**Long Term Impacts of Vouchers for Vocational Training:
Experimental Evidence for Colombia**

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Abstract

We use experimental data of a training program in 2005 in Colombia. We find that the *JeA* program had a positive and significant effect on the probability to work in the formal sector. Applicants in the treatment group also contributed more months to social security during the analyzed period. Beneficiaries of the *JeA* program were also more likely to work for a large firm. Earnings of treated applicants were 11.8 higher in the whole sample, and they made larger contributions to social security. In addition, we also present non parametric bounds that for all demographic groups show that for some percentiles of the sample, there are positive and significant effects of the program. Thus, the effects of the program would have been capitalized both in increases in the likelihood of being formal, and increases in productivity.

We also find that for the whole sample of applicants, those in the treatment group have 0.24 more years of education, and have a probability of graduating from high school 5.6 percent higher than the control group. We find no significant effect on college and school attendance, fertility decisions, marital status or some dimensions of assortative mating. Among applicants matching to the census of the poorest population, we find that beneficiaries are more likely to be employed, presumably among this population, in the informal sector. Among this population, applicants also have higher earnings.

Finally, we find that the benefits of the *JeA* program are higher than it costs, leading to an internal rate of return of 27.6 percent.

On the whole, the program was a cost-effective alternative, worth to consider to bridging the transit of youths from the informal to the formal sector in the future.

I.   Introduction

National and local governments in developing countries invest substantial amounts in providing human capital to their population through both formal and informal channels. Although it is well known that several developing countries have made notable improvements in their education coverage, much less is known about the quality of their education, and in particular, about the quality of the more informal education they provide in the form of training programs.

The case of Colombia is a good example and opportunity to illustrate these facts, and learn more on this topic. The country has made a huge effort to improve the skills of its working age population not only by increasing its formal basic and higher education programs, but in particular, it has substantially increased the coverage of formal technique and technological programs, and informal training programs. Nonetheless, with the notable exception of the work by Attanasio, Costas and Kugler (2006), the evidence provided to assess the quality of training programs in Colombia has not been strong enough to serve as a tool for deepening of reforming them.[1] In the core of this discussion there is also the need to identify the best way to provide such programs, either through supply subsidies by means of public institutions, or through demand subsidies in the form of scholarships for candidates to enroll in private institutions, and in each case, to identify the best arrangement among the public funder and the provider.

Colombia's *Servicio Nacional de Aprendizaje*, SENA, the public institution in charge of provide training program, was created in 1957 and has registered a huge increase in enrollment in its target population. Nonetheless, since it was created it has not been experimentally evaluated, or counted with the data design or methods to provide widely acceptable results. Fortunately, the country implemented between 2001 and 2005 a training program called *Jóvenes en Acción*, *JeA*, which allowed poor youths to access privately provided training programs that were funded by the government. Furthermore, among its target population, the program selected its beneficiaries by means of a lottery, which allows exploiting its experimental design to assess its results.

We use the experimental data of the *JeA* training program in 2005 in Colombia to assess its impact on a variety of outcomes obtained by merging the program's dataset to social and labor market administrative records, collected between July 2008 and June 2014. We find that the *JeA* program had a positive and significant effect on the probability to work in the formal sector, and that applicants in the treatment group contributed more months to social security during the analyzed period. Beneficiaries of the *JeA* program were also more likely to work for a large firm. Earnings of treated applicants were 11.8 higher in the whole sample, and they made larger contributions to social security. In addition, we also present non parametric bounds that for all demographic groups show that for some percentiles of the sample, there are positive and significant effects of the program. Thus, the effects of the program would have been capitalized both in increases in the likelihood of being formal, and increases in productivity.

We also find that for the whole sample of applicants, those in the treatment group have 0.24 more years of education, and have a probability of graduating from high school 5.6 percent higher than the control group. We find no significant effect on college and school attendance,

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[1] See Gaviria and Núñez (2002), Barrera and Corchuelo (2003), Medina and Núñez (2005), Sarmiento and others (2007), and Fedesarrollo (2009, 2010), among others.

fertility decisions, marital status or some dimensions of assortative mating. Among applicants matching to the census of the poorest population, we find that beneficiaries are more likely to be employed, presumably among this population, in the informal sector. Among this population, applicants also have higher earnings.

Finally, we find that the benefits of the *JeA* program are higher than it costs, leading to an internal rate of return of 27.6 percent.

On the whole, the program was a cost-effective alternative, worth to consider to bridging the transit of youths from the informal to the formal sector in the future.

We now provide the background of the *JeA* program, describe the data used in the paper and provide some descriptive statistics of it, present the estimated effects of the program and its cost-benefit analysis, and finally, we provide some conclusions.

II.   Background

The *Jóvenes en Acción* program was part of a wider strategy called *Red de Apoyo Social*, aimed at providing a safety net for the poorest population after the Colombia crisis of the late 1990s.[2] It was initially funded with a USD$70 million loan from the Inter-American Development Bank, and was targeted to unemployed youths 18 to 25, who belonged to the poorest population classified in the two lowest SISBEN levels.[3] As DNP (2000b) highlights, unemployment rates of youths between 18 and 25 years of age of the ten largest metropolitan areas, living in the first and second lowest deciles of the income distribution, were 62.8 percent and 52.8 percent respectively. That was why the program was offered in the seven main cities of the country: Bogotá, Medellín, Cali, Barranquilla, Bucaramanga, Manizales and Cartagena. According to FIP and DNP (2001), by 2001, the program planned to enroll between 2001 and 2003, about 100,000 students in these cities, but according to Attanasio et al. (2011), AKM, it actually began to enroll them in 2002, and by 2005, it had enrolled 80,000 students, somewhat less than 50 percent the original target population.

The program's goal was to increase the employability of these youths and provide them support for building was what called by the program, their *project of life*. More specifically, its specific objectives were: (i) to develop the occupational skills of youths so that they could increase their likelihood to become employed, and their performance at work, (ii) to promote the private supply

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[2] The Colombian crisis of the late 1990s is described by Medina, Núñez and Tamayo (2013). The first program of the *Red de Apoyo Social* was created by means of the CONPES policy document Number 3075 of March 15, 2000 (DNP, 2000a), and was aimed at generating employment for the poorest, while program *Jóvenes en Acción* was simultaneously created with program *Familias en Acción*, by the CONPES policy document 3081 of June 28, 2000 (DNP, 2000b), with a budget for both programs of USD$426 million, USD$320 million of which were to be provided by The World Bank and the Inter-American Development Bank.
[3] SISBEN is the acronym in Spanish for Information System for Beneficiaries Selection, and it is composed 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 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 SENA or any training institution were also eligible to *Jóvenes en Acción*.

of relevant job training for poor youths, and (iii) to put productive poor youths and training institutions closer to firms.

Jóvenes en Acción had three main components: (i) classroom training, (ii) on-the-job training, and (iii) the youth's *project of life* (FIP and DNP, 2001). The first component had itself two emphases, the development of basic abilities for becoming employed unconditional of the specific field, and the development of skills for a specific field. This component was expected to last between 280 and 350 hours. The first emphasis of this component sought to provide the youths with basic social abilities and developing their soft skills: teaching them to be proactive, resourceful and open to feedback; to improve their verbal and written communication skills; their analytic, deductive and daily work problems solving skills; to assimilate and understand their job's organizational environment; to develop teamwork skills, etc. The second emphasis of this component forms youths in the specific field of their interest, including the expertise in the use of equipment and tools, didactic material, and the procurement of services; products or services production, etc.

The second component consisted of three months of unpaid on-the-job training, and was about 480 hours long conditional on the labor schedule of the specific firms in which the youths were trained. The training institutions, ECAPs, write a *training plan* to facilitate the completion in the firm, of the process that began in their classrooms, which includes youths putting in practice the lectures received in the classes, to complement them, provide them with feedback, and develop each youth's *project of life*.[4] It also includes an assessment of the youths' performance in terms of their achievements, agreed upon by the firm, the ECAP, and the youth.

The third component, the *project of life*, seeks a comprehensive human development of the youths, orienting them towards a positive visualization of their abilities, personal and work perspectives; providing them with tools for decision making. It moves the youths to reflect on their work, their near insertion in the labor force, and its meaning with respect to their future labor perspectives, helping them build their labor identity. It also helps them to go deeper in knowing themselves, their strengths and weakness to face the labor market. This component takes place all through the six months of the intervention (See FIP and DNP, 2001, Annex 7).

The program also included a maintenance transfer during the six months of the program, of about USD$2.20 per day to trainees without children under seven years of age, and about USD$3.00 per day for women with children under seven.[5] Beneficiaries were also covered by personal accidents insurance, and by civil liability insurance.[6]

ECAPs and their requirements

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[4] ECAP is the acronym in Spanish for training institution.

[5] Transfers to women with children under seven year of age were not contingent on the number of their children under seven, and were paid weekly per day in which beneficiaries had attended their courses during their classes, and biweekly, after they completed their training, during their training period. See FIP and DNP (2001), and AKM.

[6] No health insurance is considered because it is expected that for belonging to the two lowest Sisben levels, beneficiaries should be covered by the Subsidized Regime, which covered by then nearly 60 percent of the health services included in the Contributive Regime, the health insurance available to formal workers. See FIP and DNP (2001).

Institutions interested in offering the program could be national or international, formally established, having as one of its social goals the provision of job training programs, with the required infrastructure, technique expertise, experience, and economic solvency that could allow it to provide a high quality program, and appropriately manage the public economic resources of the program. Institutions could be private, public or mixed, and among the public they could be different to the SENA, provided that their public resources were not used to provide the *Jóvenes en Acción* program, so that they had no advantage with respect to their private competitors. AKM report that 43 percent of them were for profit.

According to AKM, in 2005 there were 114 ECAPs offering 441 courses to 26,615 trainees, with their instructors teaching about 7.6 hours per day.[7] Appendix Table A1 includes the broad topics of the main categories offered by the ECAPs.

Incentives to EACPs

The design of the program had a scheme of payments that seeks to move ECAPs towards their efficiency in forming students in high quality pertinent fields, and making an appropriate assignment of them to firms: (i) when the beneficiaries were hired: in this case, and provided their employment is in a field related to the classes they received, and that the contract is for at least six months, the ECAP is allowed to receive the amount of maintenance transfers the beneficiaries had not received at the moment they were hired; (ii) system of payments: its goal is to make students to enroll and complete the course they applied for, and is such that the ECAP does not receive any payment if the students dropped out before completing at least 20 percent of the coursework (although the students could be replaced by other), it receives 30 percent of the cost if the student completed less than 80 percent of the coursework, 40 percent if the student finishes the coursework but is not hired as a trainee in a firm, 45 percent if the student is hired with delay as a trainee but does not fully finish it, 50 percent if the student is hired on time (within 5 working days) as a trainee but does not fully finish it, between 80 and 90 percent if the student is hired with delay (between day 6th and 10th, and between days 11th and 15th respectively) as a trainee and does fully finish it, and 100 percent if the student is hired on time as a trainee and does fully finish it. The total cost of the program is the sum of the cost of the course, plus the maintenance transfers disbursed during the six months (See FIP and DNP, 2001).

This incentives scheme is one of the most innovative characteristics of the program, mostly when we compare it with the one that is been in force since 1957 at SENA, Colombia's public institution for providing training to its population, where a supply system has made it an inflexible way of providing job training programs, with courses that are not always pertinent to firm's needs (See Saavedra and Medina, 2013).

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[7] 40 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. The total amount invested was US$22 Millions (See *Ministerio de la Protección Social*, 2005)

Eligible firms for on-the-job training

Firms are required to be formally constituted according to two criteria: (i) being registered at a Chamber of Commerce, and (ii) having a valid unique tax register. AKM report there were 1,009 firms participating in the program, in the manufacturing, retail, trade and services sectors. They offered 5.2 hour per day of on-the-job training.

III.   Data

1.   Experimental Data of the *Jóvenes en Acción* Program

The data we use is taken from the fourth call for applications of the *Jóvenes en Acción* program, the first of those that was based on an experimental design. The call took place between July 2004 and May 2005, enrolled 26,615 youths of the main seven cities, and the randomized sample was selected in mid January (AKM) out of the census of registered applicants. ECAPs allowed up to 50 percent more applicants than the available places they had for the courses.[8] Applicants were randomly selected from the program's information system, and the moment at which randomization took place was after applicants had been declared as eligible.[9]

Data used in our study is a random sample of the universe of applicants. In the process, this sample was selected to insure it would include a balanced share of each city, each gender, and each treatment and control group. The share of individuals included in the random sample was evenly split between treatment and control. AKM had a conservative target sample size of 4,350 applicants, but finally they collected a baseline survey that included 4,353 applicants, 2,066 treatment and 2,287 controls. According to AKM, only 3,956 of these applicants were actually randomly assigned to the program.

Table 1 shows in columns (1), (3) and (5), the mean level of the characteristics of all the control group applicants included in the 3,956 randomly assigned applicants, and by gender. It also includes in columns (2), (4) and (6), the difference between applicants in the treatment and the control groups, after controlling for the site-by-course fixed effects.[10] Women's characteristics are balanced by treatment status in most variables, nonetheless, treated applicants in the baseline were more formal, had longer tenure, more years of education, and were younger. Treated men were more likely to have a paid employment, earn less as self-employed, and additionally, they were also educated, were younger, and less likely to be married. On the whole, for women we cannot reject the hypothesis of characteristics being jointly equal for treatment and control groups, as it is shown in the *F* test in column (4) at the bottom of the table. For men and the whole sample, we can reject the hypothesis. To account for the mentioned differences in

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[8] There was actually a mean (median) surplus of 18 (23) percent applicants (Attanasio et al., 2011). The ECAPs were required to fill 100 percent of the available slots before they could begin their courses, and the randomization process offered a specific list of ordered candidates to select from, whenever they had to fill the slots of previously selected applicants who had decided not to register (DNP, 2008).

