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# Long Run Effects of Youth Training Programs: Experimental Evidence from Argentina

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February 2016

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
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# Long Run Effects of Youth Training Programs: Experimental Evidence from Argentina

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# ABSTRACT

# Long Run Effects of Youth Training Programs: Experimental Evidence from Argentina*

We study the effect of a job training program for low income youth in Cordoba, Argentina. The program included life-skills and vocational training, as well as internships with private sector employers. Participants were allocated by means of a public lottery. We rely on administrative data on formal employment, employment spells and earnings, to establish the effects of the program in the short term (18 months), but also – exceptionally for programs of this type in Latin America and in developing countries in general – in the medium term (33 months) and in the long term (48 months). The results indicate sizable gains of about 8 percentage points in formal employment in the short term (about 32% higher than the control group), although these effects dissipate in the medium and in the long term. Contrary to previous results for similar programs in the region, the effects are substantially larger for men, although they also seem to fade in the long run. Program participants also exhibit earnings about 40% higher than those in the control group, and an analysis of bounds indicates that these gains result from both higher employment levels and higher wages. The detailed administrative records also allow us to shed some light on the possible mechanisms underlying these effects. A dynamic analysis of employment transitions indicates that the program operated through an increase in the persistence of employment rather than from more frequent entries into employment. The earnings effect and the higher persistence of employment suggest that the program was successful in increasing the human capital of participants, although the transient nature of these results may also reflect better matches from a program-induced increase in informal contacts or formal intermediation.

JEL Classification: J08, J24, J68, O15

Keywords: youth labor training programs, youth unemployment, field experiment

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# 1 Introduction

Youth unemployment is a pervasive phenomenon in Latin American. Unemployment among youth is three times greater than for adults, labor informality is the norm (Gasparini et al., 2011), and, with little work experience, young people find insertion into the labor market difficult (e.g. Pallais, 2014). Governments and aid agencies have explored training programs as a potential solution to this problem in developing countries, following the extensive experience of active labor market policies carried out in more advanced economies. Policy interventions have centered on at-risk youth, including low income youth who have not completed their education, are poor or have experienced poverty, and are either unemployed or working under precarious conditions (see Vezza, 2014, for an overview of these initiatives in Latin America). Despite the ubiquity of these programs and a longstanding literature on their evaluation, there is relatively limited evidence on their long term effects, and on the mechanisms through which they operate in developing countries. This is the motivation of our study of *entra21*, a job training program for low income youth in Cordoba, Argentina. We present evidence on the programs' effect on employment and earnings. Moreover, the use of detailed monthly administrative data on employment and earnings collected over a relatively long period of time allows us to disentangle the plausible mechanisms through which the program operates. For instance, whether the positive employment effects of the program can be attributed to labor market intermediation or if they reflect real gains in participants' human capital.

In a meta-study of active labor market programs, Card et al. (2010) indicate that training programs in Europe and the United States have, at best, a moderate impact, and that they are generally most effective for women and for older workers. Moreover, firm-based training often exhibits better results than classroom training, and programs with work experience in the private sector tend to be more effective than public sector-based programs. In terms of methodology, there is a substantial literature on the evaluation of training programs by means of randomized controlled trials, covering initiatives such as the *National Supported Work* (NSW), *Job Corps* and the *Job Training Partnership Act* (JTPA) in the United States. González-Velosa et al. (2012) review the available evidence for Latin America, and they highlight the fact that most programs in the region are not evaluated, and when they are, the resulting studies often consist of quasi-experimental evaluations. However, there is a growing literature that has provided experimental evidence regarding the effectiveness of these programs in Latin America and other developing countries.

Attanasio et al. (2011) present the results form an experimental evaluation of Colombia's youth training program, *Jóvenes en Acción*, implemented in 2005. This program targeted unemployed youth (aged between 18 and 30) from poor households. The program consisted of six months of vocational training. The evidence suggests that the program had positive effects on women's wages (an increase of 19.8%). Moreover, women were also more likely to be employed,

especially in formal employment, following participation. For men, program participation had little effect on employment levels, and quality of employment was not affected either. Card et al. (2011) and Ibarrarán et al. (2014) present the evaluation of several cohorts of *Programa Juventud y Empleo*, a youth training program in the Dominican Republic, by means of randomized controlled trials. This program targeted young people who had not completed high school and fell between the ages of 18 and 29. It aimed to increase the employability of vulnerable youth through technical/vocational courses and life-skills training. Results for the 2004 cohort (Card et al., 2011) indicate no significant impact on employment and a modest impact on wages conditional on having a job. Evidence obtained from the 2008 *Programa Juventud y Empleo* cohort indicates that the program had small or null effects on overall employment, with small impacts on formal jobs and salaries for those employed (Ibarrarán et al., 2014). Ibarrarán et al. (2015) present a 6 year follow-up of the same cohort based on survey data, and while they still fail to find any effects on average employment, they report significant impacts on the probability of holding a formal job in the longer term, which seem to be sustained and growing over time.

An original feature of our work is the use of administrative data for the evaluation of longer term impacts of a job training program in a developing country. These features were present in studies of programs in developed countries. For instance, Couch (1992) studied the long term effects of the United States' *National Supported Work* program on earnings, and found that the earnings gains from the program offset its costs among one of its targets groups (low income families), but did not have a long term effect on participant youths' earnings. Schochet et al. (2008), in turn, study the effects of the United States' *Job Corps* based on 4 year survey data and 9 year administrative (tax returns) data follow ups. While the program increased participant's earnings in the few years after the programs, these results were not sustained over the longer horizon of analysis allowed by the administrative data. Since the circulation of our first draft in 2012, there have been several new efforts to study the longer term efforts of training programs in developing countries. Attanasio et al. (2015) present a longer term study of the impact of the *Jóvenes en Acción* program in Colombia, first studied in Attanasio et al. (2011). The program was carried out in 2005, and the authors rely on social security contributions data to study its effects during the period 2008 to 2014. They find that the program had a positive and significant effect on participants, who were more likely to hold a formal job, had higher earnings (11.8%) than those in the control group, and also a higher likelihood to work for a large firm. Kugler et al. (2015) study the effects of the same program in the longer term on educational outcomes. They find that beyond providing vocational training the program also induced higher levels of formal education, with a higher probability of completing secondary schooling and attending tertiary education. Our paper is also related to Hirshleifer et al.'s (2015) experimental study of a vocational training program for the unemployed in Turkey. As in our study, these authors use administrative data to follow beneficiaries for up to three years after the program. They find no

effects of training on employment and only moderate effects on the quality of employment in the short run, but these effects dissipate after three years.

The program on which we focus, *entra21*, differs in some aspects from the programs listed above. It is a regional initiative carried out in different countries. Although governments can participate in its implementation, the program is based in the private sector and it is usually run by non-governmental organizations (usually business associations). In addition to not being a national program, it is usually smaller and more costly than similar government initiatives. The program's main objective is to improve the employment opportunities of at-risk youth by building their technical skills and life-skills through courses and work experience in the private sector. The program included a regionally standardized classroom-based life-skills training module, as well as vocational training and internships coordinated with private sector employers. The edition of the *entra21* program that we study was administered by a public-private non-profit set up by professional and business associations and Cordoba's Municipality (Argentina), in close coordination with private sector employers. Our experimental impact evaluation is based on the first cohort of the program, for which participants were assigned to a treatment or a control group through a public lottery. We rely on detailed monthly administrative social security records for program participants, including formal employment status (the programs' main intended outcome), employment spells and earnings. While most evaluations of this type in Latin America cover only the short term impact of similar programs, these administrative records allow us to compare outcomes from up to eight years before the program, and to establish its effects in the short term (18 months after the program), in the medium term (33 months after the program) and in the long term (48 months).[1]

