Making public job training work: Evidence from decentralized targeting using firm input

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Job training programs are used by governments as a solution to underemployment and to improve the match between skill supply and skill demand. Targeting such programs with input from local employers has long been hypothesized as a way to improve their effectiveness. We test this hypothesis using a unique situation in Brazil in which two national skill training programs ran in parallel, with one taking local employer input in choosing course offerings while the other retained a traditional, government-led structure. The employer-informed program nearly doubled the short-term effect on trainees' employment and earnings relative to the traditional program. The differential effectiveness of the employer-informed program is not attributable to differences in course or student composition across programs. Our findings provide evidence that limited, structured input from the private sector can improve alignment between skills trained and skill demand and increase the employment and earnings of the underemployed.

Keywords: Skills, job training, technical training; training programs; labor demand; unemployment; Brazil. (JEL J24, J23, J31, J68, J62, M53)

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1 Overview

Objectives of active labor market policy are to help disadvantaged workers and to shape a workforce that matches the evolving skill demand of employers. With the potential to address skill shortages and improve the employment of job seekers, publicly funded job training programs are implemented in many countries despite a sizable literature providing only mixed evidence of effectiveness (Card, Kluve, and Weber (2015), Kluve (2010), and Heinrich et al. (2013), among others). Betcherman, Olivas, and Dar (2004) summarize the findings from over 150 evaluations of active labor market programs and suggest cautious expectations for what such programs can "realistically achieve."

Low program effectiveness can be caused by negative selection of trainees, low quality course content, or – the focus of this paper – a mismatch between skills trained and skill demand. A common proposal to address this form of mismatch is to involve the private sector in the design or administration of skill training programs. This principle has a long history, dating at least to the 1982 Job Training Partnership Act (JTPA) in the U.S. (Barnow and Smith, 2015; Orr, Bloom, Bell, Doolittle, and Lin, 1996), and has seen recent renewed interest both domestically and internationally (Foroohar, 2017; Srinivasan, 2017; World Bank, 2013). While it is believed that employers' involvement can reduce mismatch between skills trained and skill demand, it has not been established whether this happens in practice, or whether reduced mismatch can improve program effectiveness. In particular, among credible evaluations of employer-informed training programs, Attanasio, Kugler, and Meghir (2011) and Attanasio, Guarín, Medina, and Meghir (2017) point to the role of employers as a major factor behind the strong employment effects of a recent training program for youth in Colombia. In this or other contexts, however, there has not been an opportunity to compare an employer-informed program to an alternative, traditionally structured program occurring in the same institutional and economic context. Researchers are thus left to speculate about the precise reasons for higher effectiveness, as it has thus far remained difficult to empirically isolate the role of employer input from other unique contextual factors.

In this paper, we investigate the employment and earnings returns to a large-scale, publicly administered technical skills training program for unemployed workers in Brazil. This program took informational input from firms across the country in determining course offerings, which consisted of only three dimensions: the skill desired (chosen from an existing "menu" of courses), the locality in which to hold the training, and the number of people to train. The unique feature of our context is the existence of an otherwise-similar and contemporaneous national skills training program operating within the same institutional setting and carried out by the same providers that did not take input from firms. Importantly, courses in both segments were made available to potential registrants without observably distinguishing their original provenance. We empirically identify causal effects on employment an earnings using restrictions in space availability due to course oversubscription that permeated course offerings in both program segments. Our identification is based on plausibly exogenous assignment of the ability of individuals to enroll in and attend the course for which they registered, while others who registered for the same course were not allowed to attend because of class oversubscription and capacity restrictions.

Using comprehensive administrative monthly panel data capturing both formal and small-scale employment, reduced form estimates imply that individuals who received a course offer in the employer-informed program had a 3.5 percentage-point higher employment rate than non-offered individuals in two years following the course. This statistically different from the magnitude estimated for the traditional segment – 1.9 percentage points. IV estimates yield a LATE of approximately nine percentage points in the informed segment, and five percentage points in the traditional program. These effects are driven entirely by formal employment, as effects on small-scale activity are minimal.

We then show that differences across programs are not due to a direct alleviation of search frictions, as the trainees find employment at firms other than those that requested the training course from the government. This suggests that the course requests supplied by the private sector were indicative of general, rather than firm-specific, skills shortages. We also show that effects are attributable to trainees finding employment in occupations and industries in which they had not previously worked, and that the difference in effectiveness between programs programs is largely driven by employment among large firms.

Even though the undifferentiated manner in which courses appeared to potential registrants reduces concerns about overt or opportunistic selection, the results may still be attributable to compositional differences across programs. For example, if the informed segment simply shifted the course distribution towards courses that were more effective in generating employment (irrespective of the segment they were offered under), then fully parameterizing course-specific effects would reduce the gap in effectiveness seen across programs. Similarly, if courses offered in the informed segment organically attracted higher quality trainees, parameterizing heterogeneous treatment effects on trainee quality would similarly reduce the gap in effectiveness across programs. Allowing for the characterization of such selection in generating differences in effectiveness could lead to important differences in policy implications: effectiveness driven by course composition is different from effectiveness driven by attracting a subset of higher quality trainees. In implementing these tests, we show that controlling for heterogeneous treatment effects in course and trainee dimensions does not attenuate estimated differences across programs. We conclude that the employer input indicated localized, general skill shortages shared by numerous firms, rather than effectiveness derived through changes in the aggregate distribution of courses or the quality of trainees.

Our work contributes to two significant and often disparate topics in labor and development economics. First, we add to the understanding of how to improve large-scale, government-run job training programs. This literature began with the evaluation of job training programs in the U.S. (see Ashenfelter, 1978; Ashenfelter and Card, 1985; Heckman and Hotz, 1989, for some of the earliest studies)[1]; we add specifically to more recent studies of the efficacy of job training programs in developing countries (*e.g.,* Chakravarty et al., 2019; Attanasio et al., 2017, 2011; Hirshleifer et al., 2016; Card et al., 2011, among others). Despite an active literature evaluating job training programs in developing countries, none to date has explicitly tested an employer-informed design relative to a comparable, traditional government-led design.

Second, we add to a strand of work in development economics that studies whether input from non-governmental entities can improve the targeting of social welfare programs, broadly defined. This includes recent evaluations of small-scale entrepreneurship programs (Hussam et al., 2017) and antipoverty programs (Alatas et al., 2016, 2012), as well as early theoretical and empirical work by Galasso and Ravallion (2005). Given the popularity of government-sponsored job training programs and their frequently low effectiveness, our study fills a clear gap by answering whether decentralizing the process to match skill supply with skill demand can meaningfully improve the return to such investments.

The following section reviews the relevant literature. We then provide further details on the programs studied, and we describe the administrative records we used to link the course requests, classes held, and student records to monthly social security records covering the formal and small-scale employment in Brazil. We then discuss our empirical strategy and present results. We examine why the program exhibited greater effectiveness in generating employment among trainees, and conclude with observations on the program and thoughts for future work.

[1]We refer the reader to Barnow and Smith (2015) for a comprehensive review.

2 Related Literature

Both firms and workers have numerous reasons for underinvesting in technical skills (Acemoglu, 1997; Acemoglu and Pischke, 1998, 1999a,b). Although employers and individuals may know what skills are in demand, competitive labor markets or credit constraints may reduce incentives or capacity to invest in skills, with such situations providing scope for publicly funded investment in general skills. However, evidence of the effectiveness of recent skills training programs remains quite mixed. In a meta-analysis of nearly 100 studies, Card, Kluve, and Weber (2010) conclude that skills training programs usually generate employment gains only in the medium and long term, and that many programs are ultimately ineffective in reducing unemployment.

A number of recent studies in non-OECD countries have found that the effectiveness of publicly provided skills training varies widely. Positive employment effects have been found among programs in Peru (Ñopo, Robles, and Saavedra, 2007; Díaz and Rosas Shady, 2016), Colombia (Attanasio, Kugler, and Meghir, 2011; Attanasio, Guarín, Medina, and Meghir, 2017), Liberia (Adoho, Chakravarty Jr., Korkoyah, Lundberg, and Tasneem, 2014), Nepal (Chakravarty, Lundberg, Danchev, and Zenker, 2015), Malawi (Cho, Kalomba, Mobarak, and Orozco, 2013), Kenya (Honorati, 2015), and Brazil (Reis, 2015). However, other programs have shown negligible impacts, including programs in Argentina (Alzuá and Brassiolo, 2006), Germany (Caliendo, Künn, and Schmidl, 2011; Lechner, Miquel, and Wunsch, 2011), Dominican Republic (Card, Ibarrarán, Regalia, Rosas-Shady, and Soares, 2011), Kenya (Hicks, Kremer, Mbiti, and Miguel, 2013), and Jordan (Groh, Krishnan, McKenzie, and Vishwanath, 2016) and in an RCT in Turkey (Hirshleifer, McKenzie, Almeida, and Ridao-Cano, 2016).[2] Some degree of the variation in effectiveness could arise from heterogeneity in programs across contexts; there is no agreed-upon effective training program structure as

[2] For earlier reviews, see Betcherman, Olivas, and Dar (2004) and Kluve (2010); for a review of Latin American training programs see Ibarrarán and Rosas (2009).

yet, and few, if any, programs have been rigorously evaluated in a context that is conducive to comparing effectiveness across different designs.

In the case of public sector-sponsored skills training, the government's goal is typically to improve the earnings of disadvantaged workers. Achieving this goal relies in part on aligning training content to skill demand. The inherent problem in this situation stems from the fact that, while firms likely know their projected skill demand, the government does not have the means to readily and accurately access this information in designing its offerings. The design of training programs can therefore fall between two extremes of private sector involvement. On one extreme, private firms can be allowed to prescribe course offerings and content. This circumstance, however, would create perverse incentives for firms to exploit public resources for their sole benefit. At the other extreme, and as has typically been done in workforce training programs and is still common today, the government takes full responsibility for determining the content of training.

Between the two extremes fall many of the incarnations of workforce training programs that partner with the private sector. One of the better known and studied of these types of programs was the U.S. Jobs Training Partnership Act (JTPA), in which the private sector was explicitly provided a role in federally sponsored training through private industry councils that served administrative and managerial roles for local programs (Orr, Bloom, Bell, Doolittle, and Lin, 1996). The U.S. Jobs Corps similarly involved the private sector in the vocational training offerings (Schochet, Burghardt, and McConnell, 2008), and the recent Workforce Innovation and Opportunity Act (WIOA) similarly prescribed that state and local overseeing bodies be comprised of a majority of members from the private sector (Barnow and Smith, 2015). Government collaboration with private firms in the administration of training programs is not unique to the U.S., however, and has been seen in India, the U.K., Australia, and Nordic countries, among others. Despite the popularity of the principle, the degree to which private sector involvement affects training programs has not been clearly disentangled from other dimensions of design.