[9] Although randomization took place at the class level, we only have information on each type of course offered by ECAP. If a course in an ECAP had two or more courses, each with more than one class, we could only control by site-by-course effects rather than by site-by-class effects (See Attanasio et al., 2011 for more details).

[10] We include the variable tenure to provide an idea of its level rather than to test differences between treatment and control in that variable, since it is only available for 3,671 applicants in the baseline.

covariates at the baseline, and get more accurate estimates, we control for all the covariates in the table when estimating the program effects.

2. Administrative Records

Our follow up data consists of administrative records contained in two different datasets: the SISBEN survey, and the SISPRO.[11]

(i) The Sisben Data

The Sisben survey we use was collected between mid 2009 and mid 2010, and at that time, it was census of nearly the 57 percent poorest population of the country.[12] The Sisben data is used to estimate and indicator, the Sisben score, used to target social expending like health insurance, conditional cash transfers, etc. Thus, we have a cross section follow up with the matched applicants', and their households' characteristics, at the date their surveys were collected. Since this survey is a census of the poorest population, applicants that were not matched to it are assumed to belong to the upper income levels of the population.

(ii) The SISPRO Data

The SISPRO contains information of the PILA, the national information system used for firms to report their mandatory contributions to health, pensions and professional risks they pay for their employees.[13] Firms must report each month the amount contributed per each one of their employees, and the Ministry of Social Protection has universal coverage of all the monthly contributions in the country since August 2008.[14] Thus, this data provides us with follow up monthly panel data for the matched applicants between August 2008 and July 2013. Since the PILA contains the monthly census of all contributions in the country, applicants that were not matched to it are assumed to belong to the more informal labor market, the in which workers do not contribute to their health, pensions or professional risks.

IV. Descriptive Statistics

In this section we present some descriptive statistics of the outcomes related to formal employment, in particular, the average earnings of the applicants, the average earnings of applicants matched to PILA, and the likelihood of matching to PILA. Figure 1 shows the average earnings of applicants, in which we use their observed earnings in PILA for those matched, and

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[11] Sisben is the acronym in Spanish for System of Beneficiaries Selection, and SISPRO is the acronym in Spanish for Integrated Information System of Social Protection.

[12] The SISBEN survey began to be collected in 1992 by all Colombian municipalities, and after its collection, whenever any household wanted its information to be updated, it had to require it to its municipal Department of Planning, in charge of the local SISBEN administration. The SISBEN was updated for first time for the whole country between 2003 and 2005, and the second time it was updated was between 2009 and 2010. Since the first time it was updated, its records became much more reliable and its score much less manipulable (See Bottia et al., 2012).

[13] PILA is the acronym in Spanish for the Unique Form for Liquidating Contributions.

[14] Actually, the SISPRO contains PILA information since January 2008, nonetheless, it only reached full coverage by August that year.

zero for the ones who did not matched. The sample is weighted by the weights we indicated above we will use to estimate equation (1). There is a clear gap in favor of beneficiaries of the *JeA* program, which is slightly increasing over time. The figure captures what we expect to estimate later as the total effect on earnings, arising partly from a higher likelihood of being a formal employee, that is, of matching to PILA, and from increases in productivity, that is, higher earnings derived of the acquired skills. Figure 2 shows the mean earnings only of applicants who did matched to PILA, and it shows that earnings are very similar until 2011, and then they begin to become higher for the beneficiaries, with a larger gap for men than women. The figure captures the productivity effect of the program, which, due to self-selection of applicants into the group of matched individuals to PILA, does not provide an experimental estimate of the impact of the *JeA* program. We will address this issue later by means of the estimation of non parametric bounds. Finally, Figure 3 shows the effect of the program on the probability of matching to PILA, which shows a similar pattern to the one observed in Figure 1. Altogether, the figures suggest that most of the effects on earnings of the *JeA* program are capitalized by beneficiaries in the form of a higher probability of becoming a formal employee, rather than by means of an increase in their productivity.

V.   Program Effects

We exploit the experimental design of our sample to get estimates of the *JeA* effects on our outcomes of interest using the following regression model

$$Y_{ij} = \alpha D_i + X_i \beta + SC_j + \varepsilon_{ij} \tag{1}$$

Where Y_{ij} is the outcome of person i in site and course j, D_i is the treatment variable, and it is equal to 0 if the applicant was randomly denied a slot in course j, or 1 if he was randomly offered a slot in it, X_i is a vector of characteristics of the applicants that include employment, paid employment, whether applicant has a contract, whether working in the formal sector, wage and salary earnings, self-employment earnings, days worked per month, hours worked per month, education, age, and marital status. We also control for gender when we obtain the estimate with all the applicants. Finally, and considering that randomization in the experimental design took place at the course level, we include SC_j, a site-by-course fixed effect, which allows us to interpret our parameter of interest as the weighted average of the program effects across all courses, and unbiased estimator of α. Since the experimental sample was chosen so that there would be about the same number of treatment than control applicants, but in the population the share of accepted applicants varied much more across the different courses, we weigh each applicant in (1) 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 slot. Finally, ε_{ij} is a random error term. In all of our estimates below we obtain robust standard errors.

Our estimate is thus the "intent to treat" effect, that is, the mean effect of the offer of treatment. As AKM notice, since compliance among applicants is 97 percent, our estimate should not substantially differ from the treatment on the treated effect.

We assess the effects of the *Jóvenes en Acción* program on the probability that applicants had a formal employment, the probability of working for a large firm, the length of their tenure in the formal sector, their wages in the formal sector, the probability of their being eligible for government subsidies, their marital status, and finally, on their fertility.

A. Effects of the *JeA* on the Performance in the Formal Labor Market

The labor market in Latin American countries is highly informal, and depending on the criteria to define informality, the Colombian labor market ranks either at the average of the Latin American countries, or above the average. Perry et al. (2007) present figures showing that the share of the labor force not covered by a pension scheme in Latin American countries is about four times as large as it is in advanced countries, while the share of labor force self-employed is about twice as large. Colombia ranks close to the average informality in the region when it is measured according to the share of sales not reported (for tax purposes) by firms, the share of workers not contributing to their retirement, ranks above the region according to whether the worker is unskilled self-employed, or salaried worker in a small private firm, or earns zero-income, and it is also above the region in the single concept of the share of self-employed workers. Finally, informality in Colombia is below the mean of the region according to the share of the labor force not covered by a pension scheme. According to Medina et al. (2013), when informality is measured based on the definition adopted by the International Labor Organization, since the mid 1980s, informality has fluctuated in Colombia from 53 percent to 60 percent; while according to the share of self-employed workers with less than higher education, it fluctuated in the same period between 30 percent and 38 percent. Cárdenas and Mejía (2007) report levels of informality in the Colombian labor market around 60 percent between 2000 and 2005. The education level of workers has played a crucial role at the moment of determining their likelihood they work in the formal sector in Colombia. While the share of wage earners with no higher education has reduced from 27 percent in the mid nineties to 18 percent the working age population by 2010, the share of workers with some higher education has increased from 10 percent in the mid nineties to 18 percent in 2010.

Informality among youths, the main target population of job training programs, has much higher levels than in its whole working population. According to Saavedra and Medina (2013), informality among youth is around 80 percent when it is defined based on their firm's size, but it is just 60 percent when we consider the adult population between 19 and 55 years of age. AKM provide evidence of the positive effects of *Jóvenes en Acción* on the likelihood of working in the formal sector. They consider two different definitions of formality: one for workers covered by health, pension, and injury insurance, and other for workers with a written contract. They find positive effects on the likelihood of working in the formal sector, and on wages in the formal sector, and for women, on average earnings overall.

In this section we analyze several outcomes of the labor market, in particular, those that can be assessed based on the information contained in the PILA of the Ministry of Social Protection, which allows us to know whether each applicant was employed in the formal sector of the economy, defined as one in which their employer were paying the mandatory contributions to health, professional injury insurance and pensions. The PILA provides us with this information, along with the applicants' earnings in the formal sector, for each month contained between August 2008 and July 2013. In this section we estimate the effects of the *JeA* program on the probability of working in the formal sector, on the number of months and days applicants worked in the formal sector in the analyzed period, on tenure, on the probability of working for a large

firm, on formal earnings and on the amount of taxes applicants paid to the government and the contributions the paid to the social security system.

1.   Formal Employment

Table 3 presents the effects of *Jóvenes en Acción* on the likelihood of working in the formal sector, defined as working for an employer that pays your contributions to health insurance, injury insurance or pensions. We know whether someone's employer pays his contributions in any specific month between July 2008 and June 2014, whenever we could match in that month his identification number to the Ministry of Social Protection PILA dataset. Since the PILA contains the census of all workers contributing to the Colombian social security system, the estimates we obtain from our experimental data provide us with unbiased estimates of the *JeA* effect on the likelihood of working in the formal sector, based on this definition. We report the results for all applicants and for women and men separately. For each of these demographic groups, we include in columns (1), (3) and (5), the mean of the likelihood of matching with the PILA among applicants who were not offered training, and the coefficient on the treatment variable obtained from estimating equation (1) with the baseline control variables, including the site-by-course fixed effects, and weighting the observations as explained above. The table has five types of outcomes. We first estimate equation (1) using one observation per applicant to construct our first outcome variable equal to one if the applicant matched at least in one month between July 2008 and June 2014, and zero otherwise. We then use one observation per applicant and year, to construct our second outcome variable equal to one if the applicant matched at least in one month of each year, and zero otherwise. Finally, we use the whole panel by month to assess whether each applicant matched or not in each of the months contained in the analyzed period. We also report the effects of the program on the number of months and days each applicant matched to the PILA.

Applicants matched to the PILA might have reported in it all or just some of their contributions to health, injury insurance, or pensions. In Table 3, we report the results obtained by defining our outcome variable as one if the person made at least one of the three contributions and zero otherwise.[15] Likewise, we include the maximum number of months and days they reported to have made to any of those three contributions.

Table 3 shows that the *JeA* program had a positive and significant effect on the likelihood to be formal for all dependent variables and demographic groups considered. When we consider the first outcome of the table, whether the applicant matched to the PILA in at least one out of the 72 months considered, we find that applicants who were offered *JeA* were 5.6 percent more likely to have been formal than those who did not receive that offer. Women and men were 7.3 and (a non-statistically significant) 4.2 percent more likely respectively. Table 3 shows that there are 1,907 applicants in the control group observed in 72 months. To get the standard errors of our estimates, we correct them for random effects at the applicant level. Among all applicants that had not been offered *JeA*, 64.5 percent were matched to the PILA at least one month in the analyzed period. With respect to the likelihood of the control group, the likelihood of those offered *JeA* is 8.7 percent higher: 13.1 percent higher for women and 5.6 percent higher for men.

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[15] The results are very similar if we consider only one specific contribution at the moment of defining formality, or the number of contributed months or days.

The *JeA* effect becomes higher in relative terms, as we consider a more disaggregated measure, as it is when we analyze its impact on our second outcome, the likelihood to have been formal in at least one month of each of the years in the analyzed period. Even though the average likelihood of having been formal for at least one month in each of the years considered was only 4.6 percent higher for applicants that were offered *JeA* (versus a likelihood 5.6 percent higher of having been formal at least one month in the whole period), since in this case the average for the control group was even much smaller, 47.3 percent, this effect represents a relative increase of 9.7 percent in the likelihood of those receiving the *JeA* offer having been formal versus those that did not. In the case of women and men, the relative increase is 14.2 and 8.4 percent respectively.

When we analyze the effect of being offered *JeA* on the likelihood to have formal in every month of the analyzed period (outcome 3 in the table), here again, the likelihood of having been formal was just 3.6 percent higher for applicants that were offered *JeA*, but then again, the likelihood of having been formal in every month of applicants who did not receive that offer was even much smaller, 34.3 percent, so that the *JeA* effect represents a relative increase of 10.5 percent in the likelihood of those receiving the *JeA* offer having been formal versus those that did not. In the case of women and men, the relative increase is 16.5 and 9.4 percent respectively.

Table 3 also presents the effects of being offered *JeA* on the number of months and days applicants contributed between July 2008 and June 2014, a total of 72 months. Focusing again on the results obtained once considering all contributions, and controlling for the available covariates, it shows that in the whole sample, while applicants in the control group contributed an average of 24.7 out of the 72 months, those in the treatment group contributed 2.6 additional months, that is, 10.4 percent more. The relative increase for women and men in the treatment group, compared to those in the control group, was 16.6 and 9.4 respectively. There is a slightly higher effect when we use the number of days contributed in each month once we consider the 72 months of the period, and it shows that while the control applicants contributed on average 9.5 days per month, the treatment applicants contributed one additional day. In relative terms, the increase in the number of days contributed due to the *JeA* program is 10.4, 17.0 and 9.6 percent respectively for the whole sample, and the sample of women and men respectively. Finally, when the outcome is the sum of the number of days that applicants contributed during the whole period, in relative terms, the applicants that were offered *JeA* contributed 11.4, 19.0 and 10.2 percent more in the whole sample, and the samples of women and men respectively.

Table 4 repeats the estimations in columns (2), (4) and (6), in Table 3, but now it additionally includes the interactions of the treatment variable with all of the baseline covariates. As shown by the first *F*-test included at the bottom of the table, correlates do not differ between applicants that were offered *JeA* and those that were not, in the full sample of applicants, or by gender. When we use the data from the contributions to injury insurance, pensions, we obtain similar results.[16]

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[16] For the whole sample and for women there are no systematic differences in correlates between treatment and control. In the case of men, the p-value is 0.093, 0.115 and 0.079 when we use the contributions to injury, pensions and the pooled contributions respectively.

