

Vocational Training for Disadvantaged Youth in Colombia: A Long-Term Follow-Up[†]

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We evaluate the long-term impacts of a randomized Colombian training and job placement program. Following the large short-term effects, we now find that the program effects persist, increasing formal participation and earnings contributions to social security and working in larger firms. By using a large administrative source we are also able to establish that the program improved both male and female labor market outcomes by a similar amount—a result that was not apparent with the smaller evaluation sample. The results point to a cost-effective approach to reducing informality and improving labor market outcomes in the long run. (JEL I28, J13, J24, J31, O15)

Skill acquisition and participation in the formal labor market is a central policy concern in developing countries. From a growth perspective, a well-functioning economy that is able to attract investment requires individuals with sufficient levels of skills and work readiness (see, for instance, Organisation for Economic Co-operation and Development (OECD) 2012). Moreover, unskilled youth have particularly low labor market attachment and are vulnerable to poverty. Identifying interventions that can promote human capital acquisition and improve labor market outcomes in the long run has been elusive, with generally mixed results, at least in high-income countries.

In Colombia, a vocational training program, Jóvenes en Acción (JeA), carried out in 2005 and evaluated with a randomized controlled trial in Attanasio, Kugler, and Meghir (2011)—henceforth, AKM—was shown to improve formal sector employment and pay, particularly for young women for whom the implied internal rate of return of the program was at least 21.6 percent. For men, however, there was no discernible effect other than a shift to the formal sector.

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The program, which combined classroom training with an internship, was implemented in the wake of the worst recession Colombia experienced since WWII in 1999–2000.¹ It was provided mainly by private sector training institutions that could screen the applicants for suitability. In turn, the payments to the training institutions depended on successful completion of the program and on successful professional placement of the applicants.

In this paper, we carry out a long-term follow-up of the AKM study, based on administrative records for the period July 2008 to December 2014. In addition to the original evaluation sample, covering a random subset of those who participated in the experiment, we also analyze the *entire* cohort of eligible individual applicants who were part of the randomization, substantially improving precision.²

The nature of the administrative data is such that workers are only observed if they are in the formal sector. Thus, our evaluation will focus on long-term attachment to the formal sector and on the resulting earnings and other related outcomes, such as pension contributions. Informality is a major issue around the world and in Latin America in particular. Firms that operate in the formal sector tend to be higher productivity, offer higher pay (see Meghir, Narita, and Robin 2015), and often other benefits such as training and improved professional opportunities. Indeed, reallocating workers to the formal sector can improve growth. However, informality is hugely prevalent: according to Medina, Núñez, and Tamayo (2013) since the mid-1980s informality (based on the International Labour Organization (ILO) definition) has fluctuated in Colombia from 53 percent to 60 percent and is particularly high among the young (see Saavedra and Medina 2014) at a time which is important for laying the foundations of a productive career.³

The results show sustained improvements for trainees over long periods of time, making it unique (to our knowledge) in the adult training literature. We find that the JeA program had a positive and significant effect on the proportion working in the formal sector; it increased contributions to social security and other programs, and increased formal sector earnings by 12 percent overall. Importantly, using the data from the entire experimental cohort, we are able to establish that the effects for men are significant and in some cases significantly larger than those for women. Beneficiaries of the JeA program were also more likely to work for a large firm, one with at least 200 employees, inducing possibly an overall increase in the quality of jobs. On the other hand, we have no long-term information on how those not working in the informal sector are faring and this precludes a complete evaluation of the program.

¹ See Gaviria Uribe and Núñez Mendez (2003); Barrera-Osorio and Corchuelo (2003); Medina and Núñez (2005); Sarmiento et al. (2007); Santa María et al. (2009); and Steiner, Rojas, and Millán (2010), among others, for issues relating to training in Colombia. According to Székely Pardo (2012), 20 percent of Colombian youths 15–18 years old were not in education, employment, or training by 2005, while Guarín and Medina (2015) show that in Colombian cities like Medellín, the same figure is as high as 44 percent for youth 16–20 years old.