A revived debate focuses on whether programs can be formulated to more closely involve the private sector in determining which skills to provide (World Bank, 2013). Mixed results have emerged from causal evaluations of vocational training provided by more recent programs that offer training through private providers and/or combine coursework with an internship or work experience at a private firm. Attanasio, Kugler, and Meghir (2011) and Attanasio, Guarín, Medina, and Meghir (2017) show that training in Colombia improved employment and wages in the short and long run. In contrast, results from a program in the Dominican Republic indicate variable effects on employment and only very modest effects on earnings (Card, Ibarrarán, Regalia, Rosas-Shady, and Soares, 2011; Acevedo, Cruces, Gertler, and Martinez, 2017). Relatedly, Corseuil, Foguel, Gonzaga, and Ribeiro (2012) evaluate an apprenticeship-based youth employment program in Brazil and find that apprentices have a higher probability of getting a formal job in the years after the program.[3]

The studies cited so far argue that the work experience component obliges course providers to offer training in skills for which a specific demand exists, which likely leads to greater effectiveness of these programs. However, private sector involvement (in whatever form realized) is a singular feature of any of these programs, and a design using private sector involvement cannot be compared to otherwise similar traditional programs. Moreover, the evaluation literature has been limited due to the way in which government programs with multiple dimensions of new features often replace their predecessors entirely, making it difficult to credibly compare programs and their constituent components (Ashenfelter and Card, 1985). This study is the first, of which we are aware, that is able to compare the effectiveness of an employer-informed program to a traditionally structured, government-led training program. Both programs were administered by the government, provided by the same quasi-public institutions, and run at the same time. From 2014 to 2015, the employer-informed segment

[3]Recent work on other types of programs, notably Hussam, Rigol, and Roth (2017), finds that community-based private information can be useful in targeting a small-scale entrepreneurship program.

registered around 40,000 trainees, while the traditional segment registered approximately one hundred thousand individuals. This scale allows us to investigate and empirically characterize dimensions from which effectiveness derives. In the following section, we describe the institutional context and design of the program that is the focus of this study, and we then detail the analytical approach.

3 Background and program context

In 2011, the federal government of Brazil created the National Program for Access to Technical Education and Employment (*Pronatec*). The program was launched with the goal of raising the earnings and employability of lower-income segments of the workforce through participation in the formal labor market. The program enrolled individuals in technical skills training courses that have a moderate in-class training component (less than 500 hours) and teach skills relevant to a particular occupation. An existing national provider network had the capacity to offer any of a set menu of courses, ensuring a level of consistency in course objectives and content across providers and instances of course offerings.

The focus of this paper is a sub-segment of *Pronatec* which was partially administered by Brazil's Ministry of Industry, Foreign Trade, and Services (hereafter, "the Ministry"). The unique feature of this program is its "demand-driven" nature: the Ministry solicited and received requests for specific skills training courses from individual businesses. This program began with a limited number of training courses in 2013, greatly expanded in 2014, and scaled back in 2015 due to federal budget constraints. In 2014, more than 2,000 firms applied for more than 16,000 skills training courses across a wide range of industries and occupations. In the following section, we discuss the data sources used to analyze the effects of the program.[4]

[4] To fix concepts, a "course" refers to a specific set of material or concepts that teach or build skills

The Ministry received course requests through a standardized process in which firms indicated the skill, occupation, or course they desired in their locality, along with the number of "seats" (individuals trained). These requests had no pecuniary cost to the requesting firms, nor did the firms make any explicit commitment to hire any trained workers. Upon receipt, the requests went through a screening by Ministry staff in terms of their viability and appropriateness.[5] Approximately one half of firms' requests were denied at this stage. Some firms chose to reapply with a lower number of seats requested, while others did not.

We were provided a comprehensive listing of course requests received as part of this process. Each of these requests includes the number of people the requesting firm wanted trained as well as the name of the company or organization submitting the request, its tax ID, the course requested, the official occupation code corresponding to the course, and the municipality in which the course was requested.

Panel A of Table 1 shows that there were 16,782 course requests in 2014. Of these, approximately half (8,340) were approved by the Ministry. The average number of seats requested in a given course was approximately 38; the average number of seats in approved courses was only slightly higher at 43.6. 17 percent of course requests were made either by industry or workers' associations. Among the firm requestors, we are able to match 97 percent to administrative employment records (described below) based on either the tax ID or the combination of firm name and municipality.

Because of the long-standing and institutionalized nature of public sector-sponsored skills

needed for a particular occupation, *i.e.*, a course in welding. The set of courses available to offer is codified administratively, and any instance in which a given course is offered must cover the same content and will have the same objectives. When the specificity is required, refer to a "class" as a specific instance of a course being offered in a particular municipality and time, and multiple distinct classes of the same course can be held concurrently in the same municipality. Depending on the context, "course" and "class" can be used interchangeably. We also refer to the entire sample of individuals we consider in the analysis as "registrants," only some of whom were "enrollees" (or, alternatively, "attendees").

[5]In correspondence with staff involved in administering the program, we were informed that the review process ensured a reasonable volume of seats were requested relative to a firm's scale and projected needs. If found excessive, course requests would be denied (as opposed to adjusted) to discourage firms from seeking training that significantly exceeded their projected needs.

training in Brazil, the courses available to choose from constituted a "menu" of courses that (a) were determined prior to the start of the specific program at hand, and (b) corresponded to specific occupations in the Brazilian occupational classification system. The top five requested technical courses were for industrial electricians (5.6 percent), computer operators/technicians (3.9 percent), low-voltage electrical technicians (3.2 percent), production controllers (2.5 percent), and industrial mechanics (2.4 percent).

Requests approved by the Ministry were then forwarded to the Ministry of Education, which is the body responsible for the overall administration of *Pronatec*. The Ministry of Education aggregates course demands across ministries, and this aggregation is further screened according to course viability (i.e., having the minimum number of seats to hold a course in a municipality requested across ministries), technical criteria, and budget availability. The Ministry of Education is also responsible for selecting training providers and for tracking the registration of students. There are a number of providers that offer *Pronatec* courses, although the majority are offered by Brazil's *Sistema S* – which in principle ensures a certain homogeneity in the trainings provided.[6] The training providers all receive identical reimbursement from the Ministry of Education at a rate of around 10 Reais (approx. 4 USD in 2014) per student-class hour.

The essential difference between the employer-informed segment and the rest of *Pronatec* is that the choice of courses to offer is determined by direct input from firms and employers. Course requests from other ministries originate through various processes within the requesting ministries without formal consultation with local employers. Panel B of Table 1 presents summary statistics on the firm-requested courses. The average course size (conditional on being approved) had 13 seats.[7] That is, based on the average number of seats per firm request, several classes would be held to fill a single course demand. The average class

[6] *Sistema S* is an amalgamation of quasi-governmental organizations in Brazil that administer low-cost or free professional training courses at schools and learning centers throughout the country.

[7] Note that more classes are offered than were requested; requests were typically two to three times larger than class sizes available.

ran for 200 course hours, met for approximately eight hours per week, and lasted between five and six months.

A major benefit of the Brazilian context is that it enables us to analyze an otherwise similar national skills training program that did not take input from firms in allocating skills training. Instead, course offerings are determined with input from municipal bodies, social assistance centers, and unemployment insurance (UI) centers. This segment, which was implemented in the same context by the same ministries and course providers, existed simultaneously with our focal program. Over the study period, the main *Pronatec* program was more than twice as large as the employer-informed segment, serving over 100,000 trainees for whom we can also observe complete employment and wage histories in the administrative data.

Courses in the traditional segment were offered and listed in registration systems by the same providers alongside the firm-requested courses. Furthermore, the firm-requested courses were never outwardly advertised as such to applicants, nor were they known to providers to have their provenance from this channel. Consequently, we undertake a parallel analysis on all *Pronatec* trainees outside the firm-requested courses who registered for a class held in 2014 or 2015 (i.e., the same period during which the employer-requested courses were held). In the analysis, we focus on individuals who registered for training due to unemployment insurance (UI) requirements: in Brazil, UI recipients are required to register for a *Pronatec* training course as a condition of receiving unemployment benefits. Courses were open to other students, such as those supplied by the requesting firms themselves or self- or casual registrants, but these segments are of lesser generalizable policy focus. Figure 1 contains a conceptual mapping of the registration process.

Because all records contain unique national identification numbers, we are able to link the students in training courses in either segment with nationally comprehensive administrative data from the Ministry of Labor containing information on all formal sector employment as well as the system for independent contributions to the social security system for those employed in small, informal firms. In the following section we describe the employment data and how we linked records.

3.1 Monthly formal and informal employment records

Our formal sector employment data come from the Relaçao Anual de Informações Sociais (hereafter RAIS). RAIS is an annual administrative dataset containing employment and earnings information primarily collected for administering social welfare programs such as unemployment and retirement benefits. Our data contain full details on the monthly employment and wage earnings of all formally employed workers in Brazil from calendar years 2013 to 2015. RAIS contains employer-reported records of a worker's hours, earnings, and hiring/dismissal dates and reasons (if applicable), as well as the firm's industrial classification, the worker's occupational classification, and the worker's gender, education level, and age. The data importantly contain unique identifiers for both workers and employing firms, which are the standard identification provided to and used by firms and workers for their various interactions with the government. These fields are used to link the student information to their employment records as well as to identify employment at requesting firms versus other businesses. We deflate earnings to June 2012 real values using a standard monthly consumer price index (IBGE, 2016).[8]

To gauge the impact of the program on informal and self-employment, we use data on monthly contributions to social security by small-scale entrepreneurs and their employees. Known as *Micro Empreendedor Individual* (hereinafter "MEI") contributions, this system is for those owning or working in a business with sales smaller than R$81,000 per year. Established in 2008 with the objective of creating a stepping stone for self-employed peo-

[8]For workers who have multiple records within a given month or worked only part of the month (based on precise hiring and firing dates), we add all deflated earnings across jobs and construct a monthly wage rate based on the share of the month worked.

ple to transition out of informality, entrepreneurs or employees make small, fixed monthly contributions (R$47.70) in order to obtain coverage for themselves and their dependents by social security benefits.[9] We obtain these data from 2010 to 2016, and link them to the main administrative records in order to provide a fuller picture of employment effects across both the formal and informal sectors.