2. Tenure

Table 5 presents the estimated effects of the *JeA* program on the number of months (panel *A*) and days (panel *B*) of tenure. Since the PILA contains the census of all workers contributing to the Colombian social security system, the estimates we obtain provide us with unbiased estimates of the *JeA* effect on tenure in the formal sector, since although other applicants not matched to the PILA might be working in other firms, as long as they did not match with PILA, they would not be working in the formal sector according to our definition. The OLS estimates use four different dependent variables. The outcome "panel", includes all tenure spells of each individual, and constructs an unbalance panel data with those spells. In addition, they include the mean, maximum and minimum observed tenures between July 2008 and June 2014. Once we control for the baseline covariates, the only negative and significant effect we find is for the minimum tenure, which implies that *JeA* decreases the minimum number of days and months of tenure in 11.8 and 0.36 respectively, for the whole sample.

3. Probability of Working in a Risky Position, Working in a Firm With at Least 200 Employees, and Being a Dependent Worker

Table 6 presents the estimated effects of *JeA* on the probability of working in a risky position, working in a firm with at least 200 employees, and being a dependent worker. The table presents three sets of results based on the data considered in the estimation. Panel A includes only the applicants that were matched to PILA, panel B includes the balanced sample of applicants that were matched to PILA at least one month between July 2008 and June 2014, and panel C includes the whole balanced dataset with all applicants, with zeros for applicants not matching to PILA in a specific month.

Note that since the PILA contains the census of all workers contributing to the Colombian social security system, the estimates we obtain provide us with unbiased estimates of the *JeA* program on the probability of working for a large firm, and the probability of being a dependent formal worker. Regarding the outcome related to the likelihood of working for a large firm, although other applicants not matched to the PILA might be working in other firms, it is very unlikely that they would be working for a firm with at least 200 employees, since it is unlikely that firms that size would not be paying the mandatory contributions of their employees. Applicants might as well be dependent workers in the informal sector, but anyone in the formal sector must have been matched to PILA.

A different situation emerges when we consider as outcome whether the applicant is working in a riskier position, defined as the tariff workers must pay according to the implicit injury risk of their occupation. Since applicants not matched to PILA might be performing tasks as or even riskier than those performed by those matched to PILA, the estimates presented in Table 6 are potentially biased. To obtain unbiased estimates in this case, we will present below estimates based on nonparametric bounds. We still present the OLS results in Table 6 to illustrate the differences with the results based on bounds presented below.

Let us analyze the estimates presented in columns (2), (4) and (6) of the table. First, the three panels show that beneficiaries of *JeA* work in jobs as risky as the jobs of non-beneficiaries,

except for the case of women when we consider the result in panel C, which shows that they work is riskier positions.

When we consider the results in panel C, we find that 18.7 percent of the applicants in the control group work for firms with at least 200 employees, and that share among applicants in the treatment group is 2.2 percent higher by virtue of the *JeA* program, which implies a relative increase in the likelihood of working for a large firm of 10.7 percent. The relative increase in the likelihood of working for a large firm due to the *JeA* program is 25.7 percent for women, and 10.0 for men, although it is not significant for men. Only for women the results in panel C are similar to those in panels A and B.

Finally panel C shows a positive effect of *JeA* on the likelihood of applicants being dependent workers. For the whole sample, they are 10.7 percent more likely to be dependent that the control group, 15.6 and 9.3 for women and men respectively.

4. Formal Earnings

Table 7 presents the estimated effects of the *JeA* program on formal earnings. To obtain our estimates, we define the dependent variable as the wage reported by the firms that pay the contribution of the applicants when they were matched to the PILA, and zero otherwise, that is, in case they were not employed, or were employed in the informal sector. The whole sample in estimation 3 shows that applicant who were not offered training had average monthly earnings of COP$245 thousand, and those who were offered earned COP$29 thousand more, that is, their earnings were 11.8 percent higher.[17] Treatment women and men earned a significant premium of 17.9 and 12.0 percent above their control counterparts. Similar relative gains are estimated when we consider the daily earnings.

Figure 4 presents a grid obtained by estimating equation (1) each month and plotting the estimated coefficient with its 90 percent confidence interval. It shows a higher and more volatile positive effect on men than women, and a slightly increasing pattern of the effect for the whole sample.

Nonparametric Bounds

To assess whether treatment applicants working in the formal sector actually earn more that control applicants working in the formal sector by virtue of the *JeA* program, we have to consider that since the probability to work in the formal sector is strongly affected by treatment status, the sample of individuals selected into the formal sector is not anymore a random sample of the experimental design, but one self-selected, in which there usually are more treated than control applicants, and because of that, there are relatively more less skilled treated than control applicants, which might underestimate the effect of the program based on that data.

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[17] Although the average earnings of the control applicants was only COP$245 thousands when we assign a zero to those not matched to PILA, the average among those that were matched to PILA was COP$714 thousands, which is 54.7 and 15.9 percent above the minimum wages of 2008 and 2014 respectively.

To provide a figure that could allow us to learn of the effect of interest in this case, we proceed to estimate non parametric bounds of the effect with the population that is observed working in the formal sector. Suppose that winning the scholarship was never harmful, that is, $Y_{1it} \geq Y_{0it}$. This is the *monotone treatment response* assumption used by Manski (1997), and Angrist et al. (2006). The observed test-taking status T_{it}, is equal to one if earnings of applicant i are observed in period t, and it is equal to zero otherwise. Furthermore,

$$T_{it} = T_{it}D_i + T_{it}(1 - D_i)$$

We also assume that $T_{1it} \geq T_{0it}$, as Angrist et al. (2006) do, the same monotonicity assumption used by Lee (2009). Define $q_0(\theta)$ as the θ-quantile of the distribution of Y_0, and $q_1(\theta)$ as the θ-quantile of the distribution of Y_1. Following Angrist et al. (2006) we get to the following equation

$$
\begin{aligned}
E[YT|D &= 1, YT > q_1(\theta)] - E[YT|D = 0, YT > q_0(\theta)] \\
&\geq E[Y_1 - Y_0|Y_0 > q_0(\theta), T_0 = 1] \\
\geq E[YT|D &= 1, YT > q_0(\theta)] - E[YT|D = 0, YT > q_0(\theta)] = \Delta_{LB} \quad (2)
\end{aligned}
$$

Intuitively, to obtain the lower bound we begin in the first line of each panel including all the control applicants that have been matched to the PILA, which fraction among all control applicants is θ, and those beneficiaries of the *JeA* program that were matched to PILA and had earnings at least as large as those of the lowest earner among the control group. To obtain the upper bound we consider only those applicants beneficiaries of the *JeA* program that are included in the θ percent with the highest earnings among those matched to PILA, that is, we include beneficiary applicants with $Y_1 \geq q_1(\theta)$. The implicit assumption is that there is going to be a larger fraction of applicants matched to PILA among the beneficiaries than among non beneficiaries of *JeA*, so that the upper bound is always estimated with applicants with higher average earnings that those considered when estimating the lower bound.

We first get an estimate of these bounds, and then proceed to narrow them, by adding Lee's (2009) Independence Assumption. Let X be a vector of covariates, and let $(Y_{1it}, Y_{0it}, T_{1it}, T_{1it}, X)$ be independent of D (which in the case of PACES is true by randomization). We proceed to define

$$\Delta_X^{LB} = E[YT|D = 1, YT > q_0(\theta), X] - E[YT|D = 0, YT > q_0(\theta), X]$$

and

$$\Delta_0^{LB} = \int \Delta_X^{LB} dF(X) \quad (3)$$

where $F(X)$ is the distribution function of X conditional on $D = 0$ and $T = 1$. That is, Δ_0^{LB} is the expected value of the lower bounds estimated over each subset of the domain of X.

Table 8A shows the estimated non parametric bounds by demographic group obtained using equation (2), based on the applicants' contributions to health, disability, pensions, and the maximum of them. In each case, we estimate lower and upper bounds at different percentile levels. First, including all control individuals matched to PILA, and then, including only those on top of the 75th, 85th and 95th income percentiles. The table shows that the *JeA* program has high and significantly positive upper bounds, but no significant lower bounds.

Table 8B shows the estimated non parametric bounds by demographic group once we condition on the ECAP applicants attended, according to equation (3). In this case, we find positive and significant effects for some demographic groups and percentiles. Note that whenever there are in a specific ECAP, more control than treated applicants, the implicit assumption of the model does not hold, and thus the intuition behind the bounds does not apply. This situation is very likely to happen as we split the data by ECAP, since it becomes increasingly more likely to have some ECAPs in which there are more control than treated applicants matched to PILA. Whenever we find that the upper bound estimate is smaller than the lower bound, we conclude that the implicit assumption behind the approach does not hold, and we omit the analysis of those specific results (lighter figures in Table 8B).

As Table 8B shows, there are positive and significant effects of the *JeA* program on earnings for the whole sample of men and women, and for the sample of men, in the 75 and 85 percentiles. When we consider the 75 percentile, we find that the *JeA* program increases the earnings between 5.2 and 5.5 percent with respect to the control group, for the combined sample of men and women, and between 6.3 and 9.2 percent for the sample of men. In the case of the 85 percentile, the positive effect of the program ranges between 4.9 and 9.9 percent for the whole sample, and between 8.6 and 9.2 for the sample of men. For women the results are less robust across contributions, but still we find positive effects for the 75 percentile in the contributions to disability and pensions of a magnitude between 10 and 11 percent.

There is no consistent result with the implicit assumptions behind the bounds approach for the lowest ("initial") and the highest (95) percentiles.

5. Contributions

Table 9 presents the estimated effects of the *JeA* program on the amounts of taxes and contributions paid by applicants. The first contribution considered is the *Retefuente*, which is a tax paid on monthly earnings that can be deducted by the higher earners of the country from their annual income tax. The *Retefuente* is not paid by all workers but by the highest income earners and it is mandatory up to a floor of monthly earnings. The first indicator considered in Table 9 is the probability of being actually paying the *Retefuente*, as a measure of whether the earnings of applicants are above the threshold that requires its payment, which in 2014 was COP$2.6 million. Neither on the likelihood of being paying the *Retefuente*, which is negligible, nor on its amount, does the *JeA* program have a significant effect.

The effects of the *JeA* program on the contributions to health and pensions, is very similar in relative terms to the ones found on earnings, given that these contributions are a fixed percentage of earnings in the analyzed period. In the case of pensions, the relative increase in contributions of *JeA* beneficiaries versus non beneficiaries is 12.3, 17.6 and 13.0 percent for the whole sample, and the sample of women and men respectively. Similar effects are found on the contributions to health, *Cajas*, ICBF and SENA.[18]

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[18] The *Cajas* are private social entities created to administer a family subsidy for low wage employees with children, and to provide recreation for their members. The government allows them to provide other services like health insurance, job training programs, etc. They are funded with a 4 percent payroll tax on all formal sector workers. ICBF is the acronym in Spanish for Family Welfare Colombian Institute, the public entity in charge of

The *JeA* program does not have any effect on the amount contributed to injury, which in that case is not a fixed share of earnings but an amount that depends of the injury of risks faced by applicants at work. It does not either have an effect on contributions to MEN, the Pension Solidarity Fund, or the ESAP.[19]

The final outcome of Table 9 is the aggregated effect on all contributions considered in the table, and it is positive and significant for the whole sample and the sample of women. In those cases, beneficiaries of the *JeA* program contribute 9.4 and 16.0 percent more than non beneficiaries.

B. Effects of the *JeA* Program on Socioeconomic Outcomes

In this section we present the estimated effects of the *JeA* program on socioeconomic outcomes obtained from the matching of the experimental *JeA* data with the SISBEN survey, in particular, with the SISBEN survey that was collected between mid 2009 and mid 2010. The outcomes considered in this section are the applicant's education, fertility, marital status, and the labor market outcomes that can be obtained from the SISBEN survey, which could include information of applicants in both the formal and the informal labor sectors, but since the survey is targeted to the poorest 60 percent of the population, it actually contains mostly information of those applicants in the informal sector.

1. Probability of Matching to the SISBEN Survey

The first outcome we consider in this section is the probability the applicants had been matched to the SISBEN survey: applicants more likely to match to the SISBEN survey could be considered to be more vulnerable, to the extent that they belong to the universe of households the government considers to be that could become eligible for public subsidies.

As Table 10 shows, about two thirds of the whole sample of applicants in the control group was matched to the SISBEN survey, 66.7 and 64.4 percent of the women and men respectively. Nonetheless, the *JeA* did not make any difference on the likelihood of applicants matching to the SISBEN survey, which shows that even though the program might have had sizable and significant effects at the applicant level, those effects might not have been enough for their households to have shown a significant improvement in the more long-term quality of life characteristics, so that they standards could differentiate from those of applicants in the control group within a period of about five years between they graduated and they were observed in the SISBEN survey.

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implementing the policy for child and youth care, and it was funded until early 2013 with a 3 percent payroll tax on all formal sector workers, and then, it began to be funded with national budget resources. Finally, SENA is the acronym for National Service for Learning, the public entity in charge of providing job training, technique and technological programs, and it was funded until early 2013 with a 2 percent payroll tax on all formal sector workers, and then, it began to be funded with national budget resources.

[19] MEN is the acronym in Spanish for the National Ministry of Education, the Pension Solidarity Fund collects a contribution targeted to the highest wages, to fund the pensions of the poorest, and the ESAP is the acronym in Spanish for Public Administration School for Higher Education.

2. Education

We consider four outcomes related to education in Table 10: the number of years of education, whether applicants graduated from high school, whether applicants were attending college at the time of the SISBEN survey, and whether applicants were attending school at the time of the SISBEN survey. The estimates show that for the whole sample of applicants, applicants in the treatment group have 0.24 more years of education, and are 4.0 percent more likely of having of graduating from high school, than those in the control group, implying relative increases of 2.4 and 5.6 percent respectively, with respect to the outcomes of applicants in the control group. College and school attendance effects are positive but insignificant.

