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Starting on the right track: Experimental evidence on the effects of first job experience on future labor market outcomes

Berniell, Lucila
de la Mata

Starting on the right track: Experimental evidence from a large-scale apprenticeship program

Lucila Berniell[*],

Dolores de la Mata[†‡]

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Abstract

This paper evaluates a large-scale youth employment program in Argentina (*Programa Primer Paso*, PPP), which involves 12 months of apprenticeship work and no formal training other than the on-the-job training provided by formally registered firms. Causal estimates of the impacts of the program are obtained by exploiting the random assignment of beneficiaries. Impacts are measured in several dimensions, using administrative and a follow-up survey data. The program caused large short-to-medium run gains in the probability of being a formally registered worker, for both men and women, and a moderate fall in unemployment. Exploiting a special feature of the random assignment process, we show that these results are not driven by displacements effects among eligible individuals. We explore several mechanisms that could be driving the observed impacts and we find evidence favoring a signaling effect over alternative explanations, in particular skills development.

[*]CAF, Research Department.

[†]CAF, Research Department.

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1 Introduction

Unemployment rates are usually two to three times higher for young than for adult workers, a feature that is present both in developed and developing economies. However, in developing economies young workers face an additional problem in the labor market, since labor informality is much higher for them. For instance, in Latin America the labor informality among workers aged 15 to 25 is around 60% higher than among older workers (SEDLAC, 2015). Moreover, both unemployment and informality are more serious for young workers with low socioeconomic status. Since the way in which young individuals entry the labor market can have scarring effects (Bell et al., 2015; Oreopoulos et al., 2012), first job opportunities are important for labor market prospects.

There are several hypotheses explaining these bad labor outcomes for the youth, which can be classified in human capital deficiencies (low levels of the basic cognitive, technical or socio-emotional skills demanded by potential employers) or barriers to labor market entry (Pallais, 2014). In particular, informational barriers could be critical for the youth: employers may not have sufficient incentives to hire inexperienced workers whose abilities are uncertain. Moreover, many young workers have some work experience but only in informal activities, which may be more difficult to certificate. Other types of informational barriers are the lack of knowledge about how to conduct an efficient job search, low expectations and misperceptions about labor market conditions, or lack of social contacts that can serve as referrals.[1] In view of these problems, many governments have implemented different active labor market policies targeting the youth population.[2]

In this paper we study the impacts of *Programa Primer Paso* (PPP), a large-scale apprenticeship program in the second largest province in Argentina (Córdoba), aimed at improving employability of individuals between 16 and 25 years old, by means of providing a first formal job opportunity. The program gives a large wage subsidy (90% of the hourly minimum wage)

[1]Also, barriers to enter labor market could be the consequence of discrimination against some population subgroups.

[2]For instance, Vezza (2014) surveys many relevant youth employment programs –65 programs– implemented in 18 Latin American countries from 2008 to 2013. Typically, these programs combine training and short-term internships for disadvantaged youth.

during 12 months, the period along which beneficiaries work as apprentices in formally registered firms. An applicant to the program is a pair consisting of a youngster and a firm, that were voluntarily matched prior to the application. The selection of beneficiaries is done through a public lottery, because demand largely exceeds the annual number of available benefits, which creates an experimental setting to estimate causal effects of the program.

We measure impacts in several dimensions: employment, earnings, skills, perceptions and expectations, and job search strategies. We use three different types of data: (i) administrative records (program registries and formal employment from the national tax authority), (ii) a follow-up survey conducted over a representative sample of applicants 12 months after the finalization of the program, and (iii) qualitative data gathered by means of focus groups with beneficiaries and in-depth interviews to key stakeholders of the program. Matching the program registries with administrative records of formal employment allows us to have precise measures of formal labor market performance after participation in the program concluded. The field survey was designed to measure several socioeconomic aspects of the life of these youngsters, as well as to measure perceptions, expectations, and skills development (short tests of cognitive and non-cognitive abilities), allowing us to study the mechanisms through which the program operates. Qualitative data served as a guide for the design of the survey and for the interpretation of some quantitative results.

Previous research has shown mixed results (low to zero) effects of programs only offering in-class training to develop skills for the labor market. This is especially true in developed economies, and although for developing countries the impacts seem to be larger, they are usually short-lived (Kluve et al., 2014). Due to the low or null effectiveness of these training programs in improving labor market performance, most of active labor market programs for the youth, both in developed and developing countries, offer a combination of in-class training with on-the-job training through internships or apprenticeships. Although the evidence is scarce for developing countries, there are two labor market programs combining in-class and on-the-job training that have been rigorously evaluated in Latin America: *Juventud y Empleo* (JE, Dominican Republic) and *Jóvenes en Acción* (JA, Colombia) (Card et al., 2011; Attanasio et al., 2011; Ibarrarán et al., 2014; Acevedo et al., 2015).[3] The first two evaluations of JE found no overall effects on employment rates, but, conditional on being employed, the effects are positive on formality –mainly for males– and on wages (Card et al., 2011; Ibarrarán et al., 2014). However, in a third evaluation of the program, Acevedo et al. (2015) found sizable short term employment and wages gains for women but losses for men, although these effects dissipate in the longer run. Results are mixed regarding the effects of JE on non-cognitive skills of participants. While Ibarrarán et al. (2014) found positive effects on these skills, Acevedo et al. (2015) found that the program reduces self-esteem of males in the longer run. Additionally, the program reduces teenage pregnancy and increases youth expectations about the future (Ibarrarán et al., 2014). In the case of the JA Program, Attanasio et al. (2011) find that the program raises earnings as well as the probability of employment for women, but none of these outcomes are significantly affected for men. Additionally, the authors find that the program has a significant impact on formality, for both men and women.[4]

To the best of our knowledge, our paper is one of the first to isolate the causal impacts of a program only offering on-the-job training. The only exception is Gelber et al. (2015), which evaluates a summer internship program in New York. Specifically, that paper finds that participation in the internship program increases earnings and employment in the year of the program, while it decreases earnings in the three years following participation and has zero effects afterwards.

The estimated impacts of the PPP on labor outcomes using administrative data, during the 12 months after the program concluded (short to medium run impacts), show large gains in the probability of being a formally registered worker. The effects are larger for women for whom the impacts imply a 34% to 57% increase in formal employment, depending on the month analyzed, while for men the impacts range from 12% to 32%. In order to explore the causal effect of the program on labor productivity, we estimate the effects of the program on formal wages, conditional on formal employment. Due to selection into formal employment, we

[3]Both programs incorporated an experimental evaluation design.

[4]Another similar program evaluated through an experimental design is a version of *Entra21*, implemented in the city of Córdoba, Argentina. Alzúa and Cruces (2013) find that this program has a positive effect on formal employment using administrative data.

bound these effects following Attanasio et al. (2011). The most conservative of these bounds are not able to rule out a zero wage effect. Complementing the analysis with the follow-up survey information, we additionally show that 12 months after finishing the apprenticeship the PPP does not affect labor force participation decisions and generates a 10% reduction in unemployment, mainly driven by males.

One important criticism of training and employment policies is that individuals receiving the treatment may improve their employment outcomes at the expense of individuals in the control group (Crépon et al., 2013). If displacement effects exist, our estimates would be upward biased. To address this issue, we propose an exercise that exploits the variability in the share of treated individuals across cities, which resulted from the random assignment process. We show that the estimated effects of the PPP program seems not to be driven by treated individuals displacing control individuals in the labor market.

Finally, we explore the mechanisms behind the positive effects of the PPP on labor outcomes and we do not find direct evidence supporting that differential skill accumulation of treated and control individuals can explain this result. By means of exploring additional evidence from our data sources, we argue that a certification effect may help to explain the positive impacts of PPP on labor outcomes. These results highlight the relevance of informational barriers at the time of the first entry into the labor market.

2 Institutional framework

According to the *Encuesta Permanente de Hogares* (EPH) the youth unemployment rates in Argentina during the 2000's have been always 2 to 2.5 times above the national unemployment rate.[5] Importantly, while the youth represent 20% of the active population, they account for approximately for the 40% of all unemployed individuals. In 2012, the country unemployment rates were 17% and 7% for the youth and the overall population, respectively.

In this paper we study the 2012 edition of the *Programa Primer Paso* (PPP), a youth employment program administered by the *Agencia de Promoción del Empleo y Formación*

[5]The youth unemployment rates are computed for individuals aged 16-25 years old.

Profesional (from now on, *Agencia*), a ministry-level agency in the Province of Córdoba (Argentina), aimed at improving employability of individuals between 16 and 25 years old, by means of providing a first formal job opportunity. The PPP is basically an apprenticeship program, which gives no formal training other than the on-the-job training, and which provides a wage subsidy in the form of a monthly pay to apprentices. Firms can voluntarily supplement this pay, and the government pays for an insurance covering job risks. The magnitude of the subsidy is large, as in 2012 it represented around 90% of the hourly legal minimum wage. The number of beneficiaries in 2012 was around 10,000.[6]

The PPP was first launched in 1999 and it remained operative on an annual basis until 2007. During those first editions the program selected its beneficiaries in a first-come-first-served basis. The PPP was discontinued between 2008 and 2011 and reappear in 2012, and since then the selection of most of beneficiaries is done through a lottery.

The theoretical coverage of the program can be computed comparing the 10,000 beneficiaries to the number of young individuals who are eligible to apply to the program. The eligibility criteria only exclude young workers who have been formally registered during at least one month in the 6 months prior to the deadline for application (May 2012 for the 2012 Edition, PPP2012). According to this criterion, the theoretical coverage is around 2%, if the target population includes all young individuals who are not formally employed (unemployed, informal workers, and inactive individuals).[7] Instead, this coverage reaches 5.4% if we restrict the target population to young individuals who were participating in the labor market, but were not formally employed by that date.

There are some other formal requirements that young individuals need to fulfill to be eligible: they cannot be beneficiaries of other national or local employment or social programs (except for the Asignación Universal por Hijo), and they cannot be related to the owner of the firm

[6] In November 2014, the Congress of Córdoba passed the "PPP law" (*Ley Provincial* 10236) that converts the program into a policy. From 2015 onwards the PPP is mandated to cover at least 15,000 beneficiaries a year.

[7] Using labor market data for the 2nd. quarter of 2012 from the national household survey data (EPH). Notice that these computations rely only on data for the districts of Córdoba Capital and Río Cuarto, which together accounts for 48% of total population in the Province of Córdoba, and which are the only two urban agglomerates surveyed by the EPH on a quarterly basis in this province. Since beneficiaries are allocated according to population shares, the theoretical coverage uses 4,800 benefits as the numerator.

together with which they form the match of the application. All these requirements are cross-checked by means of administrative data, prior to the lottery takes place. For the case of firms, eligibility establishes that only formally registered firms with at least one registered employee can form a match with a young individual. However, this condition is relaxed for the poorest area in the province, namely the Northwest region (*Noroeste Cordobés*, or NOC). In the year 2012 there were about 40,000 applications.

The program have three different modalities: (1) training; (2) long-term contract (*Contrato Tiempo Indefinido*, or CTI); and (3) disable workers. Among the 10,000 available benefits a year, modalities (2) and (3) have the priority and take as many places as applicants demand. The rest of benefits, which are the focus of this paper, are randomly assigned among all applicants to the first modality, which usually takes the most of benefits (around 80%). To apply to the training modality a young individual need to present his or her application with the endorsement of a firm that is willing to hire him or her, in the case of resulting beneficiary in the lottery. The lottery is public and takes place every year around mid of May in the *Lotería de la Provincia de Córdoba*.

