Training Courses and Formal Employment

Danilo Aristizabal [1]
March 2025

Abstract

This paper analyzes the effect of a training component of a labor intermediation policy (LIP) called Boost to Employment (BE) on the probability of finding a formal job for vulnerable unemployed workers in a developing country. To mitigate the selection problem I instrument for whether an unemployed worker who participated in BE received the training component of the program with a measure of leniency from their labor counselor. The labor counselor determined whether the job seeker received only labor orientation or labor orientation plus the training component of the program. I also test which courses are the ones that helped vulnerable job seekers to find formal jobs. I find that those workers who received the training component increased the probability of working in the formal sector by 20 percentage points one year after the implementation of the program compared to those workers who did not receive the training component. Moreover, I find that the course that is driving results is giving information about the value of formality, which increased the probability of job seekers being hired in the formal sector by 11 percentage points compared to those who were not assigned to the the formal job benefits course.

(**JEL**: J08, J15 , J21).

Keywords: Training, judge fixed effects, formal job.

[1]Universidad de los Andes. Email: de.aristizabal411@uniandes.edu.co

1. Introduction

There is a close relationship between poverty and social and labor exclusion. In Latin American countries one fifth of the 163 million young people neither study nor are employed in the labor market. This means that nearly 30 million young people are excluded from the two key factors of social inclusion: the education system and the labor market (CEPAL 2020). 80% of young people that neither study nor are employed come from poor or vulnerable households (Abramo, Cecchini, and Morales 2019). Active labor market policies (ALMPs) usually target people who have difficulties in finding employment and they usually come from poor or vulnerable households. Within these ALMPs there are formalization programs and information interventions to increase formal employment in developing countries (Jessen and Kluve 2021; Torm and Oehme 2024). There are also labor intermediation policies (LIPs) combined with classroom training[2] or private/public on-the-job training to help vulnerable job seekers to find formal jobs.

Do these training programs could help increasing formalization of vulnerable informal workers[3]? The traditional role of labor intermediation policies (LIPs) is to connect people wishing to improve their employment situation with vacancies in the productive sector by helping them with their job search, and assisting firms with candidate selection. In developed countries, policies also include linkages to training programs, unemployment insurance, and social programs (Mazza 2011). The empirical evidence, which comes mainly from developed countries, shows that labor intermediation is a cost-effective intervention for connecting workers with employers and helps to reduce unemployment duration (Card, Kluve, and Weber 2010; Simone Filgueira 2015). LIPs are also cost-effective when compared with other active labor market policies (Kluve 2010). LIPs seem to be more effective in periods and areas that generate more vacancies (Crépon et al. 2013; Lima, Zamora, and Contreras 2013). This is also true, when they have a greater focus on making connections and assisting firms, and when they assign specialized personnel to work with these companies and find vacancies (Behncke, Frölich, and Lechner 2010).

However, in developing countries, the results of impact evaluations show the weakness of some labor supply ALMPs in fulfilling their purpose of job placement in formal jobs. This is especially the case for the poorest and most vulnerable people (Farné 2016). One possible reason is the lack

[2] Classroom training refers to structured educational programs designed to enhance the skills and knowledge of workers participating in active labor market policies (ALMPs). These training sessions typically occur in a formal classroom setting and aim to equip individuals with both technical and soft skills necessary for improving their employability and facilitating their transition into the formal labor market.

[3] For vulnerable informal workers I mean workers with no experience in the formal sector because they do not have access to the Unemployment Protection Mechanism that I discuss in section 2.1. By formal job I mean a job where the worker makes financial contributions to the health system and a pension fund and can enjoy all the benefits of formality.

of knowledge among vulnerable workers about the benefits of formality. In addition, many informal workers in developing countries do not know where to apply for a formal job. There is little evidence showing that LIPs combined with a training component have any effect on formal employment for vulnerable informal workers in developing countries. LIPs plus a course about the benefits of formality could play an important role in developing countries and especially in cities where there are large vacancies in the formal sector. Knowing about the benefits of formality could increase job search intensity in informal workers and therefore could increase the probability to find a formal job in the short run for these group of workers.

The few impact evaluations of LIPs conducted in Latin American countries have several methodological limitations. Among these limitations, studies mixed the effects of different interventions (González-Velosa, Ripani, and Rosas Shady 2012). Evaluation of a LIP program as a whole limits the possibilities of knowing the component or combination of components most effective. Knowing the most effective component or combination of components is useful for policy makers when formulating LIPs. Likewise, it is not always indicated in research under what circumstances and contexts programs are most effective and a cost-effectiveness analysis is rarely incorporated. The self selection problem is usually another limitation when we want to evaluate the causal effects of LIPs on the probability to find a job because job seekers self-select into programs based on their preexisting job opportunities (Ashenfelter 1978; McKenzie 2017).

In this paper I analyze the effect of a training component of a LIP, called Boost to Employment (BE), in Bogota (Colombia), in terms of how much it aids vulnerable informal workers in obtaining a formal job. Boost to Employment program was created in 2022 to help people over the age 18, living in Bogota, with no access to any unemployment protection, to find a formal job. BE offered two packages to eligible participants: basic and integral packages. The program's operator had labor counselors who determined whether the participant was entitled to the basic or the integral package. The objective of the program was to give training to the participants with a stronger possibility (based on higher levels of education, experience and motivation) of being hired in the formal sector. The basic package consisted of helping the participant to construct their *curriculum vitae* (CV) and send it to formal job vacancies. The integral package consisted of the basic package plus some training courses to help the participants improve their skills needed to be hired in the formal sector. The assignment to a particular training course was determined by the labor counselor. The program finished at the end of 2022. It is important to say that BE was a policy targeting the supply of labor.

In order to identify the effect of the training component on the probability of finding a formal job, I use an empirical strategy called judge fixed effects design that has been used in numerous applications where judges or other types of program administrators are given discretion on how to respond to randomly assigned caseloads. For example, Kling 2006, Aizer and Doyle Jr 2015, and Mueller-Smith 2015 use it to estimate the impact incarceration on labor market outcomes and human capital accumulation in the United States; and Di Tella and Schargrodsky 2013 use it to estimate the impact of electronic monitoring on criminal recidivism in Argentina. Recently, Humlum, Munch, and Rasmussen 2023 use it to test whether an ALMP helps unemployed job seekers find jobs in Denmark.

My empirical strategy exploits the variation in discretion among labor counselors in the assignment of eligible job seekers to the training component of the program. This empirical design has also been called leniency design because there are some judges, in this case, labor counselors, who are relatively lenient in the assignment to the treatment. This empirical strategy helps me to identify the local average treatment effect (LATE) of a training component of a LIP on the probability of finding a formal job for a job seeker who were assigned to the training component of the program compared to job seekers who already had a base of receiving help with their CVs and connecting them to vacancies. This is the first paper that uses this identification strategy to evaluate a LIP in a developing country where the informal economy is very large and where evidence does not yet exist on whether training programs could help job seekers to find a formal job.

For my instrumental variable strategy to identify the causal effect of receiving the training component on the probability of finding a formal job, the exclusion restriction and the monotonicity condition must hold (Angrist and Imbens 1994). The exclusion restriction in this setting requires both random assignment of labor counselors and that labor counselors impact the outcome of interest only through the decision to assign a person to the training component of the program. The monotonicity assumption requires that either a labor counselor is strict or that they aren't, but they can't be both in different circumstances. For instance, a labor counselor may be lenient and send people to the training component, except when the participant is a man in which case they switch and become strict. I provide evidence for the identifying assumptions that validate my empirical strategy.