Table 2 contains summary statistics on the individual-level panel dataset used for analysis. The administrative data contain more than 40,000 unique individuals who registered for any of the firm-requested courses (Panel A). 57.5 percent of this sample is male, and 32 percent of registrants enrolled in the training course for which they registered. Approximately 16 percent of this sample was denied a seat due to capacity constraints, 87.9 percent of all students were in a class that had at least one registrant not enroll in the course due to these reasons. While the average employment rate was 59.5 percent, a trivial fraction of the person-month records had employment in one of the requesting firms (less than one percent). The average employment rate of 59.5 percent is made up of a majority of formal employment (56.8 percent of person-months) and a small, but non-trivial minority of informal employment (4.3 percent; note that employment in a given month is not necessarily mutually exclusive to a single sector). The mean real monthly earnings (excluding months unemployed) was approximately R$388. In the traditional program (Panel B), there are more than 90,000 unique registrants, 55.6 percent of whom are male, had a similar enrollment rate (31.5 percent), and had similar employment rates (58.5 percent) and wage rates (R$365/month). In the analyses below, we estimate effects on any type of employment, as well as on the separate measures of formal and informal employment for these registrants.

[9] A non-negligible fraction of the informal sector is captured in this system: from 2009 to 2018, the number of individuals contributing under this system grew steadily, reaching 6.9 million people (6.6 percent of the Brazilian working age population). As comprehensive data on informal work at a national scale and linkable to individuals is not available, we view the MEI contributions data as partially capturing dynamics of the informal labor market.

4 Empirical strategy and its validation

4.1 Empirical approach

To estimate the effects of receipt of job training under either the employer-informed or traditional segment, we match program data on students and classes to comprehensive monthly social security records. The longitudinal nature of the administrative employment records allows us to fully control for individual-specific unobservables and implement an empirical strategy based on quasi-experimental variation in assignment that is uncorrelated with pre-course observables or trends. Below, we describe the structure and timing of the program and the empirical strategy used.

We analyze students who register for classes held in 2014 and 2015, during which time both the employer-informed and traditional programs were active. To estimate effects of course attendance, we exploit differential course timings and the detailed information on the reasons why students did or did not enroll in the course for which they registered. We begin by estimating the effects of course enrollment on employment, conditional on vectors of individual and time fixed effects. These estimates are then used to discuss the likely bias introduced in this approach, and motivate the empirical strategy addressing the endogeneity of course attendance.

A typical concern in the econometric analysis of job training programs is the construction of the counterfactual group, because it is well-documented that the timing of the start of skills training is related to time-variant unobservables – in particular, recent unemployment spells (i.e., the "Ashenfelter Dip"). In our context, however, the essential comparison is between groups of individuals who registered for the same offering of a specific training course but either did or did not attend the course.

For any individual, the monthly employment detail spans three time periods relative to when the course registered for was held: prior to the course start, during the course, and after the course ended. The structural equation for the difference-in-differences specification is then:

$$Y_{ict} = \beta_0 + \beta_1 * course_{ict} + \beta_2 * postcourse_{ict} + \beta_3 * course_{ict} * Took\ course_i + \beta_4 * postcourse_{ict} * Took\ course_i + \lambda_i + \gamma_t + u_{ict} \quad (1)$$

In equation 1, i indexes individuals registered for class c whose employment is being observed in month t. β_1 and β_2 capture aggregate level differences in employment in the course and post-course periods (relative to the pre-course period), β_3 captures the "during course" effect of course attendance on employment, and β_4 gives the focal difference-in-differences estimator of the course attendance on the outcome. The vector of individual fixed effects in λ_i absorbs individual-level unobservables (as well as location and classroom effects, and the "main effect" of taking the course) and γ_t controls for common (monthly) shocks to the labor market. We then correct for within-class correlations in the error term (i.e., across students taking the same course in the same place for the same period; >15,000 classes/clusters) and for potential aggregate correlations by month t (72 clusters). The sample covers all months from 2010 to 2016, and we estimate equation 1 via OLS. We also estimate outcomes for an employment indicator and deflated earnings for person i in month t separately across programs and registrant subsamples.

Table 3 contains coefficients from the OLS estimation of equation 1 for employment and earnings in Panels A and B, respectively. In both programs, we find a small negative effect of course enrollment on employment outcomes and earnings following the course. Patterns across programs are relatively comparable in magnitude and direction across measures of employment. Among registrants in both programs, the observational effect even appears negative: those who took the course are *less* likely to be employed in the post-course period than those who did not.

The identifying assumption in this specification, however, is that course attendees and non-attendees experienced common shocks in the months prior to the start of the course and would have exhibited similar employment patterns in the absence of treatment. The approach assumes that the endogeneity between course enrollment and later employment is entirely due to time-invariant individual-level unobservables. This assumption almost surely violated because unobservable job offers are likely to affect affect whether an individual enrolls. That is, some individuals may receive acceptable employment offers prior to the course that cause them to forgo attending the course. Since these offers are not observable, increase employment, and are negatively correlated with course attendance, it is then no surprise that the above approach likely generates a downward-biased estimate of the effect of training on employment.

To address the endogeneity of enrollment, we use detailed information in the administrative records on the reasons why students did not attend the courses they registered for to identify students who were prevented from attending a course due to exogenous reasons. In general, an individual might not be able to attend a course for which he or she registered either for personal reasons (which result in a recorded status equivalent to "quit prior to the course" or "no-show"), or for administrative reasons outside the control of the individual registrant. These typically come about due to class oversubscription resulting in a lack of seats or space limitations that do not permit all registrants to enroll.[10] In the case of course oversubscription, which underlies the vast majority of course offer restrictions, the segment of unemployment beneficiary registrants was typically used to make space for other "priority" groups registered through other channels (namely, the firm-supplied registrants). This process was done on a first-come, first-served basis, although the administrative program data did not retain sufficient detail to observe the precise cutoff for course offer receipt. Because trainees were often registered in batches, the cutoff would often be made in the

[10]We confirmed with the Ministry of Education that the record codes used to identify students prevented from attending corresponded to reasons for non-attendance that were outside the control of trainees themselves; *e.g.*, *force majeure* cancellations, seat reductions, or class oversubscription.

middle of the UI registrant batch, which was not ordered in any particular way within the registration system. We then use the information on these administrative restrictions to indicate whether a registrant received a "course offer" – taking the value of one for those who had registered for a class and were not restricted administratively from attending the class. Because the analysis is split into three periods relative to course start and end, we then interact the instrument separately with indicators for the during-course and post-course periods. The structural equation remains as in equation 1, where the two endogenous variables, $course_{ict} * Took\ Course_i$ and $postcourse_{ict} * Took\ Course_i$ are instrumented with $course_{ict} * Offer_i$ and $postcourse_{ict} * Offer_i$. (Note that the main effects of $Offer_i$ are absorbed in the individual fixed effects.) Because this approach allows us to recover internally valid estimates within each program, we keep the main analysis separated by the employer-informed and traditional segments.

To assess the validity of this approach, we consider the observable trends in employment for registrants in the two program segments relative to the timing of their class. In Figures 2 and 3, we plot the mean employment rates in either program based on whether or not the registrant received a course offer for the 18 months on either side of registrants' class start date. This allows us to do several things: visually confirm expected trends related to pre-program employment patterns, assess the viability of the parallel trends assumption based on pre-course trends, and gauge the likely direction of the reduced-form effect comparing net differential employment rates after the course between those offered a course seat versus those that did not receive an offer. The figures illustrate some immediately apparent patterns regarding employment and training. First, the employment rate of program participants in the month in which they start their course is at its lowest point in the preceding months, confirming that a substantial share of individuals registering for the program experienced a job loss in the year prior to the start of their course – commonly known as "Ashenfelter's dip." We can also confirm that the pre-course trends for the two groups appear parallel, if not highly overlapping. In Figure 2, we see that registrants were nearly all employed six months prior to their course, which then drops precipitously to approximately a 15 percent employment rate in the month in which the course begins. These pre-course patterns are similar for those receiving a course offer and those not within each program, as well as being of highly similar magnitudes across programs. This analysis additionally allows us to gauge the likely trend in program effects over our study horizon, suggesting that effects begin shortly after the course ends and persist through to the end of our sample period.

4.2 Test of parallel trends

As in any application, we are concerned with the parallel trends assumption underlying the difference-in-differences estimator – that is, that trends across recipients and non-recipients of an offer were parallel in the pre-course period and would have remained so in the absence of the course offer. We test the first part of this assumption (whether parallel trends existed prior to the course) by limiting the sample to all pre-course observations and estimating a slope coefficient on the number of months relative to the course and a slope differential for offer recipients, via:

$$\begin{aligned} Y_{ict} = \alpha_0 + \alpha_1 * months\ relative\ to\ course_{ict} \\ + \alpha_2 * months\ relative\ to\ course_{ict} * offer_i + \lambda_i + \gamma_t + e_{ict} \end{aligned} \tag{2}$$

In equation 2, α_2 gives the slope differential for offer recipients' pre-course employment trends. In this case, a positive coefficient would raise concerns about upward bias in the resulting estimates (and vice versa for a negative coefficient). The coefficients from this estimation across employment outcomes are in the top panels of Table 4. In Panel A, the estimated slope differentials are all small and statistically insignificant at conventional levels for the employer-informed program; similar is found in Panel B among registrations for courses in the traditional program. Similar patterns exist for earnings (Panels C and D).

The precision afforded by the sample allows us to reject slope coefficients as small as 0.001 – a magnitude itself not large enough to present meaningful concern for the estimates below.[11]

5 Results

5.1 Reduced form estimates

We next estimate a reduced-form equation, given by:

$$Y_{ict} = \beta_0^{'} + \beta_1^{'} * course_{ict} + \beta_2^{'} * postcourse_{ict} + \beta_3^{'} * course_{ict} * Offer_i + \beta_4^{'} * postcourse_{ict} * Offer_i + \lambda_i^{'} + \gamma_t^{'} + u_{ict}^{'} \quad (3)$$

where variables, subscripts, and standard error clusterings are as above. Estimated coefficients for $\beta_4^{'}$ for employment and real monthly earnings are in Table 5.

In Panel A, we find a positive effect of an enrollment offer on the post-course employment rate of approximately 3.4 percentage points in the informed program, which is entirely comprised of an increase in formal employment (Column 2). This is in contrast with effects in the traditional program, which were about half of the magnitude (Panel B), with difference in the two magnitudes statistically significantly different from zero in both Columns 1 and 2. Results in Panels C and D confirm a similar pattern of results for real monthly earnings, with the magnitudes in the informed program approximately double, and statistically difference from, those in the traditional program. In terms of magnitudes, these effects are relatively consistent with the existing literature which suggests likely small effects on employment and earnings within such a short time frame (up to two years post-course).