² Other studies on training in Latin America include Card et al. (2011), and Ibarraran et al. (2014) who provide experimental results for the short term, while Alzúa, Cruces, and Lopez (2015) present experimental evidence three years after graduation. This is the first study to look beyond this horizon, to the best of our knowledge. Subsequent to the first draft of our paper we learned that Kugler et al. (2015) also study the long-term impacts of the same program but based on the evaluation sample alone.

³ See also Perry et al. (2007), Cárdenas and Mejía (2007), and López Castaño (2010).

One concern is that some of the observed effects do not represent an increase in overall economic activity, but could arise from the displacement of other workers who are close substitutes of the trainees. Although evidence in Blundell et al. (2004) showed no displacement effects in the United Kingdom, a specially designed randomized experiment evaluating a job placement program in France showed evidence of job displacement (see Crépon et al. 2013). If the training does improve human capital, such displacement should only be a concern in the short run; in the longer run the economy should expand in response to increased resources. However, if all that happens is to create a channel through which some workers get better access to the formal sector, while others with identical skills do not, displacement can be a real concern even in the longer run. AKM discuss the possibility of displacement, and they found no sign of displacement effects. Here, we present further evidence that suggests that the effects are not due to displacement, although we can only rely on observational data for this purpose.

The rest of the paper is organized as follow: in Section I, we describe the intervention; in Section II, we describe the data; in Section III, we present the estimated effects of the program. Section IV concludes.

I. The Intervention

The Jóvenes en Acción program was a training program for urban young unemployed in Colombia. It was targeted to unemployed youths 18 to 25, who belonged to the poorest population classified in the two lowest levels of a score, called SISBEN, which is used in Colombia to target all welfare programs.⁴ The program was implemented in the seven main cities of the country.⁵ It began to enroll students in 2002, and, by 2005, it had enrolled 80,000 students.

The goals of the program, which we describe in more detail in online Appendix A, were to develop the youths' occupational skills, increasing their employability and productivity, to promote the private supply of training, and to improve the matches between workers and firms. Jóvenes en Acción consisted of training courses provided by private institutions (Entidades de Capacitación (ECAP)). Each course was expected to train about 30 unemployed youths, chosen by the ECAPs among eligible applicants. For evaluation purposes, in 2005, the ECAPs were encouraged to select more than 30 applicants; the courses were offered to 30 applicants randomly selected from this group.

The course had to have three main components: classroom training; on-the-job training; and the youth's *project of life* (Fondo de Inversión para la Paz (FIP) 2001 and Departamento Nacional de Planeación (DNP) 2016). The program also included a small stipend of about US\$2.20 per day for trainees without children

⁴SISBEN is the acronym in Spanish for Information System for Beneficiaries Selection, and it consists of six levels built with the quality of life SISBEN score, used in Colombia to target public subsidies. To apply to the program, individuals were additionally required to have a valid ID, and if the applicant was a mother of children under seven years of age, she must present an official document to prove her maternity. Applicants that had previously taken training courses at Servicio Nacional de Aprendizaje (SENA) or any training institution were also eligible for Jóvenes en Acción.

⁵Bogotá, Medellín, Cali, Barranquilla, Bucaramanga, Manizales, and Cartagena.

under seven years of age, and about US\$3.00 per day for women with children under seven. In 2005 there were 114 ECAPs offering 441 courses to 26,615 trainees, with their instructors teaching about 7.6 hours per day.⁶

A large fraction of the overall payment to the ECAPs was conditioned on the student completing a three-month apprenticeship with the participating firms in a timely fashion (see FIP 2011 and Departamento Administrativo para la Prosperidad Social 2016). Further bonus payments depended on the firms hiring the trainee on formal contract. In turn, the ECAPs would define suitable candidates for training amongst those eligible. This allowed them to identify those most likely to succeed. The incentive scheme, combined with prescreening, was an important departure from the standard practice at SENA—the Colombian government training agency (see Saavedra and Medina 2013)—and a key feature of this program.

The total cost of the program to the government is the sum of the cost of the course and the maintenance stipend to the students, which lasted six months (see FIP 2011 and Departamento Administrativo para la Prosperidad Social 2016). To this, we need to add any forgone earnings of participants. The total amount invested was US\$22 million (see Ministerio de la Protección Social 2005).