In the random assignment process there are two different types of quotas. The first one requires that the shares of beneficiaries in each one of the 26 counties mimic population shares. The second one restricts the number of PPP beneficiaries in each firm according to its size, measured by the number of formally registered employees.[8] These two quotas affect the probability that each application is assigned to the treatment group, and for this reason they will be included as control variables in all regressions.

3 Data

Our data come from several sources. First, the administrative records of all application forms of the PPP2012, which consist of matches between individuals and firms. For each match we have baseline characteristics such as age, gender, marital status and the neighborhood, city

[8]See Appendix 1 for a more detailed description of how these quotas operate.

and county of residence of the individual,[9] the number of employees of the firm, whether the application was paper or internet-based, and whether the match was selected in the lottery. We complement this information with the monitoring registries of the program, which include information about the number of months each individual was active in the program (between June 2012 and May 2013). We add to these baseline characteristics information coming from official population census data. We match the applicants dataset to the Census 2008 database,[10] from which we obtain unemployment, Unsatisfied Basic Needs (UBN), and labor informality rates at the neighborhood -or city- level of residence of the applicant.

Information about formal employment comes from administrative records of the national tax authority (*Administración Federal de Ingresos Públicos*). We construct variables of formal employment status and (monthly) salaries during a period of 30 months, from January 2012 to June 2014 (5 pre-treatment, 12 during treatment, and 13 post-treatment months).

Finally, we conducted a follow-up survey on the second half of May 2014, for a random subsample of applicants residing in the City of Córdoba (both beneficiaries a non-beneficiaries) that gathered information about socioeconomic characteristics, a large set of labor outcomes, life and job satisfaction, measures of cognitive and non cognitive skills, consumption of durable goods, risky behaviors (drug and alcohol consumption, participation in minor crimes and physical fights), among others. The survey was computer-based and all respondents were contacted by phone and invited to participate in the survey.[11] The objective of the recruitment team was to reach a survey sample size of 1000 individuals, which was accomplished since the final number of respondents was 1019.

[9]The neighborhood is only available for individuals living in the capital city, Cordoba.

[10]The *Censo Provincial de Poblacion y Vivienda* is the pilot for the 2010 national Argentinean census, and was conducted in 2008.

[11]The survey was conducted on the 17th, 24th and 31st of May 2014, at the facilities of the *Universidad Nacional de Córdoba.* Contacted applicants to the PPP 2012 were offered a stipend equivalent to 12 or to 19 USD, according to the informal or the official exchange rate in Argentina, respectively. Given that this program was not originally designed to be evaluated, the quality of contact information of applicants was rather poor, and only around a third of telephone numbers in the administrative database were correct. Since there is no a priori information indicating that this feature of the data could lead to a selection problem (those individuals with correct contact information being different of those with incorrect contact information), this low fraction is not much of a concern. In addition, as shown in Table 3, there are no significant differences in observed characteristics among the final surveyed population and the overall population of applicants in the City of Córdoba.

3.1 Sample

For the purpose of this paper, we consider the subgroup of applicants to the 2012 PPP Edition, who fulfilled all the requirements and applied under the training modality. These are the individuals that were assigned to the program through the lottery process (see Figure 1). This subgroup consists of 26,060 applications from 24,663 different individuals, since individuals are allowed to present more than one application (different matches with different firms). Only 4.9% of individuals have more than one application (mostly up to two applications).

We drop from our sample applications corresponding to firms located in the Northwest region of the province (NOC), which account for 11 % of all applications. The design of the program favors this disadvantaged area, allowing matches to firms that are not formally registered, which implies a different type of treatment to that received by beneficiaries in the rest of the province.

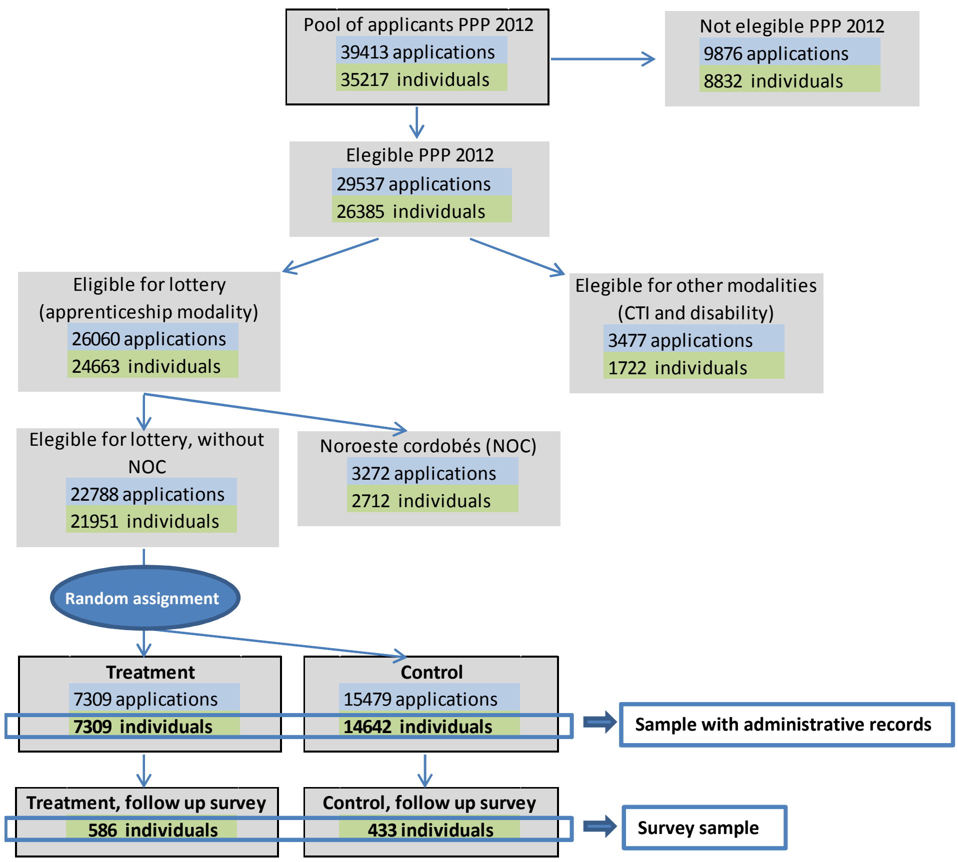
Our final sample consists of 21,951 individuals, with 7,309 individuals in the treatment group and 14,642 individuals in the control group. If a treated individual has more than one application, we only keep the raffled one, and drop the other applications. If an individual in the control group has more than one application, we randomly keep one and drop the others.

We administered a follow up-survey to a subset of 1,019 individuals residing in the city of Córdoba during the second half of May 2014, with 586 individuals in the treatment group and 433 individuals in the control group. The recruiting of these individuals was designed so as to maintain the balance between treatment and control groups and to be representative of all applicants in the city of Córdoba.

3.2 Baseline comparisons

Table 1 shows differences in baseline characteristics between those originally offered the PPP –treatment group– and those not offered the program –control group–, for all individuals as well as separately for men and women. If randomization was successful, there should not be significant differences in these characteristics. Given that there are specified quotas for different firm sizes and by county of residence, we test the existence of differences between

Figure 1: Pool of applicants, random assignment and final samples



groups after controlling for those quotas. Additionally, we control for the number of applications each individual fulfilled.[12] The reason to use these controls is that all these dimensions alter the probability of being a beneficiary of the program. Notice that although there are minor differences in some baseline measures, they are not economically significant.

Table 2 performs the same balancing exercise for the survey sample, and shows that treatment and control groups are balanced. Additionally, Table 3 shows that the sample of individuals that actually showed up for the follow up survey is similar to the group of all applicants in the city of Cordoba, except that surveyed females are slightly less likely to be married, and surveyed males have a slightly better socioeconomic background.

4 Empirical Strategy

We measure the effect of the PPP on participants of the 2012 Edition. The apprenticeships period goes from June 1st, 2012 to May 31st 2013 (Figure 2). After graduation, we are able to follow all the applicants during 13 months (June 2013 to July 2014) in the administrative records on formal employment. Additionally, in May 2014 a follow-up survey was conducted on a representative sample of applicants. These two sources of data allow us to estimate the effects of the program around a year (13 or 12 months, respectively) after the apprenticeship concluded.

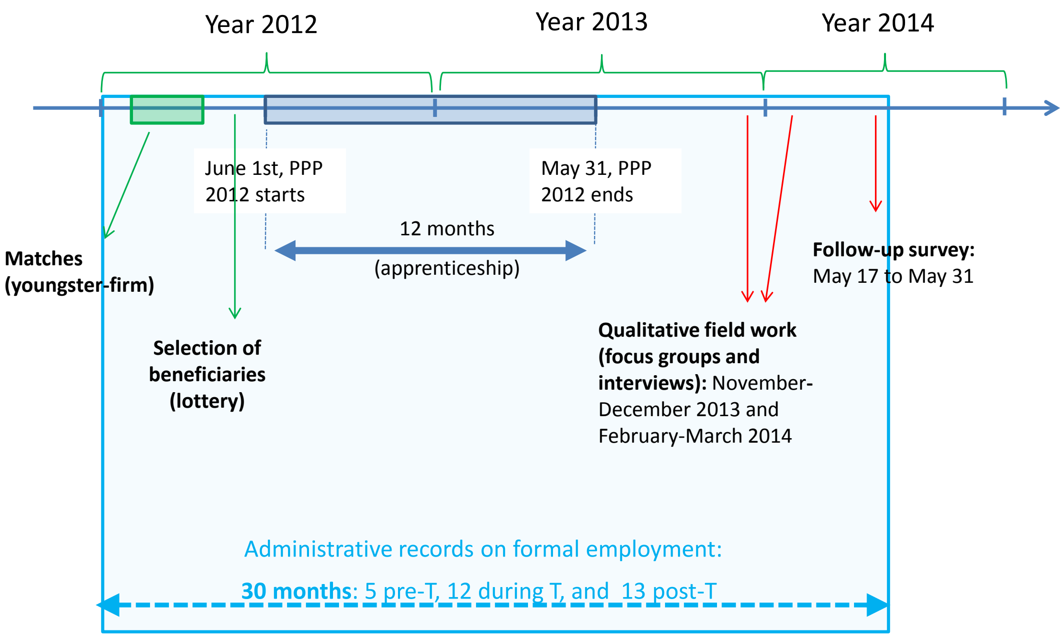
Let's define Y_{1it} and Y_{0it} the outcomes of interest of individual i, with and without the treatment, t periods after graduation. The effect of interest is

$$\tau_t = \mathbb{E}\{Y_{1it} - Y_{0it}\}. \tag{1}$$

Given the random assignment of beneficiaries and that we can follow the complete universe of applicants to the PPP2012 in the formal labor market during 13 months after graduation —participation and wages in the the formal labor market— the average outcome under treatment for the universe of applicants is equal to the average of the observed outcome for those

[12]Balance tests do not change if we exclude these controls.

Figure 2: Timeline



randomly offered the PPP. Similarly, the counterfactual may be estimated using the average of the observed outcome for those not selected for the PPP. Hence, the parameter of interest is estimated with the sample analog of

$$\tau_t = \mathbb{E}\{Y_{1it} - Y_{0it}\} = \mathbb{E}\{Y_{it}|D_i = 1\} - \mathbb{E}\{Y_{it}|D_i = 0\}, \qquad \text{for} \quad t = 1, ..., 13. \tag{2}$$

where $D_i = \{0, 1\}$ is an indicator of whether individual i was randomly selected through the lottery process to be beneficiary of the PPP and Y_{it} is the observed outcome t months after graduation (from June 2013 to June 2014). Randomization guarantees the balance in characteristics between control and treatment groups, as we show in Table 1.