The results indicate sizable gains of about 8 percentage points in formal employment with respect to the control group in the short term (about 32% higher), although these average effects tend to dissipate in both the medium and in the long term. Program participants also exhibit substantially higher earnings (up to 50% higher than those in the control group), and an analysis of bounds indicates that these gains result both from higher employment levels and from higher wages. Contrary to what has been found for similar programs in the region (e.g., Attanasio et al., 2011; Card et al., 2011; Ibarrarán et al., 2014), the effects of *entra21* on employment and earnings are substantially stronger for men. The effects for men persist in the medium run, but seem to fade in the long run, as in the *Job Corps* program in the United States (Schochet et al., 2008). In addition to these results, we also exploit the detailed panel data structure of the administrative records to shed light on the mechanisms through which the program increases employment and earnings. A dynamic analysis of employment transitions indicates that the program operates

[1]Card et al.'s (2010) meta-review classifies the time frame of programs of this type as "short-term impact" (one year after the completion of the program), "medium-term estimate" (approximately 2 years after completion), and "longer-term (3 year) impacts". We refer to our effects after 18 months as short term, 33 months as medium term, and 48 months as long term. We do not select the more parsimonious 36 months cut-off for the medium term because we only have earnings information for 33 months after the program ended.

mainly by increasing the persistence of formal employment (especially for men) rather than by fostering entries into formal employment. Beneficiaries also exhibit a higher probability of staying with the same employer over time.

Youth labor training programs are usually justified as remedies for when the well-documented (Card, 1999; Carlsson et al., 2015) schooling-skills-employment/earnings nexus fails. They are designed to build human capital and foster the acquisition of cognitive and, more recently, non-cognitive skills, and their main expected outcome is improved employment. However, these programs can also facilitate the contact of beneficiaries with the labor market, providing work experience, implicit or explicit labor market intermediation, contacts and references for future employment. These effects could be present even if the programs fail to build or reinforce beneficiaries' skills and human capital. If training programs increase participants' human capital, beneficiaries become more employable and more productive once employed, which should then be reflected in higher employment levels, in more persistent employment, and in higher labor earnings levels for those employed. Alternatively, these programs may not affect beneficiaries' human capital and productivity, but they may be successful in contacting them with future employers. In this case, we could expect higher employment but not higher earnings from training programs. Our results on employment transitions in *entra21* indicate that the program operated by helping individuals keep their jobs once they were employed, rather than by helping them find jobs. This is compatible with a situation in which the program enhanced the productivity of participants rather than just providing the means to become employed. Moreover, our results on earnings bounds indicate that *entra21* beneficiaries obtained higher wages once they were employed. Taken together, these results suggest that the program was successful in increasing the human capital of participants. However, the employment effects are relatively short lived. The discussion in Section 5 below suggests alternative interpretations, for instance that higher productivity or better matches may also have resulted from a program-induced increase in informal contacts or formal intermediation.

This paper adds to the body of evidence on the impact of youth training programs in developing countries and to the literature on active labor market policies in general. We also illustrate how useful experimental evaluations can be carried out even with small sample sizes by combining them with rich administrative records that offer precise measurements of the outcomes of interest and their dynamics over long periods of time. The use of this type of data is still relatively uncommon in studies on developing countries. Moreover, this type of data also allows us to further probe the mechanisms through which the program operates by studying the dynamics of employment transitions.

This papers is organized as follows: Section 2 describes the program and the random assignment process of potential beneficiaries to the treatment and the control group. Section 3 describes the data sources and the variables in the analysis. It also presents an analysis of baseline char-

acteristics and experimental balance in the sample, and details the estimation strategy for the empirical results. Section 4 presents the empirical results on labor market outcomes. Section 5 discusses these results and provides some concluding remarks.

# 2 Program Description and Experimental Design

## 2.1 Program Description

*entra21* is a regional initiative of job training programs for low income youth in Latin America. The program is financed by the *Multilateral Investment Fund* (MIF, based in Washington, DC) and administered at the regional level by the *International Youth Foundation* (IYF). IYF partners with local organizations, mostly in the private sector (non governmental organizations, professional and business associations), which are the program's implementers. The program specifically targets vulnerable, unemployed youths who have some secondary schooling. It differs in some aspects from the training programs in Latin America mentioned in the introduction. Although governments can participate in its implementation, one of the hallmarks of the program is the private sector's very active involvement in its various project components, especially in terms of influencing the programs' training methods and curricula. In addition to not being a national program, it is usually smaller, and more costly than the typical government program.

*entra21* was implemented in several countries in the region. The local organization in charge of the version of the program examined here is the *Agencia para el Desarrollo Económico de la Ciudad de Córdoba* (ADEC), a public-private non-profit set up by professional and business associations and Cordoba's Municipality.[2] ADEC executed the program, with funding from MIF, IYF, the government of the province of Cordoba and the Municipality of the city of Cordoba. ADEC established partnerships with other governmental and civil society organizations to implement Phase II of the *entra21* program, and with the Municipality's *Secretaría de Desarrollo Social y Empleo* (SDSE) and the province's *Ministerio de Desarrollo Social* (MDS), which provided logistic support for the program.

The program's main objective is to improve the employment opportunities of at-risk youth by building their technical skills and life-skills through courses and work experience in the private sector. Program administrators highlight that *entra21* aims to increase the probability of finding "good quality" jobs in the formal sector, with the objective of reducing joblessness and informality, which are very high among young and vulnerable individuals in the region. The local implementing agency in Cordoba set the program eligibility requirements along *entra21*'s regional guidelines. To be considered eligible, individuals had to be unemployed or underemployed, be-

[2]Cordoba is Argentina's second most populous province, and the metropolitan area of the city of Cordoba, the province's capital, is the country's second largest with about 1.3 million inhabitants, according to the 2010 Census.

tween the ages of 18 and 30, be a high school dropout or graduate, and have a total family income below the poverty line.

*entra21*'s higher cost (compared to other training programs in Latin America) is due to the number of training hours provided, which is greater than that of most training programs in the region. The training includes a regionally standardized classroom-based life-skills training module, as well as vocational training and internships coordinated with private sector employers. In addition to basic information, communications technology, and life skills training, the classroom component offered training in a specific profession, metier, or skill considered to be in demand by actual firms, as well as general labor market related skills. The participants also took part in an internship to acquire on-the-job skills. Courses were offered in the following fields: cooking and catering, sales and administration, and factory workers ("*operarios*"). Coursework was divided in different modules: 100 hours of technical classroom training, 64 hours of life skills training, and 16 extra hours which varied from basic skills to extra classroom technical training according to each type of course. Classroom training took place between mid-November 2010 and February 2011, and was followed by the internship phase (although several participants started internships before completing their coursework and did both concurrently).[3] For the internship, firms were offered a small monthly monetary incentive from the municipal government to cover basic workplace insurance. The program was promoted as offering a free recruiting service for firms with no legal obligation of continuing an employment relationship after the end of the internship. Firms were required to employ the intern for up to 4 months, with a maximum of 20 hours per week, to pay a proportion of the minimum wage according to hours worked, to provide a workplace mentor, and to issue a written certificate of the work experience and training for the intern at the end of the period.

## 2.2 Experimental Design and the Random Assignment Process

We designed an experimental evaluation strategy for this first cohort of the Cordoba edition of *entra21*, in close coordination with the implementing agencies. Since there was only a limited number of places available and, from the onset, the program was expected to be over-subscribed, participants from the first cohort were assigned to a treatment or a control group through a public lottery. ADEC advertised the program in low income neighborhoods, and it was also promoted by the municipal (SDSE) and the provincial (MDS) governments, and these efforts stressed the fact that the program aimed to include a large number of young women. Individuals signing up for the program received a personal visit by a representative of the implementing agency who conducted a baseline survey to gather information on participants and to establish eligibility.

[3] As a benchmark, the *Jóvenes en Acción* program in Colombia included 3 months of classroom training and a 3-month apprenticeship in a job (Kugler et al., 2015), whereas the Dominican Republic's *Juventud y Empleo* had a maximum duration of 350 hours for classroom training, and 2 months internships (Card et al., 2011).