3. Fertility and Marital Status

There are reasons to expect the *JeA* program could have effects on fertility, despite the short duration of the *JeA* program and the fact that at the baseline less than 10 percent of the applicants in our sample were below 19 years of age. The short duration of the program would make us expect that the incarceration effect, which would imply that applicants would delay fertility as they are occupied studying, should not be very large. Nonetheless, there have been interventions just twice as large that have had sizable incarceration effects, as it was found by Grönqvist and Hall (2013) in the case of Sweden, where the increase in the length of the upper secondary school from two to three years, reduced the likelihood that women before 20 had a child, but still did not affect the rate of births beyond age 20, which leads them to interpret it as an incarceration effect. Cygan-Rehm and Maeder (2013) also find that extending mandatory schooling from 8 to 9 years in West Germany had a causal incarceration effect on teen fertility rates.[20]

Still the incarceration effect is not the only mechanism through which *JeA* could affect fertility rates but also through the human capital effect, which by changing the applicants' opportunity costs of childbearing, could affects fertility decisions. Black, Devereux and Salvanes (2008) also find effects of compulsory schooling laws on fertility in the US and Norway, although they are mostly important at ages not binding the law. Cygan-Rehm and Maeder (2013) also find negative effects of education on fertility, mostly by the postponement of first births away from the teenage years. Yet other potential channel through which more education might affect fertility decision could be by affecting the bargaining power of women, as Komura (2013) suggests, and Varanasi (2009) and Chicoine (2012), empirically provide support of. Finally, Breierova and Duflo (2004) find evidence of differential effects of wife's education versus husband's education, with wife's education having a larger effect on fertility.

Table 11 presents the effects of *JeA* on a variety of fertility outcomes. First, it compares the number of children treatment and control applicants had at the moment of the SISBEN survey: whether applicants had at least 1, 2 or 3 children. In none of these outcomes we find a significant effect of the program. We then assess whether the number of children applicant had 9 months after the end of the program was different between treatment and control applicants, and for those outcomes again, there is no significant effect of the program, neither of the number of months until they had their first child after finishing the program. Finally, the program did not

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[20] See also Dufflo, Dupas and Kremer (2012) and Black, Devereux and Salvanes (2008) among others.

have any effect on the applicant's marital status, neither on the likelihood of the applicant being single with children.

We assess in Table 12 the effects of the *JeA* program on assortative mating by estimating whether the spouses of the treated applicants have more similar earnings, age, and years of education. First, we assess whether the program had an effect on the likelihood of applicants having their spouses' information, and we find that the program has no effect on it. Then we estimate its effect on the difference between the applicant's income, age and years of education, and the one of their spouses, and do not find any significant effect either.

4. Labor Market

The SISBEN survey collects information on the main activity of applicants last month, including no activity, working, unemployed, studying and others. Applicants also report their total earnings in the last month, and whether they were covered by any private health insurance. We use this information to construct labor market outcomes at the time of the SISBEN survey. First, we estimate the effect of the *JeA* program on the likelihood of applicants being participating in the labor market either as employed or unemployed. According to Table 13, 79.6 percent of matched men were participating while only 46.0 percent of women, for a total of 61.2 percent of applicants in the whole matched sample participating in the labor market. We find no significant effect in this case. Then we assess whether the program affected the likelihood of applicants being employed at the time of the survey. Nearly 50 percent of the control applicants were employed at that time, 36 and 65 percent of control women and men respectively. Beneficiaries of the *JeA* program are 4.5 percent more likely to be employed at the time of the survey, and women 6.9 percent more likely. These effects represent a relative increase of 9.1 and 19.2 percent with respect to the mean of the control group. 14.3 percent of men and 10.1 percent of women were unemployed at the time of the SISBEN survey, and we do not find any significant effect of the *JeA* program on their likelihood to be unemployed.

To assess the effects of the program on formality, we consider whether applicants were enrolled in any private health insurance company as proxy of labor formality. As a second proxy we additionally require them to be working at the time of the survey. A third proxy requires, in addition to the previous requirements, that applicants earned more than the minimum wage. In none of these three outcomes does the program have a significant effect. Applicants might be enrolled to any private health insurance as beneficiary of some of their household members, or they might be strategically responding to the interviewer by reporting a main activity different to employment even if they were working and paying their own health insurance, or they might just be underestimating their earnings.[21]

Finally, we assess the effects of *JeA* on the earnings reported in the SISBEN survey. We first notice that average earnings in this survey are similar to the ones estimated based on the PILA dataset and reported in Table 8. In Table 13 nonetheless, 51.8 percent of control applicants report to have earnings higher than zero, while in Table 8 only 31.9 percent do. Control applicants who report positive earnings in SISBEN, coming both from the formal and informal sectors, have average earnings of COP$418,506, and those in PILA, which include only applicants working in

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[21] See Bottia et al. (2012) for an analysis of strategic response in the SISBEN survey.

the formal sector, report average earnings of COP$641,364. The fact that average earnings are the same when we consider the control group in the PILA analysis and when we consider it in the SISBEN analysis becomes relevant because it implies that the relative increase due to the *JeA* program we estimated with the PILA, would be the same if we imputed to those not matched, their informal earnings rather than a zero, and if those earnings were actually similar to those applicants reported in the SISBEN survey, which seems very plausible. We find a positive effect of the *JeA* program on the reported monthly income, and on the likelihood of applicants earning more than the minimum wage. Earnings of beneficiaries of the *JeA* program are 19.2 percent higher, and have a likelihood 4.2 percent higher of earning more than the minimum wage, that is, 17 percent more than the control's mean.

VI.   Cost-Benefit

Table 14 presents a simple cost-benefit analysis for the whole sample, women and men. The panel on the left includes de calculations based on the estimates of this article, and the panel on the right presents the calculations based on the estimates obtained by AMK. We begin by reporting the follow up earnings of applicants in the control group, in this article, based on the results obtained by matching the experimental data of the program with the PILA of the Ministry of Social Protection between July 2008 and June 2014, and in the case of AMK based on their follow up survey that was collected in August 2006. We find that the average formal earnings of control applicant women is COP$186,500, or US$93.2, while AKM estimated them in COP$177,200, or US$88.6, a larger amount once considering that their figure is for 2006 and that both figures are in current COP$. This difference is explained by the different share of formal applicants in their follow up survey versus the PILA: 55 percent of their control women versus 27.2 percent of ours were formal. Nonetheless, formal earnings in our sample, among those with positive earnings, are COP$686,221, versus COP$322,110 in their survey, implying an approximate annual increase in earnings of 16.3 percent. Our observed earnings would likely be an overestimation of AKM's since theirs are estimated based on a larger universe of applicants, and ours are likely to be selected among the most able of theirs. AKM's estimated premium for women, due to the *JeA* program, is also larger than ours: 19.6 percent versus 17.9 percent in our case. The net present value of this flow of additional formal earnings during 40 years, discounted at a monthly rate of 0.5 percent, is COP$6.1 millions in our case and COP$6.4 millions for AKM. We include the same estimated costs considered by AKM, a total of COP$1.6 millions that include the direct costs of operating the program, a maintenance allowance, and the loss of tenure AKM estimated due to the program. AKM estimated that net gains for women were COP$4.7 millions, and that the internal rate of return of the program was 29.3 percent. According to our estimates, the net gains for women are COP$4.4 millions, and the internal rate of return is 27.6 percent.

On the whole, the results are very similar. There are several differences in the way AKM obtained their results that could help explain the differences between AKM's results and ours. Among the similarities between the studies, we find that both procedures split the effects in one for formal earnings and other for informal earnings, and both find significant effects only for formal earnings. Both definitions of formal earnings are similarly based on applicants being contributing to health, injury insurance, and pensions. Among the differences we find that AKM

directly collect information from applicants in the 2006 follow up survey, asking them for their earnings so that both applicants, regardless of whether they contributed to social security or not, reported their earnings. We do not directly contact the applicants but use the administrative records available in the PILA of the MSPS, which are supposed to contain the census of workers contributing to social security, and based on this data, we obtain the program effects on formality and formal earnings. As explained above, the amount reported by applicants in the PILA allows us to discard the possibility that applicants were underestimating their earnings at the moment of making their contributions to the social security system. Another important difference is the timing in which we observe the earnings: AKM observed them between August and October 2006, while we did between July 2008 and June 2014. Nonetheless, not only earnings of controls but also the relative gains in the likelihood of being formal are comparable. AKM found that the increase in the likelihood of women being formal due to the program was 6.8 percent, which represents a relative increase of 12.4 percent with respect to the 55 percent probability of being formal of control applicant women. We find that *JeA* increases the likelihood of being formal in 4.5 percent, which represents a relative increase of 16.5 percent with respect to the 27.2 percent probability of being formal of control applicant women in our data. It might be that gains were concentrated in the second quarter of the distribution, which was included in their 55 percent sample of formal workers and not among our formal workers. Nonetheless, we matched 89.4 percent of the baseline applicants either with the PILA of the SISBEN, thus although possible, it seems unlikely that the small unmeasured share of applicant was able to explain the observed difference.

Finally, AKM also includes a conservative estimate of net gains that assumes the effect of the program would diminish over time. Figure 4 presents the effect of the program by demographic group, without and with control baseline variables, obtained from estimating equation (1) each month. Even though the figure suggests that the effect is very stable in the analyzed period, and if something, increasing over time, we also include a conservative estimate of the net gains that assumes the program effect would diminish at a monthly rate of 0.5%. In this case, AKM estimate internal rate of return is 17.8 percent while our estimate is 20.4 percent.

VII.   Conclusions

The experimental evidence reported in this article shows that the *JeA* program had a positive and significant effect on the probability to work in the formal sector. With respect to the likelihood of applicants that had not been offered *JeA*, to be formal, the likelihood of applicants in the whole sample offered *JeA* is 8.0 percent higher, 11.1 percent higher for women, and 6.9 percent higher for men. Applicants in the treatment group also contributed more months during the analyzed period. Maximum spells in the formal sector were also longer for beneficiaries of the *JeA* program and more likely to work for a large firm. Earnings of treated applicants were 10.3 higher in the whole sample and made larger contributions to social security. Nonetheless, when we evaluate the differences in earnings by treatment status of those already working in the formal sector, we find no significant difference between the treatment and control groups, suggesting that most of the effect of the program in the labor market is capitalized in the significant increase in the likelihood of working in the formal sector, more than in the likelihood to earn more in that sector. That is, it affects more formality that productivity, once in the formal sector.

We also find that for the whole sample of applicants, those in the treatment group have 0.18 more years of education, have a probability of graduating from high school 3.1 percent higher, and a probability 2.3 percent higher of attending school. College attendance at the time of the SISBEN survey is 21.8 percent higher in relative terms for applicants in the treatment group in the whole sample. We find no significant effect on fertility decisions, marital status or some dimensions of assortative matching. We do not find any effect on the probability to work in the informal sector or on their informal earnings.

Finally, we find that the benefits of the *JeA* program are higher than it costs, leading to an internal rate of return of 15.5 percent.

On the whole, the program was a cost-effective alternative, worth to consider to bridging the transit of youths from the informal to the informal sectors in the future.

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Figure 1. Evolution of Formal Average Earnings of Applicants
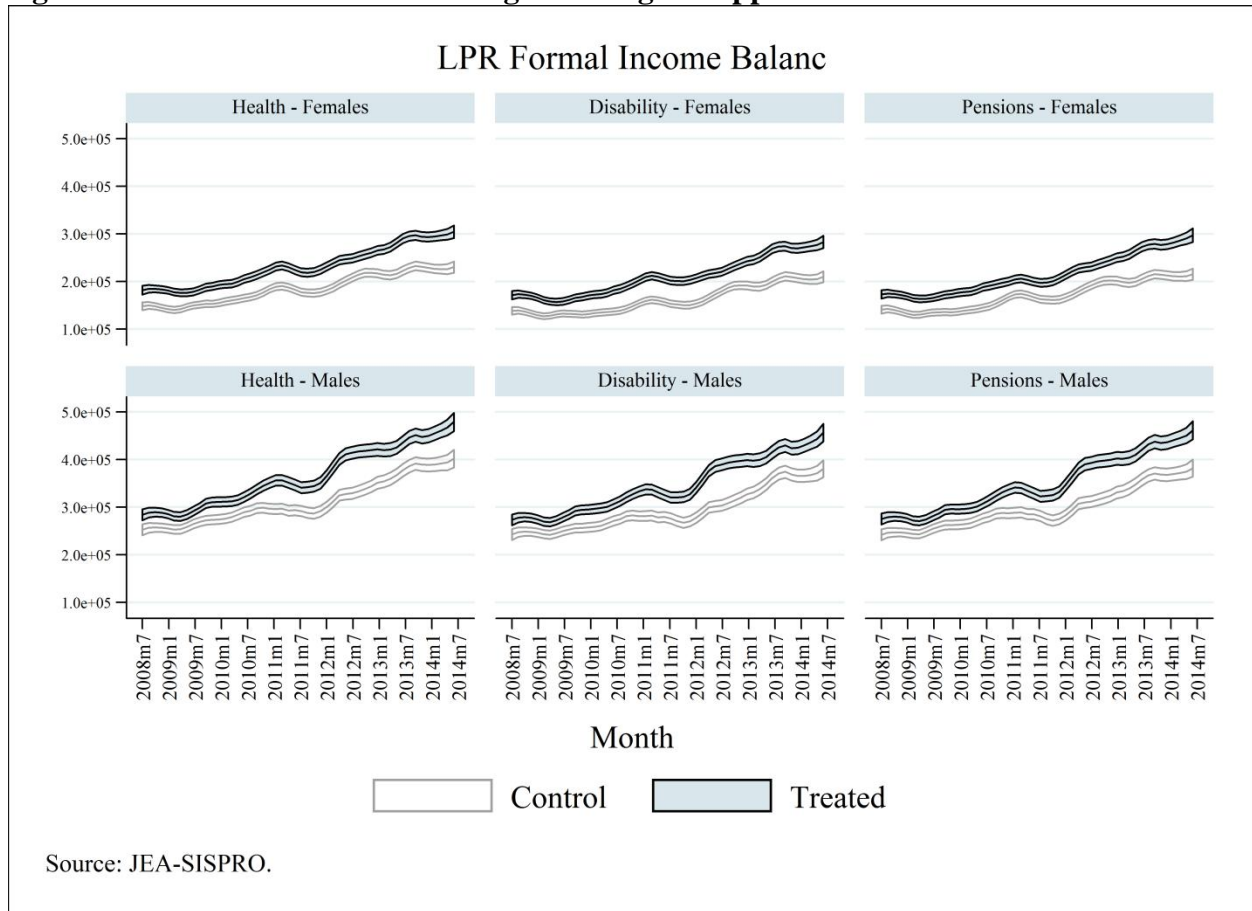
Figure 2. Evolution of Formal Earnings of Applicants that were matched to the PILA

