NBER WORKING PAPER SERIES

DO WORKFORCE DEVELOPMENT PROGRAMS BRIDGE THE SKILLS GAP

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Working Paper 34012
http://www.nber.org/papers/w34012

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2025

We thank seminar participants at Arizona State University, Carnegie Mellon, the Cleveland Fed, Columbia, the Family and Education Workshop 2025, the Federal Reserve Bank of Atlanta, the Federal Reserve Bank of Dallas, the Markets with Frictions Tinos Edition, NBER Organizational Economics, SITE: The Micro and Macro of Labor Markets, Society of Labor Economics, San Diego State University, SEML, University of Michigan, and the University of Rochester. We are indebted to Kalab Begna, Edoardo Bella, Klint Mane, and Sid Satya for excellent research assistance and to an excellent team of undergraduate students who assisted us with data collection and validation. This project was funded in part by generous support from the Smith Richardson Foundation. All views expressed in this paper are those of the authors and do not necessarily reflect the views or policies of the U.S. Bureau of Labor Statistics or of the National Bureau of Economic Research.

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NBER Working Paper No. 34012
July 2025
JEL No. J24, J42, J68, M53

ABSTRACT

Most U.S. states have workforce development programs that offer firms grants to train their own workers. We create unique data linkages between participating firms, employment, and vacancies to explore the determinants and consequences of such programs. Training grants are more prevalent in markets where firms face greater employee poaching risk, suggesting these programs help overcome a market failure in updating worker skills. After training, firms experience prolonged employment growth and down-skilling in their job posts, relative to a matched control group. Training appears to help firms move toward optimal scale and expand opportunities for less skilled workers.

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1 Introduction

Technology and international trade have changed the nature of work in the United States, shifting demand towards workers with a college degree and compressing the bottom of the earnings distribution. At the same time, employers commonly lament a "skills shortage," a problem that was exacerbated in the extremely tight labor markets of the COVID recovery.[1] Formal schooling is a proposed front-line solution for these problems, as differences in employment and wages between those with and without a college degree are stark (Abraham and Kearney, 2020; Autor, 2019). However, educational attainment has stagnated, and, due to the pace of technological change, many workers will need to acquire new skills throughout the course of their careers (Murphy and Topel, 2016; Goldin and Katz, 2008). Unfortunately, public-sector job training programs have historically had, at best, mixed success at offering an alternative to formal schooling.

Private-sector training programs may be more effective because employers know best which skills they need. However, employers will be reluctant to pay to train workers in general skills for fear that their investment will be poached away (Becker, 1964), and workers may not have the resources or knowledge to invest in training themselves (Caliendo et al., 2022). Public-private partnerships, characterized by employer-driven training funded at least in part by the public sector, may help to overcome these frictions. Federal funding for these partnerships has increased in the last decade and most states in the U.S. have at least one program whereby employers apply for grants funded by the government to train their incumbent (either existing or newly hired) workers. Nonetheless, there are no broad-based studies about how and why these programs operate and whether they are successful.

What can the presence and effect of public-private incumbent worker training programs tell us about frictions in worker training and skills gaps? In this paper, we assemble a new dataset of participating firms linked to two rich firm-level datasets – the Quarterly Census of Employment and Wages (QCEW) and the Burning Glass job vacancy data (BG).[2] We focus on 18 U.S. states with parsable firm-level data on program participation. To better understand the need for training subsidies in the private sector, we analyze the characteristics of employer participants and the markets they hire in, relative to employers and markets that have not had grants. To understand how program participation impacts labor demand, we then examine the impact of program participation on

[1]See for instance a recent McKinsey report (Laboissiere and Mourshed, 2017) which found that "almost 40 percent of American employers say they cannot find people with the skills they need, even for entry-level jobs," and Forsythe et al. (2022) on the labor supply shortage during the COVID recovery.

[2]The BG database comes from the company now known as Lightcast. They scrape and code the near universe of job vacancies posted to online websites such as job boards and individual company websites and use proprietary algorithms to parse, deduplicate, and code the content of the ads. See Hershbein and Kahn (2018) for an early use of BG and more details.

employment and vacancies using an event study and nearest-neighbor matching design.

These programs typically have explicit goals of helping to upskill the state's workforce, especially in skills that would be transferable across employers. However, training subsidies may also serve as place-based incentive policies, and some states mention focusing on out-of-state competition, especially in under-developed markets. In practice, we find that grants are much more likely to be used in competitive labor markets, as measured by the concentration of hiring firms or market tightness. Theory suggests these competitive labor markets should experience the most underinvestment in training because firms will be most concerned about losing trained workers to competitors (workers can face constraints to skill investment everywhere). We also find that grants are disproportionately allocated to larger and higher paying firms and labor markets and to firms seeking to hire more skilled workers. We find no evidence that the grants are used to even out prospects across neighboring markets or that grants are targeted to emerging labor markets, to firms that are new to the state but not new overall, or to megafirms that might have outsized influence across states.

Next, we analyze the impact of program participation on firm hiring and employment outcomes. We use an event-study design to compare treated firms to, first, all other firms and, second, a matched sample of similar firms. In our preferred specification, we use a control group that is matched on several lags of size and propensity to post vacancies captured in Burning Glass, allowing us to compare treated firms to those at a similar observed point in their life-cycle growth and recruiting trajectory. After grant receipt, vacancies and realized employment levels at participating firms increase relative to the control groups and continue to grow for many years. Using job vacancies as a proxy for the composition of employment growth, we find that ad shares shift away from professional occupations and toward lower-skilled positions. Employers also reduce requirements for education and related work experience post-training. Perhaps because of these shifts, we see no relative impact on participating firms' overall wage bill per worker. These effects on postings accrue over time and are unlikely to be driven solely by direct effects during the training period itself. Instead, grants appear to help firms shift to a long-term higher growth trajectory. The declines in skill requirements may follow from this growth, as in Engbom et al. (2023) who find that growing firms expand most at the bottom. Grants may also resolve specific skill shortages that had been bottlenecking growth or fund investment in "training capital" such that firms are now willing to take a chance on less skilled workers.

Finally, we explore heterogeneity by the types of skills being trained for, which are available for a subset of our data. We map rich text descriptions of training programs into broad occupation categories using a large language model (LLM). The majority of firms with parsable training plans target professional skills, such as leadership and process management. These firms have elevated demand for professional skills before training, which reverts back towards control firms after training. As in the aggregate results, these firms recruit for lower skilled occupations post-training. Controlling for this occupational shift, we find evidence of wage growth. We hypothesize these firms needed managerial skills to accomplish institutional change which, once resolved, enabled them to accomplish productivity growth through expansion in low wage positions. The second most common target of these training proposals is production-related skills. The smaller group of firms training primarily in production appear to be pivoting their workforce to work alongside automation technology. These firms have elevated skill requirements – both before and after training – in tasks known to complement machinery. These firms also exhibit relative employment growth after training, but, unlike the first group, do not show parallel increases in vacancies post-grant or outsized vacancies prior to training. In this case, training may help with worker retention or in avoiding layoffs.

The evidence we present suggests these grants resolve a skills gap which previously prevented firms from operating at optimal scale. The fact that labor inputs change post grant receipt means that these grants are not purely crowding out private sector funds. Instead, they are, on average, targeting firms on the margin of whether or not to train and facilitating upskilling of the state's workforce. Grants concentrate in more competitive labor markets, where firms should be most reluctant to pay to train their own workers due to poaching risk. Overall, our findings indicate that Beckerian frictions create genuine underinvestment in worker training. When public funds can overcome these frictions, firms enter a period of prolonged higher growth while expanding opportunities for less skilled workers.

We are the first to provide a broad-based evaluation of these public-private incumbent worker training programs, thereby contributing to a large body of literature on training programs more broadly. A seminal literature in economics focuses on government training programs targeted at the long-term unemployed, or other disadvantaged workers, and tends to be quite pessimistic.[3] Card et al. (2018) perform a meta-analysis of a large number of active labor market programs throughout the world and confirm the lack of impact of public sector programs on reemployment, but find positive long-run impacts for other types of programs, such as those in the private sector.[4] Katz et al. (2022) evaluate a series of sectoral training programs that target skills training in areas of local need and especially transferable skills and find positive earnings impacts. Researchers

[3]See for example Ashenfelter and Card (1985); Ashenfelter (1978); Heckman et al. (1998); LaLonde (1986) among many others. These papers tend to find no impacts on program participants and hypothesize that the programs may stigmatize participants, have other close substitutes, face compliance issues, or be poorly run.

[4]O'Connell et al. (2019) compares different types of training programs in Brazil and finds double the reemployment effect for one public employer-informed program compared to a more traditional one.

have highlighted public-private training programs as a potential solution to some of the classic problems with public sector programs, including earlier single-state analyses specifically focused on incumbent worker training programs in Massachusetts (Hollenbeck, 2008), Michigan (Holzer et al., 1993), and New Jersey (Van Horn and Fichtner, 2003). Our systematic cross-state analyses of grant allocation and their impacts help shed light on the motives and benefits of these programs at a broader scale. We are also the first to study these programs following a significant expansion in federal support from the 2014 Workforce Innovation and Opportunity Act.

Our analyses uniquely allow us to target the firm as the focal unit of observation. Much of the past research about training at the firm level focuses on how firm-financed training impacts wages and productivity (e.g., Lynch and Black, 1998; Almeida and Carneiro, 2009; Jones et al., 2012; Konings and Vanormelingen, 2015) rather than how these outcomes vary with government subsidies for training. These studies typically rely on survey-based measures of training which are subject to measurement error and yield varying rates of training provision depending on whether respondents are firms or employees (see Black et al., 2023 for a survey of this literature). Our new collection of firm-level data on state provision of training subsidies means we do not need to rely on self-reported training provision, but rather categorize a firm as offering training based on grant receipt.

Our paper also contributes to the literature exploring the relationship between firm-financed training and labor market concentration. Theoretical models (Becker, 1964; Acemoglu and Pischke, 1998; Stevens, 1996) predict that there will be under-provision of worker training in more competitive markets due to concerns about poaching. Our paper provides new evidence in the U.S. market exploring how training in the presence of subsidies varies with market concentration. We leverage a growing literature on labor market concentration (Yeh et al., 2022; Berger et al., 2022) and especially those that use BG to measure labor market concentration at a highly disaggregated level (Azar et al., 2020; Schubert et al., 2022). We provide evidence that markets with greater poaching risk may indeed suffer from an under-provision of human capital, thereby contributing to a seminal and largely theoretical literature in labor economics on human capital (Becker, 1964; Acemoglu and Pischke, 1998)).[5]

While college graduates learn general analytical skills that help them shift tasks with changing skill demands, a large group of workers with limited formal education may instead invest in specific technical skills that can become obsolete. Less educated workers face a risky and unpromising labor market, discouraging them from re-investing in new skills on their own. This uncertainty and

[5] A number of papers have explored how poaching risk correlates with training provision in the European market, finding mixed support (Muehlemann and Wolter, 2011; Rzepka and Tamm, 2016; Stockinger and Zwick, 2017; Mohrenweiser et al., 2019; Brunello and De Paola, 2008). Our paper provides novel evidence on this question by focusing on the U.S. and specifically tackling the extent to which public-sector involvement can help resolve this friction.

rapid change may have opened gaps between the characteristics of the American workforce and the skills employers need now. Our paper provides a better understanding of one policy lever aimed at closing this gap. In turn, our results shed light on the constraints that prevent firms from providing training without public support.

This paper proceeds as follows. Section 2 provides institutional detail on the training programs we study and lays out empirical tests to better understand the use of public funds for private training. Section 3 describes data sources and summarizes characteristics of training firms. Section 4 relates training allocation decisions to market-level characteristics. Section 5 examines changes in employment and vacancies as a function of grant recipiency. Section 6 discusses differences in outcomes based on type of training. Section 7 concludes.

2 Public-Private Incumbent Worker Training Programs

2.1 Policy Context

Public funding for job training programs has existed at the federal level for well over fifty years. However, the majority of this funding -– and the majority of researchers' evaluations of these programs – have focused on funds that target non-employed individuals in disadvantaged groups. These more traditional job training programs impart skills to the participants that are believed to be valuable in the private sector but typically do not have direct employer involvement. The programs we focus on, in contrast, direct public-sector funds to employers who have applied for a training grant. At the national level, the Workforce Investment Act of 1998 allowed a small use of federal funds for such state-sponsored programs and this allocation was expanded in the Workforce Innovation and Opportunity Act of 2014 (WIOA). WIOA allows states to spend up to 20% of their allocated federal funds on incumbent worker training grants. These programs have largely been overlooked by researchers since the WIOA expansion.

We conducted a comprehensive search of incumbent worker training programs by browsing state training websites and combing program annual reports for detail. We tracked programs where the primary training grant recipient is an individual firm – rather than a worker or business consortium – to distinguish from traditional training programs. We find that almost every U.S. state has at least one program. Throughout all analyses in this paper, we will restrict our attention to 18 U.S. states that have parsable firm-level data on program participation (see the map in Figure A.1). We describe the data we collect on these programs in more detail in section 3.

In Appendix section 2, we provide a comprehensive comparison of programs, which vary a great detail across states. Here we review some common themes and the most relevant details. In all states, the firms initiate the grant application process. Firms must submit a proposal that specifies training needs, a description of the planned training, estimated costs/desired funding, and the number of incumbent or newly hired workers to be trained.[6] Length of training varies, ranging from under six months to two or three years. Firms can and do apply for new grants once their current grant period is completed; 20% of the firms in our sample have multiple grants.

States typically mention wanting to upskill their workforce and help firms keep up with out-of-state competition. As such, most programs require the training to provide industry-recognized credentials, as well as wage increases for the trainees and a guaranteed retention period. States vary in their allocation processes, with rigorous scoring rubrics and competitive processes in some cases and first-come, first-serve allocations in others.

Funding amounts vary with a median of $1,000 per trainee and a mean of $2,240. Firms can therefore expect to recoup roughly $20-$40 per worker-week but not much of their salary outlay. Instead, money can cover training materials, training infrastructure, and small contributions for the opportunity cost of time. Furthermore, in most instances, firms must provide some amount of matching funds (typically 50% of training costs).

Between the limited dollar values, credentialing and pay raise requirements, and administrative overhead surrounding these grant programs, we expect substantial self-selection. Firms will likely only apply when they can usefully train a large group of workers and/or meet the administrative hurdles of application and compliance.

2.2 Conceptual Motivations for Empirical Analysis

Firms and states may want to participate in these programs for many reasons. Theoretic motivations for state governments broadly fit into two classes: easing frictions in private human capital provision or place-based development goals. We discuss each one and lay out observable predictions to motivate our empirical analyses. Our goal is to learn more about these programs and, in so doing, shed light on the role of barriers contributing to the skills gap.

The canonical theories of human capital investment suggest that employers and employees who have already reached a work agreement should also be able to come to an agreement to share both the

[6] We include programs that focus on either incumbent workers or on newly hired workers, meaning that the firm can be asking for money with the intention of hiring unskilled workers that will go through the training before starting their job. Conceptually, we consider grants earmarked for incumbent versus newly-hired workers as equivalent. Neither type of grant includes any help to firms in finding workers to employ or any restrictions on who the firm can hire (as in other programs that incentivize hiring the currently unemployed).

costs for workers to accumulate new skills and the benefits of their resulting increased productivity (Becker, 1962; Mincer et al., 1974). There is no room for the public sector to productively subsidize incumbent worker training. A worker should have to pay the full cost of her training in general skills in a competitive labor market, while the cost of specific skills that are only valuable at the current firm should be split. However, in practice, workers may be reluctant to make these investments due to barriers created by credit constraints (Becker (1964), Belley and Lochner (2007)) and risk aversion (Altonji (1993), Patnaik et al. (2022)). Workers may also face information frictions about the skills demanded by firms or underestimate the potential returns to investing in education and training (see Caliendo et al. (2022) for a review). Small and young firms may also behave like individual workers as they face some of the same borrowing constraints (Banerjee and Duflo (2004), Kerr and Nanda (2009)).[7]

Acemoglu and Pischke (1998) highlight one market imperfection that may solve the underinvestment problem, even in the face of these other constraints. When labor markets are imperfectly competitive, firms can expect to retain their workers and exercise monopsony power. Several recent papers document the degree of monopsony power in many U.S. labor markets (Yeh et al., 2022; Berger et al., 2022). Under monopsony, Acemoglu and Pischke (1998) argue that workers will be less willing to cover the cost of any kind of training, since their lack of bargaining power will prevent them from extracting the gains of their growing productivity. On the other hand, firms should be more willing to cover the cost of investments – even in general skills – since they can expect to retain the benefits without the threat of poaching. We would expect that if these grants are mainly overcoming under-investment in worker skills due to this poaching externality, then they should be more prevalent in competitive labor markets.[8]

Economic literature also motivates the broader place-based development goals of the state. There is ample evidence that states use incentive programs to compete to bring businesses to their state (Bartik, 2017). Funds earmarked for worker training may be a particularly politically appealing tool to induce a large firm to move or remain in state. These incentive programs may make economic sense for individual states, though recent work estimates only small returns (Slattery, 2020).

If place-based development goals are an important driver of funding, then grants should be allocated

[7]Minimum wage laws can create a further barrier by preventing wages from falling far enough to make training workers in general skills worthwhile for the firms (Hashimoto (1982) and others summarized there), even if workers were willing to incur the cost of training. A large literature has explored the relationship between minimum wages and worker training in practice (see Hara (2017) for a recent survey) with mixed results.

[8]Past work also highlights that unions in an imperfectly competitive market can incentivize firms to train workers Booth et al. (2003); Dustmann and Schönberg (2009). Because unions compress the wage structure, training may increase productivity faster than it increases wages, even for general skills, generating positive profits to training. This may explain why we see training grants concentrated in traditionally blue collar industries which have higher rates of unionization than other industries.

wherever the government would like to see growth or employment retention. These may be in areas that are far from the technological frontier where the state would otherwise struggle to attract firms (Neumark and Simpson, 2015), for instance areas with large and healthy neighboring labor markets. As another possibility, a state might offer grants to attract firms to move into the state, in which case we would see grants allocated to establishments that are new in the state, but part of older and larger national firms. Finally, development goals may be targeted towards retaining top employers in the state. Grants would then be allocated to industry leaders or firms with high market shares that are better able to direct funds.

No matter the states' motivations in funding these programs, there is always a risk that public dollars will crowd out private investment, in which case grants would have no impact on firm outcomes. If the grant money tips some firms over the margin of training an additional worker, we might see the impacts of such training on other labor inputs of the firm. There may or may not be many firms exactly on that margin. We expect there to be more of these marginal firms in areas with frictions in skill provision, and we will consequently observe larger effects on firm performance when states target grants to these areas.

Our analyses will proceed in two steps. First, we will describe the distribution of grant participants in terms of firm characteristics and labor market features. In light of the likely strong self-selection of firm applicants, it will be interesting to see whether grant allocations are consistent with stated place-based development goals. Furthermore, we will explore whether grants tend to be used in more competitive labor markets. A greater need for public funds when firms face poaching risk is consistent with under-investment in general skills due to market frictions.

Second, we will evaluate whether grant recipients change labor inputs following program participation, relative to an observational control group. Grant receipt may impact overall growth if production were bottlenecked by the skill being trained for. Also, once training is acquired, firms may shift demand from skill areas covered in the grant to complementary skills. Evidence of impacts implies that public funds are not simply crowding out private investment dollars. Rather, funds are being allocated to firms on the margin of training some number of workers. For any such firms, the firm-specific benefits would not outweigh their private training costs. However, combined with the analysis of how grants are distributed, we can inform whether government dollars are going towards areas where the social benefits to training outweigh the costs due to frictions in the provision of human capital.

3 Data

3.1 Hand-collected program data

After combing state websites and reports to identify programs that match our criteria, we identified 18 states that not only administer an incumbent worker training program, but also retain and publish data on the specific firms that received grants in at least one year. States vary in the number of years of data available, as well as the information about the training provided. We collect data through 2019 if available and we begin as early as 2002 in California. Appendix figure E.1 provides further details on the availability of grant data by year. In addition to firm name, the majority of in-sample states also report the county of participating firm, number of trainees requested, and the grant amount. Appendix figure E.2 reports the number and size of grants by state.