I established two headline findings. First, in the extensive margin of the treatment, my results indicate that job seekers who were assigned to the training component of BE increased the probability of finding a formal job one year after the implementation of the program by 20 percentage points compared to those who were not assigned to the training component. Second, taking a look on which course is more important in helping informal workers finding a formal job, I find that participants who received the formal job benefits course have better results in terms of finding a formal job, than participants who received any other course or that did not receive the training component of BE. This result gives evidence that explaining to job-seekers the value of formality seemed to have increased the job search intensity in the formal sector, which is then reflected in the increased probability of finding a formal job. Nevertheless, since FBC is being compared against being assigned to any other courses or against not being assigned to any course at all, I cannot determine in which of these two margins FBC is better (by margins, I mean (i) FBC vs. other courses, and (ii) FBC vs. no course). It could be that FBC course is better than receiving nothing, but it may not have much effect compared to other courses (or vice versa). However, this cannot be disaggregated with the available methodology and data. In conclusion, I know that FBC has a positive effect, but I do not know who it effectively benefits.

I make two contributions to the literature. First, to avoid the selection problem of training programs, I employ a novel identification strategy that has been applied in criminal justice using variation in propensities of rotating judges. In particular, I exploit the variation in the leniency among labor counselors as an instrument to approximate the probability that job seekers receive the training component. This is to capture the effect of a training program on the probability of finding a formal job in a developing country. Second, I contribute exploiting different components of a LIP to evaluate which, and to what extent, courses of the training component help vulnerable job-seekers find formal jobs in the short run.

The remainder of this paper is organized as follows. Section 2 introduces the context and the main characteristics of Boost to Employment program. Section 3 shows the data I use in the analysis. Section 4 discusses the empirical strategy and the identifying assumptions. Section 5 displays the main results. Section 6 presents a short cost benefit analysis and section 7 concludes.

2. Institutional setting

2.1 Context

Colombia has both high unemployment rates and high informality rates. In fact, Colombia is one of the countries with the highest unemployment rates in Latin America, around 10%, and nearly 60% of employed workers have an informal job (DANE 2024). The situation in the capital of Colombia, Bogota, is different to what happens at the national level, at least in terms of the informality rate. In fact, Bogota has an unemployment rate around 10% and an informality rate near 33% (DANE 2024). These characteristics make Colombia and in particular Bogota, an ideal city to see how a labor intermediation policy with a training component affects the probability of finding a formal job.

In Colombia there exists a program called Unemployment Protection Mechanism (UPM) that provides workers access to health, savings for pensions, monetary subsidy, and training courses to help them find a new formal job. This is for cases where they lose their existing jobs or when a person is working in the informal sector and they want to find a job in the formal sector. Training courses are given to program participants through Family Compensation Funds (CCFs)[4] and at the same time job seekers are sent to job vacancies in the formal sector. However, not all workers have access to the UPM. In order to have access to the UPM, unemployed workers must have contributed to a CCF for a year, in the last three years before being unemployed. For this, they must have been employed formally. Unemployed workers who do not have access to the UPM find it very difficult to match with an employer in the formal economy and therefore they will end up in two possibles states: unemployment or acquiring an informal job.

There is an important difference between unemployed workers who were previously working in the formal sector and those unemployed workers who were working in the informal sector. The first group is eligible for the UPM so they routinely have greater help for transitioning from unemployment to a new formal job. However, the second group find it more difficult since they are not eligible for the UPM and therefore they do not have the opportunity to receive training or upgrade their skills to find a formal job.

2.2 The program: Boost to Employment

In 2022, the Secretary of Economic Development in Bogota (SEDB) started a program called Boost to Employment. The Boost to Employment program was created to help people over the age 18, living in Bogota, with no access to any unemployment protection, to find a formal job. When a worker applied to the program, the program operator verified whether the person did not have access to any unemployment protection benefit in which case the person was eligible for the program. The program offered two packages to eligible participants: basic and integral packages. The program's operator had labor counselors who determined whether the participant was entitled to the basic or the integral package. This decision was based on the level of education, experience and motivation of the participant, which was determined in an interview with a labor counselor that lasted one hour. The basic package consisted of helping the participant to construct their *cur-*

[4]CCFs are private, non-profit entities, created with the purpose to manage the Family allowances and provides protection for unemployed people.

riculum vitae (CV) and send it to formal job vacancies. The integral package consisted of the basic package plus some training courses to help the participants improve their skills needed to be hired in the formal sector. The assignment to a particular training course was determined by the labor counselor. The objective of the program was to give training to the participants with a stronger possibility (based on higher levels of education, experience and motivation) of being hired in the formal sector.

Table 1. Training component courses of Boost to Employment

Courses	**Hours**	**Participants**
Successful interviews	4	3804
Formal job benefits*	8	2966
Excel	50	3281
Project management	40	217
Business management	40	424
Soft skills**	40	2070
Basic software tools	30	137
Food handling	10	156
Inventory management	40	220
Handling of dangerous substances	40	141
Good practices in manufacturing	40	129
Marketing	40	766
Data processing and analysis	40	1369
Programming	40	87
Customer service and sales	40	1569
People management techniques	40	257

Notes: This table provides the names of the courses given by the program operators in the training component. Some courses were only offered by one operator, while others were offered by the three program operators. *The formal job benefits course consisted in telling job seekers what are the benefits of being hired in the formal labor market, such as job stability, better job amenities, paid vacations and all mandated benefits associated with formal employment. **The soft skill course included time management, communication skills, teamwork and decision making. This course tried to develop transversal skills needed in all types of jobs.

Table 1 lists the names of the courses, the duration in hours of every course and the participants enrolled in each one. This information is taken from the program operators that I discuss in more detail in section 3. There were a variety of courses ranging from successful interviews and soft skills, to more technical courses such as excel and good practices in manufacturing. When a worker visited an operator to participate in the program, they were assigned to a labor counselor who determined whether the person would receive the basic or the integral package. If the job seeker was assigned to the integral package, the labor counselor determined which courses the worker would take. Some participants were assigned to more than one course by a labor counselor. The courses offered by the operator were short-term duration courses varying between 4 and 40 hours. For example, the successful interviews course lasted 4 hours, the formal job benefits one was 8 hours, while other courses such as soft skills or Excel had a duration of 40 hours. 50% of the courses were virtual, had an average of 20 participants, and according to the operators there was high dropout, nearly 50%. This was especially true for the courses that lasted more than 30 hours.

The operators of the Boost to Employment program were CCFs, which designed the courses of the training component. 90% of participants who finished the courses they were assigned, rated the courses as very good quality. Since I am interested in the effect of the training component of the program on the probability of finding a formal job, in my setting people who received the training component will be my treatment group while those who only received the basic package will be my control group. I discuss more the empirical strategy I use in section 4.

3. Data

I have three sources of data. The first kind comes from the operator that implemented the Boost to Employment program. It contained data from people who were assigned to the basic and integral packages in 2022. Information on individual characteristics such as age, gender, and the neighborhood where the person lives was included. The second type was from the Public Employment Survey platform (SISE), which has education and experience information for all workers who use said platform. However, I only have education level and years of experience for some participants of the program, 50% of the sample. Finally, using administrative records I obtain information on whether the person is formally employed or unemployed one year after the implementation of the program. It is important to say that the Secretary of Economic Development in Bogota merged the administrative records with the operator's data.

I apply four sample restrictions to support my identification strategy. First, I use the sample of one program operator, because it is the only one for which I have information on the labor counselor. This operator served 10,000 job seekers. Second, I exclude observations that did not receive labor orientation. This happened because there were budget restrictions: only 5,000 job seekers received labor orientation. Figure A5 in the appendix shows the distribution of participants by labor counselor. Third, to reliably estimate training tendencies by labor counselors, I require that each labor counselor in the sample must have at least 20 assigned job seekers over the sample period. Fourth, I exclude job seekers whose education level and years of labor experience I cannot identify from the Public Employment Survey platform. I make this restriction because these two variables have high explanatory power for whether the job seeker was assigned to the training component or not.