Source: JEA-SISPRO.
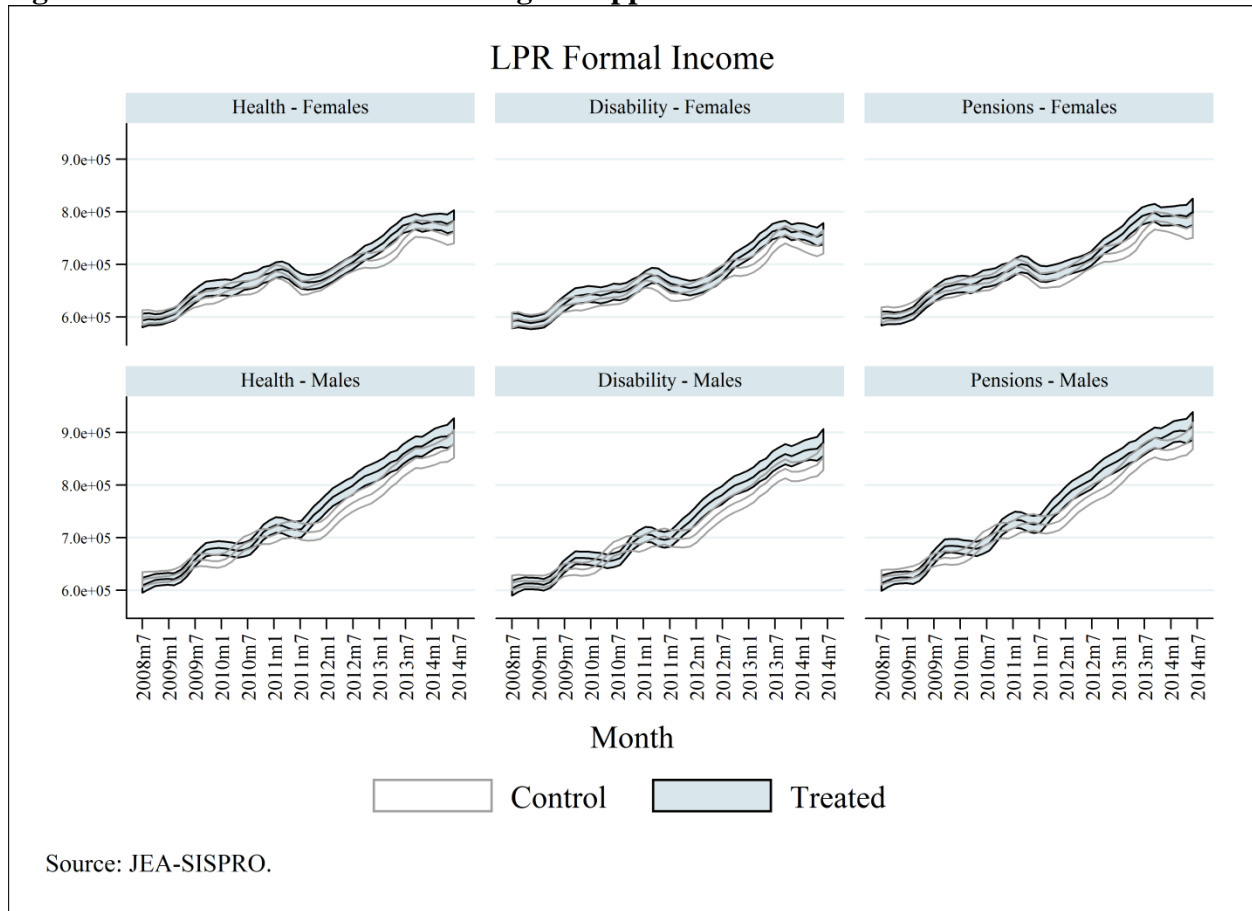
Figure 3. Match rates to formal employment data by Treatment Status, Gender and Type of Contribution

Source: JEA-SISPRO.
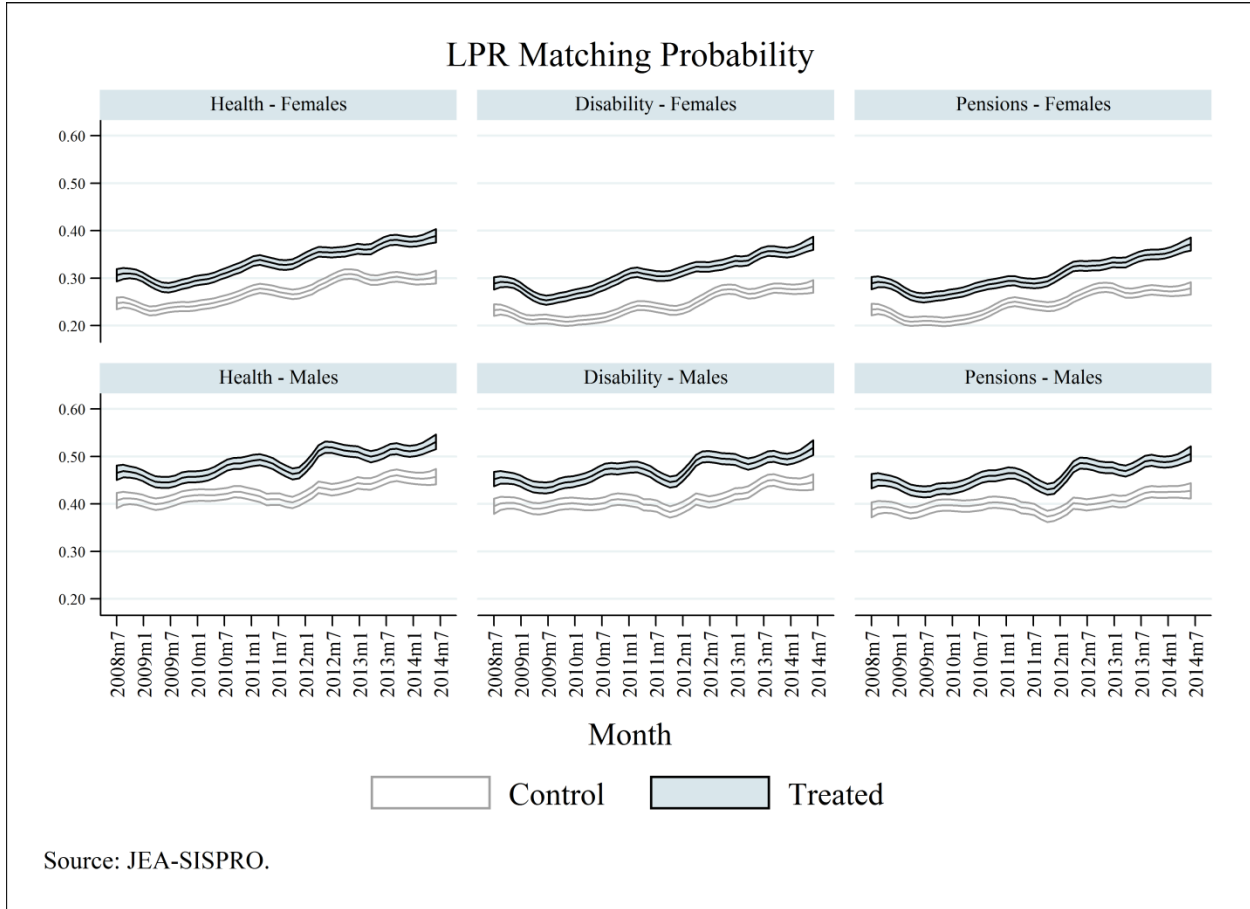
Figure 4. Evolution of the effect of the *JeA* Program on Formal Earnings (COP\$Dec 2013)
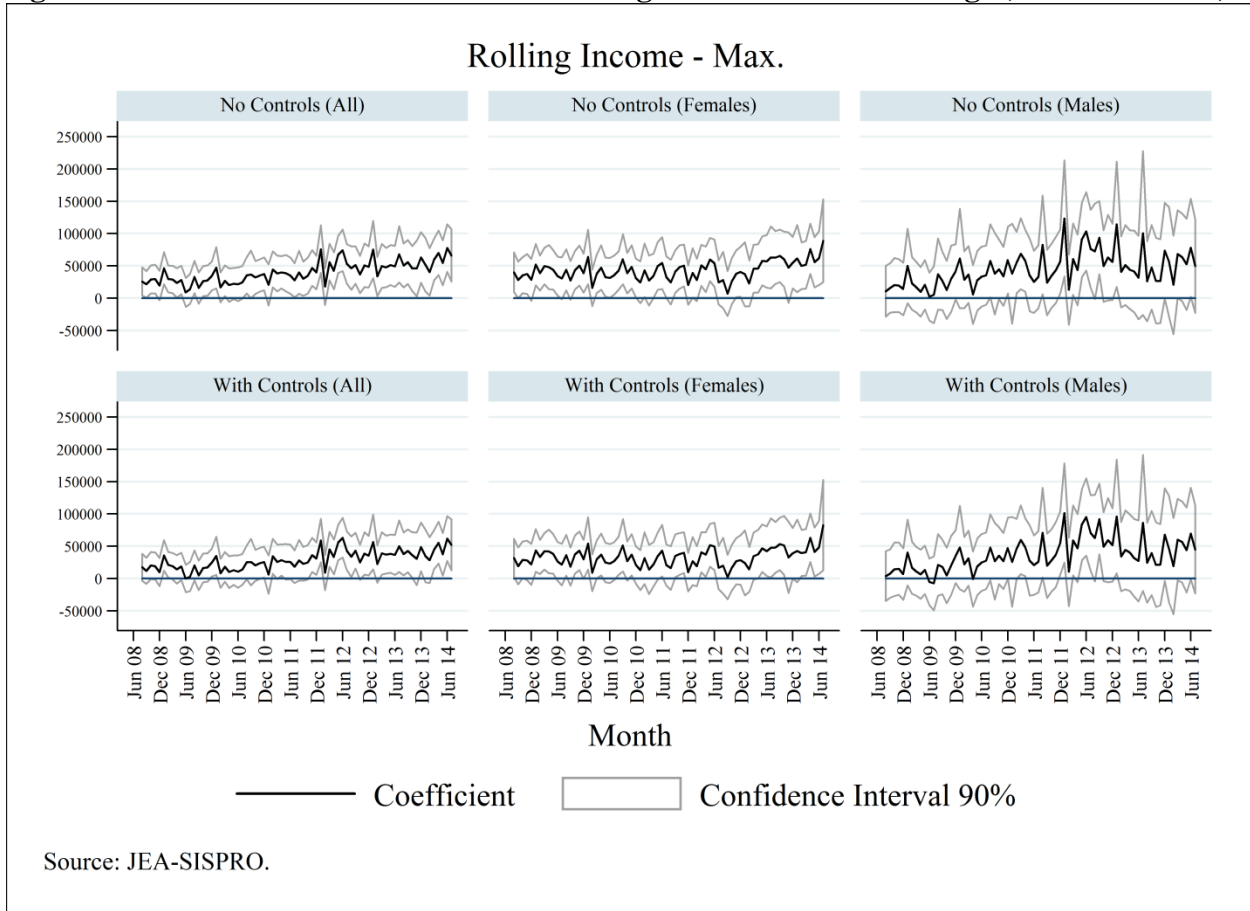
Table 1. Personal Characteristics and Treatment Status

	All		Women		Men	
	Control Mean	Treatment-Control Difference (s.e)	Control Mean	Treatment-Control Difference (s.e)	Control Mean	Treatment-Control Difference (s.e)
	(1)	(2)	(3)	(4)	(5)	(6)
Employoment	0.492	0.014	0.442	0.009	0.546	0.025
		(0.018)		(0.024)		(0.028)
Paid Employment	0.333	0.037	0.318	0.021	0.339	0.073
		(0.018)**		(0.023)		(0.027)**
Contract	0.082	-0.006	0.064	0.003	0.094	-0.005
		(0.01)		(0.013)		(0.016)
Formal	0.079	0.012	0.053	0.021	0.107	0.006
		(0.01)		(0.013)*		(0.018)
Wage and salary earnings	96,723	3,152	82,124	1,840	111,029	8,421
		(5590)		(6890)		(9292)
Self-employment earnings	22,673	-3,448	12,755	2,108	37,226	-13,990
		(2767)		(3028)		(5144)**
Tenure	2.959	0.759	2.396	1.177	3.406	0.584
		(0.357)**		(0.489)**		(0.52)
Days workep per month	11.777	0.227	10.476	0.196	13.256	0.317
		(0.462)		(0.619)		(0.708)
Hours worked per month	24.075	1.171	21.242	0.742	27.205	1.909
		(1.048)		(1.382)		(1.609)
Education	10.064	0.223	9.978	0.190	10.195	0.220
		(0.053)***		(0.075)**		(0.078)**
Age	21.255	-0.254	21.396	-0.232	21.035	-0.201
		(0.075)**		(0.104)**		(0.116)*
Married	0.194	-0.013	0.265	-0.004	0.118	-0.033
		(0.014)		(0.022)		(0.017)*
Test of joint significance	*F(11, 440) = 4.481*		*F(11, 416) = 1.411*		*F(11, 406) = 2.64*	
	p-value= 0.000		p-value= 0.165		p-value= 0.003	
Observations	3,956		2,133		1,823	

Notes: The table reports the difference in each variable between the treatment and control groups, controlling for site-by-course fixed effects. The last row reports the F-statistics and p-value of tests of differences of all of the variables (Except Tenure).