In total, 560 young people applied to the program and 407 were eligible for the first cohort of trainees.

This rather small sample makes it unlikely that the program had general equilibrium effects on employment or earnings, or that employed individuals in the treatment group might have displaced others in the control group. These effects would have contaminated our simple experimental identification. However, these considerations should be taken into account in the eventual scaling-up of the program to a larger population.

A second concern might be the representativeness of the applicants' sample. The EPH national household survey for Cordoba for the second semester of 2010 indicates that 17.9% of those aged between 18 and 30 and with up to incomplete secondary schooling worked formally, and a further 35.2% of that group were informally employed. The first figure is close to the range of formal employment from administrative data for our sample of about 13.5% in this period (see the next section). The pool of applicants was not representative of Cordoba's youth, but it was fairly representative of those from low income backgrounds. It is still possible, however, that the applicants were among the most motivated of this group, and thus the scaling-up of the program might not induce positive effects of the same magnitude as those documented below.

All eligible applicants were entered in a lottery drawing which designated those who would be offered to participate in the program (the treatment group) and those who would become part of the control group.[4] The lottery took place on November 9th, 2010 with the presence of public officials and members of the several organizations that collaborated in the program (training partners, business associations, etc.), and to stress the transparency of the process, a public notary certified the draw. Applicants were made aware of the method of assignment into treatment and control groups. They accepted the selection process' terms and provided consent for the implementing agency and the evaluation team to track their future labor market and other related outcomes for the purpose of the program's impact evaluation.

From the 407 eligible applicants, 220 were randomly assigned to participate in the program through this public lottery (for an expected intake of about 200). The remaining 187 were excluded from the program. Out of the total of 220 assigned to treatment group, 146 participated in the program, while the 74 remaining either declined participation at the beginning of the training or could not be reached by the implementing agency. A total of 106 participants completed the training phase, although several others participated partially in the training and/or internship. In terms of power calculations, the sample size of 220 individuals in the treatment group and 187 in the control group only permits the detection of relatively large effects in employment – about 8 percentage points, which amounts to an effect size of 0.30. The strategies to maximize

[4]The random assignment was a simple lottery that divided applicants between a treatment and a control group. While, the high persistence of formal employment (the main outcome variable) implied that we could have implemented some form of stratification, Bruhn and McKenzie (2009) highlight that for samples of 300 or more all commonly used methods of randomization perform similarly.

the statistical power of the estimates are discussed in the following section.

Given the timing of the program, we define the pre-program (or pre-treatment) period as up to the third quarter 2010 (Q3-2010), and the post-treatment refers spans from the second quarter of 2011 to the first quarter of 2015 (Q2-2011 to Q1-2015). We exclude from the analysis the months during which beneficiaries were undergoing training and participating in internships (the fourth quarter of 2010 and the first quarter of 2011).

# 3 Data Sources and Baseline Characteristics

## 3.1 Data Sources

We obtained detailed baseline (pre-program) information about the applicants and their households from the program's application form, which served as the program's targeting tool. We designed this application form as a short questionnaire based on the national periodical household survey. This survey was administered by the SDSE and the MDS to the 560 applicants. We use this information to verify the balance in observable socio-economic and demographic characteristics between the treatment and control groups for the 407 eligible individuals in our sample, and as controls in the estimations.

The main input to gauge the impact of the program on the expected outcomes is derived from administrative records. The SIPA (*Sistema Integrado Previsional Argentino*) is an integrated database setup jointly by the social security administration, ANSES (*Administración Nacional de Seguridad Social*), and the national tax authority (AFIP - *Administración Federal de Ingresos Públicos*), which records each registered workers' earnings and employment status on a monthly basis. The database was originally setup to track workers' "contributions" for their social insurance benefits (basically, payroll taxes earmarked for health and unemployment insurance, and payments into individual retirement accounts or the public pay-as-you-go pension system), and it has grown to include other related information such as participation in welfare programs. We matched each individual's national identification number to this database, and were able to obtain complete records on registered employment spells and the resulting gross labor earnings from January 2003 to November 2013, and for monthly employment status only (i.e., without gross earnings) for the period from January 2014 to March 2015. Since the oldest individuals in our sample were about 20 years old in 2003, we concentrate the analysis of employment and earnings from January 2008 onwards. The database was provided to us with the employers' tax identification number, although this identifier was masked for privacy reasons (also for the period from January 2003 to November 2013). We thus do not have detailed information about the employers, but we are able to tell whether individuals in our sample stay with the same employer over time.

Relying on these administrative records provides a detailed and precise record of employment status and earning levels for program participants, spanning 8 years before and 4 years after the

program. As discussed above, most evaluations of training programs in Latin America rely on follow-up surveys that suffer from attrition and cover only short term effects. However, it should be stressed that by their nature, these administrative records only provide information on formal (or registered) employment and on earnings derived from this type of employment. It is likely that some individuals in our sample engage in informal employment, for which we do not have any information. Despite this limitation, the analysis presented below can still contribute to our understanding of the impact of youth labor training programs. On the one hand, the trade-off is between more detailed information on short term outcomes at one point in time (with some attrition and measurement error), and more complete and precisely measured information on a monthly basis that allows us to analyze outcomes in the short, medium and long run, and to study employment transitions and dynamics in general. On the other hand, the program's objective was to improve the employment opportunities of at-risk youth and to increase the probability of finding "good quality" (i.e., formal) jobs, so that the administrative records allow us to study the program's main intended outcome.[5]

## 3.2 Baseline Characteristics and Experimental Balance

Table 1 provides descriptive statistics of a series of individual and household characteristics and pre-treatment outcomes for our sample, the 407 eligible applicants. As can be observed, around 29% of the program participants are male, the average age at the time of application was 23.55, and more than 70% have, at most, a high-school degree with no tertiary education. Most applicants were single (69%) and only 19% had children. The p-values in the last column indicate that individual characteristics are balanced between the treatment and control groups.

[INSERT TABLE 1 ABOUT HERE]

Panel B in Table 1 presents summary statistics of pre-treatment levels for the main outcomes: formal employment status (an indicator variable equal to one if the individual appears in the administrative database as employed in a given month or quarter) and gross labor earnings. Average employment levels between the first quarter of 2003 and the fourth quarter of 2007 are low at 7% for the treatment group and about 5% for the control group, which could be expected given the long time span of the administrative database (the average age of participants in 2003 was 16). The difference between the treatment and control groups for this variable is not statistically significant at standard levels (p-value of 0.13). The same variable computed for the more immediate pre-treatment period, from the first quarter of 2008 to the third quarter of 2010, indicates higher employment levels of 0.16 and 0.12 for the treatment and control groups respectively. These formal employment levels are higher than for the previous period, which

[5]The Appendix provides detailed definitions of all outcome variables discussed in this section, as well as a description of the different time frames we use for each outcome.

could be expected given the applicants' age, but still relatively low. This is compatible with the applicants' age and disadvantaged socio-economic background. The difference between the treatment and control groups for this period is statistically significant (p-value of 0.07), and a more in-depth analysis indicates that the difference was statistically significant for the last two quarters of 2009 and the first quarter of 2010, that is, only for 3 out of the 11 quarters before the program started (the evolution of this variable is depicted in panel A, Figure 1). We present the results from a multivariate test of balance in pre-treatment outcomes and characteristics below.

Panel B in Table 1 also presents summary statistics for earnings for the two pre-treatment periods for which we have information. These values are expressed in real Argentine pesos using January 2011 as the base month. Average monthly earnings were around 400 U.S. dollars during the period Q1-2008/Q3-2010 for individuals who were afterward selected into the treatment group, and about $366 for the earlier period (Q1-2003/Q4-2007). There were no statistical differences in earnings for either period between subjects in the treatment and control groups who were employed.

