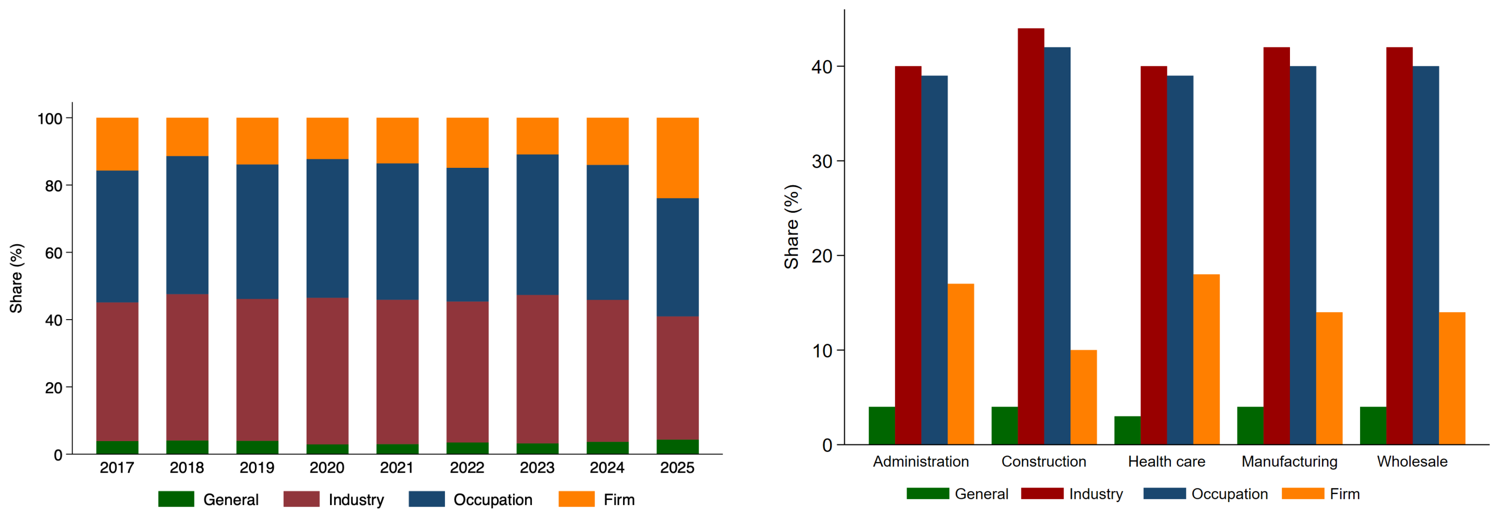
Figure A.3: Event studies for IWT



(a) Within **(b) Across Applying**

Notes: These figures present the coefficients from event study regressions estimating the earnings relative to IWT training enrollment, with leads and lags in event time. Specifications include individual, time, and industry-by-year fixed effects. Subfigure (a) compares training enrollees to their untreated coworkers. Subfigure (b) compares training enrollees to workers at firms who applied for training grants but did not receive them. All event study regressions are propensity weighted using ATT-IPW weights. 95% confidence intervals are shown. All figures were created by the author using administrative data from the state of Tennessee.

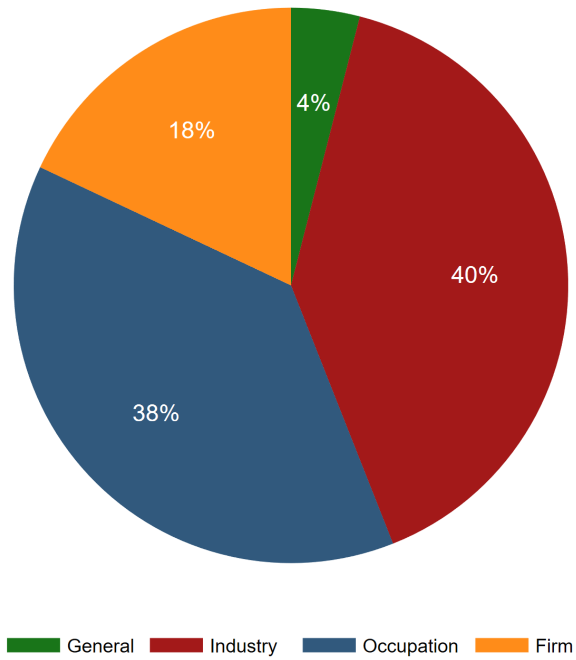
Figure A.4: NLP additional figures



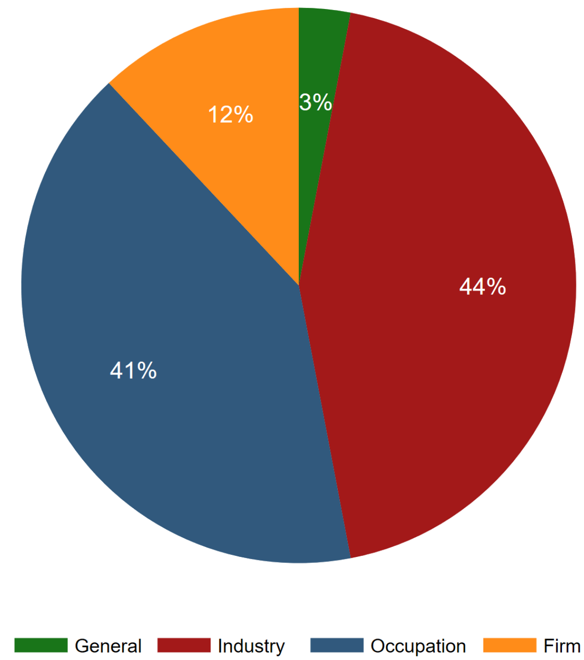
(a) Share of skills across years **(b) Share of skills across industries**

Notes: This figure plots the composition of skills over the years and by industries as classified by the natural language processing. Subfigure (a) plots the share of general, industry-specific, occupation-specific, and firm-specific scores for CJT training program descriptions each year. Subfigure (b) plots the share of general, industry-specific, occupation-specific, and firm-specific scores for CJT training program descriptions that take place in the industries where CJT training mostly takes place.

Figure A.5: NLP additional pie charts



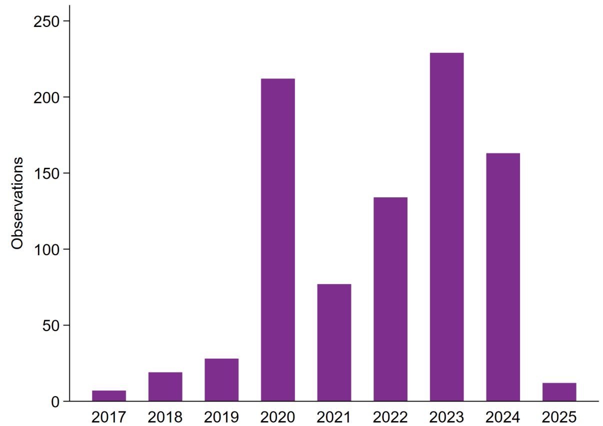
(a) OJT pie chart share of skills



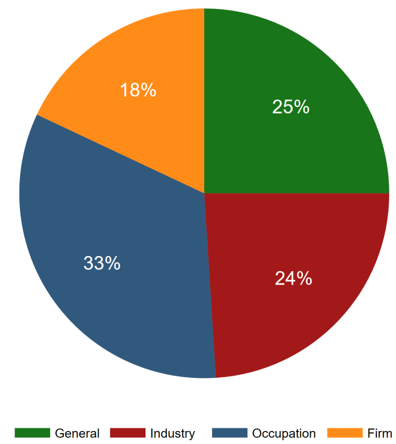
(b) IWT pie chart share of skills

Notes: This figure shows the distribution of training program content across four skill categories (general, occupation-specific, industry-specific, and firm-specific) for CJT training programs. Subfigure (a) gives the distribution for OJT training programs, and subfigure (b) gives the distribution for IWT training programs. Percentages are generated from natural language processing of training program descriptions. Data were pulled from hand-collected and digitized firm grant applications in Tennessee.

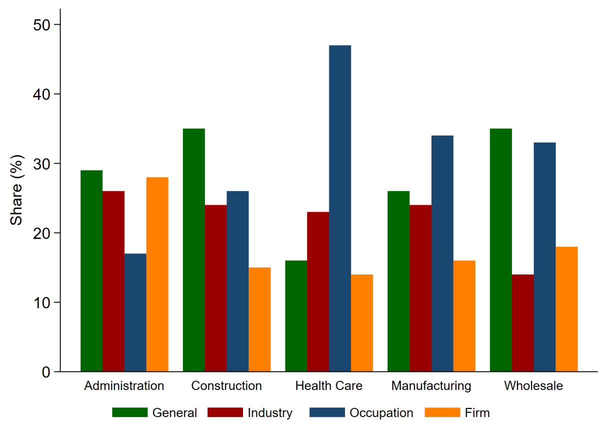
Figure A.6: Survey skill content



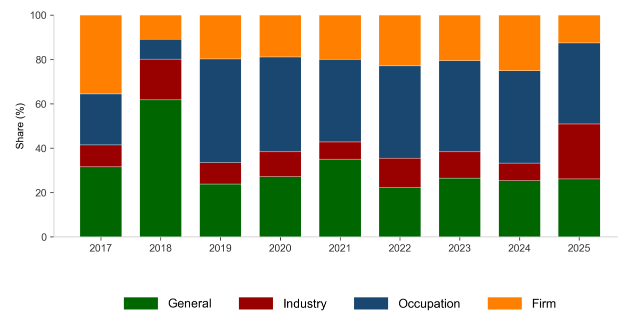
(a) Training excerpts categorized



(b) CJT pie chart share of skills



(c) Share of skills across industries



(d) Share of skills across years

Notes: Subfigure (a) plots the number of training excerpts that were highlighted by at least one survey respondent over the year when the training took place. The majority of the training excerpts that were highlighted by employers were for training that took place between 2020 and 2024. Subfigure (b) shows the distribution of training program content across four skill categories (general, occupation-specific, industry-specific, and firm-specific) for CJT training programs. Percentages were generated using survey highlighting word counts. Subfigure (c) plots the share of general, industry-specific, occupation-specific, and firm-specific scores for CJT training program descriptions that take place in the industries where CJT training mostly takes place. Subfigure (d) This figure shows the share of skills in training plans over time as categorized by employer survey respondents. The 2017 grouping contained very few training excerpts, as can be seen in subfigure (a).

