Skill certifications and Migrant Labor Market Integration: Experimental evidence from Colombia

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Abstract

This paper studies whether government-issued skill certification improves labor market integration when workers' skills are hard to verify. We evaluate a randomized offer of access to an occupation-specific certification program in Colombia among locals and migrants, two groups with different ex ante signaling capacity. The program certifies existing skills rather than providing training. Among locals, the offer increases employment by reducing inactivity and raises hours worked. The effects persist unevenly over time and are robust to attrition bounds and alternative outcome measures. Among migrants, estimates are less precise and provide no evidence of effects in employment, hours, or wages. The results show that a common skill certification generates different patterns of labor-market adjustment among locals and migrants.

Keywords: Signaling, Certifications, Skills, Migration, Colombia

JEL: J24, J61, J64, I26, O15, D82.

1 Introduction

Migrants often enter destination labor markets with education and work experience that are less familiar to local employers. This can make their skills harder for employers to assess and can limit the value of the signals they bring from their country of origin. As a result, migrants may have difficulty finding stable employment, work in jobs that do not match their qualifications (Dustmann et al., 2012; Lebow, 2023), or remain concentrated in informal employment (Delgado-Prieto, 2025). These problems can persist even after legal barriers to employment are removed (Bahar et al., 2021; Ibáñez et al., 2024). More generally, when employers have limited information about workers' skills, job seekers rely on signals such as degrees, completed courses, grades, and institutional reputation (Spence, 1973).

This paper studies whether a credible skill signal improves labor market integration. We focus on skill certification, a policy that verifies existing competencies rather than providing training. We estimate the effects of an offer of access to an occupation-specific certification program for both locals and migrants. This design allows us to test whether the same signal affects workers with different ex ante signaling capacity along different labor market margins.[1]

We study a free certification program implemented by the Colombian Ministry of Labor. The program certifies occupation-specific skills acquired through work experience or on-the-job training rather than formal education. Eligibility requires applicants to document prior work experience in the occupation linked to the certification. Among 2,160 eligible applicants, 855 randomly received an offer of access to the program, including 250 migrants. Treatment-assigned applicants were offered competency assessments and, if successful, certification. Control-group applicants were not assessed and did not receive certification during the evaluation period. Treatment and control applicants were surveyed at three, six, and twelve months after the intervention.

We estimate intention-to-treat effects separately for locals and migrants. Random assignment produced balanced treatment and control groups within both samples. Survey response rates are high, but treatment assignment increased follow-up response in some waves, especially among migrants. We report Lee bounds (Lee, 2009) to assess the sensitivity of the main estimates to differential attrition.

The effects differ across groups. Among locals, the offer of access to the certification program increases employment by 5.7 percentage points and reduces inactivity by 3.5 percentage points. It also raises weekly hours worked by 2.0 hours and full-time work by 3.7 percentage points, but does not increase formal employment or wages among

[1]The study was conducted under a pre-analysis plan accepted as part of the registered-report process, which specified the randomized offer of access to the certification program, separate analyses for locals and migrants, primary labor-market outcomes, and analyses of channels.

those already employed. Among migrants, the estimates are less precise, and we do not detect improvements in employment, hours worked, or wages. The migrant point estimates are smaller in magnitude than the minimum detectable effects implied by the design, indicating limited statistical power to detect effects of this size—and cautioning against interpreting the null results as evidence of zero effects.

Dynamic estimates show that the local employment effect emerges quickly and remains positive after one year, while the migrant estimates remain imprecise. The difference in employment effects between locals and migrants is statistically significant. Although the two groups differ along baseline characteristics, reweighting the migrant sample to match locals on observables leaves the heterogeneous pattern unchanged. These results suggest that observable differences in sample composition do not fully account for the contrasting estimates, but the experiment does not identify why the effects differ across groups.

The evidence on additional outcomes provides some guidance on potential channels but does not identify them directly. For locals, the program reduces unemployment duration by 0.23 months, without detectable changes in reservation wages or search intensity. This pattern is consistent with certification reducing screening frictions and facilitating labor market entry. For migrants, we find no clear pattern in job-search outcomes that explains the main estimates. We also find little evidence that changes in education or training enrollment explain the effects for either group over the period we study.

Compliance was high but not perfect. A small share of participants randomized into treatment did not receive a certificate because they did not successfully complete the evaluation process or because they withdrew. We therefore examine whether the intention-to-treat estimates are sensitive to incomplete compliance by instrumenting receipt of certification with random assignment to treatment. The instrumental variable estimates closely mirror the intention-to-treat estimates, as expected given the high compliance rate. Stated willingness to participate in the program without subsidies is high in both treatment and control groups, suggesting that participants valued the program regardless of assignment.

This paper contributes to the literature on labor market signaling. This literature shows that credible information about workers' skills can affect hiring and labor market outcomes. Experimental studies find positive effects from certification or information disclosure among young job seekers in Ethiopia (Abebe et al., 2021), during job interviews in Uganda (Bassi and Nansamba, 2022), among young job seekers in South Africa (Carranza et al., 2022), among young workers in New York (Heller and Kessler, 2021), and in online labor markets (Pallais, 2014). Related evidence shows that labor market signals such as grade point averages, college reputation, institutional quality, and publicly available skills information can generate substantial returns (Landaud et al., 2024; MacLeod et al., 2017; Eble and Hu, 2022; Busso et al., 2025b). We contribute by testing whether a common, occupation-specific skill certificate generates heterogeneous returns for two groups with different ex ante signaling capacity, locals and migrants. We show that this certification produces different patterns of labor-market adjustment across the two groups.

The study also contributes to the literature on migrant labor market integration. This literature analyzes the effects of policies that reduce barriers to employment in destination labor markets, including job search assistance (Battisti et al., 2019), intensive coaching (Joona and Nekby, 2012), wage subsidies, training, and placement programs (Dahlberg et al., 2024; Foged and Van der Werf, 2023; Foged et al., 2024), citizenship application subsidies (Hainmueller et al., 2018), mentorship programs (Jaschke et al., 2022), refugee-firm matching (Loiacono and Silva-Vargas, 2023), and regularization policies (Bahar et al., 2021; Ibáñez et al., 2024). These interventions can improve employment, earnings, legal status, or social integration, but evidence from Colombia suggests that regularization alone had limited effects on the labor market integration of Venezuelan migrants, especially among lower-skilled workers (Bahar et al., 2021; Ibáñez et al., 2024).

We contribute by studying a policy that targets a different barrier to migrant integration: the difficulty employers may face in assessing skills acquired abroad. Unlike training or intensive job-search programs, skill certification does not aim to build new skills. It verifies existing competencies and provides a domestic credential. Other forms of recognition, such as the validation of foreign degrees or professional credentials, can be costly and time-consuming. In their absence, migrants may retrain unnecessarily, generating productivity losses (Carranza and McKenzie, 2024a). Our results show that the effects of this type of intervention need not be the same for migrants and locals. While certification increases employment among locals, the estimates for migrants are less precise and do not show positive effects on labor market outcomes.

The rest of the paper is organized as follows. Section 2 describes the program setting and intervention. Section 3 presents the research design, data, outcomes, and empirical strategy. Section 4 assesses internal validity, including balance and attrition. Section 5 reports the main results and their dynamics. Section 6 presents robustness checks. Section 7 discusses certification receipt. Section 8 examines channels and additional outcomes. Section 9 presents a policy discussion, and Section 10 concludes.

2 Program Setting and Intervention

Colombia provides a useful setting to study whether certification can make existing skills more legible to employers. The country received a large inflow of Venezuelan migrants over a short period, many of whom obtained legal status and could participate in the labor market.[2] Despite their higher labor force participation, Venezuelan migrants face substantial occupational mismatch (Kaplan et al., 2023). Credential non-recognition and employer uncertainty about migrant qualifications push many into informal or low-skilled jobs below their competency level (Bahar et al., 2021; Lebow, 2023). Colombia also has a long-standing system of skills certification, allowing us to study whether an occupation-specific domestic signal affects labor market outcomes.

2.1 Skill Certification Program

Since 2003, the Colombian government has operated a free skill certification program through the Colombian Vocational Training Agency (SENA).[3] The program allows adults with prior work experience to validate existing occupational skills through publicly recognized certifications. It does not provide training. Instead, the certifying agency assesses whether applicants have already acquired specific competencies through work experience.[4]

The certifications can credibly signal workers' skills to Colombian employers for three reasons. First, the program certifies more than 200,000 workers each year throughout the country, making the credential widely used. Second, SENA is a recognized national institution that provides both certification and technical training, although these services are administered separately. Third, certification assessments are based on occupational standards developed with input from industry associations, giving employers a role in defining the competencies being certified. These features make the certificate a potentially informative signal of occupation-specific skills.

Eligibility requires a valid identification document, registration through the program website, acceptance of the legal terms, and evidence of at least six months of prior work experience in the occupation linked to the certification. The program offers assessments in 2,405 competencies across manufacturing, agriculture, retail, and service occupations. Applicants may be employed or unemployed, but they must select a specific competency for certification and document prior practical experience in the corresponding occupation.

[2]See Appendix A for details on the Venezuelan migration crisis, the regularization process, and migrants' labor market integration in Colombia and across the region

[3]The program is named, in Spanish, "Programa de Evaluación y Certificación de Competencias Laborales", which was regulated by Decree 933 of 2003.

[4]Appendix B provides additional details about the skills certification program.

Candidates are evaluated through a practical assessment and a knowledge test. In the practical assessment, a qualified assessor from the certifying agency evaluates whether the applicant can perform occupation-specific tasks and whether they meet the competency standards, assigning a pass/fail grade. Applicants who pass this assessment then complete a multiple-choice knowledge test administered virtually, with scores ranging from 0 to 100. Successful candidates receive a certificate that reports their assessed level: basic, intermediate, or advanced.[5] Candidates who do not pass are eligible for a six-hour remedial session and may subsequently retake the assessment. Historically, fewer than 10 percent of participants fail, consistent with the requirement that applicants document prior practical experience in the occupation before certification.

2.2 "Skills Pay Off" Program

Since 2021, *Saber Hacer Vale* ("Skills Pay Off") has extended access to the certification program to vulnerable populations. Implementation begins with the selection of a geographic area and the competencies to be certified. The agency then determines the number of available slots, opens the call for applications, and verifies applicants' documentation to determine eligibility.[6]

Certification is the central component of the intervention, but the treatment also includes two additional components: information on publicly available job-search resources and financial assistance during participation.[7] . The information resources are unlikely to drive any treatment effects, as they are publicly available to both the treatment and control groups, participation is voluntary, and they constitute light-touch interventions (Carranza et al., 2022; McKenzie, 2017; Kelley et al., 2024).

The financial assistance includes food and connectivity subsidies of approximately $82 USD for all participants, with additional transportation, dependent-care, or remedial-session support for those who qualify. Total support can reach $217 USD, equivalent to roughly 70 percent of the monthly minimum wage. Because benefits are paid upon completion, the payment structure creates incentives to start and complete the process. The transfers were intended both to compensate participants for the opportunity cost of participation and to relax access constraints, including virtual participation and childcare costs, which program officials identified as particularly salient for certain

[5]The type of certificate awarded depends on the grade obtained in the evaluation process: those with scores below 30 fail the exam and do not receive the certificate; those with scores between 30 and 60 receive a basic certificate; those between 60 and 90 receive an intermediate certificate; and those above 90 receive an advanced certificate. The issued certificate explicitly states the level at which the individual was classified.

[6]Appendix B provides additional details about "Skills Pay Off".

[7]Appendix B provides a detailed description of both additional components of the program.

demographic groups, such as migrant women. However, they could also relax liquidity constraints after certification. In this sense, the program is best understood as a certification program that reduces the cost of participation for low-income individuals.

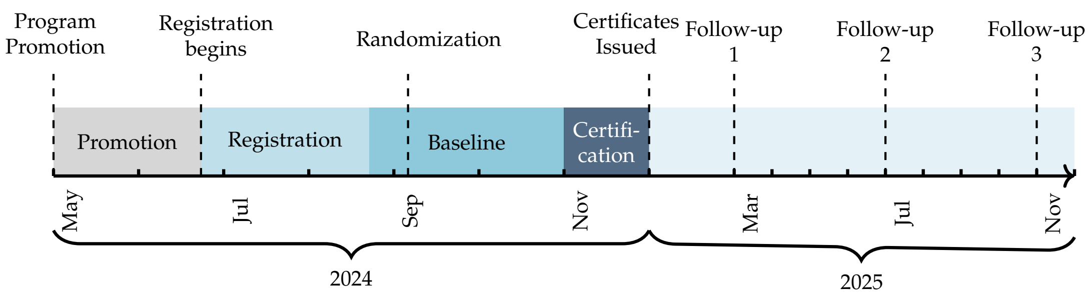
Eligibility for additional support requires meeting at least one vulnerability criterion, such as low income, caregiving responsibilities, age, ethnic-minority status, or being a migrant. At least 30 percent of available slots are reserved for migrants with regular status.

2.3 Intervention

The intervention evaluated in this paper is the 2024 cohort of "Skills Pay Off," implemented in Colombia's South-western region.[8] The cohort had 855 available slots across five competencies: health services, food preparation, food handling, design and tailoring, and recycling operations.

Figure 1 summarizes the implementation and evaluation timeline. The program was advertised in May and June 2024, and applications were open between June and August 2024. A total of 2,736 individuals registered through the online application form. Of these, 2,160 met the eligibility criteria, which primarily verified prior work experience in the occupation linked to the requested certification.[9]

Figure 1: Timeline of the Intervention



Applicants offered access to the program completed the standard certification process, which included a practical assessment administered by a qualified evaluator and a multiple-choice knowledge test from the certifying agency. Applicants who met the competency standards received an official certificate indicating their assessed skill level. Applicants who did not initially meet the competency standards were offered a remedial session and could retake the assessment.[10] Certificates were issued between November and December 2024.

[8]Specifically, in the departments of Valle del Cauca, Cauca, and Nariño.

[9]Submitted documents were reviewed between August 1st and September 3rd to determine eligibility for the program.

[10]Participants who completed the remedial session and subsequently passed the assessment received the certification and the corresponding financial incentives.

Table 1 reports certification outcomes among applicants assigned to treatment. Compliance was high but incomplete: 89 percent of treatment-assigned applicants received a certificate. Certification levels were 15 percent basic, 23 percent intermediate, and 51 percent advanced. The remaining 11 percent did not receive a certificate: four applicants were assessed but did not meet the certification criteria, while 89 withdrew before completing the assessment process.

Table 1: Certification Assessment Outcomes and Treatment Assignment by Migration Status

	Local Sample		Migrant Sample	
Treatment Status	(n)	(%)	(n)	(%)
Advanced	333	19.9	104	21.5
Intermediate	114	6.8	84	17.4
Basic	87	5.2	40	8.3
Not competent	3	0.2	1	0.2
Withdrawn	68	4.1	21	4.3
Control	1,071	63.9	234	48.3

Notes: The table reports the distribution of participants by migration status. Advanced, Intermediate, Basic, and Not Competent refer to the proficiency level at which each treated participant was classified after completing the evaluation and certification process. Withdrawn denotes individuals assigned to treatment who did not complete the process. Control denotes individuals randomly assigned to the control group.

3 Research design

3.1 Randomization

Before 2024, access to the program was allocated on a first-come, first-served basis. In the 2024 cohort studied in this paper, excess demand led the Ministry of Labor to allocate access by lottery. On September 6th, 2024, the 855 available slots were randomly allocated among the 2,160 eligible applicants.

Randomization was conducted at the individual level, stratified by type of certification (i.e., competency) and whether applicants were migrants, with 30 percent of slots reserved for migrants. Individuals assigned to treatment were offered access to the certification program as implemented.

Individuals assigned to control were informed that they would be offered access to the certification program in a later round, approximately 12 months after the intervention began.[11] This could have discouraged them from pursuing alternative certifications during the evaluation period even when, absent that promise, they would have done so. Such anticipation effects could mechanically widen treatment-control differences

[11] The randomized allocation had to satisfy the Ministry of Labor's budget constraint.

and bias our estimates upward. To assess this concern, we use administrative records from earlier *Skills Pay Off* calls, in which some eligible applicants were not certified because available slots were limited. About 4.3 percent of these eligible non-participants obtained a certification within one calendar year of being rejected, compared with 5.2 percent of individuals in the randomized control group over the corresponding period. We cannot reject equality between the two rates. These results provide no evidence that anticipated future access materially changed the behavior of the control group.[12]

3.2 Data Collection

Participants assigned to treatment and control were surveyed before and after the intervention. Baseline data come from two sources. The registration form collected demographic, socioeconomic, educational, and employment characteristics at the time of application. Before the intervention, participants were also contacted by phone in September 2024 for a complementary baseline survey, which collected additional information on labor market outcomes, mental health, and poverty.

We then conducted three follow-up surveys, at three, six, and twelve months after the program ended. In each round, participants were recontacted by phone and invited to complete a survey comparable in scope to the baseline. The first follow-up took place between February and March 2025; the timing of all survey rounds is shown in Figure 1.[13]

3.3 Pre-Analysis Plan

Our analysis follows a pre-analysis plan that specifies randomized access to the certification program as the treatment and stipulates separate intention-to-treat analyses for locals and migrants (Busso et al., 2025a). It considers effects on employment and wages and allows these effects to differ between locals and migrants.

The pre-analysis plan also lays out several potential channels through which certification could affect labor-market outcomes. On the employer side, certification may improve screening by making workers' skills easier to assess. On the worker side, it may change job-search behavior and the types of offers workers are willing to accept. The plan also considers subsequent education and training responses. These outcomes do not provide direct tests of any one mechanism, since the experiment

[12]See Appendix B.5 for additional details. Self-reported certification in the follow-up surveys also closely matches the administrative records for the randomized control group.

[13]The pre-analysis plan anticipated a nine-month follow-up survey. This round was not collected because of budgetary and implementation-timing constraints. Although the first follow-up survey was fielded before the pre-analysis plan was accepted, the research team did not receive any follow-up data until after acceptance.

neither manipulates nor directly observes employer beliefs or workers' underlying constraints. We therefore use them to assess whether the pattern of results is consistent with these different channels. The plan also identifies skill downgrading as an outcome of particular interest for migrants, reflecting the possibility that certification improves occupational allocation.

3.4 Outcome Measures

Following the pre-analysis plan, we examine labor-market outcomes together with reservation wages, unemployment duration, search intensity, and subsequent enrollment in education, certification, or training. The latter outcomes help interpret the main labor-market effects but do not identify specific mechanisms. In particular, they allow us to assess whether the pattern of results is consistent with improved employer screening, changes in workers' job-search behavior, or subsequent education and training responses.