To estimate the impacts of the program we rely on the original randomization. However, not all of them were actually treated, since 97% took up the benefit and most of them (83%) remained treated during 12 months. Only 3% of individuals in the control group received the benefit. Given that there is not full compliance, the estimated effect should be interpreted as an intention-to-treat effect, although its value may not be very far from the average treatment effect.

We use information in the follow-up survey to estimate the causal effects of the PPP on a broader set of outcomes. The survey is representative of the treatment and control group (in the city of Córdoba), therefore we can estimate the effects of interest using equation (2) for $t = 12$. Table 2 shows that treatment and control groups are balanced in pre-treatment characteristics.

5 Results

5.1 Formal employment and wages

A first aspect to analyze is whether the PPP has an impact on the employment prospects of the beneficiaries and, particularly, whether it is capable of improving the quality of the jobs they have access to, i.e. access to formal jobs. Figure 3 summarizes the estimated effects of PPP on the probability of being employed in the formal sector, by gender, using administrative data for three sub-periods (pre-treatment, during PPP, and post-treatment). Table 4 reports the estimated coefficients.

The figure shows that during the pre-treatment period (January 2012 to May 2012) there are not differences between treatment and control individuals, indicating that both groups are balanced in this pre-treatment characteristic. From June 2013 to May 2013, treated individuals where part of the PPP program, and, according to the results, they are less likely to be formally employed. The reason for this is that during the apprenticeship firms are not required to register the beneficiary as formal employee. The program increases the probability of being a formal worker during the whole post-treatment period —from June 2013 to June 2014—, both for males and females, and the magnitude of the effect remains quite stable along the period of analysis. For females, the effects go from around 3 percentage points (pp) in June 2013 to more than 6 pp in June 2014, implying an increase of 34% and 47%, respectively, relative to females in the control group. The effect is also positive and quite stable for males, going from from 2 pp in June 2013 to around 5 pp in June 2014. However, this effect is smaller comparing to those estimated for females, since for males the impacts imply an increase of 12% to 21% relative to their counterparts in the control group.[13]

The PPP can also impact wages, and it can do so through at least two mechanisms: the apprenticeship may have improved the stock of human capital of workers (skills development by on-the-job learning) and/or it may have boosted the quality of the matches formed after the program. We name these two mechanisms together as a "productivity effect". Using administrative data on formal wages we are able to explore whether this productivity effect is at work. To do so we estimate the effects of the treatment on formal earnings for the subsample of formally employed individuals (see Table 6). However, with this strategy it is not possible to analyze the existence of the productivity effect due to the differential selection into

[13]It is worth to note that some individuals in the control group were beneficiaries in posterior editions of the PPP program. This means that during the post-treatment period of the 2012 PPP Edition (June 2013 onwards) some individuals in the control group were PPP apprentices. Specifically, 5.8% were beneficiaries in the 2013 Edition starting on June 2013, and 2.2% were beneficiaries in the PPP2014plus edition, starting on January 2014. Table 5 shows that results are robust to the exclusion of individuals in the control group that participated in subsequent editions of the PPP program. Moreover, individuals that were beneficiaries of the 2013 or 2014 PPP are similar in observable characteristics to control individuals that did not participate in subsequent editions, except that they are slightly younger.

Figure 3: Effect of the program on the probability of formal employment

(a) Female

(b) Male

Source: authors' calculations.

formal employment of treatment and control individuals. To get rid of this selection concern we construct lower and upper bounds of the effect of the PPP on wages following Attanasio et al. (2011). Results are reported in Table 7.[14] Using a conservative approach we cannot rule out a zero productivity effect, given that the lower bound is always below zero (columns 2, 6 and 10). However, assuming that the average wage of the always-takers without treatment is at least as large as the average wage of the compliers, gives a lower bound that takes a positive value (denoted as "Lower Bound Attanasio" in columns 3, 7 and 11). This effect is moderately low and in any case cannot be interpreted as the sole effect on labor productivity (or on skills) since it is not possible to separate it from the fact that workers may now, as a consequence of the PPP, be employed in higher productivity firms.

5.2 Labor force participation and unemployment

Using data from the follow up survey, conducted 12 months after finishing the apprenticeship, we estimate the effect of the PPP on labor force participation decisions and unemployment status. Table 8 shows that the PPP did not change labor force participation decisions, and this conclusion holds true for both female and male subsamples. This Table also shows that the

[14] A detailed explanation of the construction of bounds is presented in Appendix 2.

rate of unemployment is 6 pp lower in the treatment than in the control group (the mean in the control group is 60%), 12 months after the PPP2012 was over. This fall in unemployment, equivalent to a 10% reduction, can be interpreted as causal given the first result indicating that labor force participation did not change as a consequence of the program.

However, the fall in unemployment is mostly explained by what happens in the male subsample. For male individuals unemployment is 8 pp lower in the treatment than in the control group, in which 57.2% of individuals remain unemployed. Even though the sign of the effect goes in the same direction for the case of women, the estimated impact is not statistically significant.[15]

6 Displacement

The empirical strategy proposed in equation (2) assumes that the potential outcomes of an individual i only depend on his own treatment status, regardless of how the treatment is distributed among the eligible population. However, one important criticism against training and employment policies is that individuals receiving treatment may improve their employment prospects at the expense of crowding out individuals in the control group. This means that the employment rate of individuals in the control group is lower than it would have been absent the program, which violates the stable unit treatment value assumption, or SUTVA (Rubin 1980, 1990). Moreover, the larger the share of treated individuals assigned to treatment, the greater the displacement effect could be. If this is the case, our results are overestimating the effect of the PPP on employment outcomes.

An ideal setup to identify displacement effects over the control group would consist on a two step randomization procedure, where in the first step one randomly assigning to different cities or labor markets the share of individuals to be treated, and, after that, randomly assign individuals to treatment and control group at the chosen rate (Crépon et al., 2013). Under

[15] Other interesting result obtained in the follow-up survey confirms what was obtained through administrative employment records, saying that labor informality is lower in the treatment than in the control group. In the follow up survey we approximate formality as having a job which offers either health insurance, social security contributions or paid vacations. Additionally, the impacts in both formal employment probability and formal wages are also positive and statistically significant for the subsample of individuals in the survey sample.

this ideal setup, one can identify displacement effects comparing the labor performance of control individuals in cities with positive shares of treated individuals with the performance of individuals in a "super control" group (eligible individuals in 0% assignment areas). Although the PPP program was not designed to randomly assign the share of individuals to be treated across cities, the lottery resulted in substantial variation across the 289 cities (see Table 9). According to Figure 4, the share of treated individuals takes values in the whole interval 0-1, and this variability is present even within county. Additionally, Table 10 shows that these shares are uncorrelated with several city characteristics after controlling for county fixed effects.[16] We exploit this variation to explore the existence of displacement effects in the control group, running the following regression with the subsample of control individuals:

$$y_{itj} = \alpha + \delta^m C^m + \delta^h C^h + \lambda X_j + u_{itj} \tag{3}$$

where y is formal employment, C^m equals 1 if i lives in a city with a moderate share of treated individuals, C^h equals 1 if i lives in a city with a high share of treated individuals, X_j are characteristics of city j, and t goes from June 2012 to July 2014 (during and post-PPP period).[17]

If the PPP program induces displacement in the control group, coefficients δ^m and δ^h should be negative, and δ^h should be larger in absolute terms. This exercise have its own limitations because if there were displacement in cities with a low share of treated individuals, we would not be able to identify it. In that case, the δ estimates would be a downward bias estimate of the true displacement in areas with moderate and high shares. Table 11 reports the estimates of equation 3, which indicate no systematic evidence of displacement.

[16]Since quotas are set at the county level it automatically creates correlation between county and share of treated individuals. Hence, we need to control for county fixed effects.

[17]Cities with a low share of treated individuals are those where less than 30% of applicants received treatment; cities with a moderate share have 30% to 45% of the applicants receiving treatment; and cities with a high share have more than 45% of the applicants receiving treatment

7 Mechanisms

The better labor outcomes described in the previous sections can result from the interaction of several mechanisms. In particular, these mechanisms may have to do with skills accumulation or with the relaxation of some barriers to entering the labor market for the first time, among which informative barriers -e.g., lack of signalling of unobserved productivity- can play a key role (Pallais, 2014). To be able to disentangle the mechanisms behind the results caused by the PPP, we analyze two types of evidence. First, we use information obtained in the follow-up survey, which includes measures designed in order to explore the mechanisms driving the changes in labor market performance of program beneficiaries. In particular, this survey included a broad set of measures on skills development, job search strategies, and expectations and perceptions about labor market conditions. Second, we look at heterogeneous effects of the PPP on two populations subgroups: among individuals older than 18, we separate the estimates for those who are high school graduates and those that did not graduate from high school (who by that age should have already attained that degree).

The first part of this section is intended to analyze whether the PPP promoted, among treated individuals, the development of some skills that are valuable for employers. The last part of this section focuses on which type of barriers, other than those related to human capital or skills, may have been relaxed by the PPP: (1) lack of social contacts that can serve as referrals; (2) lack of knowledge about how to conduct an efficient job search; and (3) low expectations and misperceptions about labor market conditions (which may imply that young individuals look for or choose the “wrong” jobs); (4) lack of certification of unobserved potential productivity (signalling problem).

7.1 Skills development

The process of skills development in a program like the PPP can take place as learning-by-doing, since the program does not require any sort of formal training other than what the hiring firm usually uses as informal training for their new employees. The learning-by-doing process is intimately related to the type of tasks that the worker do. Panel A in Table 12 reports the qualification of job tasks for individuals who were employed by the date of the survey, which of course may introduce some selection and impede the interpretation of the estimates as if they were causal impacts of the program. However, it is worth noticing that none of the estimates are significantly different from zero for the total sample, and only for the case of men we observe that the job tasks of treated and employed individuals require less often that they operate mechanical machinery and more often to use computers, compared to control individuals that were also employed.

More direct measures of skill development are obtained from the indicators of cognitive and non-cognitive skills that we obtained in the survey. Even though the measures of cognitive skills are, by its definition and construction, not very likely to be changed by having been part of the PPP, we still report in Panel B of Table 12 the estimates of the PPP on three different measures of cognitive abilities, and find no significant effects.[18] Regarding socio-emotional skills, the estimates (Panel C in Table 12) also show no significant impacts.[19]

In addition, in the survey we also produced a number survey experiments (endorsement experiments) in order to elicit truthful responses to sensitive questions addressing issues of good behavior, attitudes and beliefs in the workplace.[20] We also find no impacts on these measures.[21]

As a whole, this evidence seems not to support a channel related to skill acquisition as the driver of the better labor market outcomes caused by the PPP, which is in line with the finding that the PPP does not have productivity effects reflected in higher wages –at least according to our most conservative bounds estimates in Table 7.

[18]It is worth to mention that recent literature in psychology and economics is showing that cognitive traits are not very malleable at this age, contrary to what could happen with the measures of socio-emotional skills, which are more likely to be affected even in first adulthood. Therefore, this result coincides with what was expected beforehand.

[19]Brenlla (2014) is the technical note explaining the definitions and characteristics of these measures included in the survey.

