Tell us what you need: Matching public job training to local skill demand with employers' input

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Abstract

We test whether information on skill demand provided by local employers can improve the effectiveness of public-sponsored job training programs. From 2014-2015, a segment of Brazil's nationwide skill training program solicited local employers' input in choosing course offerings, while the remainder of the program retained a traditional government-led structure. Using a quasi-random allocation of enrollment offers to registrants, we show that the employer-informed program nearly doubled the short-term effect on trainees' employment and earnings relative to the traditional program. The increased employment effects in the employer-informed program stem from trainees finding employment in large, high-growth firms located in low-growth municipalities. The results indicate that limited, structured input from the private sector appears to improve the alignment between skills trained and skill demand. The differential effect of the employer-informed program fades after approximately four years.

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

Active labor market policy aims 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 exist in many countries despite a sizable literature providing only mixed evidence of effectiveness, with few programs ever passing a cost-benefit test.[1]

Low levels of effectiveness can be caused by trainee selection, low quality course content, or – the focus of this paper – a mismatch between skills trained and skill demand. A common proposal to address program 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 et al., 1996), and has seen recent renewed interest both domestically and internationally (Foroohar, 2017; Srinivasan, 2017; World Bank, 2013).

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 requests for courses from firms across the country in order to determine course offerings. Notably, the input was collected through an open process, and comprised only three dimensions: the occupation to train for, the locality in which to hold the training, and the number of people to train. Unique to this study is our ability to estimate employment and earnings effects both for an employer-informed program and an otherwise-similar contemporaneous national job training program, operating within the same institutional setting and carried out by the same providers, that did not take input from firms.

We identify causal effects on employment and earnings by exploiting the fact that courses

[1] See Card et al. (2015), Kluve (2010), and Heinrich et al. (2013), among others. Betcherman et al. (2004) summarize the findings from over 150 evaluations of active labor market programs and suggest cautious expectations for what such programs can "realistically achieve."

were oversubscribed, and a subset of registrants were given an enrollment offer while others were not. Since oversubscription permeated class offerings in both program segments, we use the same empirical approach to identify effects across both the employer-informed and traditional segments. Using comprehensive administrative monthly panel data on employment, reduced-form intent-to-treat (ITT) specification reveals that individuals who received an offer to attend a course in the employer-informed program had a 2.4 percentage-point higher formal employment rate than non-offered individuals over the four years following the course. This effect is statistically distinguishable from the effect estimated for the traditional segment (1.2 percentage points). Since take-up was approximately 38 percent across either segment, using the offer as an instrument for enrollment yields IV estimates of the treatment-on-the-treated (TOT) effect on the individual employment rate to be 6.4 percentage points in the employer-informed segment, compared to 3.1 percentage points in the traditional program. These effects are driven entirely by formal employment; effects on small-scale entrepreneurship 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 submitted requests to the government. This suggests that the course requests supplied by employers were indicative of general, rather than firm-specific, skills shortages in their localities. Effects are attributable to trainees finding employment in occupations and industries in which they had not previously worked, and the difference in effectiveness between programs is largely driven by employment among large, high-growth firms in low-growth municipalities. Controlling for heterogeneous treatment effects by course, locality, and trainee background does not attenuate estimated differences across programs. We conclude that the employer input indicated localized, general skill shortages shared by numerous firms, and that differences in effectiveness did not derive from changes in the aggregate distribution of courses or the quality of trainees.

By comparing the effects of an employer-informed job training program to those from a traditionally structured design, our work contributes to two, often disparate topics in labor and development economics. First, we advance the understanding of the determinants of effectiveness of large-scale, government-run skill training programs.[2] Most relevant to this work are studies of the efficacy of training programs in developing countries, particularly those with an element of design involving private businesses (*e.g.,* Hardy and McCasland, 2020; Chakravarty et al., 2019; Attanasio et al., 2017, 2011; Hirshleifer et al., 2016; Card et al., 2011, among others). Our contribution to this active literature is the ability to compare employment and earnings effects of an employer-informed program relative to an otherwise comparable, traditional government-led program.

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. This includes recent evaluations of at-scale antipoverty programs (Alatas et al., 2016, 2012), as well as earlier theoretical and empirical work by Galasso and Ravallion (2005). Given the popularity of government-sponsored job training programs and their often low effectiveness, our study shows that taking employers' input into the process of matching skill supply with skill demand can meaningfully improve the return to these public investments.

The following section reviews related literature. We then provide further details on the programs studied, and we describe the data. Section 4 discusses our empirical strategy, and Section 5 presents primary results, an examination of why the employer informed program exhibited greater effectiveness in generating employment among trainees, and robustness and heterogeneity analyses. The final section concludes.

[2]Early work on the topic began with the evaluation of job training programs in the U.S. (see Ashenfelter, 1978; Ashenfelter and Card, 1985; Heckman and Hotz, 1989), and 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 which skills are in demand, competitive labor markets or credit constraints may reduce private incentives or capacity to invest in skills – providing scope for publicly funded investment in general skills to improve welfare. However, evidence of the effectiveness of recent skills training programs remains quite mixed. In a meta-analysis of nearly 100 studies, Card et al. (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.[3]

In the case of public sector-sponsored skills training, the government's goal is typically to improve the earnings of displaced workers. Achieving this goal relies in part on aligning training content to current skill demand. This alignment is inherently challenging because 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, or provide apprenticeships associated with the course. This circumstance, however, would create incentives for firms to focus on firm specific skills and capture all the program's benefits. At the other extreme, in an approach that has typically been used in workforce training programs and remains common today, the government takes full responsibility for

[3]A number of recent studies in have found that the effectiveness of publicly provided skills training varies widely. Positive employment effects have been found among programs in Peru (Nõpo et al., 2007; Díaz and Rosas Shady, 2016), Colombia (Attanasio et al., 2011, 2017), Liberia (Adoho et al., 2014), Nepal (Chakravarty et al., 2015), Malawi (Cho et al., 2013), Kenya (Honorati, 2015), and Brazil (Reis, 2015). However, other programs have shown negligible impacts, including programs in Argentina (Alzuá and Brassiolo, 2006), Dominican Republic (Card et al., 2011), Kenya (Hicks et al., 2013), and Jordan (Groh et al., 2016) and in an RCT in Turkey (Hirshleifer et al., 2016). For earlier reviews, see Betcherman et al. (2004) and Kluve (2010); for a review of Latin American training programs see Ibarrarán and Rosas (2009).

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 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 et al., 1996). The U.S. Jobs Corps similarly involved the private sector in the vocational training offerings (Schochet et al., 2008), and the recent Workforce Innovation and Opportunity Act (WIOA) similarly prescribed that state and local overseeing bodies comprise a majority of members from the private sector (Barnow and Smith, 2015). Despite the popularity of the principle, researchers have yet to disentangle the effects of private sector involvement from other dimensions of training program design.

Mixed results have emerged from causal evaluations of training programs in developing countries offered through private providers or combining coursework with an apprenticeship or work experience at a private firm. Attanasio et al. (2011) and Attanasio et al. (2017) show that a youth training program in Colombia that had both classroom and on-the-job components improved employment and earnings in both 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 et al., 2011; Acevedo et al., 2017). Corseuil et al. (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. More recently, Hardy and McCasland (2020) show that search costs play a substantial role in the hiring of apprentices to firms in a low-income setting, and Chakravarty et al. (2019) find large effects of a vocational training program in Nepal.

While it is believed that employers' involvement can reduce the mismatch between skills trained and skill demand, it has not been established whether this happens in practice or

whether reduced mismatch improves program effectiveness. In particular, among credible evaluations of employer-informed training programs, Attanasio et al. (2011) and Attanasio et al. (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 and 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 setting. Researchers are thus left to speculate about the precise reasons for greater effectiveness, and no study has empirically isolated the role of employer input from other context-specific factors.