The labor-market outcomes capture employment status and job quality. We measure whether respondents are employed, unemployed, or economically inactive, where inactivity is defined as not working and not actively seeking work. We also measure monthly wages, weekly hours worked, full-time work, and formal employment, defined as employment in which the worker contributes to the pension system. All these outcomes are measured directly in the surveys.

We measure *skill downgrading*, as an indicator equals to one for individuals with post-secondary education who are employed in occupations requiring fewer qualifications than their educational level, and zero otherwise. To construct the measure, we use the Colombian Classification of Occupations (CUOC),[14] which maps each occupation to a skill level based on the complexity and qualifications required to perform its tasks.

To study potential mechanisms, we collected two sets of additional outcomes. The first captures job-search behavior: whether the respondent received a job offer, the number of offers received, weekly hours devoted to job search, unemployment duration, and reservation wages. Reservation wages are elicited separately for employed and unemployed respondents as the minimum salary at which they would switch jobs or accept a job offer, respectively. The second set captures post-program education and training.

We also collected secondary outcomes on job satisfaction, well-being, and program valuation. Job satisfaction is measured by an indicator for reporting being satisfied or very satisfied with the current job. Well-being is measured by anxiety, using the GAD-7

[14]The CUOC adapts the International Standard Classification of Occupations (ISCO-08) to the Colombian context.

scale, and food insecurity, using an indicator for having skipped a meal because of lack of economic resources. Program valuation is measured by respondents' willingness to pay for a similar skill certificate and willingness to participate in the program without subsidies.[15]

3.5 Empirical strategy

The primary estimand is the intention-to-treat effect (ITT) of being offered access to the certification program as implemented. The main specification pools the three post-treatment survey rounds:

$$y_{ijt} = \alpha_0 + \alpha_1 D_i + \mu_j + \mu_t + \varepsilon_{ijt}, \quad \text{for } M_i \in \{0,1\}, \tag{1}$$

where y_{ijt} represents an outcome for individual i, enrolled in certification area j, in survey wave t; D_i is an indicator equal to one if the individual was randomly assigned to receive an offer of access to the certification program; M_i is an indicator equal to one for migrants; and μ_j and μ_t denote competency and survey-wave fixed effects, respectively. The coefficient α_1 identifies the average intention-to-treat effect for the corresponding subgroup.

Because not all treatment-assigned applicants received a certificate, we also estimate IV effects of certification receipt. We instrument receipt of certification with random assignment to treatment. The resulting 2SLS estimand is a local average treatment effect (LATE) for compliers.

We also estimate time-specific effects to examine whether the impact of the offer of access to the certification program varies over time. This allows us to assess timing and persistence. The dynamic specification takes the form:

$$y_{ijt} = \beta_0 + \beta_1 D_i + \sum_{t>T_0}^{T} \beta_{2t}\,(D_i \times T_t) + \mu_j + \mu_t + \varepsilon_{ijt}, \quad \text{for } M_i \in \{0,1\}, \tag{2}$$

where all terms are defined as in Equation (1). The indicators T_t denote follow-up survey waves. The coefficients β_{2t} capture how the treatment effect differs across follow-up waves relative to the omitted survey wave. In this specification, each outcome is measured contemporaneously at each follow-up wave and reflects the respondent's status at that point in time (i.e., outcomes are not carried forward across waves). A respondent's value at a later follow-up is determined by their situation at that survey, regardless of their status in earlier waves. Respondents who do not complete a given

[15] Appendix C defines the outcome measures used in the paper, maps them to the pre-analysis plan, documents planned outcomes not reported in the final analysis, and describes the construction of derived outcomes, including skill downgrading.

follow-up are excluded from the estimate for that wave.

Equations (1) and (2) are estimated separately for locals and migrants. These subgroup estimates are internally valid because randomization was stratified by whether applicants were migrants. For completeness, we also report pooled interacted specifications and tests of equality between local and migrant effects. However, differences in estimated effects across groups should be interpreted with caution. Locals and migrants differ in pre-treatment characteristics, and these characteristics may moderate the returns to certification. As a result, cross-group differences in treatment effects may reflect migration background, observable or unobservable moderators, or some combination of these factors. The design does not separately identify these channels.

In all cases, standard errors are clustered at the individual level. Because we estimate effects on multiple outcomes, we adjust inference using sharpened q-values following Anderson (2008). We compute q-values within outcome families in each table and separately by estimation sample. Standard errors are reported in parentheses, while q-values provide the multiple-testing adjustment used to interpret significance across related outcomes.

4 Internal Validity

4.1 Balance

We assess randomization balance by comparing baseline characteristics between treatment and control applicants separately for locals and migrants. The tests condition on competency fixed effects, reflecting the stratified randomization design. For descriptive purposes, we also compare locals and migrants.[16]

Treatment and control applicants are balanced across observable baseline characteristics within both samples. Only one test rejects equality at the five-percent level, a pattern consistent with chance given the number of characteristics tested. This supports treatment-control comparisons within the local and migrant samples.

Locals and migrants differ along several baseline dimensions. Migrants are younger, more educated, more likely to be employed, and more likely to experience skill downgrading at baseline. These differences do not threaten identification within each subgroup, but they imply that cross-group differences in treatment effects cannot be attributed mechanically to migrant background alone. They also motivate the paper's focus on skill downgrading as a central margin of migrant labor market integration. Migrants are more educated—18 percent hold university or postgraduate degrees

[16] Baseline balance tests are reported in Appendix D.

compared to 8 percent of locals—yet report similar wages. Among migrants with professional education, 12 percent work in occupations requiring fewer skills than their qualifications, compared to 4 percent of locals.[17]

4.2 Survey Attrition

Attrition can be an important threat to internal validity after random assignment. Recontacting applicants after registration is difficult, and treatment assignment may affect survey response because treated applicants remain connected to the program while control applicants do not. To reduce non-response, all participants were offered monetary incentives for completing follow-up surveys, independent of treatment status.[18] These incentives were designed to preserve engagement in both the treatment and control groups.

Overall response rates are high. About 90 percent of participants completed the complementary baseline, and more than 82 percent completed the twelve-month follow-up.[19] However, response rates differ by treatment status in some waves. Differential attrition is moderate and concentrated among migrants: treatment-assigned migrants are more likely to respond at the three- and six-month follow-ups and, more weakly, at twelve months.

To assess the sensitivity of the estimates to differential attrition, we report bounds following Lee (2009). The approach trims the outcome distribution of the group with the higher response rate in proportion to the response-rate gap between treatment and control. It relies on a monotonicity assumption: treatment assignment weakly increases the probability of survey response. This assumption is plausible in our setting because treatment-assigned applicants remained connected to the program, while control applicants had no comparable program-related reason to respond. As a robustness check, we report the resulting bounds and discuss their implications for the main estimates.

[17] This pattern is less pronounced for the broader skill-downgrading measure that includes individuals with technical education, which we attribute to limited occupational detail at baseline.

[18] Participants received $7 USD for each completed survey and could receive up to $30 USD for completing all follow-up rounds. Participants who missed a round could still participate in later rounds and remain eligible for the full incentive if they completed the required survey modules.

[19] Response rates by treatment status and nationality, along with tests of differential attrition by migrant background and wave, are reported in Appendix E. Among migrants, differences are statistically significant at the 1 percent level in the three-month follow-up wave, at the 5 percent level in the six-month wave, and at the 10 percent level in the twelve-month wave. Among locals, only the three-month difference is significant, at the 10 percent level.

5 Results

5.1 Main Results

Table 2 reports intention-to-treat estimates from Equation (1). The estimates pool observations across the three follow-up rounds and are reported for the full sample (Panel A), locals (Panel B), and migrants (Panel C). All specifications include competency and survey-wave fixed effects.

In the full sample, access to the certification program increases employment by 3.6 percentage points, a 7 percent increase relative to the control mean. The effect on the zero-imputed log wage is positive, consistent with the employment response. Conditional estimates restricted to employed workers show no corresponding wage gain, indicating that the earnings effect is driven by the extensive margin rather than by higher wages among those employed (-0.020, *p-value*=0.650). We find no statistically significant effects on formal employment, weekly hours worked, full-time work, or skill downgrading in the full sample.

Among locals, access to the certification program increases employment by 5.7 percentage points and reduces inactivity by 3.5 percentage points, suggesting that part of the employment gain reflects transitions out of inactivity. Certifications also increase the zero-imputed log wage, weekly hours worked by 2.0 hours, and the probability of full-time work by 3.7 percentage points. The hours effect represents a 15.7 percent increase relative to the control mean reported in Table 2.[20]

The positive effect on the zero-imputed wage is mechanical, reflecting more locals earning a wage. Among those employed at follow-up, the effect on the log wage is small and not statistically significant (0.037, *p-value*= 0.810). This pattern runs against our original hypothesis that informative signals would generate wage returns alongside employment effects. This latter effect should be read with caution, however, because it conditions on employment at follow-up (itself an outcome affected by treatment). Workers who enter employment because of the offer are disproportionately drawn from those unemployed or inactive at baseline. If these marginal entrants earn less than workers who would have been employed regardless of treatment, their entry would lower average wages among treated workers. This selection could push the conditional wage estimate toward zero even if certification increases wages for a given match. The null conditional wage estimate should therefore not be interpreted as evidence of no wage returns to certification.[21]

[20]For locals, the employment, inactivity, log-wage, weekly-hours, and full-time effects retain significance at conventional levels after the multiple-testing adjustment (q-values below 0.05). The full-sample employment and log-wage effects do not survive the adjustment (q-values of 0.192).

[21]Beyond this selection concern, two features of this labor market may also limit wage responses,

The results are less conclusive for migrants. Most estimates in Panel C are imprecisely estimated, and we do not detect improvements in employment, hours worked, or wages. The point estimate for skill downgrading is a reduction of 8.1 percentage points, relative to a control-group mean of 25 percent, but it does not survive the multiple-hypothesis adjustment. Moreover, because skill downgrading is coded as zero for non-employed respondents, this estimate combines changes in occupational matching with changes in employment composition. This is particularly relevant because the estimated effect on migrant employment is negative, although statistically insignificant.[22]

although our design does not allow us to test these explanations directly. First, wages among low-income workers in Colombia may be compressed around the minimum wage, limiting the scope for wage differentiation. Second, widespread informality may weaken the link between formal credentials and wages. These features could mute wage responses even if certification improves employers' information about workers' skills and increases employment.

[22]Appendix F examines the skill-downgrading result in more detail. It shows that roughly half of the estimated reduction reflects an improvement in occupational match quality.

Table 2: Effects of Access to Skill Certification on Labor Market Outcomes

Intention To Treat estimated using Equation (1)

			Outcomes Replaced by Zero if Non-Employed				
	Employed	Inactive	Log Wage	Formal emp.	Weekly working hours	Full time (7+ hrs)	Skill Downgrade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Sample							
Treatment	0.036**	-0.024	0.490**	-0.001	1.223	0.024	-0.016
	(0.018)	(0.015)	(0.245)	(0.013)	(0.784)	(0.016)	(0.015)
Q-value	0.192	0.192	0.192	0.378	0.192	0.192	0.227
Mean Control	0.530	0.240	6.734	0.128	14.75	0.256	0.156
MDE	±0.050	±0.042	±0.686	±0.037	±2.197	±0.045	±0.042
Observations	5647	5647	5634	5647	5647	5647	5647
Panel B: Locals							
Treatment	0.057***	-0.035*	0.741***	-0.008	2.016**	0.037**	0.004
	(0.021)	(0.018)	(0.283)	(0.015)	(0.857)	(0.017)	(0.016)
Q-value	0.032	0.046	0.032	0.258	0.033	0.042	0.293
Mean Control	0.494	0.264	6.199	0.136	12.88	0.221	0.136
MDE	±0.059	±0.050	±0.792	±0.043	±2.401	±0.049	±0.045
Observations	4404	4404	4392	4404	4404	4404	4404
Panel C: Migrants							
Treatment	-0.033	0.011	-0.339	0.023	-1.485	-0.020	-0.081**
	(0.035)	(0.024)	(0.485)	(0.025)	(1.828)	(0.037)	(0.035)
Q-value	1.000	1.000	1.000	1.000	1.000	1.000	0.192
Mean Control	0.705	0.127	9.323	0.0903	23.79	0.424	0.250
MDE	±0.097	±0.068	±1.360	±0.070	±5.122	±0.104	±0.099
Observations	1243	1243	1242	1243	1243	1243	1243
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Treatment is an indicator equal to one for individuals randomly assigned to receive a skill certification, regardless of whether certification was completed (intent-to-treat, ITT). The table reports pooled ITT estimates from Equation 1, stacking three follow-up rounds at 3, 6, and 12 months. Panel A presents results for the full sample. Panels B and C report estimates separately for locals and migrants. Outcomes in columns (3)–(7) are set to zero when the individual is not employed; monthly earnings are set to zero before applying the log transformation log(wage + 1). All specifications include competence and wave fixed effects; full-sample regressions additionally control for migrant status. Standard errors (in parentheses) are clustered at the individual level. Sharpened q-values are reported for Panels A, B and C and are computed within each outcome family following Anderson (2008). MDE reports the minimum detectable effect at the 5% significance level with 80% power, computed using the standard error of the treatment effect. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Dynamic Effects

The pooled estimates may mask differences in timing and persistence. Effects may emerge quickly if access to the certification program improves job matching during search, or more gradually if employers learn about certified workers over time. They may also attenuate if the informational value of certification declines as employers acquire direct information about workers' productivity through labor market interactions (Farber and Gibbons, 1996). Figure 2 reports intention-to-treat estimates from Equation (2) at three, six, and twelve months after the intervention, separately for locals and migrants.[23]

For locals, the employment effect is positive in all three follow-up waves and remains statistically significant one year after the intervention. The reduction in inactivity is concentrated in the short run and fades by six months. The zero-imputed log wage follows a similar pattern to employment, consistent with earnings gains operating through the extensive margin rather than through wage increases among employed workers. Weekly hours worked also rise initially, but the effect declines from 3.5 additional hours at three months to 0.8 hours at twelve months. Overall, the dynamic pattern suggests that access to the certification program primarily accelerates labor market entry for locals, with the largest gains concentrated in the short run and persistent effects on employment after one year.

For migrants, the dynamic estimates are generally imprecise. We find no evidence of gains in overall employment, hours worked, or wages at any follow-up. Formal employment increases by 6 percentage points at three months, relative to a control-group mean of 6.4 percent, but the effect falls to essentially zero at six months and is smaller and statistically insignificant at twelve months. One possible interpretation is that certification temporarily shifted some migrants from informal to formal employment rather than increasing overall employment, although our data do not allow us to identify such transitions directly. Moreover, as discussed in Section 6, the three-month formal-employment estimate is not robust to the Lee-bounds adjustment. Another possibility is that the absence of an effect on employment may be explained by migrants beginning to search and then becoming discouraged after not finding a job. However, the dynamic estimates for inactivity are not statistically significant at three, six, or twelve months, so the pattern is not consistent with this interpretation.

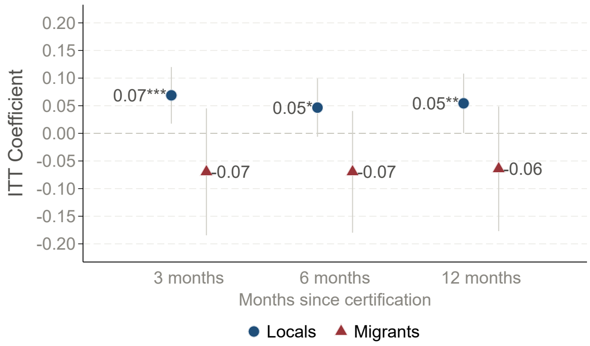
Skill downgrading is lower in all three follow-up waves, with the largest reduction at three months, but the later estimates are imprecise and the pooled estimate does not survive the multiple-hypothesis adjustment. Overall, the dynamic evidence does not point to an effect of certification on migrant labor-market outcomes.

[23] As discussed in Section 3.5, outcomes in the dynamic specification are measured at every follow-up survey and refer to the respondent's situation at that wave. For example, employment equals one if the respondent reports being employed in the reference week of that survey. The same logic applies to the other outcomes. Respondents who do not complete a given follow-up are excluded from the estimate for that wave.