Figure A.7: Survey word clouds by skill type

(a) General

(b) Industry-specific

(c) Occupation-specific

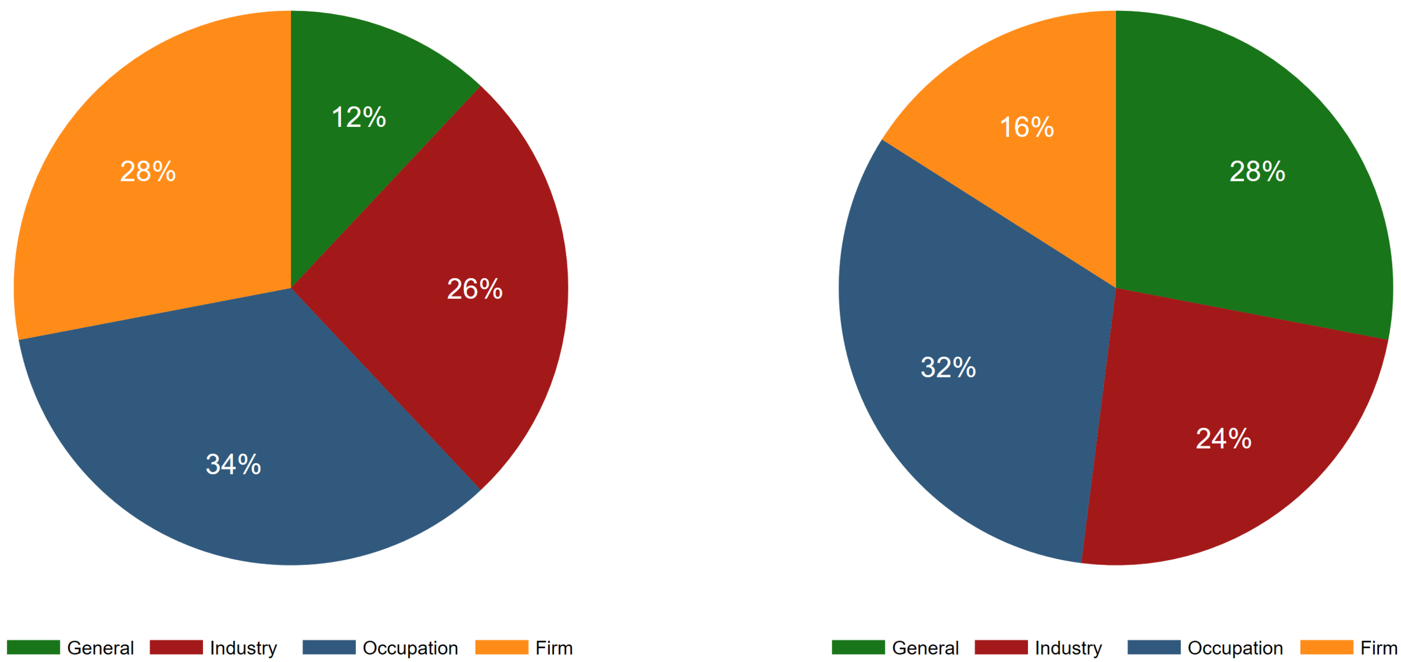
(d) Firm-specific

Notes: This figure presents word clouds generated from survey classification of training program descriptions. Data were pulled from hand-collected and digitized firm grant applications in Tennessee and classified by firms that participated in CJT between 2016 and 2024. Subfigures (a)–(d) plot the most common words, excluding the syncategorematic words (e.g., 'the,' 'a,' 'of,' 'some,' 'all'), in the training program descriptions that were highlighted as general, industry-specific, occupation-specific, and firm-specific (respectively) by the employer survey respondents.

Figure A.8: Survey respondent job titles

Notes: This figure presents a word cloud of the self-reported job titles of the employer survey respondents.

Figure A.9: Survey additional pie charts



(a) OJT pie chart share of skills **(b) IWT pie chart share of skills**

Notes: This figure shows the distribution of training program content across four skill categories (general, occupation-specific, industry-specific, and firm-specific) for CJT training programs. Subfigure (a) gives the distribution for OJT training programs, and subfigure (b) gives the distribution for IWT training programs. Percentages are generated from the employer survey of training program descriptions. Data were pulled from hand-collected and digitized firm grant applications in Tennessee.

Table A.1: OJT covariate balance before propensity weighting

	Within Firm				Across Applying Firms			
	Control	Treat	Difference	p-value	Control	Treat	Difference	p-value
Panel A: Demographics								
Age at training start	29.137	29.710	0.573	0.016	32.458	29.710	-2.748	0.000
	(8.505)	(8.799)	(0.237)		(10.516)	(8.799)	(0.300)	
Female	0.441	0.408	-0.033	0.018	0.492	0.408	-0.084	0.000
	(0.497)	(0.492)	(0.014)		(0.500)	(0.492)	(0.015)	
Hispanic	0.030	0.020	-0.010	0.030	0.034	0.020	-0.014	0.005
	(0.169)	(0.138)	(0.005)		(0.181)	(0.138)	(0.005)	
Non-Hispanic Black	0.403	0.344	-0.059	0.000	0.236	0.344	0.108	0.000
	(0.491)	(0.475)	(0.014)		(0.425)	(0.475)	(0.013)	
Non-Hispanic White	0.503	0.589	0.086	0.000	0.660	0.589	-0.071	0.000
	(0.500)	(0.492)	(0.014)		(0.474)	(0.492)	(0.014)	
Panel B: Education								
Postsecondary enroll	0.169	0.209	0.040	0.000	0.214	0.209	-0.005	0.697
	(0.375)	(0.407)	(0.011)		(0.410)	(0.407)	(0.012)	
Postsecondary credits earned	13.480	17.695	4.215	0.000	24.166	17.695	-6.471	0.000
	(36.312)	(46.094)	(1.046)		(52.147)	(46.094)	(1.502)	
Obtained 2-year degree	0.007	0.010	0.003	0.175	0.014	0.010	-0.004	0.211
	(0.083)	(0.100)	(0.002)		(0.119)	(0.100)	(0.003)	
Obtained 4-year degree	0.010	0.013	0.003	0.300	0.032	0.013	-0.019	0.000
	(0.102)	(0.115)	(0.003)		(0.177)	(0.115)	(0.005)	
Obtained certificate	0.031	0.046	0.015	0.002	0.036	0.046	0.010	0.073
	(0.174)	(0.211)	(0.005)		(0.187)	(0.211)	(0.006)	
Missing postsecondary data	0.430	0.352	-0.078	0.000	0.345	0.352	0.007	0.633
	(0.495)	(0.478)	(0.014)		(0.475)	(0.478)	(0.014)	
Panel C: Priority								
AED enrollment	0.002	0.003	0.001	0.669	0.000	0.003	0.002	0.011
	(0.046)	(0.052)	(0.001)		(0.020)	(0.052)	(0.001)	
SNAP enrollment	0.002	0.001	-0.000	0.670	0.001	0.001	0.000	0.884
	(0.043)	(0.037)	(0.001)		(0.035)	(0.037)	(0.001)	
TAA enrollment	-	-	-	-	-	-	-	-
	-	-	-		-	-	-	
WIOA enrollment	0.005	0.055	0.051	0.000	0.002	0.055	0.053	0.000
	(0.067)	(0.229)	(0.003)		(0.049)	(0.229)	(0.003)	
WP enrollment	0.059	0.164	0.106	0.000	0.039	0.164	0.125	0.000
	(0.235)	(0.371)	(0.007)		(0.193)	(0.371)	(0.007)	
UI eligible	0.061	0.081	0.020	0.003	0.039	0.081	0.042	0.000
	(0.239)	(0.273)	(0.007)		(0.193)	(0.273)	(0.006)	
Panel D: Earnings								
Earnings t = -1	$7,325.615	$5,674.770	-$1,650.846	0.000	$10,175.840	$5,674.770	-$4,501.068	0.000
	(5,923.677)	(5,145.969)	(161.923)		(8,254.845)	(5,145.969)	(226.280)	

Notes: This table reports covariate balance between OJT participants and control groups prior to inverse probability weighting. Columns compare treated individuals to controls within the same firm and across applying firms. Reported variables include demographics, education, and pre-enrollment characteristics. Differences and associated p-values are shown for each covariate. Standard errors are reported in parentheses. "—" indicates that one of the two groups had strictly zero values in that variable.