The studies cited above argue that the work experience component obliges course providers to offer training in skills for which a specific demand exists, ostensibly leading to increased effectiveness. Private sector involvement, however, can be done in different ways and is just one of the unique features of any of these programs. In the contexts studied, there is no otherwise similar program that didn't involve employers to which to compare outcomes. This is not unique, though, as the literature evaluating job training programs has long been limited by the way in which government programs with multiple dimensions of new features often replace their predecessors entirely, making it difficult to meaningfully compare programs with an array of differing constituent components (Ashenfelter and Card, 1985).

This study is the first to empirically estimate differences in the effectiveness of an employer-informed job training program with that of an otherwise-similar program occurring at the same time and in the same institutional context in which both were administered by the government and courses provided by the same quasi-public institutions. From 2014 to 2015, the employer-informed segment registered around 40,000 trainees, while the traditional segment registered approximately 90,000 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,

then detail the analytical approach.

3 Course Selection in the Pronatec Program

In 2011, the federal government of Brazil created the National Program for Access to Technical Education and Employment (*Pronatec*), a program aimed at providing skills to raise the earnings and employability of low income and recently unemployed people. The courses provided technical skills training relevant to a particular occupation. The Ministry of Education was responsible for the overall administration of the program, defining course offers, selecting and funding course providers, and tracking applications. Several providers were used, but the majority of courses were offered by Brazil's *Sistema S* – a national provider network that could offer any of a broad menu of courses that in principle, ensuring a level of consistency in course objectives and content across providers and instances of course offerings.[4] Courses were free to students and financed by federal funds at a flat rate of 10 Reais (approx. 4 USD in 2014) per student-class hour. The set of courses offered in each locality was decided via consultation with several ministries for relevant areas (education, health, social programs, and industry). Importantly for this study, although course providers and all the public-facing materials of Pronatec were the same, course selection was done by two separate criteria: by the bureaucracy, in consultation with political representatives, which we call the traditional segment, or by consulting with employers.

In the traditional segment, ministries consulted with municipal bodies, social assistance centers, and unemployment insurance (UI) centers, as well as course providers to define their course requests. After consulting with these institutions, bureaucrats at each ministry could define the set of courses selected. None of these processes included any formal consultation

[4] *Sistema S* is an amalgamation of the job training arms of the national confederations of Industry and Commerce. These are tax-funded institutions, constituting a layer of quasi-governmental organizations that administer low-cost or free professional training courses at schools and learning centers throughout Brazil.

with local employers. Neither were the courses selected from data on local labor markets.

Course selection was conducted in this manner for all of *Pronatec* between 2011 and 2013. Anecdotal evidence from interviews with policy-makers and course providers indicated that they suspected that the early lack of impact of the program, circa 2013, could be the result of course offerings not matching the skills that were not demanded locally.[5] In response to these perceived mismatches, the Ministry of Industry, Foreign Trade, and Services (hereafter, "Ministry of Industry") decided to set its course requests by aggregating course demands from the private sector.

In the second half of 2013, the Ministry of Industry established a channel for local employers to express interest in specific courses that could address local skill shortages in their existing and prospective labor force. This initiative was promoted in trade shows and via industry associations. Because of the long-standing and institutionalized nature of public sector-sponsored skills training in Brazil, firms could choose from a menu of 644 existing courses, covering most sectors and occupations. The course menu listed the course name, duration in hours, a one-paragraph description of the content, and the minimum level of schooling requirement of trainees to participate in the course.

Firms submitted course requests via a standard form in which they indicated the desired course name, municipality, along with the number of "seats" for current and future/desired employees and the firm's tax id and name(Pedrosa, 2016). Course requests were free and submitted directly by email, and later by an online form. We are able to match 97 percent of requesting firms to the employee-employer employment records described below. If a course was approved, the firm could indicate current employees to participate in it. Therefore, beyond improving the local labor supply, firms had a direct incentive to provide information because of the full subsidy for current employees' training.

[5]See Pedrosa (2016) for a description of the program's institutional setting and interviews with relevant stakeholders.

This "demand driven" segment of the 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 courses across a wide range of industries and occupations.[6]

Panel A of Table 1 shows that employers submitted 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. 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).

Table 1: Summary Statistics, course requests and courses provisioned

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

Note: Authors' calculations using data on courses requested matched to courses offered.

Course requests were then screened by Ministry of Industry staff in terms of their plausi-

[6]To fix concepts, a "course" refers to a specific set of material or concepts that teach or build skills needed for a particular occupation, *i.e.*, a course in welding. Any instance in which a given course is offered must cover the same content and will have the same objectives. When specificity is required, we refer to a "class" as a specific instance of a course being offered in a particular municipality and time. Multiple distinct classes of the same course can be held concurrently in the same municipality. 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").

bility and appropriateness, relative to firm size and sector.[7] Approximately one half of firms' requests were denied at this stage. Some firms chose to reapply with a lower number of seats requested. Approved requests were then forwarded to the Ministry of Education, which as mentioned, aggregated requests with the requests of other ministries and arranges provision. Panel B of Table 1 presents summary statistics on the courses held (that is, conditional on being approved and having a provider). The average course size had 16 seats. That is, based on the average number of seats per firm request, several classes were held to fill a single course demand. [8] The average class ran for 200 course hours, met for approximately eight hours per week, and lasted between five and six months.

The coexistence of the traditional and employer informed segments over the 2014 and 2015 period allows us compare their effectiveness to learn about the importance of employer input in aligning course offers to skill demand. Other aspects of the courses were similar across the segments: courses were offered and listed in registration systems by the same providers alongside the firm-requested courses, and 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.

In Brazil, unemployment insurance (UI) recipients are required to register for a training course as a condition to receive unemployment benefits. This set of registrants comprises 22% of all *Pronatec* enrollment over the period we study. The bulk of the remainder of registrants were recipients of the Bolsa Familia conditional cash transfer program and other low-income populations. Due to prevailing policies, these low-income applicants received priority for enrollment offers. This, in combination with the fact that providers regularly oversubscribed courses, generated a loss of enrollment offers for UI-sourced registrants, who

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

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

were in a lower priority group. This feature both creates the feature that we use to identify program effects among UI registrants, while precluding the use of the remainder of registrants in the analysis due to a lack of a viable counterfactual population. For these reasons, we focus on the sample of UI registrants for the empirical analysis.

To estimate effects of course attendance, we exploit the detailed information on the reasons why students did or did not enroll in the course for which they registered. An individual might not have enrolled in a course for which she registered either for personal reasons (which result in a recorded status equivalent to "quit prior to the course" or "no-show"), or due to the combination of oversubscription and capacity restrictions.[9] In the case of course oversubscription, which underlies the vast majority of course offer restrictions, registrants from the UI channel – due to their often late registration – were typically used to make space for other "priority" groups registered through other channels (among others, the firm-supplied registrants). This process was ostensibly based on a first-come, first-served basis, although the administrative program data did not retain sufficient detail to observe the precise registration date/time cutoff for course offer receipt. Because individuals who had to register for a training course due to their UI benefits were registered in batches, capacity constraints would be often reached in the middle of a UI registrant batch. We then use the information on whether a registrant received a "course offer" – taking the value of one for those who had registered for a class and were not subject to the capacity constraints that would keep them from attending the class. Appendix Figure 1 contains a conceptual mapping of the registration, offer, and enrollment process.

We link the program's administrative records to social security records containing information on all formal sector employment and to the registry of micro-entrepreneurs. Our formal sector employment data come from the Relaçao Anual de Informações Sociais (hereafter

[9]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.*, seat reductions, or class oversubscription.