II. The Experiment and the Data

A. The Random Assignment to the *Jóvenes en Acción* Program

For the purpose of evaluating the intervention by random assignment to treatment, the ECAPs were asked to select up to 50 percent more applicants than the places available for the courses for the 2005 cohort of trainees. Since the ECAPs were only paid for those who completed the training, they had strong incentives to select individuals accordingly. Two-thirds of the selected applicants were then randomly assigned to the program they applied for, while the remaining one-third were assigned to a control group.

B. The Data

The original evaluation of the program was based on a random sample drawn from the population that was screened by the ECAPs and thus participated in the experiment. In this study, we use a combination of this evaluation sample and administrative data for the long-run follow-up. We are also able to use the *entire* experimental cohort, increasing the sample size tenfold.

The Evaluation Sample.—The baseline evaluation data includes 2,066 individuals in the treatment group and 2,287 in the control, sampled randomly from the entire population of the eligible. The baseline data was collected in January 2005 and the follow-up in August to September 2006. Further details can be found in AKM, who also show that it was well-balanced between treatment and control.

⁶Forty percent of the beneficiaries were from Bogotá, 18 percent from Medellín, 16 percent from Cali, 11 percent from Barranquilla, 7 percent from Bucaramanga, 5 percent from Cartagena, and 2 percent from Manizales.

Administrative Records.—No further follow-up was collected by survey. However, we can carry out the long-term evaluation using two alternative sources: first, we can link those in the evaluation sample to administrative data for 2008–2014; second, we can measure some outcomes for the *entire* experimental cohort, which is almost ten times the size of the evaluation sample, by linking them to administrative records in 2010.

The SISPRO Data: The administrative data source is the SISPRO, which contains information from the Unified Register of Contributions, known as PILA. This is the national information system used by firms to file the mandatory contributions to health, pensions, and disability insurance they pay for their employees. The Ministry of Social Protection has universal coverage of all the monthly contributions in the country since July 2008, which provides us with the possibility of following up individuals in the evaluation survey and constructing a longitudinal database with monthly frequency from July 2008 to December 2014. Since the PILA contains the monthly census of all contributions in the country, individuals in the evaluation database that were not found in the PILA register are either out of work, or working in the informal labor market. The structure of the data also allows us to observe individuals entering and exiting the formal labor market based on the date of entry and exit from the register.

For those in the experimental population who are not in the evaluation sample we can only link records in 2010. Thus, if an individual is working in the formal sector in 2010, we can observe them. If they are not matched in that year, we know that in that year they were either not working or an informal worker.

In Table 1, we show the sample sizes for the evaluation sample and the entire cohort. The experimental population consisted of 33,929 screened and eligible applicants. The original evaluation sample included a random sample of those, half drawn from the controls and half from the treatment. Nearly all have valid baseline information and are used here. Overall, 54 percent were women; the average age is 21; and the average education is 10 years. For the entire experimental population we matched 31,054 with the baseline administrative data. Consistent with the original randomization, about two-thirds are treated and one-third are controls. Detailed descriptive statistics are in the online Appendix.

Balance at Baseline: For both women and men, the baseline characteristics are well-balanced in the evaluation sample. This was originally documented by AKM and also shown in Table A1a in the online Appendix. Moreover, Table A1b in the online Appendix compares baseline characteristics measured in the SISBEN data for the entire cohort. Again, for both women and men, the sample is well-balanced, except that beneficiaries are slightly younger.

III. Program Effects

We start with a graphical representation of the impacts of the program. Figure 1 shows the difference in formal employment rates between the treatment and control group over time and by gender; the graph includes a 95 percent point wise confidence

TABLE 1—SAMPLE SIZES

	Evaluation sample			Entire cohort		
	Total	Treated	Control	Total	Treated	Control
Screened and eligible applicants	3,956	2,040	1,916	33,929	22,438	11,491
Individuals with valid baseline information ^a	3,932	2,029	1,903	31,054	20,548	10,506
Proportion female	54 percent			70 percent		

^aFor the entire cohort: matched to baseline SISBEN.