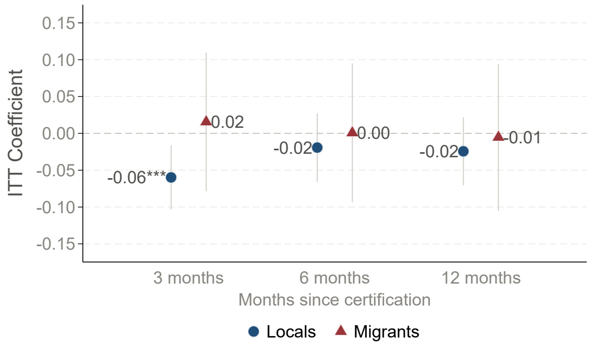
Figure 2: Effects of Access to Skills Certification on Labor Market Outcomes

Dynamic Intention To Treat Effects estimated using Equation (2)

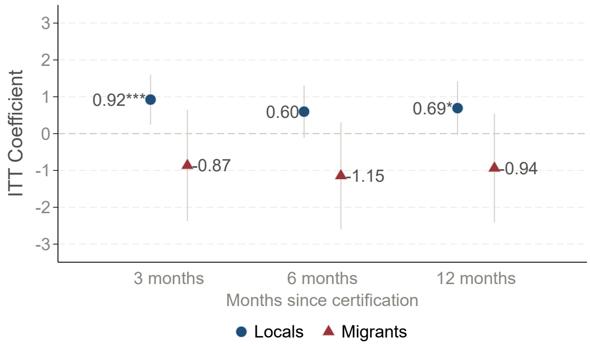
(a) Employment



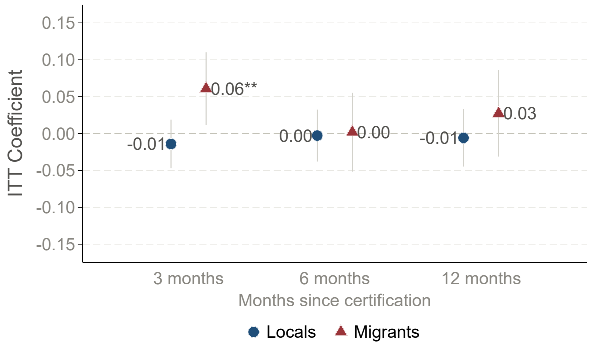
(b) Inactivity



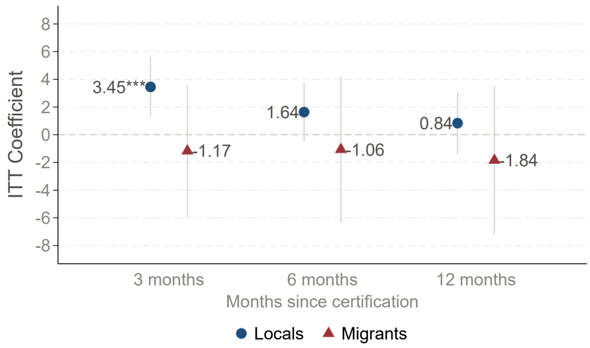
(c) Log Wage



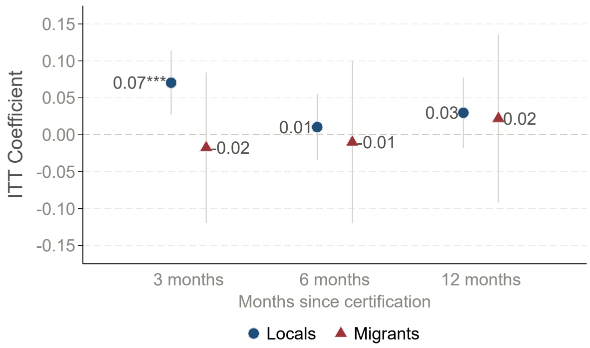
(d) Formal Employment



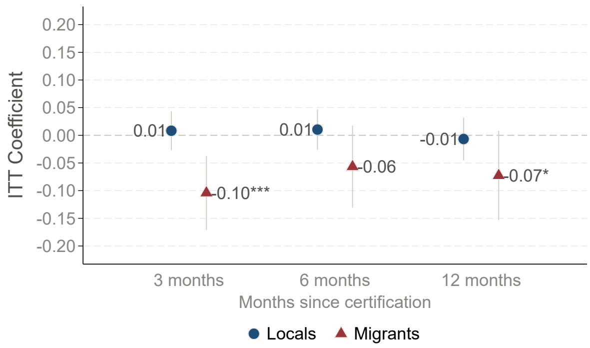
(e) Weekly Working Hours



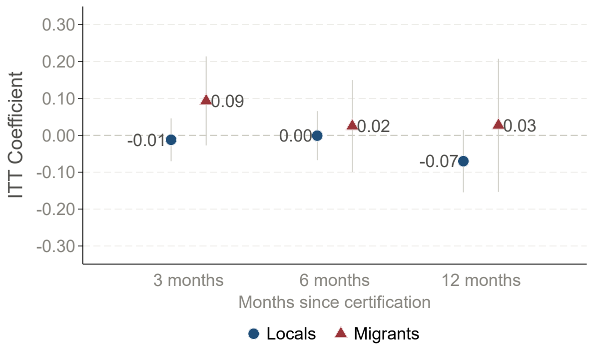
(f) Full Time Employment



(g) Skill Downgrade



(h) Log Reservation Wage



Notes: Coefficients from Equation (2). Each panel shows ITT estimates of the effect of assignment to certification at 3, 6, and 12 months after the intervention. Marker values show point estimates. Bars represent 95% confidence intervals. Standard errors are clustered at the individual level. All specifications include competence and wave fixed effects.

The dynamic evidence reinforces the pooled results for locals: employment increases quickly and remains positive after one year, while some of the other effects attenuate over time. For migrants, the estimates remain imprecise and do not reveal a clear pattern of persistent effects across outcomes. The dynamic estimates therefore help characterize the timing and persistence of the treatment effects, but they do not identify the mechanisms through which the program operates.

5.3 Sources of Heterogeneous Effects Across Locals and Migrants

The estimated effects differ across locals and migrants. Among locals, access to the certification program increases employment and reduces inactivity, consistent with greater labor-market entry. Among migrants, the estimates are less precise, and we do not detect an increase in employment. The difference in employment effects across the two groups is statistically significant.[24] These cross-group differences establish heterogeneity in the estimated effects of the program, but they do not identify why the program affects locals and migrants differently.

We examine two factors that may account for part of this divergence: limited statistical power and differences in sample composition, and then discuss mechanisms that the research design cannot separately identify.

Lack of statistical power.– The pooled estimation sample includes 1,243 migrant observations, roughly one-quarter of the 4,404 observations used for locals, and this difference is reflected in the precision of the estimates. At 80 percent power and a 5 percent significance level, the minimum detectable effect (MDE) for employment among migrants is 9.7 percentage points, nearly twice the 5.9 percentage point MDE for locals. A similar gap holds across the remaining outcomes in Table 2. For skill downgrading, the MDE is 9.9 percentage points, larger than the estimated 8.1 percentage point reduction. These calculations show that the migrant sample has limited power to detect effects of the magnitude we estimate. The absence of statistically significant effects for most migrant outcomes should therefore not be interpreted as evidence of precise zero effects.

Sample composition.– Locals and migrants differ in several baseline characteristics. Migrants are younger, more educated, and more likely to be employed. If these characteristics moderate the returns to certification, part of the difference in treatment effects could reflect sample composition rather than migrant status itself. We assess this possibility in two ways.

First, we reweight the migrant sample using normalized propensity-score weights

[24] Appendix G reports formal tests of equality between the local and migrant treatment effects. We reject equality at the 5 percent level for employment and skill downgrading, and at the 10 percent level for the zero-imputed log wage and weekly hours worked.

so that its distribution of observed baseline characteristics more closely matches that of locals (Busso et al., 2014). Reweighting substantially reduces the baseline differences between the two groups, although some differences remain.[25] The reweighted migrant estimates remain similar to the baseline estimates in sign and magnitude. Employment and inactivity effects remain small, while the estimated reduction in skill downgrading is similar in magnitude but less precisely estimated.

Second, we examine whether treatment effects vary with baseline labor-force status.[26] Among locals, the employment response is largest for individuals who were inactive at baseline: certification increases their employment probability by 10.6 percentage points. The corresponding effects on the zero-imputed log wage and weekly hours worked are also largest for this group. This pattern indicates that an important part of the local employment and hours response operates through entry into employment among individuals who were initially outside the labor force, rather than only through increased hours among workers who were already employed. The estimates for migrants show no similarly systematic pattern across baseline labor-force states.

These exercises suggest that baseline labor-force status helps explain some of the pattern among locals: individuals who were initially inactive account for a substantial part of the employment and hours response. Differences in observable composition, however, do not appear to account for the overall contrast between locals and migrants. Reweighting migrants to resemble locals on observed baseline characteristics leaves the main pattern of estimates largely unchanged.

Potential mechanisms.– These exercises do not identify why certification generates different effects for locals and migrants. One conjecture is that employers respond differently to the same certificate across the two groups. Migrants' foreign education and work experience may be less familiar to Colombian employers, so a domestic certificate may interact differently with the information employers already possess. Another possibility is that certification affects workers' own job-search and job-acceptance decisions differently across groups. Our study does not directly observe employer beliefs or separately manipulate these channels and therefore cannot distinguish between these explanations.

[25] After reweighting, most baseline characteristics are statistically indistinguishable between locals and migrants. Differences remain for household headship, primary and tertiary education, and low work experience. Excluding these variables from the propensity-score model does not materially affect the reweighted estimates. Appendix G.1 reports the reweighting procedure, balance diagnostics, and corresponding treatment-effect estimates.

[26] Appendix Figure G.1 reports treatment effects by baseline labor-force status. Among locals, tests of equality across baseline states reject equality for employment and the zero-imputed log wage, and are marginal for weekly hours worked. The corresponding tests do not reject equality among migrants.

6 Robustness: Differential Attrition

Section 4 documents high overall response rates but moderate differential attrition, concentrated among migrants in the three- and six-month follow-up surveys. If non-responding control-group members had systematically better outcomes than respondents, the ITT estimates could overstate treatment effects. To assess this possibility, we follow Lee (2009) and estimate bounds for the main treatment effects, separately for locals and migrants.

We estimate ITT effects and Lee bounds for the full sample, locals, and migrants across the three follow-up waves.[27] For locals, the bounds are tight, reflecting limited differential attrition in this subgroup. At three months, both bounds for employment are positive and statistically significant, with a lower bound of 0.059 and an upper bound of 0.082. The corresponding bounds for inactivity are negative and statistically significant. The bounds for the zero-imputed log wage and full-time employment are also bounded away from zero at three months. For weekly hours worked, the upper bound is positive and statistically significant at three months, but the lower bound is not. The lower bounds for these outcomes lose significance at six and twelve months. This pattern is consistent with the attenuation documented in the dynamic estimates. Overall, the bounds indicate that selective non-response is unlikely to explain the main employment effects for locals.

For migrants, the bounds are wider, reflecting larger differential attrition in this subgroup. At three months, both bounds for skill downgrading are negative and statistically significant. At six months, the lower bound remains negative and significant, while the upper bound is negative but not statistically significant. At twelve months, both bounds are negative, but only the lower bound is statistically significant. Thus, the Lee-bound exercise does not overturn the negative short-run estimate, but it provides weaker evidence at later follow-ups and does not establish persistence of the skill-downgrading effect.

7 Certification Receipt and Program Valuation

Stated preferences suggest that the program was valued by participants, but not differentially affected by treatment assignment. Approximately 88.2 percent of control-group respondents would participate in the certification program without subsidies. This share is similar among locals and migrants. Treatment assignment has no statistically significant effect on willingness to participate without subsidies. Willingness to pay is also positive for most respondents: 63.7 percent of control-group respondents

[27] Results are reported in Appendix H.1.

report a positive willingness to pay, with an average midpoint of 19.10 USD. The estimated treatment effects on willingness to pay and on the WTP midpoint are small and statistically insignificant in the full sample and within both subgroups. These results indicate that stated demand for certification was high, but that the offer of access to the certification program did not measurably change stated valuation.[28]

Perceived program impact was collected only for treated participants in the first follow-up and is therefore not used in the experimental analysis. Descriptively, however, 64.0 percent of treated respondents reported that the certificate helped them greatly or moderately in finding a job.

Because certification receipt was high but incomplete, we also estimate the effect of certification receipt using random assignment as an instrument. Table 3 reports the IV estimates. As expected given the high compliance rate, they are slightly larger in magnitude than the corresponding ITT estimates but lead to the same substantive conclusions. Among locals, certification receipt increases employment, weekly hours worked, and full-time work, while the estimates for formal employment and skill downgrading remain small. Among migrants, the estimates remain imprecise, with the exception of the negative estimate for skill downgrading. Overall, incomplete compliance does not materially affect the interpretation of the ITT results.

[28] See Appendix Table I.1.

Table 3: Effects of Skill Certification on Labor Market Outcomes

Instrumental Variables Estimates

			Outcomes Replaced by Zero if Non-Employed				
	Employed	Inactive	Log Wage	Formal emp.	Weekly working hours	Full time (7+ hrs)	Skill Downgrade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Sample							
Received certification	0.039**	-0.026	0.538**	-0.001	1.342	0.026	-0.018
	(0.020)	(0.016)	(0.268)	(0.014)	(0.859)	(0.017)	(0.016)
Q-value	0.192	0.192	0.192	0.378	0.192	0.192	0.227
Mean Control	0.530	0.240	6.734	0.128	14.75	0.256	0.156
First stage F	7846	7846	7815	7846	7846	7846	7846
Observations	5647	5647	5634	5647	5647	5647	5647
Panel B: Locals							
Received certification	0.063***	-0.039**	0.816***	-0.008	2.218**	0.041**	0.005
	(0.023)	(0.020)	(0.310)	(0.017)	(0.941)	(0.019)	(0.018)
Q-value	0.032	0.046	0.032	0.258	0.033	0.042	0.293
Mean Control	0.494	0.264	6.199	0.136	12.88	0.221	0.136
First stage F	5530	5530	5502	5530	5530	5530	5530
Observations	4404	4404	4392	4404	4404	4404	4404
Panel C: Migrants							
Received certification	-0.036	0.012	-0.369	0.025	-1.615	-0.022	-0.088**
	(0.038)	(0.026)	(0.527)	(0.027)	(1.984)	(0.040)	(0.039)
Q-value	1.000	1.000	1.000	1.000	1.000	1.000	0.192
Mean Control	0.705	0.127	9.323	0.0903	23.79	0.424	0.250
First stage F	2658	2658	2655	2658	2658	2658	2658
Observations	1243	1243	1242	1243	1243	1243	1243
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Received certification is an indicator equal to one for individuals who received a skill certification, instrumented by random assignment to the program. The table reports pooled 2SLS estimates of the effect of certification receipt. First stage F reports the Kleibergen-Paap rk Wald F-statistic for the excluded instrument, robust to clustering at the individual level. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

8 Channels and Additional Outcomes

This section examines outcomes that help interpret the pattern of treatment effects. The results in Section 5 show clear employment gains among locals, while the estimates for migrants are less precise and do not show comparable gains in employment, hours worked, or wages. The outcomes below do not identify specific mechanisms. Rather, they provide evidence on whether the treatment is associated with faster job finding, changes in job-search behavior, or subsequent education and training responses. Because the follow-up horizon is short, enrollment in education, training, or additional certification should be interpreted as a post-program response rather than as a mechanism that can explain the short-run labor-market effects.

8.1 Job Search, Education, and Training

Table 4 reports ITT estimates for job-search outcomes and post-program education and training. Job-search outcomes include the reservation wage, unemployment duration, and search intensity. Education and training outcomes include enrollment in additional education, other certification programs, and training courses. The pooled reservation wage combines the minimum salary at which unemployed respondents would accept a job and the minimum salary at which employed respondents would change jobs, controlling for follow-up employment status.[29]

For locals, the results suggest that employment gains are driven by shorter unemployment spells and not by changes in reservation wages or more intensive search. The estimated effect on the pooled log minimum acceptable wage is small and statistically insignificant (-0.027), with no evidence of meaningful differences by follow-up employment status. Search intensity is also unaffected, though this outcome is observed only for the unemployed and should be interpreted cautiously given the smaller sample. By contrast, the offer of access to the certification program reduces unemployment duration by 0.226 months. This pattern is consistent with the treatment facilitating labor-market entry without inducing detectable changes in reservation wages or search intensity. The offer also reduces enrollment in other certification programs by 4.3 percentage points and increases enrollment in training courses by 2.7 percentage points, with no statistically significant effect on enrollment in broader educational programs. These education and training responses do not appear to explain the short-run employment effects, but suggest some substitution away from additional certification and a modest increase in short training.

[29] This approach follows Abebe et al. (2021) and Carranza et al. (2022).

Table 4: Effects of Access to Skill Certification on Reservation Wages, Job Search Behavior, Education, and Training

Intention-To-Treat estimated using Equation (1)

	Reservation Wage			Job Search Behavior		Education and Training		
	Pooled Res. wage (1)	Res. Wage Unemp. (2)	Res. Wage Emp. (3)	Unemp. duration (months) (4)	Search intensity (hrs/week) (5)	Enr. in add. educ. (6)	Enr. in cert. program (7)	Enr. in training course (8)
Panel A: Full Sample								
Treatment	-0.016	-0.002	-0.042	-0.164**	-0.133	-0.033**	-0.045***	0.027**
	(0.023)	(0.035)	(0.027)	(0.083)	(0.159)	(0.016)	(0.004)	(0.011)
Q-value	0.932	1.000	0.601	0.108	0.253	0.029	0.001	0.022
Mean Control	14.25	14.11	14.38	1.894	1.875	0.267	0.0440	0.112
MDE	±0.065	±0.097	±0.076	±0.233	±0.446	±0.045	±0.011	±0.032
Observations	5509	2467	3042	5647	1588	5549	5549	5549
Panel B: Locals								
Treatment	-0.027	-0.032	-0.048	-0.226**	-0.264	-0.030	-0.043***	0.027**
	(0.027)	(0.037)	(0.033)	(0.099)	(0.170)	(0.019)	(0.004)	(0.013)
Q-value	0.625	0.625	0.625	0.048	0.066	0.066	0.001	0.043
Mean Control	14.25	14.13	14.38	2.067	1.886	0.275	0.0417	0.111
MDE	±0.076	±0.103	±0.093	±0.277	±0.477	±0.053	±0.011	±0.036
Observations	4299	2092	2207	4404	1356	4333	4333	4333
Panel C: Migrants								
Treatment	0.028	0.177*	-0.016	0.033	0.612	-0.037	-0.054***	0.028
	(0.046)	(0.101)	(0.045)	(0.144)	(0.443)	(0.031)	(0.011)	(0.024)
Q-value	0.913	0.322	0.913	0.695	0.509	0.203	0.001	0.203
Mean Control	14.25	14	14.35	1.056	1.775	0.226	0.0548	0.113
MDE	±0.128	±0.284	±0.126	±0.402	±1.240	±0.086	±0.030	±0.068
Observations	1210	375	835	1243	232	1216	1216	1216
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Column (1) reports the log minimum acceptable wage, defined as log(reservation wage + 1), pooling the minimum salary to accept a job (unemployed respondents) and the minimum salary to change jobs (employed respondents). Column (2) restricts to individuals unemployed at follow-up and reports the minimum salary to accept a job offer. Column (3) restricts to individuals employed at follow-up and reports the minimum salary to change jobs. Unemployment duration is set to zero for employed and inactive respondents. Search intensity was collected only in Follow-ups 2 and 3 and only among unemployed respondents. Standard errors (in parentheses) are clustered at the individual level. Sharpened q-values Anderson (2008) are computed within each panel over three outcome families matching the column groups: reservation wages (Columns 1–3), job search behavior (Columns 4–5), and education and training (Columns 6–8). All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

For migrants, the additional outcomes do not reveal a clear mechanism. The pooled effect on the log minimum acceptable wage is small and statistically insignificant. Reservation wages increase among migrants who are unemployed at follow-up, but this estimate conditions on a post-treatment employment outcome and should therefore be interpreted cautiously. We also find no statistically significant effect on unemployment duration or search intensity. The treatment reduces enrollment in other certification programs by 5.4 percentage points, while the estimates for broader educational enrollment and training are imprecise. Overall, these outcomes provide little evidence that changes in job-search behavior or subsequent education and training explain the migrant results.

Additional labor-market outcomes provide a similar contrast across groups.[30] Among locals, the offer of access to the certification program increases self-employment and employment in small firms, and it raises the probability of changing sector and changing jobs.[31] These margins are consistent with the employment effects documented above: certification appears to facilitate labor market entry and broader job mobility among locals. For migrants, the estimates provide little evidence of changes in firm type or job changes. The estimates for changing sector and for the number of job offers are negative but only marginally significant.