For a subset of the states in our sample (California, Kentucky, Massachusetts, New Hampshire, and New Jersey), we have text descriptions of firms' training plans taken from the grant applications. These descriptions range in length and detail; appendix figure E.3 provides an example of a particularly comprehensive training plan from a company in California. This company manufactures electronic signs and proposes training in machinery as well as a range of basic office skills. We analyze the data on training descriptions separately in section 6.

3.2 Supplemental datasets

We augment our hand-collected information on training grant receipt with data on firm behaviors and outcomes from two sources. The Quarterly Census of Employment and Wages (QCEW) provides administrative data on firm age, industry, employment, and total wage bill. Burning Glass job vacancy data (BG) provides a detailed picture of job posting behavior.

The QCEW is a federal government registry of virtually all businesses in the United States that pay into state Unemployment Insurance programs, plus federal government entities. It covers more than 95% of all jobs and serves as the sampling frame for all Bureau of Labor Statistics establishment surveys. We treat the QCEW as our authoritative benchmark for key firm characteristics and also for determining whether each firm survives from one year to the next.

The BG database of job vacancies is collected by Lightcast, a labor market analytics firm that scrapes websites where job vacancies are posted. Through proprietary machine-learning algorithms, they clean, code, and de-duplicate the scraped ads. Their ad-level data can include the employer name, job location, and job title – which is used by Lightcast to impute an occupation. By targeting over 40,000 websites, the BG data include the near-universe of job openings that are posted online. Their primary business model is to provide analytical tools that help businesses and educators track movements in skill demand. As such, they pay careful attention to measuring the skill requirements specified in job ads. In addition to standard skill measures such as education and experience requirements, they also regularize tens of thousands of key word skills standardized from the open text of job ads. Deming and Kahn (2018) distill these words into a categorization of 10 general skills and show wide variation across firms and geographic space, even within narrowly defined occupations. The data are available consistently from 2010 onwards.

Online job postings are not perfectly representative of all hiring behavior. Previous researchers have found the data to be stable and well aligned with national vacancy trends. Dalton et al. (2025) match BG vacancies to the QCEW and the Job Openings and Labor Turnover Survey and show how the composition of firms vary across datasets, finding a good deal of alignment, though small and low paying firms are under-represented in BG.[9]

We merge grants to establishments in QCEW using firm name, state, and county where available, using a fuzzy match when firms do not have a unique, exact match. We are able to match 95% of grants to an establishment in QCEW. From there, we leverage the QCEW-BG merge from Dalton et al. (2025). 85% of grants in the QCEW sample also have job posting activity in BG in at least one year. The resulting dataset uses firm name-county pairs as its unit of observation – the most detailed level at which we can match. When a firm has multiple establishments in the same county, we consider all establishments to be treated. Throughout, we refer to these name-county pairs as establishments or firms, although the precise unit of analysis is sometimes somewhere between the two. Further detail on the matching process is described in Appendix Section B.

3.3 Characteristics of Training Firms

We use the QCEW and BG samples to form a comparison group of firms. To begin with, we restrict attention to the universe of establishments in states and years in which grant data are available. For grant firms, we restrict our sample to firms receiving grants post-2010 and use the first observed grant as the focal year of grant receipt. For each non-grant firm, we randomly assign a "placebo" grant year to match the empirical distribution of actual grant years in the state. From here, we restrict attention to grant and non-grant firms that have non-zero employment in the year

[9] See also Hershbein and Kahn (2018) who use the BG micro data to understand how the Great Recession changed demand for worker skills. They include a wide range of sanity checks on the data and BG has since risen in popularity among academics.

of grant receipt (or placebo year) and in the prior year.[10] This placebo year assignment will help us select a time window to compare treatment and control firms and for sample selection criteria in our analyses.

The resulting sample includes 8,667 grant firms and 1.7 million control firms. Appendix table E.1 provides summary statistics for grant firms (column 1) and this full set of control firms (column 2) measured in the year before grant receipt (or the placebo year). We provide summary statistics for both the matched QCEW sample and the set of firms that ever post in BG. We also summarize differences across treatment and control group in the distribution of firm characteristics in Figures 1 and 2. These figures take the share of grant receiving establishments with a given characteristic (for instance, size bin or industry) and subtract the non-grant recipient group share.

Beginning with the employment size distribution in the top panel of figure 1, we can see that grant recipients are substantially less likely – nearly 50 ppts – to be in the smallest size class (less than 50 employees) and substantially more likely to be among the middle size classes (especially 50-249). On average, recipients had 225 workers, compared to the control average of about 20 (appendix table E.1). Interestingly, while grant firms are larger on average, we do not see an overrepresentation among megafirms (5000+ employees).

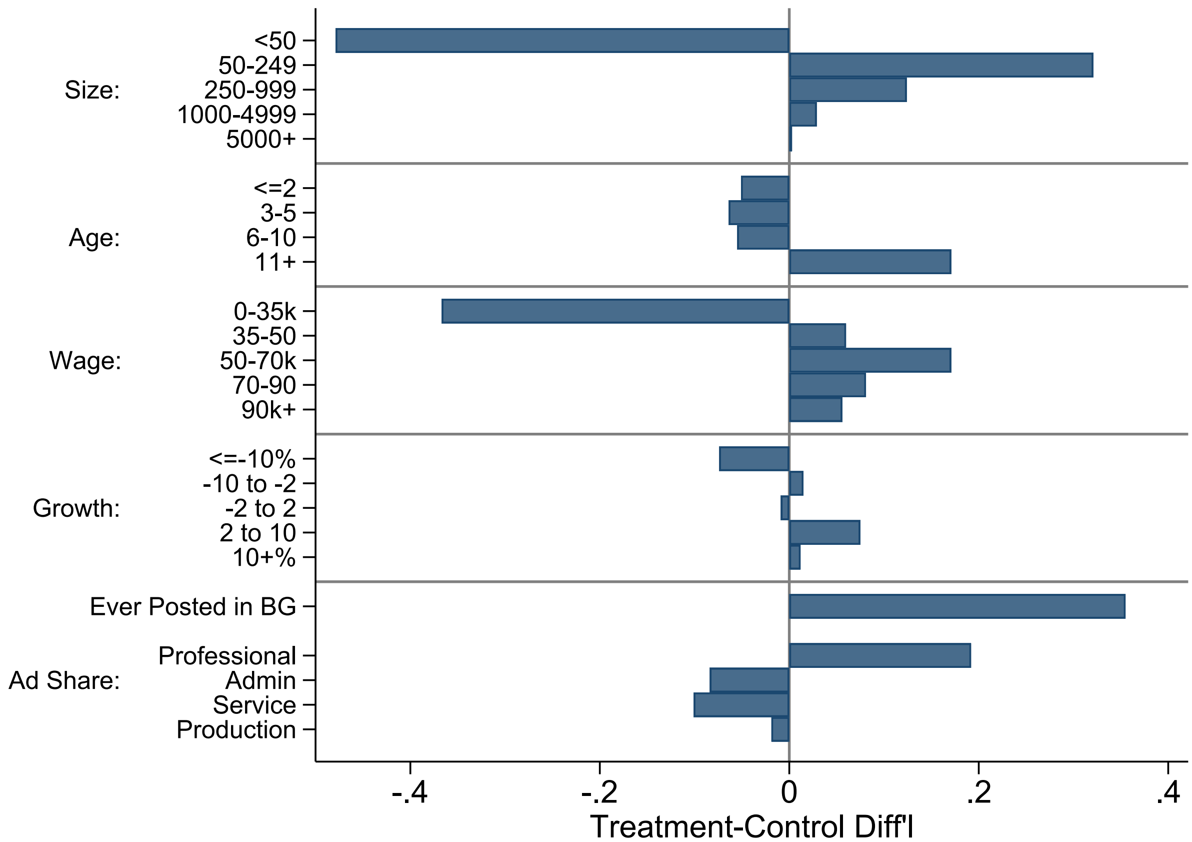
Turning to firm age, we see that grant recipient establishments are older (by about 3 years on average), with substantial overrepresentation (17 ppts) among the oldest bin (11+ years). There are fewer treated firms that were brand new upon grant receipt than in the control group.

We next look at wages. We only observe total wage bill in the data, so we define wages here as the total wage bill in a given quarter divided by the number of employees on the first day of the quarter. For many reasons, this payroll per worker metric is not equivalent to average wages. As such, we report a coarse grouping in figure 1, splitting firms into quintiles. Grant recipients are substantially less likely to be found in the lowest wage bin (37 ppts) and much more likely to be found in the middle and high wage categories. On average, grant firms' payroll per worker is about \$20K more than the control group (appendix table E.1).

For growth rate, which we define as the percent change in employment between t-2 and t-1, grant recipients are less likely than control firms to be shrinking by more than 10 percent of their employment and more likely to be growing at a moderate rate (i.e., 2 to 10 percent).

[10] Most of the time, the restriction on non-zero employment helps us focus on firms that are in operation during the grant time window. However, due to data noise issues, some firms are observed with zero employment for random years in the middle of their spell of operation. In analyses below, we drop these years. Also, from the initial set of firms, we exclude those with no more than 1 employee for average monthly employment, as this group of firms is highly unusual but represents a non-trivial fraction of establishments.

Figure 1: Treatment-Control Differential in Distribution of Establishment Characteristics



Notes: We plot the difference between the fraction of treatment establishments in a bin and the fraction of control establishments. We do this for characteristics in the year prior to grant receipt (or placebo year). Wages are quintiles of total wage bill per worker. Growth rate is measured as the t-2 to t-1 change in employment. The ad share across occupations in BG restricts to firms that post ads in t-1. See footnote 11 for definitions of the broad occupation categories.

Grant firms are also much more likely to be recruiting online in the year prior to grant receipt – 51% can be matched to BG at any point, compared to only 13% in the control group (in their placebo year). Consistent with their faster growth, grant recipients post substantially more ads than the control group, even conditional on postings any ads – from appendix table E.1 they average 24 in the year prior to grant receipt, compared to 4. Panel B of the same table also shows that, within the BG sample, differences in establishment characteristics across grant and non-grant recipients are similar to those in the full sample.

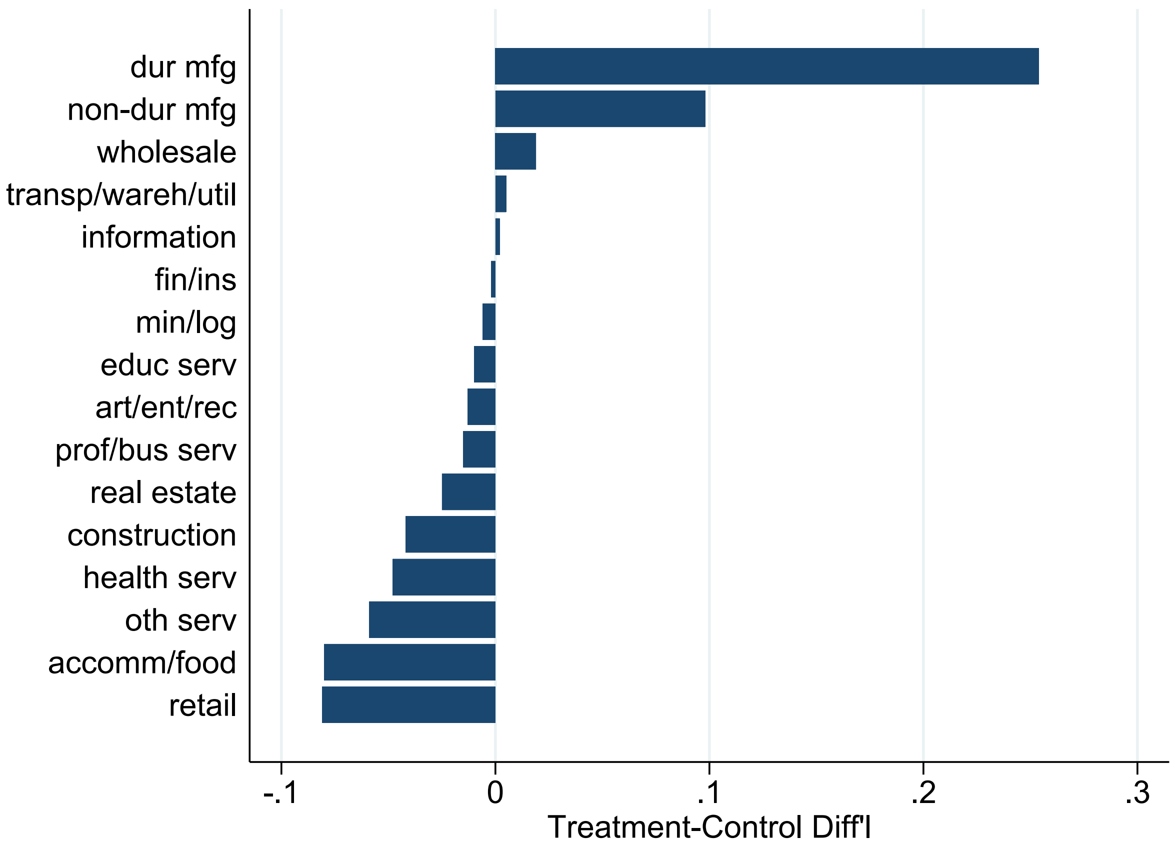
The ad characteristics provide a sense of the skill level of desired workers for grant versus control firms. First, BG codes whether employers specify an education requirement or a requirement for experience in the field, and, if so, how many years. Within the BG sample, grant firms specify skill requirements at higher rates: they specify an education (experience) requirement in 70% (60%) of ads, compared to 54% (47%) in the control group. Treated firms are also more likely to require a college degree (a subset of all education requirements).

Consistent with their higher skill requirements, treated firms hire in more skilled occupations. We use a coarse grouping of four broad occupation categories: Professional occupations are high-skilled white collar positions; administrative occupations are routine white collar positions (such as sales and office support); service occupations are low-skilled positions like servers and personal care jobs; production occupations are blue collar jobs.[11] We also use these categories below when measuring firms' recruiting behavior. Among the firms who use online hiring services, treated firms have a greater proportion of job ads asking for professional skills (65% relative to 46%) and are less likely to be searching for skills relevant to administrative/sales, service, or production occupations.

Finally, grant recipients are concentrated in different industries than non-recipients. From figure 2, we find grant recipients are more likely to be in manufacturing industries, whereas non-grant recipients are more likely to be in services (such as accommodation and food, professional and business services, and retail trade). Though not shown, the differences across grant and non-grant groups shown in Figure 1 persist even after controlling for the differences in industries shown in Figure 2.

[11] This grouping maps SOC occupation codes into four mutually exclusive and exhaustive SOC occupation code groups: Professional includes SOC 11-19, 23, 27, 29; Administrative/Sales is 21, 25, 31, 41 (excluding 412), 43; Low-skill Service is 35-39, 412; and Blue Collar is the remainder (33, 45-53).

Figure 2: Industry Distribution across Grant and Non-Grant Recipients



Notes: We plot the difference across treatment and control group in the share of establishments in two-digit NAICS sectors.

4 Grant recipiency and labor market characteristics

Grant distribution across labor markets reflects the joint outcome of firm applications and state allocation decisions. States cite many different priorities for their programs including a desire to reach small firms, under-served locations, and places struggling to keep up with out-of-state competition. States also express a desire to provide workers with industry-recognized skills that employers may not be able to find or fund on their own. Economic theory suggests that more competitive labor markets, where poaching risk is greatest, will have a greater need for this type of government intervention. However, as discussed in section 2, not all states allocate grants through competitive or strategic processes. In these cases, the distribution of grant recipients will be driven primarily by which firms choose to apply which may or may not align with the firms that represent the greatest social return to training grants. For instance, we have already seen that grants disproportionately serve larger, faster-growing, and older firms, despite the fact that several states express a preference for small businesses. Given the high administrative barriers, the relative strength of public priorities and firm needs in determining the distribution of grants is therefore an empirical question, which we tackle next.

4.1 Methods

In equation 1, we relate the likelihood that a labor market receives a grant in a given year, t, to a vector of market-level measures of economic activity motivated by our discussion above. The regressors are defined using a benchmark time period (2010-12) and we explore grant allocations in subsequent years (2013-19).[12] We limit this allocation analysis to state-years in which we observe grants. Markets are defined by commuting zone, c, and skill, j, which we classify by either occupation or industry. Our baseline specification controls for state-by-year fixed effects ($\theta_{s(c),t}$), to examine the relationship between economic activity and grant allocation within the specific grant cycle, and skill (θ_j) fixed effects. We cluster standard errors by state to account for persistent state-level correlations in grant allocation decisions.

$$Grant_{cjt} = \beta_0 + f(concentration_{cj})\beta_1 + \mathbf{X}_{cj}\beta_2 + \beta_3 NewMarket_{cj} + \theta_{s(c),t} + \theta_j + \varepsilon_{cjt} \qquad (1)$$

We add measures of economic activity that align with the motivations discussed in section 2. To understand poaching risk, we follow the previous literature in defining measures of market-level

[12] We choose these years because 2010 is the earliest year for which we have consecutive coverage of the Burning Glass data, which we will use to measure market concentration.

concentration of vacancy postings using Burning Glass (Azar et al., 2020). Our preferred measure of labor market concentration is a Herfindahl–Hirschman Index (HHI) for job vacancies as in equation 2, calculated using the full universe of job ads posted in BG from 2010 to 2012.

$$HHI_{cj} = \sum_{k} \left(\frac{(\#\text{ of ads})_{kcj}}{(\#\text{ of ads})_{cj}} \right)^2 \tag{2}$$

The HHI in market cj is the sum of squared ad shares across all firms, k, posting in the market. A higher value on this index indicates that a greater proportion of job vacancies in a given market are from a smaller number of firms (i.e., a less competitive market). This vacancy-based market concentration measure is particularly salient for thinking about poaching risk.

Our preferred labor market measure is defined at the two-digit-industry-commuting zone level. The goal in defining these markets is to identify a specific skill that an employer might wish its employees to have and better understand the labor market prospects for that skill. Because grants are allocated to firms, not occupations, and the QCEW provides information on industry of the firm but not occupational mix, our primary measure is based on industry. However, because occupation is a more natural analog to the skills that define a worker's outside option, we also use the ad distribution of the establishment to allocate grants to the modal occupation among the firm's job postings. We show that our results are quite consistent across this alternative market definition (3-digit occupation-by-CZ) as well as alternative measures of poaching risk.

We also explore the relationship between grant receipt and a range of other market characteristics ($\mathbf{X}_{cj}$) such as size and average wage, based on American Community Survey (ACS) data, as well as growth in these measures.[13] We also use the ACS to measure CZ-wide unemployment rates. To better understand economic activity in neighboring markets, we also calculate "leave-out" versions of these measures at the state-industry or state-occupation level (omitting the focal CZ-skill market from that calculation) and the Census division-skill (omitting the focal state from that calculation). We are therefore primarily capturing the relationship between grant allocation and persistent, historical economic health, rather than year-to-year fluctuations.