[11] In Section 5.4 below, we provide an alternative strategy that addresses concerns with applying this typical difference-in-differences specification to the discrete employment outcome.

5.2 Effects on subgroups

In Figure 4 we present reduced form effects on any employment for both program segments estimated separately by sex, education level, and region. Coefficients magnitudes are plotted on the X axis, with the specific subsample indicated along the Y axis; we include 95% confidence intervals and points are proportionate to the share of the subsample within each program. From this, we aim to gauge whether the effects might be driven by the composition of course offerings or students in them, or whether the informed program exhibits larger employment effects across subsamples. We find that the aggregate difference across program segments is not driven by a compositional effect, but by differential effectiveness within subsamples – particularly concentrated among men and trainees with at least a middle school education. Finally, the employer-informed program was more effective in four out of five regions of the country, although these differences are not statistically meaningful.

5.3 Tracing the source of employment gains

By design, the decentralized design required interaction between the government and firms. Due to this interaction, employers may be more aware of provisioned courses for skills they demand and the timeline on which trainees become available. We are thus not able to explicitly separate the role this interaction may have had in reducing search frictions. It is, however, possible to test for evidence of this hypothesis by exploiting the employee-employer data. We first identify and separate employment into that specifically at firms who interacted with the government in requesting training courses ("requesting firms" or "demanders") versus that at all other firms. We then estimate these mutually exclusive measures of employment for trainees, with effects derived primarily from requesting firms presenting a concern that search frictions play a role in increased effectiveness.

In Table 6, we find the opposite to be the case: in the employer-informed segment, the majority of employment gains for individuals taking the course were realized in non-requesting firms. This finding provides evidence against the search frictions hypothesis, and it furthermore suggests that the skills requested were indicative of general, local skills shortages among several employers.[12]

5.4 Alternative specification: cross-sectional reduced forms by month

Linear models with fixed effects are not necessarily ideal for estimating Bernoulli outcomes in a difference-in-differences framework, particularly in the context of training programs, as the parallel trends requirement is not necessarily satisfied by the inclusion of a vector of additive controls. An alternative, as in Card and Sullivan (1988), is to estimate discrete choice models in the cross-section while controlling for lagged dependent variables. The specification we employ estimates the employment status of individuals as of month j relative to the start of their course, and includes a vector of controls for the individual's lagged outcomes in each of the 12 months preceding the course start. The probit specification is given by:

$$Y_{i,t=j} = \Phi[\beta_0 + \beta_1 * Offer_i + \sum_{t=-12}^{0} \beta_t * Y_{i,t}] + u_{i,t=j} \tag{4}$$

where Φ denotes the logistic function, and variables, subscripts, and clustering are as above. Figure 5 plots the logit model coefficients for estimations of $j \in [0, 18]$, and Figure 6 plots the linear probability analog. In either Figure, we reach the same conclusion: flexibly

[12]This may be partially explained by the fact that "lead" firms would submit requests on behalf of smaller, local suppliers – providing further evidence that the courses requested were indicative of general skill shortages experienced by several firms, although it raises further questions about search frictions for these firms. In ongoing work, we are collecting this information to determine whether employment effects came from same industry competitors or downstream suppliers.

controlling for employment in each of the 12 months prior to the course start does not affect our conclusion of increase effectiveness of the informed program. As might be expected from Figures 2 and 3, we see a trivial effect of an offer during the course period (in the first six months), after which effects of offer receipt begin to materialize for both traditional and informed program segments. We see the largest differential between programs in the seven to 14 months after the start of the course, although a difference in the latter periods is maintained, as is our overall conclusion that the employer-informed program increases trainees employment rate more than the traditional program.

5.5 IV estimates

We next estimate the local average treatment effect of course enrollment on employment and earnings using the course offer as an instrument for endogenous enrollment (take-up). First-stage coefficients from the estimation of the focal endogenous variable, $postcourse_{ict} * Took\ course_i$, are in Table 7. The coefficient effectively reflects the enrollment rate among those offered a seat in the course (*i.e.*, compliance), which is around 38 percent in either program. Supporting an absence of endogenous selection across programs, these magnitudes are sufficiently similar so as to not be statistically distinguishable across programs.

Second-stage coefficient estimates for employment and earnings are in Table 8. The local average treatment effect (LATE) for the informed program is 8.9 percentage points (Panel A, Column 1), which is, as expected, nearly double the magnitude of the traditional program, and represent a 15 percent improvement over the mean employment rate of 59.5 percent.[13] Effects on earnings in the informed program are more than double those in the traditional program, and reflect a nearly 30 percent increase in earnings.

[13] Relative to the literature, these effect sizes are above average; it is important to note, however, that identifying variation comes from students in courses that had sufficient demand to be oversubscribed. If student demand is at all an indicator of the quality of courses or the employment prospects they generate, we can reason that these estimates will be larger than those for the entire course distribution.

6 Explaining program effectiveness: controlling for course and student selection

6.1 Course selection hypotheses

Why did the employer-informed program exhibit such substantially larger employment effects than the traditional program? Our primary hypothesis is that the input from employers contained meaningful information that reallocated training resources to be better aligned with future skill demand. The content of this input and its relationship to program effectiveness remains to be empirically characterized, however. For example, employers may have reallocated trainings towards occupations or regions they expected to experience greater growth in aggregate in the coming years. Or, employers may have effectively targeted training towards particular labor markets (in our context, an occupation-locality pair) with expected demand growth and resulting skill shortages. These mechanisms suggest specific, empirically testable hypotheses as to whether differences in program effects were due to reallocation in the informed program towards more effectives courses or areas. That is, if the informed program was simply reallocated toward more effective courses or regions, we could pool the sample across programs, fully interact the model with an indicator for the program segment, and control for course- or region-specific heterogeneous effects of a course offer to see whether a control for heterogeneous effects by course eliminate the differential employment effect between the informed and traditional programs. The specification in such a specification would then be (focusing on the regressors and parameters of interest):

$$Y_{it} = \beta_0 + ... + \beta_2 * postcourse_{it} + ... + \beta_4 * postcourse_{it} * Offer_i$$
$$+ ... + \beta_6 * postcourse_{it} * informed_i + ...\beta_8 * postcourse_{it} * Offer_i * informed_i$$
$$+\sum_{\forall c}(...+\beta_{2,c}*postcourse_{it}*I[Course_i = c]+...+\beta_{4,c}*postcourse_{it}*Offer_i*I[Course_i = c])$$
$$+ \lambda_i + \gamma_t + \Gamma_t * informed_i + u_{ict} \quad (5)$$

where $informed_i$ is an indicator for whether the individual was taking a course in the informed segment or not, and course-specific effects of offer receipt are captured in $\beta_{4,c}$. Table 9 contains results of a reduced-form specification that tests the above hypotheses. Panel A presents a base specification, which does not include the vector controlling for course-specific effects. (Note that this yields the parametric difference between coefficients in Panels A and B of 5.) Panel B of Table 9 repeats the pooled and fully interacted specification in Panel A, with the addition of a vector of heterogeneous effects of offer receipt by course (i.e., by occupation trained for), as in equation 5. We see no reduction in the estimated differential across programs, which in fact grows larger. In Panel C, we instead include controls for heterogeneous effects by state, and find a similar lack of reduction in the differential across programs.

Not finding aggregate shifts in the allocation of trainings as an explanation for differential effectiveness, we next investigate the potential role of registrant selection across programs. Even though both registrants and course providers were blind to the provenance of classes, and classes were offered alongside one another in an undifferentiated manner at registration, more sophisticated registrants may still have systematically gravitated toward offerings with the strongest demand in coming years. Although the descriptive and empirical exercises undertaken find no evidence for this type of selection, it remains a possibility and potential cause of the differential effectiveness. Furthermore, we believe this situation to potentially be of substantial value: if there was endogenous (though unintentional) selection across programs leading to differential program effectiveness, the analysis could provide a useful empirical characterization of such selection that might be experienced in real-world settings. We thus add to the specification in Equation 5 another vector of controls interacted by registrants' education levels (classified as primary or below, some or completed middle school, and some secondary school and above), and in Panel D, we find this has no result on the estimates of the program differential. Panel E further includes another vector of controls for heterogeneous effects of offer receipt by sex, which still does not remove the differential employment effect of the informed program. We can thus show that neither student selection nor aggregate shifts in the course or geographic distribution of training classes led to differential program effectiveness. This leaves us to conclude that the course requests from employers contained labor market-specific information about projected skill demand, and this was of sufficient accuracy to increase program effectiveness.

As a further robustness exercise to support that student selection across programs was not the cause of differential effectiveness, we restrict the sample to municipality-months in which all available classes had either originally come from an employer request or were part of the traditional program, but not both. In these locations at these times, we expect there to be less opportunity for individual selection across programs, as the only way that prospective trainees could effectively select across programs would be to wait for other courses to appear in the future. We believe that this behavior would be unlikely, since most individuals would register for any acceptable course available in their municipality in order to avail their UI benefits as soon as possible. Estimations on this subsample of courses, seen in Table 10, closely mirror those in the full sample. Thus in a sample of classes in which trainee selection across programs was not possible, we find highly similar results to those in the main estimates.

6.2 Labor market targeting and government request filtering

An open question is whether the government's filtering of the original set of course requests had a meaningful effect on course composition. That is, would the program's effectiveness have been different had the government not filtered nearly 50% of the requests received from firms? To assess this possibility, we present measures of future employment growth and labor market competitiveness (measured as the Herfindahl-Hirschmann Index (HHI) of private employers of the municipality-occupation pair) for courses in either segment, and among the informed segment, for those requested and either fulfilled or denied by the ministry.

In Table 12, we show that the government's filtering process had a beneficial effect in changing the offering of skill trainings to be even more aligned with future skill demand and to be in more competitive labor markets than the original set of requests, which itself already outpaced the main program on these dimensions. A comparison of Columns 1 and 2 shows that the average rate of subsequent employment growth in the occupation-municipality pair nearly doubled after the governmental selection process, and the HHI of employers for the courses was also substantially reduced. Columns 3 and 4 present these measures expressed in terms of standard deviations – showing that ministry-approved requests were, on average, serving labor markets that experienced subsequent employment growth rate of 1.14 standard deviations above the national mean. From conversations with ministry staff, this result is not surprising: requests were screened on dimensions such as the ability of the private sector to absorb the trainees among several employers, and many of the successful requests were submitted by "lead" firms, pooling demand on their own behalf as well as that of their suppliers into a single request.