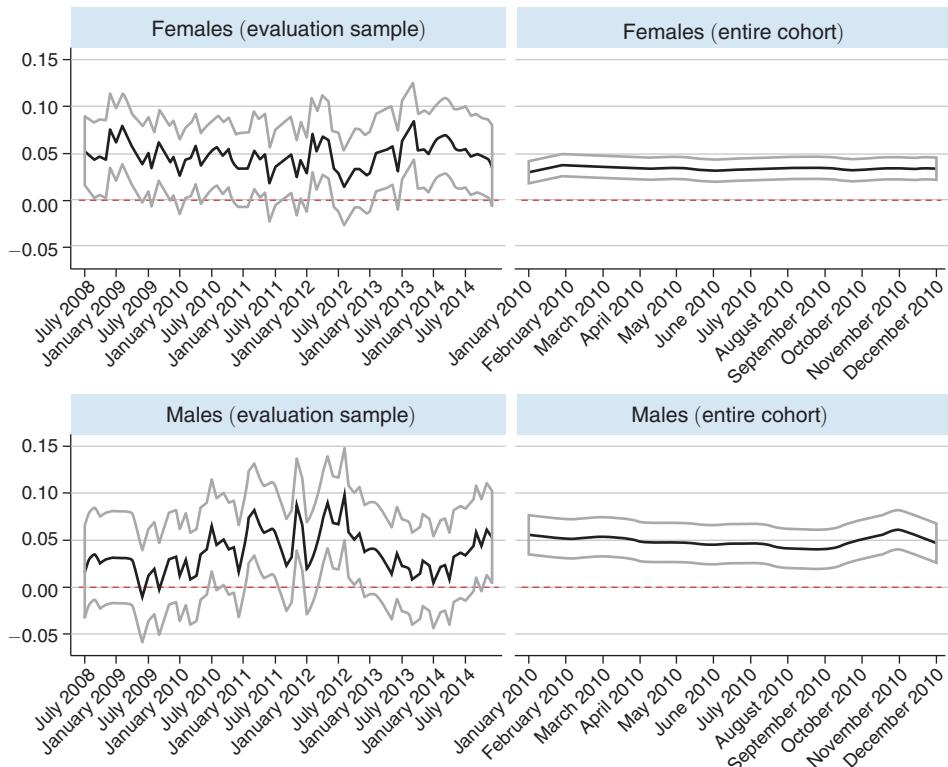


FIGURE 1. TREATMENT EFFECT ON FORMAL EMPLOYMENT BY TREATMENT AND GENDER

Source: JEA-SISPRO

interval around the estimated impact. While the results for the evaluation sample are much noisier than those of the full sample, they are both implying a positive effect, which is stable over time.⁷ The results with the entire cohort show clearly that these are significant effects. Similar patterns obtain if we plot the effect for formal earnings. The important conclusion is that the increased attachment to the formal sector caused by the program seems to be sustained for both women and for men, despite the fact that the original evaluation did not identify significant effects for men.

⁷The evaluation sample and the entire cohort contain a different proportion of women.

A. Program Estimates and Inference

In what follows, we present average results for the evaluation sample and the entire cohort, based on regression analysis using the following model:

$$(1) \quad Y_{ij} = \alpha D_i + X_i \beta + SC_j + \varepsilon_{ij},$$

where Y_{ij} is the outcome of person i in site and course j ; D_i is the treatment indicator, which is one for those assigned to treatment and zero otherwise; and SC_j are site by course fixed effects. X_i is a vector of characteristics of the applicants at baseline. The X_i differ depending on whether we use the evaluation sample, where we have detailed baseline information, or the entire cohort, where the baseline characteristics are those reported in the SISBEN⁸ survey. Characteristics used as controls are reported in the notes to the tables.

The randomization was at the individual level and it took place within the population of eligible and screened applicants for each course. We thus include site-by-course fixed effects (SC_j). The estimates we present will be a weighted average (α) of intent-to-treat effects across all courses.⁹ However, since compliance among applicants is 97 percent, our estimates should not differ substantially from the average treatment effect in this population. We weigh each applicant by $P_{SCI}(1 - P_{SCI})$, where P_{SCI} is the population probability taken from administrative records that an applicant i that applied to course C at site S be offered a place.¹⁰

Inference is based on 1,000 bootstrap replications stratified by city, gender, and treatment status. We report standard errors and p -values adjusted for multiple testing following the step-down procedure described in Romano and Wolf (2005) and Romano, Shaikh, and Wolf (2008). Each table of results represents a group being jointly tested for the entire sample and then also for men and women separately. p -values are then interpreted as the significance level that would have to be applied to the *entire family of hypotheses* if we were to accept the null that the effect is zero.