We restrict measures of economic activity to CZ-skill pairings which have at least 50 ads posted in the base period (2010-2012), ensuring these markets have enough active employers to reasonably

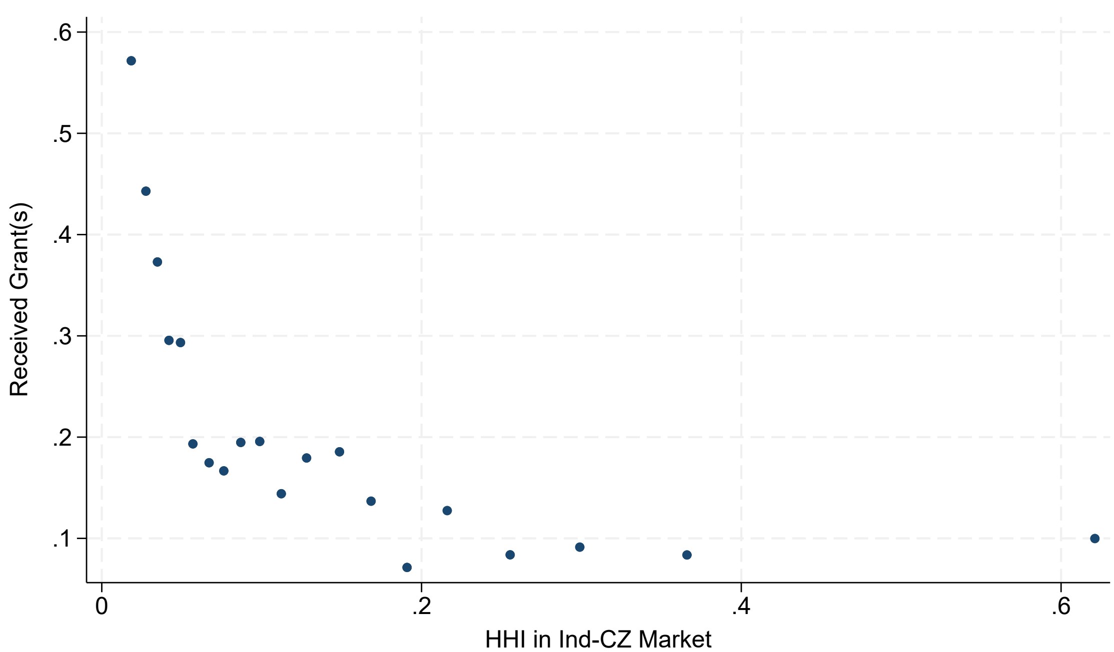
[13]We use ACS 2010-2012 waves (Ruggles et al., 2022), combined with crosswalks between public-use micro areas from Dorn (2009). We calculate the average number of employed people age 25 to 64 working in each market per year and the average wage per hour for workers in this age range in each market. For growth, we use the change in these variables between 2010 and 2012. Average wage is defined as the total earnings from wages and salary, divided by the reported usual hours worked per week times weeks worked in the past year. We top- and bottom- code wages, omitting individuals whose reported salary and hours worker indicate an hourly wages less than 5 or more than 150 dollars per hour.

measure recruiting. However, we would like to explore whether grants are allocated to markets with little past activity, consistent with a place-based incentive policy designed to draw in large firms from out of state. We therefore include these markets in the regression with the indicator $NewMarket_{cj}$ and imputed values of zero for all measures of baseline economic activity.

4.2 Results

Figure 3 provides a bin scatter of the likelihood that an industry-CZ market receives at least one grant on the y-axis and the market-level HHI on the x-axis. We see that lower HHI (i.e., more competitive) markets are more likely to receive grants. The relationship is non-linear, quite steep in the beginning and flattening for higher levels of concentration. This pattern motivates the quadratic functional form we will use in our regression analyses.

Figure 3: Training Grants and Market Concentration



Notes: We divide markets (CZ-by-two-digit industry pairs) into 20 equally-sized bins based on the HHI of job vacancies posted from 2010-12 (see equation 2). For each bin, we then plot the average HHI and the share of markets that received any grants between 2013 and 2019.

Appendix figure E.4 shows a similar relationship with concentration for the total number of grants or grant dollars (including zeros) allocated to a market, so for remaining analyses we proceed with the indicator for whether the market ever received a grant. This simple negative relationship is suggestive of the theoretical mechanism described above where markets with greater poaching risk face an underprovision of general skills. However, less concentrated markets may receive more grants for reasons other than market concentration. For instance, larger markets may be less concentrated and would also mechanically receive more grants even if grants were randomly allocated across firms.

Our multivariate analysis, reported in Table 1, controls for size and many other possible drivers of grant allocation. Column 1 shows that the negative relationship between HHI and grant receipt holds after controlling for pre-period, market-level employment and wages, CZ-year unemployment, and two sets of fixed effects. Industry fixed effects control for the possibility that certain industries are in favor with state grant agencies and also happen to be more or less concentrated. State-by-year controls place our comparison within a grant cycle. To provide some context for the magnitude of the relationship, the mean and standard deviation of the HHI are 0.155 and 0.150, respectively, and the average market receives a grant with 20.5% likelihood, meaning that a one standard deviation increase in HHI from the mean is associated with a 7.6 ppt (37%) decrease in the likelihood that a market receives a grant.

We also see evidence that grants are more likely to go to stronger labor markets, in terms of number of workers, average wages, and the unemployment rate. A market with 1,000 more workers is associated with a 4.4 ppt (21%) higher likelihood that the market received at least one grant; CZ's with a 1 ppt higher unemployment rate are slightly less likely to receive a grant (by about 0.6 ppt).

Column 2 of Table 1 illustrates robustness to the inclusion of industry-by-year and industry-by-state fixed effects. The former helps if there are any skills that are rising in popularity that happen to have more or less concentrated markets on average, for instance, states may increasingly value programming skills and jobs in the technology industry may tend to be located in concentrated markets. Industry-by-state effects help control for the possibility that preferences for a given industry are clustered in particular states that also tend to have more or less concentrated markets, for instance, California may preference programming skills and Silicon Valley may be an especially dispersed market. Reassuringly, the negative relationship between HHI and grant allocation holds within these controls.

Columns 3 and 4 test whether grant receipt is associated with the economic characteristics of the surrounding region. We control for own-market employment and wage growth, as well as neighboring market employment, wage level, and growth rates. Column 3 defines the neighboring market as the population-weighted average of all other industry-CZ's in the state, the "leave-out

Table 1: Training firms and market characteristics: 2-digit Industry-by-CZ

Dependent Variable	Any Grants Received (mean = 0.207)			
	(1)	(2)	(3)	(4)
HHI	-0.945***	-0.976***	-0.925***	-0.958***
	(0.098)	(0.104)	(0.105)	(0.106)
HHI^2	0.952***	0.963***	0.911***	0.963***
	(0.111)	(0.118)	(0.112)	(0.119)
CZ unemp rate	-0.571***	-0.618***	-0.592***	-0.584***
	(0.171)	(0.209)	(0.173)	(0.178)
New Market	-0.120	-0.128	-0.192***	-0.124*
	(0.072)	(0.078)	(0.045)	(0.071)
Employment (1,000s)	0.045***	0.045***	0.045***	0.045***
	(0.010)	(0.010)	(0.010)	(0.010)
Wage ($100s)	0.620**	0.625*	0.816	0.666**
	(0.289)	(0.318)	(0.517)	(0.310)
Emp growth			-0.004	-0.002
			(0.024)	(0.026)
Wage growth			-0.001	0.017
			(0.064)	(0.056)
Leave-out State Emp			-21.415*	
			(10.929)	
Leave-out State Wage			-0.461	
			(0.465)	
Leave-out State Emp Growth			0.169	
			(0.097)	
Leave-out State Wage Growth			0.246	
			(0.191)	
Leave-out Region Emp				-2.768
				(2.878)
Leave-out Region Wage				-0.007
				(0.037)
Leave-out Region Emp Growth				0.152
				(0.106)
Leave-out Region Wage Growth				0.050
				(0.126)
Observations	20,031	20,030	20,031	20,031
R-squared	0.270	0.310	0.272	0.271

Standard errors in parentheses clustered by state.
*** p<0.01, ** p<0.05, * p<0.1

Notes: Observations are 2-digit industry-by-CZ-by-year. HHI, Employment, and Wages are industry-by-CZ averages from 2010-12. Emp and wage growth are the rate of change in 2012 from 2010 for the industry-by-CZ. The CZ unemployment rate varies by year. The Leave-out State and Region variables are also at the industry-by-geography level, averages over 2010-12 or the rate of change over that period and leave out the focal CZ or state, respectively. Regression observations restricted to 2013-2019. Covariates are defined for the 2,255 markets that posted at least 50 ads in the baseline 2010-12 period and have coverage in the ACS, and other markets are considered "New".

state" market, while column 4 uses the population-weighted average of all other state-industries in the census division, the "leave-out region" market. We revert to the original sets of fixed effects since these neighboring market variables have little or no variation within industry-year or industry-state. We find little evidence that characteristics of the industry within the state as a whole impact the empirical grant distribution, nor do neighboring states. The signs on the coefficients point towards grants in markets whose neighbors in the state are smaller and lower paying, with potentially faster employment growth. Though, these patterns are noisy and for the most part not statistically significant.

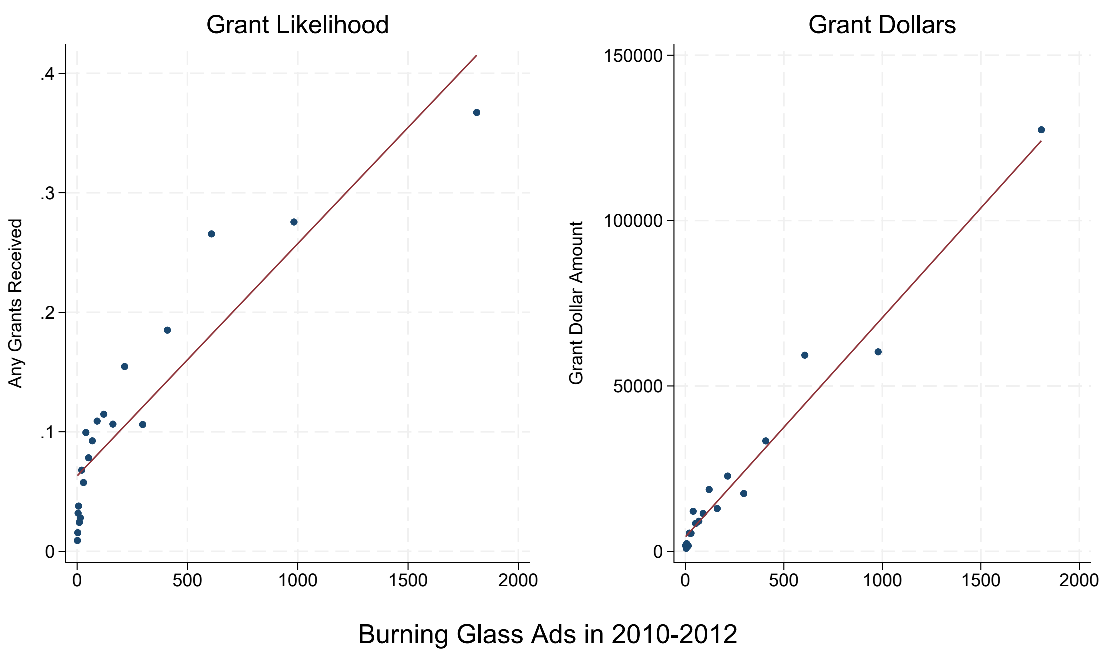
Finally, we see that "new" markets – those that rarely show up in the vacancy data – are consistently less likely to receive grants in the multivariate analysis. Figure 4 provides bin scatters of number of ads posted in the baseline period (2010-12) including zeros and the likelihood that the market receives any grants (left) or average grant size in the market (right, including zeros) in the analysis period (2013-2019). These plots find the consistent pattern that markets with more posted ads are more likely to receive grants. In other words, grants are observed across markets in proportion to their baseline economic activity. We do not see evidence indicating other types of strategic choices by states, for instance disproportionately targeting new markets which might be the case if grants were frequently designed to entice large firms to relocate to new markets.

Appendix figure E.5 and table E.2 show the results hold when defining markets by CZ and occupation rather than industry. While it has a less straightforward mapping between firm-level grants and we cannot use our match to the QCEW to define market, it is probably a better measure of outside options for a particular skill set. Reinforcing the findings at the industry level, grants are significantly more likely to be allocated to bigger and more competitive markets.

Finally, we explore other measures of market competition, rather than the HHI of job vacancies. Results reported in appendix table E.3 show that the result that grants are more common in more competitive markets holds up when considering an alternative functional form for concentration: the share of ads posted to the three largest firms in the market (defined as either industry- or occupation-location). When markets are defined by industry, we can also explore the concentration of employment shares using County Business Patterns. In both cases, more concentration of ads/employment is associated with lower grant receipt. Lastly, we show robustness to another measure of market competition: labor market tightness. Tighter markets should have more poaching and indeed we find that they are also more likely to receive grants.[14]

[14]We define tightness at the CZ-industry level as the average number of vacancies posted in BG between 2010-2012 divided by the average number of unemployed workers who previously worked in the industry as measured in the ACS over the same period. Another desirable measure would be the rate of job-to-job transitions in the labor market,

Figure 4: Training Grants and # Ads Posted in the Market



Notes: We divide markets (CZ-by-two-digit industry pairs) into 20 equally-sized bins based on the number of ads posted in the market from 2010-12. We then plot the share of markets that receive at least one grant (left) or the average dollar amount per grant (right, including zeros) on the average number of ads in the baseline period. We restrict to markets that post no more than the 90th percentile (1277 ads) for visual clarity for the smaller markets, though the slope of the line is fairly similar when we include them.

4.3 Discussion

In summary, we find a strong and robust negative relationship between market concentration and grant allocation. This finding is consistent with the hypothesis that firms are reluctant to pay to train workers when they are competing heavily for talent within the market. In these instances, public sector subsidies can help solve market failures and, in fact, we find training grants are much more likely to show up in these markets. It is not clear from these results whether this pattern is driven by state governments targeting these labor markets or firms in these markets applying at higher rates. To better understand these trade-offs, we categorize states based on whether the grant allocation process seems to be competitive (i.e., a strategic evaluation process that results in only some applicants receiving grants based on public priorities) or firm-led (e.g., first-come, first serve) and look at whether the relationship between HHI and grant receipt varies by allocation method. For the latter, allocations will be driven almost completely by firm application decisions, while, in the competitive case, allocations will be driven by the combination of firm application decisions and state allocations. Appendix Figure E.6 shows a bin scatter of the likelihood of grant receipt against HHI separately by state-level allocation method. We see that both types of states have a similar negative relationship between concentration and grant receipt. This similarity suggests a strong role for firm application decisions in driving the empirical correlation.

We also see that grants are allocated to bigger, well-established, higher paying markets, with lower unemployment rates. Grants are not allocated to new markets or markets with growth capacity (i.e., small and fast growing). If anything, grants are instead allocated to markets whose neighbors exhibit growth capacity. These patterns would seem to be at odds with place-based development policies that may prioritize markets that are lagging their neighbors or typically prioritize small or growing markets. In section 3, we also saw that grants are allocated to older, larger, and faster growing firms. The fact that grants go to more established firms and markets could be evidence of regulatory capture, though we do not see that grants are more likely to go to industry leaders or firms with very high market shares themselves. Furthermore, if place-based policies targeted large firms that had greater regulatory capture, we might have expected the allocation to go towards more concentrated markets overall.

As mentioned in section 2, firms face both administrative costs when applying for these grants as well as financial costs in the form of matching funds and guaranteed wage increases for trainees. Hence, only some firms will find the program worthwhile to participate in. Our finding on concentration is consistent with the self selection driven by firms whose social value of training is larger than its private value due to poaching risk.

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but it is unfortunately not possible to measure transition rates at these levels of granularity.

5 Outcomes of Grant Recipients

Having established that grants tend to concentrate in more competitive labor markets, we next turn to the question of whether individual establishments change their employment and hiring behavior in response to receiving a grant.

5.1 Methods

We estimate a series of event study models, leveraging two-way fixed effects to compare the firm-level outcomes for grant recipients to the trajectory for non-recipients. Equation 3 specifies a regression of outcomes for firm i in year t on an indicator for whether t is τ periods before or after the grant year of an establishment, T, defined as the first year we observe the firm receiving any grant. We again cluster standard errors by state, the level at which treatment is determined. Because we have assigned placebo training years to the control group, we can also control for placebo event time (i.e., main effects in event time), which can help to address problematic control comparisons that may arise in some specifications with staggered adoption of treatment (Sun and Abraham, 2021; Goodman-Bacon, 2021). The event time indicators of interest are all interacted with the ever treated indicator – $\mathbb{1}(grant_i)$.

$$y_{it} = \beta + \sum_{\tau \neq -1} \beta_{\tau}^{grant} \mathbb{1}(\mathrm{t} = \mathrm{T} + \tau) * \mathbb{1}(grant_i) + \sum_{\tau \neq -1} \beta_{\tau} \mathbb{1}(\mathrm{t} = \mathrm{T} + \tau) + \theta_i + \theta_t + \varepsilon_{it} \tag{3}$$

We restrict attention to grants received between 2010 and 2019 and restrict the regression sample to a window surrounding grant receipt (or placebo receipt) of at most plus or minus 5 years. For outcomes measured in the QCEW, we begin the sample as early as 2005 – to observe a full five years pre-treatment for even the earliest treated cohorts – and stop our analysis in 2022 due to data availability. BG data are only available from 2010-2022 so the earliest treated cohorts are not observed in the pre-period. Our baseline sample is therefore an imbalanced panel.[15] We explore a wide range of outcome variables to better understand patterns in employment, wages, and vacancies. These include log employment and wage bill per worker as measured in the QCEW, the number of vacancies posted in BG, and the distribution of vacancies across occupation groups and skill requirements.

Our identification strategy relies on the standard parallel trend assumption of a two-way fixed effects model: in the absence of the grant, the treated establishment's employment and vacancies would

[15] Balanced panel estimates, with a much reduced sample size, are generally qualitatively similar but noisier.

have followed the average trend to other establishments. This is, of course, a strong assumption and likely to be violated for at least some outcomes of interest in our sample. For example, Figure 1 illustrates that grant recipient firms are growing at a faster rate prior to receiving a grant.

To address these asymmetries, our preferred specification uses a nearest neighbor matching design to find a control group that has similar trends to the treated group on a key set of characteristics. Specifically, we identify a single nearest-neighbor (Rosenbaum and Rubin, 1983; Abadie and Imbens, 2006) in the same industry as each treated firm that minimizes the Euclidean distance between the treated and the control firms on 1) five lags of log employment leading up to treatment (or placebo treatment) and 2) five lags of indicators for whether the firm posted in BG. To avoid capturing spillover effects within our control sample, we exclude all untreated firms in industry-county markets where at least 20% of workers were at firms that received a training grant in any year within two years of the (placebo) treatment. Following Abadie and Spiess (2022), we match without replacement, which allows us to construct valid analytic confidence intervals in the later event study regressions by clustering on match pair.

This matching approach leverages the richness of our data – the fact that we have the near-universe of businesses in the U.S. – to flexibly control for baseline characteristics that might drive differential trends. Appendix Section C describes the matching algorithm in more detail, including placebo tests to validate our specification. Table E.1 provides a comparison of treated and control firms in the matched sample (columns 3 and 4) to treated and control firms in the full sample (columns 1 and 2).[16] While the full set of non-grant firms is smaller, lower paying, and younger than treated firms, the matched sample is much closer on these dimensions. By design, the matched sample is also quite a bit closer on the propensity to post vacancies in BG, which helps not only conceptually – since we compare firms with similar hiring needs in the pre-period – but also with later analysis on the composition of postings that must restrict to firms that post ads in BG. The distribution of ads in BG are not targeted, but the matched control group does better on some of these, for instance, education requirements and the occupation distribution, compared to the full sample.

We control for establishment fixed effects (θ_i) to absorb any time-invariant differences across program and non-program participants and calendar year fixed effects (θ_t) to absorb any common macroeconomic shocks.[17] The full sample control group and the matched control group each have conceptual advantages and disadvantages. Differences across grant and non-grant firms leading up to the grant application are interesting in their own right. The full sample control comparison helps

[16] As detailed in the appendix, we exclude from the matching analysis treated firms that do not achieve a sufficiently close match among the control firms – 15% of treated firms.

[17] In robustness checks, we add controls for industry-year to capture common sectoral shocks. When we include industry-year fixed effects in additional specifications reported in Appendix Section E, results are not substantively different though more noisily estimated.

us better understand the nature of the skills gap problem and the ways in which the training firms have attempted to solve it, prior to training. Our goal is to describe these differences, as well as those post-training, compared to firms operating in similar markets and time periods. In contrast, the matched control helps us rule out alternative stories in which the post-grant firm outcomes are driven by differences in the types of firms which apply for training grants. For instance, if patterns reflect that firms tend to apply for training at a certain phase of their life-cycle, those should be picked up by our match on trends in size and recruiting behavior. The matched sample provides the cleanest estimate of the added impact of these training grants apart from selection effects.