7 Conclusion

The fast-evolving skill demand of employers in both developed and developing countries has the potential to leave individuals who are less skilled, less connected, and already economically marginalized even further behind. Technical skill training programs are a common active labor market policy with the potential to address skill shortages and labor market mismatch. Whether private sector involvement can increase its effectiveness is particularly important for policymakers choosing to start, adapt, or abandon such programs. Private sector participation is often presented as a simple solution to the government's information problem in targeting labor market programs, although it has not been rigorously analyzed to date.

In this paper, we evaluated the effects of a recent large-scale technical skills training program in Brazil that used informational input from firms in providing publicly administered technical job training. The context and availability of comprehensive monthly labor market data allow us to avoid several of the common challenges in evaluating occupational training programs. We find that the informed program had a substantial causal effect on formal employment that appeared shortly after course completion and persisted for at least one year following the course. Program effects are substantially larger than those in a similarly run occupational training program that does not take input from firms in deciding the content and scale of skills training. The informational input from firms allows the allocation of skill trainings to better match *future* growth in skill demand rather than the static distribution of employment, which we show likely explains the majority of increased program effectiveness.

We find strong evidence of employment effects among registrants not previously connected to requesting firms who gain employment at non-requesting firms. This finding has several implications. First, program effects are not derived simply from overcoming labor market search frictions for participating firms. Second, the firm-requested skills are likely indicative of local skills shortages applicable to more than just the requesting firm. Finally, for the same reason, the trainings requested under the program are likely providing general rather than firm-specific skills. In this case, trainees (as opposed to firms) are the residual claimants to the skills provided, which suggests that the training program is effective in garnering information about broader local skills shortages. It also suggests that the ultimate beneficiaries of the training are trainees, rather than partnering firms. This effect is likely augmented by the government filtering of requests, which further targeted the requested courses to competitive labor markets with high demand growth over the subsequent year.

Overall, our results confirm the potential for partnership between the public and private sectors in public-sponsored skills training, although the program analyzed is just one of the various designs that involve the private sector. Future work should additionally investigate how employer-informed training programs affect requesting businesses' labor demand and output expansion, as well as workers' productivity and occupational mobility within and across firms.

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Table 1: Summary Statistics, MDIC course requests and courses

Variable	Mean	Std. Dev.	Obs
Panel A: Course requests			
Whether course was approved [0/1]	0.50	0.50	16,782
Number of seats requested	37.8	178.1	16,782
Number of seats requested | approved	43.6	82.3	8,340
Requestor is worker/industry association [0/1]	0.17	0.37	16,782
Whether requestor found in RAIS | firm	0.97	0.17	13,969
Panel B: Courses held			
Whether course was approved [0/1]	0.74	0.44	35,834
Number of seats | approved	16.0	36.5	22,198
Course hours | approved	198.4	44.6	26,666

Source: Authors' calculations using MDIC course demand data and MEC course data. Note: Sample comprised of records matching course-municipality of requests to MDIC. Less than 1% of approved requests were among courses with greater than 150 seats.

Table 2: Summary statistics, registrant panel

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: Employer-informed segment					
Individual's number of observations	72	0	72	72	40237
Male	0.575	0.494	0	1	40237
Course Offer	0.836	0.37	0	1	40237
Enrolled	0.32	0.467	0	1	40237
Course cap reached	0.879	0.326	0	1	40237
Any employment	0.595	0.491	0	1	2897064
Formal employment	0.568	0.495	0	1	2897064
Informal or self-employment	0.043	0.202	0	1	2897064
Employed in a requesting firm	0.009	0.094	0	1	2897064
Gross deflated formal monthly earnings (R$)	387.902	673.701	0	6092.08	2897064
Earnings in informal or self-employment	14.411	153.628	0	64639.34	2897064
Panel B: Traditional segment					
Individual's number of observations	72	0	72	72	90554
Male	0.566	0.496	0	1	90554
Course Offer	0.822	0.383	0	1	90554
Enrolled	0.315	0.465	0	1	90554
Course cap reached	0.835	0.371	0	1	90554
Any employment	0.585	0.493	0	1	6519888
Formal employment	0.556	0.497	0	1	6519888
Informal or self-employment	0.045	0.208	0	1	6519888
Employed in a requesting firm	0.006	0.079	0	1	6519888
Gross deflated formal monthly earnings (R$)	365.435	637.779	0	6092.293	6519888
Earnings in informal or self-employment	15.519	153.97	0	81625.141	6519888

Notes: Authors' calculations using RAIS and MEI (2010-2016) and program administrative records.

Table 3: Program employment effects, OLS difference-in-differences estimates

Activity:	Any employment	Formal empt.	Informal empt.
	(1)	(2)	(3)
Outcome: Employment [0/1]			
Panel A: Employer-informed program			
Took course * post	-0.01943***	-0.02307***	0.00443**
	(0.006177)	(0.006604)	(0.001779)
Mean of outcome	0.595	0.568	0.043
St. dev. of outcome	0.491	0.495	0.201
R^2	0.26	0.27	0.34
N	2,897,064	2,897,064	2,897,064
Panel B: Traditional program			
Took course * post	-0.02467***	-0.03025***	0.00727***
	(0.005166)	(0.005625)	(0.001404)
Mean of outcome	0.585	0.556	0.045
St. dev. of outcome	0.493	0.497	0.208
R^2	0.26	0.27	0.34
N	6,519,888	6,519,888	6,519,888
Outcome: Earnings (2012 Rs.)			
Panel C: Employer-informed program			
Took course * post	-36.5***	-38.2***	1.7
	(8.6)	(8.6)	(1.1)
Mean of outcome	402	388	14
St. dev. of outcome	687	674	154
R^2	0.40	0.40	0.23
N	2,897,064	2,897,064	2,897,064
Panel D: Traditional program			
Took course * post	-32.2***	-34.2***	2.0**
	(7.3)	(7.3)	(0.7)
Mean of outcome	381	365	16
St. dev. of outcome	652	638	154
R^2	0.40	0.40	0.25
N	6,519,888	6,519,888	6,519,888

Notes: Table presents present difference-in-differences estimates of taking a course on an indicator for employment. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $*\ p < .1$, $**\ p < .05$, $*\!*\!*\ p < .01$.

Table 4: Testing for differential pre-course trends

Activity:	Any employment	Formal empt.	Informal empt.
	(1)	(2)	(3)
Outcome: Employment [0/1]			
Panel A: Employer-informed program			
Months relative to course *	-0.00026	-0.00022	0.00001
course offer	(0.000216)	(0.000228)	(0.000094)
R^2	0.26	0.26	0.49
N	1,656,034	1,656,034	1,656,034
Panel B: Traditional program			
Months relative to course *	-0.00014	-0.00017	0.00000
course offer	(0.000143)	(0.000143)	(0.000066)
R^2	0.27	0.28	0.50
N	3,703,889	3,703,889	3,703,889
Outcome: Earnings (2012 Rs.)			
Panel C: Employer-informed program			
Months relative to course *	-0.465	-0.362	-0.102**
course offer	(0.40)	(0.40)	(0.05)
R^2	0.53	0.53	0.45
N	1,656,034	1,656,034	1,656,034
Panel D: Traditional program			
Months relative to course *	0.175	0.174	0.001
course offer	(0.22)	(0.22)	(0.03)
R^2	0.53	0.54	0.46
N	3,703,889	3,703,889	3,703,889

Notes: Table presents estimates from the estimation of equation 2 in the text, adjusted to test parallel trends in pre-course employment differentially across those receiving the course offer and those not. Sample comprised of all periods prior to the start of registrants' training course. The coefficient for [Months relative to course*course offer] gives the differential slope term for those offered a course seat in the pre-course period. Heteroskedasticity-consistent robust standard errors two-way clustered by individual and month*year reported in parentheses. All specifications include an unreported constant term, a primary slope coefficient for months relative to the course, and vectors of individual and month*year fixed effects. Significance indicated by: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

Table 5: Reduced form estimates for employment using course offer

Activity:	Any employment	Formal empt.	Informal empt.
	(1)	(2)	(3)
Outcome: Employment [0/1]			
Panel A: Employer-informed program			
Course offer * post	0.03422***	0.03694***	-0.00198
	(0.005375)	(0.005592)	(0.002161)
Mean of outcome	0.595	0.568	0.043
St. dev. of outcome	0.491	0.495	0.201
R^2	0.25	0.27	0.34
N	2,897,064	2,897,064	2,897,064
Panel B: Traditional program			
Course offer * post	0.01935***	0.01822***	0.00078
	(0.003637)	(0.003757)	(0.001475)
Mean of outcome	0.585	0.556	0.045
St. dev. of outcome	0.493	0.497	0.208
R^2	0.26	0.27	0.34
N	6,519,888	6,519,888	6,519,888
Outcome: Earnings (2012 Rs.)			
Panel C: Employer-informed program			
Course offer * post	45.68***	47.26***	-1.57
	(8.18)	(8.25)	(1.35)
Mean of outcome	402	388	14
St. dev. of outcome	687	674	154
R^2	0.40	0.40	0.23
N	2,897,064	2,897,064	2,897,064
Panel D: Traditional program			
Course offer * post	20.97***	21.75***	-0.78
	(4.96)	(4.94)	(1.00)
Mean of outcome	381	365	16
St. dev. of outcome	652	638	154
R^2	0.40	0.40	0.25
N	6,519,888	6,519,888	6,519,888

Notes: Table presents coefficients from the estimation of the reduced form model for employment and earnings. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $* \, p < .1$, $** \, p < .05$, $*** \, p < .01$.

Table 6: Sources of program employment effects by type of formal employment

	Formal employment					
Detail:	At req. firm	At nonreq. firm	Same ind. as req. firm (3 digit)	Diff. ind. as req. firm	In occupation trained (3 digit)	Other occupation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employer-informed program						
Course offer * post	0.00113	0.03581***	0.00060	0.03634***	0.00545	0.03149***
	(0.001471)	(0.005625)	(0.001403)	(0.005595)	(0.003571)	(0.005804)
R^2	0.36	0.27	0.35	0.27	0.45	0.31
N	2,897,064	2,897,064	2,897,064	2,897,064	2,897,064	2,897,064
Panel B: Traditional program						
Course offer * post	0.00138	0.01683***	0.00048	0.01774***	0.00312	0.01509***
	(0.000937)	(0.003827)	(0.000647)	(0.003789)	(0.002189)	(0.003792)
R^2	0.35	0.27	0.33	0.27	0.45	0.32
N	6,519,888	6,519,888	6,519,888	6,519,888	6,519,888	6,519,888

Notes: Table presents reduced form coefficients capturing effects receiving a course offer on types of employment differentiated in column headers. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $*\ p < .1$, $**\ p < .05$, $***\ p < .01$.