B. Effects of the Jóvenes en Acción on Participation in the Formal Labor Market

We focus on three outcome variables: whether an individual is working in a formal job in any one month, whether they are working in a large formal firm, and their earnings. As before, and in everything that follows, the outcomes are set to zero for those who have no formal job recorded.

The results for men and women combined are reported in Table 2, while results by gender are shown in Table 3. In Table 2, the p -values are adjusted for testing the three outcomes in that table. In Table 3, the Romano-Wolf p -values account for testing of all six outcomes reported there. All levels of significance are reported based on the Romano-Wolf p -values and not on the standard t -statistics.

⁸We explain the nature of the SISBEN survey in the online Appendix.

⁹The randomization probability is in principle the same across courses, so it is not strictly necessary to include such fixed effects. They have little or no influence on the results.

¹⁰While we report the results obtained using this weighting scheme, we also compute unweighted estimates, which are available on request. Weighting does not make any substantial difference to our results.

TABLE 2—LABOR MARKET OUTCOMES

	Evaluation sample ^b		Entire cohort ^c	
	Control means	Coefficient on being offered training	Control means	Coefficient on being offered training
Formal income ^a	258,922 (432,480)	35,331 (10,766) [0.025]	220,126 (393,121)	26,825 (4,267) [0.000]
Working in the formal sector	0.36	0.042 (0.012) [0.009]	0.33	0.038 (0.005) [0.000]
Working in large formal sector firm	0.189	0.032 (0.010) [0.025]	0.174	0.027 (0.005) [0.000]
Observations ^d	148,746	306,696	126,072	372,648

Notes: Romano-Wolf *p*-values (in square brackets) for this group (three coefficients). Standard errors (in parentheses) are clustered at the applicant level. All regressions control for site-by-course fixed effects.

^aCOP\$ of 2013.

^bThese regressions control for the following pretreatment characteristics interacted with gender: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week. Romano-Wolf *p*-values for this group. Standard errors are clustered at the applicant level.

^cThese regressions control for the following pretreatment characteristics interacted with gender: whether the applicant is in a low socioeconomic stratum, whether living in a house or apartment, whether living at home without threats, age in 2005, homeownership, household size, education of the head of the household, age in 2005 of the head of the household, number of children under 5 years old and number of adults over 65 years old in the household, SISBEN score and its square, whether the applicant is the head of the household, and whether the applicant is the spouse/partner of the head of the household.

^dObservations for evaluation sample: 78 months × individuals; observations for entire cohort: 12 months × individuals.

TABLE 3—LABOR MARKET OUTCOMES BY GENDER

	Evaluation sample ^b				Entire cohort ^c			
	Women		Men		Women		Men	
	Control means	Coeffic.	Control means	Coeffic.	Control means	Coeffic.	Control means	Coeffic.
Total formal income ^a	200,103 (366,562)	35,495 (12,421) [0.065]	327,673 (489,732)	35,126 (18,597) [0.251]	187,022 (373,297)	23,178 (4,805) [0.000]	287,686 (422,794)	35,632 (8,814) [0.000]
Working in formal sector	0.29	0.047 (0.015) [0.052]	0.43	0.036 (0.019) [0.251]	0.28	0.03 (0.006) [0.000]	0.42	0.05 (0.011) [0.000]
Work in large formal firm	0.146	0.038 (0.013) [0.065]	0.24	0.025 (0.016) [0.251]	0.15	0.020 (0.005) [0.000]	0.223	0.044 (0.009) [0.000]
Observations	81,588	165,750	67,158	140,946	84,612	259,788	41,460	112,860

Notes: Standard errors are in parentheses, clustered at the applicant level. Romano-Wolf *p*-values (in square brackets) for six hypotheses in each sample, respectively. Other notes as in Table 2.