[20]These experiments were designed to measure the impact of the PPP in the agreement with the following statements: *lack of timekeeping in the workplace should be punished*, *maltreatment at the workplace is unacceptable*, and *teamwork helps in being more efficient at work*.

[21]However, given the sample size available the tests are low-powered.

7.2 Signaling/certification of work experience

The PPP not only offers beneficiaries the possibility to have a labor market experience and aquire, eventually, skills trough on the job training, but it also provides the opportunity to credibly certify this experience, as long as the apprenticeship takes place in a formal firm. This certification is important for employers – who may not have sufficient incentives to hire inexperience workers whose abilities are uncertain– as it provides a signal of the capacities of the young worker. Hence, part of the effects of the PPP on employment outcomes may be explained by a signaling/certification effect, and not by the acquisition of new skills provided by the experience per se.

If the signal of having had a formal work experience is valuable for prospective employers (as a way to infer unobserved productivity of the young worker), then it should be the case that PPP has a greater impact on individuals that lack certification of their abilities. An exercise that allows to explore this hypothesis is to compare the impact of the PPP across individuals with and without certification provided by the educational system (high school completed versus dropouts). Figure 6 shows the heterogeneous impacts by educational level for individuals older than 18 years old who completed high school education (left panel) and for those that did not (right panel). A comparison between the two panels in this figure indicates that the effects of the PPP are much larger (as % of the mean observed for the control group) for individuals who has not been signaled by the educational system.

Another exercise provides further evidence supporting the signaling hypothesis. Given that the signal of having worked for a formal firm was randomly assigned among applicants, some low types may take advantage of this and use the certification that the PPP provides to look as high types at least until the employer discovers their true type. This is exactly what we find in Figure 7 , where we show heterogeneous impacts for individuals with different levels of measured ability (bottom 50% of the distribution of results in the cognitive test, versus top 50% in that distribution). Notice there that there are positive impacts for both groups during the first months after the program finished, although these effects vanish for the low type group.

7.3 Other barriers to enter the labor market

To explore whether the results about labor market performance are related to other barriers, we examine measures obtained in the survey regarding job search strategies, proxies of social ties (share of leisure time spent with different groups of people), and expectations and perceptions regarding labor market conditions. As shown in Table 13, most of the effects are not statistically significant, except for a few estimates regarding expectations about the future. Hence, we conclude we find no robust evidence that the PPP relaxes these other barriers.

8 Conclusions

Labor market outcomes for the youth are noticeably worse than for adults. Both unemployment and informal employment are more common among younger workers, specially among those from disadvantaged backgrounds. These problems are likely to be associated to barriers in entry-level labor markets. Moreover, a bad start -i.e., a low quality first job experience- is likely to have persistent effects on labor markets prospects, which makes these features of the entry-level jobs more worrying.

In this paper we provide evidence on the importance of entry-level job experience in the formal sector, which proxies for a high quality job for the youth. To identify the causal effects of interest we exploit a lottery that randomly assigns vacancies to an apprenticeship program in formal firms.

We measure impacts in several dimensions, using both administrative and survey data. We find that the apprenticeship program caused large short-to-medium run gains in the probability of formal employment and a moderate fall in unemployment. The evidence about the impacts on wages is not conclusive and our preferred interpretation of the estimates on this regard says that if there is any positive effect on (formal) salaries, its size is not large. This null to low impacts on wages inform about the importance of mechanisms other than human capital (skills) accumulation as the main channels through which the program actually boosted the labor market outcomes of beneficiaries.

We also explore in other ways which alternative mechanisms could be driving the observed impacts and we find evidence favoring a signaling effect. Moreover, exploiting a special feature of the random assignment process, we are able to rule out the existence of one type of general equilibrium effects (displacement). Therefore, the overall evidence indicates that the program produces a signal that is valuable in the labor market while it does not crowds out the job opportunities of non beneficiaries.

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Table 1: Baseline differences between treatment and control group

Baseline Characteristics	(1) Mean Control group	(2) Difference	(3) se	(4) N
A. All				
Female	0.548	-0.016**	(0.007)	21,939
Age (years)[a]	21.017	0.045	(0.036)	21,932
Single[a]	0.936	0.002	(0.003)	21,939
Have children[a]	0.109	-0.010**	(0.004)	21,939
High school graduate (if +18 years old)[a]	0.643	0.015*	(0.008)	16,029
College graduate (if +21 years old)[a]	0.090	0.013*	(0.008)	7,274
UBN rate 2008 (city or neighborhood)	0.075	0.001	(0.000)	21,671
Unemployment rate 2008 (city or neighborhood)	0.057	-0.000	(0.000)	21,711
Labor informality rate 2008 (city or neighborhood)	0.434	0.001	(0.002)	21,671
Paper application	0.631	0.006	(0.007)	21,939
Fomal employment Jan 2012	0.012	0.001	(0.002)	21,939
Fomal employment Feb 2012	0.004	0.000	(0.001)	21,939
Fomal employment Mar 2012	0.006	0.002	(0.001)	21,939
Fomal employment Apr 2012	0.010	0.001	(0.001)	21,939
Fomal employment May 2012	0.019	0.000	(0.002)	21,939
Wage in formal job Jan 2012	17.862	6.200	(4.060)	21,939
Wage in formal job Feb 2012	5.092	3.853	(2.752)	21,939
Wage in formal job Mar 2012	7.404	3.367	(2.924)	21,939
Wage in formal job Apr 2012	12.109	2.465	(2.640)	21,939
Wage in formal job May 2012	36.380	6.188	(5.611)	21,939
B. Female				
Age (years)[a]	21.432	0.069	(0.049)	11,899
Single[a]	0.923	0.001	(0.005)	11,902
Have children[a]	0.162	-0.008	(0.007)	11,902
High school graduate (if +18 years old)[a]	0.713	0.015	(0.010)	9,317
College graduate (if +21 years old)[a]	0.109	0.017	(0.010)	4,765
UBN rate 2008 (city or neighborhood)	0.075	0.000	(0.001)	11,779
Unemployment rate 2008 (city or neighborhood)	0.056	-0.000	(0.000)	11,793
Labor informality rate 2008 (city or neighborhood)	0.436	-0.000	(0.003)	11,779
Paper application	0.641	0.000	(0.009)	11,902
Fomal employment Jan 2012	0.009	0.001	(0.002)	11,902
Fomal employment Feb 2012	0.003	0.001	(0.001)	11,902
Fomal employment Mar 2012	0.004	0.004***	(0.002)	11,902
Fomal employment Apr 2012	0.008	0.001	(0.002)	11,902
Fomal employment May 2012	0.014	0.001	(0.002)	11,902
Wage in formal job Jan 2012	14.886	6.218	(5.641)	11,902
Wage in formal job Feb 2012	3.836	6.397	(4.353)	11,902
Wage in formal job Mar 2012	5.279	7.951*	(4.682)	11,902
Wage in formal job Apr 2012	10.340	1.332	(3.161)	11,902
Wage in formal job May 2012	28.851	1.926	(7.047)	11,902
C. Male				
Age (years)[a]	20.512	0.044	(0.051)	10,033
Single[a]	0.952	0.003	(0.004)	10,037
Have children[a]	0.045	-0.009**	(0.004)	10,037
Secondary education complete (if +18 years old)[a]	0.543	0.022*	(0.013)	6,712
College graduate (if +21 years old)[a]	0.051	0.009	(0.010)	2,509
UBN rate 2008 (city or neighborhood)	0.075	0.001*	(0.001)	9,892
Unemployment rate 2008 (city or neighborhood)	0.057	0.000	(0.000)	9,918
Labor informality rate 2008 (city or neighborhood)	0.430	0.002	(0.003)	9,892
Paper application	0.619	0.013	(0.010)	10,037
Fomal employment Jan 2012	0.015	0.001	(0.003)	10,037
Fomal employment Feb 2012	0.005	-0.000	(0.001)	10,037
Fomal employment Mar 2012	0.009	-0.001	(0.002)	10,037
Fomal employment Apr 2012	0.012	-0.000	(0.002)	10,037
Fomal employment May 2012	0.024	-0.000	(0.003)	10,037
Wage in formal job Jan 2012	21.475	6.163	(5.839)	10,037
Wage in formal job Feb 2012	6.617	0.871	(3.158)	10,037
Wage in formal job Mar 2012	9.984	-1.947	(3.246)	10,037
Wage in formal job Apr 2012	14.257	3.753	(4.360)	10,037
Wage in formal job May 2012	45.520	10.362	(8.906)	10,037

Note:[a] Measured at the date of application. Column (2) reports the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Column (3) reports robust standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Survey Sample: Baseline differences between treatment and control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		A. All				**B. Female**				**C. Male**		
Baseline Characteristics	**Mean Control Group**	**Diff.**	**se**	**N**	**Mean Control Group**	**Diff.**	**se**	**N**	**Mean Control Group**	**Diff.**	**se**	**N**
Female	0.488	0.031	(0.031)	1,019								
Age (years)	21.753	0.136	(0.158)	1,019	22.184	0.058	(0.229)	513	21.342	0.152	(0.213)	506
Single	0.959	-0.003	(0.013)	1,019	0.943	0.020	(0.019)	513	0.973	-0.026	(0.017)	506
Have children	0.053	0.008	(0.015)	1,019	0.085	0.009	(0.026)	513	0.023	0.003	(0.014)	506
High School graduate (+18 years old)	0.786	-0.041	(0.037)	561	0.892	-0.073*	(0.041)	296	0.663	-0.006	(0.060)	265
College graduate (+21 years old)	0.101	-0.032	(0.037)	244	0.102	0.005	(0.053)	141	0.100	-0.086	(0.056)	103
Unemployed mother	0.104	-0.011	(0.019)	1,019	0.090	-0.007	(0.025)	513	0.117	-0.015	(0.028)	506
Unemployed father	0.069	0.016	(0.017)	1,019	0.080	0.006	(0.025)	513	0.059	0.022	(0.023)	506
Education mother up to high school	0.685	-0.005	(0.030)	1,006	0.681	0.033	(0.042)	506	0.689	-0.043	(0.042)	500
Education father up to high school	0.708	-0.001	(0.030)	958	0.710	0.003	(0.042)	479	0.706	-0.002	(0.042)	479
Lowest SES	0.237	-0.006	(0.027)	1,019	0.231	0.001	(0.038)	513	0.243	-0.011	(0.038)	506
Middle -low SES	0.159	-0.000	(0.023)	1,019	0.189	-0.011	(0.035)	513	0.131	0.007	(0.031)	506
Middle SES	0.325	0.007	(0.030)	1,019	0.321	0.011	(0.042)	513	0.329	0.005	(0.042)	506
Middle high SES	0.256	-0.016	(0.027)	1,019	0.236	-0.017	(0.038)	513	0.275	-0.014	(0.040)	506
Highest SES	0.023	0.014	(0.011)	1,019	0.024	0.015	(0.015)	513	0.023	0.013	(0.015)	506
Material deprivation rate in households 2008 (neighborhood)	0.137	0.011*	(0.006)	1,019	0.142	0.013	(0.009)	513	0.132	0.009	(0.008)	506
UBN rate 2008 (neighborhood)	0.084	0.005	(0.005)	1,019	0.090	0.005	(0.008)	513	0.079	0.006	(0.006)	506
Head of households dropout highschool 2008 (neighborhood)	0.490	-0.008	(0.015)	1,019	0.498	-0.011	(0.021)	513	0.482	-0.007	(0.020)	506
Head of households with college degree 2008 (neighborhood)	0.116	0.003	(0.007)	1,019	0.114	-0.001	(0.010)	513	0.117	0.007	(0.010)	506
Illiteracy rate 2008 (neigghborhood)	0.024	-0.000	(0.001)	1,019	0.024	-0.000	(0.001)	513	0.024	-0.000	(0.001)	506
Population aged 15-19 y.o. in formal education 2008 (neighborhood)	0.707	-0.005	(0.009)	1,019	0.703	-0.001	(0.013)	513	0.711	-0.007	(0.012)	506
Population aged 20-24 y.o. enrolled in univ. 2008 (neighborhood)	0.295	0.011	(0.013)	1,019	0.293	0.014	(0.020)	513	0.296	0.009	(0.019)	506
Population aged 20-24 y.o. in post-sec. educ. 2008 (neighborhood)	0.383	0.007	(0.015)	1,019	0.378	0.013	(0.021)	513	0.388	0.002	(0.020)	506
Labor informality rate 2008 (neighborhood)	0.318	0.002	(0.010)	1,019	0.324	0.002	(0.014)	513	0.313	0.001	(0.013)	506
Unemployment rate 2008 (city or neighborhood)	0.070	0.003**	(0.001)	1,019	0.071	0.003	(0.002)	513	0.070	0.003*	(0.002)	506
Fomal employment Jan 2012	0.009	0.003	(0.006)	1,019	0.014	-0.002	(0.011)	513	0.005	0.006	(0.008)	506
Fomal employment Feb 2012	0.005	-0.003	(0.004)	1,019	0.009	-0.007	(0.008)	513	0.000	0.000	(0.000)	506
Fomal employment Mar 2012	0.012	-0.001	(0.007)	1,019	0.019	-0.012	(0.011)	513	0.005	0.009	(0.008)	506
Fomal employment Apr 2012	0.014	-0.003	(0.007)	1,019	0.024	-0.017	(0.012)	513	0.005	0.009	(0.009)	506
Fomal employment May 2012	0.018	0.002	(0.009)	1,019	0.014	-0.003	(0.010)	513	0.023	0.009	(0.015)	506
Wage in formal job Jan 2012	29.593	3.940	(25.891)	1,019	31.937	2.952	(39.872)	513	27.354	2.878	(33.928)	506
Wage in formal job Feb 2012	12.311	-8.883	(13.184)	1,019	25.203	-19.717	(27.874)	513	0.000	0.000	(0.000)	506
Wage in formal job Mar 2012	18.155	-2.902	(15.317)	1,019	36.535	-23.274	(30.065)	513	0.604	15.559	(10.725)	506
Wage in formal job Apr 2012	26.532	-2.277	(19.761)	1,019	41.501	-24.639	(31.342)	513	12.239	18.094	(27.740)	506
Wage in formal job May 2012	31.705	15.602	(21.919)	1,019	43.353	-16.929	(34.146)	513	20.581	48.700	(30.628)	506
Paper application	0.493	-0.005	(0.032)	1,019	0.509	-0.056	(0.045)	513	0.477	0.050	(0.045)	506