Therefore, I need to control for these two variables in the regression analysis. The final sample for my analysis is 3,200 job seekers.

The operator of the program had 80 labor counselors and on average a labor counselor served 80 job seekers. These labor counselors assigned the treatment based on three criteria: motivation, labor experience and the education level of the person. The objective of the program was to give training to the participants with a higher probability of being hired in the formal sector. One important point to the identification strategy is that eligible participants could not choose the labor counselor. The operator assigned labor counselors to participants based on said counselors availability.

Table 2. Descriptive statistics of Boost to Employment program

	Number of participants			
	No training	Training	Sample	Difference
Panel A: Program operator				
Operator A	1211	2022	3233	
Panel B: Individual characteristics				
Female	738	1378	2116	
Male	473	644	1117	
Average age	28,6	31,4	30,4	-2,82***
Average years of experience	4,42	5,43	5,05	-1,01***
Panel C: Education				
Unfinished high school	62	73	135	
	45,93%	54,07%	100%	
High school	545	757	1302	
	41,86%	58,14%	100%	
Technical and technology	354	679	1033	
	34,27%	65,73%	100%	
Bachelor degree	250	513	763	
	32,77%	67,23%	100%	
Panel D: Formal job				
Participants hired in a formal job	59,4%	67,6%		

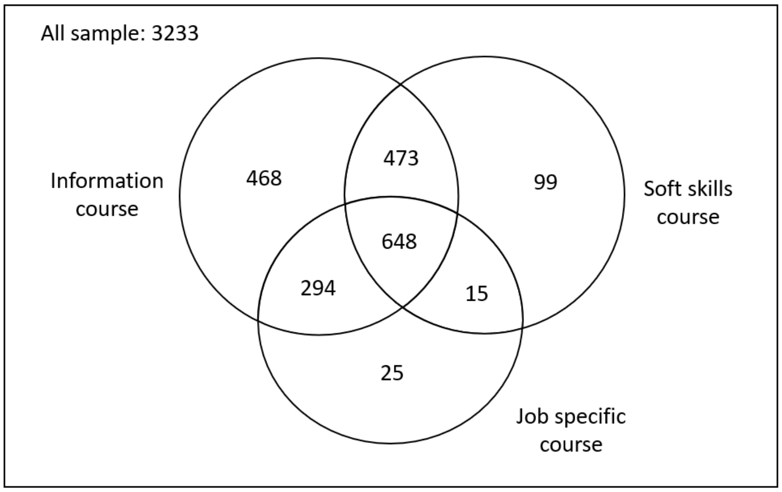
Note: This table shows the descriptive statistics of the Boost to Employment (BE) program developed by the Secretary of Development in Bogota. No training means job seekers who were assigned to the basic package, while training means job seekers who were assigned to the integral package. The basic package consisted of helping the job seeker to construct their *curriculum vitae* (CV) and send it to formal job vacancies. The integral package consisted of the basic package plus some training courses determined by the labor counselor.

Table 2 shows the summary of statistics for participants in Boost to Employment for the program operator, I call it operator A, after applying the sample restrictions described above. Panel A shows the number of participants in the program. Panel B shows that on average, people who received training were older than people who did not receive training, had more years of experience and were mostly female. This is consistent with criteria that labor counselors used to assign to the training component of the program. In fact, panel C shows that as the level of education increased, the fraction of people who received training also increased. Finally, panel D shows the percentage of participants from the basic and integral packages who were hired in the formal sector one year after the implementation of the program. In particular, 59% of participants who received the basic package and 67% of participants who were assigned to the integral package, had a formal job one year after the implementation of the program. The summary of statistics of my sample are similar to the summary of statistics when using the participants for all the three program operators which is shown in table A1 in the appendix.

Operator A offered 7 courses: formal job benefits, soft skills, basic accounting, intermediate excel, basic software tools, customer service and sales and business management. I group them into three types of training courses according to their nature: formal job benefits (FBC), soft skills (SK) and job specific (JS) courses. There are 5 job specific courses in total. Table A2 in appendix shows summary statistics on the number and frequency of different courses that job seekers can take. On average, each participant was assigned to 1.2 courses. 15% of participants was assigned only to the FBC, 15% was assigned to the FBC and SK courses, 9% was assigned to the FBC and JS, and 20% was assigned to the three courses: FBC, SK and JS. Table A2 also shows the average hours of coursework for each course or combination of courses.

The labor counselor determined how many courses the job seekers who were assigned to the training component of the program took. The FBC course consisted of telling job seekers what the benefits were of being hired in the formal labor market. These benefits include job stability, better job amenities, paid vacations and all mandated benefits associated with formal employment. The SK course included time management, communication skills, teamwork and decision making. This course attempted to develop transversal skills needed in all types of jobs. Finally, JS courses attempted to develop skills, such as excel or customer service and sales, needed in particular jobs. Figure 1 shows the distribution of people who were not assigned to take any course, 1,211 participants, those who were assigned only to one course, 592 participants, and those who were assigned to take two or three courses of the training component, 782 and 648 respectively.

Figure 1. Distribution of the courses taken by job seekers given by the program operator



Note: This figure reports the courses taken by job seekers who were assigned by the labor counselor. Some job seekers were assigned to only one course, while others were assigned to more than one course.

4. Empirical strategy

I am interested in the relationship between receiving the training component of the program and being hired in the formal labor market, which is represented in the following equation:

$$hired_i = \lambda_0 + \lambda_1 training_i + \lambda_2 Controls_i + \varepsilon_i \tag{1}$$

where $hired_i$ is an indicator for whether individual i was hired in the formal sector one year after the implementation of the program, $training_i$ is an indicator of whether the job seeker i received the training component, λ_1, which is the parameter of interest, it states what the effect is of receiving training on the probability of being hired in the formal labor market, which I expect to be positive. $Controls_i$ are individual characteristics, such as age, gender, education level, labor experience, and the locality where the job seeker lives, and ε_i is the error term.

I cannot estimate equation (1) using OLS estimates because there are unobserved factors that are correlated with both receiving training and being hired in the formal sector, so OLS estimates, which assume selection on observables only, would be biased (Dale and Krueger 2002). For example, job seekers with more ability may decide to take training programs because they find it easier to take those courses and at the same time they have a higher probability of being hired in the formal economy because they are more productive. Similarly, it can also be the case that job seekers with less ability decide not to take the training program because they find it hard to finish and at the same time they have a lower probability of being hired in the formal sector because their productivity is not high enough (Ashenfelter 1978).

The literature on job training has used several methods to overcome this identification challenge. A growing strand of the literature uses randomized controlled trials (RCTs) that have the potential to address the identification challenges in ALMPs with training components (Baird, Engberg, and Gutierrez 2022; Attanasio et al. 2017). However, the results from RCTs have many concerns because of external validity. IV methods are uncommon in the vast literature evaluating ALMPs, due to data limitation and lack of exogenous variation determining selection into training.[5] I follow the judge IV literature, and in particular I exploit the variation in the leniency among labor counselors as an instrument to approximate the probability that job seekers receive the training component of the program. Using the leniency of labor counselors as instruments helps me to capture the causal effect of training on the probability of finding a formal job. This is the case because the leniency of labor counselors affects whether the participant receives the treatment or not and at the same time, counselors are not directly affecting the probability of finding a formal job. In other words, labor counselors are not an omitted variable. This approach has been commonly used for identifying causal effects in criminal justice using variation in propensities of rotating judges (Kling 2006; Mueller-Smith 2015), and recently it has also been used in the literature on ALMPs with training components (Humlum, Munch, and Rasmussen 2023). I will use two estimators: Two-Stage least squares (2SLS) and jacknife instrumental variables estimator (JIVE).