Among locals, the treatment reduces unemployment duration and increases employment without detectable changes in reservation wages or search intensity, and it is accompanied by greater labor-market mobility. These patterns are consistent with faster labor-market entry, although they do not identify the mechanism through which the program generated that response. For migrants, neither the job-search outcomes nor the additional labor-market measures reveal a systematic pattern that explains the main estimates. We also find little evidence that subsequent education, training, or credential acquisition accounts for the treatment effects over the period we study.

8.2 Job Satisfaction and Well-Being

We find little evidence that the offer of access to the certification program affected job satisfaction or broader measures of well-being.[32] In the full sample, the estimates for job satisfaction, skipping a meal, and anxiety are small and statistically insignificant. Among locals, we find no corresponding improvement in job satisfaction, food insecurity or anxiety. Among migrants, the estimates are also statistically insignificant and do not point to systematic improvements in well-being. Overall, these results suggest that the program's detectable effects are concentrated in labor-market outcomes rather than broader short-run well-being.

[30] Appendix Table I.2 reports effects on job mobility and firm characteristics.

[31] This effect is less directly tied to employer screening, since self-employed workers do not face an employer in the same way as wage workers. One interpretation is that certification may also operate as a broader market signal, helping self-employed workers communicate skills to clients, contractors, or other parties that hire their labor.

[32] Appendix Table I.3 reports effects on job satisfaction, food insecurity, and anxiety.

8.3 Certifications and financial assistance

Because the intervention combines certification with financial assistance, the employment effects could partly reflect a relaxation of liquidity constraints rather than certification alone. This alternative is particularly relevant because the transfers were paid upon completion, shortly before the first follow-up, when the local employment effect is largest. The experiment does not separately randomize certification and financial assistance, so we cannot identify their independent effects.

We nevertheless examine whether treatment effects are larger among participants eligible for higher financial support. All participants received \$82, and those with children under their care or over 60 years old received an additional \$75.[33] We interact treatment assignment with baseline caregiver status for those under 60 and find no evidence that the employment effect is larger among this group. Among locals, if anything, the estimated employment and full-time-work effects are smaller for caregivers. Among migrants, we find no systematic differential effects.[34] Likewise, interacting treatment assignment with a dummy for being over 60 at baseline excluding caregivers shows no heterogeneous effect on employment.

This exercise does not isolate the causal effect of the transfer amount, because caregiver status and being over 60 years old may themselves moderate the returns to the program. It does, however, provide little support for the conjecture that the main employment results are driven by larger financial transfers.

9 Policy Discussion

The findings raise two policy questions: whether certification is cost-effective relative to other active labor market policies, and whether the estimated effects are likely to persist as the program expands in scale.

Cost-effectiveness. How does the program compare with other active labor market policies? Using the 2024 implementation cost of \$203 per participant (excluding financial incentives) and the 5.7 percentage point ITT effect on employment among locals, the implied cost per additional worker employed is \$3,561.[35] The evaluated cohort was considerably smaller than previous rounds of the program: it covered 855 participants, compared with 2,500–3,150 participants per year between 2021 and 2023. The average

[33] Additional incentives for transportation and the remedial session are negligible in the 2024 wave of the program, as only 0.2% of the sample needed the remedial session (see Table 1) and transportation incentives applied only to participants in rural areas.

[34] See Appendix J for details.

[35] Appendix Table K.1 reports the calculations and benchmarks. Program costs are reported in Appendix Table B.1.

cost per participant in those earlier rounds, expressed in 2024 dollars, was \$142. Applying the employment effect estimated in our experiment to this historical cost yields an implied cost of \$2,486 per additional worker employed. This second calculation is illustrative: it assumes that the employment effect would remain unchanged if the program were implemented at the scale and cost of previous rounds.

We compare these figures with evidence on other active labor market policies in developing countries. McKenzie (2017) reviews vocational-training programs with costs ranging from \$17 to \$2,170 per participant and implied costs of approximately \$21,700–\$76,700 per additional job in 2024 dollars. In Colombia, the program *Jóvenes en Acción* cost approximately \$1,093 per participant in 2024 dollars and increased formal employment by 4 percentage points (Attanasio et al., 2011), implying a cost of approximately \$27,320 per additional formal job. Job-search and matching interventions are much cheaper per participant, at approximately \$4.50–\$260, but employment effects are uncommon. Among the interventions summarized by McKenzie (2017), only one of ten generated a statistically significant employment effect. For that intervention, implemented in Jordan, the implied cost per additional placement is approximately \$29,100. Wage-subsidy programs show a different pattern: employment effects tend to be concentrated during the subsidy period and fade after the subsidy expires.

These comparisons suggest that skill certification can be a relatively low-cost way to increase employment among locals. Even using the higher cost of the evaluated 2024 cohort, the estimated cost per additional worker employed is about one-quarter of the lower end of the vocational-training benchmark and well below the cost per placement in the job-search intervention that generated a significant employment effect. At the historical per-participant cost, the implied figure is lower still. This comparison is particularly relevant because certification verifies existing skills rather than providing training and therefore does not require the program to teach new skills. These comparisons should nevertheless be interpreted cautiously. The programs differ across countries, populations, outcomes, and implementation settings; some benchmarks measure formal employment rather than employment more broadly; and cost definitions may not be fully comparable. Most importantly, our lower-cost calculation assumes that the treatment effect estimated in the 2024 experiment would persist at the larger scale of earlier rounds. Our experiment does not test that assumption.

Scalability.– Certification may operate partly as a relative signal. If a certificate improves a worker's position relative to other applicants rather than increasing labor demand, gains for certified workers could come partly at the expense of non-certified workers. Such displacement would also imply that treatment effects estimated at small scale need not persist when certification becomes widespread: as coverage increases, the credential may become less scarce and its informational value may decline. Similar displacement effects have been documented for job-search assistance (Crépon et al., 2013).

This concern is also relevant for interpreting the local and migrant estimates in our experiment. Locals and migrants participate in the same regional labor markets, so, in principle, employment gains among treated locals could partly reflect displacement of migrants or other non-treated workers. Such spillovers would violate the no-interference assumption underlying the standard interpretation of the experimental estimates. The scale of the intervention, however, makes large displacement effects within local labor markets unlikely. Treated participants account for less than 0.1 percent of the working-age population in most municipalities in our sample; the largest share is 1.03 percent, in Santa Rosa, Cauca.[36] These small treatment intensities reduce the scope for the program to generate meaningful changes in aggregate labor-market conditions or substantial displacement across the two groups at the scale evaluated here. They do not, however, rule out spillovers within more narrowly defined occupations or hiring networks.

The implications for scaling are therefore less certain. A larger program could generate spillover effects. Thus, the value of certification itself could change as the share of certified workers rises. Our design cannot identify these general-equilibrium effects. Doing so would require variation in program intensity across (isolated) local labor markets.

10 Conclusions

This paper provides experimental evidence on whether a skill certificate can improve labor market outcomes for workers whose skills are difficult for employers to assess. We study a randomized offer of access to an occupation-specific certification program in Colombia, implemented among both locals and Venezuelan migrants. The program is designed to certify existing skills rather than provide training, making it a useful setting to study the value of labor market signals.

The effects differ across groups. For locals, the offer of access to the certification program increases employment and reduces inactivity. The earnings effect is driven by this extensive-margin response rather than by higher wages among those already employed. The dynamic estimates show that these effects emerge quickly and partly persist after one year. The evidence from the mechanism is consistent with certification reducing screening frictions and facilitating labor market entry.

For migrants, the estimates are less precise and do not show comparable gains in

[36] Appendix Figure K.1 reports the number of local and migrant participants by municipality and the number of treated participants as a share of the municipal working-age population.

employment, hours worked, or wages. We find suggestive evidence of a reduction in skill downgrading, but this estimate does not survive the multiple-hypothesis adjustment and partly reflects changes in employment composition. We therefore do not interpret the migrant results as establishing an improvement in occupational matching. More generally, the experiment provides limited evidence on how certification affects migrants and lacks the statistical power to rule out economically meaningful effects for this group.

These findings have two implications. First, credible certification of existing skills can affect labor-market outcomes even without providing training. Second, the effects of certification can differ substantially across groups. In our setting, the evidence is strongest for employment gains among locals, while the migrant estimates are less precise and do not show comparable gains in employment, hours worked, or wages. This heterogeneity matters for policy design and evaluation: average effects may mask important differences across target populations, and understanding the sources of those differences requires evidence beyond the treatment effects themselves.

Several limitations remain. The estimates come from one cohort in Colombia's South-western region and cover outcomes up to twelve months after certification. The migrant sample is smaller than the local sample, which limits precision for some outcomes. The experiment also does not directly observe employer beliefs or hiring behavior, so the signaling interpretation remains indirect. Future work that measures employer responses to certified and non-certified applicants would help distinguish more directly between demand-side screening, worker search behavior, and other mechanisms.

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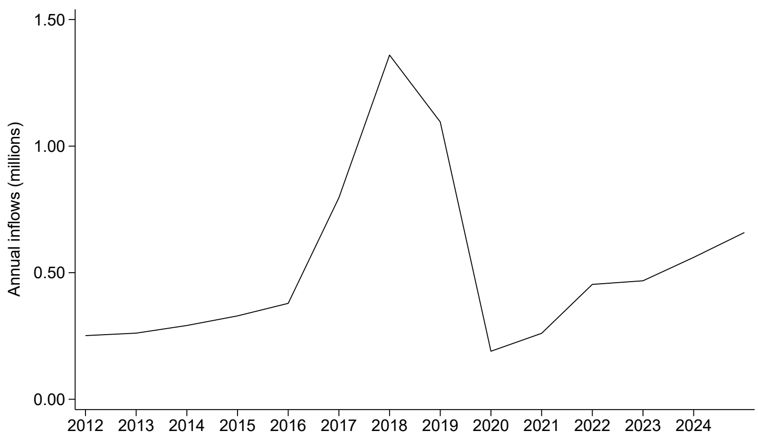
A Venezuelan Migration to Colombia

Forced displacement has increased in recent years, driven by armed conflict, political instability, economic crises, and natural disasters. Recent estimates indicate that more than 304 million people are currently displaced worldwide, including approximately 8 million asylum seekers. Latin America is an important part of these flows, with more than 48 million migrants recorded in 2024 (UNDESA, 2025).

A major driver of recent migration in Latin America is the economic and political crisis in Venezuela. Since the late 1990s, Venezuela has experienced increasing state intervention, fiscal expansion, institutional deterioration, and political repression (Rodríguez, 2008; Corrales, 2015; Faria, 2008). The economy remained highly dependent on oil revenues, leaving it exposed to the collapse in global oil prices in 2014. The subsequent deterioration contributed to hyperinflation, shortages of basic goods, and a severe humanitarian crisis.

Colombia became the main destination for Venezuelan migrants, partly because of geographic proximity and a shared language (Bahar et al., 2021). The two countries share a border of more than 2,000 kilometers. As of 2024, Colombia hosted approximately 2.86 million Venezuelan migrants, or roughly one-third of all Venezuelan migrants, with the bulk of the inflow concentrated between 2016 and 2020 (see Appendix Figure A.1; R4V, 2024). This inflow placed pressure on Colombia's labor market and institutional capacity (Delgado-Prieto, 2024).

Appendix Figure A.1: Annual Inflows of Venezuelans to Colombia



Notes: Annual entries of Venezuelans recorded at Colombian border crossings. *Source: Colombian migration authority.*

In response, Colombia implemented a large-scale regularization process that granted legal status to many Venezuelan migrants (Bahar et al., 2021; Ibáñez et al., 2024). As of August 2024, Colombia had issued over 2,092,619 temporary protection permits under the *Estatuto Temporal de Protección*, a program that allows migrants to work legally and access health and education services. As in the rest of the region, Venezuelan migrants in Colombia represent a demographic and economic asset: they are younger, participate in the labor force at higher rates, and are more likely to be employed than native workers (Kaplan et al., 2023). Yet this potential is offset by systematic disadvantages in job quality—migrants across LAC face higher rates of informality (47.8% versus 40.4% among natives) and overqualification (23.9% versus 17.0%), reflecting a broader pattern of human capital underutilization (Kaplan et al., 2023).

A key mechanism behind this gap is occupational mismatch. Venezuelan migrants continue to face high rates of informality (Delgado-Prieto, 2025), poverty, and precarious employment, particularly among those whose skills and credentials are difficult to verify in the Colombian labor market (Bahar et al., 2021; Lebow, 2023). This reflects a broader regional pattern: across Latin America and the Caribbean, approximately 24% of highly educated foreign-born workers are employed in low- or medium-skilled jobs, compared to 17% of their native-born counterparts (Kaplan et al., 2023). The Colombian case has a distinct profile within this trend: Venezuelan migrants in Colombia tend to have lower levels of formal education than those who settled further afield—in Peru, Ecuador, or Chile—likely because migrants with higher educational attainment had the resources to travel longer distances (Kaplan et al., 2023; Liebig et al., 2023). As a result, overqualification rates in Colombia, while present, are less pronounced than in those destinations. Yet even within this lower-education population, skill downgrading is prevalent, with many migrants working in occupations below their actual competency level (Bahar et al., 2021; Lebow, 2023). These frictions—informality, credential barriers, and occupational mismatch—constitute the principal integration gaps that employment support policies have sought to address.

B Skills Certification Program

B.1 Institutional Background on the Skill Certification Program

The Colombian skill certification program, formally known in Spanish as "*Programa de Evaluación y Certificación de Competencias Laborales*", was established under Decree 933 of 2003 and is administered by SENA (Servicio Nacional de Aprendizaje), the Colombian Vocational Training Agency. The program was created to formally recognize occupational skills acquired through work experience, on-the-job training, or non-formal learning, complementing SENA's traditional role as the country's main provider of technical and vocational training. Certification is occupation-specific and is based on national occupational standards (*Normas Sectoriales de Competencia Laboral*) developed in consultation with sectoral round-tables (*Mesas Sectoriales*) that bring together representatives from industry, workers' organizations, government, and academia. This structure ensures that the competencies assessed reflect skills demanded in the Colombian labor market.

Since its inception, the program has expanded substantially in both scope and scale. It currently covers 2,405 competencies across manufacturing, agriculture, retail, and service occupations, and certifies more than 200,000 workers each year throughout the country. Certification is free of charge for applicants and is delivered by qualified assessors trained and accredited by SENA, who follow standardized evaluation protocols to ensure consistency across regions and competencies.

B.2 The "Skills Pay Off" Program

B.2.1 Origin and Eligibility

Saber Hacer Vale ("Skills Pay Off") was introduced as part of Colombia's National Development Plan 2018–2022 (*Plan Nacional de Desarrollo 2018–2022: Pacto Por la Equidad*), a government strategy aimed at promoting social inclusion and productivity. The program is implemented jointly by the Colombian Ministry of Labor and SENA. It preserves the core features of the underlying skill certification program described above, but complements it with additional light-touch resources and financial assistance (Appendix B.2.2 describes these components).

The program targets foreign migrants and Colombians classified as vulnerable. To be eligible for the program, participants must meet at least one of the following criteria: being a household head or having dependents, being between 18 and 28 years old or over 60, belonging to an ethnic group or the LGBTIQ+ community, being a migrant or Colombian returnee,[1] or having low income.[2] Additionally, participants must demonstrate at least 6 months of experience related to the certification area. At least 30 percent of the available slots are reserved for migrants with regular permit status.

Skills Pay Off is implemented in four stages. First, the Ministry of Labor identifies a

[1] Participants must be registered in the public list known as *Registro Único de Retornados*.

[2] Colombia classifies its population based on a score used to target social aid programs, known as SISBEN (*Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales*). The first three categories in the SISBEN score are considered low income and eligible for social aid.

geographic area for implementation and defines the set of competencies to be certified in consultation with productive-sector representatives.[3] Second, the number of participants is determined according to the available budget. Third, the call for applications is launched, and candidates complete an enrollment form on the Ministry's website. Finally, applicants' documentation is reviewed and verified to determine eligibility for the program.

B.2.2 Program Bundle

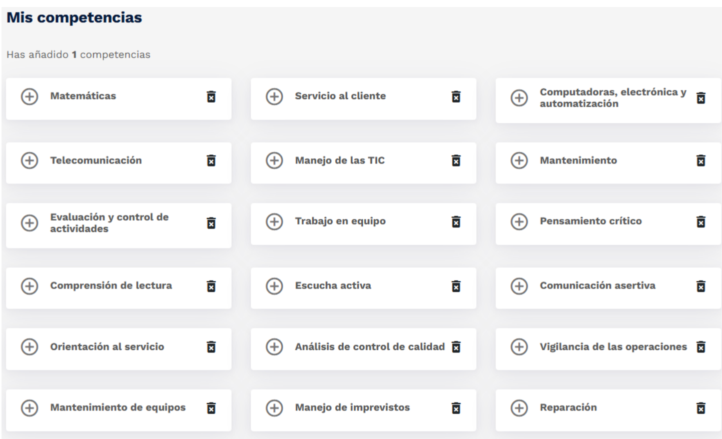
Skills Pay Off complements the certification process with two additional components: (i) information and support resources, and (ii) financial assistance during participation.

Information and Support Resources. This component bundles three resources that distinguish *Skills Pay Off* from the underlying SENA certification:

1. Motivational support: The program provides participants with a tracking mechanism (using text messages, emails, phone calls, or in-person aid) intended to avoid dropout. This component is only offered to treated individuals who do not show up for the evaluation process. It applies only to participants who miss the evaluation appointment and therefore affects a small share of the treatment group.

2. Occupational support: The intervention includes a one-hour voluntary online training on using the digital platform *TuBio*, a digital tool that creates a curriculum template and provides recommendations on potential occupations. *TuBio* gathers information about the participant's prior education, experience, activities (such as hobbies, household chores, voluntary work, and others), languages, and competencies (see Figure B.1 for a list of the offered competencies). Based on this information, *TuBio* creates a curriculum template (an example is shown in Figure B.2) and provides feedback about potential occupations that might be of interest to the participant (Figure B.3 presents an example of recommended occupations for the profile shown in Figure B.2).

Appendix Figure B.1: Competencies Required by *TuBio*



[3]The competencies evaluated are typically selected based on labor demand data and the historical success of technical certifications.

Appendix Figure B.2: Curriculum Created by *TuBio*

Appendix Figure B.3: Occupational Recommendations Given by *TuBio*

3. Labor Market Platforms Training: *Skills Pay Off* provides beneficiaries with information about the Public Employment Service. This mainly includes access to the public job-search platform. During the first session of the evaluation process, participants receive information about these platforms. If they request it, they can then receive support to register on these services.