5.2 Results

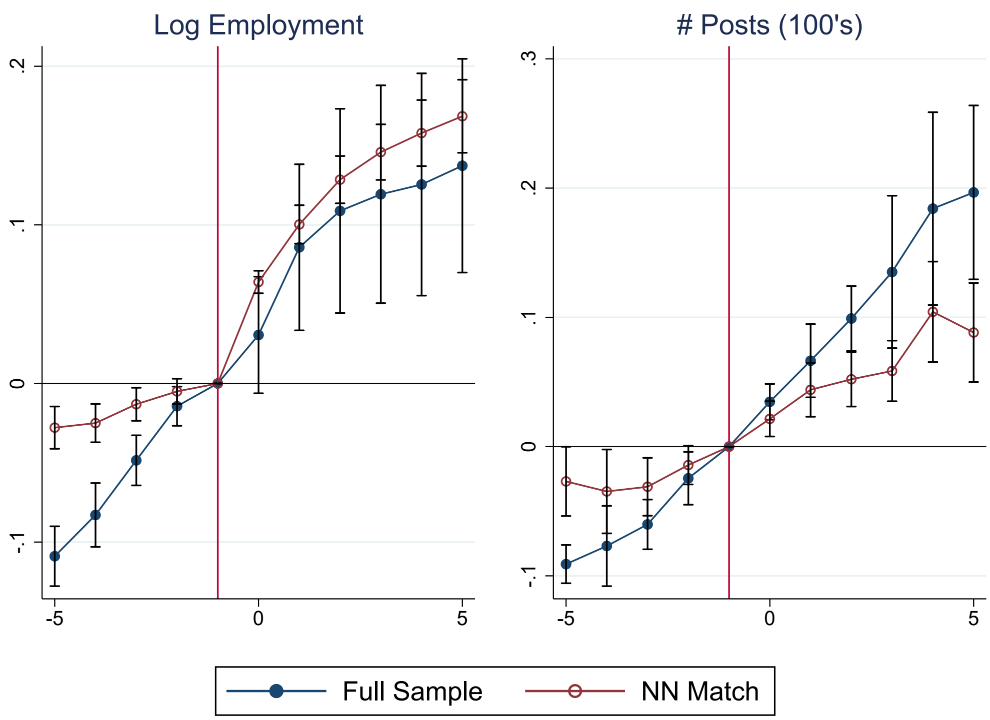
Quantity of Employment and Vacancies

We begin with log employment as measured in the QCEW. The impact of these training grants on log employment is theoretically ambiguous, but likely to be positive. Effective training should increase the marginal productivity of targeted workers at the firm. This productivity shift could increase or decrease employment of these workers depending on the elasticity of demand for their output. However, the firms' willingness to invest in this training suggests a strong demand for those skills and perhaps unmet needs that the grants can resolve. Some states explicitly require firms to hire additional workers in exchange for receiving a grant. Finally, if there are complementarities across types of workers, this increase in productivity for the trained workers should increase demand for other types of workers at the firm.

Figure 5 plots event study coefficients and 90% confidence intervals for the full sample (navy dots) and for the matched sample (maroon hollow dots). Appendix Table E.4 reports the coefficients and standard errors for these specifications, as well as specifications that add sector-year fixed effects. In keeping with the descriptive statistics, the full sample specification shows grant-receiving firms growing on a different trajectory prior to grant receipt. By construction, this gap closes when comparing to the matched sample.[18] Firms grow even more rapidly after receiving their first training grant. By 5 years after training receipt, treated firms have grown roughly 15%, relative to their counterfactual trajectory.

[18] The coefficients in periods t-2 to t-5 are significantly different than the coefficient normalized to zero in period t-1, but they are not significantly different from each other and they are tiny in magnitude, relative to the impacts in the post-period.

Figure 5: Firms Grow After Training: Employment and Vacancies Event Studies



Notes: This figure reports coefficients estimated using equation 3, our event study regression, for log employment measured in QCEW (left) and number of postings in 100's measured in BG (right) separately for the full sample and the matched sample. We limit to firm-year observations with non-zero employment and the right panel further limits to years of BG availability. Within these conditions, we impute a value of zero postings if the firm did not show up in BG in that year. We control for establishment fixed effects, year fixed effects, and event dummy main effects. We plot coefficients on event dummies interacted with treatment. We also report the 90% confidence interval. For the full sample analysis, standard errors are clustered at the state level, while for the NN match we cluster at the match pair level.

The mean grant receiving firm in the NN match sample had 144 workers in the year before grant receipt (see Appendix Table E.1). The added 15% growth represents an additional 22 workers over five years. When firms specify a number of new hires to be trained (40% of the time), that averages to 58 additional workers. However, those new hires would come at the beginning of the grant period, while the bulk of excess growth in Figure 5 accrues only over time. Grant amounts average roughly $90,000, though factoring in that many firms receive multiple grants, the total grant dollars received by the average firm over 5 years is $150,000. Furthermore, most firms are required to match government fund 50-50, for a rough total investment of $300,000. Still, the growth we document here, especially outside the initial grant period, is quite large. Were the 22 additional workers drawn from non-employment, this policy would be an extremely effective use of government funds for generating job growth. However, we have no way of knowing whether these workers were drawn from non-employment or from other jobs.[19] We are unaware of any other study that measures the impact of these types of programs on measured employment, but these large changes are broadly consistent with substantial productivity changes measured by the small literature on the impacts of firm-led worker training (for example, Konings and Vanormelingen (2015) estimate that firm-led training in Belgium makes workers 23% more productive on average).

We next examine the quantity of vacancy postings to better understand whether firms had a stated preference for this growth (as opposed to passive hiring or changes in their separation rates). The second panel of Figure 5 and Appendix Table E.5 report effects of receiving a grant on the annual number of BG posts (in hundreds). For this sample, we restrict to years where BG is available and to firm-year observations with non-zero employment. If a firm meeting this restriction does not post in BG in that year, we impute a value of zero. These effects are consistent with the net changes in employment.

Furthermore, these event studies show that the change occurs gradually, reaching its highest point 5 years out. Almost all grants last for two years or less, with most being completed within a year of receipt. Therefore, while some firms may have increased hiring needs around the time of grant receipt due to a promise to train newly-hired workers, the mechanical effect cannot explain the increases in the later years shown.

Composition of Vacancies

As vacancies and employees increase, we might think the characteristics of the jobs firms are hiring in has also changed. One possible outcome of the grants is that firms no longer need to include skill requirements in their job vacancies. Conversely, once the firm has built a workforce in the desired skill, it might need to hire for tasks that complement the trained workers leading to either an increase or a decrease in skill requirements depending on the type of complementarity. To get a better sense of employment composition, we exploit the rich detail in the BG vacancy data, using job ads as a proxy for how the workforce is changing.

We start by looking at whether firms change which occupations they are hiring in. We examine the proportion of ads across the four occupation groups described above: Professional, Administrative, Low-Skill Service, and Production/Blue Collar. We estimate equation 3 at the establishment-year level for both the full sample and the nearest neighbor sample. To better understand how the distribution of vacancies has changed over time, we weight observations by the number of ads

[19] We conducted some exploratory analyses at the market level and found noisy zero impacts on overall employment following receipt of a grant by at least one firm in a market.

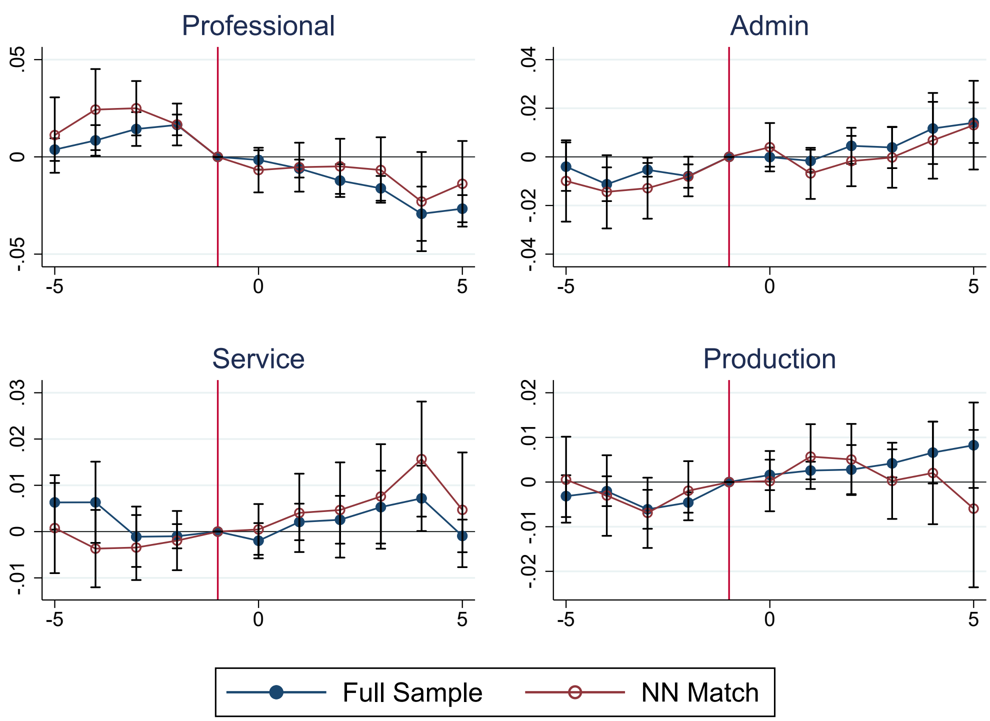
posted. Thus, results can be interpreted as impacts on the average vacancy of a treated firm, rather than the impact on the average treated firm. As such, our analysis restricts to firm-year observations with non-zero BG posts.[20] Figure 6 and Appendix Table E.6 report the results of these regressions.

When firms receive grants, the composition of their ads shifts away from professional occupations. Effects are statistically significant from zero in the full sample (top left panel of figure 6) and are of similar magnitude but more noisily estimated in the matched sample. Four-to-five years post-grant receipt, grant recipients' hiring requests are 2 pp less likely to be in professional occupations relative to their pre-grant baseline of 63%. Consistent with Appendix Table E.1, we see that training firms were slightly more likely than even their matched firms to post in professional occupations in the pre-period. However, vacancy composition (not targeted in our match) does not exhibit the strong pre-trends evident in log employment. These relative decreases in professional posting are distributed across the other occupation categories where point estimates are positive but insignificant.

These changes in occupation composition develop several years after firms receive a grant. The modest compositional changes occur in the context of strong overall growth in log employment and total job posts, so the decline in the *relative* frequency of professional vacancies reflects a less-than-proportional growth in hiring in this space rather than an absolute decline. Interestingly, as we will discuss below, for the subset of firms where we observe training plans, a plurality of these plans target skills for professional workers. Therefore, we find that firms who receive training grants grow faster overall and shift modestly away from hiring in the most commonly targeted occupation and towards all other groups.

[20] For comparability, Appendix Figure E.7 reproduces QCEW results for log employment restricting to this same sample and finds similar magnitudes and patterns. Furthermore, we have explored a range of restrictions on the number and regularity of posting for a firm to ensure that the posting distribution across occupations and skills captures a large fraction of firm activity. Because we weight these regressions by postings, such restrictions make almost no difference.

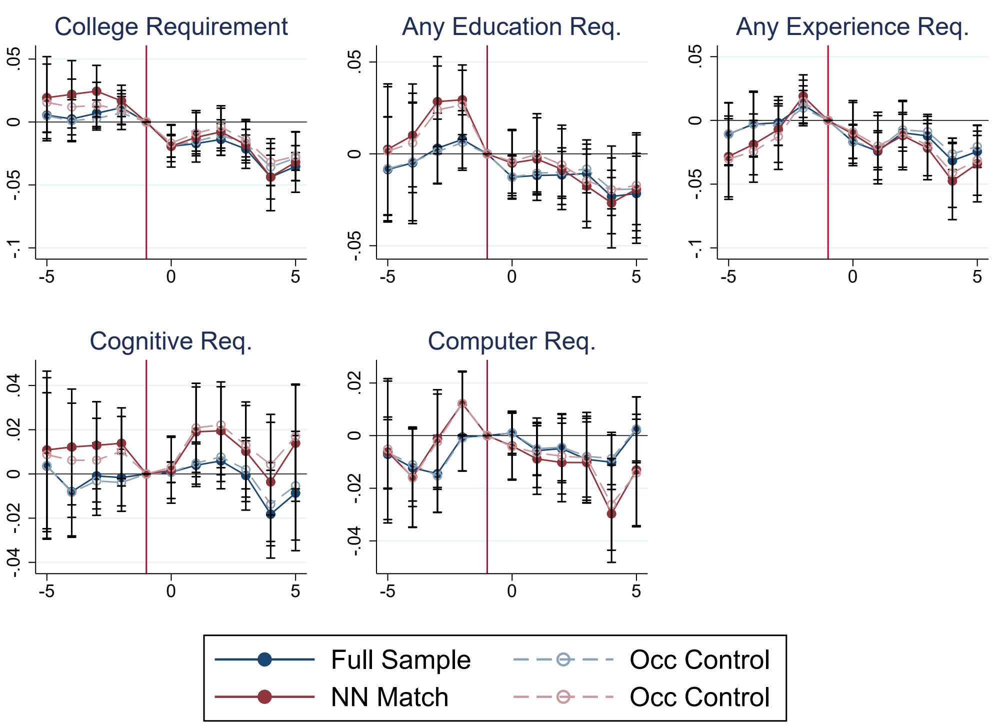
Figure 6: Ad Shares across Broad Occupation Groups (BG)



Notes: See figure 5 for regression specification information. Here we restrict to establishments that had BG postings in the year. Outcomes include the fraction of postings in each of four mutually exclusive and exhaustive occupation groups: professional, routine white collar, Low-skill service, and blue collar (see footnote 11).

Nexy, as shown in Figure 7 and Appendix Table E.6, training firms also modestly reduce skill requirements following the receipt of their first grant. By four years after grant receipt, the share of vacancies requiring a college degree declines by 4.4 percentage points (10%) and the share requiring at least a year of related experience falls by 4.8 percentage points (8%). The share of vacancies including any education requirement follows the same pattern, though the change is not statistically significant. We also leverage two of the most common keyword skills categorized in Deming and Kahn (2018), cognitive and computer skill requirements. While we do not see consistent effects for these outcomes, the NN match shows a decline in computer requirements, commensurate with the other skills. These shifts could be driven by the changes in occupation if professional job postings tended to include stronger skill requirements, however the final two lines in these figures show the patterns persist even after controlling for the occupational composition of ads. These additional specifications suggest that the changes are driven by within-occupation reductions in skill requirements.

Figure 7: Skill Requirements (BG) Event Studies

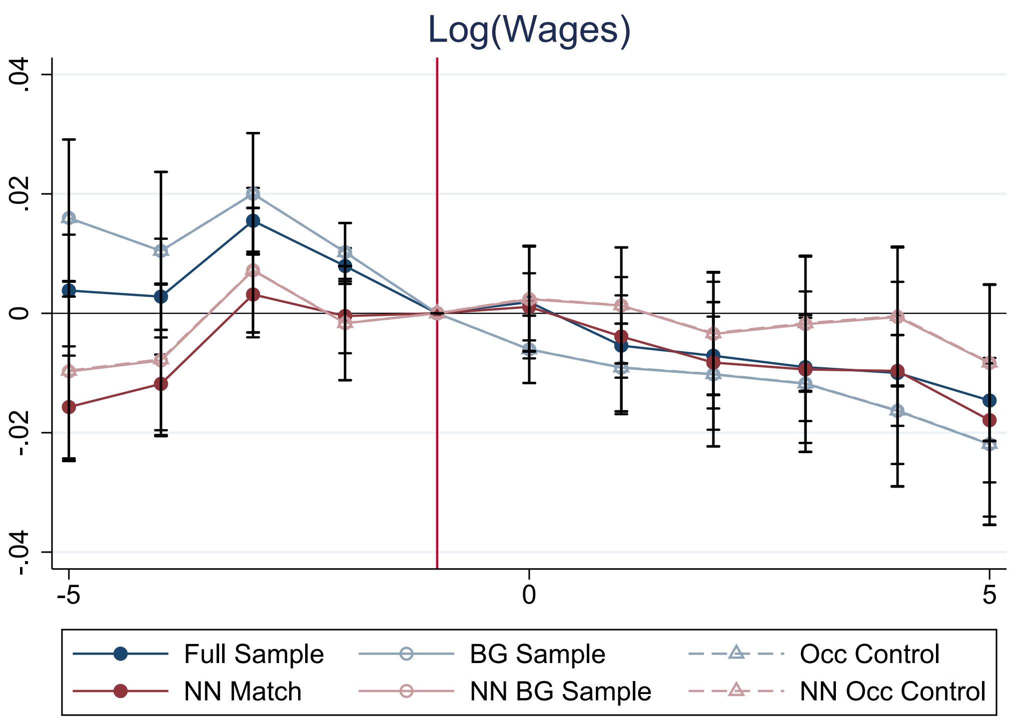


Notes: See figure 5 for regression specification information. Outcomes are the proportion of ads specifying the indicated skill requirement. Cognitive and computer skill indices are taken from Deming and Kahn (2018). Regressions with dotted lines control for the composition of ads across the 4 occupations groups in the year.

Wages

Training grants should raise wages for the targeted workers as their marginal productivity increases. In fact, several states require that firms achieve certain pay raises for the trained workers as a condition of the grant. Several earlier studies (Jones et al., 2012; Konings and Vanormelingen, 2015) have found that firm-led training increases wages for the targeted workers, though growth in wages tends to be substantially smaller than growth in worker productivity. The QCEW contains information on the total wage bill at firms, but we have no way to distinguish the wages of workers targeted for training. As shown in Figure 8 and Appendix Table E.7, we find no significant changes in the log total wage bill (controlling for log employment) following receipt of a training grant.

Figure 8: Log Wage Event Studies



Notes: See figure 5. This figure reports coefficients estimated using equation 3, our event study regression, for the log of total wage bill in the year, controlling for log employment. We run these regressions separately for the full sample (navy) and matched sample (red). In the BG postings sample, we run a specification without (hollow circle) and with (hollow triangle) controls for proportion of ads in the 4 occupation groups in the year.

Several factors may explain these non-effects. First, and most importantly, the total wage bill may exhibit very different patterns than the wages of the targeted workers. We control for log employment in these event studies, so the effects can be roughly interpreted as wages per worker and net out the effects of the overall growth in employment.[21] However, we cannot distinguish the trained workers from the untrained workers, new hires from incumbents, or even the targeted occupations from complementary ones.

As we have already seen, training firms shift away from hiring in the higher-paying professional occupations in the years following training and lower skill requirements. These shifts could all lower the average wage at the firm, drowning out any gains for the incumbent, trained workers. The dotted lines in Figure 8 add controls for the occupational mix of job postings.[22] However, this imperfect control for the composition of the stock of workers at the firm does not substantively change the effects on wages.

5.3 Effects by Market Concentration

As shown in section 4, firms facing more competition in their local labor market are significantly more likely to receive grants to train their workers. Labor market conditions may also affect the impact of these grants. As discussed earlier, firms facing a strong poaching threat may be particularly constrained from funding an optimal level of worker training in the absence of public subsidies, suggesting that the employment and vacancy composition effects of grants could be particularly strong for these firms. Greater competition and poaching risk may also force firms to pass a larger share of any productivity gains from training on to their workers in the form of higher wages.

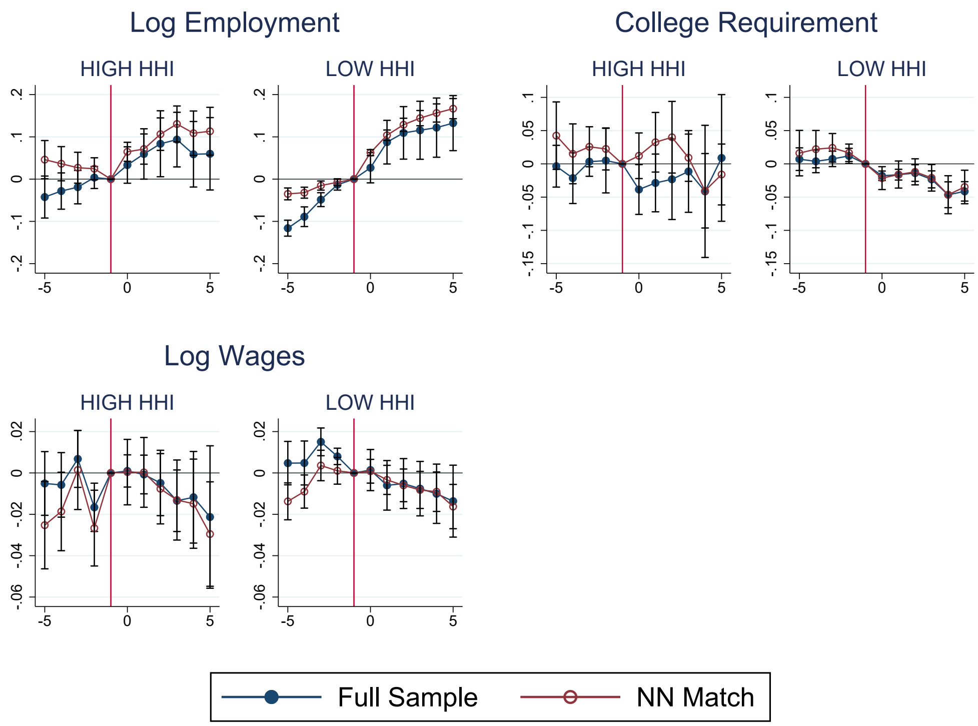
Figure 9 plots the effects of grant receipt on three key outcomes separately for grant-receiving firms in local labor markets with an HHI above or below 0.15. This threshold is a focal point for Department of Justice merger guidelines, and product markets above this point have been considered "moderately concentrated." The strong selection of training grants by market concentration illustrated in Section 4 constrains us from setting the threshold any higher. Even at this relatively weak definition of high concentration, only 13% of firms receiving training grants operate in markets with an $HHI > 0.15$. Because of the low rate of treatment in very concentrated markets, we cannot use a more strict definition of concentration and the estimated effects for the high HHI sample are consistently more noisy.