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 2. Descriptive Statistics

Table 3. Match Rates to Formal Employment Data, and Frequency of the Match in the Whole Period

Type of Estimation		All		Women		Men	
		Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
		(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
		(1)	(2)	(3)	(4)	(5)	(6)
1. Match in any month	*Coefficient*	0.645	0.056	0.556	0.073	0.749	0.042
	s.d./s.e.	(0.479)	(0.019)***	(0.497)	(0.028)***	(0.434)	(0.032)
	Observat.	1,907	3,932	1,046	2,125	861	1,807
2. Annual	*Coefficient*	0.473	0.046	0.381	0.054	0.581	0.049
	s.d./s.e.	(0.499)	(0.015)***	(0.486)	(0.021)***	(0.494)	(0.025)**
	Observat.	13,349	27,524	7,322	14,875	6,027	12,649
3. Monthly	*Coefficient*	0.343	0.036	0.272	0.045	0.427	0.040
	s.d./s.e.	(0.475)	(0.013)***	(0.445)	(0.017)***	(0.495)	(0.022)*
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
4. Months Matched	*Coefficient*	24.706	2.567	19.564	3.238	30.716	2.873
	s.d./s.e.	(25.808)	(1.021)**	(24.019)	(1.392)**	(26.532)	(1.809)
	Observat.	1,907	3,932	1,046	2,125	861	1,807
5. Days Contributed per Month	*Coefficient*	9.537	0.992	7.656	1.299	11.736	1.127
	s.d./s.e.	(13.737)	(0.377)***	(12.894)	(0.506)**	(14.352)	(0.647)*
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
6. Days Contributed	*Coefficient*	634.114	72.555	495.174	94.260	796.517	81.077
	s.d./s.e.	(737.668)	(29.21)**	(677.315)	(39.937)**	(771.764)	(52.157)
	Observat.	1,907	3,932	1,046	2,125	861	1,807

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

**Table 4. Match rates to Formal Employment Data
and Balance of Correlates between Treatment Groups**

Variable	*All* With Controls		*Women* With Controls		*Men* With Controls	
	Coef. (1)	*S.E.* (2)	*Coef.* (3)	*S.E.* (4)	*Coef.* (5)	*S.E.* (6)
Selected	0.109	0.164	0.079	0.218	0.237	0.271
Age * Selected	0.003	0.007	0.003	0.009	-0.008	0.012
Married * Selected	0.051	0.038	0.003	0.044	0.114	0.074
Education * Selected	-0.005	0.007	-0.01	0.010	-0.001	0.013
Employment * Selected	-0.115	0.093	-0.037	0.140	-0.21	0.154
Paid Employment * Selected	0.094	0.078	0.005	0.117	0.092	0.126
Salary * Selected	0.000	0.000	0.000	0.000	0.000	0.000
Profit * Selected	0.000	0.000	0.000	0.000	0.000	0.000
Formal * Selected	0.07	0.071	0.123	0.115	0.128	0.097
Contract * Selected	-0.07	0.078	-0.147	0.116	-0.114	0.107
Days * Selected	0.000	0.004	0.000	0.006	0.003	0.006
Hours * Selected	0.000	0.001	0.000	0.002	0.000	0.002
Women * Selected	-0.133***	0.020				
Age	-0.016***	0.005	-0.013*	0.007	-0.01	0.009
Married	-0.027	0.028	-0.022	0.032	0.031	0.054
Education	0.038***	0.005	0.033***	0.008	0.039***	0.009
Employment	0.006	0.071	0.014	0.094	-0.004	0.119
Paid Employment	0.005	0.053	0.096	0.086	0.049	0.093
Salary	0.000	0.000	0.000	0.000	0.000	0.000
Profit	0.000	0.000	0.000	0.000	0.000	0.000
Formal	-0.009	0.049	0.015	0.067	-0.054	0.073
Contract	0.124**	0.053	0.12*	0.067	0.154*	0.079
Days	0.002	0.003	0.002	0.004	0.002	0.005
Hours	-0.001	0.001	-0.001	0.001	-0.001	0.001
Constant	0.259**	0.126	0.157	0.162	0.195	0.216
R-squared	0.153		0.205		0.201	
p-value F-test[1]	0.589		0.964		0.690	
p-value F-test[2]	0.000		0.412		0.454	
p-value F-test[3]	0.000		0.000		0.001	
Number of observations	283,104		153,000		130,104	

note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$, [1] Over interacted regressors, [2] Over interacted regressors + "Selected", [3] Over Non-Interacted regressors

Table 5. Effects of the *JeA* Program on the Number of Months and Days of Tenure

Type of Estimation	Variable		All		Women		Men	
			Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
			(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
			(1)	(2)	(3)	(4)	(5)	(6)
A. Days	**1. Panel**	*Coefficient*	142.1	-1.0	148.5	3.3	137.6	2.2
		s.d./s.e.	(222.94)	(5.4)	(227.06)	(8.31)	(219.9)	(8.08)
		Observat.	9375	20589	3969	8684	5406	11905
	2. Mean	*Coefficient*	135.8	-4.6	118.8	1.6	155.7	-9.2
		s.d./s.e.	(218.14)	(7.2)	(184.89)	(10.37)	(250.15)	(14.51)
		Observat.	1907	3932	1046	2125	861	1807
	3. Max.	*Coefficient*	307.5	17.3	260.6	25.2	362.5	22.0
		s.d./s.e.	(381.34)	(13.23)	(357.54)	(20.91)	(400.67)	(25.33)
		Observat.	1907	3932	1046	2125	861	1807
	4. Min	*Coefficient*	46.0	-11.8	40.1	-6.3	52.9	-21.4
		s.d./s.e.	(171.05)	(5.38)**	(120.27)	(5.96)	(215.63)	(12.09)*
		Observat.	1907	3932	1046	2125	861	1807
B. Months	**1. Panel**	*Coefficient*	5.2	0.0	5.3	0.1	5.1	0.0
		s.d./s.e.	(7.43)	(0.18)	(7.59)	(0.28)	(7.3)	(0.27)
		Observat.	9375	20589	3969	8684	5406	11905
	2. Mean	*Coefficient*	4.8	-0.1	4.2	0.1	5.6	-0.3
		s.d./s.e.	(7.33)	(0.24)	(6.28)	(0.35)	(8.34)	(0.49)
		Observat.	1907	3932	1046	2125	861	1807
	3. Max.	*Coefficient*	10.6	0.6	9.0	0.8	12.5	0.7
		s.d./s.e.	(12.81)	(0.44)	(12.05)	(0.7)	(13.4)	(0.85)
		Observat.	1907	3932	1046	2125	861	1807
	4. Min	*Coefficient*	1.906	-0.361	1.624	-0.199	2.237	-0.659
		s.d./s.e.	(5.708)	(0.181)**	(4.09)	(0.206)	(7.138)	(0.406)
		Observat.	1907	3932	1046	2125	861	1807

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

Table 6. Effects of the *JeA* Program on the Probability of working in a risky position, Working in a Firm With at Least 200 employees, and being a dependent worker

Type of Estimation	Variable		All		Women		Men	
			Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
			(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
			(1)	(2)	(3)	(4)	(5)	(6)
A. Only Observed Values	**1. Riskier Position**	*Coefficient*	0.022	-0.001	0.014	-0.001	0.027	-0.002
		s.d./s.e.	(0.019)	(0.001)	(0.012)	(0.001)	(0.021)	(0.001)
		Observat.	45,520	100,958	19,000	42,218	26,520	58,740
	2. Large Firm	*Coefficient*	0.544	0.000	0.516	0.063	0.564	-0.019
		s.d./s.e.	(0.498)	(0.022)	(0.5)	(0.037)*	(0.496)	(0.03)
		Observat.	48,738	107,726	21,224	46,885	27,514	60,841
	3. Dependent	*Coefficient*	0.954	0.003	0.943	0.002	0.962	-0.009
		s.d./s.e.	(0.209)	(0.009)	(0.232)	(0.02)	(0.19)	(0.01)
		Observat.	48,738	107,726	21,224	46,885	27,514	60,841
B. Balancing Panel within Actual Contributors	**1. Riskier Position**	*Coefficient*	0.011	-0.0003	0.006	0.0001	0.015	-0.001
		s.d./s.e.	(0.017)	(0.0005)	(0.01)	(0.0005)	(0.021)	(0.001)
		Observat.	90,648	197,640	42,840	93,744	47,808	103,896
	2. Large Firm	*Coefficient*	0.289	0.010	0.252	0.038	0.321	0.009
		s.d./s.e.	(0.453)	(0.015)	(0.434)	(0.022)*	(0.467)	(0.023)
		Observat.	90,648	197,640	42,840	93,744	47,808	103,896
	3. Dependent	*Coefficient*	0.507	0.015	0.461	0.018	0.548	0.019
		s.d./s.e.	(0.5)	(0.014)	(0.498)	(0.023)	(0.498)	(0.021)
		Observat.	90,648	197,640	42,840	93,744	47,808	103,896
C. Balancing Panel Within All Applicants	**1. Riskier Position**	*Coefficient*	0.007	0.0001	0.003	0.00050	0.011	-0.000003
		s.d./s.e.	(0.015)	(0.0004)	(0.008)	(0.00029)*	(0.019)	(0.001)
		Observat.	137,304	283,104	75,312	153,000	61,992	130,104
	2. Large Firm	*Coefficient*	0.187	0.022	0.140	0.036	0.241	0.024
		s.d./s.e.	(0.39)	(0.011)*	(0.347)	(0.015)**	(0.427)	(0.019)
		Observat.	137,304	283,104	75,312	153,000	61,992	130,104
	3. Dependent	*Coefficient*	0.327	0.034	0.256	0.040	0.411	0.038
		s.d./s.e.	(0.469)	(0.013)***	(0.437)	(0.017)**	(0.492)	(0.021)*
		Observat.	137,304	283,104	75,312	153,000	61,992	130,104

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

Table 7. Effects of the *JeA* Program on Monthly Earnings

Type of Estimation		All		Women		Men	
		Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
		(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
		(1)	(2)	(3)	(4)	(5)	(6)
1. Only Observed Values	*Coefficient*	713,859	4,336	686,221	17,133	734,436	730
	s.d./s.e.	(397728)	(14199)	(337385)	(21263)	(436149)	(21716)
	Observat.	48,738	107,726	21,224	46,885	27,514	60,841
2. Balancing Panel within Actual Contributors	*Coefficient*	379,699	17,346	335,264	22,753	418,259	30,374
	s.d./s.e.	(459370)	(14376)	(416266)	(18784)	(490487)	(23737)
	Observat.	90,648	197,640	42,840	93,744	47,808	103,896
3. Balancing Panel Within All Applicants	*Coefficient*	244,948	28,959	186,458	33,359	313,316	37,458
	s.d./s.e.	(411265)	(11595)**	(352298)	(13582)**	(461623)	(21216)*
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

Table 8A. Effects of the *JeA* Program on Monthly Earnings: Non Parametric Bounds