Financial Incentives: All beneficiaries of *Skills Pay Off* receive food and connectivity subsidies totaling approximately $82 USD. Some beneficiaries qualify for additional support, including $75 USD for those with dependents or those over the age of 60, a $30 USD transportation subsidy if the assessment is conducted in person, and a $30 USD subsidy for those enrolled in the remedial session. Participants can receive up to $217 USD in total support, equivalent to approximately 70 percent of the monthly minimum wage. The average cost per participant among all waves of the program (2021-2024) is approximately $182 USD, as shown in Table B.1. For the 2024 wave of interest, the cost per participant was $202.8 USD without financial incentives and $333.2 USD including them.

Appendix Table B.1: Implementation Costs of *Skills Pay Off*

Year	Full Cost USD	Beneficiaries	Cost per-person USD
2021	373,253	3,050	122.4
2022	402,959	3,150	127.9
2023	357,129	2,500	142.9
2024 (excluding incentives)	173,402	855	202.8
2024 (total)	284,923	855	333.2
Avg. cost per person			181.6

Notes: Cost per-person USD is calculated as the ratio between total implementation costs and the number of beneficiaries in each year. The 2024 total includes the cost of participation incentives paid to the evaluation cohort ($111,521). The average cost per person is the simple average across the four yearly totals (2021–2023 and 2024 total).

B.3 Program Roll-Out and the 2024 Cohort

Before 2024, access to *Skills Pay Off* was allocated on a first-come, first-served basis. More than 12,000 individuals were certified through the program during its first four years, including approximately 4,000 migrants. These rounds were implemented nationwide and covered 38 competencies. Table B.2 reports participation by implementation wave since 2021.

Appendix Table B.2: Yearly Participation in *Skills Pay Off* Program

Call	Actual Beneficiaries	Colombian	Migrants	% of Migrants
2021	2,947	1,989	958	32.51
2022-1	1,636	1,114	522	31.91
2022-2	4,429	2,943	1,486	33.55
2023	2,623	1,581	1,042	39.73
2024	855	605	250	29.24
Total	12,490	8,232	4,258	

Notes: The table reports historical participation figures for the *Skills Pay Off* program.

The 2024 call was advertised through several channels, including social media, radio, and television. On-site agents were also hired to facilitate enrollment. Interested individuals registered through an online application form between June and August 2024. Submitted documents were reviewed between August 1st and September 3rd to determine whether applicants met the program's eligibility criteria.

The five competencies certified in the 2024 cohort were defined as follows. *Health services* referred to care activities performed according to protocols for basic daily activities and degree of autonomy. *Food handling* covered food handling according to technical procedures and regulations. *Food preparation* covered preparation based on production orders and standard recipes. *Design and tailoring* covered modifications according to tailoring and dressmaking techniques. *Recycling operations* covered conditioning recovered materials according to technical requirements. Table B.3 shows the number of participants by treatment status.

Appendix Table B.3: Sample sizes by Competencies and Treatment Status

Competency	Local Sample		Migrant Sample	
	Treatment	Control	Treatment	Control
Health Services	294	459	36	34
Food Preparation	153	300	132	122
Food handling	72	130	48	41
Design and tailoring	42	111	18	23
Recycling Operations	44	71	16	14
Total	605	1071	250	234

Notes: The table reports the distribution of participants across competencies, by treatment status and separately for locals and migrants.

B.4 Treatment Bundle of the 2024 Cohort

In addition to certification and financial assistance, *Skills Pay Off* includes motivational support, occupational support, and platform training. The available evidence suggests that these light-touch components are unlikely to account for the main treatment effects.Three reasons support this interpretation. First, these resources are free and publicly available regardless of program status, and applicants in both the treatment and control groups were informed of their availability at registration. Second, use of these resources is voluntary, including among individuals assigned to treatment, so any effects would depend on take-up by a self-selected subset of participants. Third, the resources are light-touch and unlikely, on their own, to generate large effects on the outcomes we study (Carranza and McKenzie, 2024b; McKenzie, 2017; Kelley et al., 2024).

Appendix Table B.4 provides empirical support for this interpretation by reporting take-up of the *TuBio* online platform across treatment and control groups, separately for locals and migrants. Overall take-up is low: around 2 percent among locals and 6 percent among migrants. For locals, usage is statistically indistinguishable between the treatment and control groups (2.1 percent versus 1.9 percent; p-value = 0.709). For migrants, treatment-assigned participants are more likely to use *TuBio* than controls (8.4 percent versus 3.8 percent; p-value = 0.037), but take-up remains low in absolute terms in both groups, making it unlikely to drive the skill-downgrading effects documented among migrants.[4]

Appendix Table B.4: TuBio Usage by Treatment and Migration Status

	Locals			*Migrants*			
	Treatment (1)	Control (2)	p-value (3)	Treatment (4)	Control (5)	p-value (6)	Total (7)
Used TuBio	13	20	0.709	21	9	0.037	63
Percentage	2.1%	1.9%		8.4%	3.8%		2.9%
N	605	1,071		250	234		2,160

Notes: Each cell shows the number of participants who accessed the platform and the corresponding share of the group. Columns (3) and (6) report the p-value from an OLS regression of platform access on treatment assignment, controlling for competence fixed effects, estimated separately for locals and migrants, and with robust standard errors.

Platform training consisted of a one-hour session on the Public Employment Service, focused on how to access the public job-search platform. This light-touch intervention is unlikely to drive the estimated effects.

Finally, only 89 treatment-assigned participants (10 percent) withdrew before assessment, so motivational support was relevant for a small share of the treatment group (see Table 1).These patterns suggest that the information, platform, and motivational-support components are unlikely to account for the main treatment effects. The financial assistance is different: because it was available only through treatment and could be economically meaningful for participants, its contribution cannot be separately identified from that of certification. In the paper we examine whether treatment effects are larger among participants eligible for higher transfers and finds no evidence of larger employment effects for that group.

B.5 Anticipation Effects

One concern about our experimental design is that, since control-group participants were offered the option to obtain a skills certification 12 months after the program finished, they may have postponed alternative investments while waiting, such as enrollment in other certification programs.

Table B.5 reports the number of eligible participants in the *Skills Pay Off* program before 2024 (2021–2023) who were not beneficiaries but obtained a skills certification within 12 months after each wave ended. Before the 2024 wave of interest, there were 469 eligible but non-certified participants, of whom 20 obtained a skills certification (4.26%). The share of participants in the 2024 wave who obtained a certification after the program ended was 5.26%. The difference between the two percentages is not statistically significant (*p-value* = 0.419), suggesting that participants in the 2024 control group exhibit the same enrollment rate as previous non-certified eligible participants of the program.

[4]Data on use of the Public Employment Service are not available. However, the service is publicly accessible, and all participants were informed of its existence at registration.

Figure B.4 shows the share of control-group participants in the 2024 wave who were enrolled in a certification program at 3, 6, and 12 months after the program ended. First, these self-reported enrollment rates match the official data reported in Table B.5. Enrollment among locals remains at 4% at 3, 6, and 12 months, and we cannot reject the null hypothesis that the rates are equal across the three points. Migrants exhibit an enrollment rate of 7% at 3 months and 4% at 12 months, which may be explained by attrition between surveys. We also cannot reject the null hypothesis for this group, even when using a balanced-panel test.

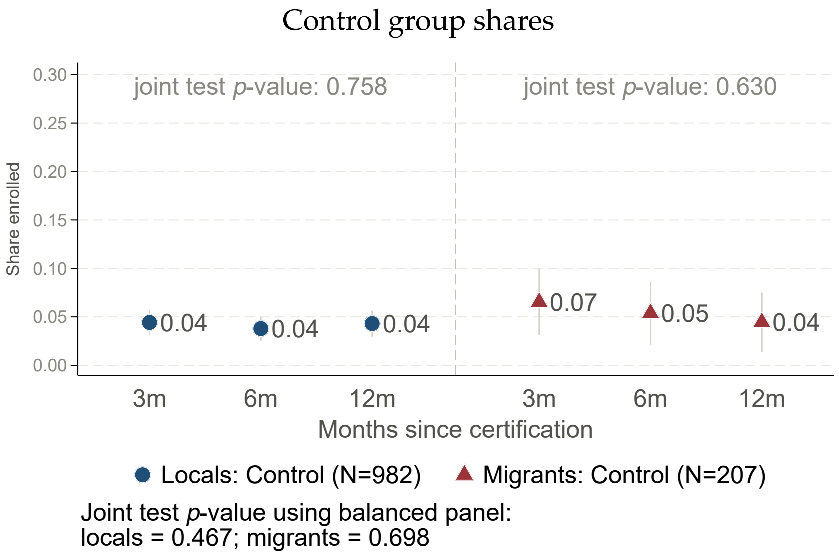
These results provide no evidence that control-group participants postponed alternative certification investments relative to previous cohorts or that enrollment increased as promised program access approached.

Appendix Table B.5: Subsequent Certification

	2021-2023	2024
Eligible but not certified	469	1,376
Certified	20	71
Certification rate	4.26%	5.16%
Difference (pp)	0.90	
p-value	0.419	

Note: The first row shows eligible but non-certified participants of the *Skills Pay Off* program in 2021-2023 and 2024. Difference between certification rates and *p*-value come from a linear probability model on a certification dummy against a 2024-wave dummy with robust standard errors.

Appendix Figure B.4: Control Group Enrollment in Certification Programs by Follow-up Wave



Notes: The figure shows the control group's share enrolled in a certification program at the 3-, 6-, and 12-month follow-ups, separately for locals (N=982) and migrants (N=207). Shares are means from a regression of the outcome on survey-wave indicators and competence fixed effects. Bars are 95% confidence intervals, with standard errors clustered at the individual level. The joint *p*-value tests the equality of the control group's enrollment shares across the three waves.

C Outcomes Definitions

Table C.1 reports the outcome measures used in the paper. For each outcome, the table gives the variable name, its definition, and whether it was included in the pre-analysis plan (PAP). Some reported outcomes are constructed from pre-specified variables to better align the analysis with the paper's research questions. Occupation and sector variables are used to construct measures of occupation change and sector change. Occupation is also used to construct the skill-downgrading measure. Inactivity is derived from employment status, and full-time work is derived from weekly hours worked.

There are three outcomes in the pre-analysis plan that we intended to collect and use but do not include in the analysis. First, we do not report maximum expected salary because baseline responses contained implausible values, raising concerns about measurement reliability. Second, we do not use perceived program impact in the main analysis because it was collected only for treated participants in the first follow-up. We report only the treated-group average for descriptive purposes. Third, we do not report time to first job offer because this item was not included in the follow-up questionnaires. The final survey instrument prioritized realized labor-market outcomes and contemporaneous job-search measures, given survey-length constraints and concerns about recall error in the timing of job offers.

Appendix Table C.1: Variable Definitions

Category	Variable	In PAP	Definition
Labor-Market	Employment status	Yes	Indicator equal to one if the respondent reports being employed in the reference week.
	Inactivity	No	Indicator equal to one if the respondent is neither employed nor actively searching for a job.
	Weekly hours worked	Yes	Self-reported weekly hours worked in the main job; set to zero for non-employed respondents.
	Full-time work	No	Indicator equal to one if the respondent reports working seven or more hours per day.
	Log monthly wage	Yes	Log of monthly labor earnings from the main job in USD, computed as log(wage + 1) with zero earnings imputed for non-employed respondents.
	Formal employment	Yes	Indicator equal to one if the respondent is employed and contributes to the pension system.
	Skill downgrade	Yes	Indicator equal to one if a respondent with post-secondary education is employed in an occupation at skill level 1 or 2 of the Colombian Classification of Occupations (CUOC).
Job Search	Search intensity	No	Self-reported weekly hours spent on job-search activities by the unemployed.
	Unemployment duration	Yes	Self-reported duration of the current job-search spell in months, top-coded at five or more.
	Log reservation wage	Yes	Natural logarithm of the monthly reservation wage in USD. The minimum monthly salary at which the respondent would accept a job (unemployed) or switch jobs (employed).

continued on next page

Table C.1 continued from previous page

Category	Variable	In PAP	Definition
	Maximum expected salary	Yes	Self-reported maximum monthly salary the respondent expects to earn given their qualifications, in USD. Collected only at baseline.
Education and Training	Enrollment in additional education (any)	Yes	Indicator equal to one if the respondent reports enrollment in or completion of any additional educational activity in the prior three months.
	Enrollment in certification program	Yes	Indicator equal to one if the respondent reports being enrolled in a different skill-certification program.
	Enrollment in training course	Yes	Indicator equal to one if the respondent reports being enrolled in a short training course or diploma program.
	Enrollment in degree program	Yes	Indicator equal to one if the respondent reports enrollment in a professional, technological, or technical career program.
Firm Characteristics and Job Mobility	Solo / self-employed	No	Indicator equal to one if the respondent works alone in a single-person firm.
	Small firm	Yes	Indicator equal to one if the respondent is employed at a firm with 2–10 workers.
	Medium/large firm	Yes	Indicator equal to one if the respondent is employed at a firm with 11 or more workers.
	Economic sector of employer	Yes	Indicators for the broad economic sector of the respondent's main employer, approximating the 1-digit level of the industry classification (CIIU).
	Changed sector	No	Indicator equal to one if the sector of the respondent's employer at the follow-up differs from the sector reported at baseline (conditional on being employed at both points).
	Occupation type	Yes	Indicators for the broad occupation groups (1-digit CUOC) of the respondent's main job.
	Changed job	No	Indicator equal to one if a respondent employed at both baseline and the follow-up holds a job that began after baseline.
	Changed Occupation	No	Indicator equal to one if the broad occupation group (1-digit CUOC) of the main job differs between baseline and follow-up, conditional on being employed at both.
	Got a job offer	Yes	Indicator equal to one if the respondent reports having received at least one job offer in the reference period.
	Number of job offers	Yes	Self-reported count of job offers received in the reference period; set to zero if the respondent reports no offers.
	Time to first job offer	Yes	Time elapsed between starting the job search and the first job offer.

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Table C.1 continued from previous page

Category	Variable	In PAP	Definition
Job Satisfaction and Wellbeing	Wants a different job	Yes	Indicator equal to one if an employed respondent reports wanting to change jobs.
	Job satisfaction	Yes	Indicator equal to one if the respondent reports being "satisfied" or "very satisfied" with their current job, on a five-point satisfaction scale. Collected at follow-ups 2 and 3.
	Anxiety / nervousness	Yes	Indicator equal to one if the respondent's GAD-7 total score indicates moderate or severe anxiety (score $\geq$ 10 out of 21).
	GAD-7 anxiety score	Yes	Total score on the Generalized Anxiety Disorder 7-item scale, ranging from 0 to 21, with higher values indicating more severe symptoms.
	Skipping a meal	Yes	Indicator equal to one if the respondent reports having skipped a meal due to lack of economic resources "sometimes" or "often" in the reference period.
Stated Preferences about Program	WTP for certification (USD midpoint)	Yes	Willingness-to-pay measure in USD, constructed as the midpoint of the reported willingness-to-pay interval. Collected only at follow-up 1 and 3.
	Willingness to pay for certification	Yes	Indicator equal to one if the respondent reports being willing to pay at least 50,000 COP (approximately 12 USD) for the skill certification. Collected only in follow-up 1 and 3.
	Willingness to participate without subsidies	Yes	Indicator equal to one if the respondent reports being "willing" or "very willing" to have participated in the program without financial subsidies. Collected only at follow-up 1.
	Perceived program impact	Yes	Indicator equal to one if the respondent perceives the program as having helped them improve their employment situation "moderately" or "a lot." Collected only at follow-up 1 only for treated participants.

Notes: This table lists the outcomes pre-specified in the PAP and their definitions. Monetary outcomes are reported in USD using the average official Colombian peso exchange rate in 2024 of 4052.27 COP/USD.

D Balance

Appendix Table D.1: Balance within Locals and Migrants

Variable	Locals			Migrants			Total		
	Treatment (1)	Control (2)	*p-value* (3)	Treatment (4)	Control (5)	*p-value* (6)	Locals (7)	Migrants (8)	*p-value* (9)
Panel A: General characteristics									
Demographics									
Age	42.671	42.350	0.819	35.964	36.073	0.947	42.466	36.017	0.000
Male	0.119	0.118	0.908	0.176	0.137	0.243	0.118	0.157	0.361
Caregiver (young or elderly)	0.526	0.551	0.111	0.464	0.449	0.811	0.542	0.457	0.441
No children under 5 years *	0.615	0.612	0.967	0.504	0.530	0.554	0.613	0.517	0.001
One child under 5 years *	0.233	0.223	0.592	0.348	0.338	0.804	0.227	0.343	0.000
More than one child under 5 years *	0.152	0.165	0.500	0.148	0.132	0.606	0.161	0.140	0.145
Head of household	0.539	0.533	0.925	0.628	0.679	0.207	0.535	0.653	0.000
Identifies as LGBTIQ	0.041	0.039	0.829	0.064	0.081	0.500	0.040	0.073	0.006
Disability (mental or physical)	0.058	0.057	0.997	0.028	0.060	0.104	0.057	0.043	0.233
Victim of conflict	0.448	0.479	0.283	0.021	0.031	0.524	0.468	0.026	0.000
Ethnic minority	0.326	0.369	0.100	0.032	0.030	0.890	0.353	0.031	0.000
Eligible for government assistance	0.618	0.614	0.705	0.548	0.568	0.641	0.616	0.558	0.015
Receives government or NGO assistance	0.140	0.140	0.781	0.012	0.013	0.913	0.140	0.012	0.000
Education									
Primary or less	0.198	0.187	0.551	0.080	0.051	0.209	0.191	0.066	0.000
Secondary	0.503	0.482	0.356	0.608	0.611	0.979	0.490	0.610	0.000
Technical education	0.217	0.246	0.139	0.136	0.150	0.643	0.236	0.143	0.000
University or postgraduate	0.082	0.084	0.783	0.176	0.188	0.721	0.083	0.182	0.000
Labor									
Employed	0.324	0.332	0.929	0.540	0.504	0.402	0.329	0.523	0.000
Pooled monthly salary (USD)	105.848	97.962	0.380	109.555	120.080	0.514	100.804	114.713	0.695
Less than 1 year of experience	0.145	0.127	0.347	0.100	0.094	0.850	0.134	0.097	0.097
1-2 years of experience	0.185	0.183	0.820	0.260	0.282	0.619	0.184	0.271	0.008
3-5 years of experience	0.202	0.193	0.506	0.268	0.295	0.489	0.196	0.281	0.006
6-10 years of experience	0.155	0.169	0.465	0.176	0.141	0.305	0.164	0.159	0.592
More than 10 years of experience	0.312	0.328	0.380	0.196	0.188	0.788	0.322	0.192	0.000
N (Sample Size)	605	1,071		250	234		1,676	484	
Panel B: Employed									
Monthly salary (USD)	140.053	142.248	0.828	138.463	159.594	0.470	141.467	148.319	0.781
Overall skill downgrading *	0.218	0.234	0.658	0.247	0.246	0.997	0.228	0.247	0.641
Skill downgrading among professionals *	0.053	0.024	0.230	0.111	0.130	0.662	0.035	0.120	0.003
Have written employment contract	0.138	0.134	0.998	0.086	0.085	0.890	0.136	0.086	0.055
Contributes to pension fund	0.179	0.190	0.686	0.078	0.094	0.660	0.186	0.085	0.000
Full-time job	0.510	0.451	0.186	0.437	0.500	0.391	0.472	0.466	0.614
Half-time job	0.301	0.321	0.645	0.296	0.288	0.614	0.314	0.292	0.440
Quarter-time job	0.189	0.228	0.265	0.267	0.212	0.621	0.214	0.241	0.157
N (Sample Size)	196	356		135	118		552	253	
Panel C: Unemployed									
Last job monthly salary (USD)	84.292	69.115	0.248	68.476	74.812	0.794	74.629	71.772	0.337
Reservation Wage (USD) *	347.416	355.453	0.533	324.837	361.976	0.137	352.661	342.041	0.430
Never been employed	0.032	0.011	0.040	0.026	0.017	0.677	0.019	0.022	0.478
Actively seeking job (last 4 weeks)	0.343	0.302	0.121	0.365	0.422	0.400	0.317	0.394	0.186
Job search intensity: less than 5 hrs/day	0.600	0.581	0.747	0.667	0.490	0.204	0.589	0.571	0.736
Job search intensity: 5-10 hrs/day	0.314	0.330	0.765	0.262	0.367	0.745	0.324	0.319	0.966
Job search intensity: Over 10 hrs/day	0.086	0.088	0.946	0.071	0.143	0.130	0.087	0.110	0.618
N (Sample Size)	409	715		115	116		1,124	231	

Notes: Panel A covers all registered participants. Panels B and C restrict the sample to those employed and unemployed at baseline, respectively. Columns (1)–(2) and (4)–(5) report means by treatment assignment within locals and migrants. Columns (7)–(8) report means pooling treated and controls within each group. *P-values* in columns (3) and (6) test the null hypothesis of equal means between treatment and control, using an OLS regression of each variable on a treatment indicator with competency fixed effects and robust standard errors. *P-values* in column (9) test the null hypothesis of equal means between locals and migrants, estimated with a migrant indicator. Pooled salary combines the current monthly salary of employed individuals with the last monthly salary of unemployed individuals. Variables marked with * were collected in a complementary baseline survey administered three months after enrollment but before certification. Rows with different sample sizes: Skill Downgrading: N = 133, 209, 81, 69, 342, 150 and Reservation Wage: N = 262, 492, 73, 63, 754, 136.