Table 7: First stage estimates, difference-in-differences IV

Outcome:	**Post-course period [0/1] * Took course**	
Sample	Employer-informed program	Traditional program
	(1)	(2)
Offer * post	0.384488***	0.38680***
	(0.005826)	(0.00378)
R^2	0.55	0.55
N	2,897,064	6,519,888

Notes: Table presents first-stage coefficients from the estimation of the endogenous variable [Post-course * Took course] in equation 1 by an indicator for being in the post-course period and having received a course offer. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $*\ p < .1$, $**\ p < .05$, $***\ p < .01$.

Table 8: Program employment effects, difference-in-differences IV estimates

Activity:	Any employment	Formal empt.	Informal empt.
	(1)	(2)	(3)
Outcome: Employment [0/1]			
Panel A: Employer-informed program			
Course offer * post	0.08903***	0.09610***	-0.00515
	(0.013999)	(0.014552)	(0.005623)
Mean of outcome	0.595	0.568	0.043
St. dev. of outcome	0.491	0.495	0.201
R^2	0.25	0.26	0.34
N	2,897,064	2,897,064	2,897,064
Panel B: Traditional program			
Course offer * post	0.05002***	0.04709***	0.00202
	(0.009418)	(0.00973)	(0.003816)
Mean of outcome	0.585	0.556	0.045
St. dev. of outcome	0.493	0.497	0.208
R^2	0.26	0.27	0.34
N	6,519,888	6,519,888	6,519,888
Outcome: Earnings (2012 Rs.)			
Panel C: Employer-informed program			
Course offer * post	119.03***	123.67***	-4.09
	(21.37)	(21.54)	(3.53)
Mean of outcome	402	388	14
St. dev. of outcome	687	674	154
R^2	0.39	0.40	0.23
N	2,897,064	2,897,064	2,897,064
Panel D: Traditional program			
Course offer * post	54.20***	56.23***	-2.03
	(12.87)	(12.80)	(2.59)
Mean of outcome	381	365	16
St. dev. of outcome	652	638	154
R^2	0.40	0.40	0.25
N	6,519,888	6,519,888	6,519,888

Notes: Table presents second-stage coefficients capturing effects of course enrollment induced by receiving a course offer. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $* \, p < .1$, $** \, p < .05$, $*** \, p < .01$.

Table 9: Pooled models with fully-specified heterogeneous course treatment effects

Activity:	Any employment	Formal empt.	Informal empt.
	(1)	(2)	(3)
Panel A: Parametric difference, employer-informed (-) traditional			
Course offer * post * informed segment	0.01472**	0.01870***	-0.00288
	(0.006244)	(0.006533)	(0.002632)
R^2	0.26	0.27	0.34
N	9,416,952	9,416,952	9,416,952
Panel B: Parametric difference conditional on heterogeneous course effects			
Course offer * post * informed segment	0.02121***	0.02480***	-0.00148
	(0.007224)	(0.007571)	(0.003136)
R^2	0.26	0.27	0.34
N	9,416,952	9,416,952	9,416,952
Panel C: Parametric difference conditional on heterogeneous course effects + heterogenous education level effects			
Course offer * post * informed segment	0.02138***	0.02501***	-0.00155
	(0.007228)	(0.007572)	(0.003140)
R^2	0.26	0.27	0.34
N	9,416,952	9,416,952	9,416,952
Panel D: Parametric difference conditional on heterogeneous effects by course, education, and sex			
Course offer * post * informed segment	0.02067***	0.02425***	-0.00145
	(0.007226)	(0.007562)	(0.003139)
R^2	0.26	0.27	0.34
N	9,416,952	9,416,952	9,416,952

Notes: Table presents coefficients from the pooled reduced-form model including different vectors of fixed effects across panels. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $*\, p < .1$, $**\, p < .05$, $***\, p < .01$.

Table 10: Reduced form estimates for employment using course offer - subsample of municipality-months without segment choice

Activity	Any employment	Formal empt.	Informal empt.
	(1)	(2)	(3)
Outcome: Employment [0/1]			
Panel A: Employer-informed program			
Course offer * post	0.05158***	0.05516***	-0.00063
	(0.008503)	(0.009026)	(0.003355)
R^2	0.25	0.27	0.35
N	1,202,328	1,202,328	1,202,328
Panel B: Traditional program			
Course offer * post	0.01256	0.01410*	-0.00356
	(0.007653)	(0.00777)	(0.003412)
R^2	0.26	0.27	0.34
N	1,601,424	1,601,424	1,601,424
Outcome: Earnings (2012 Rs.)			
Panel C: Employer-informed program			
Course offer * post	58.93***	60.88***	-1.95
	(12.91)	(13.06)	(2.08)
R^2	0.40	0.40	0.25
N	1,202,328	1,202,328	1,202,328
Panel D: Traditional program			
Course offer * post	6.90	11.45	-4.54*
	(11.08)	(10.78)	(2.28)
R^2	0.40	0.41	0.26
N	1,601,424	1,601,424	1,601,424

Notes: Table presents coefficients from the estimation of the reduced form model for employment and earnings on the subsample of classes offered in municipality-months in which there were only either traditional or employer-informed courses offered, but not both. Heteroskedasticity-consistent robust standard errors two-way clustered by class and month*year reported in parentheses. All specifications include an unreported constant term and vectors of individual and month*year fixed effects. Significance indicated by: $* \, p < .1$, $** \, p < .05$, $*** \, p < .01$.

Table 11: Share of course seats in 2014-requested occupation-municipality pairs, by year

Year	**Fraction of seats**
2012	0.001
2013	0.001
2014	0.124
2015	0.122

Notes: Table presents the share of firm-requested courses in all years. The sharp jump in 2014 shows how the demand-driven segment changed the distribution of courses towards these municipality-occupations pairs relative to previous offerings.

Table 12: Effect of request filtering on labor market characteristics of course distribution

	Rates/levels		**Std. dev.**	
	Empt. growth rate 2013-2015	Employer HHI 2014	Empt. growth rate 2013-2015	Employer HHI 2014
Segment	(1)	(2)	(3)	(4)
Main program	0.011	3042	0.50	-0.950
Firm requests	0.017	2403	0.80	-1.133
Ministry-denied requests	0.010	2748	0.45	-1.030
Ministry-approved requests	0.024	2055	1.14	-1.230

Notes: Table presents labor market characteristics of the occupation-municipality pairs of courses requested in the demand-driven segment, and for subsamples of those requests denied/approved by the administering body compared to offerings in the main program segment. HHI is defined as the standard Herfindahl-Hirschman Index of employers in the focal occupation-location pair ($\sum share_i^2 * 10000$).

Figure 1: Conceptual process flow for registrants

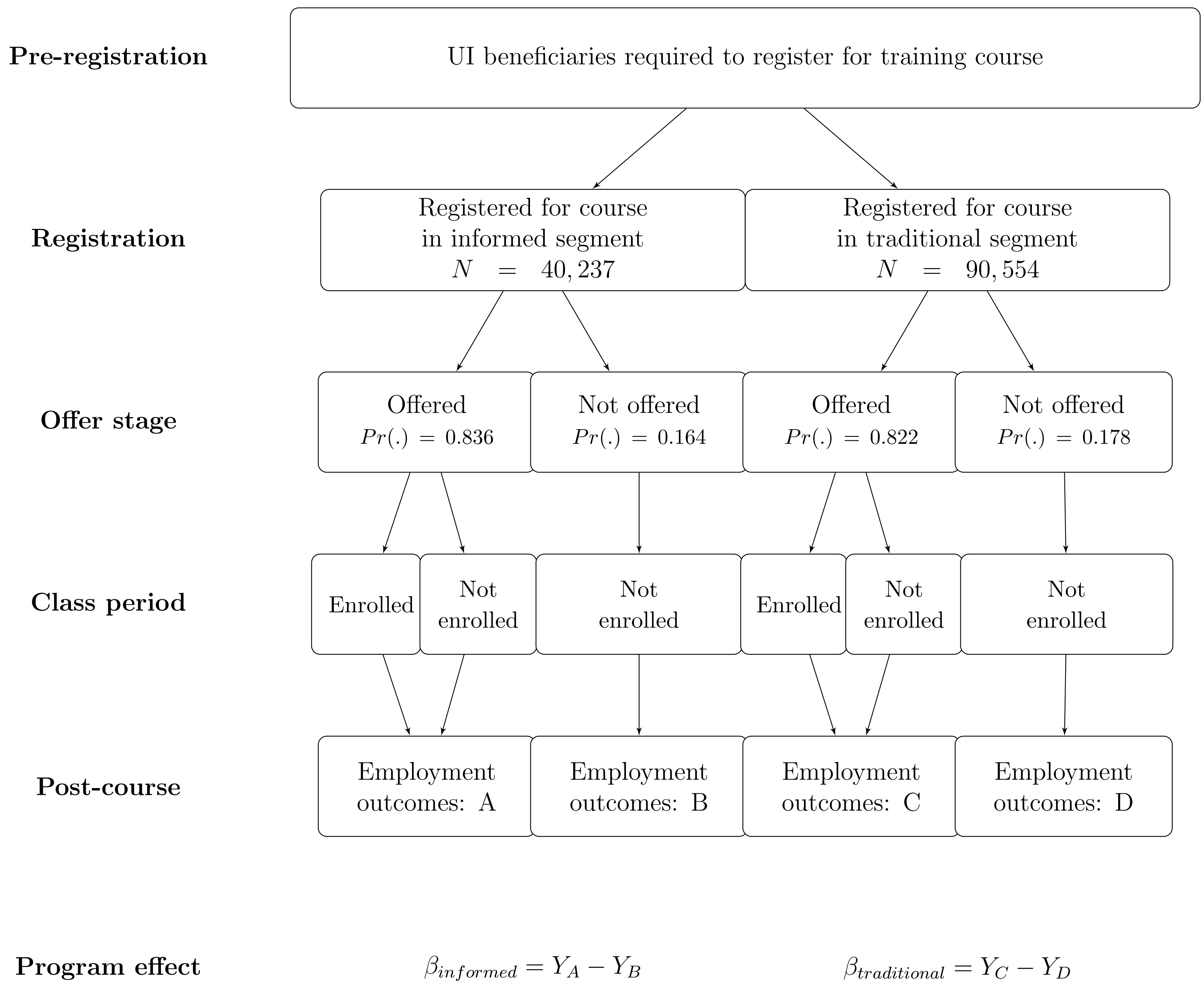
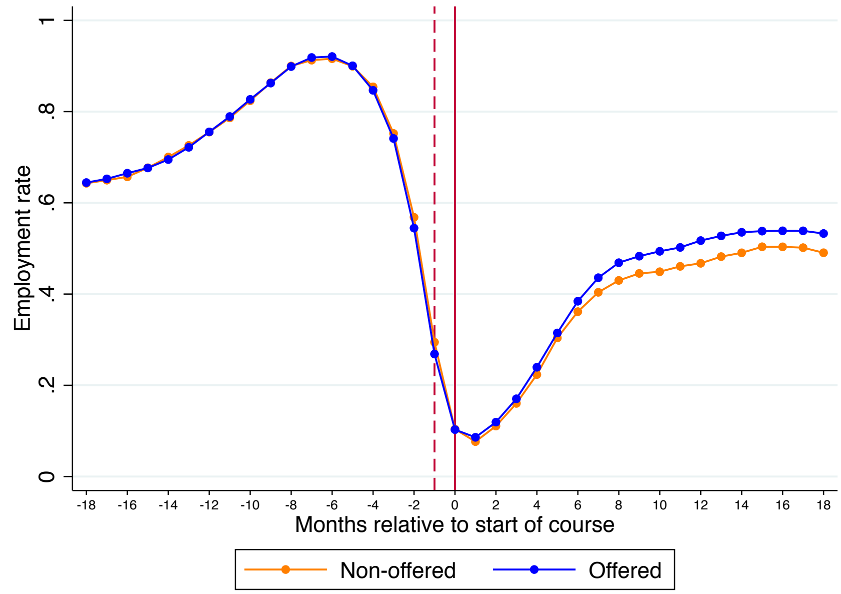
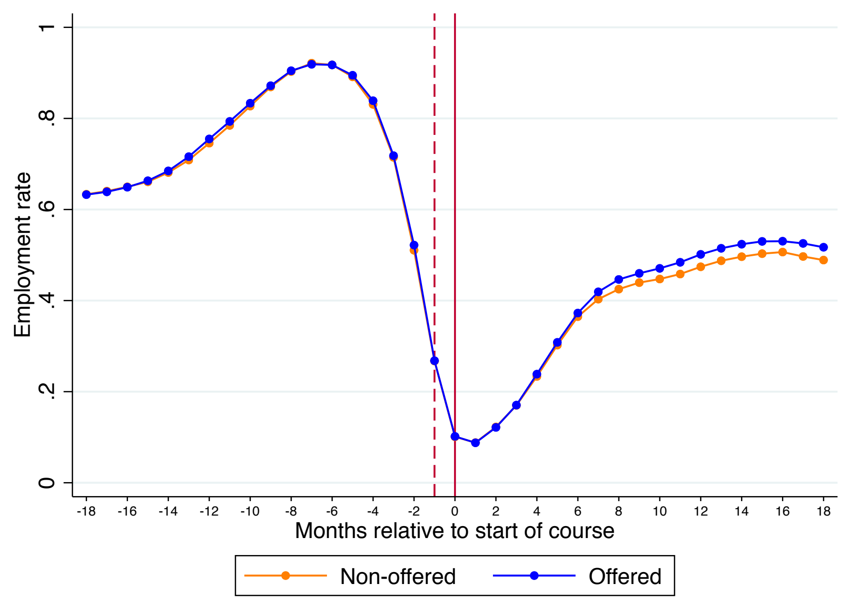