Formal Earnings.—We find that formal earnings are about COL\$35,000 higher among the individuals who were randomly assigned to training, which corresponds to a 13.6 percent increase and is significant at the 2.5 percent level. This shows a remarkable persistence of the effects of the program. In terms of pesos, the effect is similar for males and females, at about 35,000 (Table 3), although for women it represents a higher percentage increase: female earnings in the control group are COL\$200,000, while those for males are COL\$327,000. Thus, the respective percentage increases are 17.5 percent for females and 10.7 percent for males. Once we control for multiple testing the female effect is only significant at the 6.5 percent level, while the male effect is not significant. However, when we turn to the larger sample from the entire cohort we get much more precise results: they confirm the large and significant effect overall and, crucially for the value of the program, we now can establish that there was a large and significant effect for both females *and* males. While the point estimate for males is substantially larger than that of women, the difference is not significant. This result is key because it puts the entire program in a different perspective, showing that it also improved male outcomes.

Formal Sector Employment.—The increase in formal earnings is driven by an increase in formal sector employment: the probability of working in the formal sector is increased by 4 percentage points (Table 2). In both datasets the effects are highly significant. In Table 3, we find that for females the probability of being a formal employee increases by 5 percentage points (Romano-Wolf *p*-value of 0.052). For males, the point estimate is similar but not significant in the evaluation data. But again, when we turn to the larger data, we find a significant and larger effect on formal employment for men (5 percent, *p*-value 0). In both cases it represents an impact of about 12 percent higher probability of being formal with respect to the control applicants. This confirms that the program was beneficial for men as well as for women, and importantly, the effects are persisting well after the program. From the evaluation sample we find that participants in JeA worked on average 3.33 months longer in the formal sector (standard error of 1.19).

Probability of Working in a Large Firm.—Finally, as a proxy for quality of jobs, we estimate the effect of the program on working for a large firm. About 15 percent of control applicants do so. Overall, both datasets show an increase of about 3 percentage points, which is highly significant. As before the evaluation sample is not large enough to provide precise enough estimates for men and women separately, but when we turn to the larger dataset, we find a 2 percentage points increase for women and a 4.4 percentage points increase for men. Both have *p*-values of zero and are significantly different from each other. Thus, the program increased participation in the formal sector of both men and women, and seems to have improved the quality of jobs (as measured by working in a large firm) particularly for men.

Contributions to Social Security.—In Table A2 of the online Appendix, we also present impacts of the program on the amounts of mandatory contributions paid to various programs based on the data for the whole cohort. These increased significantly by 10–15 percent for men and women, confirming directly the greater

attachment to the formal sector. As before, the entire cohort helps us in establishing that the effect for men was large and significant.

Displacement.—As discussed in the introduction, a concern with programs such as JeA, particularly when associated with job placement, is that at least part of the effect is due to displacement of workers who are not treated but are otherwise close substitutes and could have obtained these jobs if the trainees had not been actively placed.¹¹ Moreover, in areas with a high level of treatment, the competition for jobs can increase, decreasing everyone's arrival rates. Based on Crépon et al. (2013), one way to gauge the extent to which this is the case is to use the varying proportion of treated (out of the unemployed population) for each city and occupation. In the presence of negative externalities, an increased proportion of treated individuals should reduce overall formal employment rates. Moreover, to the extent that treated individuals are being placed at the expense of control individuals, the treatment effect will increase with the proportion treated as long as the negative externality on the controls was larger than that on the treated. We can repeat the exercise by also considering the proportion treated out of the total number of unemployed and employed in that occupation.

To construct proportion treated, we use the entire experimental cohort of the JeA program and a Colombian household survey,¹² which allows us to estimate how many people are unemployed and looking for a job in a specific occupation and city. We also know how many were trained by occupation (from the type of course they participated in). Combining the two, we derive the proportion treated out of the unemployed in each city and occupation for the baseline period. This varies substantially. In Table 4, we show the proportions treated averaged across occupations in each of the cities. Bogotá has the lowest proportion at 12 percent and Bucaramanga the highest, where 61 percent of the unemployed were treated. Once we go to the occupation level, the variation is much higher.