The last row of the table presents the p-value from an experimental balance test implemented on the variables in Table 1. The test is a likelihood-ratio test for equality of means between groups, which amounts to a version of Hotelling's T-squared generalized means test that allows for heterogeneous covariance matrices across groups. The p-value of 0.302 implies that we cannot reject the null hypothesis that the means of the variables in Table 1 are equal between the two groups when tested jointly.[6] Given this result, and the public lottery that assigned applicants to the treatment and control groups, we attribute this difference to chance. There are 16 variables in the table, which implies that we can expect statistically significant differences to appear randomly, especially since our sample of 407 is relatively small. The observable individual characteristics in panel A and in most pre-treatment outcomes appear to be balanced. Only three out of thirty pre-treatment quarters for which we have information appear to have a statistically significant difference in employment between the two groups. Moreover, as described in the previous section, the random assignment process was transparent and conducted by means of a public lottery certified by a notary. In order to gain precision in our estimates of the program's effects, we exploit these differences and include controls for pre-treatment employment levels, for other relevant pre-treatment outcomes, and for individual characteristics in the regressions presented below (Duflo et al., 2008).[7]

[6]The null for Hotelling's test and for the likelihood ratio test is that a set of means is equal between two groups. The former assumes that the covariance matrices in the two groups are the same, whereas the second allows for heterogeneous covariance matrices across groups.

[7]The results are robust to the exclusion of these controls, as documented in the Appendix table. Moreover, we also constructed inverse probability weights to correct for any imbalances in pre-treatment outcomes and characteristics between the treatment and control groups. The results using these weights are quantitatively very similar and qualitatively the same as those presented below (results not shown).

## 3.3 Estimation

Most of the results presented below are derived from OLS regressions where the regressor of interest is the indicator of whether an eligible applicant was randomly selected to participate in the program (treatment group) or not to participate in the program (control group). As discussed above, we include controls for individual characteristics and pre-treatment outcomes to control for minor chance imbalances in the randomization and to gain precision in our estimates.[8] Most of the regressions presented below are of the form:

$$Y_i = \alpha + \beta TreatmentGroup_i + \delta X_i + \varepsilon_i \quad (1)$$

where $Y_i$ indicates the outcome of interest (employment, earnings) for each individual $i$ in the sample, $\alpha$ is the constant, $TreatmentGroup_i$ the indicator for being assigned to the treatment group ($TreatmentGroup = 1$) or the control group ($TreatmentGroup = 0$) and $X_i$ is a vector of individual characteristics (including the individual's age, sex, educational achievement and marital status), pre-treatment average employment for the period Q1-2008/Q3-2010 (included in all regressions), and the pre-treatment level of the dependent variable $Y_i$. The estimate of $\beta$ from regressions of this type corresponds to an intention to treat (ITT) estimator. We also carry out an analysis of heterogeneous effects by sex and by age group for some of the outcomes of interest by including interactions between the treatment group indicator and the relevant variables, although the small sample size implies that this analysis might be limited in terms of statistical power.

For the main outcomes summarized in Table 2, we also computed the effect of the program from regressions of the outcomes of interest as a function of actual participation in the program, $D$, of the form: $Y_i = \alpha + \beta D_i + \delta X_i + \varepsilon_i$, with participation $D$ instrumented by the random assignment variable $TreatmentGroup$. Since in the case of *entra21* none of the individuals in the control group ended up participating in the program, this one sided non-compliance implies that the estimate of $\beta$ in the instrumental variables regression captures the treatment on the treated (TOT) effect of the program (Angrist et al., 1996). This amounts to up-scaling the ITT effects by the first stage effect of the instrument on the participation variable.[9] However, we follow most of the training evaluation literature and concentrate the discussion on the ITT effects. On the one hand, ITT is arguably the policy relevant parameter: since in most cases individuals are free to decide whether to take up a program or not, ITT provides policy makers with the effect of offering a program. Moreover, the selection of individuals into the program after the random assignment and the different alternatives for defining actual participation (e.g., some training,

[8] When we exclude these controls and conduct simple comparisons of means between the treatment and the control groups, the results are qualitatively the same and quantitatively very similar for all outcomes discussed below, although there are some minor losses in precision in some cases (but also some gains in others). The equivalent results for the main results (Table 2) are presented in the online appendix (Table F.1).

[9] A total of 106 out of 220 individuals in the treatment group completed all phases of the program, so the scaling up factor is 0.481. This first stage effect of $Z$ on $D$ is significant at the 1% level.

all the training phase, training and internship phases, etc.) complicates the interpretation of the TOT effects (Flores et al., 2012; Hirshleifer et al., 2015).

We construct the dependent variables in these regressions as averages or other statistics of the underlying indicators for different spells of the administrative panel data. For our main results, we also estimate regressions exploiting the full nature of the panel, following McKenzie's (2012) discussion of evaluations in the context of long panels. McKenzie (2012) shows that a specification including pre-treatment averages of the outcomes as controls, time controls, and the full panel of the outcome variable as the dependent variable in the post-treatment period can have more power than simple post-treatment estimation and other alternatives, such as difference-in-differences. However, the gains in efficiency of this ANCOVA estimation depend on the level of autocorrelation of the outcome variable, which in our case is very high (for instance, about 0.8 for quarterly employment). For this reason, we opt to use aggregates of the outcomes as dependent variables in most of our results.

The analysis presented below presents the effect of the program on the main outcomes of interest for different post-treatment time frames. The program's assignment lottery was conducted in November 2010. The training started shortly afterward and was completed by February 2011. Most participants had completed their internships by March 2011. In keeping with the analysis of programs of this type, we consider observations up to the third quarter of 2010 as the pre-treatment period, and exclude from the analysis the fourth quarter of 2010 and the first quarter of 2011, when participants were undergoing training and/or internships.[10] We carry out the analysis in terms of short run effects, which refer to the first third of our post-treatment period (the quarterly average outcomes for the period between the second quarter of 2011 and the third quarter of 2012, and the effect computed for outcomes in the third quarter of 2012); the medium run effects, which refer to the second third of the post-treatment period (the quarterly average of outcomes for the period between the fourth quarter of 2012 and the fourth quarter of 2013, as well as outcomes from the fourth quarter of 2013 only); the long run effects (for employment), which refer to the last third of the post-treatment period (the quarterly average of outcomes for the period between the first quarter of 2014 and the first quarter of 2015, as well as outcomes from the first quarter of 2015 only) and the average effect over the whole period of analysis (from the second quarter of 2011 to the fourth quarter of 2013, for earnings, and from the second quarter of 2011 to the first quarter of 2015, for employment).

[10] All the results presented below are robust to the exclusion of the second quarter of 2011 from the analysis, when a minority of participants were still engaged in the program's internships.

# 4 Employment and Labor Earnings

## 4.1 Employment Levels

Training programs are designed to build human capital and foster the acquisition of skills, and their main expected outcome is employment improvement. However, these programs can also facilitate the contact of beneficiaries with the labor market, providing work experience, implicit or explicit labor market intermediation, contacts and references for future employment. These effects could be present even if the programs fail to build or reinforce beneficiaries' skills and human capital. However, distinguishing between these two set of effects is not evident. This section summarizes basic results on the impact of *entra21* on employment levels, and then builds on estimates of the program effects on labor earnings and employment transitions to discuss which of these effects may be at play in this program.

[INSERT TABLE 2 ABOUT HERE]

Table 2 presents the impact of the program on the main outcomes of interest for the time frames detailed above. Panel A presents the main benchmark results for employment. The results in the first column indicate that the program was successful in increasing formal employment in the short run: individuals in the control group exhibit a 7.96 percentage points higher probability of being in formal employment over this period (significant at the 5% level), which represents a 31.8% increase with respect to the control group's mean rate. This short term effect is even stronger when we compute it for the endpoint of the first third of the post-treatment period, the third quarter of 2012: the difference is 10.2 percentage points, a 41.5% increase with respect to the control group.