In my first approach to estimate the causal effect of being assigned to training on being hired in the formal sector, I estimate a standard two-stage least squares (2SLS) using as instruments the labor counselors. First stage will be:

$$training_i = \omega_0 + \sum_j \pi_j lcounselor_{ji} + \omega_1 Controls_i + \epsilon_i \tag{2}$$

Where $lcounselor_{ji}$ are dummy variables that takes the value of one if individual i was assigned to labor counselor j and 0 otherwise. In equation (2), the coefficients π_j measure differences in the probability of being assigned to the training by the counselor j, I called it the measure of leniency (Stevenson 2018; Humlum, Munch, and Rasmussen 2023). And the second stage will be:

$$hired_i = \beta_0 + \beta_1 \widehat{training}_i + \beta_2 Controls_i + \nu_i \tag{3}$$

[5] McCall, Smith, and Wunsch 2016 identify only one published study using an IV approach: Frölich and Lechner 2010. Their instrument exploits variation in training propensities across regions in Switzerland. Cederlöf, Söderström, and Vikström 2021 develop a caseworker instrument for the Swedish labor market to examine the characteristics of effective caseworkers. They do not evaluate the impact of ALMP programs on the unemployed.

In order for β_1 to identify the causal effect of training on the probability of finding a formal job, instruments $lcounselor_{ij}$ must satisfy the usual assumptions of relevance, independence, exclusion, and monotonicity (Angrist and Imbens 1994). I discuss the four assumptions applied to the Boost to Employment program in subsection 4.1.

However, even if we satisfy these assumptions, when there are many instruments, the consistency of the estimation for the first stage coefficients becomes questionable because we may have a problem with many weak instruments (Bekker 1994; Mikusheva and Sun 2024). In particular, Angrist, Imbens and Krueger (1999) show that bias of 2SLS estimator is equal to

$$E(\widehat{training}_i \epsilon_i) = \frac{K}{N}\sigma_{\epsilon\nu} \tag{4}$$

Where $\widehat{training}_i$ is the first-stage fitted values, K is the number of instruments, N is the number of observations and $\sigma_{\epsilon\nu}$ is the covariance between ϵ and ν, which in general is not equal to zero. Even though the bias vanishes in large samples as $K/N \to 0$, it increases with the number of instruments for a fixed sample and a fixed $\sigma_{\epsilon\nu}$.[6]

The bias of the 2SLS estimator arises from the correlation between the OLS estimate of the optimal instrument matrix $lcounselor_i\hat{\pi}$ and the residual ϵ_i. Thus what is needed is an alternative estimator of $lcounselor_i\pi$ that does not suffer from such correlation. Angrist, Imbens and Krueger (1999) and Blomquist and Dahlberg (1999), building on the work of Phillips and Hale (1977), suggest using all observations except observation i to estimate the parameter matrix π and then using this estimate along with $lcounselor_i$ to compute the fitted value of the instrument for observation i (Blomquist and Dahlberg 1999; Phillips and Hale 1977). This process is repeated for each $i = 1,...,N$. That is, let

$$\hat{\pi}_{-i} = (lcounselor^{\intercal}_{-i} lcounselor_{-i})^{-1} lcounselor^{\intercal}_{-i} training_{-i} \tag{5}$$

Where $lcounselor_{-i}$ denotes the $(N-1) \times K$ matrix consisting of all rows of $lcounselor$ except the *i*th row and similarly for $training_{-i}$. The *i*th row of the optimal instrument matrix is estimated by $lcounselor_i\hat{\pi}_{-i}$. Notice that

$$E(\hat{\pi}^{\intercal}_{-i} lcounselor^{\intercal}_i \epsilon_i) = 0 \tag{6}$$

[6] Moreover, this bias persists even if the instruments $lcounselor_i$ are uncorrelated with ϵ_i (as valid instruments must be) (Nagar 1959).

because observations are assumed to be independent[7]. Therefore, the Jacknife Instrumental Variables estimator (JIVE) defined by

$$\hat{\beta}_{JIVE} = (\widehat{\textbf{training}}^{\intercal}\textbf{training})^{-1}\widehat{\textbf{training}}^{\intercal}\textbf{hired} \tag{7}$$

where the ith row of $\widehat{\textbf{training}}$ is defined as $lcounselor_i\hat{\pi}_{-i}$, does not suffer from the finite-sample bias of 2SLS. Therefore, to avoid the finite sample bias of 2SLS, in a second step I use the JIVE.

4.1 Instrument diagnostics

In this section I provide evidence that the four fundamental assumptions behind the judge IV design seemed to be satisfied in my setting. First I test the relevance assumption. Table 3, presents descriptive statistics of the labor counselors and the average number participants per labor counselor, who were sent to the training component of the program, and the F-statistic of the first stage regression with and without individual controls. Table 3 shows that the program operator had 80 labor counselors who served on average 64 participants. It also shows that an average 49% of people served by a labor counselor were sent to the training component of the program, but there was high variability among them, which is reflected in the different percentiles of the unconditional leniency.[8] Table 3 also reveals that the F-statistic of the first stage regression is above 10 with and without individual controls, using the labor counselors as instruments for training. I also applied the recently proposed approach by Angrist and Kolesár (2024) that suggests conditioning on the sign of the estimated first stage between treatment and the jackknifed instrument. They showed that conditioning on a right-signed estimated first stage reduces weak-instrument bias without distorting inference.

Second, I test for the independence assumption. Job seekers who applied to the program could not choose the labor counselor who determined whether the job seeker received the training component of the program or not. In Table 4, I test the independence of the labor counselor instrument. The table is based on the following logic: if job seekers cannot choose the labor counselor, I should not be able to predict a labor counselor's leniency based on the characteristics of job seekers (Humlum,

[7] Notice that

$$\begin{aligned}
E(\widehat{training}_i\epsilon_i) &= E\{(training^{\intercal}_{-i}lcounselor_{-i}(lcounselor^{\intercal}_{-i}lcounselor_{-i})^{-1}lcounselor^{\intercal}_i\epsilon_i)|lcounselor\} \\
&= E\{E(training^{\intercal}_{-i}\epsilon_i|lcounselor)lcounselor_{-i}(lcounselor^{\intercal}_{-i}lcounselor_{-i})^{-1}lcounselor_i\} \\
&= 0
\end{aligned}$$

[8] Figure A1 in the appendix shows a histogram of the collapsed residuals by labor counselor for the regression of training over control variables. Figure A1 shows that there is high variability in leniency among labor counselors.

Table 3. Descriptive statistics of labor counselors and unconditional leniency

Labor counselors	Average particip. per counselor	Uncondit. leniency (UL)	Percentiles of (UL)					F-stat w/o controls	F-stat with controls
			10	25	50	75	90		
80	64	49.77%	0	12	63	74	82	19,19	13,68

Note: This table reports the average number of participants per labor counselor, the average unconditional leniency, different percentiles of the unconditional leniency and the F-statistic of first-stage without and with controls. Controls include age, age squared, gender, level of education and previous experience measured in years.

Munch, and Rasmussen 2023; Stevenson 2018; Dahl, Kostøl, and Mogstad 2014).

The independence test yields two takeaways. First, the assignment of job seekers to training component is highly endogenous (Column (1)), confirming the common finding that job seekers select into training. Second, labor counselor training tendencies and job seekers' characteristics are uncorrelated (Column (2)), with the exception of experience suggesting that job seekers with higher experience were assigned to more lenient judges.[9] However, this should not be a big problem because I can control for labor experience in my specification.

Third, the exclusion restriction requires that the labor counselor leniency affects job-seeker outcome only through the assignment to the training component. An obvious threat to the exclusion restriction is that a labor counselor serves multiple purposes: assigning the participant to the training component and giving job search advice. However, the labor counselor was only in charge of helping the participant to build their *CV* and decide whether or not assign the job seeker to the training component. The person in charge of sending the eligible job seeker to job vacancies was different from the labor counselor. In addition, as I discussed previously, the education and experience of the job seeker were variables that affected the assignment of receiving the training component of the program, therefore, after I control for these two variables, the labor counselor's decision affects the outcome variable only through their decision to assign a person to the training component of the program.