Note: Column (2),(6) and (10) report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Survey sample representativeness (city of Cordoba)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A. All				**B. Female**				**C. Male**			
Baseline Characteristics	**Eligible in Cordoba city**	**Diff.**	**se**	**N**	**Eligible in Cordoba city**	**Diff.**	**se**	**N**	**Eligible in Cordoba city**	**Diff.**	**se**	**N**
Female	0.523	-0.019	(0.017)	8,882								
Age (years)	20.459	0.101	(0.083)	8,862	20.841	0.086	(0.120)	4,598	20.040	0.146	(0.113)	4,264
Single	0.947	0.011*	(0.007)	8,882	0.937	0.022**	(0.010)	4,612	0.958	-0.000	(0.009)	4,270
Paper application	0.515	-0.022	(0.017)	8,882	0.527	-0.038	(0.023)	4,612	0.503	-0.005	(0.024)	4,270
Material deprivation rate in households 2008 (neighb.)	0.153	-0.009***	(0.003)	8,842	0.157	-0.004	(0.005)	4,598	0.149	-0.013***	(0.005)	4,244
UBN rate 2008 (neighb.)	0.092	-0.003	(0.003)	8,842	0.092	0.002	(0.004)	4,598	0.092	-0.009***	(0.003)	4,244
Head of hhd dropout high school 2008 (neighb.)	0.491	-0.003	(0.008)	8,842	0.485	0.010	(0.011)	4,598	0.498	-0.018	(0.011)	4,244
Head of hhd with college degree 2008 (neighb.)	0.117	-0.000	(0.004)	8,842	0.119	-0.007	(0.005)	4,598	0.115	0.006	(0.006)	4,244
Illiteracy rate 2008 (neighb.)	0.025	-0.001	(0.000)	8,842	0.024	0.000	(0.001)	4,598	0.025	-0.001*	(0.001)	4,244
Population aged 15-19 y.o. in formal education 2008 (neighb.)	0.698	0.005	(0.005)	8,842	0.701	-0.001	(0.007)	4,598	0.696	0.012*	(0.006)	4,244
Population aged 20-24 y.o. enrolled in univ. 2008 (neighb.)	0.303	-0.003	(0.007)	8,842	0.311	-0.011	(0.010)	4,598	0.294	0.005	(0.010)	4,244
Population aged 20-24 y.o. in post-sec. educ. 2008 (neighb.)	0.387	-0.002	(0.008)	8,842	0.395	-0.012	(0.011)	4,598	0.378	0.009	(0.011)	4,244
Labor informality rate 2008 (neighb.)	0.327	-0.006	(0.005)	8,842	0.325	0.003	(0.007)	4,598	0.329	-0.015**	(0.007)	4,244
Unemployment rate 2008 (city or neighb.)	0.073	-0.001	(0.001)	8,842	0.073	-0.000	(0.001)	4,598	0.073	-0.001	(0.001)	4,244
Fomal employment Jan 2012	0.014	-0.004	(0.004)	8,882	0.011	0.002	(0.005)	4,612	0.017	-0.010**	(0.004)	4,270
Fomal employment Feb 2012	0.004	-0.002	(0.002)	8,882	0.003	0.003	(0.003)	4,612	0.005	-0.006***	(0.001)	4,270
Fomal employment Mar 2012	0.007	0.003	(0.003)	8,882	0.006	0.004	(0.005)	4,612	0.009	0.002	(0.005)	4,270
Fomal employment Apr 2012	0.011	-0.001	(0.004)	8,882	0.011	0.001	(0.005)	4,612	0.012	-0.003	(0.005)	4,270
Fomal employment May 2012	0.023	-0.005	(0.005)	8,882	0.019	-0.010*	(0.005)	4,612	0.026	-0.000	(0.008)	4,270
Wage in formal job Jan 2012	20.437	11.072	(12.599)	8,882	20.692	15.831	(18.758)	4,612	20.159	6.166	(16.802)	4,270
Wage in formal job Feb 2012	4.832	1.536	(5.861)	8,882	4.740	9.187	(11.508)	4,612	4.933	-6.218***	(1.823)	4,270
Wage in formal job Mar 2012	8.304	8.061	(7.137)	8,882	7.340	15.272	(12.687)	4,612	9.360	0.754	(6.444)	4,270
Wage in formal job Apr 2012	14.580	9.400	(10.013)	8,882	14.647	11.957	(13.638)	4,612	14.507	6.801	(14.672)	4,270
Wage in formal job May 2012	46.952	-12.027	(12.098)	8,882	40.884	-11.302	(16.163)	4,612	53.596	-13.412	(18.041)	4,270

Note: Column (2), (6) and (10) report the difference in each variable between eligible individuals in the city of Cordoba and surveyed individuals. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Effect of the program on the probability of being employed in the formal sector

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Month	Mean control	Difference	se	N	Mean control	Difference	se	N	Mean control	Difference	se	N
June 2013	0.118	0.026***	(0.005)	21,939	0.091	0.031***	(0.006)	11,902	0.150	0.018**	(0.008)	10,037
July 2013	0.126	0.037***	(0.005)	21,939	0.097	0.039***	(0.007)	11,902	0.162	0.034***	(0.008)	10,037
Aug 2013	0.133	0.046***	(0.005)	21,939	0.102	0.051***	(0.007)	11,902	0.171	0.039***	(0.008)	10,037
Sept 2013	0.137	0.054***	(0.005)	21,939	0.105	0.055***	(0.007)	11,902	0.177	0.050***	(0.009)	10,037
Oct 2013	0.144	0.060***	(0.006)	21,939	0.108	0.059***	(0.007)	11,902	0.187	0.060***	(0.009)	10,037
Nov 2013	0.153	0.059***	(0.006)	21,939	0.115	0.063***	(0.007)	11,902	0.199	0.052***	(0.009)	10,037
Dec 2013	0.156	0.063***	(0.006)	21,939	0.118	0.067***	(0.007)	11,902	0.203	0.057***	(0.009)	10,037
Jan 2014	0.163	0.060***	(0.006)	21,939	0.122	0.065***	(0.007)	11,902	0.213	0.052***	(0.009)	10,037
Feb 2014	0.161	0.057***	(0.006)	21,939	0.120	0.062***	(0.007)	11,902	0.212	0.049***	(0.009)	10,037
Mar 2014	0.162	0.057***	(0.006)	21,939	0.121	0.061***	(0.007)	11,902	0.212	0.050***	(0.009)	10,037
Apr 2014	0.161	0.060***	(0.006)	21,939	0.120	0.065***	(0.007)	11,902	0.210	0.052***	(0.009)	10,037
May 2015	0.166	0.060***	(0.006)	21,939	0.126	0.066***	(0.007)	11,902	0.214	0.050***	(0.009)	10,037
Jun 2014	0.174	0.056***	(0.006)	21,939	0.134	0.063***	(0.008)	11,902	0.223	0.046***	(0.009)	10,037

Note: Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Effect of the program on the probability of being employed in the formal sector (without control individuals participating in subsequent editions of PPP)

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Month	Mean control	Difference	se	N	Mean control	Difference	se	N	Mean control	Difference	se	N
June 2013	0.126	0.018***	(0.005)	20,755	0.097	0.025***	(0.006)	11,229	0.160	0.008	(0.008)	9,526
July 2013	0.134	0.029***	(0.005)	20,755	0.103	0.032***	(0.007)	11,229	0.172	0.023***	(0.008)	9,526
Aug 2013	0.142	0.037***	(0.005)	20,755	0.108	0.044***	(0.007)	11,229	0.182	0.028***	(0.009)	9,526
Sept 2013	0.147	0.044***	(0.006)	20,755	0.111	0.048***	(0.007)	11,229	0.189	0.038***	(0.009)	9,526
Oct 2013	0.153	0.051***	(0.006)	20,755	0.115	0.052***	(0.007)	11,229	0.200	0.047***	(0.009)	9,526
Nov 2013	0.162	0.050***	(0.006)	20,755	0.121	0.056***	(0.007)	11,229	0.212	0.040***	(0.009)	9,526
Dec 2013	0.165	0.054***	(0.006)	20,755	0.124	0.060***	(0.007)	11,229	0.215	0.045***	(0.009)	9,526
Jan 2014	0.173	0.051***	(0.006)	20,755	0.129	0.058***	(0.008)	11,229	0.225	0.041***	(0.009)	9,526
Feb 2014	0.170	0.049***	(0.006)	20,755	0.125	0.056***	(0.007)	11,229	0.223	0.039***	(0.009)	9,526
Mar 2014	0.170	0.048***	(0.006)	20,755	0.127	0.054***	(0.007)	11,229	0.223	0.039***	(0.009)	9,526
Apr 2014	0.169	0.052***	(0.006)	20,755	0.126	0.059***	(0.008)	11,229	0.221	0.042***	(0.009)	9,526
May 2015	0.174	0.051***	(0.006)	20,755	0.132	0.060***	(0.008)	11,229	0.225	0.039***	(0.009)	9,526
Jun 2014	0.178	0.052***	(0.006)	20,755	0.135	0.062***	(0.008)	11,229	0.230	0.039***	(0.009)	9,526