RAIS). RAIS a matched employee-employer dataset containing monthly detail on employment and wage earnings of all formally employed workers in Brazil; we use records from calendar years 2009 to 2019. We deflate earnings to June 2012 real (IBGE, 2016).[10]

Over the study period, the traditional *Pronatec* segment was more than twice as large as the employer-informed segment, serving nearly 90,000 UI trainees. Table 2 presents summary statistics on the individual-level panel dataset used for analysis. The administrative data contain 39,962 unique individuals who registered for any of the firm-requested courses (Panel A). 51.9 percent of this sample is male, and 32 percent of registrants enrolled in the training course for which they registered. Approximately 16 percent of individuals in this sample were denied a seat due to capacity constraints, and 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 54 percent, a trivial fraction of the person-month records had employment in one of the requesting firms (approximately one percent). The average employment rate of 54 percent is made up of a majority of formal employment (52.3 percent of person-months) and a small, but non-trivial minority of informal employment (2.7 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$540. In the traditional program (Panel B), there are 89,735 unique registrants, 51.1 percent of whom are male, had a similar enrollment rate (31.8 percent), and had similar employment rates (52.9 percent) and wage rates (R$509/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.

[10]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.

Table 2: Summary statistics, registrant panel

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: Employer-informed segment					
Monthly observations per registrant	132	0	132	132	39,962
Male	0.519	0.5	0	1	39,962
Course Offer	0.842	0.365	0	1	39,962
Enrolled	0.322	0.467	0	1	39,962
Course cap. reached	0.878	0.327	0	1	39,962
Any employment	0.54	0.498	0	1	5,274,984
Formal employment	0.523	0.499	0	1	5,274,984
Informal or self-employment	0.027	0.163	0	1	5,274,984
Employed in a requesting firm	0.01	0.102	0	1	5,274,984
Gross deflated formal monthly earnings (R$)	540.685	699.461	0	4478.136	5,274,984
Earnings in informal or self-employment (R$)	8.851	118.741	0	64639.34	5,274,984
Panel B: Traditional segment					
Monthly observations per registrant	132	0	132	132	89,735
Male	0.511	0.5	0	1	89,735
Course Offer	0.828	0.377	0	1	89,735
Enrolled	0.318	0.466	0	1	89,735
Course cap. reached	0.834	0.372	0	1	89,735
Any employment	0.529	0.499	0	1	11,845,020
Formal employment	0.51	0.5	0	1	11,845,020
Informal or self-employment	0.029	0.167	0	1	11,845,020
Employed in a requesting firm	0.008	0.088	0	1	11,845,020
Gross deflated formal monthly earnings (R$)	509.22	668.409	0	4478.169	11,845,020
Earnings in informal or self-employment (R$)	9.414	118.347	0	81625.141	11,845,020

Note: Table contains summary statistics of constructed registrant monthly panel data from 2009 to 2019 for individuals registered for a class in either the employer-informed segment or the traditional segment over the period of 2014-2015.

4 Empirical design

4.1 Using course offers to identify employment effects of training

Because the analysis is split into three periods relative to course start and end, we interact the indicator for the offer separately with indicators for the during-course and post-course periods. We analyze students who register for classes held in 2014 and 2015, during which time both the employer-informed and traditional programs were active, and estimate monthly outcomes spanning the calendar years of 2009 to 2019 in a reduced-form specification given by:

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

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 differences in employment in the course and post-course periods (relative to the pre-course period), β_3 captures the "during course" effect of an enrollment offer, and β_4 gives the focal difference-in-differences estimator of offer receipt on the outcome. The vector of individual fixed effects in λ_i absorbs individual-level unobservables (as well as location and classroom-specific effects) 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 across both employment and earnings outcomes separately for program segments.

We also 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). It is unlikely that a course offer has any direct impact on employment, because offers are private information to registrants and the training organizations do not select students based on quality. Therefore the offer does not convey information about worker quality to potential employers. The first-stage specification is given by:

$$Enrolled_{ict} = \delta_0 + \delta_1 * course_{ict} + \delta_2 * postcourse_{ict} + \delta_3 * course_{ict} * Offer_i + \delta_4 * postcourse_{ict} * Offer_i + \Lambda_i + \Gamma_t + u_{ict} \quad (2)$$

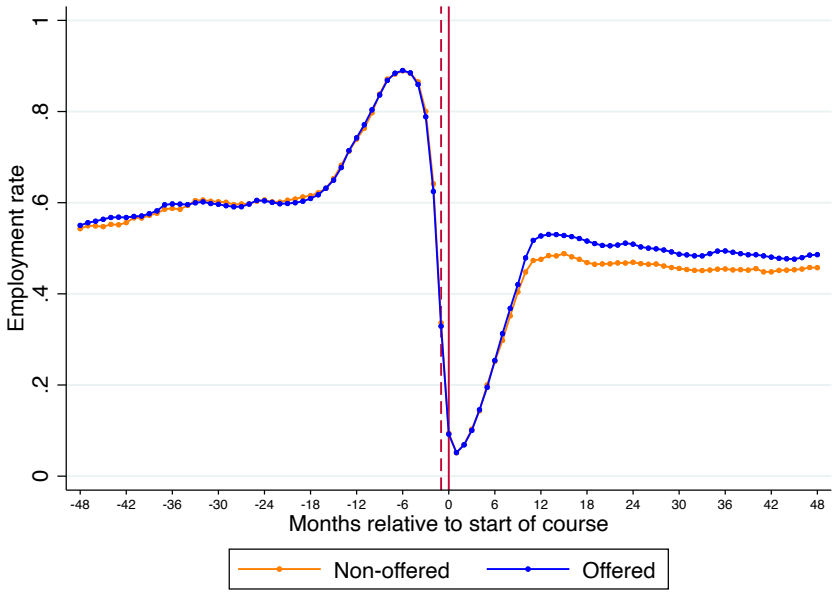
The second stage of the IV specification is thus:

$$Y_{ict} = \beta_0 + \beta_1 * course_{ict} + \beta_2 * postcourse_{ict} + \beta_3 * course_{ict} * \widehat{Enrolled}_i + \beta_4 * postcourse_{ict} * \widehat{Enrolled}_i + \lambda_i + \gamma_t + u_{ict} \quad (3)$$

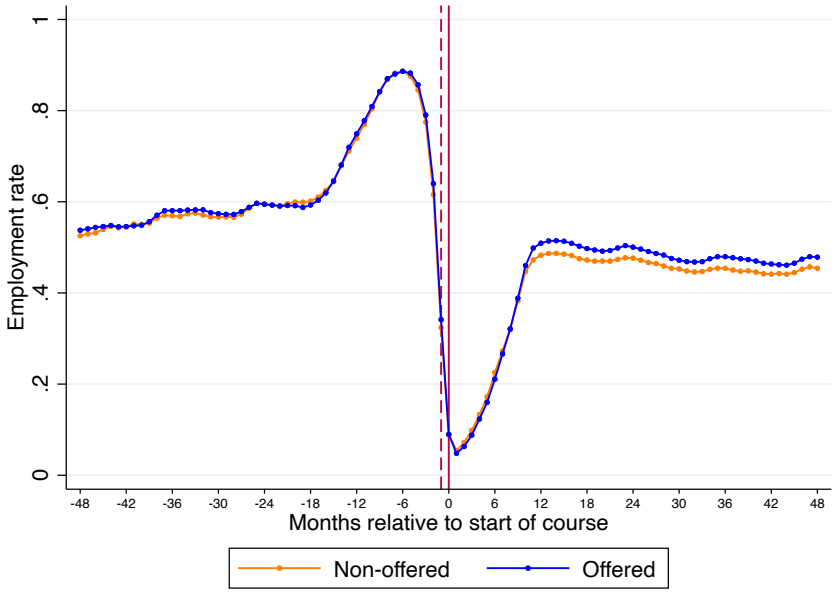
To assess the validity of this approach, we consider the observable trends in employment for registrants in the two program segments relative to the timing of their class. In Figures 1a and 1b, we plot the mean employment rates in either program based on whether or not the registrant received a course offer for the 48 months on either side of registrants' class start date. This allows us to visually confirm expected trends related to pre-program employment patterns, assess the viability of the common trends assumption based on pre-course data, 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 who did not receive an offer.