[9] column 2 of table 4 shows results from regressing covariates on the leave-one-out measure of judge leniency. The leave-one-out is computed in the following way:

$$leniency_i = \frac{\sum_{k \neq i \in \Omega_j} training_k}{n_j - 1} \tag{8}$$

Where Ω_j is the group of participants assigned to the labor counselor j and n_j denotes the total number of job seekers assigned to labor counselor j.

Table 4. Testing for Random Assignment of Job seekers to Labor Counselors

	(1) Training	(2) Judge leniency
Male	-0.0617***	-0.0132
	(0.0202)	(0.0119)
Age	0.0058***	0.0010
	(0.0012)	(0.0007)
Experience	0.0016	0.0043***
	(0.0024)	(0.0012)
Education		
Primary	-0.0710	-0.0285
	(0.0697)	(0.0404)
Secondary	-0.1133*	-0.0652
	(0.0671)	(0.0435)
Technical	0.0545**	0.0147
	(0.0254)	(0.0148)
Technology	0.0709**	0.0237
	(0.0352)	(0.0196)
Bachelor degree	0.0877***	0.0175
	(0.0279)	(0.0159)
Master	-0.0301	0.0032
	(0.0455)	(0.0236)
Observations	3,233	3,233
R-squared	0.2119	0.1948
F-statistic	9.00	4.58
Neighborhood FE	Yes	Yes

Notes: This table implements a randomization test of the labor counselors' instruments. There are 80 labor counselors. Column (1) regresses the assignment to the training component on job seekers' covariates. Column (2) regresses the judge leniency on the job seekers' covariates. The judge leniency is constructed by calculating the leave-out mean for all cases a labor counselor has handled. Both regressions include neighborhood fixed effects. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Fourth, the monotonicity assumption requires that the instrument weakly operates the same across all participants in the program. This assumption implies that either a labor counselor is strict or they are not, but they can't be both in different circumstances. Yet humans are complex bundles of thoughts and experiences, and biases may operate in non-transitive ways. For instance, a labor counselor may be lenient and send people to training, except when the participant is a man, in which case they switch and become strict. Given this concern, I use the test proposed by Frandsen, Lefgren and Leslie (2023) to assess both exclusion and monotonicity based on relaxing the monotonicity assumption. This test requires that the average treatment effect among some individuals who violate monotonicity be identical to the average treatment effect among some subset of individuals who satisfy it. This test is based on two observations: the average outcome, conditional on labor counselor assignment, should fit a continuous function of labor counselor propensities, and secondly, the slope of that continuous function should be bounded in magnitude by the width of the outcome variable's support (Frandsen, Lefgren, and Leslie 2023). Figure A4 shows that both conditions are satisfied and therefore the identifying assumptions seemed to be satisfied.

5. Results

In this section I use the instruments proposed in the empirical strategy to estimate the effect of receiving training on the probability to find a formal job for job seekers, using 2SLS and JIVE estimators. I benchmark my results to OLS estimates that assume selection on observables only.

5.1 Estimation results

Table 5 shows the results for OLS estimates where the dependent variable is being hired one year after the implementation of the program. Column (1) of table 5 shows that those who were assigned to the training component increased the probability of being hired in the formal sector by 7 percentage points. However this effect is only significant at 10% once I control for neighborhood fixed effects, column (2).[10] This estimation may underestimate the real effect of training on employment because it may be the case that job seekers with worse job prospects are the individuals who opt into training or are more likely to be assigned to training by counselors, revealing a prospective version of the Ashenfelter dip (Ashenfelter 1978).

Table 6 shows the main results using the labor counselors as instruments for receiving training compared to OLS estimates. Column (2) shows the results using 2SLS. The effect of receiving training is three times the coefficient of OLS, suggesting that OLS was underestimating the real impact of training on formal employment. Furthermore, using the jacknife instrumental variables estimator (column (3) of table 6) the effect of training on the probability to find a formal job is 21 percentage points, 2 percentage points more than the results from 2SLS. To understand the magnitude of these results, the job seekers who were assigned to the basic package and found a formal job one year after the implementation of the program was 59%. Therefore, the probability of being

[10]I also include a quadratic term for the experience and age in the control variables because these variables seem to have a quadratic component in the probability of being employed (see figure A2 and A3 in the appendix).

Table 5. OLS estimates from operator A

	Formal job (1)	(2)
Training	0.0777***	0.0681*
	(0.0177)	(0.0362)
Observations	3,233	3,233
R-squared	0.0394	0.2239
Controls	Yes	Yes
Neighborhood FE	No	Yes

Notes: This table reports the results using OLS estimates. Individual controls include gender, age, age squared, labor experience measures in years and labor experience squared. All Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

hired in the formal sector for those who received the training component compared to those who did not, increased by 36%. These results are consistent with what other authors have found for the case of Denmark using a similar empirical strategy (Humlum, Munch, and Rasmussen 2023). In Colombia, using panel data, the probability of making a transition from an informal to a formal job in one year, is around 28% for a sample with similar characteristics to the one I use in this analysis (Chaves Hernández 2016). This suggests, that not only the training helped job seekers find a formal job, but the basic package also increased the probability of finding a formal job. However, I cannot identify the effect of the basic package since everyone in my sample received help with their CVs and were referred to formal vacancies.

Table 6. OLS, 2SLS and JIVE estimation

VARIABLES	OLS	2SLS	JIVE
Training	0.0681*	0.1937***	0.2141**
	(0.0362)	(0.0654)	(0.0882)
Observations	3,233	3,233	3,233
R-squared	0.2239	0.2611	0.2443
Controls	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes

Notes: This table reports the effect of assignment to training on formal employment one year after the implementation of the program using OLS, IV and JIVE estimators. Individual controls include gender, age, age squared, labor experience measured in years and labor experience squared. All regressions include neighborhood fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Different mechanisms could explain why the training courses help job seekers find a formal job. One possible reason is that job seekers now make more successful job applications because the training provides new skills that make the job seeker better suited for the applied jobs. Another possible mechanism is that the training component may provide job seekers with a network and informal ties to the target sector, which could create new job opportunities (Katz et al. 2022). In section 5.2 I provide evidence that suggests that the second mechanism seems to be driving my results.

5.2 Types of courses

The results discussed so far provide evidence about the effect of receiving the training component of the program on the probability of finding a formal job. I refer to this part of the treatment (being assigned to it) as the “extensive margin” of the treatment. However, I am also interested in which courses are most important in helping job seekers find a formal job (Humlum, Munch, and Rasmussen 2023).

I have multiple treatments in the training component, and participants could have been assigned to one course or a combination of courses. Due to the limited number of observations assigned solely to the soft skills or job-specific courses, I compared participants assigned to the FBC with those assigned to any other course or not receiving the training component. Again, I use labor counselors as instruments to estimate the 2SLS estimator. The parameter λ_1 in equation 9 examines the effect of FBC on the probability of finding a formal job.

$$hired_i = \lambda_0 + \lambda_1 FBC_i + \lambda_2 Controls_i + \varepsilon_i \tag{9}$$

Table 7 shows the first-stage results using labor counselors as instruments. Counselors have a strong influence on the assignment to the Formal Benefits course. The estimates are highly significant and indicate that a participant assigned to a labor counselor that is 10 percentage points more likely to send job seekers to the training component, is 4 percentage points more likely to be assigned to the training component (see Table A3). Following Bhuller et al. (2020), I report the Effectvie F-statistic of 18.42 for FBC, which is above the Montiel Pfluegger critical value of 11.97 for a worst case bias of 10%.