Note: Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effect of the program on formal wage (monthly, nominal Argentinean Pesos) and firm size, conditional on being employed

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Month	Mean control	Difference	se	N	Mean control	Difference	se	N	Mean control	Difference	se	N
June 2013	5947.993	-194.002	(140.777)	2,804	5732.858	-94.768	(216.069)	1,233	6107.498	-261.253	(183.983)	1,571
July 2013	4681.037	194.744**	(99.078)	3,069	4488.578	315.140**	(160.004)	1,327	4820.875	105.965	(123.477)	1,742
Aug 2013	4706.061	202.982**	(98.395)	3,283	4497.007	334.527**	(148.701)	1,436	4857.662	113.002	(130.716)	1,847
Sept 2013	4809.740	144.211	(94.949)	3,433	4598.687	310.784**	(152.959)	1,481	4961.315	12.946	(117.378)	1,952
Oct 2013	4993.613	155.987	(98.596)	3,624	4743.081	303.104*	(159.597)	1,544	5170.101	48.857	(121.601)	2,080
Nov 2013	5224.096	61.257	(100.922)	3,820	4937.129	172.457	(162.111)	1,637	5425.060	2.976	(125.677)	2,183
Dec 2013	7354.084	159.684	(145.633)	3,924	6896.224	521.852**	(227.083)	1,688	7677.118	-89.720	(186.088)	2,236
Jan 2014	5506.776	107.718	(107.416)	4,087	5126.274	361.242**	(164.000)	1,752	5772.802	-62.265	(139.111)	2,335
Feb 2014	5590.203	110.962	(110.116)	4,019	5375.142	237.794	(172.455)	1,705	5737.616	21.595	(141.761)	2,314
Mar 2014	5675.229	198.479*	(108.997)	4,013	5394.922	259.582	(159.866)	1,704	5868.826	183.347	(147.175)	2,309
Apr 2014	6114.643	345.950***	(114.010)	4,006	5850.732	456.101**	(184.990)	1,712	6298.052	289.459**	(140.102)	2,294
May 2015	6245.473	239.151**	(121.184)	4,104	5979.963	257.909	(191.764)	1,783	6435.418	252.289*	(151.498)	2,321
Jun 2014	8812.682	373.641**	(162.909)	4,261	8454.727	404.264	(253.033)	1,866	9073.564	390.165*	(208.134)	2,395

Note: This table considers only the subsample of individuals who are working in a formal job in the post treatment period. Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. (^{a}) Only for individuals working in firms with less than 5000 employees (to avoid outliers).

Table 7: Effect of the program on formal wage (monthly, nominal Argentinean Pesos). Bounds.

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Month	Mean control	Lower bound	Lower bound (Atanassio)	Upper bound	Mean control	Lower bound	Lower bound (Atanassio)	Upper bound	Mean control	Lower bound	Lower bound (Atanassio)	Upper bound
June 2013	5947.993	-1642.816	-194.002	1254.812	5732.858	-2093.753	-94.768	1904.218	6107.498	-1125.729	-261.253	603.223
July 2013	4681.036	-1085.207	194.744**	1474.696	4488.578	-1288.022	315.140**	1918.301	4820.875	-895.497	105.965	1107.427
Aug 2013	4706.061	-1280.602	202.982**	1686.566	4497.007	-1620.689	334.527**	2289.743	4857.662	-1013.014	113.002	1239.018
Sept 2013	4809.740	-1500.057	144.211	1788.479	4598.687	-1735.963	310.784**	2357.530	4961.315	-1348.717	12.946	1374.608
Oct 2013	4993.613	-1648.028	155.987	1960.001	4743.081	-1887.404	303.104*	2493.612	5170.101	-1502.066	48.857	1599.781
Nov 2013	5224.096	-1742.934	61.257	1865.448	4937.129	-2059.846	172.457	2404.761	5425.060	-1353.451	2.976	1359.403
Dec 2013	7354.084	-2611.391	159.684	2930.759	6896.224	-2948.193	521.852**	3991.897	7677.117	-2257.847	-89.720	2078.407
Jan 2014	5506.775	-1767.150	107.718	1982.587	5126.274	-1932.921	361.242**	2655.406	5772.802	-1411.045	-62.265	1286.515
Feb 2014	5590.203	-1636.282	110.962	1858.206	5375.142	-2083.145	237.794	2558.732	5737.616	-1304.090	21.595	1347.280
March 2014	5675.229	-1724.350	198.479*	2121.308	5394.922	-2186.894	259.582	2706.057	5868.826	-1256.038	183.347	1622.732
Apr 2014	6114.643	-1710.779	345.950***	2402.679	5850.732	-2069.345	456.101**	2981.546	6298.052	-1190.198	289.459**	1769.117
May 2014	6245.473	-1861.892	239.151**	2340.193	5979.963	-2448.474	257.909	2964.292	6435.418	-1292.296	252.289*	1796.874
June 2014	8812.682	-2458.265	373.641**	3205.547	8454.727	-3241.942	404.264	4050.471	9073.564	-1632.967	390.165*	2413.296

Notes: This table considers only the subsample of individuals who are working in a formal job in the post treatment period. Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups (wage effect), controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Calculation of bound follows Attanasio et al. (2011). See Appendix 2 for details on the bounds calculation.

Table 8: Follow-up survey: Basic labor outcomes (12 months after the program ended)

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outcome variable	Mean control	Diff.	se	N	Mean control	Diff.	se	N	Mean control	Diff.	se	N
Labor force participation	0,892	0.012	(0.019)	1,019	0,901	0.013	(0.026)	513	0,883	0.007	(0.028)	506
Unemployed (if active)	0,594	-0.060*	(0.032)	916	0,618	-0.036	(0.044)	467	0,572	-0.080*	(0.046)	449
Health insurance	0,175	0.050**	(0.025)	1,019	0,179	0.028	(0.035)	513	0,171	0.068*	(0.036)	506
Social Security contributions	0,161	0.058**	(0.025)	1,019	0,156	0.042	(0.034)	513	0,167	0.068*	(0.035)	506
Paid vacation days	0,161	0.092***	(0.025)	1,019	0,175	0.074**	(0.036)	513	0,149	0.106***	(0.035)	506
Annual bonus (aguinaldo)	0,173	0.085***	(0.026)	1,019	0,184	0.065*	(0.037)	513	0,162	0.098***	(0.036)	506
Formally registered (May 2014)	0,120	0.074***	(0.023)	1,019	0,113	0.050	(0.030)	513	0,126	0.095***	(0.033)	506

Note: Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Descriptive statistics of city's share of treated individuals

County	Num. of cities	mean	sd	min	max
CALAMUCHITA	17	0.56	0.27	0.00	1.00
CAPITAL	1	0.35	.	0.35	0.35
COLON	20	0.45	0.09	0.33	0.67
GENERAL ROCA	12	0.34	0.16	0.00	0.67
GENERAL SAN MART	15	0.43	0.19	0.14	1.00
JUAREZ CELMAN	13	0.57	0.24	0.30	1.00
MARCOS JUAREZ	19	0.39	0.17	0.00	0.75
PUNILLA	24	0.60	0.27	0.00	1.00
RIO CUARTO	23	0.24	0.15	0.00	0.50
RIO PRIMERO	23	0.39	0.33	0.00	1.00
RIO SEGUNDO	19	0.24	0.09	0.00	0.40
PTE ROQUE SAENZ	8	0.17	0.08	0.10	0.33
SAN JUSTO	33	0.29	0.19	0.00	1.00
SANTA MARIA	22	0.27	0.26	0.00	1.00
TERCERO ARRIBA	15	0.22	0.14	0.00	0.60
UNION	25	0.32	0.17	0.14	1.00
Total	289	0.37	0.24	0.00	1.00

Table 10: Correlation between the share of treated individuals at the city level and municipality characteristics

	(1)	(2)	(3)
Variables	coeff.	sd	N
Population 2008	-2328.354	(2,032.031)	278
Female population 2008	-1118.848	(978.128)	278
Male population 2008	-1209.506	(1,054.084)	278
Population aged 15-19, 2008	-186.432	(170.624)	278
Population aged 20-24, 2008	-164.651	(157.846)	278
UBN rate 2008	0.027	(0.023)	281
Unemployment rate 2008	-0.002	(0.008)	281
Unemployment rate,population aged 15-24 1524	-0.003	(0.019)	280
Labor informality rate 2008	-0.000	(0.023)	281
Health insurance coverage 2008, population aged 15-24 (%)	-0.049	(0.036)	281
Literacy rate 2008	-0.006	(0.007)	281
Educ. enrollment rate, overall population	0.009	(0.011)	281
Educ. enrollment rate, population aged 15-24	0.036	(0.035)	281
High-school graduates, population aged 20+	-0.006	(0.032)	281
Labor force participation 1524	-0.054**	(0.026)	280

Note: Column 1 report the coefficient of an OLS regression of each city characteristic on the share of treated individuals at the city level, controlling department fixed effects. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Displacement effects on the control group

	(1)	(2)	(3)	(4)	(5)
Month	Moderate share	se	High share	se	N
A. During- PPP					
June 2012	0.005	(0.005)	0.007	(0.009)	14,529
July 2012	-0.001	(0.006)	-0.010	(0.009)	14,529
Aug 2012	-0.002	(0.007)	-0.017	(0.011)	14,529
Sept 2012	-0.000	(0.007)	-0.016	(0.012)	14,529
Oct 2012	-0.001	(0.008)	-0.004	(0.013)	14,529
Nov 2012	0.000	(0.008)	0.001	(0.014)	14,529
Dec 2012	0.003	(0.009)	-0.001	(0.014)	14,529
Jan 2013	-0.002	(0.009)	-0.008	(0.016)	14,529
Feb 2013	-0.002	(0.010)	-0.005	(0.015)	14,529
Mar 2013	0.003	(0.010)	-0.007	(0.015)	14,529
Apr 2013	0.002	(0.010)	0.001	(0.016)	14,529
May 2013	0.001	(0.010)	0.009	(0.017)	14,529
B. Post- PPP					
July 2013	0.001	(0.011)	-0.006	(0.018)	14,529
Aug 2013	-0.005	(0.011)	-0.007	(0.018)	14,529
Sept 2013	-0.008	(0.011)	-0.007	(0.018)	14,529
Oct 2013	-0.003	(0.011)	-0.009	(0.019)	14,529
Nov 2013	-0.005	(0.012)	-0.013	(0.019)	14,529
Dec 2013	-0.002	(0.012)	0.007	(0.019)	14,529
Jan 2014	0.006	(0.012)	0.018	(0.021)	14,529
Feb 2014	0.011	(0.012)	0.021	(0.020)	14,529
Mar 2014	0.007	(0.012)	0.002	(0.020)	14,529
Apr 2014	0.006	(0.012)	0.000	(0.020)	14,529
May 2015	0.012	(0.012)	0.018	(0.020)	14,529
Jun 2014	0.007	(0.013)	0.001	(0.020)	14,529