Table A.2: OJT covariate balance after propensity weighting

	Within Firm				Across Applying Firms			
	Control	Treat	Difference	p-value	Control	Treat	Difference	p-value
Panel A: Demographics								
Age at training start	29.470	29.710	0.240	0.396	31.433	29.710	-1.724	0.063
	(8.601)	(8.799)	(0.283)		(10.885)	(8.799)	(0.926)	
Female	0.417	0.408	-0.008	0.591	0.462	0.408	-0.054	0.134
	(0.493)	(0.492)	(0.016)		(0.499)	(0.492)	(0.036)	
Hispanic	0.020	0.020	-0.000	0.971	0.028	0.020	-0.008	0.442
	(0.139)	(0.138)	(0.004)		(0.164)	(0.138)	(0.010)	
Non-Hispanic Black	0.355	0.344	-0.011	0.467	0.437	0.344	-0.093	0.015
	(0.479)	(0.475)	(0.015)		(0.496)	(0.475)	(0.038)	
Non-Hispanic White	0.578	0.589	0.011	0.498	0.495	0.589	0.094	0.007
	(0.493)	(0.492)	(0.016)		(0.500)	(0.492)	(0.035)	
Panel B: Education								
Postsecondary enroll	0.202	0.209	0.007	0.624	0.175	0.209	0.034	0.061
	(0.402)	(0.407)	(0.014)		(0.380)	(0.407)	(0.018)	
Postsecondary credits earned	17.529	17.695	0.165	0.910	15.770	17.695	1.925	0.726
	(45.422)	(46.094)	(1.465)		(42.951)	(46.094)	(1.767)	
Obtained 2-year degree	0.011	0.010	-0.001	0.845	0.011	0.010	-0.001	0.810
	(0.103)	(0.100)	(0.003)		(0.104)	(0.100)	(0.004)	
Obtained 4-year degree	0.011	0.013	0.002	0.514	0.010	0.013	0.003	0.290
	(0.105)	(0.115)	(0.003)		(0.099)	(0.115)	(0.003)	
Obtained certificate	0.043	0.046	0.003	0.616	0.048	0.046	-0.001	0.913
	(0.204)	(0.211)	(0.006)		(0.213)	(0.211)	(0.012)	
Missing postsecondary data	0.341	0.352	0.010	0.467	0.390	0.352	-0.038	0.306
	(0.474)	(0.478)	(0.014)		(0.488)	(0.478)	(0.037)	
Panel C: Priority								
AED enrollment	0.003	0.003	0.000	0.985	0.003	0.003	-0.000	0.896
	(0.052)	(0.052)	(0.001)		(0.055)	(0.052)	(0.003)	
SNAP enrollment	0.002	0.001	-0.000	0.791	0.002	0.001	-0.000	0.742
	(0.041)	(0.037)	(0.001)		(0.043)	(0.037)	(0.001)	
TAA enrollment	-	-	-	-	-	-	-	-
	-	-	-		-	-	-	
WIOA enrollment	0.073	0.055	-0.018	0.220	0.160	0.055	-0.105	0.049
	(0.260)	(0.229)	(0.014)		(0.367)	(0.229)	(0.054)	
WP enrollment	0.186	0.164	-0.022	0.167	0.271	0.164	-0.107	0.026
	(0.389)	(0.371)	(0.016)		(0.445)	(0.371)	(0.048)	
UI eligible	0.081	0.081	-0.000	0.968	0.117	0.081	-0.036	0.285
	(0.273)	(0.273)	(0.009)		(0.321)	(0.273)	(0.034)	
Panel D: Earnings								
Earnings t = -1	$5,707.761	$5,674.770	-$32.992	0.832	$5,091.482	$5,674.770	$583.288	0.061
	(5,121.443)	(5,145.969)	(115.938)		(5,160.658)	(5,145.969)	(311.551)	

Notes: This table reports covariate balance between OJT participants and control groups after inverse probability weighting. Columns compare treated individuals to controls within the same firm and across applying firms. Reported variables include demographics, education, and pre-enrollment characteristics. Differences and associated p-values are shown for each covariate. Standard errors are reported in parentheses. "—" indicates that one of the two groups had strictly zero values in that variable.

Table A.3: IWT covariate balance before propensity weighting

	Within Firm				Across Applying Firms			
	Control	Treat	Difference	p-value	Control	Treat	Difference	p-value
Panel A: Demographics								
Age at training start	35.661	36.386	0.725	0.000	37.761	36.386	-1.375	0.000
	(10.973)	(10.802)	(0.147)		(11.302)	(10.802)	(0.159)	
Female	0.412	0.280	-0.133	0.000	0.476	0.280	-0.197	0.000
	(0.492)	(0.449)	(0.007)		(0.499)	(0.449)	(0.007)	
Hispanic	0.024	0.022	-0.002	0.351	0.026	0.022	-0.004	0.062
	(0.152)	(0.146)	(0.002)		(0.159)	(0.146)	(0.002)	
Non-Hispanic Black	0.199	0.105	-0.095	0.000	0.198	0.105	-0.093	0.000
	(0.400)	(0.306)	(0.005)		(0.398)	(0.306)	(0.005)	
Non-Hispanic White	0.712	0.787	0.075	0.000	0.705	0.787	0.082	0.000
	(0.453)	(0.409)	(0.006)		(0.456)	(0.409)	(0.006)	
Panel B: Education								
Postsecondary enroll	0.150	0.191	0.041	0.000	0.146	0.191	0.045	0.000
	(0.357)	(0.393)	(0.005)		(0.353)	(0.393)	(0.005)	
Postsecondary credits earned	27.724	29.132	1.408	0.078	33.680	29.132	-4.548	0.000
	(58.554)	(62.926)	(0.798)		(64.562)	(62.926)	(0.912)	
Obtained 2-year degree	0.015	0.016	0.001	0.618	0.014	0.016	0.002	0.249
	(0.122)	(0.125)	(0.002)		(0.117)	(0.125)	(0.002)	
Obtained 4-year degree	0.027	0.027	-0.000	0.933	0.029	0.027	-0.003	0.282
	(0.162)	(0.162)	(0.002)		(0.169)	(0.162)	(0.002)	
Obtained certificate	0.023	0.030	0.007	0.001	0.018	0.030	0.012	0.000
	(0.151)	(0.170)	(0.002)		(0.134)	(0.170)	(0.002)	
!Missing postsecondary data	0.347	0.283	-0.064	0.000	0.332	0.283	-0.049	0.000
	(0.476)	(0.451)	(0.006)		(0.471)	(0.451)	(0.007)	
Panel C: Priority characteristics								
AED enrollment	0.000	0.001	0.000	0.164	0.000	0.001	0.001	0.019
	(0.016)	(0.025)	(0.000)		(0.010)	(0.025)	(0.000)	
SNAP enrollment	0.000	0.000	0.000	0.201	0.000	0.000	0.000	0.420
	(0.005)	(0.012)	(0.000)		(0.007)	(0.012)	(0.000)	
TAA enrollment	0.000	0.000	0.000	0.201	-	-	-	-
	(0.005)	(0.012)	(0.000)		-	-	-	
WIOA enrollment	0.000	0.001	0.001	0.000	0.000	0.001	0.001	0.000
	(0.013)	(0.033)	(0.000)		(0.010)	(0.033)	(0.000)	
WP enrollment	0.013	0.021	0.007	0.000	0.006	0.021	0.015	0.000
	(0.115)	(0.142)	(0.002)		(0.074)	(0.142)	(0.001)	
UI eligible	0.038	0.015	-0.023	0.000	0.022	0.015	-0.007	0.000
	(0.191)	(0.120)	(0.002)		(0.145)	(0.120)	(0.002)	
Panel D: Earnings								
Earnings t = -1	\$15,618.490	\$16,731.620	\$1,113.130	0.000	\$16,723.930	\$16,731.620	\$7.686	0.951
	(8,148.406)	(8,195.410)	(109.764)		(9,078.660)	(8,195.410)	(126.080)	

Notes: This table reports covariate balance between IWT participants and control groups prior to inverse probability weighting. Columns compare treated individuals to controls within the same firm and across applying firms. Reported variables include demographics, education, and pre-enrollment characteristics. Differences and associated p-values are shown for each covariate. Standard errors are reported in parentheses. "—" indicates that one of the two groups had strictly zero values in that variable.

Table A.4: IWT covariate balance after propensity weighting

	Within Firm				Across Applying Firms			
	Control	Treat	Difference	p-value	Control	Treat	Difference	p-value
Panel A: Demographics								
Age at training start	35.528	36.386	0.859	0.000	36.757	36.386	-0.371	0.025
	(10.979)	(10.802)	(0.150)		(11.213)	(10.802)	(0.166)	
Female	0.284	0.280	-0.004	0.460	0.282	0.280	-0.002	0.711
	(0.451)	(0.449)	(0.006)		(0.450)	(0.449)	(0.007)	
Hispanic	0.022	0.022	-0.000	0.889	0.022	0.022	-0.000	0.983
	(0.147)	(0.146)	(0.002)		(0.146)	(0.146)	(0.002)	
Non-Hispanic Black	0.106	0.105	-0.002	0.680	0.107	0.105	-0.002	0.631
	(0.308)	(0.306)	(0.004)		(0.309)	(0.306)	(0.004)	
Non-Hispanic White	0.787	0.787	0.000	0.997	0.787	0.787	0.000	0.971
	(0.409)	(0.409)	(0.006)		(0.410)	(0.409)	(0.06)	
Panel B: Education								
Postsecondary enroll	0.182	0.191	0.008	0.130	0.181	0.191	0.010	0.116
	(0.387)	(0.393)	(0.006)		(0.385)	(0.393)	(0.006)	
Postsecondary credits earned	28.371	29.132	0.761	0.390	28.081	29.132	1.051	0.273
	(60.534)	(62.926)	(0.885)		(60.464)	(62.926)	(0.959)	
Obtained 2-year degree	0.015	0.016	0.001	0.618	0.149	0.159	0.001	0.603
	(0.122)	(0.125)	(0.002)		(0.121)	(0.125)	(0.002)	
Obtained 4-year degree	0.026	0.027	0.001	0.753	0.025	0.027	0.001	0.585
	(0.160)	(0.162)	(0.002)		(0.158)	(0.161)	(0.002)	
Obtained certificate	0.030	0.030	0.000	0.946	0.031	0.030	-0.001	0.838
	(0.170)	(0.170)	(0.002)		(0.172)	(0.170)	(0.003)	
!Missing postsecondary data	0.287	0.283	-0.004	0.552	0.287	0.283	-0.004	0.578
	(0.452)	(0.451)	(0.006)		(0.452)	(0.451)	(0.007)	
Panel C: Priority characteristics								
AED enrollment	0.001	0.001	-0.000	0.869	0.004	0.001	0.000	0.717
	(0.026)	(0.025)	(0.000)		(0.021)	(0.025)	(0.000)	
SNAP enrollment	0.000	0.000	0.000	0.799	0.000	0.000	0.000	0.811
	(0.010)	(0.012)	(0.000)		(0.010)	(0.012)	(0.000)	
TAA enrollment	0.000	0.000	-0.000	0.927	-	-	-	-
	(0.013)	(0.012)	(0.000)		-	-	-	
WIOA enrollment	0.001	0.001	0.000	0.891	0.002	0.001	-0.001	0.629
	(0.031)	(0.033)	(0.001)		(0.042)	(0.033)	(0.001)	
WP enrollment	0.021	0.021	-0.000	0.930	0.024	0.021	-0.002	0.421
	(0.143)	(0.142)	(0.002)		(0.152)	(0.142)	(0.004)	
UI eligible	0.015	0.015	0.000	0.963	0.015	0.015	0.000	0.956
	(0.120)	(0.120)	(0.002)		(0.119)	(0.120)	(0.002)	
Panel D: Earnings								
Earnings t = -1	$16,756.040	$16,731.620	-$24.422	0.835	$16,699.010	$16,731.620	$32.610	0.802
	(8,731.406)	(8,195.410)	(117.470)		(8,889.589)	(8,195.410)	(129.789)	

Notes: This table reports covariate balance between IWT participants and control groups after inverse probability weighting. Columns compare treated individuals to controls within the same firm and across applying firms. Reported variables include demographics, education, and pre-enrollment characteristics. Differences and associated p-values are shown for each covariate. Standard errors are reported in parentheses. "—" indicates that one of the two groups had strictly zero values in that variable.