Figure 2: Change in employment relative to course start offer recipients and non-recipients, demand-driven program UI registrants



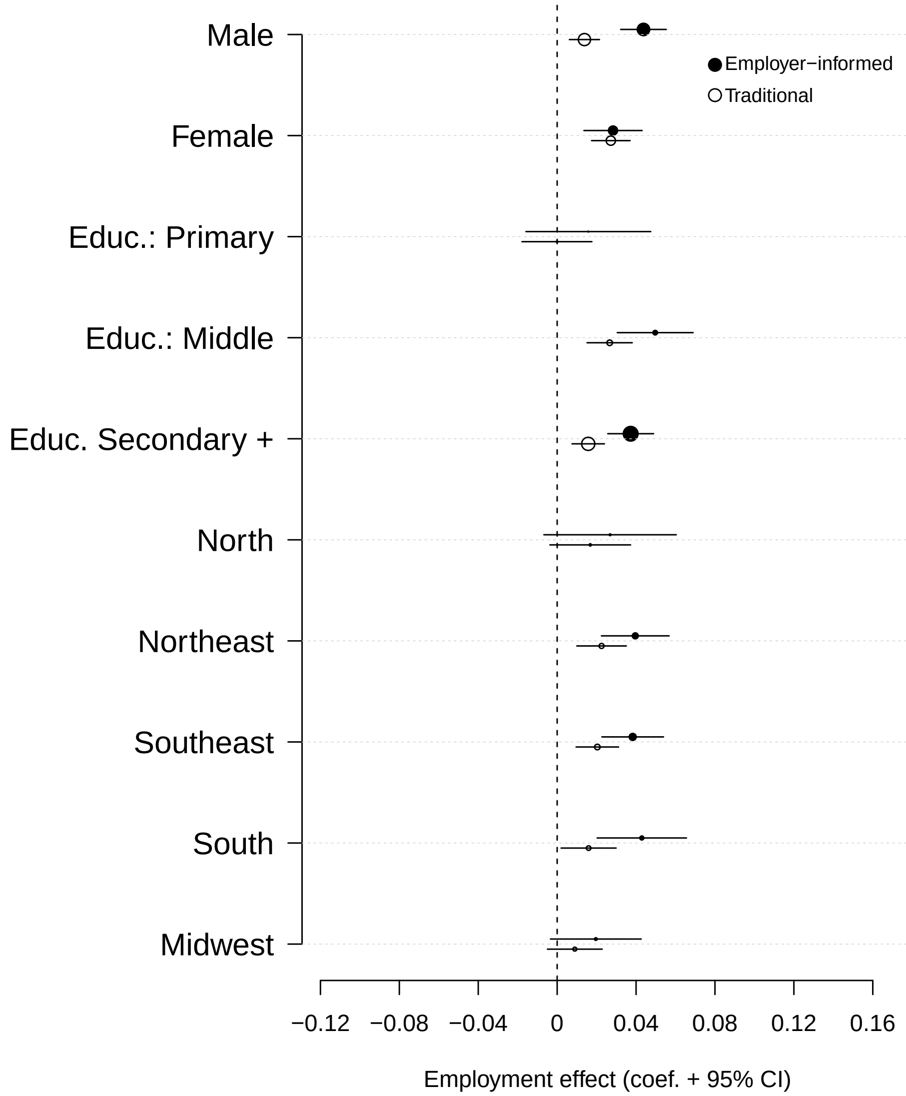
Notes: Figure depicts the mean employment rate for course offer recipients and non-recipients before and after the course.

Figure 3: Change in employment relative to course start: offer recipients and non-recipients, main program UI registrants



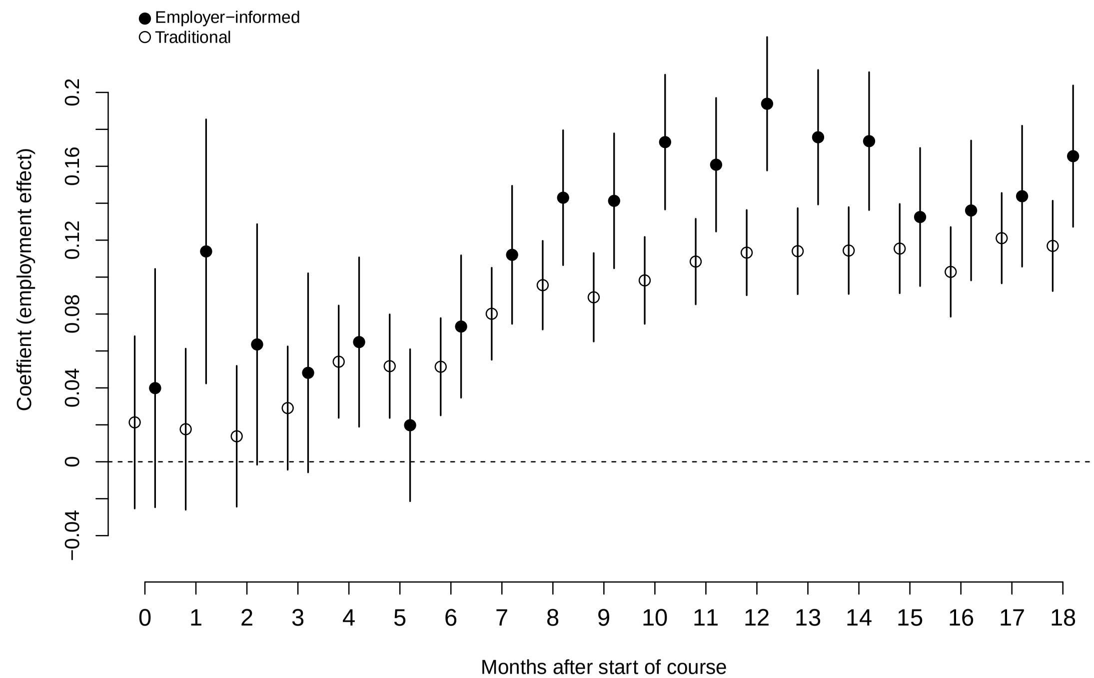
Notes: Figure depicts the mean employment rate for course offer recipients and non-recipients before and after the course.