Note: Columns 1 and 3 report the OLS estimates of coefficient δ^m and δ^h, respectively, of equation 3. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 12: Mechanisms: Skills development (Survey sample)

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Mean control	Diff.	se	N	Mean control	Diff.	se	N	Mean control	Diff.	se	N
A. Characteristics of job tasks (if employed)												
Operates computer at work	0.558	0.088	(0.055)	338	0.700	0.028	(0.077)	159	0.435	0.142*	(0.077)	179
Operates mechanical machinery at work	0.372	-0.087	(0.053)	338	0.183	0.006	(0.065)	159	0.536	-0.175**	(0.077)	179
Operates transport vehicles	0.186	-0.017	(0.043)	338	0.050	0.004	(0.033)	159	0.304	-0.030	(0.071)	179
Use mostly operative knowledge at work	0.775	0.027	(0.046)	338	0.717	0.089	(0.074)	159	0.826	-0.007	(0.059)	179
Use mostly high-level (univ.) knowledge at work	0.295	-0.010	(0.051)	338	0.333	0.040	(0.080)	159	0.261	-0.052	(0.066)	179
Use mostly technical knowledge at work	0.364	-0.071	(0.053)	338	0.383	-0.051	(0.079)	159	0.348	-0.099	(0.072)	179
B. “Cognitive” skills												
Cognitive (conceptual verbalization)	7.395	-0.050	(0.151)	1.017	7.632	-0.197	(0.187)	512	7.167	0.057	(0.237)	505
Spelling	9.628	0.075	(0.130)	1.017	9.778	0.157	(0.167)	512	9.484	-0.049	(0.197)	505
Seconds to complete survey	2514.191	-137.182	(108.477)	1.019	2498.415	-144.889	(158.916)	512	2529.257	-114.854	(148.321)	507
C. “Socio-emotional” skills												
Stress management (total)	29.366	0.271	(0.241)	1.019	29.274	0.353	(0.353)	512	29.455	0.151	(0.329)	507
Negative strategies for stress management	12.638	-0.000	(0.132)	1.019	12.585	0.045	(0.187)	512	12.689	-0.068	(0.186)	507
Resolute strategies for stress management	8.622	0.114	(0.120)	1.019	8.476	0.144	(0.173)	512	8.761	0.085	(0.166)	507
Social strategies for stress management	8.106	0.157	(0.131)	1.019	8.212	0.165	(0.194)	512	8.005	0.134	(0.177)	507
Personal projects	8.376	-0.066	(0.069)	1.019	8.491	-0.046	(0.093)	512	8.266	-0.097	(0.101)	507
Self-control	8.157	0.070	(0.070)	1.019	8.217	0.082	(0.095)	512	8.099	0.047	(0.101)	507
Self-efficacy	8.949	0.064	(0.100)	1.019	8.877	-0.027	(0.143)	512	9.018	0.153	(0.139)	507
Time planning	17.392	0.037	(0.184)	1.019	17.759	0.169	(0.262)	512	17.041	-0.132	(0.257)	507

Note: Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 13: Mechanisms: Other barriers to labor market entry (Survey sample)

	All				Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Month	Mean control	Difference	se	N	Mean control	Difference	se	N	Mean control	Difference	se	N
A. Job search strategies (if active)												
Ask friends same neighborhood	0.806	-0.011	(0.027)	916	0.800	-0.009	(0.039)	466	0.811	-0.012	(0.039)	450
Ask friends other neighborhood	0.736	-0.001	(0.030)	916	0.732	0.009	(0.042)	466	0.740	-0.010	(0.043)	450
Use referrals from former boss	0.254	0.022	(0.029)	916	0.211	0.036	(0.040)	466	0.296	0.010	(0.044)	450
Put up flyers	0.073	-0.001	(0.017)	916	0.079	-0.005	(0.025)	466	0.066	0.005	(0.024)	450
Newspaper job ads	0.736	-0.040	(0.030)	916	0.763	-0.034	(0.041)	466	0.709	-0.047	(0.045)	450
Internet job ads	0.715	0.027	(0.030)	916	0.747	0.033	(0.040)	466	0.684	0.014	(0.044)	450
Employment agencies	0.692	0.001	(0.031)	916	0.689	0.002	(0.044)	466	0.694	0.002	(0.045)	450
Send CV to firms	0.826	-0.003	(0.026)	916	0.816	0.035	(0.036)	466	0.837	-0.044	(0.037)	450
B. Time use (leisure time)												
Share (%) of leisure time spent with old-time friends	25.474	-1.153	(0.960)	993	22.854	-0.548	(1.293)	498	27.972	-1.575	(1.396)	495
Share (%) of leisure time spent with new friends	15.573	-0.033	(0.867)	974	15.946	-0.203	(1.226)	491	15.211	0.173	(1.233)	483
Share (%) of leisure time spent with family	43.702	1.460	(1.278)	1,014	46.867	0.714	(1.900)	510	40.667	1.943	(1.672)	504
Share (%) of leisure time spent alone	17.798	-0.945	(0.931)	980	16.240	0.042	(1.219)	495	19.303	-1.823	(1.388)	485
C. Expectations												
Country situation will improve (1-5 scale)	2.517	0.073	(0.071)	1,019	2.645	-0.181*	(0.097)	512	2.396	0.328***	(0.103)	507
Youth situation will improve (1-5 scale)	2.945	0.062	(0.066)	1,019	2.957	0.012	(0.089)	512	2.932	0.109	(0.099)	507
Plan to be own-account worker by the age of 30	0.159	0.034	(0.024)	1,019	0.156	0.038	(0.034)	512	0.162	0.029	(0.034)	507
Plan to work for a big firm by the age of 30	0.316	-0.056*	(0.029)	1,019	0.303	-0.037	(0.041)	512	0.329	-0.072*	(0.041)	507
Plan to work for a SME by the age of 30	0.032	0.008	(0.012)	1,019	0.047	-0.011	(0.018)	512	0.018	0.026*	(0.015)	507
Plan to work in the public sector by the age of 30	0.134	0.013	(0.022)	1,019	0.180	-0.008	(0.034)	512	0.090	0.031	(0.028)	507
Plan to be an employer by the age of 30	0.358	0.002	(0.031)	1,019	0.313	0.019	(0.042)	512	0.401	-0.013	(0.044)	507
D. Perceptions about market returns to education												
Perceived earnings 30 y.o. high school completed (AR$, monthly)	5690.705	-160.050	(133.772)	1,016	5381.818	-158.766	(179.035)	510	5981.505	-141.394	(194.306)	506
Perceived earnings 30 y.o. college degree (AR$, monthly)	8802.738	-33.153	(255.122)	1,018	8374.882	-552.565	(343.742)	512	9211.235	512.478	(368.591)	506
Perceived earnings 50 y.o. high school completed (AR$, monthly)	6409.250	-277.733	(186.414)	1,018	5764.286	24.894	(234.476)	511	7019.351	-558.096**	(280.247)	507
Perceived earnings 50 y.o. high school dropout (AR$, monthly)	4349.374	-84.928	(124.494)	1,017	4123.014	19.343	(162.384)	510	4562.477	-182.840	(186.482)	507
Perceived earnings 50 y.o. college degree (AR$, monthly)	11099.290	-432.061	(418.819)	1,017	9951.185	-489.678	(533.932)	512	12200.440	-336.157	(631.078)	505
Earnings growth between 30-50 y.o. college degree	0.350	1.703	(1.746)	1,015	0.234	0.108	(0.079)	511	0.461	3.111	(3.285)	504
Earnings growth between 30-50 y.o. high school completed	0.168	-0.008	(0.036)	1,015	0.112	0.058	(0.054)	509	0.221	-0.073	(0.046)	506

Note: Columns 2, 6 and 10 report the difference in each variable between the treatment and control groups, controlling for firm and county quotas and for the number of applications fulfilled by each individual. Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 4: Distribution of the share of treated individuals across cities

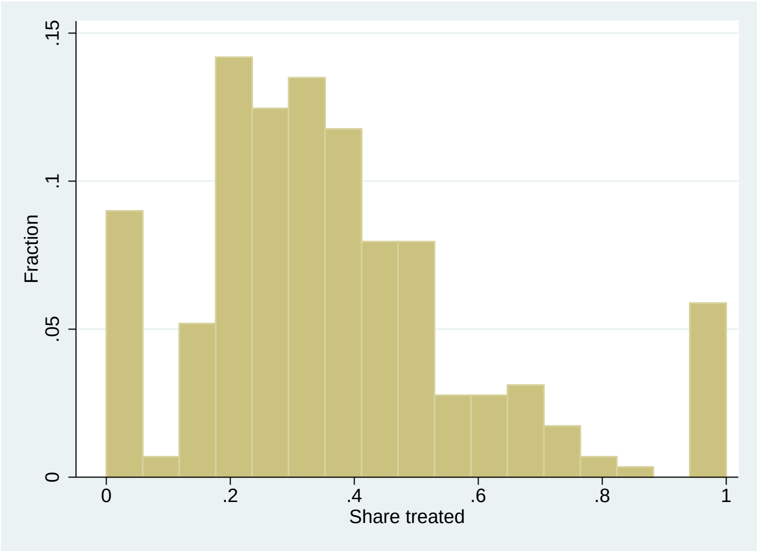


Figure 5: Distribution of the share of treated individuals across cities, by county