Figure 1: Mean employment rate relative to course start

(a) Employer-informed segment



(b) Traditional segment



Note: Figure depicts the mean employment rate for course offer recipients and non-recipients before and after the course. The dashed line represents the month prior to the projected start of the course.

The figures illustrate notable patterns regarding employment and selection into training. First, the employment rate of program participants in the month in which they start their course is at its lowest point in the window observed, 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 – a pattern of selection that yields a sharply-declining employment trend in the months prior to job training, first highlighted in Ashenfelter (1978). The figures also suggest that the pre-course trends for the two groups are highly overlapping – particularly in view of the magnitudes of post-training employment differences for offer recipients versus non-recipients. In Figure 1a, we see that registrants were nearly all employed six months prior to their course, and this employment rate then drops precipitously to approximately 10 percent in the month in which the course begins. Within each program, these pre-course patterns are similar for those who did and did not receive a course offer; these patterns also have 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.

For comparison, we present similar graphs that depict the magnitude of endogenous selection into enrollment in Appendix Figures 2 and 3. Enrollees are clearly positively selected on pre-course employment rates, and new employment occurring around the start of the course likely causing endogenous non-enrollment. This contrasts starkly with the highly overlapping pre-course trends between those restricted by capacity and those offered an enrollment option in Figures 1a and 1b.

4.2 Validating the empirical design

Within program segments, there are two classes of threats to empirical identification. The first is cross-sectional selection in offer receipt. To test this, we estimate the relationship be-

tween offer receipt and available characteristics of registrants: sex, education, last reported wage rate, and employment over each of the nine months prior to course start in a multivariate specification regressing offer status on these measures and a vector of class fixed effects. Table 3 contains the results of this test for the employer-informed segment in column 1, the traditional segment in column 2, and the difference between these in column 3.

There is a small difference in the probability of offer receipt for men in the informed segment of less than one percentage point (0.009). This is economically small relative to a mean offer rate of .84, and is precisely estimated enough to be significant at the five percent level. There is also a slightly higher probability of offer receipt per year of education in the traditional program, but this magnitude is also not economically meaningful relative to the mean offer rate, and is no different across the two programs. There are a few scattered differences in pre-course employment rates' ability to predict offer status, but these do not present a clear pattern of selection. Our identification relies on quasi-random allocation of course offers in a large and complicated national program; in this case, the sample size – orders of magnitudes larger than the typical randomized experiment – affords a degree of precision to otherwise small magnitudes of differences across offer recipients and non-recipients. We show in Section 5.5 (Figure 4) and Section 5 (Table 8) that differences in these characteristics do not explain the pattern of results presented below.

Table 3: Testing for differences in pre-course observables

Program segment:	Employer-informed	Traditional	Difference
	(1)	(2)	(3)
Outcome: Received course offer [0/1]			
Male [0/1]	0.0091**	-0.0017	0.0109**
	(0.0040)	(0.0028)	(0.0049)
Years education	0.0008	0.0012**	-0.0004
	(0.0008)	(0.0005)	(0.0010)
Last wage rate	0.0003	-0.0009	0.0012
	(0.0008)	(0.0006)	(0.0010)
Employment, t-1 [0/1]	0.0006	0.0036	-0.0030
	(0.0058)	(0.0041)	(0.0072)
Employment, t-2 [0/1]	-0.0143***	-0.0011	-0.0132**
	(0.0052)	(0.0037)	(0.0064)
Employment, t-3 [0/1]	-0.0019	-0.0034	0.0015
	(0.0070)	(0.0052)	(0.0087)
Employment, t-4 [0/1]	-0.006	0.001	-0.007
	(0.01)	(0.00)	(0.01)
Employment, t-5 [0/1]	-0.002	0.002	-0.004
	(0.01)	(0.00)	(0.01)
Employment, t-6 [0/1]	0.0057	-0.0083	0.0140
	(0.0136)	(0.0092)	(0.0165)
Employment, t-7 [0/1]	0.0089	-0.0029	0.0119
	(0.0128)	(0.0087)	(0.0156)
Employment, t-8 [0/1]	-0.0172	0.0070	-0.0243*
	(0.0110)	(0.0081)	(0.0137)
Employment, t-9 [0/1]	0.006	0.001	0.003
	(0.00)	(0.00)	(0.00)
R^2	0.369	0.441	0.419
N	34,732	75,449	110,181

Notes: Table presents estimates from the estimation of a crosssectional specification of offer status on registrant characteristics. Sample comprised of all registrants in the programs indicated by column headers. Heteroskedasticity-consistent robust standard errors reported in parentheses. All specifications include an unreported constant term and a vector of class fixed effects. Significance indicated by: $* \; p < .1$, $** \; p < .05$, $*** \; p < .01$.

The second concern over identification would be a violation of the common trends assumption, even if level differences in employment rates have not been shown to differ across offer status and programs. Although limited in various ways in general, as well as in a context with expected nonlinear pre-intervention trends, we still test for parametric differences in the pre-course period to empirically characterize the pattern visually apparent in Figures 1a and 1b. Because of the nature of the trend in employment relative to the course start, we test differences in both slope and curvature using a second-order polynomial in a sample limited to all pre-course observations. The specification is:

$$\begin{aligned} Y_{ict} = \alpha_0 &+ \alpha_1 * months\ relative\ to\ course_{ict} \\ &+ \alpha_2 * months\ relative\ to\ course_{ict} * offer_i \\ &+ \alpha_3 * months\ relative\ to\ course^2_{ict} \\ &+ \alpha_4 * months\ relative\ to\ course^2_{ict} * offer_i + \lambda_i + e_{ict} \end{aligned} \quad (4)$$

In equation 4, α_2 and α_4 give the slope and curvature differential for offer recipients' pre-course employment trends relative to non-recipients. The coefficients from this estimation across employment outcomes are in Table 4. The estimated slope differentials are all small and statistically insignificant at conventional levels for both programs, confirming the visually overlapping pre-course employment patterns across offered and non-offered registrants. The precision afforded by the sample allows us to reject coefficients as small as 0.001 – a magnitude itself not large enough to present meaningful concern for the estimates below. In later sections, we also present cross-sectional estimates of outcomes that condition on a vector of monthly pre-course employment status indicators to provide further robustness of the results and address concerns regarding the parametric nature of this the tests in Table 4.

Table 4: Testing for differential pre-course trends

Activity:	Informed Program	Traditional program
	(1)	(2)
Outcome: Employment [0/1]		
Months relative to course *	-0.0001	0.0001
course offer	(0.0004)	(0.0002)
Months relative to course^2 *	0.0001	0.0003
course offer * 100	(0.0007)	(0.0004)
R^2	0.22	0.23
N	2,603,801	5,824,302

Notes: Table presents estimates from the estimation of equation 4 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, and the coefficient for [Months relative to course^2*course offer] gives the quadratic curvature differential. 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 its square, and a vector of individual fixed effects. Significance indicated by: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

5 Results

5.1 Program effects across segments

We first empirically illustrate takeup by estimating the first-stage specification in Equation 2 across programs. Table 5 presents the coefficients from the estimation of the focal endogenous variable, $postcourse_{ict} * Enrolled_i$. In the first stage, the coefficient effectively reflects the take-up rate among those offered a seat in the course (*i.e.*, compliance), which is around 38 percent – similar to take-up in other large-scale training programs for the unemployed (Crepon et al., 2013) – and statistically indistinguishable across programs. We interpret this

as further evidence of a lack of endogenous selection across programs, as these magnitudes might be expected to be diverge if registrants differentially assessed the value of the courses offered under either segment.