Table 7. First stage results for FBC

VARIABLES	FBC
Male	-0.0540***
	(0.0180)
Experience in years	-0.0053
	(0.0051)
Experience squared in years	0.0001
	(0.0002)
Age	0.0210***
	(0.0059)
Age squared	-0.0002***
	(0.0001)
Primary education	-0.0803*
	(0.0422)
Secondary education	-0.0531
	(0.0625)
High shcool	-0.0112
	(0.0585)
Technician	0.0168
	(0.0225)
Technologist	0.0326
	(0.0316)
Bachelor degree	0.0353
	(0.0260)
Constant	-0.0484
	(0.1486)
Observations	3,233
R-squared	0.4616
F statistic	33.66
Effective F statistic	18.42
Labor counselor FE	Yes
Neighborhood FE	Yes

Notes: This table reports the first stage results of being assigned to the FBC using labor counselors as instruments. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The formal job benefits course consisted of telling job seekers what the benefits are of being hired in the formal labor market. These include job stability, better job amenities, paid vacations and all mandated benefits associated with formal employment. This result shows that explaining to job-seekers the benefits of a formal job seemed to have increased the job search intensity in the formal sector, which is then reflected in the increased probability of finding a formal job (Lichter 2016). Table 8 shows that being assigned to the FBC increased the probability of finding a formal job by 11 pp compared to those who received any other course different from the FBC or did not receive the training component of the program, one year after BE implementation.

Table 8. 2SLS estimation for job-seekers assigned to FBC vs No training or receiving any other course different than FBC

VARIABLES	Formal job
$\widehat{FBC}$	0.1164**
	(0.0362)
Observations	3,233
R-squared	0.2149
Controls	Yes
Neighborhood FE	Yes

Notes: This table reports the effect of being assigned to the Formal Benefits Course (FBC) of Boost to Employment on formal employment one year after the implementation of the program using 2SLS estimator. Individual controls include gender, age, age squared, labor experience measured in years and labor experience squared. All regressions include neighborhood fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

In summary, my results suggest that the formal jobs benefits course (FBC) is driving the positive effect of the training component of Boost to Employment on the probability of being hired in the formal sector one year after the implementation of the program. This is significant for policy-makers who are interested in implementing ALMPs for increasing formal employment in developing countries. It is important to acknowledge the limitations of my findings. Specifically, since FBC is being compared to other courses or to no course at all, I cannot ascertain in which of these two contexts FBC performs better (by contexts, I mean (i) FBC vs. other courses, and (ii) FBC vs. no course). It is possible that the FBC course is more beneficial than receiving no instruction, but it may not have a significant impact when compared to other courses (or vice versa). However, this distinction cannot be made with the current methodology and data. In summary, I know that FBC has a positive effect, but I do not know who it benefits most effectively.

6. Costs and benefits

The costs Bogota's Secretariat of Economic Development faced in the implementation of Boost to Employment through the program operator were around 1.2 million dollars (2022 dollars). As shown in table 1, operator A served 10,000 job seekers. According to the sample restrictions and administrative records, 64% of the job seekers, were working in a formal job one year after the implementation of the program. This implies that the cost for each person who found a formal job thanks to the program was around 175 dollars. Other active labor market policies that have the same objective usually cost between 700 and 1,200 dollars per worker and have effects on increasing the probability of being hired in the formal sector of around 5% (Levy Yeyati, Montané, and Sartorio 2019). This suggests that including information about the value of formality in the training component of a labor intermediation policy is much cheaper than implementing other ALMPs. In particular, the cost of providing classroom training is around 75% less and is more than twice as effective in helping job seekers to find a formal job compared to other active labor market programs.

My empirical results suggest that giving more information about the benefits of having a formal job, along with connecting job seekers to the vacancies of formal firms is a cost effective policy that helps vulnerable job seekers to find formal jobs. In this paper, I evaluate whether this kind of policy has any impact on the probability to find a formal job in the short term. In the future I plan to evaluate medium to long term effects.

7. Conclusions

This paper analyzes the effect of a training component of a labor intermediation policy (LIP) called Boost to Employment (BE) on the probability of obtaining a formal job for vulnerable unemployed job seekers in a developing country. It is hard to find causal effects of a training component of a LIP on employability since we usually have a self selection problem. To avoid the selection problem I use a novel identification strategy that has been implemented in criminal justice employing variation in propensities of rotating judges. In particular, I exploit the variation in the leniency among labor counselors as an instrument to approximate the probability that job seekers receive the training component of the program. I find large employment effects of assignment to classroom training: job seekers assigned to the classroom training component are 36% more likely to find a formal job compared to those who did not take the training component.

The formal job benefits course seems to be driving my results. In particular, being assigned to the FBC increased the probability of finding a formal job by 11 pp compared to those who received any other course different from the FBC or did not receive the training component of the program, one year after BE implementation. These results provide evidence that explaining to job-seekers the benefits of a formal job seems to have increased their job search intensity in the formal sector, which is then reflected in the increased probability of finding a formal job. It is important to recognize the limitations of my findings. Specifically, since FBC is being compared to other courses or to no course at all, I cannot determine in which of these two scenarios FBC performs better (by scenarios, I mean (i) FBC vs. other courses, and (ii) FBC vs. no course). It is possible that the FBC course is more advantageous than receiving no instruction, but it may not have a significant impact when compared to other courses (or vice versa). However, this distinction cannot be made with the current methodology and data. In summary, I know that FBC has a positive effect, but I do not know who benefits from it most effectively.

Interestingly, I find a stark difference between my IV estimates and OLS estimates that assume selection on observables only. The latter approach is widely used in the literature and suggests classroom training does not help job seekers to find a job. The differences between OLS and IV highlight the importance of controlling for unobserved job-seeker characteristics.

Finally, making a simple cost-benefit analysis, my results suggest that including training components in labor intermediation policies are less expensive (around 25% of the cost of other active labor market programs) and they are twice as effective in helping unemployed workers to find a formal job. The results from this evaluation indicate that it is important for classroom training programs be closely tracked and rigorously evaluated.

This paper only evaluates the effect of a training component in a LIP on the probability of finding a formal job in the short run, one year after the implementation of the program. It would be interesting not only to see the short term effects of these kinds of programs but also to identify the medium and long term effects of training component of LIPs in developing countries. This could provide a better understanding of these kinds of programs in terms of how they can help to increase formal employment in developing countries.

References

Abramo, Lais, Simone Cecchini, and Beatriz Morales (2019). *Programas sociales, superación de la pobreza e inclusión laboral: aprendizajes desde América Latina y el Caribe*. Cepal.

Aizer, Anna and Joseph J Doyle Jr (2015). "Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges". In: *The Quarterly Journal of Economics* 130.2, pp. 759–803.

Angrist, Joshua and Guido Imbens (1994). "Identification and Estimation of Local Average Treatment Effects". In: *Econometrica* 62.2, pp. 467–475.

Angrist, Joshua and Michal Kolesár (2024). "One instrument to rule them all: The bias and coverage of just-ID IV". In: *Journal of Econometrics* 240.2, p. 105398.

Ashenfelter, Orley (1978). "Estimating the effect of training programs on earnings". In: *The Review of Economics and Statistics*, pp. 47–57.

Attanasio, Orazio et al. (2017). "Vocational training for disadvantaged youth in Colombia: A long-term follow-up". In: *American Economic Journal: Applied Economics* 9.2, pp. 131–143.

Baird, Matthew D, John Engberg, and Italo A Gutierrez (2022). "RCT evidence on differential impact of US job training programmes by pre-training employment status". In: *Labour Economics* 75, p. 102140.

Behncke, Stefanie, Markus Frölich, and Michael Lechner (2010). "Unemployed and their caseworkers: should they be friends or foes?" In: *Journal of the Royal Statistical Society: Series A (Statistics in Society)* 173.1, pp. 67–92.