The estimates in the last columns of panel A in Table 2 indicate that these short term effects dissipate in the medium and in the long run. The effects for the Q4-2012/Q4-2013 average and for the endpoint of the medium run, the fourth quarter of 2013, are still positive but lower (0.0434 and 0.0382 respectively) and not statistically significant at standard levels. The same is true for the programs' effects on employment for the Q1-2014/Q1-2015 average and for the endpoint of the whole period, the first quarter of 2015, which are also still positive but substantially lower (0.0142 and 0.0171 respectively) and not statistically significant at standard levels. The combination of strong short term effects and weaker medium and long run effects still results in a positive effect of 4.78 percentage points for the overall period, but this effect is not statistically significant, as indicated by the coefficient in the "Average Post Treatment" column of the table. As expected from the first stage coefficient, the impact is almost twice as high for all the TOT estimates, and these results have the same pattern of statistical significance as the ITT effects. Finally, the final column presents results following McKenzie's (2012) ANCOVA estimation strategy, that is, a panel data regression for the whole post treatment period, which includes time (quarter)

controls, the average of the pre-treatment outcome (employment for the period Q1-2010 to Q3-2010), and with standard errors clustered at the individual level. Despite the substantially higher number of observations (6,512 versus 407), the clustering at the individual level and the high auto-correlation of the outcome variable imply that the increase in power does not seem to be enough. This procedure yields similar estimates as those for the "Average Post Treatment": the effect of the program on employment is 0.0417 and not statistically significant at standard levels.[11]

[INSERT FIGURE 1 ABOUT HERE]

These results for employment indicate that the effects of the program tend to fade over time. This is apparent by inspection of panel A in Figure 1. The employment trends indicate that employment levels increased for all applicants to the program at the time of application, but substantially more for those selected to participate, although the gap between the treatment and control groups falls substantially around the end of 2012 (the boundary of our "short term" period). However, this may not be true for all participant groups. For instance, evidence from existing evaluations of training programs in Latin America indicate disparities by gender and by age-group. Table 3 presents a breakup of the previous results for formal employment along these dimensions, including interactions in the main regression. We present first the treatment effect for the main group, then the difference between the treatment effects for the two subgroups, and finally, for comparison, the difference in levels between the two subgroups in the control group.

[INSERT TABLE 3 ABOUT HERE]

Panel A in Table 3 presents this breakup of the ITT estimates of program impact on employment by gender. The results indicate a comparatively large treatment effect for men of 24.46 percentage points for the average of the Q2-2011/Q3-2012 period, 19.92 percentage points for the average of the Q4-2012/Q4-2013 period, and 14.14 percentage points for the average of the Q1-2014-Q1-2015 period, with an average effect for the whole post-treatment period of 19.82 percentage points (all statistically significant at standard levels). These effects seem to decrease over time. This is confirmed by the estimates for the last quarters of our three subperiods, which indicate a difference of 12.38 percentage points in Q1-2015, not statistically significant at standard levels. The table also indicates large and statistically significant differences between the treatment effects (i.e., the treatment-control differences conditional on the control variables) for men and women. In fact, these large negative differences indicate the lack of significant effects on employment for women for all of the subperiods and quarters considered.[12] The differences in employment between men and women in the control group are relatively small and not statistically

[11] We also computed the programs' effects for the sub-periods by means of ANCOVA estimates, and we found very similar results as with the "Average" columns in Table 2 (results not reported).

[12] The coefficients for women can be derived by adding up the effect for men and the treatment effect difference. They are all small (ranging from a positive 1.54 to a negative 4.29 percentage points) and not statistically significant at standard levels.

significant. These heterogeneous results by gender may in part be explained by the differences between men and women at baseline. Young men who postulated to the program were on average about 1.2 years younger than women candidates (22.85 compared to 24.03), they had a lower probability of having children (19.5% compared to 22.6%) and of cohabiting (21.9% compared to 26.5%), they also had higher average educational achievements (for instance, 36.72% had completed secondary school, compared to 31.2% for women), and they had substantially higher levels of pre-intervention formal employment (the probability of having ever been employed formally before the program was twice as large for men than for women). The program might have been more effective for younger, more educated beneficiaries and more experienced participants, and this may explain the difference in results between male and female participants.

The results in panel B of Table 3 partially confirm this. Following the same structure as in the previous panel in the same table, we find positive and statistically significant effects for the younger group (those aged 18 to 24 at the baseline) of more than 10 percentage points for the short and medium term and for the average of the whole post-treatment period, although these effects are not statistically significant for the longer term subperiod, Q1-2014 to Q1-2015. The difference in treatment effects between the two groups is not statistically significant in the short and medium run, and the negative and significant effects for the period Q1-2014 to Q1-2015 indicates implicitly a negative treatment effect of about 6 percentage points for the older group (although this difference is not statistically significant at standard levels). Finally, the results in the table indicate that there are no significant differences in employment between the two age groups in the control group.

These heterogeneous results complement the main results for formal employment from panel A in Table 2: they show that effects are stronger for male and for younger participants over the whole period than the average effect for the full sample, and they remain strong for male participants in the medium run, although they seem to wind down in the long run.

## 4.2 Monthly Labor Earnings

Another important dimension of the impact of labor training programs is their potential effect on real labor earnings. Panel B in Figure 1 presents the evolution of earnings (in real January 2011 Argentine pesos) from the first quarter of 2008 to the fourth quarter of 2013. There appears to be a post-treatment divergence between the two groups, with substantially higher levels and steeper trajectories for those in the treatment group. Panel B in Table 2 reports the ITT estimates of the program's effects on real earnings for the same sub-periods discussed earlier. The pattern of results is similar to that of formal employment: there are sizable and statistically significant effects in the short run: differences of 332.23 pesos (about $83 USD, significant at the 1% level) for average monthly earnings for Q2-2011/Q3-2012, and of 328.18 pesos (about $82 USD, significant at the 5% level) for the third quarter of 2012 (first and second column). The effects on average

earnings are still positive but smaller and not statistically significant for Q4-2012/Q4-2013 and for Q4-2013 (179 and 98.96, third and fourth column), with an average effect for all the short and medium term post-treatment period of 265.19 pesos (about $62.3 USD), significant at the 5% level (seventh column).[13] As with employment, the results from the McKenzie's (2012) ANCOVA estimates in the last column are very similar in size and statistically significance to those of the "Average Post Treatment".

[INSERT TABLE 4 ABOUT HERE]

Panels A and B in Table 4, in turn, present the heterogeneous impact of the program on monthly earnings by gender and by age group, respectively. While there are almost no statistically significant differences by age group, the impact of the program on earnings is markedly stronger for men than it is for women. In fact, the treatment effect for men and the difference between treatment effects between men and women indicate a virtually nil effect on women's earnings (implied coefficients ranging from -171.31 to 12.99 pesos, none of them statistically significant).

The estimates of the program's effects on earnings presented in Tables 2 (panel B) and 4 (panels A and B) have a mechanical relationship with those for employment. Increases in total labor earnings can be caused by higher employment levels, by increases in earnings for those already employed, or by both. The total impact is a combination of productivity gains and changes in employment composition. If the program increases participants' human capital, beneficiaries become more employable and more productive once employed, which would be reflected in both higher employment and higher labor earnings levels for those employed. Alternatively, the program may not have an effect on human capital and thus it would not change beneficiaries' productivity, but it may be successful in contacting beneficiaries with future employers. In this case, we could expect higher employment but not higher earnings. Since the estimates we presented so far are based on outcomes that include zero incomes (i.e., those who are not employed), the positive impact on earnings alone does not allow us to separate the employment effect from any direct impact on earnings.

We follow Attanasio et al.'s (2011) approach,[14] which makes additional assumptions based on the distribution of earnings and employment for the control group in order to estimate the program's impact on productivity.[15] They divide the sample of individuals in four groups: those who work regardless of program participation (what Angrist and Imbens, 1994, refer to as "always takers"), those who would never work, those who begin to work because of the program (what

[13]The same pattern of results holds when we consider additional earnings aggregates, for instance, bounding extreme values at the 99th percentile of the earnings distribution in the control group, or using an inverse hyperbolic sine transformation as in Hirshleifer et al. (2015) (results not reported).