We find weak evidence in support of the hypothesis that firms facing stronger competition shift

[21] This estimate of log wage per worker is quite noisy. Wages are the total wage bill paid out by the firm in a given quarter (summed over the year), while number of employees is measured at a snapshot date in the month. Thus firms experiencing heavy churn will appear to pay higher wages per worker.

[22] To include these controls, we must restrict the sample to firm-years with vacancy data from BG. The "BG Sample" plots in Figure 8 illustrate that this restriction alone does not change the qualitative story.

Figure 9: Event Studies by HHI



Notes: See figure 5 for regression specification information. High HHI subsample includes grant-receiving firms located in markets with an HHI> 0.15. Control sample does not condition on HHI. HHI calculated by commuting zone-industry, as discussed in section 4.

employment and hiring more in response to grants. The growth in log employment following grant receipt, and particularly the decline in the share of job postings requiring a college degree, appear mainly driven by treated firms in less concentrated markets. Five years post-grant, firms in low HHI markets are about 13% larger than pre-grant whereas firms in high HHI markets are about 6% larger. However, the large standard errors prevent us from rejecting that effects are equal across groups. We find less evidence that greater labor market competition drives firms to increase wages following training, though all the caveats for this imperfect measure of wages remain.

5.4 Discussion

In this section, we have shown that, establishments grow post-training both in terms of the number of employees and the number of vacancies. The composition of jobs also appears to change with training firms shifting away from professional occupations and away from explicit education and other skill requirements. Despite this compositional shift, average wages at the firm are unchanged.

These effects either occur after the typical training window (1-2 years post receipt) or persist well after. We therefore interpret these effects as reflecting the changing nature of production after training is complete, rather than direct effects during the training window.

We can think of a few alternative hypotheses for why these changes occur. First, though we have done our best to find reasonable control groups, training is non-randomly assigned. We cannot rule out that training firms would have seen these outcomes even absent training. That does seem unlikely, however, given the consistency of results and the lack of pre-trends for outcomes in the matched sample. Clearly, there is something changing for training firms around the training period.

Second, training could have a real impact on production. Grants appear to help firms shift to a long-term higher growth trajectory. Downskilling may follow from this growth as expansion tends to happen from the bottom (Engbom et al., 2023). It may be that firms had a bottleneck in the production process and, once resolved, the firm is able to produce at scale and grow. As production needs are resolved, the firm will wish to grow in tasks that are complementary to training, such as front-line sales and service positions – consistent with our results on occupational outcomes. Why do we see skill requirements decline? It could be that even within broad occupation categories, the tasks that complement training do not require as much skill. Alternatively, firms may have realized that training is a viable option for upskilling its workforce. They may back off of requirements they thought they needed for a wide range of positions, in favor of producing those skills in house.

The result is that after training, firms grow disproportionately in areas that have fewer barriers to entry for low-skilled workers. That is an especially interesting result for policy makers, given that at baseline training firms appear to be good places to work (i.e., larger, higher wages, more established).

6 Heterogeneity by Training Targets

For a subset of the states in our sample (California, Kentucky, Massachusetts, New Hampshire, and New Jersey), we have text descriptions of firms' training plans taken from the grant applications. We use these descriptions to identify which broad occupation categories the training is directed towards. Because of the large number of training plans and their varied formats, we use Open AI's Generated Pretrained Transformer (GPT) 3.5, a large language model (LLM), to classify each firm's text into these categories. Through trial and error we found the four coarse occupation categories used above yield more accuracy, measured with a hand-coded test sample, than disaggregated occupations. See appendix D for detail.

Both conceptually and empirically, training plans can map into multiple categories. For instance, the earlier example for the sign company (appendix figure E.3) proposes both production and office skills and specifically mentions cross-training employees to diversify from their current specializations. Our prompt to GPT allows the algorithm to identify multiple occupations associated with each training plan, with a probability weight assigned to each.

Table 2 reports the proportion of training plans that are categorized in each occupational grouping. We report two measures: 'Any Mention' (Col. 1) defined as the proportion of plans in an occupational category allowing for a plan to be in multiple categories and 'Top Mention' (Col. 2-7) defined as the proportion of plans in an occupational category based on the highest classification score. The most common types of training are in the 'professional' skills group (60% any mention and 51% top mention), and in 'production' skills (42% any mention and 29% top mention). These overall averages are somewhat distorted by Massachusetts, which provides nearly half of all the training descriptions and awards disproportionately in professional skills.

Interestingly, while many grants (37%) mention administrative or sales skills these are unlikely to be the primary skill being trained for – admin/sales represents only 17% of top mentions. Some training plans do mention service related skills (usually customer service training) but these are only a tiny fraction of top mentions. As such, we limit the analysis in this section to grants that target professional or production skills and focus on the top mention categorization, though using any mention produces similar results.

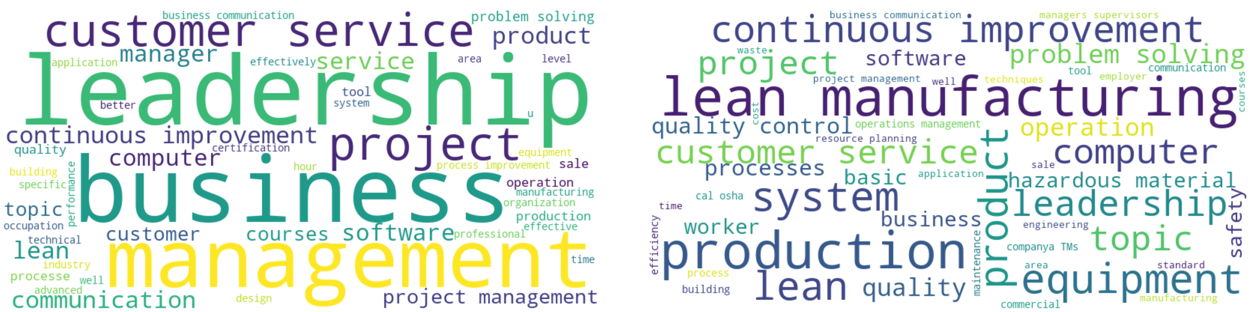
Table 2: Proportion of Training Plans that Include Each Skill Group

	Any Mention	Top Mention					
		All	CA	KY	MA	NH	NJ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Professional	0.660	0.505	0.409	0.143	0.676	0.533	0.281
Administrative/Sales	0.374	0.168	0.318	0.286	0.126	0.133	0.219
Service	0.120	0.036	0.046	0	0.0182	0	0.060
Production	0.416	0.291	0.227	0.571	0.180	0.333	0.440
Number of Grants	1290	1290	22	7	716	15	530

Notes. This table reports the proportion of training plans that were characterized as containing training in the four occupation groupings. For 'Any Mention', each plan can be in multiple categories, so the columns will not add to 1. For 'Top Mention' columns, categories are mutually exclusive and based on the category with the highest classification score. These statistics are calculated using the BG-only match data conducted outside the BLS enclave; see Appendix B for more details.

Figure 10 illustrates the words most commonly included in training descriptions we identify as targeting Professional or Production workers as the top mention. Grants targeting professional workers are strongly focused on leadership and management skills. Grants targeting production workers have a more dispersed focus, with common terms focusing on efficiency and lean manufacturing and some suggesting newer technologies ("computer", "software").

Figure 10: Content of Training Grant Applications by Target Occupation



(a) Professional

(b) Production

Notes: These figures illustrate the most commonly used words in the descriptions of planned training programs that GPT3.5 most associated with Professional or Production occupations. See Appendix D for more details.

We next explore heterogeneity in skill requirements in BG as a function of training targets, both in terms of the baseline skill mix and the impact of training. For the former, we focus on the pre-training period and regress ad shares of skill requirements in firm-year cells on an indicator for whether the firm received a training grant in a sample of all control firms and firms receiving the indicated type of training. As above, we weight by number of ads in the cell. We do not observe what skills untreated firms would have trained in if they received grants. To capture some of the variation in skill needs across firms, we control for industry fixed effects in the pre-period comparisons. For measuring the impact of training, we focus on difference-in-difference estimates, rather than event studies, trading nuance for precision in this small treatment sample.[23]

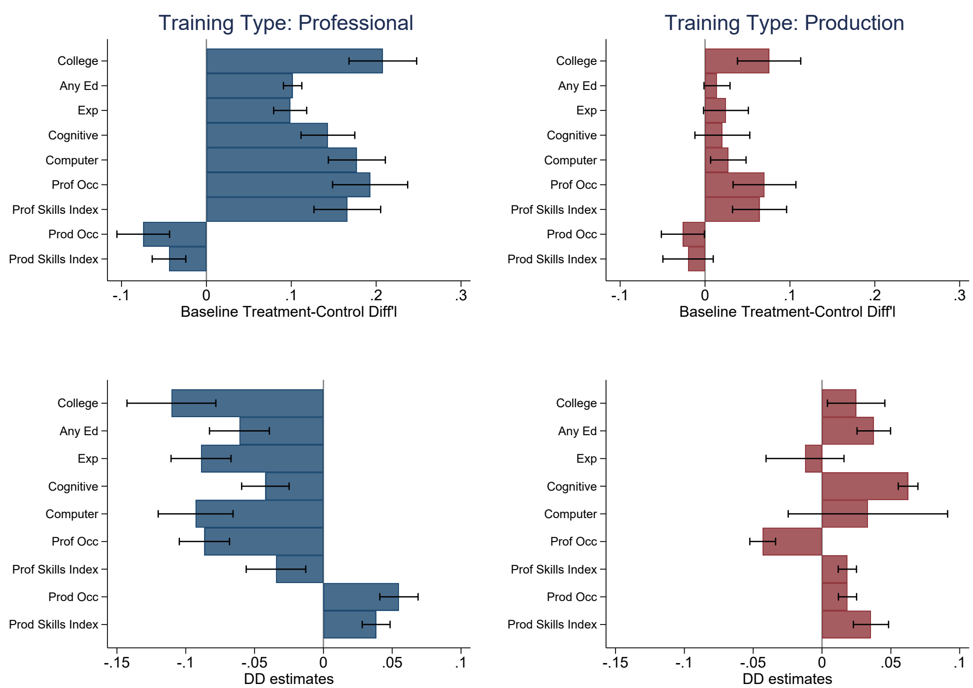
We explore the skill outcomes analyzed above and the share of ads requesting the targeted occupation (either professional or production). In addition, to distill a large number of other skill requirements contained in the data, we aggregate the mix of skills for a given ad into two skill indices, one for professional occupations and another for production. These indices allow us to explore whether firms proposing training in a given occupation tend to request skills commonly used in that occupation across a much broader set of skills than the ones analyzed thus far.[24]

[23]For the difference-in-differences specification, we employ one analogous to equation 3 but do not disaggregate by event time. We include controls for firm and year fixed effects and an "after" dummy that equals 1 if the year is on or after the (placebo) grant year. Our reported treatment effects are the coefficients on the interaction of this "after" dummy and an indicator for being a treated firm. Finally, we add a separate indicator for the year of the grant itself to allow for separate treatment effects in the year the grant phases in.

[24]Specifically, we regress an indicator for whether an ad was in the target occupation on indicators for the presence

Figure 11 plots coefficients on the differential baseline skill mix in the top panel and difference-in-difference estimates in the bottom panel, as well as 90% confidence bands. The treated group is limited to professional skills training in the left panels (blue bars) and production skills training in the right panels (maroon).

Figure 11: Skill Requirements by Target Occupation



Notes: The top panel plots differences in mean ad-level characteristics for firms receiving grants targeting Professional or Production workers vs. all control firms, controlling for industry fixed effects. The bottom panel plots difference-in-difference estimates separately by training type, controlling for firm and year fixed effects as well as a grant year indicator and a main effect for after (using placebo treatment years in the control group). All figures indicate 90% confidence bands when clustering on state. For these additional descriptives on posts, we use a sample created outside the BLS enclave that uses only BG information to match grants to firms. See appendix B for details.

The upper left panel shows that firms targeting professional skills had elevated skill requirements

of various skills. Then, for any given firm year, we aggregate across the skills posted in their ads, using the coefficients from this regression as weights. The index thus allows us to aggregate a wide range of specific skills into a single number, using each skill's predictive power for whether the ad was posted in the target occupation as a weight. We estimate the regression on a random three-quarters of the control group sample and validate the strong predictive power in the one-quarter holdout sample. The regressors include indicators for 10 skill groupings identified in Deming and Kahn, a manual skill category identified in a similar manner, as well as 28 skill clusters used by BG containing a more detailed and broader categorization (e.g., including skills such as "Business" or "IT" but also "Supply Chain" and "religion"). BG culled this list using a clustering algorithm plus some human tweaks related to specific needs of clients.

on many dimensions in the pre-period. They were much more likely to specify a wide range of general skills: college and experience requirements, as well as keyword skills related to the Deming-Kahn cognitive skill index and computer skills. These skills are well-known to be highly related to professional jobs (Autor, 2019). Interestingly, they also had a higher share of postings in professional occupations ("prof occ") compared to control firms and their ads scored higher on the professional skills index. This means that these firms not only had outsized demand for workers in the target occupations, but they also requested skills similar to those being trained for. That pattern alone is reassuring that the grant proposals do contain true information about firms' labor demand needs.

The bottom left panel shows how skill demand evolves post-training. Firms that trained in professional skills see significant declines in demand for education, experience, cognitive skills, and computer skills. They also revert on their demand for professional occupations and skills related to professional jobs. As another aggregate metric, we also find that they decrease the number of skills requested per ad by 10% (not shown). These reversals in skill requirements post training suggest that the training actually does resolve the firm's needs. In addition, they significantly shift their demand towards production-related occupations ("prod occ") and skills. Before training, they had relatively less demand for these occupations, but after training it appears they reverse this behavior.

For firms receiving training in production skills, the patterns are different. The upper right panel of figure 11 shows that they did not disproportionately demand production occupations or production related skills prior to receiving a training grant. If anything, they require more general skills (such as college and computer skills) and demand more jobs in professional occupations. Post-training, they continue their need for these general skills with significant increases in education and cognitive requirements and a noisy but large positive for computer. They also increasingly demand professional skills, though not professional occupations. Instead, they shift more towards production occupations and production skills. When we probed further (not shown), we found that these increased general and professional skill requirements are actually more pronounced within ads of production workers, rather than ads for occupations outside the targeted area. Additionally, these firms increase the overall number of skills they list per ad. Interestingly, the firms that target production skills increase demand for production occupations post-training but also elevate skill requirements for these positions.

Table 3 explores difference-in-differences estimates in QCEW outcomes and the quantity of vacancies.[25] Firms training professional workers follow a pattern that is largely consistent with the

[25]For these outcomes, which exhibited strong pre-trends in the aggregate results, we further reduce our sample by using the nearest neighbor matched control sample. For the ad characteristics explored above, where the parallel trends assumption seems plausible even with the full set of control firms, we take advantage of the larger sample size.

Table 3: Wage and Growth Outcomes by Training Type

	Training Type			
	Professional		Production	
	Pre-period diff'l	Diff-in-diff	Pre-period diff'l	Diff-in-diff
	(1)	(2)	(1)	(2)
# Posts (100s)	0.062	0.13**	-0.044	0.018
		(0.055)		(0.025)
Observations		13,353		8,406
Log Employment	0.031	0.136***	0.0020	0.078***
		(0.027)		(0.029)
Observations		15,402		10,787
Log Wages	0.27	0.059***	0.19	-0.0049
		(0.016)		(0.025)
Observations		5,647		2,729

Standard errors in parentheses clustered on nearest-neighbor match pair.
*** p<0.01, ** p<0.05, * p<0.1

Notes: Each panel summarizes a different dependent variable. Columns labeled (1) report differences in mean characteristic for firms receiving the indicated training type versus their nearest-neighbor matches. Columns labeled (2) report difference-in-difference estimates separately by training type, controlling for firm and year fixed effects as well as a grant year indicator and a main effect for after (using placebo treatment years in the control group). The difference-in-differences analysis also restricts to the nearest neighbor matched sample and clusters standard errors on match pair. The log wages outcome controls for log employment and the ad share distribution across broad occupation categories and as such, restricts to firm-year observations with non-zero BG posts.

aggregate results. These firms posted substantially more BG ads before receiving grants and experience strong growth in log employment and number of BG job posts after grant receipt. This subset of professional-targeting firms also demonstrates significant increases in log wages per worker when controlling for shifts in the occupation mix of new hires. This control is important because, again consistent with the aggregate findings, this group of training firms decreases demand for a wide range of highly compensated skills and roles after receiving the grant.

Firms training production workers also experience notable, though smaller, increases in log employment relative to their matched control firms after receiving the grants. However, this increase is not mirrored in the ads they post. Moreover, these firms posted fewer ads than the average firm before grant receipt, suggesting they may have been on a slower hiring trajectory. The post-grant relative growth in employment is therefore likely coming from increased retention or perhaps fewer layoffs than the comparison firms. Furthermore, these firms do not see wage increases following grant receipt.

While we caution against inferring too much from the small sample of firms with grant descriptions, these patterns suggest two distinct use cases for training grants. In the first and most common case, growing firms may be blocked from further expansion by a shortage of managerial and operations related skills. Prior to receiving the grant, these firms posted many vacancies and demanded these kinds of professional skills that can become increasingly important during periods of rapid institutional change. Post-training, they grow even more rapidly and hire more, but are able to lower the skill requirements for new hires and no longer have outsized demand for professional positions. Controlling for these changes in the kinds of workers hired, these firms raise wages after grant receipt. These patterns are largely echoed, albeit sometimes more noisily, in the aggregate results in the previous section. On the whole, then, our evidence points to a plurality of training grants going to firms that need deeper managerial infrastructure to grow. Training resolves these needs and also results in growth in low barrier-to-entry positions, which may now be possible because of deeper skills at the top. Based on the wage evidence, it is likely that productivity improves as well.

In contrast, the smaller set of firms targeting production skills appear to be pivoting their workforce rather than fueling continued growth. As illustrated in Figure 10, these firms propose training in production tasks, but with a focus on adaptability, efficiency, and technology. Many states highlight a desire to help firms and workers keep up with the pace of technological change, particularly in production tasks (see appendix section A). Further, the focus on higher skill levels within production jobs aligns with management literature highlighting that today's manufacturing workers will need digital and technical skills and also adaptability given the increasing adoption of new production technologies.[26] These firms emphasize hiring in cognitive and computer skills both pre and post training (in Figure 11), skills that are complementary with automation machinery (Hershbein and Kahn, 2018). It could be that training is intended to transition production workers to work alongside automation technology rather than displacing them, consistent with some evidence on technological change in Germany (Battisti et al., 2023).

7 Conclusions

Public-private incumbent worker training programs have the potential to improve outcomes, relative to typical public-sector training programs that tend to have disappointing results. Direct input by employers on the types of skills they need can help with employment prospects. Further, employers

[26] Here are two examples of recent consulting reports: https://www.ey.com/en_us/industries/advanced-manufacturing-realized/prioritizing-next-generation-skills-for-manufacturing, https://themanufacturinginstitute.org/new-report-dives-into-the-skills-needed-for-modern-manufacturing/.

may be reluctant to pay to train workers themselves when they risk their investments being poached away. At the same time, workers may lack the resources or awareness to find training on their own.