To test for negative externalities, we run two sets of regressions. In one, we include the proportion of the unemployed that are treated as well as the interaction of this proportion with the treatment dummy. The coefficient on the proportion has a coefficient of 0.01 (standard error 0.007); in the presence of a negative externality this coefficient would be negative. The coefficient on interaction of the treatment dummy with the proportion is -0.015 (standard error 0.01).

In the second regression, we add site-by-course fixed effects (making the effects of the proportions unidentifiable) and include only the interaction with the treatment dummy. We obtain an estimate of the interaction term of -0.008 (standard error 0.014), which is not significant. Using the proportion treated out of all those in the same occupation (whether employed or unemployed) the conclusions are the same. In both cases the impact on formal sector participation remains about the same at 0.044 and 0.040, respectively, and significant at the 1 percent level.

¹¹ See Blundell et al. (2004) and Crépon et al. (2013).

¹² The Colombian household survey is used by the Administrative Department of National Statistics of Colombia, DANE, to obtain Colombia's labor market indicators.

TABLE 4—PROPORTION OF THE UNEMPLOYED TREATED BY CITY (*average across occupations*)

Barranquilla	Bogotá	Bucaramanga	Cali	Cartagena	Manizales	Medellín
0.275	0.118	0.607	0.242	0.131	0.383	0.166

Thus, the evidence based on these simple regressions suggests that displacement was not a major issue in this intervention and that we can take the treatment effects as implying a direct increase on economic activity without negative externalities on other labor market participants. However, these results must be treated with some care because the treatment intensities are potentially endogenous. Moreover, this does not mean that such effects (or indeed other general equilibrium effects) would not occur on an expanded and permanent version of this program.

Cost-Benefit Analysis.—The administrative data does not provide enough information for a complete cost-benefit analysis without any further assumptions because we do not observe outcomes for those working in the informal sector. However, if we make the conservative assumption that overall employment did not increase (i.e., the increase in the proportion of those working in the formal sector results in an equal decrease in those working in the informal) and that earnings in the informal sector remain unaffected, we can obtain a lower bound to the benefits of the program under the assumption of no displacement. The costs of the program, including loss of tenure, were calculated in AKM to be US\$812. Based on the entire sample, and on an annual depreciation rate for the benefits of 6 percent and a discount rate of 6 percent as well, the lifecycle benefits for men are US\$1,046 and for women US\$1,010,¹³ implying a gain of approximately US\$200 for men and women, which is equivalent to an internal rate of return of about 10 percent for both men and women. With no depreciation, which seems reasonable given the evolution of the gains we observed, the IRR is as high as 16 percent.

IV. Conclusions

The Jóvenes en Acción program was focused on training and work experience for young people. Its original evaluation by AKM showed positive effects on women, for whom earnings and formal employment increased; for men, formal employment increased but not earnings. In this paper, we consider the longer run effects of the program. Moreover, by using a dataset that includes ten times the number of individuals in the original evaluation sample, we are able to obtain much more precise estimates.

The results are remarkable: the increase in formal employment is sustained for both men and women with no indication of it fading away, and average formal earnings increase for both men and women. Moreover, we also find that the probability of working in a large firm (a proxy for better quality jobs) increases, as do

¹³We are using an exchange rate of 2,000 COP to the US dollar.

contributions to pensions and other programs for both men and women. In testing whether the impacts can be attributed to displacement of workers who may otherwise have obtained jobs instead of our subjects, we find no such evidence. Finally, the internal rates of return, which are 10 percent with the most pessimistic scenario, far exceed interest rates. Unfortunately, the lack of data on the informal sector precludes a complete evaluation of the program. Indeed, the positive impacts in the formal sector could in principle be counteracted from negative impacts in the informal one; however, the short run results in AKM do not point to any such negative effects in the short run.

By all accounts this has been a successful intervention and offers a rare example of a vocational program with long-term sustained effects, if not for overall employment, at least in improving the levels of formal employment relative to the informal one—this is of key importance in developing countries. Evidence on the importance of the complementarity between the classroom component and work experience would further enhance our ability to design successful vocational education.

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