Table 5: First stage estimates

Outcome:	**Post-course period [0/1] * Enrolled**	
Sample	Employer-informed program	Traditional program
	(1)	(2)
Offer * post	0.383***	0.384***
	(0.005)	(0.003)
R^2	0.61	0.61
N	5,274,984	11,845,020

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: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

Figures 1a and 1b plot unadjusted means, but still give a preview of our results. In Table 6, we report ITT and LATE estimates of β_4 from Equations 1 and 3 in Columns 1 and 2, respectively for the employment outcome, and in Columns 3 and 4 in real monthly earnings. In Column 1, we find a positive effect of an offer on the post-course employment rate of approximately 2.4 percentage points in the informed program. This contrasts with effects in the traditional program, which were about half of the magnitude (Column 2). Results in Columns 3 and 4 confirm a similar pattern of results for real monthly earnings, with the magnitudes in the informed program approximately double those in the traditional program. For either outcome, the coefficient estimates are statistically distinguishable across program segments at any conventional level of statistical significance.[11]

[11]Appendix Table 1 contains coefficients from the direct estimation of endogenous enrollment effects on employment and earnings. This specification is expected to recover downward-biased estimates given the negative correlation between time-variant job offer receipt and enrollment (among other confounds).

Second-stage IV coefficient estimates in Panel B yield the local average treatment effect (LATE) for compliers. The informed program increases the post-course employment rate by 6.4 percentage points for enrollees (Panel B, Column 1) – representing an 12 percent increase over the mean employment rate of 52.3. Because of similar first stages, the traditional program LATE effects are again about half the magnitude of the informed program, and these same patterns are present in estimations of monthly earnings.

The parametric estimates above largely reflect the average differences across series that are visually detectable in Figures 1a and 1b. Overall, these effect magnitudes are consistent with effect sizes in the existing literature, even when viewed relative to the mean employment rate. While the magnitudes of employment effects are small – probably too small to ever pass a cost-benefit test – we view these results as strong evidence in the test of the longstanding hypothesis that employer information can improve skill training programs. In the following sections, we characterize the evolution of effects over time and trace the sources of employment among informed-program trainees to specific employers, occupations, and localities in order to answer more precisely how input from local businesses increased training effectiveness.

Table 6: Effect of training on formal employment and earnings across programs

Outcome::	**Employment [0/1]**		**Earnings (2012 Rs)**	
Estimate	Informed	Traditional	Informed	Traditional
	(1)	(2)	(3)	(4)
Panel A: Effect of course offer (ITT)				
Course offer * post	0.024***	0.012***	36.30***	17.24***
	(0.005)	(0.003)	(7.78)	(4.77)
Mean of outcome	0.523	0.51	541	509
St. dev. of outcome	0.499	0.5	699	668
R^2	0.24	0.24	0.28	0.28
N	5,274,984	11,845,020	5,274,984	11,845,020
Panel B: Effect of enrollment (IV LATE)				
$\widehat{Enrolled}$ * post	0.064***	0.031***	94.64***	44.86***
	(0.013)	(0.008)	(20.29)	(12.43)
Mean of outcome	0.523	0.51	541	509
St. dev. of outcome	0.499	0.5	699	668
R^2	0.03	0.03	0.02	0.02
N	5,274,984	11,845,020	5,274,984	11,845,020

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: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

5.2 Effects on employment in small-scale businesses

To gauge the impact of the programs on informal and self-employment, we use data on monthly contributions to social security by small-scale entrepreneurs and their employees. Known as *Micro Emprendedor Individual* (hereinafter "MEI") contributions, this system is for those who own or work in a business with sales less than R$60,000 per year. Established in 2008 with the objective of creating a stepping stone for self-employed people to transition out of informality, MEI enables entrepreneurs or employees to make small, fixed monthly contributions (R$47.70) in order to obtain coverage for themselves and their dependents by social security benefits. From 2009 to 2018, the number of individuals contributing under

this system grew steadily, reaching 6.9 million people in 2018 (6.6 percent of the Brazilian working age population). We obtain data as of 2019 that contain information on the first contribution to this system, allowing us to retrospectively measure the point at which any individual begins contributing to this program via employment or entrepreneurship in a small-scale business. We then link a constructed measure of whether the individual has, as the focal month, ever contributed to the MEI system to the monthly individual panel data. Table 7 contains results of these estimates, showing that there is no discernible effect on employment or entrepreneurship in small-scale businesses through to 2019.

Table 7: Reduced form estimates for small-scale employment using course offer

Outcome::	**Employment [0/1]**	
Estimate	Informed	Traditional
	(1)	(2)
Course offer * post	-0.002	0.00
	(0.002)	(0.00)
Mean of outcome	0.021	0.021
St. dev. of outcome	0.143	0.142
R^2	0.39	0.39
N	5,274,984	11,845,020

Notes: Table presents coefficients from the estimation of the reduced form specification on an outcome based on small-scale 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: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

5.3 Tracing the sources of employment effects

We next fully characterize the evolution of program effects in a traditional event study specification over the four years prior to, and five years after, registrants' course start month. Figure 2 plots the monthly coefficients, in which we see the largest employment rate differ-

ences between the informed and traditional programs occurring shortly after the training course, around eight months after the course start. The difference is sustained, although slowly converges over time up to approximately 48 months after course start, when employment effects across the two segments are no longer statistically discernible. Thus while program effects are larger for the employer-informed program over the first three years after a training course, employment rates eventually converge over time – and this convergence is not due to traditional-segment trainees catching up, but rather inform-segment trainees reverting to the employment levels of those trained under the traditional segment. Thus while the pooled results show substantial increases in employment and earnings in the employer-informed program, there is a limited horizon on which employers can accurately predict skill demand into the future.

Figure 2: Effect evolution over time: Monthly event study specification

Notes: Figure depicts coefficient estimates from a monthly event study specification. 95% confidence intervals shown based on standard errors clustered by class and month of course start.

5.4 Skill and location targeting

But how 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 relationship of this input to program effectiveness remains to be empirically characterized, however. For example, employers' input may have reallocated training 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-municipality pair) with recent demand growth and resulting skill shortages.

The above potential mechanisms suggest specific, empirically testable hypotheses as to whether differences in program effects were due to reallocation in the informed program towards more effective occupations, areas, or labor markets. We approach this question in two ways. The first is in a pooled-sample interacted model which fully parameterizes heterogeneous effects by occupation or municipality. If the informed program simply reallocated training toward more effective skills or regions, then allowing for heterogeneous effects along these dimensions would close the estimated employment effect differential across program segments. The empirical test thus entails pooling the sample across programs, fully interacting the model with an indicator for the program segment, and controlling for skill- or region-specific heterogeneous effects of a course offer. If the differential in employment effects across segments closes with the inclusion of occupation-specific heterogeneous effects, for example, then we would conclude that the employer input reallocated training to occupations (or skills or courses) in which training resulted in higher employment effects – regardless of the program segment in which they were offered. Focusing on the regressors and parameters of interest, the specification is then:

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

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 8 contains results of a reduced-form specification that tests the above hypotheses. Column 1 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 Columns 1 and 2 of 6.) Column 2 of Table 8 repeats the pooled and fully interacted specification in Column 1, with the addition of a vector of heterogeneous effects of offer receipt by course (i.e., by occupation trained for) as in equation 5, under the assumption that these heterogeneous effects are constant across program. We see no reduction in the estimated differential across programs, which in fact grows larger. In Column 3, we control for heterogeneous effects by municipality to see whether geographic targeting explains the differential, which it does not, and in Column 4 we include the vectors for both municipality and occupation to show that the magnitude of the program difference remains similar to Column 1 at 0.013, although statistical precision is somewhat reduced. We thus conclude that broad targeting of training to occupations or municipalities does not explain the differential employment effects across segments, with the remaining explanation being that firms helped direct training to particular labor markets characterized by skill shortages in particular locations. This leaves us to conclude that the course requests from employers contained local-

labor-market-specific information about skill demand, and this information was of sufficient accuracy to increase program effectiveness.