Type of Contribution	Quantile		All			Women			Men		
			Loser's Mean Above Quantile (s.d)	Lower Bound Application Controls (s.e)	Upper Bound Application Controls (s.e)	Loser's Mean Above Quantile (s.d)	Lower Bound Application Controls (s.e)	Upper Bound Application Controls (s.e)	Loser's Mean Above Quantile (s.d)	Lower Bound Application Controls (s.e)	Upper Bound Application Controls (s.e)
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Health	**Initial**	*Coefficient*	712,623	4,195	64,820	683,566	13,932	68,409	734,309	1,750	62,558
		s.d./s.e.	(397660)	(14243)	(14876)***	(335285)	(21100)	(21718)***	(437215)	(21773)	(22685)***
		Observat.	48,121	106,229	99,749	20,978	46,245	43,436	27,143	59,971	56,928
	75th perc.	*Coefficient*	786,460	3,419	37,685	713,833	13,917	37,882	906,201	-7,928	43,201
		s.d./s.e.	(384771)	(16064)	(16868)**	(325424)	(21396)	(21646)*	(465856)	(26225)	(28646)
		Observat.	40,090	88,648	83,077	19,485	42,935	41,637	16,618	36,723	33,647
	85th perc.	*Coefficient*	982,297	-9,971	35,986	783,682	1,335	96,220	1,114,162	-40,693	-13,026
		s.d./s.e.	(467500)	(24970)	(25960)	(350551)	(25720)	(29287)***	(546812)	(34903)	(36118)
		Observat.	20,396	45,268	41,854	14,141	31,553	25,983	9,218	20,332	19,165
	95th perc.	*Coefficient*	1,381,335	-3,685	31,391	1,174,972	10,354	39,811	1,574,710	-105,975	-51,878
		s.d./s.e.	(647977)	(39626)	(40446)	(495983)	(35547)	(32445)	(768225)	(66527)	(68262)
		Observat.	6,777	14,506	13,901	3,699	7,961	7,539	3,054	6,559	6,326
B. Disability	**Initial**	*Coefficient*	698,558	2,951	71,891	669,810	14,670	83,172	718,450	-2,488	59,686
		s.d./s.e.	(390337)	(14037)	(14569)***	(336885)	(21153)	(22100)***	(422255)	(21091)	(21769)***
		Observat.	45,016	99,797	92,656	18,802	41,739	38,416	26,214	58,045	54,890
	75th perc.	*Coefficient*	779,100	1,620	33,247	660,692	10,092	75,589	867,062	-6,624	44,103
		s.d./s.e.	(369436)	(15505)	(16190)**	(352386)	(20316)	(21179)***	(432301)	(25877)	(27870)
		Observat.	37,459	83,031	78,506	18,655	41,295	38,786	17,200	38,082	34,633
	85th perc.	*Coefficient*	944,881	-8,930	35,643	769,490	2,530	58,940	1,077,920	-38,411	-7,399
		s.d./s.e.	(439549)	(23334)	(24438)	(335938)	(25668)	(27177)**	(508736)	(33870)	(34913)
		Observat.	20,502	45,483	41,964	13,335	29,991	26,224	9,226	20,498	19,168
	95th perc.	*Coefficient*	1,329,781	-16,987	19,005	1,117,706	-14,063	26,138	1,522,511	-96,550	-36,087
		s.d./s.e.	(605573)	(38830)	(40252)	(478151)	(38349)	(34072)	(700113)	(66545)	(67873)
		Observat.	6,773	14,510	13,902	3,700	8,045	7,540	3,058	6,644	6,323
C. Pensions	**Initial**	*Coefficient*	724,590	-1,217	59,581	694,696	9,075	61,891	746,017	-3,352	56,815
		s.d./s.e.	(395312)	(15087)	(15805)***	(342329)	(23457)	(24231)**	(428008)	(22794)	(23771)**
		Observat.	44,561	98,510	92,572	18,907	41,781	39,392	25,654	56,710	53,795
	75th perc.	*Coefficient*	792,334	265	32,051	685,149	4,629	57,658	898,743	-5,636	45,626
		s.d./s.e.	(380007)	(16983)	(17761)*	(356547)	(22588)	(23257)**	(446960)	(27263)	(29669)
		Observat.	38,021	84,072	79,573	18,854	41,588	39,768	16,566	36,595	33,550
	85th perc.	*Coefficient*	968,167	-9,994	34,479	789,382	-4,179	73,240	1,102,165	-39,358	-10,705
		s.d./s.e.	(451604)	(25299)	(26399)	(352693)	(27981)	(31164)**	(520208)	(35901)	(37115)
		Observat.	20,533	45,514	42,004	13,363	29,917	25,448	9,224	20,365	19,168
	95th perc.	*Coefficient*	1,364,633	-3,118	27,005	1,156,904	12,189	49,131	1,551,809	-89,318	-36,232
		s.d./s.e.	(621485)	(42936)	(43050)	(496679)	(41771)	(38157)	(718147)	(65907)	(69330)
		Observat.	6,759	14,363	13,893	3,699	7,905	7,532	3,059	6,573	6,332
D. Health, Disability & Pensions	**Initial**	*Coefficient*	715,079	4,632	64,947	687,176	17,794	70,411	735,875	1,261	62,804
		s.d./s.e.	(399045)	(14290)	(14919)***	(338147)	(21455)	(22120)***	(437820)	(21867)	(22788)***
		Observat.	48,162	106,392	99,915	20,986	46,314	43,564	27,176	60,065	57,004
	75th perc.	*Coefficient*	787,569	3,925	40,062	716,793	16,940	40,270	907,994	-7,409	44,365
		s.d./s.e.	(387076)	(16082)	(16917)**	(328938)	(21960)	(22157)*	(467443)	(26148)	(28487)
		Observat.	40,217	88,961	83,071	19,511	43,077	41,795	16,625	36,735	33,654
	85th perc.	*Coefficient*	985,770	-8,811	38,026	785,626	4,101	102,505	1,116,580	-39,075	-11,136
		s.d./s.e.	(470778)	(25081)	(26064)	(355459)	(26461)	(30369)***	(548347)	(34754)	(35810)
		Observat.	20,392	45,312	41,855	14,203	31,693	26,068	9,224	20,368	19,172
	95th perc.	*Coefficient*	1,389,766	-8,387	33,464	1,184,518	15,083	49,937	1,580,393	-103,865	-47,965
		s.d./s.e.	(651307)	(38778)	(40083)	(503215)	(37042)	(34355)	(770169)	(64238)	(66662)
		Observat.	6,773	14,578	13,898	3,698	8,000	7,544	3,053	6,572	6,325

All regressions control for siteby-course fixed effects. The regressions in columns 3, 6 and 9 also control for the following pretreatment characteristics: 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.

Table 8B. Effects of the *JeA* Program on Monthly Earnings: Non Parametric Bounds Conditioning on Baseline ECAP

Type of Contribution	Quantile		All			Women			Men		
			Loser's Mean Above Quantile (s.d)	Lower Bound Application Controls (s.e)	Upper Bound Application Controls (s.e)	Loser's Mean Above Quantile (s.d)	Lower Bound Application Controls (s.e)	Upper Bound Application Controls (s.e)	Loser's Mean Above Quantile (s.d)	Lower Bound Application Controls (s.e)	Upper Bound Application Controls (s.e)
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Health	**Initial**	Coefficient	712,627	45,909	8,151	683,575	66,965	-32,713	734,314	65,908	-19,855
		s.d./s.e.	(397660)	(14500)***	(17966)	(335284)	(22207)***	(28588)	(437215)	(21647)***	(28550)
		Observat.	48,120	98,818	93,109	20,977	39,852	39,357	27,142	52,776	52,298
	75th perc.	Coefficient	734,071	40,169	63,597	585,138	57,807	51,523	790,511	59,366	66,028
		s.d./s.e.	(457859)	(17413)**	(20676)***	(432235)	(20545)***	(25455)**	(521099)	(26100)**	(33763)*
		Observat.	35,949	79,692	73,573	19,442	42,117	39,874	16,388	35,796	34,318
	85th perc.	Coefficient	856,246	46,155	60,789	666,973	90,385	77,555	867,314	82,742	95,997
		s.d./s.e.	(535052)	(23001)**	(28436)**	(472612)	(24219)***	(33042)**	(621453)	(32131)**	(43572)**
		Observat.	20,658	46,872	41,472	12,390	27,187	24,893	10,152	22,846	21,142
	95th perc.	Coefficient	917,436	89,216	72,087	654,965	129,098	65,666	844,968	131,011	77,326
		s.d./s.e.	(724060)	(32598)***	(44061)	(556870)	(30170)***	(44123)	(719273)	(39293)***	(53835)
		Observat.	9,195	20,919	18,577	7,705	16,657	15,893	7,004	15,527	14,796
B. Disability	**Initial**	Coefficient	698,562	49,417	20,801	669,820	75,687	-22,261	718,462	66,883	-14,850
		s.d./s.e.	(390338)	(14358)***	(17863)	(336886)	(22983)***	(29699)	(422255)	(20799)***	(28206)
		Observat.	45,015	91,864	86,294	18,801	35,126	35,514	26,212	50,594	50,214
	75th perc.	Coefficient	703,812	36,334	65,813	528,094	58,914	68,044	767,445	48,526	57,518
		s.d./s.e.	(449975)	(16489)**	(19678)***	(439592)	(19761)***	(25172)***	(499224)	(25071)*	(31762)*
		Observat.	35,160	78,176	71,994	19,059	41,424	38,908	16,261	35,583	34,130
	85th perc.	Coefficient	827,836	40,501	54,835	617,133	92,560	88,460	839,914	72,621	79,075
		s.d./s.e.	(511780)	(22240)*	(27391)**	(478359)	(23625)***	(31200)***	(591326)	(30803)**	(41770)*
		Observat.	20,346	46,381	41,083	11,909	26,514	24,194	10,128	22,763	21,119
	95th perc.	Coefficient	880,254	71,812	61,994	598,915	133,728	86,543	816,090	119,671	60,307
		s.d./s.e.	(690095)	(31559)**	(42147)	(553725)	(28986)***	(40528)**	(681492)	(38947)***	(51694)
		Observat.	9,141	20,788	18,470	7,463	16,412	15,555	6,983	15,323	14,775
C. Pensions	**Initial**	Coefficient	724,594	44,449	7,422	694,706	65,315	-36,476	746,022	66,135	-24,953
		s.d./s.e.	(395313)	(15507)***	(19382)	(342329)	(25128)***	(31294)	(428007)	(22675)***	(30845)
		Observat.	44,560	90,858	85,456	18,906	35,535	35,696	25,653	49,399	49,116
	75th perc.	Coefficient	716,545	37,154	65,179	549,833	55,526	55,444	779,233	55,086	65,785
		s.d./s.e.	(464307)	(17969)**	(21262)***	(450659)	(21889)**	(27207)**	(513895)	(27279)**	(34482)*
		Observat.	35,211	78,120	72,055	19,153	41,271	39,176	16,178	35,507	33,944
	85th perc.	Coefficient	846,185	43,990	54,739	638,134	92,752	79,341	854,855	78,580	90,366
		s.d./s.e.	(528591)	(24139)*	(29501)*	(492014)	(26598)***	(34708)**	(606544)	(33321)**	(45054)**
		Observat.	20,258	46,147	40,957	11,984	26,459	24,329	10,089	22,728	21,036
	95th perc.	Coefficient	899,655	88,706	68,110	622,472	143,736	77,240	828,426	129,872	64,585
		s.d./s.e.	(711507)	(35028)**	(46055)	(571395)	(33152)***	(46401)*	(699130)	(41410)***	(56280)
		Observat.	9,120	20,655	18,432	7,532	16,199	15,638	6,966	15,376	14,722
D. Health, Disability & Pensions	**Initial**	Coefficient	715,079	46,210	9,078	687,176	69,971	-27,502	735,875	65,750	-19,006
		s.d./s.e.	(399045)	(14528)***	(18049)	(338147)	(22563)***	(29417)	(437820)	(21738)***	(28710)
		Observat.	48,162	99,007	93,202	20,986	39,955	39,395	27,176	52,865	52,348
	75th perc.	Coefficient	736,204	40,607	65,315	587,637	62,364	57,763	792,481	58,677	67,662
		s.d./s.e.	(460059)	(17429)**	(20696)***	(435625)	(21112)***	(26023)**	(522712)	(26041)**	(33610)**
		Observat.	35,978	79,816	73,584	19,470	42,231	39,925	16,387	35,816	34,309
	85th perc.	Coefficient	859,275	46,283	63,377	670,079	93,196	85,088	869,945	82,778	96,962
		s.d./s.e.	(538142)	(23035)**	(28491)**	(477388)	(24727)***	(33104)**	(623311)	(31962)***	(43215)**
		Observat.	20,660	46,947	41,449	12,397	27,240	24,906	10,152	22,841	21,144
	95th perc.	Coefficient	922,313	91,329	75,809	658,480	135,909	76,682	847,995	129,766	77,930
		s.d./s.e.	(727780)	(32483)***	(43957)*	(562648)	(30230)***	(43724)*	(722070)	(39136)***	(53484)
		Observat.	9,196	20,933	18,580	7,710	16,714	15,906	6,995	15,496	14,786

All regressions control for siteby-course fixed effects. The regressions in columns 3, 6 and 9 also control for the following pretreatment characteristics: 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.

Table 9. Effects of the *JeA* Program on Taxes and Contributions to Social Security

Variable		All		Women		Men	
		Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
		(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
		(1)	(2)	(3)	(4)	(5)	(6)
1. Contributes to Retefuente	*Coefficient*	0.0003	0.0003	0.0001	0.0002	0.0005	0.0004
	s.d./s.e.	(0.0167)	(0.0002)	(0.0078)	(0.0001)*	(0.0231)	(0.0005)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
2. Retefuente	*Coefficient*	69	61	26	104	120	52
	s.d./s.e.	(7282)	(56)	(4070)	(79)	(9780)	(137)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
3. Pensions	*Coefficient*	36,566	4,507	27,264	4,810	47,439	6,157
	s.d./s.e.	(66081)	(1924)**	(58432)	(2223)**	(72530)	(3367)*
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
4. Health	*Coefficient*	28,706	3,290	21,896	3,652	36,665	4,384
	s.d./s.e.	(49034)	(1406)**	(42270)	(1603)**	(54852)	(2507)*
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
5. Cajas	*Coefficient*	8,553	1,071	6,260	1,133	11,232	1,375
	s.d./s.e.	(16111)	(457)**	(13677)	(532)**	(18193)	(819)*
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
6. ICBF	*Coefficient*	5,148	559	3,811	618	6,711	800
	s.d./s.e.	(10982)	(294)*	(9317)	(344)*	(12474)	(525)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
7. MEN	*Coefficient*	6,303	-1,152	4,098	-144	8,881	-2,630
	s.d./s.e.	(63980)	(1426)	(51927)	(1557)	(75592)	(2897)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
8. SENA	*Coefficient*	3,358	403	2,466	429	4,401	510
	s.d./s.e.	(7053)	(188)**	(5940)	(222)*	(8039)	(341)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
9. Injury	*Coefficient*	4,862	59	2,210	266	7,961	40
	s.d./s.e.	(12945)	(328)	(7234)	(248)	(16865)	(658)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
10. Pension Solidarity Fund	*Coefficient*	52	24	15	15	95	37
	s.d./s.e.	(1318)	(23)	(710)	(15)	(1782)	(56)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
11. ESAP	*Coefficient*	21	-1	22	16	20	-1
	s.d./s.e.	(471)	(15)	(469)	(25)	(472)	(17)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104
12. Total	*Coefficient*	93,637	8,821	68,068	10,898	123,525	10,724
	s.d./s.e.	(174734)	(4881)*	(146457)	(5564)*	(198724)	(8729)
	Observat.	137,304	283,104	75,312	153,000	61,992	130,104

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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. The *Cajas* are private social entities created to administer a family subsidy for low wage employees with children, and to provide recreation for their members. ICBF is the acronym in Spanish for Family Welfare Colombian Institute, the public entity in charge of implementing the policy for child and youth care. SENA is the acronym for National Service for Learning, the public entity in charge of providing job training, technique and technological programs. MEN is the acronym in Spanish for the National Ministry of Education, the Pension Solidarity Fund collects a contribution targeted to the highest wages, to fund the pensions of the poorest. ESAP is the acronym in Spanish for Public Administration School for Higher Education.