[14]Lee (2009), Chen and Flores (2012) and Blanco et al. (2013) have proposed several alternatives that separate these effects and obtain bounds for treatment effects on labor earnings in the case of training and similar active labor market policies.

[15]The procedure to construct these bounds is derived in Appendix B of Attanasio et al. (2011).

Angrist and Imbens, 1994, label as "compliers") and those who stop working because of the program. Randomization ensures that the size of each group is independent of assignment to treatment. Using the monotonicity assumption, individuals who would work without the program would also work if they did the training. This allows us to decompose the effect of the program as the sum of the effect on the earnings of compliers plus the effect on the earnings of always takers. We can estimate the productivity gain from the program and the change in composition. The previous results show that average earnings increased, but we cannot conclude whether this is due to productivity gains or to changes in employment. The bounds are determined by estimating the productivity effects and the distribution of wages in the control group, and they are presented in panel C of Table 4. The bounds computed for the average earnings over the whole post-treatment period (fifth column) are large and positive, and the lower bound with the monotonicity assumption only is negative, it is close to zero (-24.33). To narrow the interval, we also consider an additional assumption: non-program earnings of those who always work are at least as high as the non-program earnings of individuals who are no longer unemployed. The bounds with this additional assumption are tighter, and indicate an effect on monthly earnings between 576.75 and 1,177.82 pesos for the average over the whole post-treatment period.

We interpret this result as limited evidence that the program increased earnings for those employed, over and above its effect on employment. This evidence and that of positive employment effects seems to support the hypothesis that the program managed to increase beneficiaries' productivity.

## 4.3 Employment Transitions

The discussion so far is in line with that of most studies of training programs in Latin America: while we are able to gauge the effect over a longer period, we presented the impact of the program at one, two or three post-treatment subperiods using follow-up information. However, the rich administrative data we use can be further exploited to establish some of the mechanisms through which the program generates its positive impacts on formal employment. To do this, we use the full panel data structure of monthly information from the administrative records to estimate models of employment transitions, spells and related outcomes.[16]

[INSERT TABLE 5 ABOUT HERE]

Table 5 presents the results from an analysis of the program's impact on simple indicators of employment transitions and individual aggregates of employment over time. Panel A presents the results from a regression where the dependent variable indicates whether the individual was

[16]Card et al. (2011) also carry out an analysis of this type, although theirs is based on retrospective information collected at a single point in time after the program. They reconstruct employment spells and transitions from this information.

employed in any month within each period, for the first third of the post-treatment subperiod (first column), the second third (second column), the last subperiod (third column) and the whole treatment period (fourth column). Focusing on the latter, we observe that 58.29% of individuals in the control group were formally employed for at least one month over the 16 quarters considered, and that this proportion was 8.05 percentage points higher for the treatment group (a 13.81% increase, significant at the 10% level). The same effect appears for the first two thirds of the post-treatment period, and it is again stronger in the short term.

The dependent variables in the regressions presented in panels B and C of Table 5 represent basic summaries of employment transitions. In panel B, the dependent variable is an indicator of whether individuals ever entered formal employment in each period,[17] whereas the dependent variable in panel C is an indicator of whether individuals ever left formal employment. The coefficients for the overall post-treatment period (fourth column) are positive for both dependent variables, but relatively small and not statistically significant.

The dependent variables in Table 5 represent summary statistics of employment transitions over the post-treatment period. We also estimate a dynamic model using the full monthly panel data on employment. We follow Card et al. (2011) and estimate a simple dynamic model of the form:

$$Y_{it} = \alpha + \beta TreatmentGroup \times Y_{i,t-1} + \rho TreatmentGroup \times (1 - Y_{i,t-1}) + \theta Y_{i,t-1} + \delta X_i + \phi_t + \varepsilon_{it} \quad (2)$$

where $Y_{it}$ is employment, the outcome of interest (taking values 0 or 1), $Y_{i,t-1}$ is the same outcome in the previous month, and $TreatmentGroup_i$, as above, is the indicator for being assigned to the treatment group. The coefficient $\beta$ on the interaction between the treatment group indicator and the outcome in the previous period captures the degree of persistence of formal employment (i.e., the probability of continuing in employment once the individual is employed). The coefficient $\rho$ on the interaction between the treatment group indicator and the transformation $1-Y_{i,t-1}$ (which indicates whether individual $i$ was not employed in the previous period) captures what we label as an access effect (i.e., the probability of entering employment when the individual is unemployed). The coefficient $\theta$ captures the overall degree of dependence of current employment status on that of the previous period for individuals in both the treatment and the control group. We also include controls $\phi_t$ for every month, a set of individual characteristics $X_i$ as controls, and we cluster standard errors by individual. While this analysis is relatively common in the evaluation of training programs (see for instance Card et al., 2011), we should be careful with the interpretation of the results. The original random assignment of applicants to treatment and control groups only warrants the identification of overall post-treatment outcomes, whereas in these regressions

[17]This dependent variable differs from that in Panel A in that individuals that were always employed every month in a given sub-period are not classified as having entered into formal employment.

we include lagged post-treatment outcomes in the right hand side. While this is useful in terms of illustrating the post-treatment employment dynamics, the causal interpretation of the coefficients is no longer warranted by the random assignment, since the inclusion of $Y_{i,t-1}$might lead to the "bad controls" problem described by Angrist and Pischke (2008, Chapter 3).

[INSERT TABLE 6 ABOUT HERE]

The results from this model are presented in Table 6 for the first third of the post-treatment period (first column), the second third (second column), the last third (third column) and the whole treatment period (fourth column). Panel A presents the results for the full sample. Besides a strong dependence of current employment with respect to the previous month, the persistence and access effects are small and not statistically significant, with the exception of the persistence effect for the short run of 0.0346, significant at the 10% level.

Panel B presents the results of the same model when restricting the sample to female applicants. The persistence effect over the second third of the post-treatment period is negative and significant, -0.0362 (significant at the 10% level) which indicates a small negative effect for women in the medium term. There is also a negative and significant coefficient for the whole post treatment period (-0.0236, significant at the 10% level) for women.

Panel C, in turn, presents the results for men in our sample. The short term persistence effects are much larger (0.0996) and significant at the 1% level. While the persistence effect for the medium and long term is close to zero and not significant, the short term effect still drives a positive and statistically significant effect of 0.0384 (significant at the 5% level) for the whole post-treatment period. The pattern is similar for younger individuals (panel D), although the short term persistence effect is weaker than that for men (0.0560), and the resulting coefficient for the whole period is smaller (0.0206) and not statistically significant. The program does not seem to have altered significantly the employment transitions of older individuals (panel E) for the whole post treatment period, although we find a negative and significant coefficient that indicates persistence effects in the long run (-0.0403, significant at the 10% level).

[INSERT TABLE 7 ABOUT HERE]

Finally, Table 7 presents the results from a simpler setup that complements the previous analysis of transitions. The table presents the differential probability of staying with the same employer between two points in time for individuals in the treatment and control groups.[18] The table presents this analysis for three periods of time: from the third quarter of 2010 to the third quarter of 2011 (that is, right before the start of the program and about 6 months after

[18]For privacy reasons, we cannot identify the employers, but we can match anonymised indicators of the employers' tax identification numbers to establish whether workers changed jobs or stayed in the same firms over time.

its end), from the fourth quarter of 2011 to the fourth quarter of 2012 (corresponding roughly to the short term after the intervention), and form the fourth quarter of 2011 to the fourth quarter of 2013 (i.e., spanning about two years after the program). The results indicate that the individuals in the treatment group did not have a higher probability of returning to their pre-intervention employers when compared to those in the control group (first column). The results in the second column, however, indicate that males and younger members of the treatment group had a significantly higher probability of remaining with the same employer they had after the end of the program for a year. The pattern of results for this outcome, however, is consistent with that of other outcomes: the coefficients in the third column are substantially smaller and not statistically significant, indicating that the positive employment retention results for the first year of the program dissipated after two years.