In this paper, we compile a dataset of training grants that are allocated to private companies but administered by state governments using public funds. Exploiting unique linkages between the grants, the U.S. business registry, and the job postings of participating firms, we evaluate the characteristics of firms and markets that apply for and receive grants and then examine impacts of program participation. We find that grants are allocated to larger, older, faster growing firms that tend to hire more skilled workers. They are allocated to firms operating in labor markets that are larger and have greater poaching risk. Finally, we find that grant participation facilitates growth. This growth is disproportionately concentrated in lower skilled positions. Even conditional on the changing composition of jobs, firms relax skill requirements in job postings.

Overall, our findings are inconsistent with place-based development motivations. In particular, we do not see grants allocated to small or under-developed markets or to firms that are new to the state but have a larger presence elsewhere. We do not see grants allocated to megafirms that might hold out-sized influence. Finally, we see grants having actual impact on labor inputs, ruling out perfect crowd out of private investments.

This collection of facts is consistent with the idea that training grants help resolve a market failure that prevented training from happening in the private market. After program participation, these high-quality firms reduce barriers to entry, either because they have learned they can train workers rather than imposing up front skill requirements or because training resolved a specific need for the firm who now hires in complementary jobs.

Furthermore, the existence, allocation, and effect of these programs speaks to a seminal literature in economics on the frictions associated with human capital provision in the private sector. By leveraging these unique programs, we highlight that Beckerian frictions are likely present in the private sector, and public funds can help to alleviate these barriers to training.

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Appendix

A Policy Details on Training Programs

Breadth and Funding

We focus on public-private incumbent worker training programs, which are characterized by a firm-led proposal for state funds to share in the cost of training its own workers. At the national level, the Workforce Investment Act of 1998 allowed a small use of federal funds for such state-sponsored programs and this allocation was expanded in the Workforce Innovation and Opportunity Act of 2014 (WIOA). WIOA allows states to spend up to 20% of their allocated federal funds on incumbent worker training grants.

Beyond the federal level, state-level programs that provide funding for public-private training have existed since the 1960s with the majority of programs beginning in the 1980s and 1990s. In addition to WIOA funds, states use a combination of revenue from state unemployment taxes, general appropriation funds, and training-specific taxes to provide grants directly to firms to train incumbent or newly hired workers. A survey of 30 states by the Upjohn Institute in 2006 (Hollenbeck, 2008) found that states were investing around \$550 to \$800 million into public-private training partnerships, which is analogous to about 1% of what private firms spend on training. However, these programs have been largely overlooked by researchers since the WIOA expansion.

We conducted a comprehensive search of state incumbent worker training programs by browsing state training websites and combing program annual reports for detailed data. We tracked programs where the primary training grant recipient is an individual firm. Out of the fifty states and DC, we identified 45 which have programs that meet this criteria and 40 for which we can find aggregate annual expenditures on these programs. Figure A.1 reports average annual spending per-capita for these 40 states.

Of these 40 states, 18 have parsable firm-level data on program participation. Throughout all analyses, we restrict our attention to these 18 states, indicated in Figure A.1 with stars. The median spending among states with firm-level data is approximately \$2.60 per capita (Michigan), and the largest spender is New Mexico at approximately \$10 per capita.

Stated Program Motivations

In promotional materials and program reports, most states reference a desire to improve the overall quality of jobs workers can attain and to target mismatches between worker skills and firm needs. For instance, Massachusetts asks applicants to "address selection criteria associated with job growth or increases in skills/opportunities of low-skill or low-wage workers" (Commonwealth Corporation, 2024). Similarly, Michigan hopes its program will "address skill shortages by reskilling and upskilling"(Michigan Department of Labor & Economic Opportunity, 2024).

Many states particularly highlight the challenges that both workers and firms face in keeping up with the pace of technological change. From Vermont's 2019 program report, pg 3: "Advanced manufacturing is continuously evolving with more complex equipment which requires more technically advanced workers to program and maintain them. Meanwhile, employers continue to lose their content experts who are aging out of the workforce, often taking their institutional knowledge with them as few employers can afford succession planning. VTP is an excellent means of helping businesses to 'up-skill' existing employees allowing them to advance into the vacated positions"(Vermont Agency of Commerce & Community Development, 2020).

From a California report: "As rapid advancements in technology, automation, and artificial intelligence reshape the economy and the nature of work, more needs to be done to promote high-quality jobs and economics security for workers, families, and communities"(California Panel Members, 2022).

Several states also indicate some place-based development goals. West Virginia describes their program as "play[ing] an important role in attracting new enterprises and encouraging the growth and expansion of the state's existing companies"(West Virginia Economic Development, 2012).[27] Half of the states in our sample list prioritized industries in their program descriptions. For example, California prioritizes manufacturing, healthcare, biotechnology, information technology, construction, agriculture, and logistics firms and in particular "targets firms threatened by out-of-state competition or who compete in the global economy" (Rice et al., 2005), while Florida targets "businesses able to locate in other states and serving multi-state and/or international markets"(CareerSource Florida, 2015).

Finally, states sometimes mention a desire to bolster economically disadvantaged labor markets, workers, and firms. Six states prioritize firms in areas with more disadvantaged workers. States often design their programs to ease the burden for smaller firms. For example, Maine requires firms

[27] Four states – New Hampshire, New Jersey, Oklahoma, and West Virginia – extend eligibility to firms that intend to physically relocate to the state, rather than only offering grants to firms already in the state. In contrast, Florida, Louisiana, and Ohio all require firms to have been located in the state for a minimum period of time before application.

with over 100 employees to pay 50% of training costs, firms with between 51 and 100 employees to pay 25% of training costs, and firms with less than 50 employees have zero required contribution. Ten of the eighteen states explicitly prioritize small businesses, with some states such as Michigan or Arizona providing additional points in their rubrics for businesses below a certain employee count.

Process

The 18 programs we study share some common features, but vary significantly in process, scope, and focus. As mentioned, some states reserve training for incumbent workers while others require firms to hire new workers to train. In practice, 11 states allow for both incumbent and newly hired workers, 6 provide funding only for incumbents, and 1 limits to newly hired workers.

States vary in the total administrative burden of applying for these grants. While some states report high rejection rates or describe a competitive process, others either have much less information on how they allocate grants or expressly state a first-come, first-serve approach. Six states evaluate grants using published scoring rubrics. For example, Michigan's 50-point rubric covers industry priorities, training provider quality, diversity considerations, post-training certification for workers, wages at the firm, and size of the funding request. Others may publish strong criteria, but in actuality accept all applicants. California has many application requirements, meaning that firms typically hire expert consultants to navigate the process, but the vast majority of applications that reach the final review board are approved.

Most programs either give higher priority to firms which promise to increase wages following training or explicitly require that workers receive a particular wage. For example, Vermont requires that at the completion of training, the firm must pay a wage that equals or exceeds a 'livable wage' ($15.33 as of 2022). We document that 15 out of 18 states require firms to report employment status and wages of trained employees to the state.[28] For example, firms in Michigan must provide a company payroll query at three-months post-training reporting the name, hourly wage, hire date, and termination date (if applicable) for all employees trained, and they do not receive full reimbursement for training costs unless the trainee retained employment for 90 consecutive days post-training.

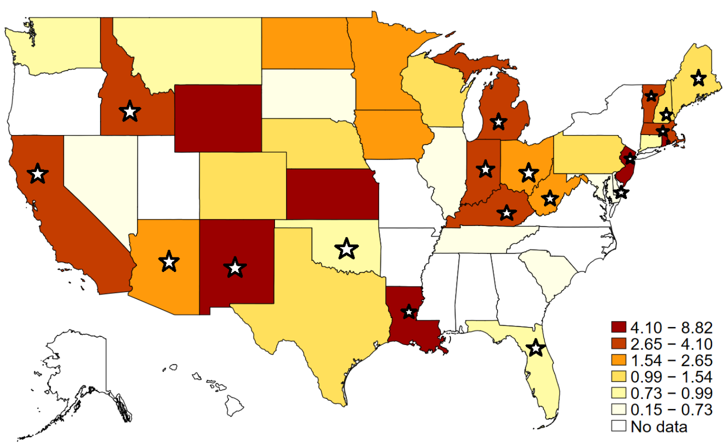
In addition, many states structure the program to provide workers with credentials that can be carried across firms. Though some states allow for training to be internal (i.e., on-the-job), a number

[28] West Virginia also requires post-training reports from the firms, but information is not available on what these reports must include. There is no available information on whether New Hampshire or Oklahoma require post-training reports.

of states either explicitly require that training take place off-site through the state/community college system or a third party provider. Four states– Idaho, Indiana, Michigan, and Ohio– verify that workers have an industry-recognized credential at the end of training.[29]

States put caps on the amount of funding the firm can apply for ranging from $1,000 per worker in Idaho to $8,000 per worker in Arizona.[30] Figure A.2 summarizes the distribution of grant dollars per worker, which is available for 75% of grants in our database. The median value is around $1,100 dollars, though there is a sizable right tail so the mean ($2,240) is considerably higher. Considering the typical training duration, these values amount to about $20-$40 per worker-week. Employers cannot recoup much of their salary outlay. Instead, money can cover training materials and infrastructure, and small contributions for the opportunity cost of time. In most states, firms must provide some amount of matching funds (typically 50% of training costs).

Figure A.1: Per-Capita Spending on Public-Private Incumbent Worker Training Programs

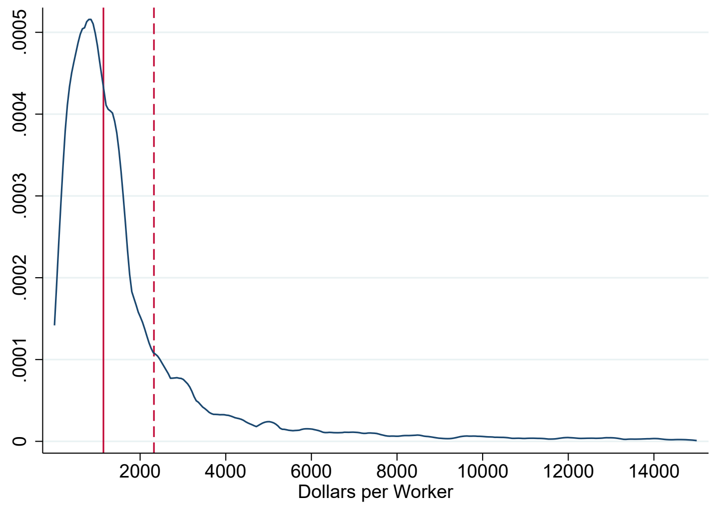


Note: Stars indicate states which report firm-level data and are used in our analytical sample. Average per-capita spending on public-private training grants in author-collected data. We restrict attention to states that publish aggregate state-level data on spending. Per-capita spending is defined as the total dollars granted to firms in a state per fiscal year divided by the working age population (19 to 64 year olds) in that state with population data taken from the Current Population Survey (2013-2019). No data includes both states which have a program but do not report spending and states which do not have an identified program.

[29]For example, firms in Ohio must provide the state with copies of a class roster, transcript, or a copy of the certificate for each trainee in order to receive reimbursement for the training. Maine's program partners with the community college system, creating credit and non-credit based courses at specific colleges to meet the training needs of firms.

[30]Some states cap total grant amount rather than per worker amounts. Grant size caps range from $70,000 per grant in New Hampshire to $850,000 per grant in California.

Figure A.2: Grant Amount per Trainee



Note: Density plot of grant dollars per trainee across grants in author-collected data. Solid vertical line is the median; dashed line is the mean. For clarity, we omit from the figure (but not the mean and median calculations) grants with more than $15,000 per trainee, 2% of our database.

B Matching Grants to QCEW and BG Data

We use firm name plus geography to match training participants to QCEW establishments, limiting attention to grants allocated from 2010-2019 (80% of our collected data). We first regularize employer names by removing common components such as LLC or "the", removing punctuation, standarizing common word stems, etc. We then look for matches on exact (cleaned) name and county. When an exact match is not available, we use fuzzy matching techniques to find similar names across datasets, while relying on common geography to identify higher quality matches. Once training grants are matched to QCEW, we take advantage of the QCEW-BG match produced by Dalton et al. (2025) to bring in ad characteristics.[31] The resulting dataset uses firm name-county pairs as its unit of observation – the most detailed level at which we can match. Throughout, we refer to these name-county pairs as establishments or firms.

Table B.1 summarizes the grants data and our matches to QCEW and BG. The full sample contains 13,375 cleaned grants averaging about $92,000 in annual grant money. When available in the data, we observe that an average of 94 workers are to be trained, 58 of which are promised to be new hires. Average grant dollars per trainee is around $2,000.

[31] Note, for this latter match, we must restrict attention to the 70% of BG vacancy postings that specify an employer name. Ads with a missing name tend to be jobs posted by recruiting agencies.

We are able to match 95% of the grants to an establishment QCEW. Columns 2 and 3 compare grant characteristics for matched versus unmatched grants. The grants that cannot be matched are larger in dollar amount and number of trainees. We also report the method used to match firms. The vast majority are matched on exact firm name after the initial clean, though we do pick up a non-trivial number of matches with the fuzzy match.

Of the QCEW matches, we are able to match 85% to a firm that posts at least one ad in BG. Columns 4 and 5 compare the BG matched to unmatched samples, among the QCEW matched grants. Again, grant dollar amounts are larger in the unmatched sample, while number of trainees and new hires is smaller. Dalton et al. (2025) as shown that small firms are less likely to post in BG. However, the grants that do not match to BG might be overall a noisier sample as indicated by their much lower exact-match rate to QCEW (48%, compared to 75% among the BG matched grants).

For a sub-set of our analyses, we re-run the linking algorithm on a BG-only dataset. This linkage allows us to conduct analyses for skill outcomes outside of the data enclave. Unlike the QCEW-BG-grant match, we cannot observe in this data set if a year where we observe no vacancies is due to the firm not existing or due to the firm not posting any vacancies online. For this reason, we prefer the QCEW-BG-grant data for analyses with number of vacancies as the outcome. However, for skill-outcomes which measure the proportion of ads that report a skill, the BG-grant match is equivalent as zero ad years are dropped from both samples.

Table B.1: Summary Statistics of Training Grants across Merge Samples

	(1)	(2)	(3)	(4)	(5)
	All Cleaned Grants	QCEW Match		BG Match	
		Matched	Unmatched	Matched	Unmatched
Grant Dollars	92137	90837	115976	87633	108667
	(191109)	(188184)	(237387)	(167363)	(276163)
	N=13249	N=12564	N=685	N=10650	N=1914
# Trainees	94.39	92.67	122.89	96.34	74.72
	(210.91)	(206.33)	(274.82)	(214.73)	(157.67)
	N= 9966	N=9400	N= 566	N=7808	N=1592
# New Hires	57.60	58.05	50.04	60.85	45.91
	(1443.53)	(1485.58)	(130.32)	(1645.58)	(182.68)
	N= 5335	N= 5035	N= 300	N=4092	N=943
Grant Dollars per Trainee	2240.0	2259.8	1904.4	2040.6	3365.4
	(4522.2)	(4600.7)	(2866.1)	(3676.8)	(7635.9)
	N= 9645	N= 9107	N= 538	N=7600	N=1507
Grant year	2015.2	2015.2	2015.3	2015.3	2014.8
	(2.6)	(2.6)	(2.6)	(2.5)	(2.9)
Match to QCEW	0.95	1	0	1	1
Exact match	0.67	0.71	0	0.75	0.48
Match to BG	0.80	0.85	0	1	0
N (# Grants)	13375	12681	694	10750	1931

Notes: We report means of grant characteristics, as well as standard deviations in parentheses, and sample sizes (for the variables that are sometimes missing from the data). Grant data are assembled by the authors by reviewing state department of labor websites for training programs characterized by public funds flowing to individual firms to train their own workers. Column 1 includes the full sample of grants. Columns 2 and 3 compare grants that can be matched to the QCEW versus those that cannot, using the matching procedure described in the text. Columns 4 and 5 take the QCEW matched sample and compare grants that can be further matched to a firm in BG versus those that cannot, using the Dalton et al. (2025) merge, which follows a similar procedure.

C Nearest Neighbor Matching Algorithm

We match each grant-receiving firm without replacement to their one most similar untreated firm, considering only firms in the same two-digit industry within states with available training grant data for the reference year. To avoid capturing spillover effects within our control sample, we exclude all untreated firms in industry-county markets where at least 20% of workers were at firms that received a training grant in any year within two years of the reference year. This exclusion removes control firms that are most likely to experience significant spillover effects while retaining a large and representative control sample. A stricter exclusion criteria that dropped all markets that ever received a grant in our data would remove virtually all large markets and limit our ability to find good matches for all treated firms. The "reference year" in this matching process is the year of first grant receipt for treated firms and the randomly assigned placebo year for untreated firms. Each untreated firm is therefore only eligible to be selected as a match in one, randomly assigned, year. This choice simplifies and speeds up the process of matching without replacement at the cost of reducing the pool of eligible matches in each year. In practice, the pool of untreated firms is so large that this restriction does not affect match quality.

Within the set of eligible firms, we select the single best match for each treated firm based on minimizing the Minkowski distance between log employment in periods $t-1$ to $t-5$ relative to the reference year and indicators for having any job-posting activity in Burning Glass in $t-1$ to $t-5$. Log employment is an effective summary measure of the size and growth trajectory of firms. The indicator of having and Burning Glass activity is less interesting as an outcome in its own right (for larger firms like the typical grant recipient, variation in this feature is mostly at the firm rather than firm-year level). However, the choice to participate in online job hiring may capture some interesting firm characteristics that affect trends in outcomes and certainly helps reduce sample loss for our nearest-neighbor analyses of outcomes from job postings. For firms with missing log employment for some years of the pre-period, which largely reflect true zeros when the firm was not active, we fill in a value of -1,000, which is sufficient to ensure that we almost never match a firm with positive employment in some pre-period year to a firm with no employment in that year.

We use the BallTree nearest neighbor matching algorithm, implemented in SciKitLearn, to match efficiently. Finally, we drop firms from the matched analysis if we are unable to find a close match. In practice we drop matched pairs where the mean difference in log employment over the pre-period (including any -1,000 missing indicators) is greater than 0.16. Matching directly on these three core firm characteristics, industry, pre-treatment log employment, and pre-treatment hiring behavior, is sufficient to resolve the main violation of parallel trends in our full sample analysis: firms that apply for and receive training grants grow faster than the average firm in the years leading up to grant receipt.

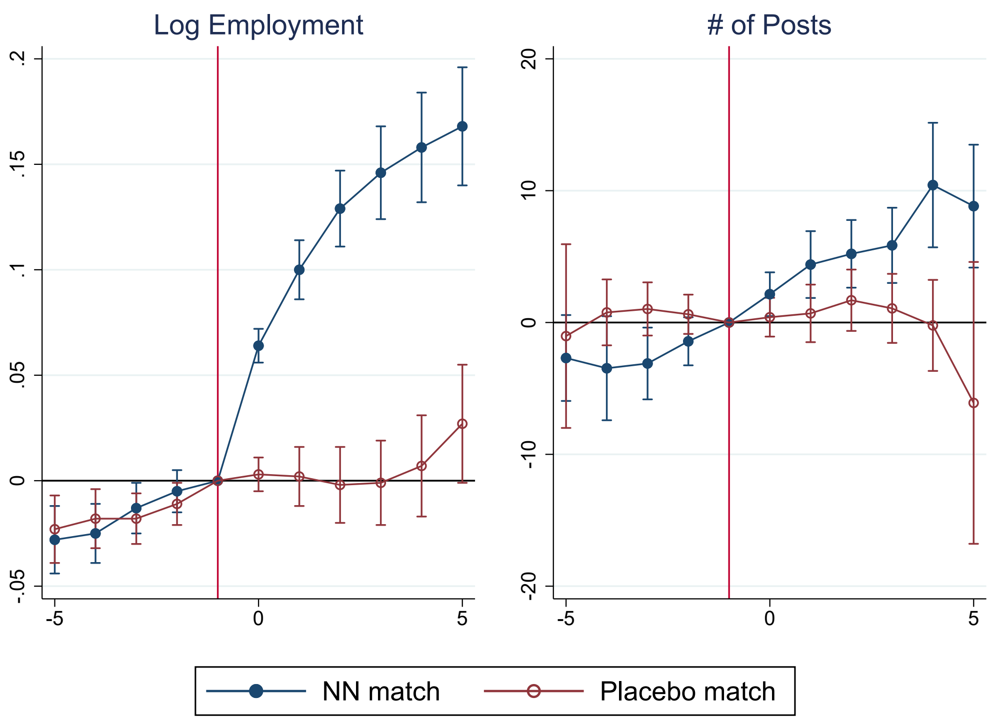
One concern with matching on all pre-periods is that any post-period effects that we see are attributable to over-fitting and mean reversion. Specifically, if the control firm matched to the treatment has idiosyncratically high employment in the pre-periods which makes it a good match to the treated firms (which on average have higher employment), it is possible that it will revert back downwards in the post-period, resulting in an upwardly inflated treatment effect. We address this concern in two ways. First, Appendix Figure C.1 plots the mean values of two outcomes of interest for the grant-receiving firms and the selected matched control firms. The change in the differences between these two series approximates our estimated causal effects. Mean log employment shows no differences on average in the pre-period, as we would expect since this outcome is targeted in our match, while the number of BG posts shows a consistent pre-period gap. For both outcomes, the treated firms show a clear trend break after grant receipt while the control firms do not, providing reassuring evidence that our results are driven by true changes within the treated firms rather than mean reversion within the controls.