Table 8: Pooled models with fully-specified heterogeneous treatment effects

Estimate:	Parametric difference	with heterog. course effects	with heterog. location effects	with course and municipality effects
	(1)	(2)	(3)	(4)
Informed segment differential	0.012**	0.016**	0.008	0.013
	(0.006)	(0.006)	(0.006)	(0.008)
R^2	0.24	0.24	0.25	0.25
N	17,120,004	17,120,004	17,120,004	17,120,004

Notes: Table presents coefficients from the pooled reduced-form model including different vectors of interacted fixed effects across columns. 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: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

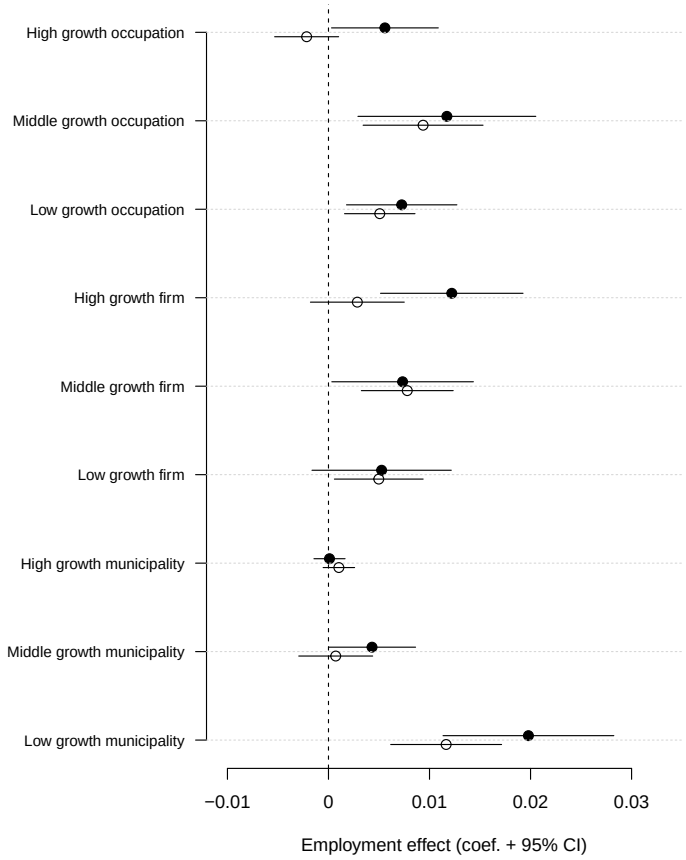
The second empirical test of the source of effectiveness directly estimates adjusted outcome measures that characterize employment by the nature of growth over 2014 exhibited by the location or occupation of the training. To do this, we calculate growth rates of occupations nationally, and municipalities in aggregate, from 2014 to 2019 in order to characterize post-training employment effects into higher- versus lower-growth areas or occupations. For simplicity, we group occupations and municipalities by the tercile of their employment growth rate over the period of 2014-2019, and refer to these groups as high, middle, and low growth units. Appendix Table 2 shows mean growth rates by tercile and dimension. The top tercile of municipalities, for example, saw employment grow by more than 30 percent over the course of 2014 to 2019 – while the bottom tercile saw employment contract by around 19 percent. Given the comprehensive employee-employer data available to us, we are able to characterize growth rates of firms over the 2014-2019 period as well, and additionally include this dimension in the analysis. The top tercile of continuing firms saw employment more than double on average, while the bottom tercile contracted by around 58 percent on average.

These indicators of relative growth rates are then used to estimate employment outcomes

redefined as taking the value of one if, for example, the individual was employed in a high growth municipality, occupation, or firm, and zero if unemployed or employed in a moderate- or low-growth municipality.[12] Figure 3 displays results graphically, where we compare coefficient magnitudes for each of the dimensions and growth-rate groups across the program segments. The increased employment effects in the employer-informed program stem from trainees finding employment in high-growth occupations, in high-growth firms, located in low-growth municipalities. The employer-provided information thus appears to direct skill formation toward economically lagging areas that have firms with high growth potential and employ workers in occupations that are also growing nationally.

[12]We recognize endogeneity concerns here, in terms of training programs potentially causing or facilitating growth in these dimensions, so we undertake this analysis only with the intention of empirically describing the *ex post* characteristics of employment that trainees found themselves in after the training course in order to shed light on the channels through which the informed program was able to be more effective.

Figure 3: Employment effects by subsequent occupation, municipality, and firm growth rates



Note: Figure depicts the point estimates and confidence intervals of employment effects across segment and alternative outcomes.

Tables 9 and 10 test a set of remaining hypotheses. The first is that the interaction between firms, the government, and training providers reduced search frictions, allowing requesting firms to find and hire workers more easily. We test for evidence of this hypothesis by linking the request records to the employment data to distinguish employment at requesting firms from that at all other firms. We also test whether either segment differentially increased labor market mobility in terms of facilitating workers to find employment in new occupations, new locations, or among new or certain types of employers.

Table 9: Sources of program employment effects by type of formal employment

	Formal employment					
Detail:	At any req. firm in municipality	At nonreq. firm	Same 3 digit industry as req. firm	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	0.000	0.023***	-0.001	0.025***	0.006*	0.018***
	(0.001)	(0.005)	(0.001)	(0.005)	(0.003)	(0.005)
R^2	0.28	0.24	0.35	0.24	0.38	0.26
N	5,274,984	5,274,984	5,274,984	5,274,984	5,274,984	5,274,984
Panel B: Traditional program						
Course offer * post	0.000	0.011***	-	0.012***	0.004**	0.007**
	(0.000)	(0.003)	-	(0.003)	(0.001)	(0.003)
R^2	0.27	0.24	-	0.24	0.38	0.28
N	11,845,020	11,845,020	-	11,845,020	11,845,020	11,845,020

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: $* \, p < .1$, $** \, p < .05$, $*** \, p < .01$.

Across Columns 1 and 2 in Table 9, we find clear evidence against the firm-specific search frictions hypothesis, as the effects in both segments were derived from employment in non-requesting firms. This finding further suggests that the skills requested were indicative of general skills shortages among several employers in the locality. We also use the industry information of each course's requesting firm to see whether employment effects were industry-specific; Columns 3 and 4 provide evidence that employment effects are derived from industries outside that of the requesting firm, again suggesting that the skills requested were neither employer- nor industry-specific. Columns 5 and 6 uses the fact that courses were designed to teach a particular occupation, which map directly to Brazilian occupational codes. While the two programs do not differ substantially in the degree to which effects are derived from employment in the occupation trained (Column 5), the higher effectiveness of the informed program appears largely attributable to employment increases outside the specific occupation for which students trained.

Table 10 uses the worker's employment history to further characterize the firms, industries, and areas in which the training program increased employment. We define a "new" employer, industry, or occupation as relative to the trainee's employment history – as one in which a trainee had no recorded employment history of working at or in over the period of 2009 to 2014. The informed program increased employment among employers, industries, and occupations new to the worker – but the largest difference between programs is among employment at large, 200+ employee firms. This difference – of approximately 1.1 percentage points in employment rate – comprises nearly the entire difference between the two programs' effects on employment of 1.2 percentage points in Table 1.