These results indicate that the program operates by helping individuals keep their jobs once they are employed rather than by helping them find a job, but only in the short term. This is compatible with a situation in which the program enhances the productivity of participants rather than just providing the means to find new jobs.

# 5 Discussion and Conclusions

The results for *entra21* discussed in this paper indicate that the program successfully increased the employment levels and the earnings of beneficiaries over the short run, with sizable gains that persisted over the medium run but fell and seem to dissipate in the longer run for men. An analysis of employment transitions indicates that the program operated by helping individuals keep their jobs once they are employed rather than by helping them find jobs. Moreover, an analysis of earnings bounds indicates that beneficiaries obtained higher wages once employed. Both results are compatible with a situation in which the program enhanced the productivity of participants, and suggest that the program was successful in increasing the human capital of participants. However, these effects were relatively short lived, and may also have resulted from a situation in which the program provided the means to find new jobs through more contacts or through formal intermediation, or through better matches, without necessarily affecting the human capital of beneficiaries. For instance, the increase in potential employment opportunities induced by the program implies that employees could afford to be more selective, and thus end up in jobs they liked more. A stronger motivation derived from a highly esteemed job may have translated into better work performance, and thus higher productivity, increasing tenure and wages at a given firm without directly affecting the worker's human capital. A related explanation is that the program's training on non-cognitive abilities and life skills may have resulted in a change of attitudes for participants that could benefit them in the labor market, both directly (i.e., increased valuation of having a career, a steady job, etc.) or indirectly (i.e., higher self

esteem).[19] The increase in persistence of employment, in this case, would be the result of a change in beneficiaries' appraisal of labor market opportunities and trajectories. There are also other plausible alternative explanations for these findings.[20] As discussed below, individuals in the treatment group may get hired by a different type of employer than those in the control group, and this type of employer may be more reluctant to dismiss employees, especially those from a youth training program (for public relations or because of a more socially minded objective). Finally, these results may also be interpreted in terms of screening: since initial productivity signals are probably noisy, the program's internship stage may have allowed employers to learn about worker productivity earlier on and thus to keep those with higher productivity. It should be stressed that most of these alternative interpretations for the pattern of results imply some permanent changes in beneficiaries, and that these changes in turn translated into better labor market outcomes for participants (higher employment levels in the short run, remuneration and persistence of employment in the longer run).

These positive effects are larger than those for most programs of this type in Latin America, but they must be gauged against the programs' costs. As highlighted in the description of the program, *entra21* entailed higher costs as compared to other training programs in the region that targeted the same beneficiary groups. According to program documentation, the cost of operation per trainee in the program analyzed here was approximately 1722 USD, more than twice the cost of the programs for Colombia (750 USD, Attanasio et al., 2011) and the Dominican Republic (330 USD; Card et al., 2011). The average gain in monthly earnings from the program was 265.19 pesos or about 66.3 USD (last column of Table 2, panel B), and the simplest measure of return (with no discounting) indicates that it took about 26 months to recoup the cost of the program.

We can compare the costs and benefits from *entra21* to those obtained by Attanasio et al. (2011) for the Colombian initiative. We compute our cost-benefit analysis by matching their scenario in which they assume that the gains from the program are permanent but that they depreciate at a 10 percent annual rate.[21] Like Attanasio et al. (2011), we assume that the working life of applicants is 40 years and apply a 5% yearly discount rate. The Colombian program was significantly less expensive, at 750 USD per person, compared to 1722 USD per person for *entra21*. Despite the difference in costs, the driving factor of this large difference in returns is the effect of the program: the Colombian program results in higher earnings of about 17.6 USD per month (for women), whereas our estimate for *entra21* yields about 66.3 USD per month.[22] Attanasio et al.

[19] If these changes are permanent, they result in better job market prospects for beneficiaries, and the discussion of whether this is an effect on human capital or not is merely definitional.

[20] We are grateful to Robert Jensen for suggesting some of these alternative explanations.

[21] For comparison purposes, we follow Attanasio et al.'s (2011) computation and presentation of rates of returns, but there might be some caveats. For instance, the overall impact of training is not significant for the second post-treatment period, which could imply that the effect of training is zero for periods after the first. In that case, we could rely instead on estimates for men only.

[22] These estimates ignore additional potential costs and issues such as the job displacement of others by program beneficiaries or the welfare cost arising from distortionary taxes to finance the intervention.

(2011) compute a life cycle net benefit of 666 USD for women, which corresponds to an internal rate of return of 21.6 percent. We compute a much higher gain of 3,835.30 USD for individuals in our sample, which yields an internal rate of return of 67.29%. The availability of longer term high frequency administrative data allows us to refine these estimates. For instance, the effect of the program declines between the first and the second third of the post-treatment period by about 20%. Using this as a more realistic input for the discount rate yields a still substantial but lower internal rate of return of 48.7%, which falls even further when using alternative measures of depreciation (for instance, to 11.5% with a more conservative implicit depreciation of 40%).[23]

The analysis and the results allow us to draw some conclusions. The results presented in our paper add to the evidence that private sector involvement typically results in better outcomes for participants in job training programs (Card et al., 2010). However, our evidence does not allow us to distinguish whether this is a general result or whether it corresponds to some form of selection bias. Employers from participating firms may be more socially minded, for instance, and this might explain some of our results such as increased employment and persistence among beneficiaries (which may not respond only to a monetary cost-benefit analysis for socially minded firms), or it could be done for public relations. While this implies an alternative explanation for our findings (i.e., not necessarily an increase in productivity), from a policy point of view this self-selection of firms is a positive outcome, since it is better to have more motivated employers participating in training programs, and employment levels among participants would still increase. If the firms with more socially minded employers participated in the pilot, however, this might be a problem in terms of scaling-up the program – or at least it would imply that an expanded version of the program might not benefit as much from employers of this type.

From a methodological perspective, the evaluation of training programs should attempt to follow beneficiaries over a longer period of time than the usual one or two years. Our results for the whole sample coincide with other programs evaluated over similar time periods in the region, but these average employment gains dissipate in the longer run. This contrasts with the available evidence for developed countries, which typically finds positive medium-term impacts of training programs that often appear ineffective in the short term (Card et al., 2010). Moreover, our analysis illustrates the usefulness of experimental evaluations even with small sample sizes when combined with rich administrative records that offer precise measurements of the outcomes of interest and their dynamics over long periods of time. The analysis also illustrates how this type of data allows us to further probe the mechanisms through which the program operates, by studying the dynamics of employment transitions. The use of administrative data is still relatively uncommon in studies on developing countries, but it could provide fruitful results in the future.

[23] We should of course stress that the gains from the program may come from displacement effects (i.e., job gains for the treatment group might have accrued to others in the absence of the program). As with most evaluations at the micro level, our analysis is limited by not being able to assess these general equilibrium effects.

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# Appendix A: Main Variables

**Random assignment**: this variable indicates whether the individual was randomly assigned to participate in the training program (takes value 1 for the treatment group, 0 for the control group).

**Completed training**: dummy variable that indicates if an eligible individual participated and completed the training phase.

**Formal employment**: dummy by quarter that takes value 1 if a person was employed at least one month in the quarter.

- *Average Q2-2011 to Q3-2012*: average of the formal employment variable for these quarters – labeled as short term.
- *Average Q4-2012 to Q4-2013*: average of the formal employment variable for these quarters – labeled as medium term.
- *Average Q1-2014 to Q1-2015*: average of the formal employment variable for these quarters – labeled as long term.
- *Average Q2-2011 to Q1-2015*: average of the formal employment variable for all the post-treatment period.