E Attrition Tests

This appendix documents survey response across follow-up rounds and tests whether attrition differs by treatment status. Table E.1 reports the number and share of participants who completed each survey round, answered all rounds, or responded only to the registration form, disaggregated by migrant background and treatment assignment. Table E.2 then tests for differential attrition by estimating, separately for locals and migrants and for each wave, a linear probability model of non-response on the treatment indicator with competency fixed effects.

Appendix Table E.1: Attrition and Survey Completion Rates across Follow-up Rounds

	Total	Comp. Baseline		3 Months		6 Months		12 Months		Answered all rounds		Registration only	
	(n)	(n)	(%)	(n)	(%)	(n)	(%)	(n)	(%)	(n)	(%)	(n)	(%)
Panel A: Locals													
Treatment	605	540	89.26	564	93.22	537	88.76	515	85.12	492	81.32	28	4.63
Control	1071	948	88.52	974	90.94	933	87.11	881	82.26	835	77.96	52	4.86
Panel B: Migrants													
Treatment	250	236	94.40	238	95.20	222	88.80	207	82.80	201	80.40	4	1.60
Control	234	216	92.31	204	87.18	193	82.48	179	76.50	174	74.36	10	4.27
N	2160	1940	89.81	1980	91.67	1885	87.27	1782	82.50	1702	78.80	94	4.35

Notes: This table reports response rates at baseline and all follow-up rounds, disaggregated by nationality (locals vs. migrants) and treatment assignment (treatment vs. control). Columns show the number and percentage of participants who completed each survey, answered all surveys, or responded only to the registration form (Reg. Only). Percentages are calculated relative to the original registration sample.

Appendix Table E.2: Differential Attrition by Follow-up Wave

	Comp. Baseline		3 months		6 months		12 months	
	Local	Migrant	Local	Migrant	Local	Migrant	Local	Migrant
Treatment	-0.007	-0.020	-0.025*	-0.079***	-0.018	-0.063**	-0.030	-0.065*
	(0.016)	(0.023)	(0.014)	(0.026)	(0.017)	(0.032)	(0.019)	(0.036)
Constant	0.115***	0.077***	0.091***	0.128***	0.129***	0.175***	0.178***	0.236***
	(0.010)	(0.017)	(0.009)	(0.022)	(0.010)	(0.024)	(0.012)	(0.027)
Observations	1676	484	1676	484	1676	484	1676	484

Notes: Each column reports estimates from a linear probability model where the dependent variable equals one if the participant did not complete the survey in the corresponding wave and zero otherwise. *Treated* is an indicator for assignment to the certification program. All specifications include competence fixed effects. Regressions are estimated separately by migration status. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

F Effects on Skill Downgrading

This appendix provides details on the construction and validation of the skill-downgrading measure and further examines the corresponding treatment effects. Because measuring skill downgrading requires translating respondents job titles and task descriptions into occupational codes and associated skill requirements, we first describe the classification procedure and assess its accuracy using independent human annotations. We then decompose labor-market status into non-employment, employment in a matched occupation, and employment in a downgraded occupation.

F.1 Details of the Skill-Downgrading Measures

We asked each participant to report their occupation title and describe the tasks performed in that job. Based on this information, surveyors were instructed to assign an occupation code using the Colombian Classification of Occupations (CUOC). Codes were assigned using the categories in Table F.1. Occupation codes 5 (Service and Sales) and 9 (Elementary Occupations) were further disaggregated into 3-digit subgroups because they contained the largest number of respondents at baseline, allowing for a more precise manual classification.

Figure F.1 illustrates the procedure used to match each job description to a CUOC occupation code. First, we provide the job title and task description to the OpenAI o4-mini model and ask the model to assign a 5-digit CUOC code.[5] To improve precision, we restrict the set of possible 5-digit codes using the occupation category manually assigned during the survey. We also provide the full set of 3-digit codes to allow for potential surveyor misclassification. When the procedure identifies a mismatch between the manually assigned category and the model's classification, we repeat the assignment using the corrected occupation category to obtain the final 5-digit code.

Using this classification, we measure skill downgrading as an indicator equal to one for individuals with post-secondary education who are employed in occupations requiring fewer qualifications than their educational level, and zero otherwise. This measure relies on the CUOC mapping of each occupation to one of four skill levels, defined by the complexity of the tasks and the qualifications required to perform them. Skill levels 1 and 2 typically correspond to secondary education or less, while levels 3 and 4 correspond to post-secondary education. A consequence of this mapping is that only participants with post-secondary education can be classified as downgraded.

As an alternative measure, we use the observed educational composition of occupations in Colombia, following Leuven and Oosterbeek (2011). This measure equals one for participants with post-secondary education who are employed in occupations in which the modal worker has secondary education or less, and zero otherwise. We compute the modal education level within each occupation using Colombia's *Gran Encuesta Integrada de Hogares* (GEIH), the country's main household labor-force survey.[6]

[5]The full prompt provided to the model is reproduced in Figure F.2.

[6]To ensure comparability between the GEIH and our survey data, we map the more aggregated GEIH occupation codes (based on ISCO-68) into our CUOC-based classification (ISCO-08). Given the greater aggregation of the GEIH coding, multiple CUOC occupation codes map into a single GEIH code. We restrict the GEIH sample to employed individuals of working age (15–64), yielding 1,682,333 observations.

We use the 2010–2014 waves, prior to the large Venezuelan inflow into Colombia that began in 2015, so that modal educational attainment within occupations is measured before the migration shock.[7]

F.2 Evaluation of Model Classification

To assess the quality of the matching procedure, two research assistants independently evaluated a random sample of 200 model-generated matches. For each observation, they assigned one of three labels: (1) Good, (2) Partial, or (3) No match. A match was labeled "Good" when the assigned occupation was the best available classification. It was labeled "Partial" when the assigned occupation was related to the job description but a more precise alternative existed, such as a heavy-truck driver classified as a bus driver. It was labeled "No match" when the assigned occupation did not reflect the job description. For observations labeled "Partial" or "No match", the research assistants also assigned a manual 5-digit CUOC code.

Table F.2 reports the results of the validation exercise. Both human annotators agreed on 87% of classifications. We then consolidated their annotations using a conservative adjudication rule: when labels differed, the stricter classification prevailed, with "No match" ranked below "Partial" match and "Partial" match ranked below "Good" match. Under this rule, 76% of model-generated matches were classified as "Good" (Wilson CI: [69.6%, 81.4%]), and 95% were classified as either "Good" or "Partial" (Wilson CI: [91.0%, 97.3%]).[8]

To assess the sensitivity of the *skill-downgrading* measure, we use the original model-generated codes for observations labeled "Good" and the research assistants' manual annotations for observations labeled "Partial" or "No match". When the research assistants assigned different 5-digit occupation codes, they deliberated and agreed on a final code. We then map these human-validated occupation codes to skill levels using the classification provided by the Colombian statistical office (DANE).[9]

Our *skill-downgrading* estimates would be affected by occupation-matching errors only if the model-generated and human-assigned occupations imply different skill levels. Table F.4 reports this comparison. We group occupations into two skill categories: those requiring secondary education or less (levels 1 and 2) and those requiring post-secondary education (levels 3 and 4) (ILO, 2012). Agreement between the human and model classifications across these two groups is 97.5%. A McNemar test yields a *p-value* of 0.375, indicating no statistically significant difference between the two classifications. This suggests that even when the model does not identify the exact occupation, it usually assigns an occupation with the same broad skill level. This

[7] This modal approach has been used to characterize occupational downgrading among Venezuelan migrants in Colombia by Lebow (2023), who reports that 52 percent of migrants in Colombia work in occupations where the modal worker is less educated than they are.

[8] Table F.3 reports the classification of model-generated matches under this adjudication rule.

[9] Skill levels are defined by the International Labor Organization (ILO) according to the complexity of tasks performed in each occupation. Skill Level 1 is associated with simple, routine physical or manual tasks and typically requires primary education. Skill Level 2 covers tasks such as operating machinery, driving vehicles, and performing maintenance and repair, which typically require secondary education. Skill Level 3 encompasses complex technical and practical tasks that require specialized knowledge in a particular field. Skill Level 4 involves tasks requiring advanced problem-solving, decision-making, and creativity, usually associated with a degree that takes three to six years to complete.

may partly reflect the algorithm's use of the manually assigned occupation category to restrict the candidate set, making the *skill-downgrading* measure less sensitive to fine-grained occupation-coding errors.

F.3 Effects on skill downgrading

Since our skill-downgrading measure is set to zero for non-employed respondents, the total effect can arise either from participants moving into matched occupations or from participants becoming non-employed. We therefore decompose the total effect across columns (1) to (3) of Table F.5. The dependent variable in each column is a mutually exclusive dummy equal to one if the respondent is (1) not employed, i.e., either inactive or searching for a job; (2) employed in a matched occupation; or (3) employed in a downgraded occupation, where downgrading is defined according to each measure.

Panel C shows that treatment reduces the probability that a migrant is employed in an occupation below their skill level by 8.1 percentage points, relative to a control-group mean of 25 percent. This represents a reduction of approximately one-third relative to the control group, although we cannot reject the null hypothesis of no effect after applying the multiple-hypothesis adjustment, as shown in Table 2.

For migrants, this reduction consists of a 4.8-percentage-point increase in employment in a matched occupation and a 3.3-percentage-point increase in non-employment. Thus, improved occupational matching accounts for approximately 59 percent of the observed reduction in skill downgrading, while lower employment accounts for the remaining 41 percent. This pattern contrasts with the results for locals in Panel B. Among locals, treatment reduces non-employment by 5.7 percentage points and increases employment in a matched occupation by 5.3 percentage points, while leaving employment in downgraded occupations virtually unchanged.

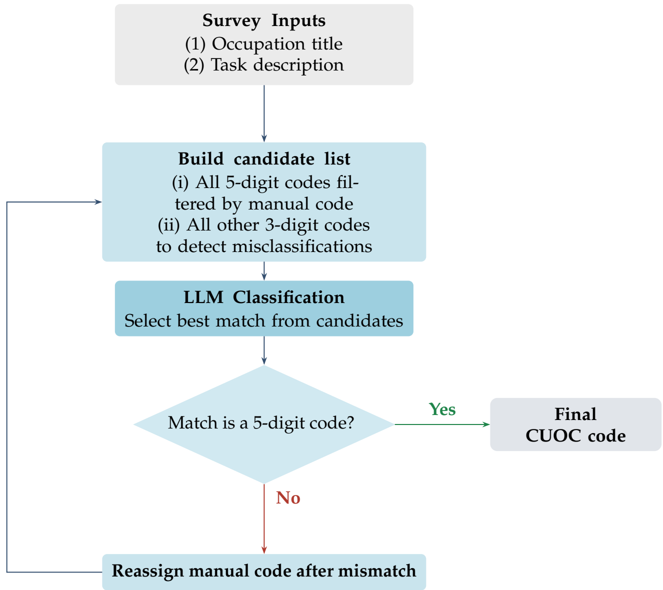
Finally, columns (4) and (5) reproduce the analysis using the alternative measure described in Section F.1. The estimates closely mirror those obtained with the standard measure. Among locals, both measures show an increase of approximately 5.4 percentage points in matched employment and virtually no change in downgraded employment. Among migrants, the alternative measure yields a 6.1-percentage-point reduction in downgraded employment and a 2.8-percentage-point increase in matched employment, compared with reductions of 8.1 and 4.8 percentage points, respectively, under the standard measure.

Appendix Table F.1: Occupation Categories Used in the Surveys

CUOC Code	Occupation Name
Panel A: Major Groups (1-digit)	
0	Armed Forces Occupations
1	Managers
2	Professionals
3	Technicians and Associate Professionals
4	Clerical Support Workers
6	Skilled Agricultural, Forestry and Fishery Workers
7	Craft and Related Trades Workers
8	Plant and Machine Operators and Assemblers
Panel B: Occupation Sub-Groups (3-digit)	
511	Travel Attendants and Travel Stewards
512	Cooks
513	Waiters and Bartenders
514	Hairdressers, Beauticians and Related Workers
515	Building and Housekeeping Supervisors
516	Other Personal Services Workers
521	Street and Market Salespersons
522	Shop Salespersons
523	Cashiers and Ticket Clerks
524	Other Sales Workers
531	Child Care Workers and Teachers' Aides
532	Personal Care Workers in Health Services
541	Protective Services Workers
911	Domestic, Hotel and Office Cleaners and Helpers
912	Vehicle, Window, Laundry and Other Hand Cleaning Workers
921	Agricultural, Forestry and Fishery Labourers
931	Mining and Construction Labourers
932	Manufacturing Labourers
933	Transport and Storage Labourers
941	Food Preparation Assistants
951	Street and Related Service Workers
952	Street Vendors (Excluding Food)
961	Refuse Workers
962	Other Elementary Workers

Notes: CUOC codes correspond to the Colombian Unified Classification of Occupations (ISCO-08 adaptation). Panel A reports 1-digit major groups used. Panel B reports 3-digit sub-major groups for the most frequently reported occupations at baseline.

Appendix Figure F.1: Two-Step LLM Classification Procedure



Notes: This figure describes the two-step procedure used to classify each participant's occupation into a 5-digit CUOC code.

Appendix Figure F.2: LLM Prompt for Occupation Classification

System message

You are a helpful assistant with deep knowledge of Latin-American labor markets. Avoid hallucinations. Also, you will have a list of occupation tuples, take that as ground truth for the query. Reply ONLY with the chosen tuple.

List of occupation tuples: «Input list of occupation tuples».

User prompt

You have three inputs in Spanish:
1. An occupation title: «Input occupation title».
2. An occupation description: «Input occupation description».
3. A list of occupation tuples: [Provided in the system message for this request]. Each tuple has the form (`"code"`, `"occupation name"`).

Task:
From the provided list of standardized occupation tuples, choose the one that best matches the given occupation title and description. Do not include any additional text, context, or chat.

Notes: Prompt used within the OpenAI API (o4-mini model) to classify each participant's occupation into a 5-digit CUOC code. The system message is shared across all API calls within the same occupation family. Inputs replaced before each call include: «Input list of occupation tuples», which contains the set of candidate CUOC codes; «Input occupation title», the job title reported by the participant; and «Input occupation description», the description of tasks reported by the participant.

Appendix Table F.2: Annotator Agreement on CUOC Codes

	Annotator 2		
Annotator 1	Good Match	Partial Match	No Match
Good Match	152	19	0
Partial Match	6	13	1
No Match	0	0	9

Notes: Annotator 1 versus Annotator 2 response distribution. Percentage of agreement of 87%.

Appendix Table F.3: Accuracy of LLM-Assigned CUOC Codes

Evaluation	N	%	95% CI
Good Match	152	76.0	[69.6, 81.4]
Partial Match	38	19.0	[14.2, 25.0]
Good + Partial Match	190	95.0	[91.0, 97.3]
No Match	10	5.0	[2.7, 9.0]
Total	200	100.0	

Notes: Accuracy using the most strict label between two independent annotators. 95% Wilson confidence intervals in brackets.

Appendix Table F.4: Skill-Level Agreement Between Model and Human classification

	Human Label		
Model response	Secondary or less	Post-Secondary	Total
Secondary or less (Levels 1-2)	168	4	172
Post-Secondary (Levels 3-4)	1	27	28
Total	169	31	200

Notes: Skill levels from CUOC classification are grouped as Secondary Education (Skill Levels 1–2) and Post-Secondary (Skill Levels 3–4). Percentage of agreement between model and human labels is 97.5%. McNemar's test *p-value* = 0.375.