Bekker, Paul A (1994). "Alternative approximations to the distributions of instrumental variable estimators". In: *Econometrica: Journal of the Econometric Society*, pp. 657–681.

Bhuller, Manudeep et al. (2020). "Incarceration, recidivism, and employment". In: *Journal of political economy* 128.4, pp. 1269–1324.

Blomquist, Sören and Matz Dahlberg (1999). "Small sample properties of LIML and jackknife IV estimators: Experiments with weak instruments". In: *Journal of Applied Econometrics* 14.1, pp. 69–88.

Card, David, Jochen Kluve, and Andrea Weber (2010). "Active labour market policy evaluations: A meta-analysis". In: *The economic journal* 120.548, F452–F477.

Cederlöf, Jonas, Martin Söderström, and Johan Vikström (2021). *What makes a good caseworker?* Tech. rep. Working Paper.

CEPAL, NU (2020). "Claves de la CEPAL para el desarrollo Nº 7: Panorama Social de América Latina 2019". In.

Chaves Hernández, Catherine (2016). "Transiciones de informalidad a formalidad y viceversa, productividad y salario mínimo-evidencia de datos panel para Colombia". In.

Crépon, Bruno et al. (2013). "Do labor market policies have displacement effects? Evidence from a clustered randomized experiment". In: *The quarterly journal of economics* 128.2, pp. 531–580.

Dahl, Gordon B, Andreas Ravndal Kostøl, and Magne Mogstad (2014). "Family welfare cultures". In: *The Quarterly Journal of Economics* 129.4, pp. 1711–1752.

Dale, Stacy Berg and Alan B Krueger (2002). "Estimating the payoff to attending a more selective college: An application of selection on observables and unobservables". In: *The Quarterly Journal of Economics* 117.4, pp. 1491–1527.

DANE (2024). *https://www.dane.gov.co/files/operaciones/GEIH/bol-GEIHEISS-abr-jun2024.pdf*. Tech. rep. Ocupacion informal Trimestre abril - junio 2024.

Di Tella, Rafael and Ernesto Schargrodsky (2013). "Criminal recidivism after prison and electronic monitoring". In: *Journal of political Economy* 121.1, pp. 28–73.

Farné, Stefano (2016). "Programas de empleo público en América Latina". In.

Frandsen, Brigham, Lars Lefgren, and Emily Leslie (2023). "Judging judge fixed effects". In: *American Economic Review* 113.1, pp. 253–277.

Frölich, Markus and Michael Lechner (2010). "Exploiting regional treatment intensity for the evaluation of labor market policies". In: *Journal of the American Statistical Association* 105.491, pp. 1014–1029.

González-Velosa, Carolina, Laura Ripani, and David Rosas Shady (2012). "¿ Cómo mejorar las oportunidades de inserción laboral de los jóvenes en América Latina?" In.

Humlum, Anders, Jakob Munch, and Mette Rasmussen (2023). "What Works for the Unemployed? Evidence from Quasi-Random Caseworker Assignments". In: *Evidence from Quasi-Random Caseworker Assignments (March 27, 2023). University of Chicago, Becker Friedman Institute for Economics Working Paper* 2023-43.

Jessen, Jonas and Jochen Kluve (2021). "The effectiveness of interventions to reduce informality in low-and middle-income countries". In: *World Development* 138, p. 105256.

Katz, Lawrence F et al. (2022). "Why do sectoral employment programs work? Lessons from WorkAdvance". In: *Journal of Labor Economics* 40.S1, S249–S291.

Kling, Jeffrey R (2006). "Incarceration length, employment, and earnings". In: *American Economic Review* 96.3, pp. 863–876.

Kluve, Jochen (2010). "The effectiveness of European active labor market programs". In: *Labour economics* 17.6, pp. 904–918.

Levy Yeyati, Eduardo, Martín Montané, and Luca Sartorio (2019). "What works for active labor market policies?" In: *CID Working Paper Series*.

Lichter, Andreas (2016). "Benefit duration and Job search effort: Evidence from a Natural Experiment". In.

Lima, Jose Gregorio Roberto Flores, Salvador Zamora, and Elba Contreras (2013). “Transiciones entre el trabajo formal e informal y medios de intermediación laboral en México: 2005-2010”. In.

Mazza, Jacqueline (2011). “Fast tracking jobs: Advances and next steps for labor intermediation services in Latin America and the Caribbean”. In.

McCall, Brian, Jeffrey Smith, and Conny Wunsch (2016). “Government-sponsored vocational education for adults”. In: *Handbook of the Economics of Education*. Vol. 5. Elsevier, pp. 479–652.

McKenzie, David (2017). “How effective are active labor market policies in developing countries? a critical review of recent evidence”. In: *The World Bank Research Observer* 32.2, pp. 127–154.

Mikusheva, Anna and Liyang Sun (2024). “Weak identification with many instruments”. In: *The Econometrics Journal*, utae007.

Mueller-Smith, Michael (2015). “The criminal and labor market impacts of incarceration”. In: *Unpublished working paper* 18.

Nagar, Anirudh L (1959). “The bias and moment matrix of the general k-class estimators of the parameters in simultaneous equations”. In: *Econometrica: Journal of the Econometric Society*, pp. 575–595.

Phillips, Garry David Alan and C Hale (1977). “The bias of instrumental variable estimators of simultaneous equation systems”. In: *International Economic Review*, pp. 219–228.

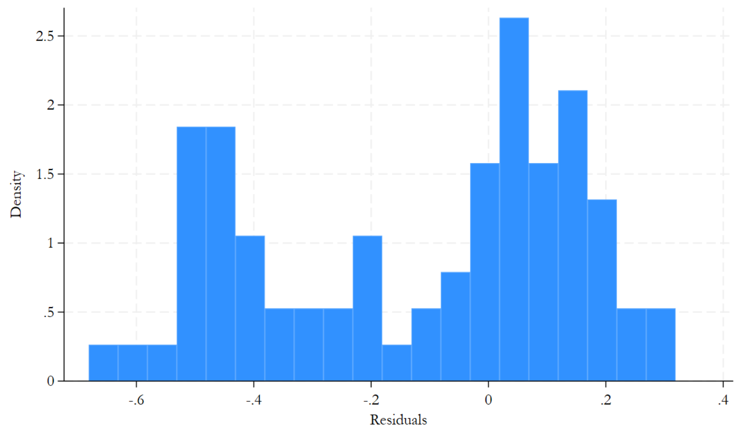
Simone Filgueira, Rodrigo Rossel (2015). *Instrumentos de protección social*. CEPAL.

Stevenson, Megan T (2018). “Distortion of justice: How the inability to pay bail affects case outcomes”. In: *The Journal of Law, Economics, and Organization* 34.4, pp. 511–542.

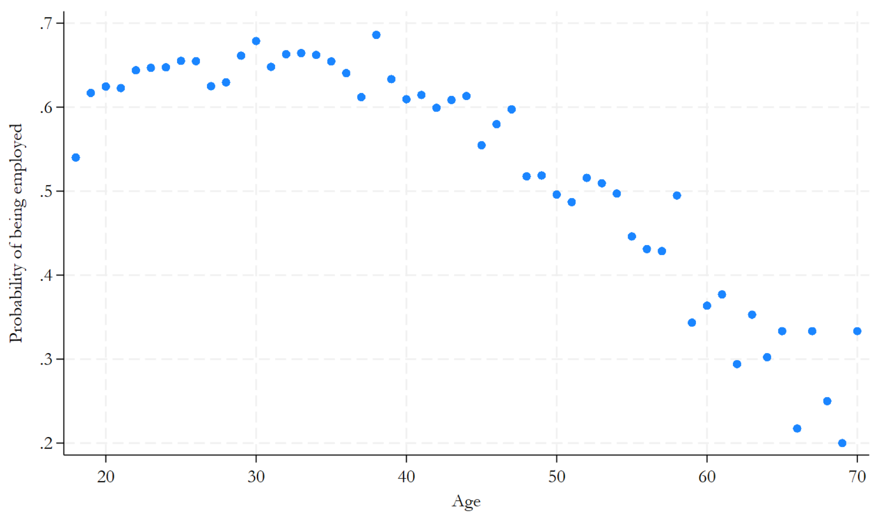
Torm, Nina and Marty Oehme (2024). “Social protection and formalization in low-and middle-income countries: A scoping review of the literature”. In: *World Development* 181, p. 106662.