Table A.5: MVPF and cost-benefit estimates

	Difference-in-Differences		Difference-in-IV
	Within ATT	Across Applying ATT	Within and Across ATT
Panel A: OJT			
Enrollment Effect	$979.00	$643.69	$475.53
	(132.51)	(162.06)	(217.55)
Willingness to pay	$17,934.13	$11,791.65	$8,711.15
	[$15,506.71, $20,361.56]	[$8,822.90, $14,760.40]	[$4,725.89, $12,696.41]
Public cost per trainee	$1,989.34	$1,989.34	$1,989.34
Public cost - federal income tax revenue	-$162.76	$574.34	$944.00
per trainee	[-$454.05, $128.53]	[$218.09, $930.59]	[$465.77, $1,422.23]
MVPF without income tax revenue	9.02	5.93	4.38
	[7.79, 10.24]	[4.44, 7.42]	[2.36, 6.38]
MVPF with income tax revenue	inf	20.53	9.23
	[120.65, inf]	[9.48, 67.68]	[3.32, 27.26]
Back-of-the-envelope quarters to payoff	2.03	3.09	4.18
based on public cost	[1.79, 2.35]	[2.47, 4.13]	[2.87, 7.71]
Back-of-the-envelope quarters to payoff	3.05	4.64	6.27
based on public and private cost	[2.69, 3.53]	[3.71, 6.20]	[4.31, 11.57]
Panel B: IWT			
Enrollment Effect	$1,000.04	$723.49	$699.45
	(65.80)	(124.00)	(253.12)
Willingness to pay	$18,319.56	$13,253.49	$12,813.10
	[$17,114.18, $19,524.94]	[$10,981.95, $15,525.02]	[$8,176.24, $17,449.97]
Public cost per trainee	$898.63	$898.63	$898.63
Public cost - federal income tax revenue	-$3,131.67	-$2,017.14	-$1,920.25
per trainee	[-$3,396.86, -$2,866.49]	[-$2,516.87, -$1,517.40]	[-$2,940.36, -$900.14]
MVPF without income tax revenue	20.39	14.75	14.26
	[19.04, 21.73]	[12.22, 17.28]	[9.10, 19.42]
MVPF with income tax revenue	inf	inf	inf
	[inf, inf]	[inf, inf]	[inf, inf]
Back-of-the-envelope quarters to payoff	0.90	1.24	1.28
based on public cost	[0.84, 0.96]	[1.06, 1.50]	[0.94, 2.01]
Back-of-the-envelope quarters to payoff	1.35	1.86	1.92
based on public and private cost	[1.26, 1.44]	[1.59, 2.25]	[1.41, 3.02]

Notes: This table reports estimates of the marginal value of public funds (MVPF) of CJT enrollment in the framework of Hendren and Sprung-Keyser (2020) as well as back-of-the-envelope cost-benefit calculations. The effect of enrolling in training is presented in the first row of Panels A and B for the three research designs. Willingness to pay is the net present value of 5 years of earnings gains discounted at 3% per year. The public cost of training is calculated empirically using annual public cost records provided by the Tennessee Department of Labor and Workforce Development. Federal income tax revenue is calculated as the marginal tax revenue from the willingness to pay. The marginal tax bracket was calculated using 2024 federal income tax brackets applied to annual mean pre-enrollment earnings for trainees, less the 2024 individual federal standard deduction. The MVPF is the ratio of willingness to pay to cost per trainee. The estimated private cost per trainee is calculated using the 50% match requirement for firms with more than 100 employees. Back-of-the-envelope cost-benefit calculations are calculated as the ratio of the cost of training per individual to the earnings effect. Standard errors are in parentheses, and 95% confidence intervals are in brackets.

B Natural language processing

B.1 Skill specificity score methodology

Let T_i denote the ith training description. For each DWA D_j, the pairing of a firm's training description with DWAs generated by the LLM defines a binary indicator

$$f(T_i, D_j) = \begin{cases} 1 & \text{if } D_j \text{ is related to } T_i \text{ according to the LLM} \\ 0 & \text{otherwise.} \end{cases}$$

For each O*NET major occupation group O_g, the following binary function can be defined from the O*NET DWA-to-major occupation group crosswalk:

$$h(D_j, O_g) = \begin{cases} 1 & \text{if the O*NET data shows } D_j \text{ is relevant to } O_g \\ 0 & \text{otherwise.} \end{cases}$$

Using these relations, I calculate C_{ig}, the number of connections between training description T_i and occupation group O_g, as

$$C_{ig} = \sum_j \sum_g f(T_i, D_j) h(D_j, O_g).$$

By normalizing the connection count, I obtain a probability $p_{ig} = C_{ig}(\sum_g C_{ig})^{-1}$ that training description T_i will be applicable to major occupation group O_g. Let the occupation-specificity of training description T_i be given by

$$\text{OccSpec}_i = 1 - \frac{-\sum_g p_{ig} \ln(p_{ig})}{\text{Maximum occupation entropy}}.$$

The numerator is commonly referred to as the Shannon entropy of a variable. Higher values of OccSpec_i indicate high occupational specificity (training is concentrated in few occupations) for training description T_i, while lower values indicate broader occupational relevance and a lower specificity. This entropy calculation implicitly accounts for the varying numbers of DWAs associated with different training descriptions since training programs associated with more DWAs have more opportunities to span multiple occupation groups.

The industry-specificity score employs a probabilistic approach using empirical data on the distribution of occupations across industrial sectors from OEWS and builds on the DWA-to-major occupation group matching established in the previous score. Using the OEWS data, let $q_{sg} = \mathbb{P}(O_g | I_s)$ be the conditional probability that a random worker sampled from industry sector I_s will work in major occupation group O_g. I then calculated the weighted industry count R_{is} for training program T_i and industry sector I_s as

$$R_{is} = \sum_g q_{sg} C_{ig}$$

and normalized it to obtain a probability $p_{igs} = R_{is}(\sum_s R_{is})^{-1}$ that training description T_i will be applicable to industry sector I_s. Let the industry-specificity of training description T_i be given by

$$\text{IndSpec}_i = 1 - \frac{-\sum_s p_{is} \ln(p_{is})}{\text{Maximum industry entropy}}.$$

Again, higher entropy corresponds to greater industry specificity.

Measuring firm-specificity presents an interesting empirical challenge since my data do not contain granular firm descriptions; therefore, I rely on NLP to read training descriptions and calculate the firm-specificity score directly. I prompted OpenAI's GPT-4 model using a structured annotation framework to identify whether training description T_i referenced company-specific processes, proprietary technologies, or organization-specific practices. The process began with the LLM identifying discrete skills and competencies mentioned in the training description, and requiring this step caused the LLM to generate reasoning traces that I was likely to agree with, since it involved a deeper consideration of many elements of the training description.

After this enumeration, I asked the LLM to classify each enumerated element into three categories: generic skills applicable across any organization and industry, industry or occupation-specific skills that apply broadly within a sector but are not unique to individual firms, and firm-specific skills that reference company-unique processes, systems, or applications. This tripartite classification enables the LLM to identify firm-specific content while distinguishing it from broader forms of specificity. The evaluation framework includes carefully crafted criteria for identifying firm-specificity markers, including direct references to company names or proprietary systems (e.g. "Company X's workflow"), mentions of unique client types or product lines (e.g. "plastics for performance automobiles"), and language suggesting organization-specific approaches ("our way of doing things" or "how we handle this process"). The full annotation criteria are reproduced in Appendix B.3.1. The methodology emphasizes that operational details alone do not constitute firm specificity unless they represent practices truly unique to the individual organization rather than standard industry practices.

Each description was scored by the LLM on a five-point Likert scale, where 1 is no firm-specific elements, 2 is minimal firm-specific elements, 3 is moderate firm-specific elements, 4 is substantial firm-specific elements, and 5 is entirely firm-specific. Let the firm-specificity of training description T_i be given by

$$\text{FirmSpec}_i = \frac{\text{Likert score}}{5}.$$

The resulting score is sensitive to the language in the training descriptions; however, the model's reasoning was generally consistent with the intended classification criteria.

Generality serves as a complementary measure to the other three specificity measures. I count the number of distinct DWAs associated with each training to obtain the generality of training description T_i

$$\text{Generality}_i = \frac{\sum_j f(T_i, D_j)}{\text{Total \# of DWAs}}.$$

This captures the breadth of competencies covered, irrespective of whether they are general or specific. The core idea is that training descriptions that are either intended to improve or likely to improve many DWAs should contain skills that are highly general.

As an example of these four measures in practice, consider a Tennessee manufacturer that used CJT funding to train workers in advanced injection molding. The LLM mapped the training description to DWAs such as "set up and operate molding machines," "inspect finished products for defects," and "apply safety protocols in machine operation." The occupation-specificity (OccSpec) score is relatively low, reflecting a low occupation entropy that comes from concentration in production-focused occupations. The industry-specificity score (IndSpec) is also low, since nearly all DWAs fall within plastics manufacturing. The program scored "3" on the firm-specificity score (FirmSpec) since the LLM determined it combined generic molding skills with company-specific production techniques. Finally, the generality measure (Generality) captured the broad transferability of these skills by counting three distinct DWAs.

B.2 Verbatim GPT-4o prompt for generating training description to DWA connections

Overview

You will evaluate the relationship between job training programs and specific Detailed Work Activities (DWAs) by assessing two key dimensions: **intent** and **likely impact**. For each training program-DWA pair, you will receive:

- **Training Program Description**: A high-level summary of the training's topics, objectives, and purposes

- **DWA Description**: A detailed description of a specific work activity that workers perform in their jobs

Evaluation Questions

Question 1: Intent to Improve Performance

"Is the job training intended or designed to improve the worker's performance in this work activity?"