Appendix Table F.5: Effects of Access to Skills Certification on Employment Status by Occupational Match

Intention To Treat Effects estimated using Equation (1)

		Standard Measure		Alternative Measure	
	Not employed (1)	Employed, matched occupation (2)	Employed, downgraded occupation (3)	Employed, matched occupation (4)	Employed, downgraded occupation (5)
Panel A: Full Sample					
Treatment	-0.036**	0.052***	-0.016	0.048**	-0.012
	(0.018)	(0.019)	(0.015)	(0.019)	(0.013)
Mean Control	0.470	0.374	0.156	0.403	0.127
Observations	5,647	5,647	5,647	5,647	5,647
Panel B: Locals					
Treatment	-0.057***	0.053**	0.004	0.054**	0.003
	(0.021)	(0.021)	(0.016)	(0.021)	(0.014)
Mean Control	0.506	0.357	0.136	0.384	0.109
Observations	4,404	4,404	4,404	4,404	4,404
Panel C: Migrants					
Treatment	0.033	0.048	-0.081**	0.028	-0.061*
	(0.035)	(0.041)	(0.035)	(0.040)	(0.033)
Mean Control	0.295	0.455	0.250	0.493	0.212
Observations	1,243	1,243	1,243	1,243	1,243
Competence FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Notes: The outcomes decompose labor-market status at follow-up into three mutually exclusive categories: not employed, employed in an occupation matching the worker's educational attainment, and employed in a downgraded occupation. Columns (2)–(3) use the main skill-downgrade classification (Standard Measure) and columns (4)–(5) the alternative classification. Standard errors (in parentheses) are clustered at the individual level. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

G Heterogeneity Tests

We examine whether the contrast between local and migrant treatment effects reflects differences in sample composition rather than migrant status itself, and report formal tests of heterogeneity. We proceed in two steps.

G.1 Re-weighted Estimations

Appendix Table G.1: Covariate balance before and after propensity score reweighting

	Locals (1)	Migrants (2)	Migrants (normalized weights) (3)	p-value (2) − (1)	p-value (3) − (1)
Panel A: Demographics					
Age	42.47	36.02	41.09	0.00	0.25
Male	0.12	0.16	0.13	0.36	0.69
Household head	0.54	0.65	0.60	0.00	0.09
Has children	0.28	0.42	0.29	0.00	0.66
Caregiver	0.54	0.46	0.49	0.44	0.21
Sisben	0.62	0.56	0.63	0.01	0.65
Panel B: Education					
Primary or less	0.19	0.07	0.12	0.00	0.01
Secondary	0.49	0.61	0.49	0.00	1.00
Tertiary	0.32	0.32	0.39	0.34	0.06
Panel C: Labor market					
Employed at baseline	0.33	0.52	0.35	0.00	0.56
Wage (USD), employed	141.47	148.32	154.18	0.78	0.27
Log wage, employed	12.67	11.82	12.78	0.00	0.40
Reservation Wage (USD)	367.79	356.96	357.41	0.06	0.30
Panel D: Experience					
Less than 1 year	0.13	0.10	0.09	0.10	0.01
1-2 years	0.18	0.27	0.18	0.01	0.76
3-5 years	0.20	0.28	0.20	0.01	0.87
6-10 years	0.16	0.16	0.18	0.59	0.53
Panel E: Competency level					
Recycling	0.07	0.06	0.06	0.60	0.80
Health Services	0.45	0.14	0.41	0.00	0.31
Food Manipulation	0.27	0.52	0.29	0.00	0.43
Design and tailoring	0.09	0.08	0.09	0.65	0.94
Food Preparation	0.12	0.18	0.14	0.00	0.37
Observations	1,676	484	484		

Notes: This table reports baseline characteristics for locals and migrants. Column (3) reports weighted means for migrants after re-weighting using normalized inverse propensity-score weights, so the distribution of observable characteristics among migrants matches that of locals. Columns (4) and (5) report p-values from regressions of each covariate on an indicator for migrant status, estimated unweighted and with reweighting, respectively. In the reweighted specification, locals retain a weight of one and only the migrant sub-sample is reweighted by the normalized inverse propensity-score. The wage and log wage rows are computed among participants employed at baseline. All regressions include competency fixed effects and use robust standard errors, except for the competency share variables in Panel E, where the fixed effects coincide with the outcome and are therefore omitted.

First, we reweight the migrant sample using normalized inverse propensity-score weights so that its distribution of observed baseline characteristics matches that of locals (Busso et al., 2014); Table G.1 reports covariate balance before and after reweighting, and Table G.2 reports the corresponding treatment-effect estimates. Second, we estimate the pooled specification with treatment interacted with migrant status in Table G.3, which provides direct tests of equality between the two groups' effects, and we estimate treatment effects within subgroups defined by baseline labor-force status, separately for locals and migrants (see Figure G.1).

Appendix Table G.2: Effects of Access to Skills Certification on Labor Market Outcomes

Migrants Reweighted to Local Baseline Composition
Intention To Treat Effects Estimated using Equation (1)

			Outcomes Replaced by Zero if Non-Employed				
	Employed	Inactive	Log Wage	Formal emp.	Weekly working hours	Full time (7+ hrs)	Skill Downgrade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Sample							
Treatment	0.037*	-0.027	0.476	-0.012	0.652	0.025	-0.015
	(0.021)	(0.017)	(0.294)	(0.016)	(0.931)	(0.020)	(0.019)
Observations	5,647	5,647	5,634	5,647	5,647	5,647	5,647
Mean Control	0.521	0.248	6.629	0.133	14.50	0.250	0.163
Panel B: Locals							
Treatment	0.057***	-0.035*	0.741***	-0.008	2.016**	0.037**	0.004
	(0.021)	(0.018)	(0.283)	(0.015)	(0.857)	(0.017)	(0.016)
Observations	4,404	4,404	4,392	4,404	4,404	4,404	4,404
Mean Control	0.494	0.264	6.199	0.136	12.88	0.221	0.136
Panel C: Migrants							
Treatment	-0.023	-0.005	-0.332	-0.018	-3.832	-0.007	-0.067
	(0.062)	(0.047)	(0.869)	(0.041)	(2.724)	(0.066)	(0.066)
Observations	1,243	1,243	1,242	1,243	1,243	1,243	1,243
Mean Control	0.646	0.177	8.589	0.121	21.85	0.382	0.285
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are obtained by weighted least squares using normalized inverse-odds that align the baseline covariate distribution of migrants to that of locals. Weights equal $(1-\hat{p})/\hat{p}$ for migrants and one for locals, where $\hat{p}$ is the predicted probability of being a migrant from a baseline logit model on age and its square, sex, household headship, number of dependent children, caregiving status, education, Sisben status, prior employment, prior wage and log wage, work experience, and competence. Migrant weights are rescaled to unit mean so that the weighted and unweighted migrant sample sizes coincide. Mean Control reports the weighted control-group mean of the outcome in the corresponding estimation sample. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

G.2 Heterogeneity by Migrant Status and Baseline Labor Status

Appendix Table G.3: Effects of Access to Skills Certification on Labor Market Outcomes

Intention To Treat Estimated using Equation (1)–Heterogeneity by Migrant Status

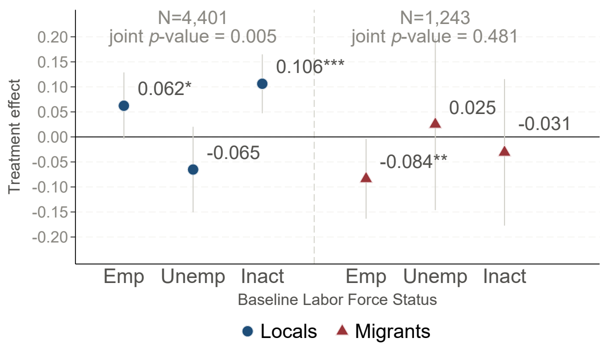
			Outcomes Replaced by Zero if Non-Employed				
	Employed (1)	Inactive (2)	Log Wage (3)	Formal emp. (4)	Weekly working hours (5)	Full time (7+ hrs) (6)	Skill Downgrade (7)
Treatment	0.057***	-0.035*	0.740***	-0.008	2.033**	0.037**	0.003
	(0.021)	(0.018)	(0.283)	(0.015)	(0.858)	(0.017)	(0.016)
Migrant	0.190***	-0.118***	2.789***	-0.046**	10.126***	0.187***	0.114***
	(0.029)	(0.021)	(0.401)	(0.021)	(1.502)	(0.030)	(0.030)
Treatment × Migrant	-0.089**	0.047	-1.070*	0.031	-3.476*	-0.057	-0.083**
	(0.041)	(0.030)	(0.562)	(0.029)	(2.023)	(0.041)	(0.039)
P-value (T × Mig.)	0.028	0.120	0.057	0.297	0.086	0.165	0.034
Observations	5,647	5,647	5,634	5,647	5,647	5,647	5,647
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the pooled ITT specification of Table 2 estimated on the full sample with the treatment indicator interacted with migrant status. Treatment reports the ITT effect for locals. Migrant reports the baseline difference between migrants and locals. Treatment × Migrant reports the differential effect for migrants relative to locals; and P-value (T × Migrant) reports the p-value of the test that the interaction term equals zero. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

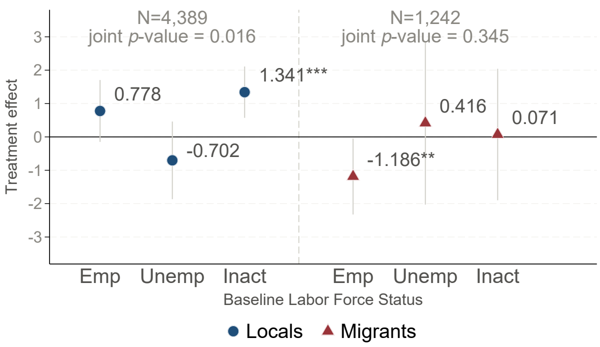
Appendix Figure G.1: Heterogeneous Effects of Access to Skills Certification on Labor Market Outcomes

Intention To Treat Effects estimated using Equation (1) – Heterogeneity by Baseline Labor Force Status

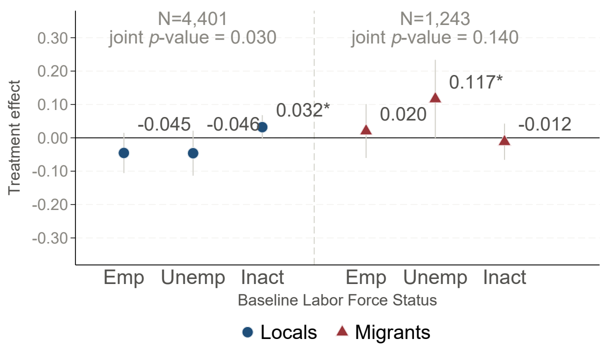
(a) Employment



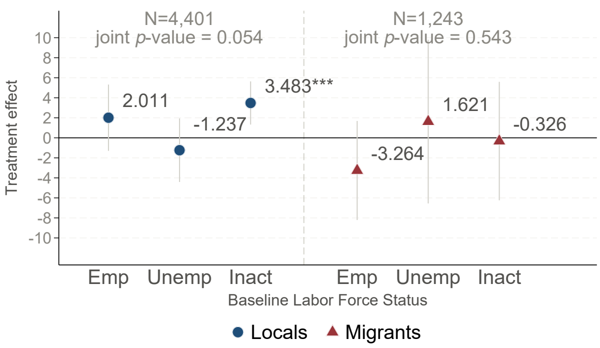
(b) Log Wage



(c) Formal Employment



(d) Weekly Working Hours



Notes: Split-sample ITT estimates of Equation (1) are estimated within baseline subgroups separately for locals and migrants. Subgroups are defined by baseline labor force status: employed individuals (Emp) held a job at baseline; unemployed individuals (Unemp) were not employed but were actively searching for a job; inactive individuals (Inact) were neither employed nor searching. Bars are 95% confidence intervals, with standard errors clustered at the individual level. The joint *p*-value tests the null that treatment effects are equal across the subgroups within each block. Outcomes in panels (b)–(d) are set to zero when the individual is not employed. *** p<0.01, ** p<0.05, * p<0.1.

H Robustness

H.1 Lee Bounds Estimations by Wave and Migrant Status

This appendix assesses whether differential attrition documented could account for the main treatment effects. Following Lee (2009), we bound the ITT estimates by trimming the outcome distribution of the group with excess respondents, so that the bounds cover the range of effects consistent with any pattern of selective non-response.

Appendix Table H.1.1: Effects of Access to Skills Certification (ITT)

Lee Bounds – by follow-up Round using Full Sample

			Outcomes Replaced by Zero if Non-Employed				
	Employed (1)	Inactive (2)	Log Wage (3)	Weekly working hours (4)	Formal emp. (5)	Full time (7+ hrs) (6)	Skill Downgrade (7)
Panel A: 3 months							
Treatment	0.046**	-0.041**	0.667**	2.399**	0.002	0.046**	-0.018
	(0.023)	(0.019)	(0.304)	(0.998)	(0.014)	(0.020)	(0.016)
Observations	1,980	1,980	1,980	1,980	1,980	1,980	1,980
Lower Bound	0.029	-0.072***	0.409	0.073	-0.033***	0.022	-0.051***
Upper Bound	0.066**	-0.034*	0.667**	2.399**	0.007	0.058***	-0.013
Mean Control	0.497	0.244	6.187	13.53	0.113	0.227	0.148
Panel B: 6 months							
Treatment	0.027	-0.012	0.329	0.947	-0.004	-0.000	-0.007
	(0.023)	(0.020)	(0.316)	(1.003)	(0.015)	(0.021)	(0.017)
Observations	1,885	1,885	1,872	1,885	1,885	1,885	1,885
Lower Bound	0.016	-0.034*	0.124	-0.524	-0.030**	-0.020	-0.032*
Upper Bound	0.043*	-0.005	0.329	0.947	-0.000	0.008	-0.002
Mean Control	0.539	0.245	6.945	14.65	0.125	0.264	0.149
Panel C: 12 months							
Treatment	0.035	-0.017	0.464	0.213	-0.000	0.025	-0.024
	(0.024)	(0.020)	(0.323)	(1.052)	(0.017)	(0.022)	(0.018)
Observations	1,782	1,782	1,782	1,782	1,782	1,782	1,782
Lower Bound	0.020	-0.050**	0.129	-1.823**	-0.034**	0.000	-0.055***
Upper Bound	0.059***	-0.009	0.464	0.213	0.005	0.037*	-0.018
Mean Control	0.557	0.232	7.120	16.21	0.147	0.280	0.172
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each panel reports results for one follow-up round (Panel A: 3 months, Panel B: 6 months, Panel C: 12 months): the ITT coefficient of Treatment estimated on that round alone, with robust standard errors in parentheses, and the lower and upper bounds of Lee (2009). Standard errors for the bounds are obtained via bootstrap with 100 replications. Bounds are reported for the primary labor market outcomes only. Specifications include competence fixed effects and control for migrant status. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table H.1.2: Effects of Access to Skills Certification (ITT)

Lee Bounds – by follow-up Round using Sample of Locals

			Outcomes Replaced by Zero if Non-Employed				
	Employed (1)	Inactive (2)	Log Wage (3)	Weekly working hours (4)	Formal emp. (5)	Full time (7+ hrs) (6)	Skill Downgrade (7)
Panel A: 3 months							
Treatment	0.070***	-0.060***	0.938***	3.528***	-0.014	0.071***	0.009
	(0.026)	(0.022)	(0.348)	(1.113)	(0.017)	(0.022)	(0.018)
Observations	1,538	1,538	1,538	1,538	1,538	1,538	1,538
Lower Bound	0.059**	-0.079***	0.706**	1.659	-0.035**	0.054***	-0.011
Upper Bound	0.082***	-0.055**	0.938***	3.528***	-0.011	0.078***	0.013
Mean Control	0.461	0.270	5.683	11.78	0.123	0.190	0.128
Panel B: 6 months							
Treatment	0.046*	-0.019	0.586	1.617	-0.003	0.009	0.010
	(0.027)	(0.024)	(0.364)	(1.090)	(0.018)	(0.023)	(0.019)
Observations	1,470	1,470	1,458	1,470	1,470	1,470	1,470
Lower Bound	0.038	-0.031	0.428	0.280	-0.018	-0.004	-0.005
Upper Bound	0.055**	-0.015	0.586	1.617*	-0.001	0.013	0.012
Mean Control	0.501	0.270	6.367	12.75	0.129	0.229	0.130
Panel C: 12 months							
Treatment	0.054**	-0.024	0.694*	0.777	-0.006	0.030	-0.008
	(0.027)	(0.024)	(0.371)	(1.139)	(0.020)	(0.024)	(0.020)
Observations	1,396	1,396	1,396	1,396	1,396	1,396	1,396
Lower Bound	0.039	-0.050**	0.378	-1.516	-0.034*	0.006	-0.037**
Upper Bound	0.074***	-0.017	0.694**	0.777	-0.000	0.040*	-0.003
Mean Control	0.522	0.251	6.592	14.23	0.157	0.247	0.152
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All notes are the same as in Appendix Table H.1.1, with estimates restricted to the sample of locals; specifications include competence fixed effects only. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table H.1.3: Effects of Access to Skills Certification (ITT)

Lee Bounds – by follow-up Round using Migrants Sample

			Outcomes Replaced by Zero if Non-Employed				
	Employed (1)	Inactive (2)	Log Wage (3)	Weekly working hours (4)	Formal emp. (5)	Full time (7+ hrs) (6)	Skill Downgrade (7)
Panel A: 3 months							
Treatment	-0.036	0.022	-0.228	-1.278	0.054**	-0.036	-0.108***
	(0.045)	(0.032)	(0.627)	(2.226)	(0.027)	(0.046)	(0.037)
Observations	442	442	442	442	442	442	442
Lower Bound	-0.071	-0.058**	-0.748	-4.843**	-0.027	-0.093**	-0.187***
Upper Bound	0.022	0.036	-0.228	-1.278	0.064**	-0.002	-0.096**
Mean Control	0.667	0.118	8.593	21.88	0.0637	0.402	0.240
Panel B: 6 months							
Treatment	-0.032	0.008	-0.479	-1.183	-0.005	-0.026	-0.056
	(0.045)	(0.033)	(0.630)	(2.378)	(0.030)	(0.049)	(0.040)
Observations	415	415	414	415	415	415	415
Lower Bound	-0.053	-0.055*	-0.911	-4.740*	-0.070***	-0.070	-0.115***
Upper Bound	0.018	0.017	-0.479	-1.183	0.003	0.003	-0.045
Mean Control	0.725	0.124	9.729	23.80	0.109	0.430	0.244
Panel C: 12 months							
Treatment	-0.030	0.001	-0.311	-2.015	0.019	0.005	-0.076*
	(0.046)	(0.036)	(0.649)	(2.504)	(0.032)	(0.051)	(0.043)
Observations	386	386	386	386	386	386	386
Lower Bound	-0.054	-0.068**	-0.700	-4.875*	-0.050*	-0.040	-0.138***
Upper Bound	0.026	0.012	-0.311	-2.015	0.029	0.041	-0.060
Mean Control	0.726	0.140	9.719	25.96	0.101	0.441	0.268
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All notes are the same as in Appendix Table H.1.1, with estimates restricted to the sample of migrants; specifications include competence fixed effects only. *** p<0.01, ** p<0.05, * p<0.1.