Appendix

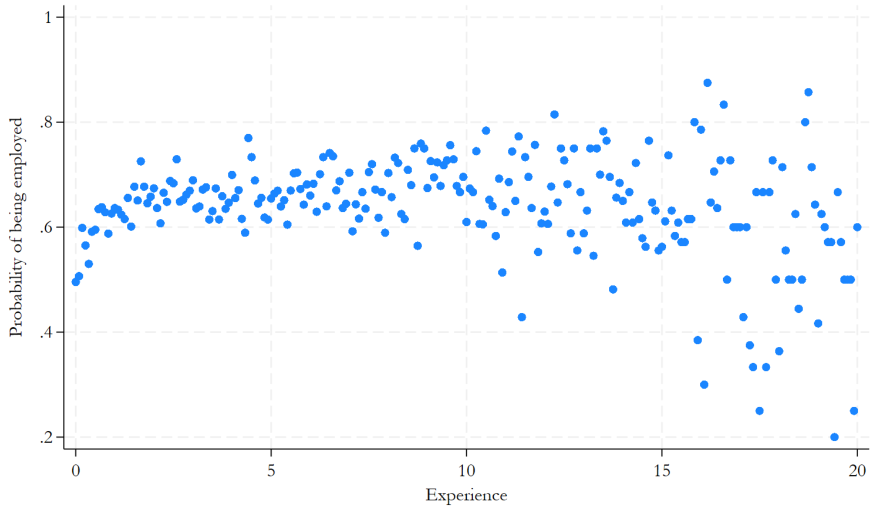
Figue A1



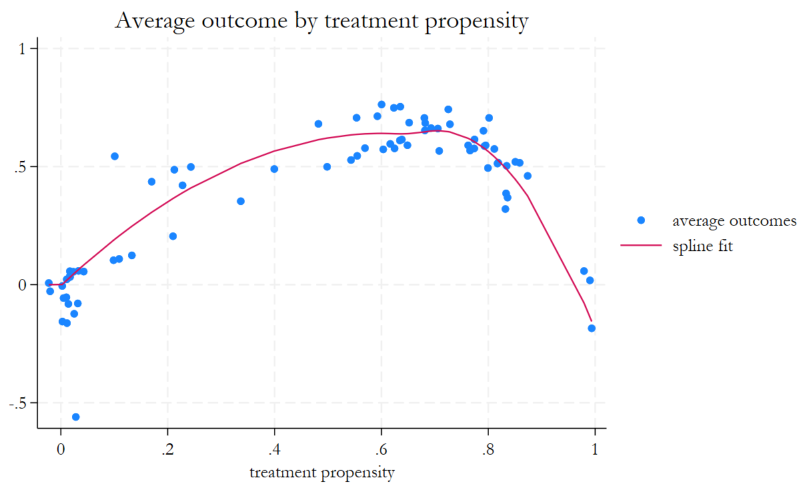
Figue A2



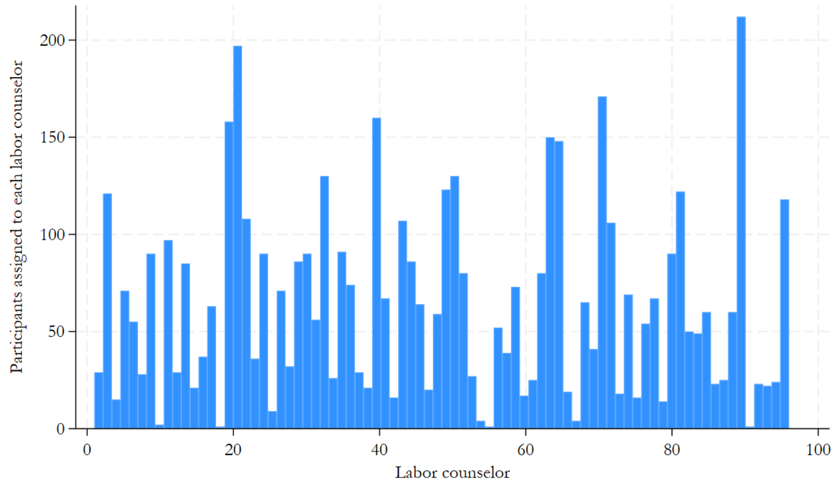
Figue A3



Figue A4



Figue A5



Participants assigned to each labor counselor
200
150
100
50
0
0
20
40
60
80
100
Labor counselor

Table A1. Descriptive statistics of Boost to Employment program

	Number of participants			
Panel A: Compensation Fund	No training	Training	Full sample	
operator A	6863	3115	9978	
operator B	6580	3813	10393	
operator C	14343	2724	17067	
Total	27786	9652	37438	
Panel B: Individual characteristics	No training	Training	Full sample	Difference
Female	16486	6623	23109	
Male	11300	3029	14329	
Average age	28,6	30,2	29,02	-1,57***
Average years of experience	3,72	4,6	3,91	-0,87***
Panel C: Education	No training	Training	Full sample	
Unfinished high school	1371	192	1563	
	87,7%	12,3%	100%	
High school	10633	2212	12845	
	82,8%	17,2%	100%	
Technical and technology	5670	1973	7643	
	74,2%	25,8%	100%	
Bachelor degree	2795	1460	4255	
	65,7%	34,3%	100%	
Panel D: Formal job				
Participants hired in a formal job	63,2%	59,1%		

Note: This table shows the descriptive statistics of the Boost to Employment (BE) program developed by the Secretary of Development in Bogota for the three operators that were in charge of the program. No training means job seekers who were assigned to the basic package, while training means job seekers who were assigned to the integral package. The basic package consisted of helping the job seeker to construct their *curriculum vitae* (CV) and send it to formal job vacancies. The integral package consisted of the basic package plus some training courses determined by the labor counselor.

Table A2. Distribution of courses assigned to participants

Type of course	**Participants**	**Avg. Hours**	**%**	**# Courses assigned**	**%**
No training	1211	0	37,46%	0	37,46%
FBC	468	8	14,48%	1	18,31%
SK	99	40	3,06%		
JS	25	40	0,77%		
FBC + SK	473	48	14,63%	2	24,19%
FBC + JS	294	48	9,09%		
SK + JS	15	80	0,46%		
FBC + SK + JS	648	88	20,04%	3	20,04%
Total	3233		100%		

Note: This table shows the descriptive statistics of the courses assigned to participants: type of course, number of participants by course and average hours intensity of courses.

Table A2. OLS estimates by operator

VARIABLE	Formal job	Formal job	Formal job
Training	-0.0238*	-0.0812***	-0.0483***
	(0.0133)	(0.0148)	(0.0127)
Observations	7,405	6,295	12,113
R-squared	0.1248	0.1551	0.1157
Individual controls	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes
Program operator	A	B	C

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3. First stage results

	FBC
Leniency	0.4023***
	(0.0377)
Observations	3,233
R-squared	0.2838
Neighborhood FE	Yes

Note: This table shows the first-stage results of being assigned to the FBC with the leave-one-out measure of judge leniency. The leave-one-out is computed in the following way:

$$leniency_i = \frac{\sum_{k \neq i \in \Omega_j} FBC_k}{n_j - 1} \tag{10}$$

Where Ω_j is the group of participants assigned to the labor counselor j and n_j denotes the total number of job seekers assigned to labor counselor j.