We further assess the validity of our matching assumption with a placebo exercise. We run each control firm selected as a match for a treated firm through the matching algorithm a second time, identifying a match to the matched firm. We then re-run our event study estimates giving the original matched sample a placebo treatment at time 0. If our algorithm is sensitive to overmatching, we might estimate spurious "treatment effects" even in this placebo control-to-control sample because of mean reversion. Instead, as illustrated by Appendix Figure C.2, this placebo exercise yields clear zero effects.

Figure C.1: Mean Outcomes for Treated and Matched Control Firms

Notes: Average outcomes by event time for firms receiving grants and the set of untreated firms identified as matched controls.

Figure C.2: Null Effects with Placebo Match



Notes: See figure 5 for regression specification details. Here we plot the baseline nearest neighbor match results alongside a placebo match. The placebo match takes the firms selected as match controls for the treated firms and repeats the matching algorithm to find a second set of matching control firms. See Appendix C for more details.

D Categorization of Training Grant Descriptions

For a sub-set of the states in our sample, we have text descriptions of the firm's training plans taken from grant applications. To better understand what types of skills firms are using these grants to develop, we classify each training plan into one of four occupation groupings: (1) Professional, (2) Production, (3) Sales and Administrative Support, and (4) Service occupations. Because of the large number of training plans and the varied format of these plans, we use Open AI's Generated Pretrained Transformer (GPT) 3.5, a large language model (LLM) to classify each firm's text into these categories.

To construct predicted labels for each training plan text, we first supply a system-level prompt to GPT- 3.5. These system level instructions serve as a meta-prompt for the model and outline how the model should respond to subsequent user-level prompts. Figure D.1 shows the system-level prompt that we supplied to GPT for the classification task, and Figure E.3 provides an example of the training plan texts that are fed in as user-level prompts for classification. Specifically, we provided in-depth details on the objectives of the task, what each groups consists of and their corresponding Bureau of Labor Statistics SOC codes, and in what manner the model should respond. To generate a prediction for each training plan text, then, we fed in each training plan text one at a time as user-level prompts to the model and collected responses. Finally, similar to Ziems et al. (2023), we set the temperature of the model to 0 to reduce the variance in GPT responses and create reproducible results as much as possible. We set all other model parameters to their default values.

We generated 5 GPT-classified samples for 3,540 training plans scraped from grant applications submitted to California, Kentucky, Massachusetts , New Hampshire, and New Jersey. We then constructed the predicted occupational targets for each training plan by taking the mode across the 5 samples. For example, if the set of occupational targets (in order) predicted by GPT-3.5 are (Professional,Production), (Professional), (Professional, Production), (Professional), (Professional, Production), the final predicted targeting would be (Professional, Production). In the case that GPT-3.5 did not have a majority prediction across the 5 samples (at least 3 of the predictions matching), those training plans were handlabeled. A similar approach is discussed in Ziems et al. (2023), where the authors average LLM responses over 5 different types of system-level prompts in order to generate predictions. In total, GPT-3.5 had complete consensus (all 5 predictions were the same) for 2,474 training plans, majority consensus (at least 3 predictions were the same) for 3,189 training plans, and did not reach consensus (and therefore required hand-labeling) for for 64 training plans.

To give a concrete example, the firm depicted in Figure E.3 is a sign manufacturer which received a training grant in California. Based on the text used to classify this firm's training plan, this firm is listed as professional, production, and sales and administrative support. While the company itself is a manufacturing firm and some of the training related to production skills such as safety procedures for crane usage or sign installation, many of the training skills listed include white-collar skills such as working with computer software like Microsoft Excel, improving HR skills, or negotiation skills.

Figure D.1: System Prompt to GPT-3.5

Assistant is an intelligent chatbot designed to help determine the occupational targeting of workforce development grants.

Each string of text that Assistant will receive is the training plan outlined by a company that is applying for a workforce development grant.

Each training plan is targeted to one or more occupation groupings.

Assistant's task is to determine which occupation group(s) the training plan is targeting given the training plan text by first determining which 2-Digit (major) SOC code (as provided by the Bureau of Labor Statistics) the plan is targeting and then aggregating into defined occupation groups defined below.

Here are the possible occupation groups, their descriptions, and their corresponding 2-digit SOC Codes, as provided by the Bureau of Labor Statistics (BLS):

1. *Group: Professional, Description: Highly skilled white collar occupations. BLS SOC Codes: 11, 13, 15, 17, 19, 23, 27, 29.*
2. *Group: Sales & Administrative Support, Description: Routine white collar positions such as sales and office support. BLS SOC Codes: 21, 25, 31, 41 (excluding occupations with minor SOC codes starting with 412, which are Retail Sales Workers), 43.*
3. *Group: Service, Description: Positions such as servers and personal care jobs. BLS SOC Codes: 35, 37, 39, and occupations with minor SOC codes starting with 412 (Retail Sales Workers).*
4. *Group: Production, Description: Blue collar jobs such as construction, production, and related occupations. BLS SOC Codes: 33, 45, 47, 49, 51, 53.*

In the case that there are multiple occupation groups that Assistant thinks the training plan is targeting, Assistant must rank their choices in order of most likely (first) to least likely (last).

Assistant's answer should be presented as such: Groups: (group choices) ; Reasons: (reasons). Note that the group choices should be listed FIRST in the area denoted "(group choices)" and the reasons should be listed in the area denoted "(reasons)".

The first answer in the group choice list must be the group that Assistant thinks the training plan is primarily targeting. The last answer in the group choice list must be the group that Assistant thinks the training plan is least likely to be targeting, but is still a focus of the plan itself. Respond ONLY with the group name, not the number. If Assistant does not think the Training Plan is targeting an occupation group, it should not include it in the list.

For example, if Assistant believes the training plan is targeting the Professional and Production groups with Professional being the most likely, Assistant's answer should be formatted as:

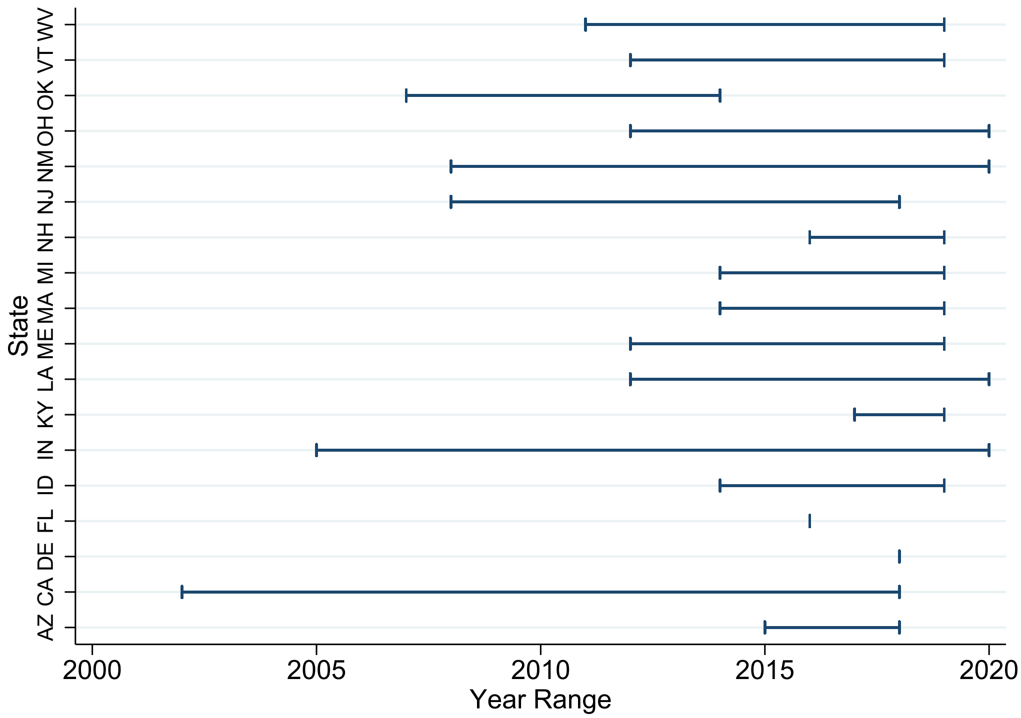
Groups: Professional, Production ; Reasons: Professional Reasons, Production Reasons.

Lastly, if Assistant does not think the training plan contains enough information to make a prediction, Assistant should simply return "ERROR: Not enough information contained in training text."

Notes: This figure shows the system-level prompt fed into GPT-3.5. This prompt is also referred to as the Aggregated prompt.

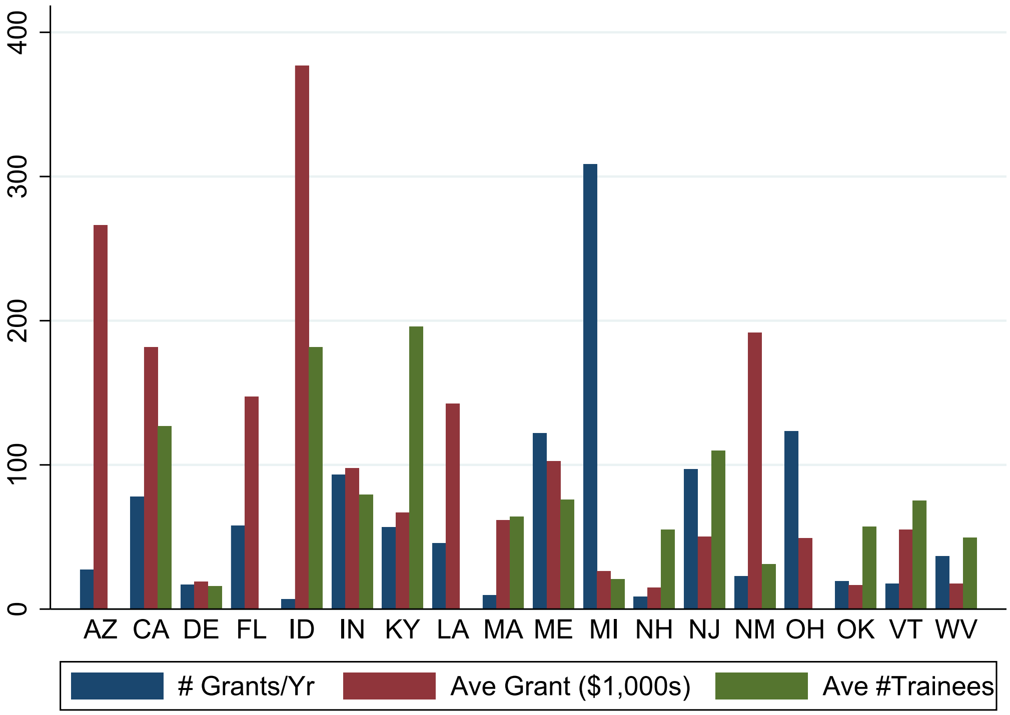
E Appendix Figures and Tables

Figure E.1: Availability of Grant Data by State and Year



Notes: We summarize the range of years for which grant data are available for a given state. Grant data are assembled by the authors by reviewing state department of labor websites for training programs characterized by public funds flowing to individual firms to train their own workers. We include data from any program that lists individual employer participants.

Figure E.2: Size and Number of Grants by State



Notes: We plot characteristics of grants by state for the matched sample of grants (see table B.1). We report unweighted means of the number of grants per year, grant dollars and number of trainees. The latter is unavailable in a small number of states. For these additional descriptives on posts, we use a sample created outside the BLS enclave that uses only BG information to match grants to firms. See appendix B for details.

Figure E.3: Example of Training Description

OVERVIEW

For over 60 years, The Company has manufactured electric and architectural signs. The Company's products are used primarily for brand identification and business location visibility. basic sign product is composed of either steel or aluminum and decorated to reflect the customer's name and/or logo. As a full-service sign company, provides services ranging from initial design concepts to detailed plans. The Company also provides fabrication, installation and maintenance of their products. customers include major hotels, property management companies, building owners, shopping centers, and general contractors.

uses Computer-Aided Design and Computer-Aided Manufacturing software integrated for design and fabrication. This technology provides with a competitive advantage. In order to meet growing customer demands and preserve its market share, seeks ETP funding to train employees at company sites in Oakland and Stockton.

Training Plan

All the proposed training is new content designed to supplement previous training. While some of the types and topics appear to be the same, the content has been updated. The training will be delivered by in-house trainers and vendors.

Business Skills: This training will be delivered to Contract Control, Project Coordinators, Sales, and Managers. Training will assist The Company as they manage growth and new project initiatives and implement ongoing business changes, such as reforms in HR processes to support growth. Expanding the skillsets of employees reinforces Company's commitment to creating a high performance workplace. Topics such as Estimating, Human Resources, and Effective Communications will be delivered.

Commercial Skills: Training will be offered to Production Staff and Installation Staff. This training will cross-train employees and diversify their current specializations so that employees have broader skillsets. Training will improve the ability of individual employees to perform more functions and services in order to boost overall productivity, improve safety, and gain specific competencies. Crane Operations, Electric Sign Installation, and Rigging are some examples of topics delivered. Driving related training does not include required licensing requirements. Some training topics will be delivered by vendors that offer certifications to demonstrate gained competencies such as forklift driving. Certifications generally add value to employees readiness to accept higher skilled higher paying positions.

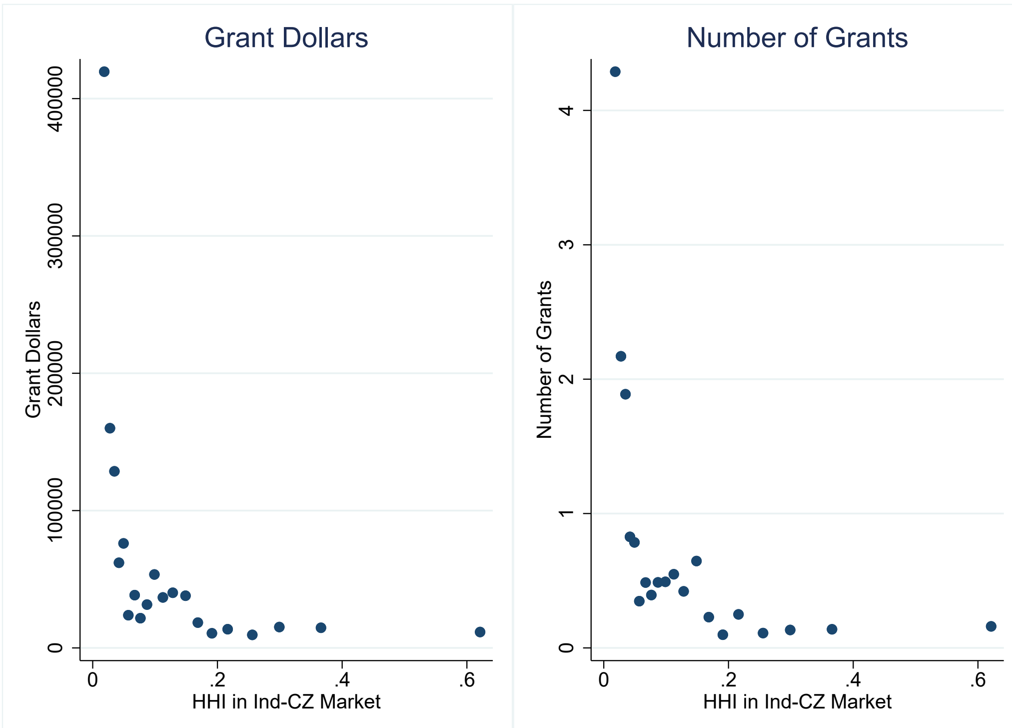
Computer Skills: Training will be offered to Administrative Staff, Sales Staff, and Management Staff. Products like Gaant Charts and Microsoft Excel are being used by key contractors. Staff needs to be proficient and current on the newest software skills.

Manufacturing Skills: Training will be offered to Production Staff and Engineers. This training will help speed product fulfillment. New machinery including; mill saw, trimming, drills, vacuum, sander and spray gun were purchaed to keep pace with business changes. Training topics include; Tools, Structual engineering, Welding, and Certified Welding Inspector.

Continuous Improvement: Training will be offered to all staff to improve efficiency. Training topics include; Improving Sales Skills, and Negotiations. Sales Staff will receive Sales Skills Training which combines new product knowledge and customer relations. Construction Methodology will be given to Engineers to enable them to competitively bid and retain customers.

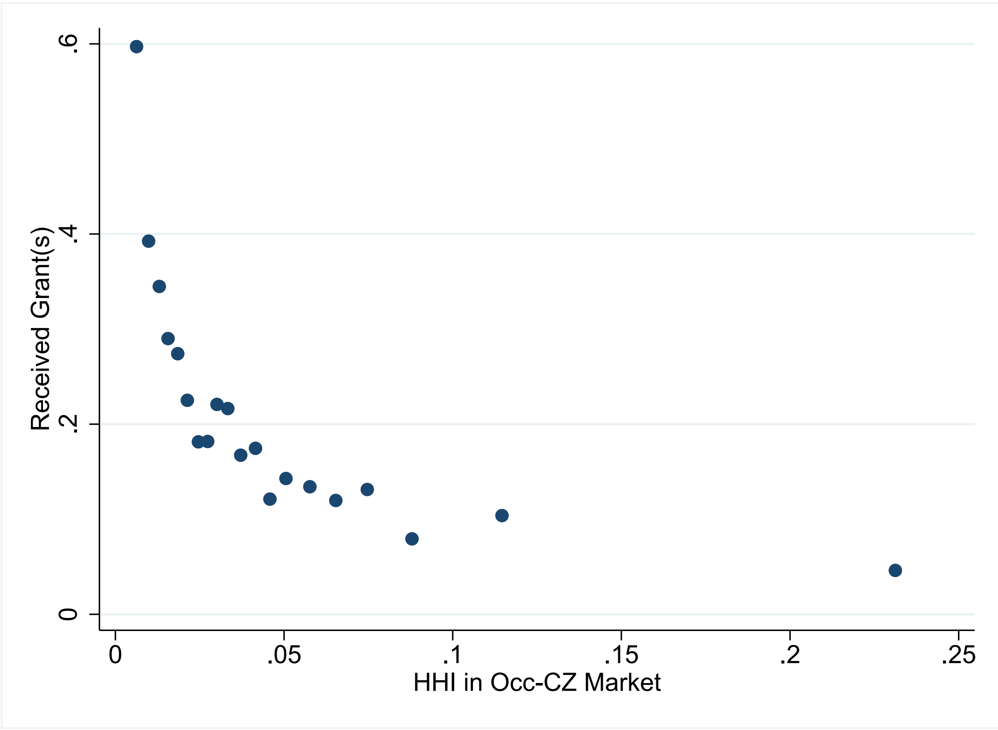
Notes: Example of a company-specific training plan that can be found in the state grant proposal documents. In this example, the text outlined in red is scraped, preprocessed, and fed into GPT-3.5 as a user-level prompt to determine its occupational targeting. This company may or may not appear in our merged analytic sample.