This question focuses on the **explicit purpose** and **design intent** of the training program.

What to Look For:

- **Direct mentions** of the work activity or closely related activities in the training objectives
- **Explicit learning goals** that align with work activity requirements
- **Stated purposes** that clearly connect to work activity performance
- **Target skills** that are specifically mentioned as training outcomes
- **Training modules** explicitly designed around the work activity or its components

Examples of Intent Indicators:

- Training description states: "This program teaches employees how to..."
- Objectives include: "Upon completion, workers will be able to..."
- Modules specifically titled or described as addressing the work activity
- Learning outcomes that directly mention activity-related skills or knowledge

What Does NOT Indicate Intent:

- Vague, general skill development that could apply to many activities
- Broad organizational or cultural training without specific skill focus

- Training that only tangentially relates to the work activity domain

Question 2: Likely to Improve Performance

"Is the job training likely to indirectly improve the worker's performance in this work activity, even if that is not its intent?"

This question assesses **potential indirect benefits** and **transferable skills**, regardless of the training's stated purpose.

What to Consider:

- **Related, similar skills** that are highly similar to those taught in the course, but not directly covered

- **Transferable knowledge and competencies** that benefit other activities in the same discipline

- **Related technical skills** that complement activity requirements

- **Application of skills in new contexts** that were not included in the original training, but are likely to benefit from it.

Examples of Likely Improvement:

- Training on communication with employees also indirectly improves communication with colleagues or supervisors

- Training on chemical safety in an industrial environment also indirectly improves chemical safety in a lab environment

- Training on environmental compliance and environmental laws also indirectly improves ability to learn and apply knowledge of regulatory compliance in other domains

Factors That May Limit Improvement:

For performance in a task to be indirectly improved by a training, it is not sufficient that some element of the task is marginally improved. For example, a creative writing training is not likely to improve any and all tasks that might tangentially involve writing, like documenting company policies.

Evaluation Framework

Step 1: Analyze the Training Program

Before considering the specific work activity, create a list of skills the job training is intended to improve.

Step 2: Analyze the Work Activity

- Break down the activity into **key components** and **required skills**

Step 3: Assess Intent (Question 1)

- Look for **explicit connections** between training objectives and activity requirements
- Check if the activity or its components are **directly mentioned** in training materials
- Evaluate whether the training was **specifically designed** to address this type of work
- Consider the **specificity** of the connection (direct vs. general)

Step 4: Assess Likely Impact (Question 2)

- Identify **potential skill transfers** from training to work activity
- Consider **indirect benefits** and **supporting competencies**
- Evaluate the **strength of the connection** between training content and activity performance

Decision Guidelines

For "intended_to_improve: true"

- Training explicitly mentions the work activity or very similar activities
- Learning objectives directly align with activity performance requirements
- Training modules are specifically designed around activity-related skills
- Clear, unambiguous connection between stated purpose and work activity

For "intended_to_improve: false"

- No explicit mention of the work activity in training materials
- Training objectives focus on different areas/skills
- Training appears designed for other purposes
- Connection to activity is only inferential or indirect

For "likely_to_improve: true"

- Training develops skills that reasonably transfer to the work activity
- Content provides specific knowledge supporting activity performance

For "likely_to_improve: false"

- Training content is unrelated to work activity requirements
- Skills taught don't transfer meaningfully to the work activity
- Training level is inappropriate for activity needs
- No reasonable pathway for improvement in work activity performance

Response Format

After your analysis, provide your reasoning in paragraph form, then conclude with:

intended_to_improve: true | false
likely_to_improve: true | false

B.3 Inter-rater reliability: grader instructions

Overview

You will evaluate the relationship between job training programs and specific Detailed Work Activities (DWAs) by assessing two key dimensions: **intent** and **likely impact**. For each training program–DWA pair, you will receive:

- **Training Program Description**: A high-level summary of the training's topics, objectives, and purposes
- **DWA Description**: A detailed description of a specific work activity that workers perform in their jobs

B.3.1 Evaluation questions

Question 1: Intent to improve performance "Is the job training intended or designed to improve the worker's performance in this work activity?"

This question focuses on the *explicit purpose* and *design intent* of the training program.

What to look for:

- Direct mentions of the work activity or closely related activities in the training objectives
- Explicit learning goals that align with work activity requirements
- Stated purposes that clearly connect to work activity performance
- Target skills that are specifically mentioned as training outcomes
- Training modules explicitly designed around the work activity or its components

Examples of intent indicators

- Training description states: "This program teaches employees how to..."
- Objectives include: "Upon completion, workers will be able to..."
- Modules specifically titled or described as addressing the work activity
- Learning outcomes that directly mention activity-related skills or knowledge

What does not indicate intent

- Vague, general skill development that could apply to many activities
- Broad organizational or cultural training without specific skill focus
- Training that only tangentially relates to the work activity domain

Question 2: Likely to improve performance *"Is the job training likely to indirectly improve the worker's performance in this work activity, even if that is not its intent?"*

This question assesses potential indirect benefits and transferable skills, regardless of the training's stated purpose.

What to consider

- Related skills that are highly similar to those taught in the course, but not directly covered
- Transferable knowledge and competencies that benefit other activities in the same discipline
- Related technical skills that complement activity requirements
- Application of skills in new contexts that were not included in the original training, but are likely to benefit from it

Examples of likely improvement

- Training on communication with employees indirectly improves communication with colleagues or supervisors
- Training on chemical safety in an industrial environment indirectly improves chemical safety in a lab environment
- Training on environmental compliance and laws indirectly improves ability to apply regulatory knowledge in other domains

Factors That May Limit Improvement Marginal overlap is not sufficient. For example, creative writing training is not likely to improve tasks that only tangentially involve writing (e.g., documenting company policies).

B.3.2 Evaluation framework

1. **Analyze the Training Program**: Create a list of skills the job training is intended to improve.
2. **Analyze the Work Activity**: Break down the activity into key components and required skills.
3. **Assess Intent (Question 1)**:
    - Look for explicit connections between training objectives and activity requirements
    - Check if the activity or its components are directly mentioned in training materials
    - Evaluate whether the training was specifically designed to address this type of work
    - Consider the specificity of the connection (direct vs. general)

4. **Assess Likely Impact (Question 2)**:
   - Identify potential skill transfers from training to work activity
   - Consider indirect benefits and supporting competencies
   - Evaluate the strength of the connection between training content and activity performance

B.3.3 Decision guidelines

For intended_to_improve: true

- Training explicitly mentions the work activity or very similar activities
- Learning objectives directly align with activity performance requirements
- Training modules are specifically designed around activity-related skills
- Clear, unambiguous connection between stated purpose and work activity

For intended_to_improve: false

- No explicit mention of the work activity in training materials
- Training objectives focus on different areas/skills
- Training appears designed for other purposes
- Connection to activity is only inferential or indirect

For likely_to_improve: true

- Training develops skills that reasonably transfer to the work activity
- Content provides specific knowledge supporting activity performance

For likely_to_improve: false

- Training content is unrelated to work activity requirements
- Skills taught don't transfer meaningfully to the work activity
- Training level is inappropriate for activity needs
- No reasonable pathway for improvement in work activity performance

B.3.4 Response format

- You **may** jot down notes in the analysis and notes column of the spreadsheet, but this is optional.
- The required step is to populate the dropdown columns with `true/false` values for each of `intended_to_improve` and `likely_to_improve`.

B.4 Inter-rater reliability analysis

I sketch out a labeling criteria to determine the link between training programs and DWAs. I use this criteria to annotate each link using a large language model. This allowed us to scale the methodology. Because there are more than 1,000 training programs and 2,000 DWAs, I needed to label more than 2,000,000 training-DWA pairs in order to apply our occupation and industry specificity methodology. In order to validate the use of an LLM for this task, I conduct an inter-rater reliability analysis that allows us to determine (1) whether an LLM performs like a human in this context and (2) whether the labeling criteria are sufficiently precise as to enable valid inferences based on the labels that I see.

From the population of training-DWA pairs, I sample a total of 300 unique pairs, stratified evenly between positive and negative predictions. I assign these tasks to three human labelers. Each labeler labels 200 pairs, with 100 pairs each shared with one of the other labelers. I compared their labels using standard agreement and accuracy metrics as well as Cohen's kappa, a measure of the degree of agreement between raters after correcting for chance agreement.

I find that the model, grader 1, and grader 3 all agreed to the same extent, with a Cohen's kappa of around 0.25 for grader-model pairings and 0.30 for the grader-grader pairing. Grader 1 performed differently, with a much lower Cohen's kappa (0.13 and 0.17) with the human graders and a near chance (0.04) kappa with the LLM predictions.

On the one hand, the Cohen's kappa among the model and the first two graders is well above chance. This suggests that there is a real measurement we're making here, and that different people agree on some standard of the 'intent to improve' a DWA with a training. However, the degree of agreement remains 'fair'; there is disagreement between graders and between the model about how to apply this concept. In future work, we may want to attempt to clarify our annotation criteria further, although some degree of disagreement is to be expected in a judgement task such as this one. Grader 2's wider divergence is a little concerning; it suggests that our criteria may be unclear and interpreted differently by some labelers.

It is important to note that a better or worse IRR doesn't bear on any results that we find. This paper attempts to make measurements based on some underlying signal of the relationship between DWAs and trainings. The clearer I can be about defining the signal I'm interested in, the better humans and LLMs can identify it, and the less noisy the data. The conclusion from the IRR is that I am recovering some signal I am interested in, but I'm also getting some noise. Any inferences I draw based on this data would be valid, but in future work, I might be able to improve the power of the inference by reducing the noise introduced at the labeling stage.

The best proof for the validity of the method that we use to draw out signal about specificity is to just look at the list of trainings with a few examples from each strata of score; this clearly illustrates that I am successful in measuring some concept of interest.

C Employer survey

C.1 Survey design

The survey was constructed using Stanford Qualtrics. The survey was tailored to each employer's industry sector, grouped by industry code, and contained four modules: instructions on the highlighter tool, employer-submitted training plans, randomized training plans from other firms in the same industry, and participant characteristics. To manage survey length, employers with more than 200 plans were randomly split across multiple surveys. Question randomization was calibrated so that each training plan would be categorized at least three times if all employers participated.