Table 10. Effects of the *JeA* Program on Education Outcomes Based on the Sisben Survey

Variable		All		Women		Men	
		Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
		(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
		(1)	(2)	(3)	(4)	(5)	(6)
1. Matching	*Coefficient*	0.657	-0.003	0.667	0.014	0.644	-0.029
	s.d. - s.e.	(0.475)	(0.017)	(0.471)	(0.026)	(0.479)	(0.032)
	Observat.	1907	3932	1046	2125	861	1807
2. Years of Education	*Coefficient*	10.053	0.243	10.101	0.163	9.994	0.287
	s.d. - s.e.	(2.364)	(0.094)**	(2.286)	(0.143)	(2.456)	(0.159)*
	Observat.	1259	2571	717	1452	542	1119
3. High School Graduation	*Coefficient*	0.714	0.040	0.723	0.029	0.703	0.050
	s.d. - s.e.	(0.452)	(0.019)**	(0.448)	(0.029)	(0.457)	(0.031)
	Observat.	1259	2571	717	1452	542	1119
4. College Attendance	*Coefficient*	0.075	0.027	0.076	0.001	0.073	0.042
	s.d. - s.e.	(0.263)	(0.018)	(0.266)	(0.022)	(0.26)	(0.031)
	Observat.	1259	2571	717	1452	542	1119
5. School Attendance	*Coefficient*	0.079	0.018	0.067	0.027	0.093	0.013
	s.d. - s.e.	(0.27)	(0.016)	(0.251)	(0.024)	(0.291)	(0.03)
	Observat.	1259	2571	717	1452	542	1119

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

Table 11. Effects of the *JeA* Program on Fertility and Marital Status Outcomes Based on the Sisben Survey

Variable		All		Women		Men	
		Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
		(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
		(1)	(2)	(3)	(4)	(5)	(6)
1. Has at least 1 child	*Coefficient*	0.536	-0.010	0.741	-0.022	0.288	0.002
	s.d. - s.e.	(0.499)	(0.022)	(0.438)	(0.035)	(0.453)	(0.04)
	Observat.	1259	2571	717	1452	542	1119
2. Has at least 2 children	*Coefficient*	0.246	-0.003	0.378	-0.024	0.087	0.004
	s.d. - s.e.	(0.431)	(0.022)	(0.485)	(0.038)	(0.282)	(0.022)
	Observat.	1259	2571	717	1452	542	1119
3. Has at least 3 children	*Coefficient*	0.068	-0.011	0.113	-0.021	0.013	0.003
	s.d. - s.e.	(0.252)	(0.012)	(0.317)	(0.022)	(0.112)	(0.01)
	Observat.	1259	2571	717	1452	542	1119
4. Had at least 1 child (9 Months after the end of the program)	*Coefficient*	0.273	0.014	0.333	0.012	0.200	-0.001
	s.d. - s.e.	(0.445)	(0.023)	(0.471)	(0.037)	(0.4)	(0.037)
	Observat.	1259	2571	717	1452	542	1119
5. Has at least 2 children (9 Months after the end of the program)	*Coefficient*	0.039	-0.007	0.055	-0.016	0.018	0.016
	s.d. - s.e.	(0.193)	(0.01)	(0.229)	(0.015)	(0.134)	(0.016)
	Observat.	1259	2571	717	1452	542	1119
6. Months to the first child after the program	*Coefficient*	39.494	-0.262	37.733	0.087	41.627	-0.092
	s.d. - s.e.	(13.419)	(0.714)	(14.776)	(1.134)	(11.213)	(0.944)
	Observat.	1259	2571	717	1452	542	1119
7. Single at the Time of the Survey	*Coefficient*	0.538	-0.022	0.437	-0.020	0.660	-0.029
	s.d. - s.e.	(0.499)	(0.025)	(0.496)	(0.038)	(0.474)	(0.039)
	Observat.	1259	2571	717	1452	542	1119
8. With Children and not married at the Time of the Survey	*Coefficient*	0.198	-0.001	0.335	0.003	0.032	-0.002
	s.d. - s.e.	(0.399)	(0.019)	(0.472)	(0.033)	(0.177)	(0.017)
	Observat.	1259	2571	717	1452	542	1119

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

Table 12. Effects of the *JeA* Program on Assortative Mating

		All			Women			Men		
		Loser's Mean	Coefficient on being		Loser's Mean	Coefficient on being		Loser's Mean	Coefficient on being	
			No Controls	Controls		No Controls	Controls		No Controls	Controls
		(s.d)	(s.e)	(s.e)	(s.d)	(s.e)	(s.e)	(s.d)	(s.e)	(s.e)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Has spouse Information	*Coefficient*	0.254	-0.009	-0.002	0.309	-0.005	0.001	0.187	0.003	0.015
	s.d. - s.e.	(0.435)	(0.014)	(0.014)	(0.462)	(0.022)	(0.021)	(0.39)	(0.021)	(0.021)
Income[1]	*Coefficient*	-96,370	-21,709	-19,660	-283,811	-2,408	-7,856	278,510	-65,935	-81,613
	s.d. - s.e.	(406034)	(32631)	(32368)	(323089)	(31231)	(32251)	(274052)	(54627)	(58745)
Age[1]	*Coefficient*	-2.416	0.100	0.038	-4.113	0.103	0.049	0.977	-0.283	-0.569
	s.d. - s.e.	(6.212)	(0.461)	(0.462)	(6.146)	(0.572)	(0.584)	(4.805)	(0.853)	(0.905)
Years of Education[1]	*Coefficient*	0.963	0.271	0.296	1.284	0.192	0.350	0.321	0.086	-0.566
	s.d. - s.e.	(3.255)	(0.236)	(0.233)	(3.434)	(0.332)	(0.33)	(2.761)	(0.41)	(0.413)
	Observat.	486	980	979	324	649	648	162	331	331

All regressions control for siteby-course fixed effects. The regressions in columns 3, 6 and 9 also control for the following pretreatment characteristics: 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.

[1] Differences between applicants and their spouses

Table 13. Effects of the *JeA* Program on Labor Market Outcomes Based on the Sisben Survey

Variable		All		Women		Men	
		Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training	Loser's Mean	Coefficient on being offered training
		(s.d.)	(s.e.)	(s.d.)	(s.e.)	(s.d.)	(s.e.)
		(1)	(2)	(3)	(4)	(5)	(6)
1. Labor Force Participation at the Time of the Survey	*Coefficient*	0.612	0.019	0.460	0.042	0.796	0.002
	s.d. - s.e.	(0.487)	(0.024)	(0.499)	(0.039)	(0.403)	(0.042)
	Observat.	1259	2571	717	1452	542	1119
2. Employed at the Time of the Survey	*Coefficient*	0.492	0.045	0.359	0.069	0.653	0.019
	s.d. - s.e.	(0.5)	(0.026)*	(0.48)	(0.038)*	(0.476)	(0.048)
	Observat.	1259	2571	717	1452	542	1119
3. Unemployed at the Time of the Survey	*Coefficient*	0.120	-0.026	0.101	-0.027	0.143	-0.017
	s.d. - s.e.	(0.325)	(0.017)	(0.301)	(0.024)	(0.35)	(0.035)
	Observat.	1259	2571	717	1452	542	1119
4. Private Health Insurance[1]	*Coefficient*	0.242	0.021	0.232	0.046	0.255	0.019
	s.d. - s.e.	(0.429)	(0.023)	(0.423)	(0.037)	(0.436)	(0.039)
	Observat.	1259	2571	717	1452	542	1119
5. Formal1[2]	*Coefficient*	0.181	0.011	0.139	0.039	0.233	-0.011
	s.d. - s.e.	(0.385)	(0.02)	(0.346)	(0.029)	(0.423)	(0.037)
	Observat.	1259	2571	717	1452	542	1119
6. Formal2[3]	*Coefficient*	0.151	0.004	0.111	0.013	0.200	-0.015
	s.d. - s.e.	(0.358)	(0.019)	(0.315)	(0.026)	(0.4)	(0.035)
	Observat.	1259	2571	717	1452	542	1119
7. Monthly Income	*Coefficient*	226,169	43,343	168,782	21,817	295,665	71,196
	s.d. - s.e.	(341399)	(22200)*	(381119)	(22857)	(270476)	(53218)
	Observat.	1259	2571	717	1452	542	1119
8. Monthly Income at Least the Minimum Wage	*Coefficient*	0.247	0.042	0.167	0.049	0.345	0.026
	s.d. - s.e.	(0.432)	(0.024)*	(0.373)	(0.032)	(0.476)	(0.045)
	Observat.	1259	2571	717	1452	542	1119

The estimates in columns 2, 4 and 6 are obtained after controlling for siteby-course fixed effects and the following pretreatment characteristics: 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.

[1] 1 if enrolled in private health insurance, 0 if enrolled in public health insurance or uninsured.

[2] Formal1: if enrolled in private health insurance and working at the time of the survey.

[3] Formal2: if enrolled in private health insurance, working and earning at least the minimum wage at the time of the survey.

Table 14. Cost-Benefit Analysis

		This Article					**Attanasio et al. 2011**			
		All	Women	Men	All	Women	Men	**Paid Women**		**Formal Women**	
		COP$000			**US$**			**COP$000**	**US$**	**COP$000**	**US$**
Benefits											
Control's Monthly Earnings		244.9	186.5	313.3	122.5	93.2	156.7	177.2	88.6	79.6	39.8
Monthly Gains in Earnings		29.0	33.4	37.5	14.5	16.7	18.7	34.7	17.3	26.6	13.3
Relative Gain vs Control		11.8%	17.9%	12.0%	11.8%	17.9%	12.0%	19.6%	19.6%	33.4%	33.4%
Net Present Value of Benefits											
Monthly Rate	0.5%	5,263	6,063	6,808	2,632	3,031	3,404	6,397	3,198	4,835	2,418
Monthly Decrease in Gains	0.5%	2,879	3,316	3,724	1,439	1,658	1,862	2,721	1,360	2,056	1,028
Costs[*]		**1,624**	**1,624**	**1,624**	**812**	**812**	**812**	**1,624**	**812**	**812**	**406**
Direct Cost		1,500	1,500	1,500	750	750	750	1,500	750.0	750	375.0
Loss of Tenure		124	124	124	62	62	62	124	62.0	62	31.0
Benefit-Cost											
Monthly Rate	0.5%	3,639	4,439	5,184	1,820	2,219	2,592	4,773	2,386	4,023	2,012
Monthly Decrease in Gains	0.5%	1,255	1,692	2,100	627	846	1,050	1,097	548	1,244	622
Annual IRR											
Monthly Rate	0.5%	23.6%	27.6%	31.5%	23.6%	27.6%	31.5%	29.3%	29.3%	21.5%	21.5%
Monthly Decrease in Gains	0.5%	16.6%	20.4%	24.0%	16.6%	20.4%	24.0%	17.8%	17.8%	10.6%	10.6%

[*] Source: Attanasio et al. (2011).

Table A1—Distribution of Training Courses

Course name	Number of courses	Course name	Number of courses
Sales representative	43	Library assistant	3
Secretary/administrative assistant	34	Gas station assistant	3
Marketing assistant	20	Environmental assistant	3
Inventory and warehouse assistant	18	Metal fabrication	3
Archival assistant	18	Bank teller	3
Seamstress/industrial textile prod.	17	Taxi/bus driver	2
Electrician	16	Clinical lab assistant	2
Client relations assistant	16	Foreign trade assistant	2
Data entry assistant	14	Real estate assistant	2
Kitchen/cooking assistant	13	Cashiers' assistant	2
IT assistant	13	Plumbers' assistant	2
Doctor/dentist/nurse assistant	12	Journeyman	2
Auto/motorcycle mechanic assistant	11	Molding and foundry worker	2
Recreation assistant	11	Vocational training teacher	2
Preschool teacher assistant	10	Welding assistant	1
Accounting assistant	10	Refrigeration equipment assistant	1
Mail delivery assistant	10	Upholster	1
Busboy/waiter/waitress	10	Gas installations	1
Surveyor assistant	9	Agricultural machinery mechanic	1
Security guard/building maintenance	8	Organic waste processor	1
Carpenter	7	Construction operator	1
Pharmacy assistant	6	Sports referee	1
Senior citizens assistant	6	Cattle farming	1
Meat processor	6	Organic farming	1
Bakery assistant	5	Waste processor	1
Human resources assistant	5	Packing operator	1
Graphic design assistant	5	Shoe repair services	1
Call center/telemarketing assistant	5	Florist	1
Flower cultivation	5	Tourism assistant	1
Cleaning services	5	Wooden machine operator	1
Beautician	4	Journalism assistant	1
Industrial production operator	4	Physical rehabilitation	1
Food processing	4	Quality control assistant	1
Management assistant	3	Worker safety assistant	1
Wooden furniture painter assistant	3		

Source: Attanasio et al. (2011)