Taken together, the results above show that the employer-informed segment's greater effectiveness derived from targeting general skills training to particular labor markets (skill-location pairs) with demand from large, fast-growing firms located in low-growth municipalities.

Table 10: Sources of program employment effects by type of formal employment

Detail:	Formal employment					
	Same municipality	New employer	New industry	New occupation	Small firm (< 20)	Large firm (> 200)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employer-informed program						
Course offer * post	0.021***	0.033***	0.030***	0.021***	0.006*	0.014***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
R^2	0.27	0.43	0.38	0.35	0.28	0.30
N	5,274,984	5,274,984	5,274,984	5,274,984	5,274,984	5,274,984
Panel B: Traditional program						
Course offer * post	0.020***	0.023***	0.021***	0.015***	0.004**	0.003
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
R^2	0.28	0.42	0.37	0.35	0.29	0.30
N	11,845,020	11,845,020	11,845,020	11,845,020	11,845,020	11,845,020

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: $* p < .1$, $** p < .05$, $*** p < .01$.

5.5 Robustness and heterogeneity

5.5.1 Registrant sample with no segment choice

Even though both registrants and course providers were blind to the provenance of classes (whether employer-requested or not), and classes were listed 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. 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 11, closely mirror those in the full sample. Thus, in a sample of classes in which trainee selection across programs was not possible, we find results similar to those in the main estimates.

Table 11: Reduced form estimates for employment using course offer - subsample of municipality-months without segment choice

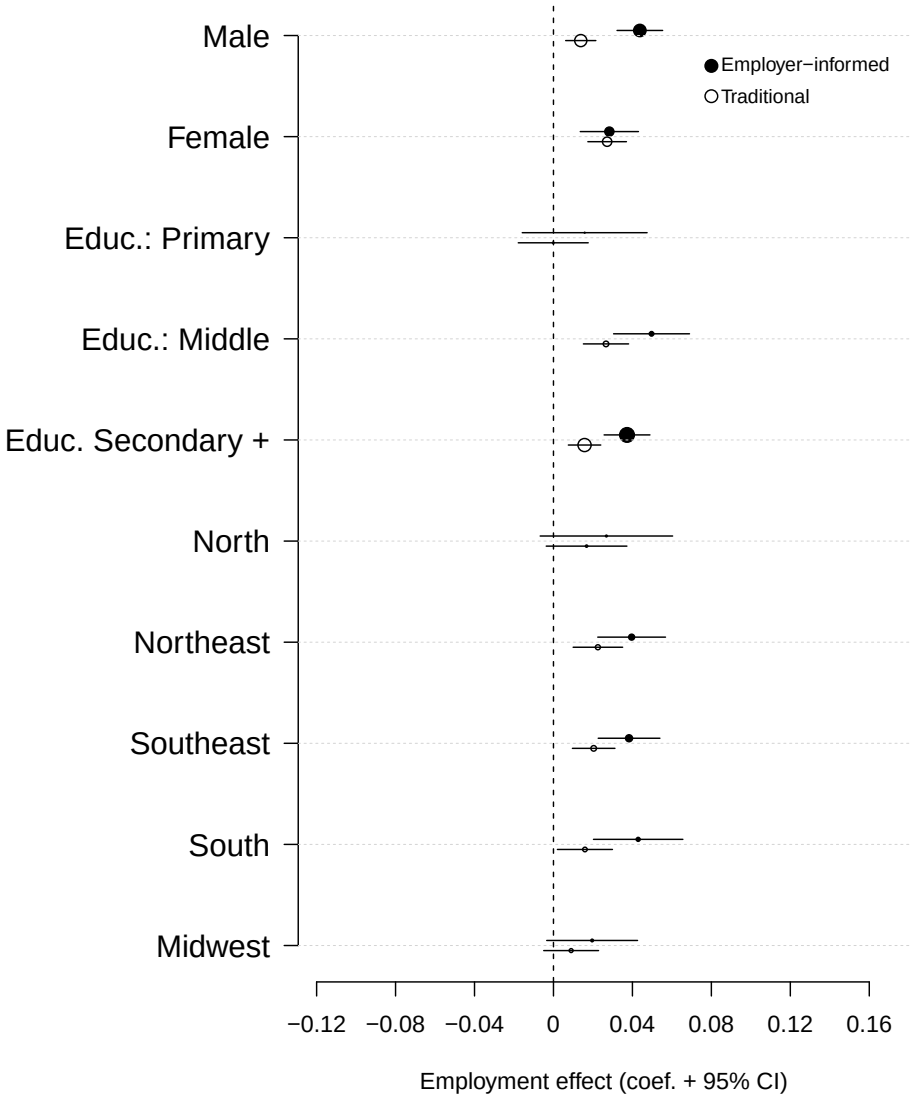
Outcome::	**Employment [0/1]**		**Earnings (2012 Rs)**	
Estimate	Informed	Traditional	Informed	Traditional
	(1)	(2)	(3)	(4)
Panel A: Effect of course offer (ITT)				
Course offer * post	0.037***	0.008	48.10***	14.22
	(0.008)	(0.006)	(12.60)	(9.77)
R^2	0.23	0.25	0.27	0.28
N	2,191,200	2,920,104	2,191,200	2,920,104

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: $* p < .1$, $** p < .05$, $*** p < .01$.

5.5.2 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. Coefficient 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 student demographics, 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.

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.

6 Conclusion

Job training programs are a common active labor market policy, and have a joint mandate of generating sustainable employment for workers while ensuring judicious use of public funds. Employer participation is often presented as a simple solution to the government's informa-

tion problem in designing job training programs that achieve these goals. This paper shows that a public job training program nearly doubled its effectiveness when using input from local employers to target training to skill-location pairs. The differential effects derive from employment in industries and occupations in which trainees had not previously worked, and particularly among large firms in the municipality of the training. Trainees find employment at non-participating firms, suggesting that the skills requested by employers in this context are likely to have been indicative of local skills shortages needed by more than just the requesting firms. Tempering these results is evidence that the value of employer input to such programs is not perennial: the differential effectiveness between the informed and traditional programs appears to close around three to four years after training was held.

The program analyzed is just one of the various designs that involve employers, and had several dimensions that precluded "capture" by the private sector – namely, a highly limited structure of the input taken from employers (place, course, and volume) and a significant retention of government oversight in the course provisioning process. Future work should investigate different informed program designs to compare their effectiveness both in the medium and longer-run. Although the overall magnitudes are small, our results empirically confirm the long-held belief that private sector input can make public job training programs more effective.