Approximately half of all training plans belonged to the manufacturing sector. Considering loading time limitations and the availability of respondents, we randomly assigned firms to five survey groups specific to the manufacturing sector. We iterated over 50,000 random seeds to determine an optimal allocation of firms (and therefore their corresponding training excerpts) that maintained an average survey duration within 30 seconds across groups, while also ensuring a training excerpt to firm count ratio of 0.5 across groups. Similar procedures were applied to the survey group, comprised of the health and education sectors. Another consideration of loading times was to condense training plans.

Average loading times were 5.6 seconds for employer-specific plans and 6.8 seconds for randomized plans. To achieve these loading times, training plans exceeding 1,024 characters were condensed. Of the 1,366 training plans, 26.65% exceeded 1,024 characters. I used a large language model (LLM) to reduce length while preserving key content. The LLM was implemented in two steps. First, instructions to the LLM required retention of occupational titles and industry-specific terminology, maintenance of the original task structure (e.g., bullets or enumerated steps), and elimination of redundant narrative detail. Following the first step, 16 training plans were reduced to less than 1,024 characters. Second, plans were condensed only if these criteria still exceeded the 1,024-character limit. Following the second step, 348 training plans were condensed.

The first pass LLM prompt instructed the model with two overarching instructions with 10 discrete instructions. Overall, the model was told: you are preparing a training description for a Qualtrics survey and your task is purely to trim and polish the text—do not summarize or omit content beyond formatting fixes. The model was also provided 10 following formatting rules. First, expand shorthand (e.g., "w/" → "with"), fix spacing, and remove any garbled or odd characters (e.g., "??", "€"). Second, identify items sharing a common prefix and factor out that prefix. For example, if the input is "Web Intelligence Document

Management, Web Intelligence Documents with Queries, Web Intelligence Document Design, Web Intelligence Document Formatting", the the output should be "Web Intelligence Document Management, Documents with queries, design, and formatting." Third, convert ANY numbered or lettered list into a comma-separated series with "and" before the last item. For example, if the input is "1. A 2. B 3. C", then the output should be "A, B, and C." Fourth, remove all list prefixes, including "-" at the start of lines, and any "Part 1:," "Part 2:," etc. Fifth, remove all "|" characters, replacing each with a comma. Sixth, strip out all hour counts and any dollar amounts. Seventh, retain existing acronyms and avoid condensing existing text into acronyms. Eighth, remove weekly or phased headings such as "Week 1:. . . " or "Phase II:. . . ". Ninth, produce the entire excerpt as a single, smooth paragraph suitable for Qualtrics. Tenth, ensure full, grammatical English with no new acronyms, ellipses, or bullet markers. Finally, return only the transformed text.

The second pass LLM prompt instructed the model by stating "You are preparing a training description for a Qualtrics survey. Excerpts may be technical and written years ago. Respondents are industry professionals who need detailed, accurate, readable summaries." The model was provided with skill definitions and instructed to "keep words or phrases that correspond to these terms intact)". General skills are applicable across all industries, occupations, and companies. Industry-specific skills are unique to a specific industry, but not tied to a single occupation or company. Occupation-specific skills are unique to a specific occupation, but not tied to a single industry or company. Company-specific skills are unique to a single company. The model was asked to "Please return only the cleaned text (*strictly under 1024 characters*) that do the following. First, preserve core technical content and any words or phrases that correspond to General, Industry-specific, Occupation-specific, or Company-specific skills terms. Second, keep all nouns and noun modifiers. For example, keep "Company specific machinery" where "company" is a noun modifier as this will correspond to a company-specific skill. Third, remove or compress repetitive details as needed to reach the character limit. Fourth, compresses secondary details as needed to reach the character limit. Fifth, produce the entire excerpt as a single, smooth paragraph suitable for Qualtrics that stays under 1024 characters. Sixth, retain existing acronyms and avoid condensing existing text into acronyms.

C.2 Sampling and outreach

The survey targeted employers that had previously received state training grants. Partnering with the Tennessee Department of Labor and Workforce Development and nine regional Executive Directors, I compiled contact information from digitized grant applications. Executive Directors provided updated email addresses and sent advance notifications highlighting collaboration between Stanford, the Department, and workforce offices, and expressing the survey's role in informing future training initiatives (see Figure 6). We did not have contact information for 8.92% of businesses implying the business had closed or could not be contacted via email or phone.

C.3 Implementation and incentives

On August 18, 2025, I distributed personalized survey links via Mailchimp to 336 employers. Of these, 199 opened the email, 140 began the survey, and 116 completed it. Fifteen emails bounced permanently and 22 emails bounced temporarily. Open rates were tracked through embedded HTML pixels.

The survey remained open for three weeks. Respondents completing within the first two weeks received a \$50 gift card; those in the final week received \$100. Incentives were distributed within two weeks. Non-respondents were contacted with email and phone reminders. I made 204 follow-up phone calls. If the call was not answered, I left a voicemail and left a standardized reminder message.

C.4 Response rates and participant characteristics

Overall, 54.7% of training plans were categorized at least once. Of 116 completers, 88% answered the optional participant-characteristics module: 62% held upper-management or administrative roles (COO, VP, Manager), and 38% worked in human resources. On average, respondents reported 8.3 years in their current position, 11.9 years with their employer, and 17 years in their industry.

C.5 Results

The NLP-based classifications of training content provide the primary measures of skill specificity in this paper, and the employer survey offers complementary validation. Between August and September 2025, I surveyed employers who had previously participated in CJT, asking them to categorize training plans by skill type. Each firm reviewed both its own training plans and randomly selected excerpts from other firms in the same industry, using a highlighting tool to classify text into four predefined categories: general, industry-specific, occupation-specific, and firm-specific skills. Surveys were customized to each employer, and participants also reported their job role and experience in the company and industry.

The employer survey required extensive collaboration with state partners to secure employer participation. With the support of the Tennessee Department of Labor and Workforce Development and the nine regional Workforce Executive Directors, I obtained contact information from application records and supplemented missing details through regional staff. Executive Directors formally introduced the project to employers and requested their participation, lending the survey official backing. Each employer then received a personalized survey link and access code, with the state copied on all correspondence. To maximize participation, I followed up with reminder emails, phone calls, and voicemails to all non-completers. Participants were compensated with gift cards, and when initial response rates lagged, the honorarium was increased from $50 to $100. These efforts underscore the scale of resources required to elicit employer evaluations of training content at scale.

The employer survey yielded broad coverage. Out of 1,366 training plans in the dataset, 54.7% were reviewed and categorized by at least one participant. Of the 321 employers surveyed, 62% opened the survey, 44% participated, and 36% completed it.[14]

The survey responses align closely with the entropy-based and firm-specificity measures, providing confidence that the classifications capture meaningful distinctions between general and firm-specific human capital. The survey finds that the training programs are 83% transferable or partially transferable and 17% nontransferable; agreeing with the NLP classification of the transferability of training content despite the survey requiring each skill to be mutually exclusive. Report the correlation of the measures.

Training excerpts that were categorized by survey respondents were not evenly distributed across program years. As shown in Figure A.6a, the majority of training excerpts reviewed by employers were from programs delivered between 2020 and 2024, with ample variation within those years. This concentration ensures that employer classifications reflect the most contemporary training practices in Tennessee, rather than being driven by earlier program cohorts.

The employer survey confirms that CJT programs deliver a blend of skill types with varying degrees of transferability. Respondents reported that 26% of competencies were general, 24% industry-specific, 33% occupation-specific, and 17% firm-specific (Figure A.6b). These distributions closely align with the NLP-based classifications, reinforcing the conclusion that training content is not purely firm-specific but instead spans a mix of portable and tailored skills.

Beyond coverage, the survey provides direct evidence on how the composition of skills taught has evolved over time. Figure A.6d shows the distribution of general, occupation-specific, industry-specific, and firm-specific skills across program years. While 2017 contained very few excerpts, later years exhibit relatively stable shares of each skill type, consistent

[14]Survey open rates were tracked using a pixel integrated into email outreach. Survey response rates for employer training surveys are typically low. For example, Barron et al. (1997) reports that only about 20% of firms and their recently hired workers completed their survey. In this context, the participation rate in my survey compares favorably.

with the patterns captured by the NLP-based measures.

Finally, the survey makes these distinctions concrete by showing the language employers associate with each skill type. Figure A.7 presents word clouds generated from the text highlighted by employers in each category. General training emphasizes broad, transferable workplace competencies. Occupation-specific training highlights technical and role-based tasks. Industry-specific training emphasizes sectoral knowledge. Firm-specific training reflects proprietary processes and company-unique practices. Some words are repeated in each word cloud, such as "safety." Since the survey was completed by humans who do not have a unified or empirical reason for categorizing text other than their personal experience, it is likely that "safety" could be considered a general skill to one survey respondent and occupation- or industry-specific to another.

The credibility of these survey measures is supported by both the depth of respondent experience and the quality of engagement. Participants who completed the survey spent an average of 19 minutes on the instrument, consistent with pre-tested completion times (Appendix C). Respondents were typically senior managers or human resource professionals, positions well-suited to evaluating training practices (see Figure A.8). On average, participants reported 8.3 years in their current role, 11.9 years at their company, and 17.8 years in the industry, providing confidence that the classifications reflect informed assessments of training content.

At the same time, two features of the survey design may explain differences relative to the NLP classifications. Employers may overstate the degree of transferability if they perceive that reporting firm-specific content could signal that subsidies are underwriting narrow, private training rather than public skill development. In addition, because of task complexity, respondents were required to classify each training element into a single category rather than allocate across multiple types. These constraints may account for the gaps between the survey-based and NLP-based measures.

C.6 Validation of engagement

Survey metrics suggest respondents were attentive. Average duration was 22.7 minutes (median 19.3), with predicted times based on reading speeds and buffer questions closely aligned. Completers averaged 22.7 minutes versus 6.2 minutes for non-completers.

Phone outreach further reinforced survey legitimacy: employers who answered calls or received voicemail reminders acknowledged the collaboration with state partners and appreciated the opportunity to contribute. Combined with direct feedback from Executive Directors, this supports the conclusion that survey responses were genuine and engaged.