I Secondary Outcomes

This appendix reports intention-to-treat effects on outcomes that complement the main labor-market results. All tables follow the main specification 1, reporting estimates for the full sample, locals, and migrants. Table I.1 covers stated preferences about the program: willingness to participate without subsidies and willingness to pay for certification. Table I.2 reports effects on firm characteristics and job mobility, including self-employment, firm size, changes in sector, occupation, and job, and the number of job offers received. Table I.3 reports effects on job satisfaction, food insecurity, and anxiety.

Appendix Table I.1: Effects of Access to Skill Certification on Stated Preferences

Intention To Treat Effects Estimated using Equation (1)

	Participate w/o subsidy (1)	Willing to pay (2)	WTP (USD, midpoint) (3)
Panel A: Full Sample			
Treatment	0.004	-0.016	0.314
	(0.015)	(0.019)	(0.744)
Observations	1,944	3,344	3,344
Mean Control	0.882	0.637	19.10
Panel B: Locals			
Treatment	-0.009	-0.009	0.707
	(0.018)	(0.023)	(0.828)
Observations	1,512	2,569	2,569
Mean Control	0.880	0.614	18.65
Panel C: Migrants			
Treatment	0.045	-0.032	-0.318
	(0.028)	(0.036)	(1.611)
Observations	432	775	775
Mean Control	0.890	0.740	21.09
Competence FE	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes

Notes: Willingness to participate without subsidies was collected at Follow-up 1. Willingness to pay was collected at Follow-ups 1 and 3. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table I.2: Effects of Access to Skills Certification on Firm Characteristics and Job Mobility

Intention To Treat Effects Estimated using Equation (1)

	Solo/ self-emp. (1)	Small firm (2)	Med./large firm (3)	Changed sector (4)	Changed occupation (5)	Changed job (6)	Number of job offers (7)
Panel A: Full Sample							
Treatment	0.025*	0.017	-0.005	0.005	0.012	0.019	-0.051**
	(0.014)	(0.016)	(0.010)	(0.011)	(0.012)	(0.014)	(0.024)
Observations	5,647	5,647	5,647	5,647	5,647	5,647	5,549
Mean Control	0.177	0.254	0.0835	0.0895	0.119	0.161	0.221
Panel B: Locals							
Treatment	0.041**	0.032*	-0.013	0.021*	0.016	0.030**	-0.039
	(0.016)	(0.017)	(0.011)	(0.012)	(0.013)	(0.015)	(0.026)
Observations	4,404	4,404	4,404	4,404	4,404	4,404	4,333
Mean Control	0.169	0.228	0.0814	0.0803	0.106	0.136	0.200
Panel C: Migrants							
Treatment	-0.029	-0.029	0.020	-0.046*	-0.003	-0.020	-0.094*
	(0.030)	(0.037)	(0.023)	(0.024)	(0.031)	(0.033)	(0.056)
Observations	1,243	1,243	1,243	1,243	1,243	1,243	1,216
Mean Control	0.219	0.377	0.0938	0.134	0.182	0.286	0.323
Competence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Firm size outcomes (columns 1–3) are set to zero for respondents not employed at the follow-up. Sector change (column 4), occupation change (column 5), and job change (column 6) are set to zero for respondents not employed at baseline or follow-up. Number of job offers is set to zero for respondents reporting no offers. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table I.3: Effects of Access to Skills Certification on Job Satisfaction and Well-Being

Intention To Treat Effects Estimated using Equation (1)

	Job satisfaction (1)	Skipping a meal (2)	Anxiety (MS) (3)	Anxiety (GAD-7) (4)
Panel A: Full Sample				
Treatment	0.019	0.011	-0.007	-0.110
	(0.018)	(0.018)	(0.020)	(0.246)
Observations	3,667	5,549	1,944	1,944
Mean Control	0.290	0.302	0.285	6.548
Panel B: Locals				
Treatment	0.030	0.025	-0.020	-0.143
	(0.020)	(0.021)	(0.024)	(0.292)
Observations	2,866	4,333	1,512	1,512
Mean Control	0.268	0.304	0.313	6.940
Panel C: Migrants				
Treatment	-0.017	-0.034	0.036	0.010
	(0.041)	(0.035)	(0.036)	(0.448)
Observations	801	1,216	432	432
Mean Control	0.395	0.293	0.150	4.680
Competence FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes

Notes: Job satisfaction is a binary indicator (satisfied or very satisfied) collected among employed respondents at Follow-ups 2–3. Anxiety (MS) and Anxiety (GAD-7) are collected at Follow-up 1 only. All other notes are the same as in Table 2. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

J Effects of Transfer Amount

The *Skills Pay-Off* program offers a set of financial incentives, as explained in Appendix B. One possibility is that these incentives relax liquidity constraints for transportation, job applications, or work-related expenses, which could generate positive employment effects. To test this, we exploit the variation in the incentives received by participants and report whether treatment effects vary with the size of the transfer.

Every participant received $82 USD upon completion of the program. However, the main variation in incentives comes from the possibility of receiving an additional $75 USD if participants either (i) have children under 5 years old under their care or (ii) are over 60 years old.[10] Thus, we can exploit the main variation in incentives using participants' baseline information.

Table J.1 presents the heterogeneous effects of the financial incentive based on the first condition, having children under their care. We augment equation 1 with an interaction between treatment and a caregiver dummy, and estimate it excluding people over 60 years old. For locals, the interaction suggests that the effect of the skill certification on employment is reduced by 8.4 percentage points among caregivers, coupled with a 7.6 percentage point reduction in full-time employment. Similarly, we estimate equation 1 with an interaction between treatment and a dummy for being over 60 years old, excluding caregivers. The results do not suggest any heterogeneous effect consistent with the transfers helping participants find a job by relaxing liquidity constraints.

However, these results should be interpreted with caution, since in this setting we cannot disentangle the effect of the variation in the transfer amount from the effect of being a caregiver or being over 60 years old. In fact, these heterogeneous effects may be due to caregivers facing time constraints that prevent them from entering the job market, and to the difficulties people over 60 face in entering it. At the very least, these results point in the opposite direction to the expected effect of the transfer, since relaxing the budget constraint should make it easier for participants to enter the labor market.

[10] Additional incentives for transportation and the remedial session are negligible in the 2024 wave of the program, as only 0.2% of the sample needed the remedial session (see Table 1) and transportation incentives applied only to participants in rural areas.

Appendix Table J.1: Effects of Access to Skill Certification by Transfer Amount: Caregiver Margin

Intention To Treat interacted with the caregiver indicator, participants under 60

			Outcomes Replaced by Zero if Non-Employed				
	Employed	Inactive	Log Wage	Formal emp.	Weekly working hours	Full time (7+ hrs)	Skill Downgrade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Locals							
Treatment	0.105***	-0.040	1.333***	0.003	2.921**	0.085***	0.021
	(0.033)	(0.027)	(0.442)	(0.025)	(1.376)	(0.028)	(0.028)
Treatment × Caregiver	-0.084*	0.008	-0.981	-0.006	-1.130	-0.076**	-0.015
	(0.044)	(0.037)	(0.604)	(0.034)	(1.857)	(0.038)	(0.036)
Caregiver	0.016	0.011	0.226	0.003	-0.137	0.033	-0.016
	(0.027)	(0.023)	(0.372)	(0.021)	(1.123)	(0.022)	(0.021)
Mean Control	0.505	0.242	6.369	0.145	13.21	0.230	0.148
Share caregivers	0.549	0.549	0.550	0.549	0.549	0.549	0.549
Observations	3900	3900	3889	3900	3900	3900	3900
Panel B: Migrants							
Treatment	-0.067	0.061*	-0.968	0.015	-2.240	-0.035	-0.112**
	(0.049)	(0.034)	(0.678)	(0.033)	(2.537)	(0.051)	(0.045)
Treatment × Caregiver	0.070	-0.099**	1.343	0.019	1.809	0.024	0.054
	(0.071)	(0.050)	(0.999)	(0.052)	(3.774)	(0.076)	(0.072)
Caregiver	-0.021	0.036	-0.422	-0.015	-0.092	0.018	0.039
	(0.052)	(0.036)	(0.727)	(0.036)	(2.832)	(0.057)	(0.056)
Mean Control	0.705	0.125	9.317	0.091	23.86	0.425	0.248
Share caregivers	0.455	0.455	0.454	0.455	0.455	0.455	0.455
Observations	1227	1227	1226	1227	1227	1227	1227

Notes: Each column reports the Table 2 pooled specification augmented with an interaction term between treatment and the caregiver indicator, estimated on participants aged under 60, separately for locals and migrants. Standard errors in parentheses are clustered at the individual level. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table J.2: Effects of Access to Skill Certification by Transfer Amount: Age Margin

Intention To Treat interacted with the aged-60+ indicator, non-caregivers

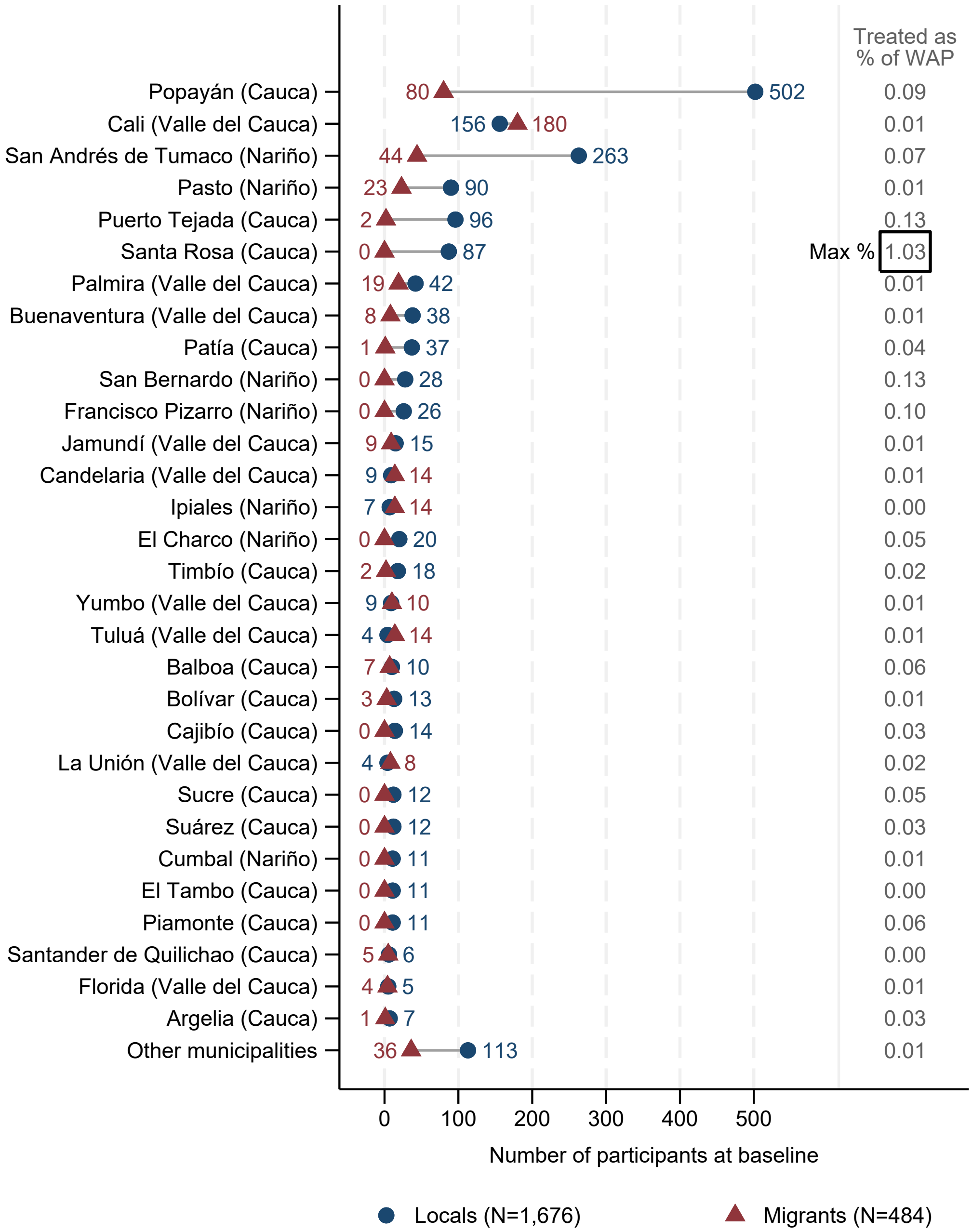
			Outcomes Replaced by Zero if Non-Employed				
	Employed (1)	Inactive (2)	Log Wage (3)	Formal emp. (4)	Weekly working hours (5)	Full time (7+ hrs) (6)	Skill Downgrade (7)
Panel A: Locals							
Treatment	0.104***	-0.039	1.324***	0.001	2.854**	0.085***	0.020
	(0.033)	(0.027)	(0.442)	(0.025)	(1.378)	(0.027)	(0.028)
Treatment × Aged 60+	0.049	-0.077	0.774	-0.045	2.584	-0.022	-0.050
	(0.089)	(0.085)	(1.154)	(0.039)	(3.688)	(0.071)	(0.034)
Aged 60+	-0.137**	0.204***	-1.942***	-0.085***	-4.398**	-0.055	-0.136***
	(0.054)	(0.055)	(0.698)	(0.031)	(2.039)	(0.043)	(0.025)
Mean Control	0.495	0.251	6.207	0.133	13.20	0.212	0.141
Share aged 60+	0.127	0.127	0.127	0.127	0.127	0.127	0.127
Observations	2012	2012	2005	2012	2012	2012	2012
Panel B: Migrants							
Treatment	-0.065	0.063*	-0.936	0.016	-2.015	-0.029	-0.111**
	(0.049)	(0.034)	(0.684)	(0.033)	(2.580)	(0.052)	(0.045)
Treatment × Aged 60+	-0.079	-0.102*	-1.438	-0.036	-21.200**	0.017	0.173
	(0.163)	(0.055)	(2.150)	(0.048)	(9.598)	(0.400)	(0.289)
Aged 60+	0.291***	-0.095***	4.251***	-0.101***	5.847	0.095	0.512*
	(0.043)	(0.023)	(0.562)	(0.031)	(8.986)	(0.172)	(0.265)
Mean Control	0.722	0.107	9.624	0.095	24.31	0.422	0.235
Share aged 60+	0.015	0.015	0.015	0.015	0.015	0.015	0.015
Observations	679	679	679	679	679	679	679

Notes: Each column reports the Table 2 pooled specification augmented with the interaction term between treatment and an indicator for being aged 60 or older at registration, estimated on non-caregivers, separately for locals and migrants. Standard errors in parentheses are clustered at the individual level. All other notes are the same as in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

K Policy Discussion

This appendix provides the supporting material for the cost-effectiveness and scalability discussion in Section 9. Figure K.1 reports the geographic distribution of local and migrant participants at baseline and, for each municipality, the number of treated participants as a share of its working-age population.

Appendix Figure K.1: Geographic Distribution of Locals and Migrants at Baseline



Notes: Each row is a municipality of residence at baseline. Markers show the count of all local and migrant participants residing there at baseline. Rows list the thirty municipalities with the most participants at registration. The remaining 61 municipalities are grouped in the bottom row. The right column reports the number of treated participants in each municipality as a percentage of its working-age population (WAP), defined as the projected population by aged 15–64 in 2025 according to Colombian Statistical Office (DANE) estimations. The black box highlights the maximum participation in WAP of 1.03% in Santa Rosa (Cauca)

Table K.1 reports the cost per participant, the estimated employment effect, and the implied cost per additional job for the 2024 cohort and for the earlier program rounds, and compares them with benchmarks for vocational training, job-search assistance, and wage subsidies drawn from the literature.

Appendix Table K.1: Cost-Effectiveness Compared to Other Active Labor Market Policies (2024 USD)

Policy type (1)	Study / context (2)	Cost per participant (3)	Employment effect (4)	Implied cost per additional job (5)
Skill certification	Colombia, 2024 cohort	$ 203	+5.7 pp (locals)	$3,561
	Colombia, 2021–2023 rounds[a]	$142	+5.7 pp (locals)	$2,486
Vocational training	12-country review (McKenzie, 2017)[b]	$17–$2,170	∼2.3 pp avg. (3 of 9 sig.)	$21,700–$76,700
	Colombia, Jóvenes en Acción[c]	$1,093	+4.0 pp (formal emp.)	$27,320
Job-search/ matching assistance	10-intervention review (McKenzie, 2017)[b]	$4.50–$260	∼2.7 pp avg. (1 of 10 sig.)	$29,100 (Jordan, only sig. case)[d]
Wage subsidies	3-RCT review (McKenzie, 2017)	n.a.	Fades after subsidy ends	Not well defined

Notes: All figures are in constant 2024 US dollars. Implied cost per additional job = cost per participant ÷ employment effect (in percentage points, expressed as a decimal). Skill-certification figures are from this paper (Appendix Table B.1 and Table 2, Panel B).

[a] Beneficiary-weighted average cost per participant across the 2021–2023 rounds (Appendix Table B.1). Each year's nominal cost is inflated to 2024 USD via the US CPI-U (nominal: $122 [2021], $128 [2022], $143 [2023]; pooled nominal average $130, adjusted average $142).

[b] McKenzie (2017), *World Bank Research Observer* 32(2): 127–154. Nominal figures as reported (not adjusted for inflation in the original review): vocational training $13–$1,700/participant, $17,000–$60,000/job (12 evaluations, 8 countries); job-search/matching assistance $3.50–$203/participant (9 RCTs, 10 interventions). We approximate the underlying studies' cost-reference year using the review's 2017 publication year (CPI factor 1.279 to 2024 USD).

[c] Employment effect (+4.0 pp formal employment, ITT) is from Attanasio, Kugler, and Meghir (2011), *American Economic Journal: Applied Economics* 3(3): 188–220. The cost figure (originally COP$1,624,000 ≈ US$812 in 2013 USD) is from the cost-benefit analysis in Attanasio, Guarín, Medina, and Meghir (2015), NBER Working Paper No. 21390, adjusted to 2024 USD using a 2013–2024 CPI factor of 1.346 (nominal cost per job: $20,300).

[d] Groh, Krishnan, McKenzie, and Vishwanath (2015), Jordan; nominal 2015 USD ($22,000) adjusted to 2024 USD using a 2015–2024 CPI factor of 1.323, as cited in McKenzie (2017).