Figure 4: Heterogeneous effects by sex and schooling level



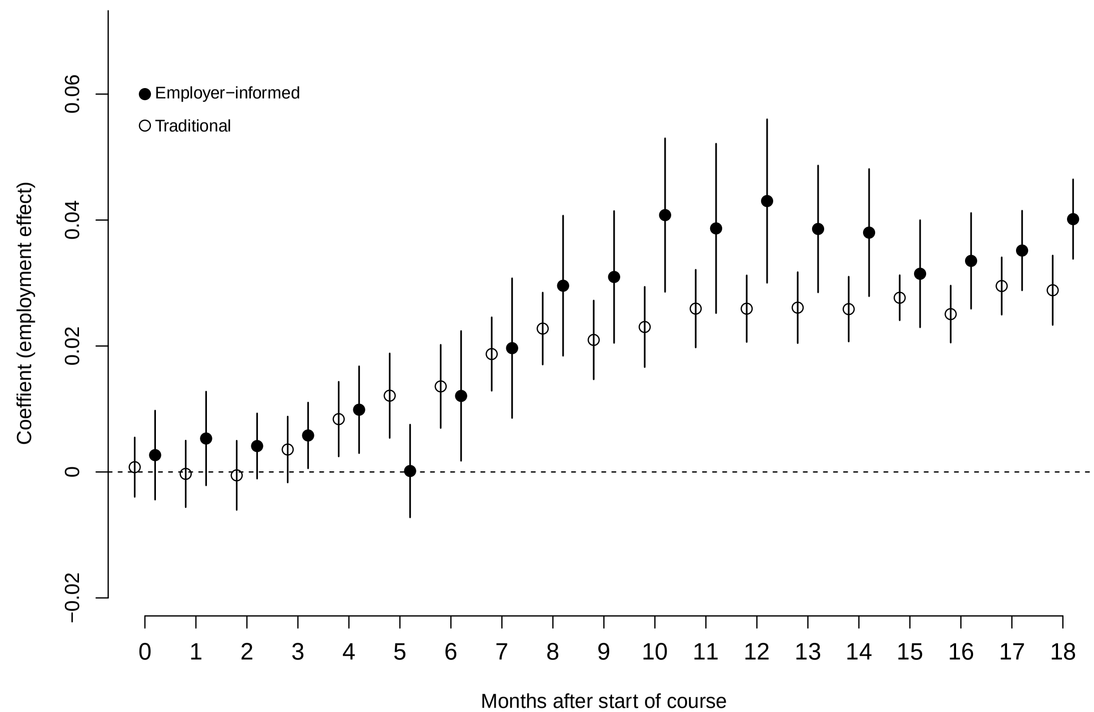
Notes: Figure depicts coefficient estimates across indicated subsamples and programs. Points sized proportionate to relative share of program size.

Figure 5: Effect evolution over time: Cross-sectional logit models



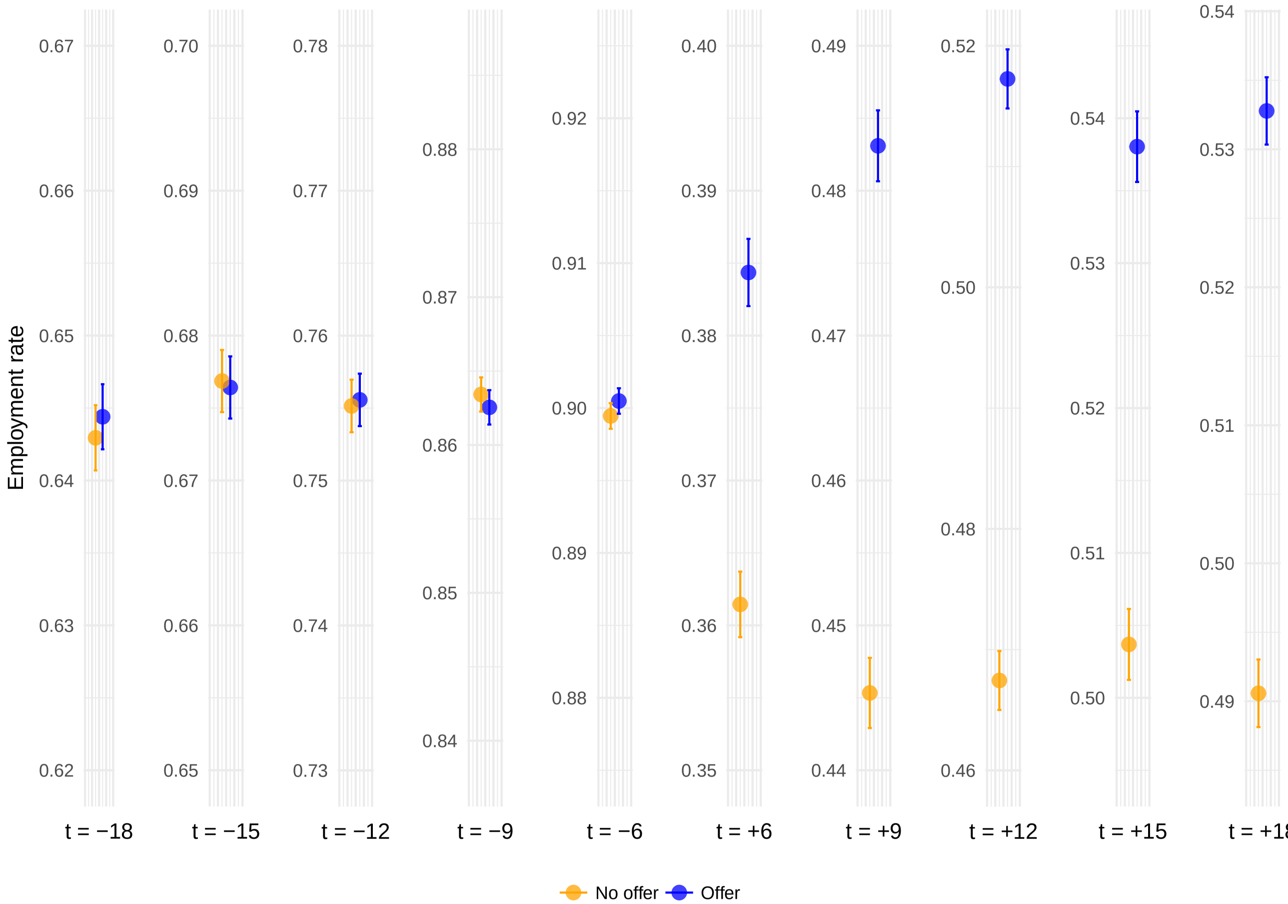
Notes: Figure depicts coefficient estimates from cross-sectional monthly sample controlling for formal employment in each of the previous 12 months prior to course start. 95% confidence intervals shown based on standard errors clustered by class.

Figure 6: Effect evolution over time: Cross-sectional linear probability model



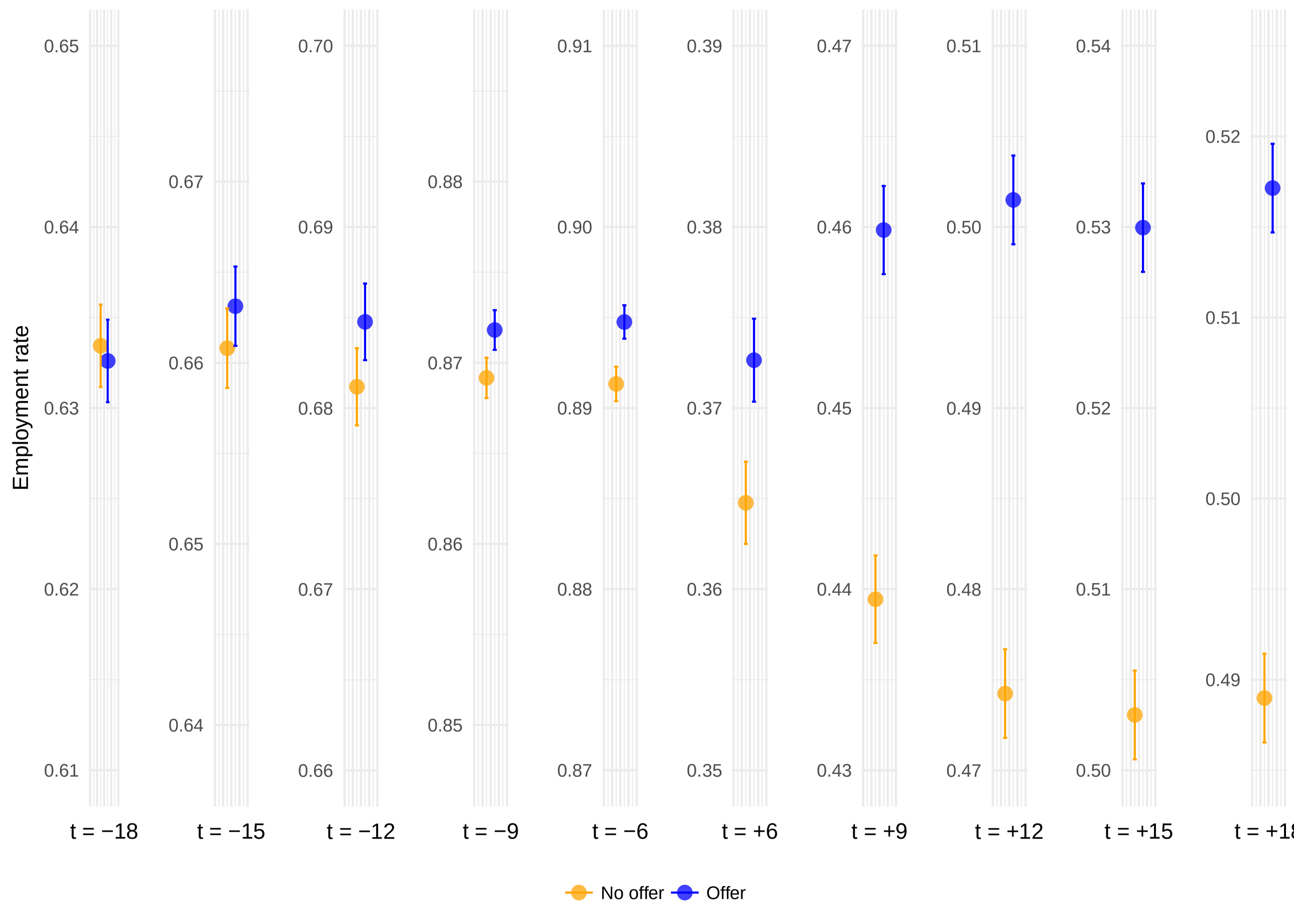
Notes: Figure depicts coefficient estimates from cross-sectional monthly sample controlling for formal employment in each of the previous 12 months prior to course start. 95% confidence intervals shown based on standard errors clustered by class.

Appendix Figure 1: Sample means and confidence intervals employment rate at periods relative to course start
Registrants in employer-informed training program



Notes: Figure depicts values and 95% confidence intervals of sample means of the employment rate at different periods relative to course start. The vertical axis range of five percentage points changes levels at each position on the horizontal axis for purposes of presentation.

Appendix Figure 2: Sample means and confidence intervals employment rate at periods relative to course start
Registrants in traditional training program



Notes: Figure depicts values and 95% confidence intervals of sample means of the employment rate at different periods relative to course start. The vertical axis range of four percentage points changes levels at each position on the horizontal axis for purposes of presentation.