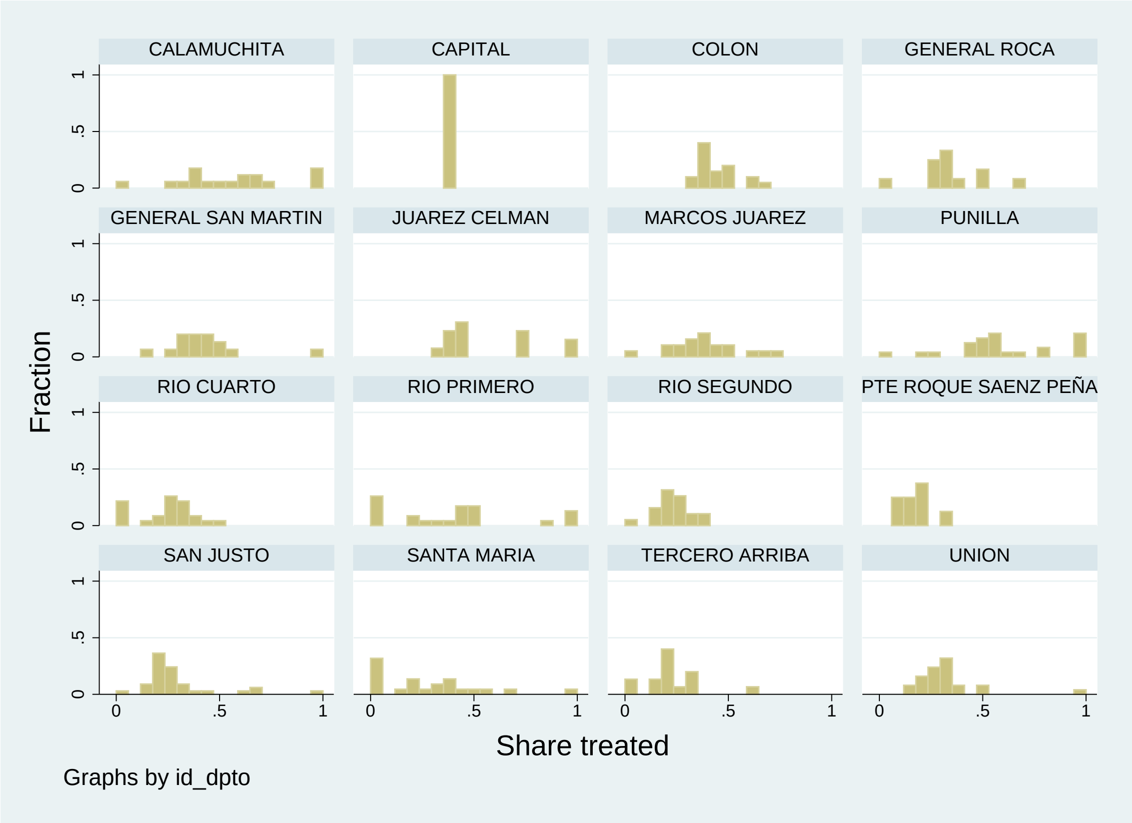
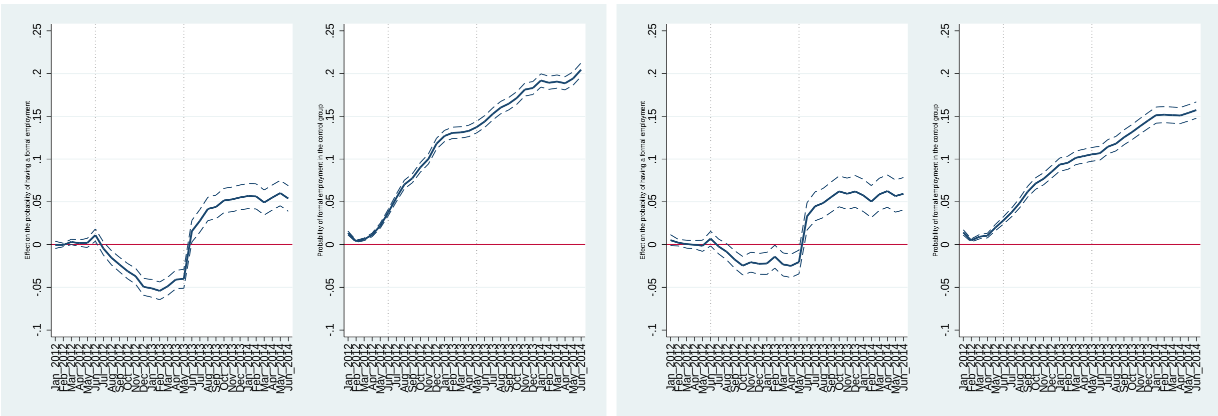


Figure 6: Effect of the program on the probability of formal employment, by educational attainment

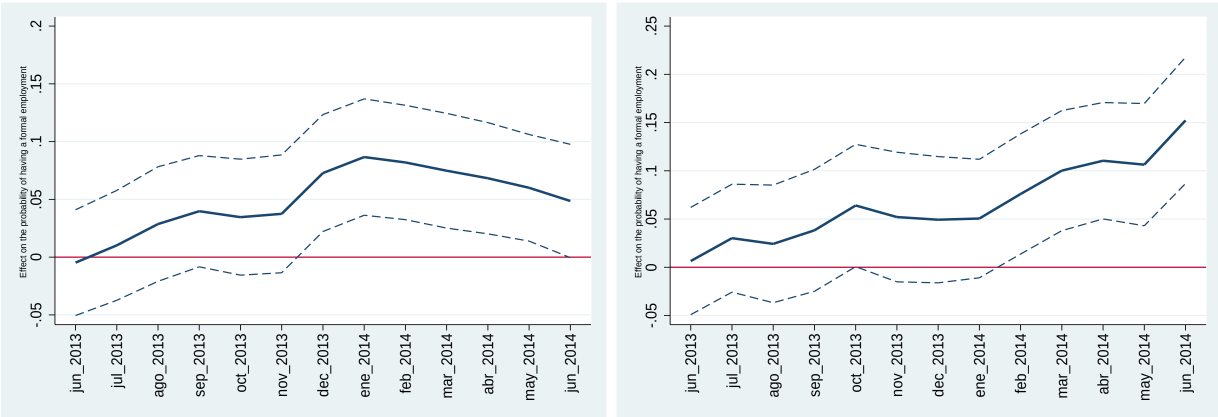


(a) High School Complete

(b) High School Incomplete

Source: authors' calculations. In each panel, the left-hand side graphs report the impact of the PPP and the right-hand side graphs report the average outcome for the control group..

Figure 7: Effect of the program on the probability of formal employment, by cognitive ability levels



(a) Low type (cognitive test) (b) High type (cognitive test)

9 Appendix 1: Description of the random assignment process

The algorithm used in the lottery that assigns the benefits considers two types of quotas: (1) a quota that assures that the share of beneficiaries in each county equals the share of total population residing in each county of the Province of Córdoba; and (2) a quota restricting the number of apprentices that a firm can have in a given year. The first quota is defined according to official data on population, coming from the *Censo Nacional de Población y Vivienda* 2010 (CENSO 2010). The second quota is defined following the cutoffs over firm sizes, which are displayed in Table 14.

These two quotas affect the probability with which any given application (pair youngster-firm) is included in the treatment group. Therefore, all regressions need to control for these two sources of variation in the probability of treatment. We do so by including as covariates in the estimations two variables reflecting each one of these quotas. We construct these variables in the following way. First, we construct a variable that compares the size of the quota per county with the number of applications received in that county. This variable takes the form

Number of registered employees by the time of application	Maximum number of PPP apprentices under training modality
0	None
1 to 5	1 apprentice
6 a 10	Up to 2 apprentices
11 to 25	Up to 3 apprentices
26 to 50	Up to 20% of total employees
More than 50	Up to 10% of total employees

Source: `http://empleo.cba.gov.ar/wp-content/uploads/2015/02/INSTRUCTIVO-PRIMER-PASO-2015.doc.`

Table 14: Maximum number of PPP apprentices according to firm size (measured as the number of formally registered employees).

$$\text{Quota for county } j = \frac{\text{Maximum number of beneficiaries in county } j}{\text{Number of applications in county } j}. \tag{4}$$

Second, we construct a variable that compares the maximum number of PPP apprentices that each firm can receive (following Table 14) with the total number of applications received by each firm, as follows

$$\text{Quota for firm } f = \frac{\text{Maximum number of beneficiaries in firm } f}{\text{Number of applications in firm } f}. \tag{5}$$

Moreover, since each individual i can fill more than one application, the probability that an individual i result treated changes with the number of applications filled. Therefore, in all regressions we also control for the number of applications by individual i. Notice that variables in equations (4) and (5) can take non-negative values.

As a result of this random assigment process, all regressions presented in the paper are of the form

$$y_{i,j,f,t} = \alpha + \tau_t D_i + \delta_1 \text{quota-county}_j + \delta_2 \text{quota-firm}_f + \delta_3 \text{num-applic}_i + u_{i,j,f,t}.$$

10 Appendix 2: Bounds calculation for the causal effect of the program on wages

The effect of the program on wages in panel A of table 6 are estimates of the following expression.

$$\text{Wage effect} = \mathbb{E}[wage|L=1, D=1] - \mathbb{E}[wage|L=1, D=0] \tag{6}$$

However this is not a causal effect. In this appendix we show (following Attanasio et al. (2011)) how we bound the causal effect of interest (productivity effect).

Le $L(j)$, for $j \in \{0, 1\}$, be the formal working status given treatment assignment. The population can be divided in four groups:

- Always takers: $L(1) = 1, L(0) = 1$
- Never takers: $L(1) = 0, L(0) = 0$
- Compliers: $L(1) = 1, L(0) = 0$
- Defiers: $L(1) = 0, L(0) = 1$

Assuming monotonicity, we rule out the existance of defiers. Additionanlly, given treatment randomization, the size of each group is independent of treatment status. Given these two assumtions, we can write the two terms of the wage effect as

$$\begin{aligned}\mathbb{E}[wage|L=1, D=1] = \quad & \mathbb{E}[wage|complier, D=1] \times \frac{Pr[complier]}{Pr[complier] + Pr[Always\ \ taker]} \\ & +\mathbb{E}[wage|Always\ \ taker, D=1] \times \frac{Pr[Always\ \ taker]}{Pr[complier] + Pr[Always\ \ taker]}\end{aligned}$$

$$\mathbb{E}[wage|L=1, D=0] = \quad \mathbb{E}[wage|Always\ \ taker, D=0]$$

Lets call

$$K = \frac{Pr[complier]}{Pr[complier] + Pr[Always\ \ taker]}$$

Then

$$\begin{aligned}
\text{Wage effect} &= \mathbb{E}[wage|L=1, D=1] - \mathbb{E}[wage|L=1, D=1] \\
& \mathbb{E}[wage|complier, D=1] \times K + \mathbb{E}[wage|Always\ \ taker, D=1](1-K) \\
& -\mathbb{E}[wage|Always\ \ taker, D=1]
\end{aligned}$$

Adding and subtracting the term $\mathbb{E}[wage|complier, D=0] \times K$ and rearranging we get

$$\begin{aligned}
\text{Wage effect} = \quad & \left\{ \underbrace{E[wage|complier, D=1] - E[wage|complier, D=0]}_{\text{Causal effect on compliers}} \right\} \times K \\
& + \left\{ \underbrace{E[wage|Always\ \ taker, D=1] - E[wage|Always\ \ taker, D=0]}_{\text{Causal effect on Always takers}} \right\} \times (1-K) \\
& + \left\{ \underbrace{E[wage|Complier, D=0] - E[wage|Always\ \ taker, D=0]}_{\text{Baseline wage difference between compliers and always takers}} \right\} \times K
\end{aligned}$$

Then, we can estimate the causal effect of interest (productivity effect), which is a weighted average of the causal effect of the program on the wages of compliers and always takers, as

$$\text{Causal effect} = \text{wage effect} - \{E[wage|Complier, D=0] - E[wage|Always\ \ taker, D=0]\} \times K$$

Note that the wage effect and K can be estimated from the data.

$$
\begin{aligned}
Pr[Always\ \ taker] &= Pr[L=1|D=0] \qquad (7)\\
Pr[complier] + Pr[Always\ \ taker] &= Pr[L=1|D=1]
\end{aligned}
$$

$\Rightarrow Pr[complier] = Pr[L=1|D=1] - Pr[L=1|D=0]$

Hence,

$$
\begin{aligned}
K &= \frac{Pr[complier]}{Pr[complier] + Pr[Always\ \ taker]}\\
&= \frac{Pr[L=1|D=1] - Pr[L=1|D=0]}{Pr[L=1|D=1]}
\end{aligned}
$$

However, the baseline wage difference between compliers and always takers is not observed. This difference is the selection effect partially explaining the wage effect. This term can be bounded by using the mean wage in the tenth and ninetieth percentile for those in the control group who have formal employment, assuming that it could be $\pm\{\mathbb{E}[wage(p_{0.90,it})] - \mathbb{E}[wage(p_{0.10,it})]\}$. However, the lower bound estimate with this proxy would be extremely conservative. One can assume that the average wage of the always takers without treatment is at least as large as the average wage of the compliers, resulting in the following bounds

$$
\begin{aligned}
\text{Lower bound Causal effect} &= \text{wage effect}, \qquad (8)\\
\text{Upper bound Causal effect} &= \text{wage effect} - \{\mathbb{E}[wage(p_{0.90,it})] - \mathbb{E}[wage(p_{0.10,it})]\} \times K. \qquad (9)
\end{aligned}
$$