Figure C.1: Survey preface email from Tennessee state officials

Dear [Business Name/Employer Contact],

As a valued participant in Tennessee’s Incumbent Worker Training (IWT) or On-the-Job Training (OJT) programs, your organization plays a vital role in advancing workforce development across the state.

We’re pleased to inform you that the Tennessee Department of Labor and Workforce Development is partnering with Stanford University's Hoover Institution on a research study aimed at better understanding the return on skills workers gain through these training programs.

This critical study will help us evaluate the effectiveness of workforce training efforts and guide future initiatives that more effectively support both employers and employees.

In the coming weeks, you will be contacted by Natalie Millar on behalf of the research team to complete an online survey. As a token of appreciation for your time and insights, Stanford University’s Hoover Institution will provide a **$50 gift card** upon completion of the survey.

Thank you for your continued commitment to workforce development.

Notes: Survey participants were first informed about the survey from a standardized outreach email written by state officials. This text was used in emails sent to firms by Executive Workforce Directors.

Figure C.2: Survey email

TN Department of Labor & Workforce Development

The Tennessee Department of Labor and Workforce Development, with Stanford University's Hoover Institution, invites you to complete an online survey on Incumbent Worker Training (IWT) and On-the-Job Training (OJT) programs.

Complete the survey and receive a **$50 gift card** from Stanford University's Hoover Institution.

Survey details
Access code: << Test Survey_Access_Code >>
Link: << Test Qualtrics_Survey_URL >>
Opens: August 18, 2025 (8:00 a.m. CT)
Closes: August 24, 2025 (11:59 p.m. CT)
Time: << Test Estimate_for_Email >>

Responses are confidential. Questions? Contact Dr. Natalie Millar (nemillar@stanford.edu).

Thank you for your time and insights.

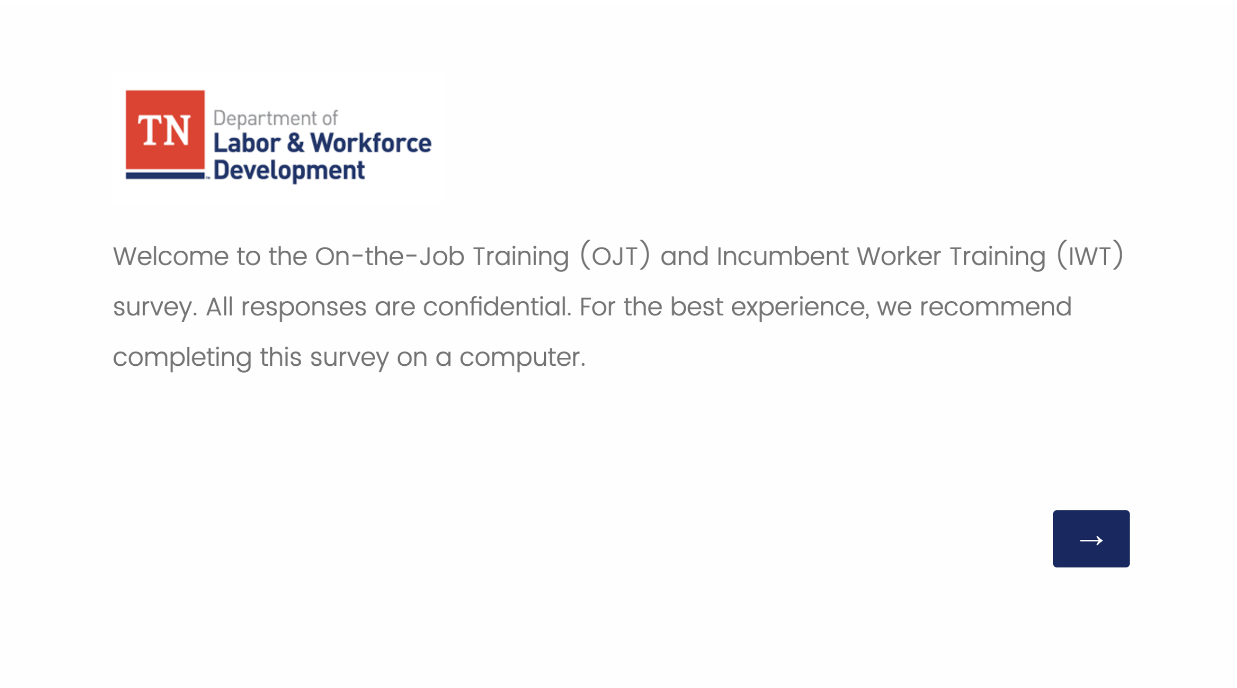
View email in browser

Stanford University · 655 Knight Way · Stanford, CA 94305 · USA

This email was sent to << Test Email Address >>
why did I get this? unsubscribe from this list update subscription preferences
Stanford University · 655 Knight Way · Stanford, CA 94305 · USA

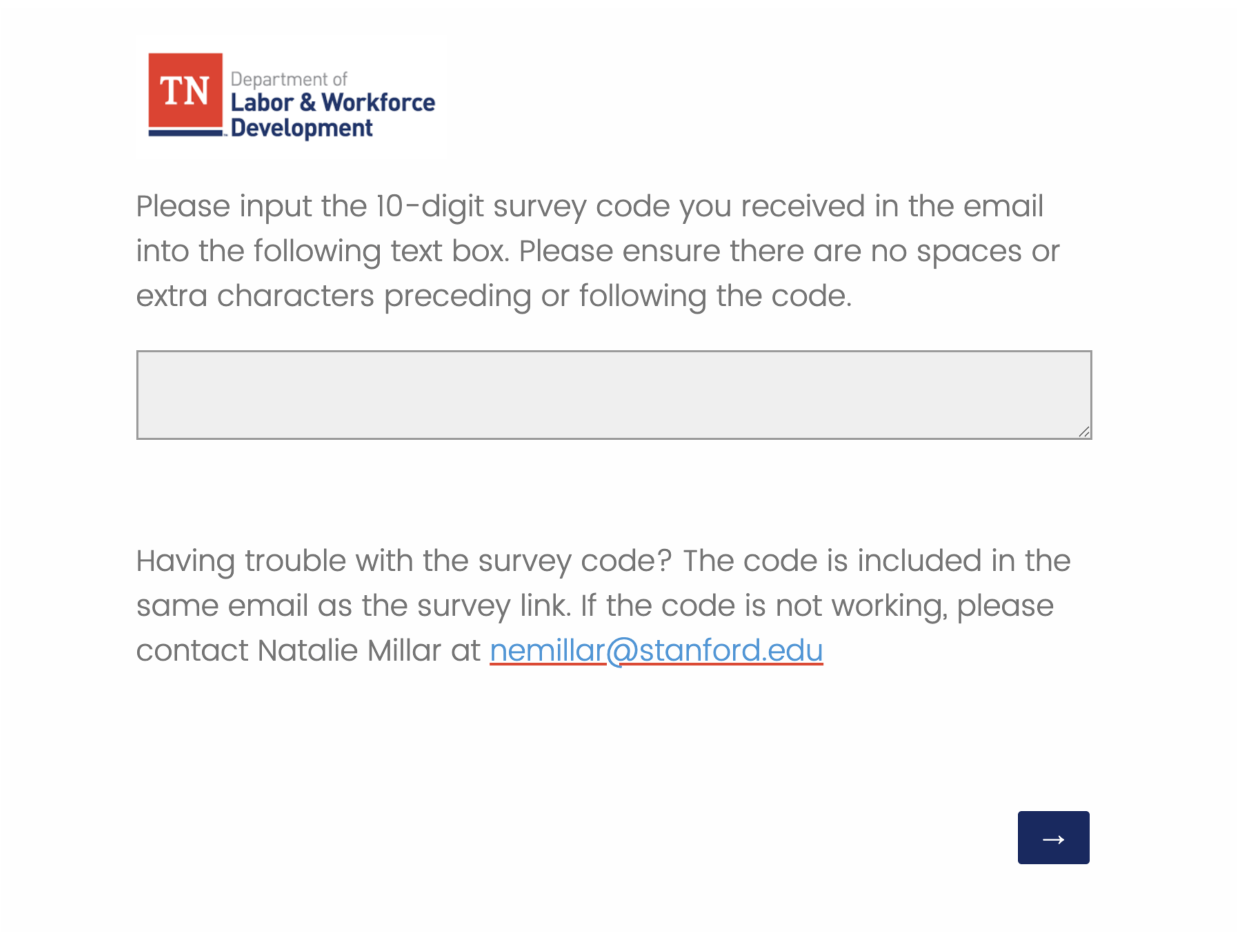
Notes: Survey participants were initially contacted via Mailchimp with the text in this outreach email. Participants were provided a unique link and access code to the survey.

Figure C.3: Survey welcome page



Notes: This figure shows the welcome page of the survey. Participants' individual responses in the survey are confidential.

Figure C.4: Survey access code



Notes: To participate in the survey, employers received an access code that was specific to their company. This access code populated the training plans previously submitted by that company, as well as a set of random training plans for other companies in the same industry.

Figure C.5: Survey instructions

TN Department of Labor & Workforce Development

In this survey, you'll review excerpts from training plans submitted by your company and others in your industry.You'll then highlight words or phrases that fit into one of four categories:

General Skills: apply across all industries and occupations
Industry-Specific Skills: unique to an industry, but not a single occupation or company
Occupation-Specific Skills: unique to an occupation, but not to one industry or company
Company-Specific Skills: unique to a company

Excerpts are shown exactly as submitted and may contain minor errors. Next, you'll see a short video showing how to use the highlighter tool.

→

Notes: This survey page provides survey instructions and definitions of the four skill types: general, occupation-specific, industry-specific, and company-specific.