Figure E.4: Training Grants and Market Concentration: Robustness to Number and Size of Grant Outcomes



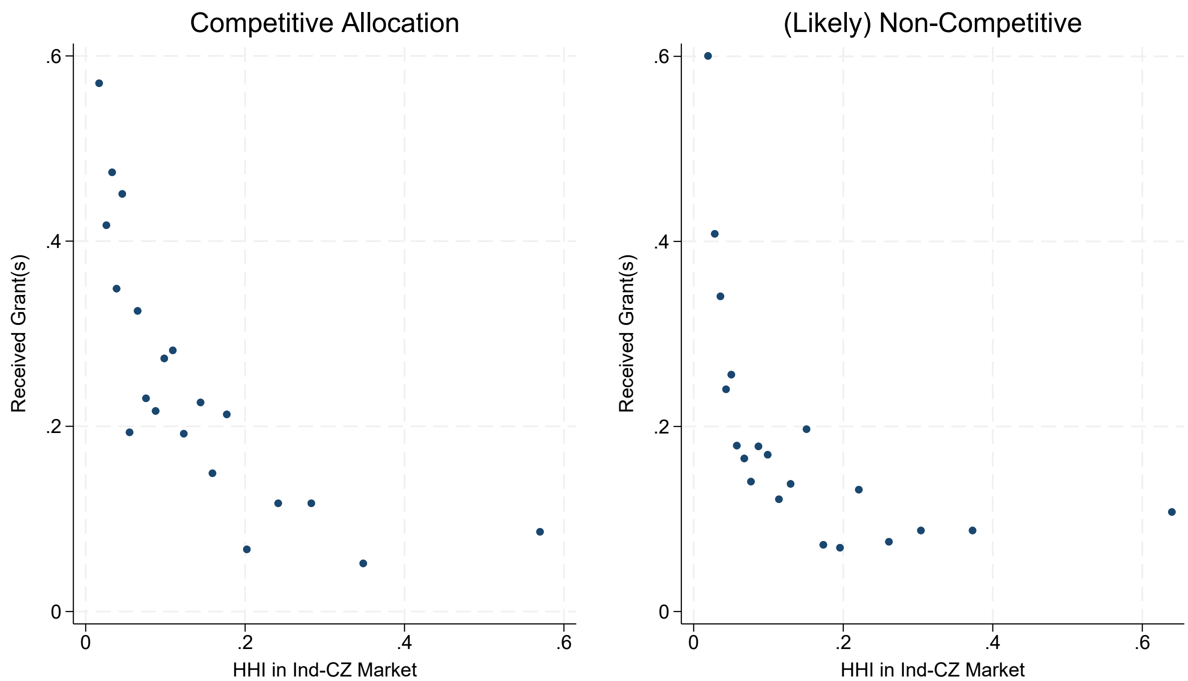
Notes: See figure 3. Here we plot bin scatters of total grant dollars or total number of grants (both including zeros) in a market on the concentration of vacancies. Markets are defined at the CZ-by-two-digit industry level and concentration is the HHI of job vacancies posted in the market

Figure E.5: Training Grants and Market Concentration using occupation-by-CZ Market Definition



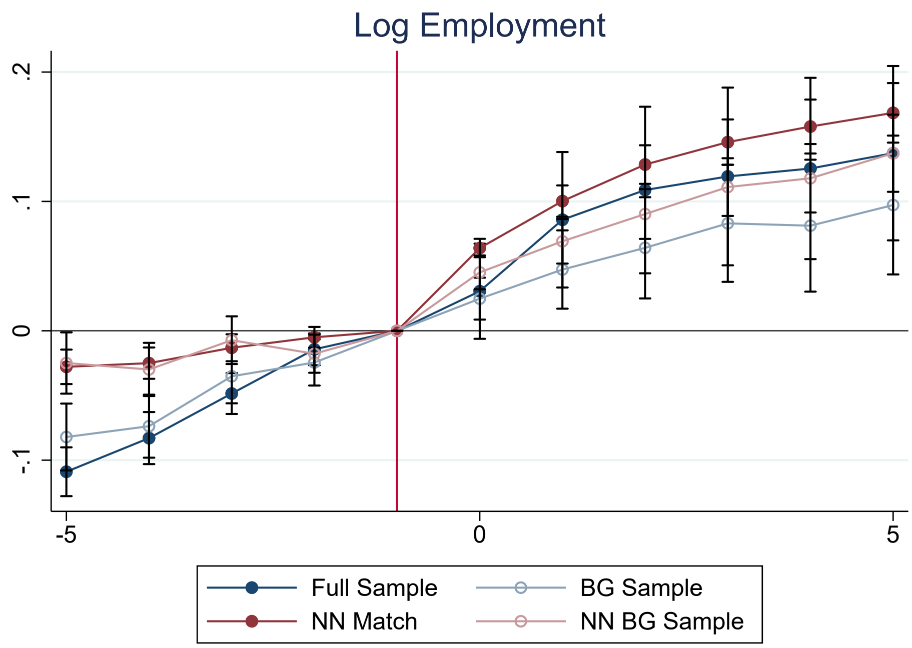
Notes: See figure 3. We divide markets (CZ-by-three-digit SOC occupation pairs) into 20 equally-sized bins based on the HHI of job vacancies posted in the market (see equation 2). We then plot the share of markets that received any grants and average HHI within each bin.

Figure E.6: Training Grants and Market Concentration by State-Level Competitiveness



Notes: See figure 3. Here we split states into those with a rigorous scoring rubric and competitive selection process (left) and those with no apparent rigor (right). The former include Arizona, Kentucky, Louisiana, Michigan and New Hampshire.

Figure E.7: QCEW Outcome Restricted to BG-Matched Sample



Notes: See figure 5 for regression specification details. The BG samples in this figure restrict to establishments that post at least one ad in the year. We also report the 90% confidence interval based on standard errors clustered at the state level.

Table E.1: Summary Statistics for Grant and Non-Grant Firms

	All Firms		NN Matched	
	Grant	Non-Grant	Grant	Non-Grant
Panel A:	QCEW Sample			
Characteristic				
Employment	227.67	22.05	144.28	139.45
Wage per Worker	62101	43412	61591	57637
Age	16.93	13.50	16.93	17.10
Annual Growth Rate	0.076	0.058	0.070	0.048
Semi-Firms	8667	1742145	7517	7517
Panel B:	BG Matched Sample			
Semi-Firm-Level Characteristics:				
Characteristic				
Employment	258.12	40.24	159.98	163.62
Wage per Worker	63349	49557	62739	60393
Age	17.30	14.09	17.29	17.85
Annual Growth Rate	0.078	0.064	0.073	0.046
Semi-Firms	7042	619190	5995	5391
# BG Postings in t-1	23.76	4.43	19.36	15.15
Ad-Weighted Characteristics:				
Characteristic				
Education Req.	0.702	0.541	0.692	0.634
College Req.	0.467	0.281	0.442	0.365
Experience Req.	0.601	0.473	0.59	0.51
Computer Req.	0.325	0.25	0.315	0.291
Cognitive Req.	0.418	0.293	0.403	0.36
Professional Occ.	0.649	0.457	0.63	0.544
Admin Occ.	0.162	0.246	0.171	0.183
Service Occ.	0.036	0.137	0.043	0.086
Production Occ.	0.102	0.121	0.106	0.146
# Ads Total	167232	2736863	110682	81622

Notes: This table reports characteristics for grant and non-grant recipients. Panel A uses the entire QCEW matched sample, while panel B restrict so the BG sample. Establishment-level characteristics and the number of BG postings are measured in the year before grant receipt; the employment growth rate measures the change between t-2 and t-1. BG ad characteristics are ad-weighted averages for the entire pre-grant period. For comparison, non-grant recipients are assigned a placebo grant year at random, excluding the first and last 2 years of operation. The occupation variables divide SOC occupations codes into four mutually exclusive and exhaustive groups: Professional includes SOC 11-19, 23, 27, 29; Sales/Admin is 21, 25, 31, 41 (excluding 412), 43; Low-skill Services is 35-39, 412; and Blue Collar is the remainder (33, 45-53).

Table E.2: Training firms and market characteristics: 3-digit Occupation-by-CZ

Dependent Variable	Any Grants Received (mean = 0.207)			
	(1)	(2)	(3)	(4)
HHI	-1.687***	-1.938***	-1.599***	-1.623***
	(0.426)	(0.406)	(0.424)	(0.407)
HHI^2	2.763***	3.241***	2.629***	2.653***
	(0.803)	(0.856)	(0.777)	(0.753)
CZ unemp rate	-0.392	-0.512	-0.288	-0.471
	(0.647)	(0.608)	(0.648)	(0.787)
New Market	-0.139**	-0.154**	-0.135**	-0.152**
	(0.051)	(0.053)	(0.055)	(0.064)
Employment (1,000s)	0.075***	0.072***	0.076***	0.075***
	(0.005)	(0.004)	(0.005)	(0.006)
Wage ($100s)	0.262***	0.318***	0.307	0.120
	(0.061)	(0.077)	(0.215)	(0.140)
Emp growth			-0.083	-0.083
			(0.078)	(0.071)
Wage growth			0.046	0.062
			(0.083)	(0.082)
Leave-out State Emp			-0.025*	
			(0.013)	
Leave-out State Wage			-0.064	
			(0.254)	
Leave-out State Emp Growth			0.016	
			(0.155)	
Leave-out State Wage Growth			0.130	
			(0.078)	
Leave-out Region Emp				-0.006
				(0.004)
Leave-out Region Wage				0.037
				(0.040)
Leave-out Region Emp Growth				-0.049
				(0.068)
Leave-out Region Wage Growth				-0.071
				(0.225)
Observations	77,224	77,224	77,224	77,224
R-squared	0.217	0.269	0.218	0.218
Occ, State-by-Year FEs	X	X	X	X
Occ-by-year,Occ-by-State		X		

Standard errors in parentheses clustered by state.
*** p<0.01, ** p<0.05, * p<0.1

Notes: Observations are 3-digit occ-by-CZ-by-year. HHI, Employment, and Wages are occupation-by-CZ averages from 2010-12. Emp and wage growth are the rate of change in 2012 from 2010 for the occ-by-CZ. The CZ unemployment rate varies by year. The Leave-out State and Region variables are also at the occupation-by-geography level, averaged over 2010-12 or the rate of change over that period and leave out the focal CZ or state, respectively. Regression observations restricted to 2013-2019. Covariates are defined for the 13,902 markets that posted at least 50 ads in the baseline 2010-12 period and have coverage in the ACS, and other markets are considered "New".

Table E.3: Robustness to alternative measures of market concentration

Dependent Variable	Received Grant (Mean: .2)					
	Occupation Mkt		Industry Mkt			
	(1)	(2)	(3)	(4)	(5)	(6)
HHI	-2.208***		-0.999***			
	(0.513)		(0.123)			
HHI^2	5.432***		0.943***			
	(1.439)		(0.129)			
Share of ads to top 3 Firms		-0.555***		-0.420***		
		(0.141)		(0.051)		
Share of Emp in top 3 Firms					-0.236***	
					(0.059)	
Industry Tightness						0.0001***
						(0.000)
Employment (1,000s)	0.076***	0.072***	0.047***	0.044***	0.050***	0.042**
	(0.005)	(0.005)	(0.012)	(0.011)	(0.013)	(0.015)
Wage ($100s)	0.733**	0.689**	0.819	0.801	0.943	1.165**
	(0.285)	(0.298)	(0.669)	(0.601)	(0.707)	(0.477)
Observations	13,860	13,860	11,278	11,278	11,278	10,849
R-squared	0.399	0.401	0.343	0.347	0.334	0.340
Two-way FEs	X	X	X	X	X	X

Standard errors in parentheses, clustered by state
*** p<0.01, ** p<0.05, * p<0.1

Notes: See tables 1 and E.2. Regression observations restricted to 2013-2019 and restrict to markets with at least 50 ads from 2010-12. Industry tightness is the number of jobs posted in BG, averaged over 2010-2012, divided by 100 times the number of unemployment workers who previously worked in the industry as measured in the ACS in the same time period.

Table E.4: Event Study Coefficients: Log Employment (QCEW)

Dependent Variable:	Log Employment (QCEW)			
	Full Sample		NN Sample	
	(1)	(2)	(3)	(4)
t-5	-0.109	-0.124	-0.028	-0.027
	(0.011)	(0.010)	(0.008)	(0.008)
t-4	-0.083	-0.092	-0.025	-0.025
	(0.012)	(0.010)	(0.007)	(0.007)
t-3	-0.048	-0.055	-0.013	-0.013
	(0.010)	(0.009)	(0.006)	(0.006)
t-2	-0.014	-0.017	-0.005	-0.005
	(0.008)	(0.008)	(0.005)	(0.005)
t-1	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
t=0	0.031	0.032	0.064	0.064
	(0.022)	(0.022)	(0.004)	(0.004)
t+1	0.086	0.088	0.100	0.100
	(0.032)	(0.031)	(0.007)	(0.007)
t+2	0.109	0.112	0.129	0.128
	(0.039)	(0.039)	(0.009)	(0.009)
t+3	0.119	0.125	0.146	0.146
	(0.042)	(0.041)	(0.011)	(0.011)
t+4	0.125	0.132	0.158	0.158
	(0.043)	(0.042)	(0.013)	(0.013)
t+5	0.137	0.145	0.168	0.169
	(0.041)	(0.040)	(0.014)	(0.014)
Firm FE	X	X	X	X
Year FE	X		X	
Sector-Year FE		X		X
R-squared	0.8891	0.8895	0.9216	0.9223
Observations	16,469,264	16,469,264	148,184	148,184

Notes: This table reports event study coefficients for regression specification 3 with log employment as the outcome. Column 1 and 2 use the full-sample control and cluster standard errors by state; Column 3 and 4 use the nearest neighbor matched control and cluster by matched pair. Odd columns correspond to event studies graphed in Figure 5 and even columns add two-digit industry by year fixed effects.

Table E.5: Event Study Coefficients: Vacancies

Dependent Variable:	# Posts (100s)			
	Full Sample		NN Sample	
	(1)	(2)	(3)	(4)
t-5	-0.091	-0.093	-0.027	-0.026
	(0.009)	(0.009)	(0.016)	(0.016)
t-4	-0.077	-0.078	-0.035	-0.034
	(0.019)	(0.019)	(0.020)	(0.020)
t-3	-0.060	-0.061	-0.031	-0.031
	(0.012)	(0.012)	(0.014)	(0.014)
t-2	-0.025	-0.025	-0.014	-0.014
	(0.012)	(0.013)	(0.009)	(0.009)
t-1	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
t=0	0.035	0.035	0.021	0.021
	(0.008)	(0.009)	(0.008)	(0.008)
t+1	0.066	0.067	0.044	0.044
	(0.017)	(0.017)	(0.013)	(0.013)
t+2	0.099	0.100	0.052	0.051
	(0.015)	(0.015)	(0.013)	(0.013)
t+3	0.135	0.137	0.059	0.058
	(0.036)	(0.036)	(0.014)	(0.014)
t+4	0.184	0.186	0.104	0.104
	(0.045)	(0.045)	(0.024)	(0.024)
t+5	0.197	0.198	0.088	0.087
	(0.041)	(0.041)	(0.023)	(0.023)
Firm FE	X	X	X	X
Year FE	X		X	
Sector-Year FE		X		X
R-Squared	0.644	0.644	0.654	0.656
Observations	15,062,242	15,062,242	131,855	131,855

Notes: This table reports event study coefficients for regression specification 3 with hundreds of postings in BG as the outcome. Column 1 and 2 use the full-sample control and cluster standard errors by state; Column 3 and 4 use the nearest neighbor matched control and cluster by matched pair. Odd columns correspond to event studies graphed in Figure 5 Panel B and even columns add two-digit industry by year fixed effects.

Table E.6: Event Study Coefficients: Composition of Vacancies in NN Sample

Dep Var:	Occupation Ad Share				Skill Requirement Ad Share				
	Prof	Sales	Serv	Prod	Coll	Any Ed	Exp	Cog	Comp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
t-5	0.011	-0.010	0.001	0.001	0.019	0.002	-0.028	0.011	-0.006
	(0.012)	(0.010)	(0.006)	(0.006)	(0.020)	(0.022)	(0.019)	(0.022)	(0.016)
t-4	0.024	-0.014	-0.004	-0.003	0.022	0.010	-0.019	0.012	-0.016
	(0.013)	(0.009)	(0.005)	(0.005)	(0.016)	(0.017)	(0.014)	(0.016)	(0.012)
t-3	0.025	-0.013	-0.003	-0.007	0.024	0.028	-0.007	0.013	-0.001
	(0.008)	(0.008)	(0.004)	(0.005)	(0.012)	(0.015)	(0.016)	(0.012)	(0.011)
t-2	0.017	-0.008	-0.002	-0.002	0.017	0.029	0.019	0.014	0.012
	(0.007)	(0.005)	(0.004)	(0.004)	(0.008)	(0.012)	(0.010)	(0.010)	(0.007)
t-1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
t=0	-0.007	0.004	0.000	0.000	-0.019	-0.005	-0.011	0.002	-0.004
	(0.007)	(0.006)	(0.003)	(0.004)	(0.010)	(0.011)	(0.015)	(0.009)	(0.008)
t+1	-0.005	-0.007	0.004	0.006	-0.012	-0.003	-0.023	0.019	-0.009
	(0.008)	(0.006)	(0.005)	(0.004)	(0.012)	(0.014)	(0.016)	(0.012)	(0.008)
t+2	-0.005	-0.002	0.005	0.005	-0.008	-0.008	-0.012	0.020	-0.010
	(0.009)	(0.006)	(0.006)	(0.005)	(0.011)	(0.013)	(0.016)	(0.012)	(0.009)
t+3	-0.007	0.000	0.008	0.000	-0.018	-0.018	-0.022	0.010	-0.010
	(0.010)	(0.008)	(0.007)	(0.005)	(0.011)	(0.014)	(0.015)	(0.013)	(0.009)
t+4	-0.023	0.007	0.016	0.002	-0.044	-0.027	-0.048	-0.004	-0.030
	(0.015)	(0.010)	(0.008)	(0.007)	(0.016)	(0.015)	(0.018)	(0.016)	(0.011)
t+5	-0.014	0.013	0.005	-0.006	-0.032	-0.019	-0.034	0.014	-0.013
	(0.013)	(0.011)	(0.008)	(0.011)	(0.015)	(0.018)	(0.018)	(0.016)	(0.013)
Firm FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
R-Squared	0.551	0.468	0.641	0.614	0.539	0.466	0.445	0.482	0.459
Observations	59,668	59,668	59,668	59,668	59,668	59,668	59,668	59,668	59668

Notes: This table reports event study coefficients for regression specification 3. Outcomes are the share of ads across each broad occupation group (cols 1-4) or the share of ads requiring the indicated skill (5-9). All are in the nearest neighbor matched sample, include firm and year fixed effects and cluster standard errors by matched pair. These correspond to the nearest neighbor event studies in Figures 6 and 7.

Table E.7: Event Study Coefficients: Log Wages in NN Sample

Dependent Variable:	Log Wages (QCEW)		
	All NN Obs	BG Sample	BG Sample
	(1)	(2)	(3)
t-5	-0.016	-0.010	-0.010
	(0.005)	(0.009)	(0.009)
t-4	-0.012	-0.008	-0.008
	(0.005)	(0.008)	(0.008)
t-3	0.003	0.007	0.007
	(0.004)	(0.006)	(0.006)
t-2	0.000	-0.002	-0.002
	(0.004)	(0.006)	(0.006)
t-1	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
t=0	0.001	0.002	0.002
	(0.003)	(0.005)	(0.005)
t+1	-0.004	0.001	0.001
	(0.004)	(0.006)	(0.006)
t+2	-0.008	-0.003	-0.003
	(0.005)	(0.006)	(0.006)
t+3	-0.009	-0.002	-0.002
	(0.005)	(0.007)	(0.007)
t+4	-0.010	-0.001	0.000
	(0.006)	(0.007)	(0.007)
t+5	-0.018	-0.008	-0.008
	(0.006)	(0.008)	(0.008)
Firm FE	X	X	X
Year FE	X	X	X
Occ Control			X
R Squared	0.9862	0.9904	0.9904
Observations	148,180	59,667	59,667

Notes: This table reports event study coefficients for regression equation 3 for log wages. All specifications control for log employment in the year. Column 1 is the nearest neighbor matched sample; column 2 restricts to observations that post in BG in the year; and column 3 adds controls for the proportion of ads across the 4 occupation groups in the year. These correspond to the nearest neighbor event studies in Figure 8.