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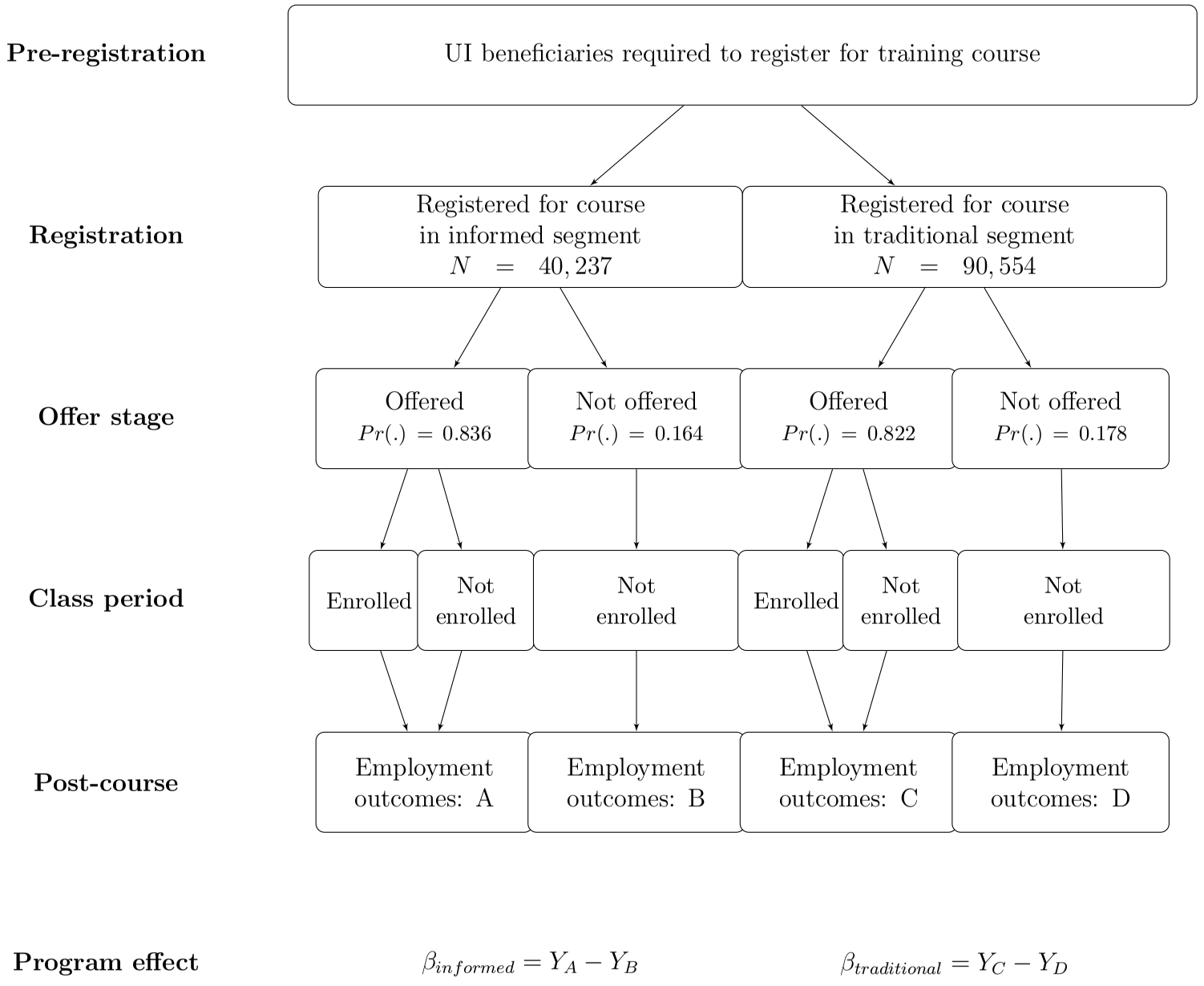
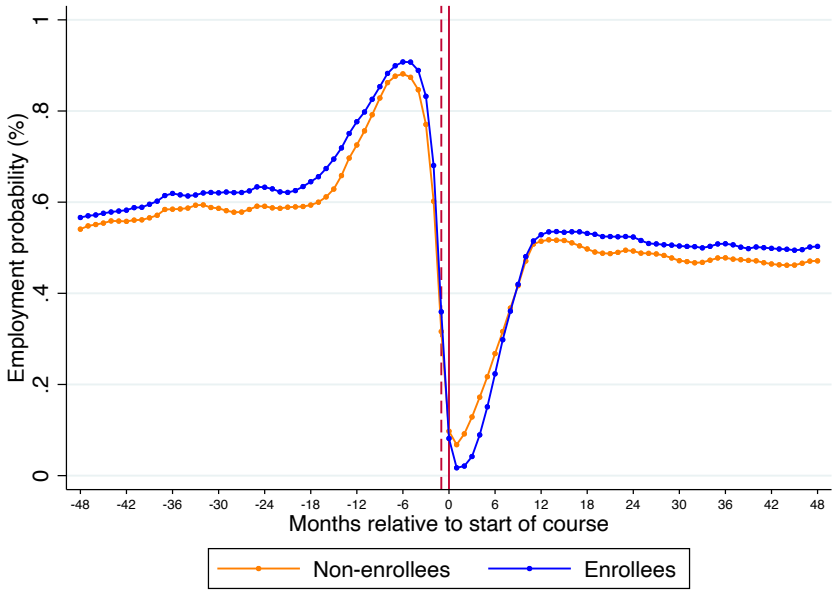
A1 Appendix Figures

Appendix Figure 1: Conceptual process flow for registrants

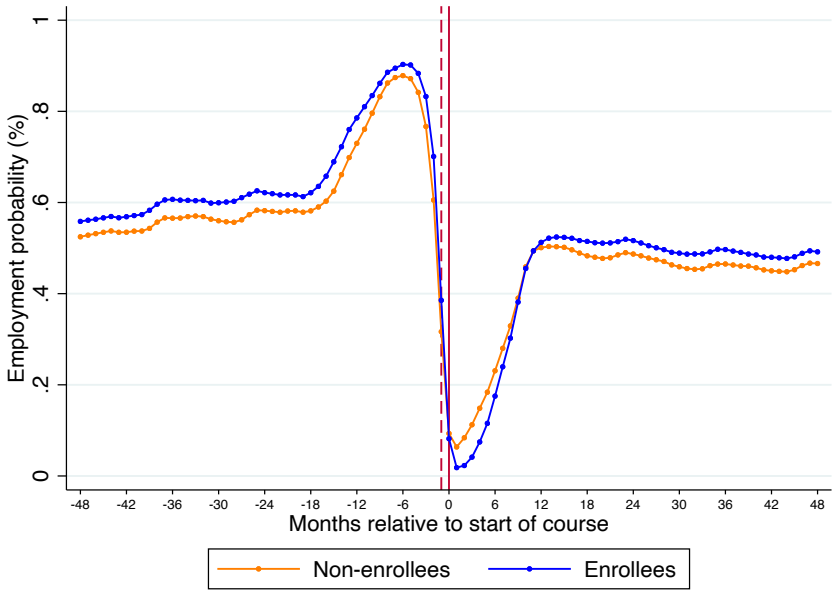


Appendix Figure 2: Mean employment rate relative to course start, employer-informed program



Note: Figure depicts the mean employment rate for enrollees and non-enrollees before and after the course. The dashed line represents the month prior to the projected start of the course.

Appendix Figure 3: Mean employment rate relative to course start, traditional program



Note: Figure depicts the mean employment rate for enrollees and non-enrollees before and after the course. The dashed line represents the month prior to the projected start of the course.

A2 Appendix Tables

Appendix Table 1: Program employment effects, OLS difference-in-differences estimates

Outcome::	**Employment [0/1]**		**Earnings (2012 Rs)**	
Estimate	Informed	Traditional	Informed	Traditional
	(1)	(2)	(3)	(4)
Panel A: Effect of course offer (ITT)				
Enrolled * post	-0.009**	-0.017***	-18.4****	-24.6****
	(0.004)	(0.003)	(6.54)	(4.68)
R^2	0.24	0.24	0.28	0.28
N	5,274,984	10,768,200	5,274,984	11,845,020

Notes: Table presents present difference-in-differences estimates of course enrollment on an indicator for employment as defined 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: $* \ p < .1$, $** \ p < .05$, $*** \ p < .01$.

Appendix Table 2: 2014 to 2019 Employment growth rates by growth tercile of occupations, municipalities, or firms

Tercile	Employment ratio: 2019/2014	std. dev.
(1)	(2)	(3)
Panel A: by municipality		
Top	1.33	1.43
Middle	0.98	0.04
Bottom	0.81	0.12
Panel B: by 3-digit occupation		
Top	1.09	0.12
Middle	0.88	0.04
Bottom	0.67	0.10
Panel C: by firm		
Top	2.64	14
Middle	0.91	0.11
Bottom	0.42	0.18

Notes: Table presents summary statistics of growth rates over the period of 2014 to 2015 by terciles of occupations, municipalities, or firms.