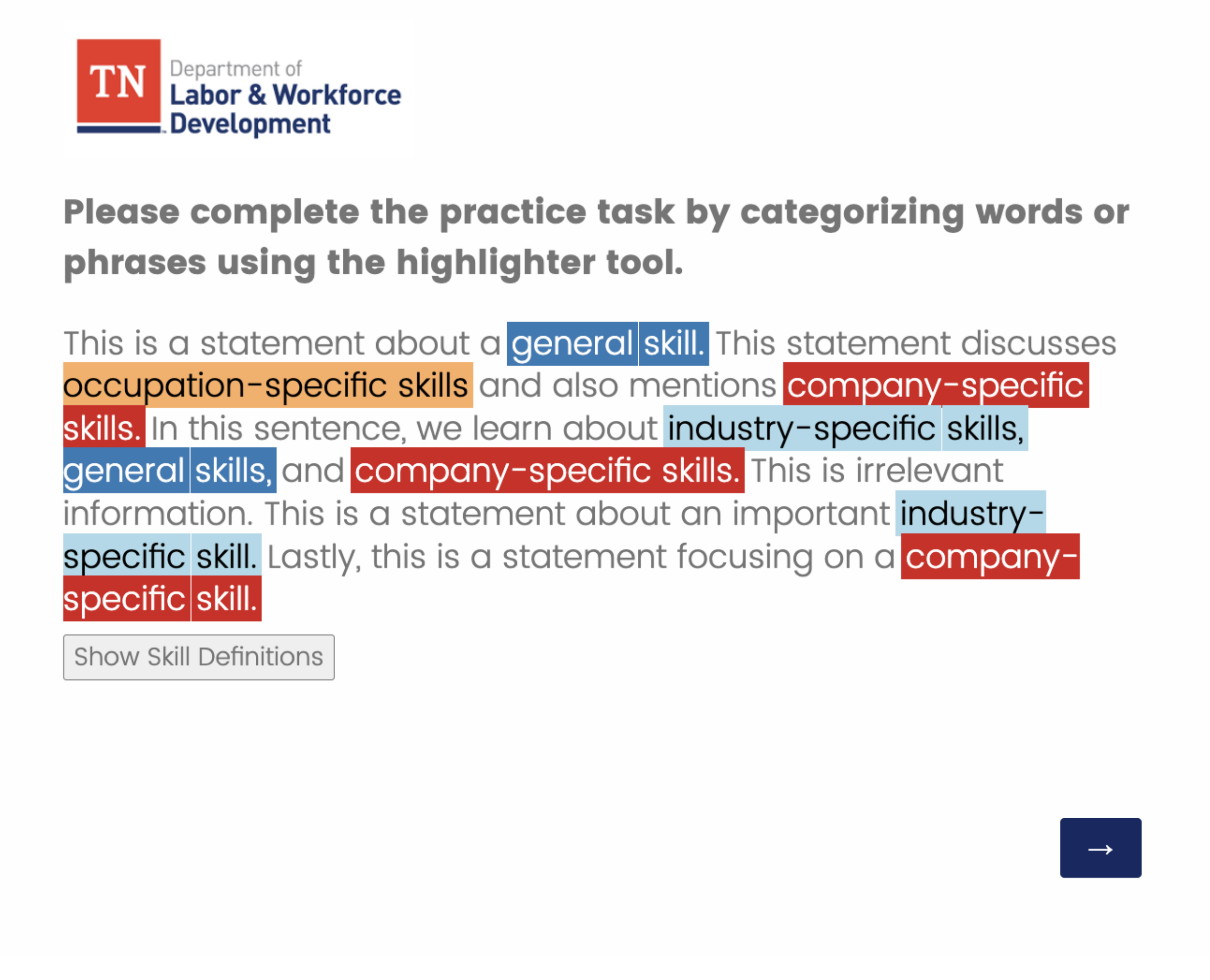
Figure C.6: Instructional video - How to use the highlighting tool

Notes: Participants had the option to view a short YouTube video that walked them through the highlighting tool.

Figure C.7: Instructional video - "Show Skill Definitions" button

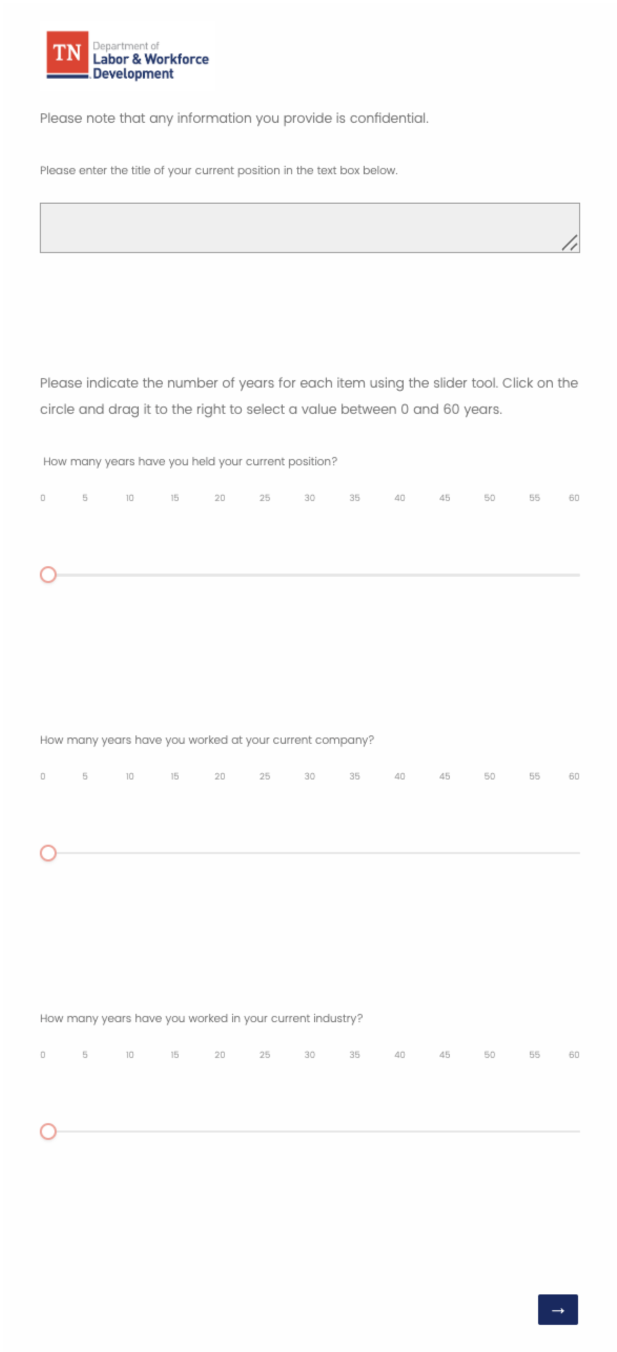
Notes: Participants were able to click the "Show Skill Definitions" button at any time.

Figure C.8: Practice question



Notes: Participants were encouraged to complete a practice question to familiarize themselves with the highlighter tool.

Figure C.9: Participant characteristics



TN Department of Labor & Workforce Development

Please note that any information you provide is confidential.

Please enter the title of your current position in the text box below.

Please indicate the number of years for each item using the slider tool. Click on the circle and drag it to the right to select a value between 0 and 60 years.

How many years have you held your current position?

0 5 10 15 20 25 30 35 40 45 50 55 60

How many years have you worked at your current company?

0 5 10 15 20 25 30 35 40 45 50 55 60

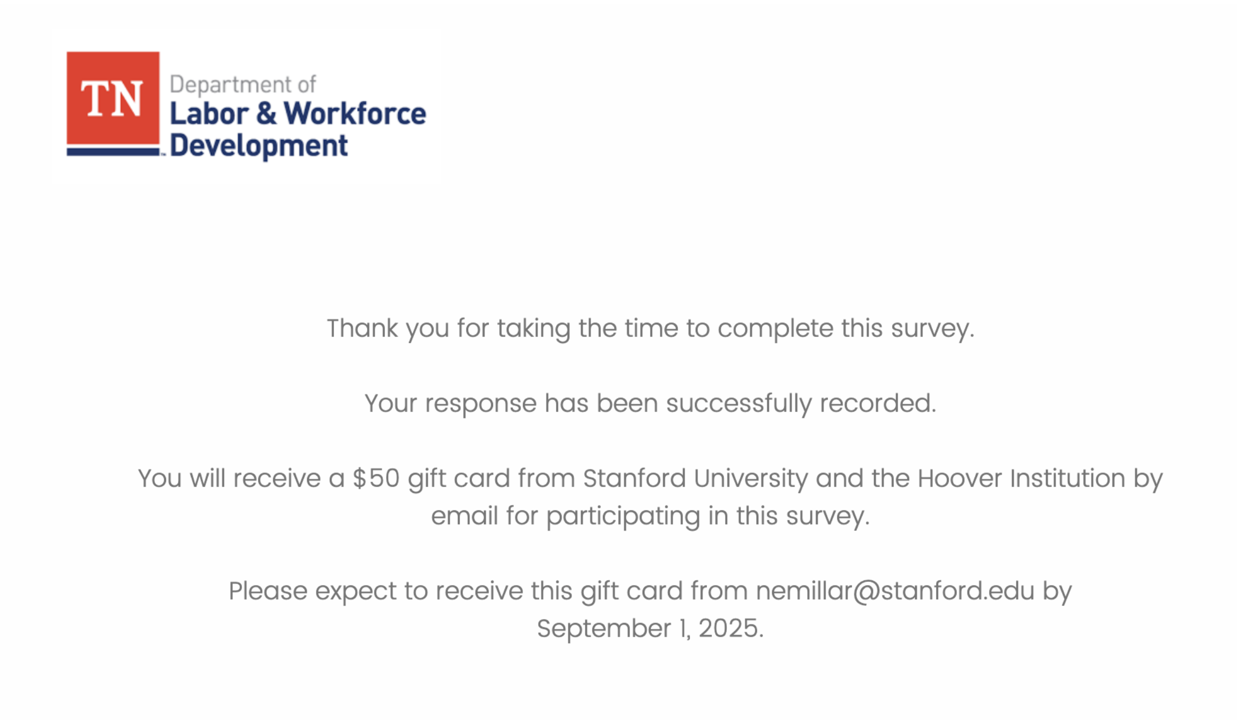
How many years have you worked in your current industry?

0 5 10 15 20 25 30 35 40 45 50 55 60

→

Notes: At the end of the survey, participants were provided the option to share the title of their current position and other measures of experience in their position, at their company, and within the respective industry of their employer.

Figure C.10: Survey completion



Notes: Survey participants were thanked for their participation and notified of when they would receive a gift card for their participation.