**Monthly earnings**: Monthly earnings.

- *Average Q2-2011 to Q3-2012*: average of monthly earnings for these quarters (short term).
- *Average Q4-2012 to Q4-2013*: average of monthly earnings for these quarters (medium term).
- *Average Q2-2011 to Q4-2013*: average of monthly earnings for all the post-treatment period.

Table 1: Summary statistics: Individual characteristics at baseline and pre-treatment outcomes

| | Treatment | | Control | | Difference | | |
|---|---|---|---|---|---|---|---|
| Variables | Mean (1) | SE | Mean (2) | SE | (1)-(2) | SE | P-value |
| *A) Baseline survey data* | | | | | | | |
| Male | 0.295 | 0.031 | 0.337 | 0.035 | -0.041 | 0.046 | 0.371 |
| Age | 23.545 | 0.238 | 23.797 | 0.263 | -0.251 | 0.354 | 0.478 |
| Incomplete elementary school | 0.036 | 0.013 | 0.027 | 0.012 | 0.010 | 0.018 | 0.583 |
| Complete elementary school | 0.077 | 0.018 | 0.053 | 0.016 | 0.024 | 0.025 | 0.338 |
| Incomplete high school | 0.277 | 0.030 | 0.326 | 0.034 | -0.049 | 0.046 | 0.284 |
| Complete high school | 0.327 | 0.032 | 0.332 | 0.035 | -0.004 | 0.047 | 0.927 |
| Incomplete tertiary level/college | 0.177 | 0.026 | 0.160 | 0.027 | 0.017 | 0.037 | 0.653 |
| Complete tertiary level/college | 0.068 | 0.017 | 0.059 | 0.017 | 0.009 | 0.024 | 0.701 |
| Children in the household | 0.191 | 0.027 | 0.246 | 0.032 | -0.055 | 0.041 | 0.179 |
| Single | 0.695 | 0.031 | 0.668 | 0.035 | 0.027 | 0.046 | 0.561 |
| Married/cohabiting | 0.232 | 0.029 | 0.273 | 0.033 | -0.041 | 0.043 | 0.344 |
| Divorced/separated | 0.045 | 0.014 | 0.037 | 0.014 | 0.008 | 0.020 | 0.688 |
| *B) Administrative data* | | | | | | | |
| Avg. employment Q1 2003-Q4 2007 | 0.069 | 0.012 | 0.046 | 0.010 | 0.024 | 0.016 | 0.135 |
| Avg. employment Q1 2008-Q3 2010 | 0.162 | 0.019 | 0.115 | 0.017 | 0.047 | 0.026 | 0.067 |
| Avg. earnings Q1 2003-Q4 2007 | 1,463.963 | 88.441 | 1,351.149 | 106.361 | 112.814 | 140.324 | 0.424 |
| Avg. earnings Q1 2008-Q3 2010 | 1,599.478 | 83.751 | 1,513.600 | 105.938 | 85.878 | 134.372 | 0.524 |
| *Balance test p-value* | | | | | | | 0.302 |

Notes: The total number of observations is 220 for the treatment group and 187 for the control group. Panel B: *Avg. employment* indicates the average of the quarterly formal employment variable for the period. *Avg. earnings* indicates the average of monthly real labor earnings over a given period, using January 2011 as the base month. The balance test is a likelihood-ratio test for equality of means between groups, a version of Hotelling's T-squared generalized means test that allows for heterogeneous covariance matrices across groups. The null hypothesis that the means of all variables in the Table are equal between the treatment and the control groups when tested jointly. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA).

Table 2: Program impact, main results: Intention to treat and treatment on the treated effects

| | Average Q2-2011/Q3-2012 | Q3-2012 | Average Q4-2012/Q4-2013 | Q4-2013 | Average Q1-2014/Q1-2015 | Q1-2015 | Average Post Treatment | Panel ANCOVA |
|---|---|---|---|---|---|---|---|---|
| *A. Formal employment* | | | | | | | | |
| Treatment group - ITT | 0.0796** | 0.1020** | 0.0434 | 0.0382 | 0.0142 | 0.0171 | 0.0478 | 0.0417 |
| | [0.0371] | [0.0465] | [0.0403] | [0.0450] | [0.0436] | [0.0475] | [0.0351] | [0.0340] |
| Completed training - TOT | 0.1641** | 0.2104** | 0.0895 | 0.0787 | 0.0390 | 0.0352 | 0.0987 | 0.0849 |
| | [0.0762] | [0.0962] | [0.0831] | [0.0925] | [0.0895] | [0.0979] | [0.0723] | [0.0691] |
| Control group mean | 0.2504 | 0.2460 | 0.3048 | 0.2888 | 0.3294 | 0.3262 | 0.2921 | 0.2921 |
| *B. Monthly earnings* | | | | | | | | |
| Treatment group - ITT | 332.23*** | 328.18** | 179.00 | 98.96 | - | - | 265.19** | 253.53** |
| | [124.77] | [149.34] | [151.70] | [159.27] | | | [127.59] | [117.00] |
| Completed training - TOT | 691.20*** | 664.43** | 362.41 | 200.35 | - | - | 536.91** | 517.62** |
| | [243.32] | [300.55] | [307.27] | [322.60] | | | [257.31] | [237.87] |
| Control group mean | 512.04 | 623.50 | 775.47 | 809.29 | | | 627.29 | 632.80 |
| Observations | 407 | 407 | 407 | 407 | 407 | 407 | 407 | A 6,512<br>B 4,477 |

Notes: the TOT coefficients correspond to instrumental variable regressions where the completed training indicator is instrumented by the treatment assignment (lottery) variable. All regressions include controls for gender, year of birth dummies, children in the household, education level, and marital status. All regressions also include the average of the pre-treatment formal employment variable for the period Q1-2008 to Q3-2010. Regressions in Panel B also include measures of pre-treatment earnings for the same period. Earnings data not available for the Q1-2014/Q1-2015 period. The ANCOVA estimates correspond to panel regressions with time effects for the post-treatment period, including the pre-treatment average as a control (McKenzie, 2012). Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Effects on formal employment: Heterogeneous impact

| | Average Q2-2011/Q3-2012 | Q3-2012 | Average Q4-2012/Q4-2013 | Q4-2013 | Average Q1-2014/Q1-2015 | Q1-2015 | Average Q2-2011/Q1-2015 |
|---|---|---|---|---|---|---|---|
| *A. By gender* | | | | | | | |
| Treatment effect: Men | 0.2446*** | 0.2947*** | 0.1992*** | 0.2072** | 0.1414* | 0.1238 | 0.1982*** |
| | [0.0726] | [0.0884] | [0.0759] | [0.0917] | [0.0836] | [0.0900] | [0.0662] |
| T.E. Difference: Women/Men | -0.2392*** | -0.2793*** | -0.2258*** | -0.2450** | -0.1843* | -0.1547 | -0.2179*** |
| | [0.0822] | [0.1010] | [0.0870] | [0.1026] | [0.0953] | [0.1038] | [0.0748] |
| Women/Men difference (controls) | 0.0240 | 0.0182 | 0.0927 | 0.0756 | 0.0256 | 0.0538 | 0.0460 |
| | [0.0601] | [0.0729] | [0.0665] | [0.0760] | [0.0706] | [0.0770] | [0.0552] |
| Observations | 407 | 407 | 407 | 407 | 407 | 407 | 407 |
| Control group mean | 0.2137 | 0.2177 | 0.2500 | 0.2500 | 0.2952 | 0.2823 | 0.2505 |
| *B. Age groups 18-24/25-30* | | | | | | | |
| Treatment effect: Age 18-24 | 0.1188** | 0.1415** | 0.1019* | 0.0901 | 0.0862 | 0.0943 | 0.1033** |
| | [0.0507] | [0.0632] | [0.0549] | [0.0621] | [0.0590] | [0.0652] | [0.0473] |
| T.E. Difference: Younger/Older | -0.0889 | -0.0973 | -0.1202 | -0.1026 | -0.1463* | -0.1615* | -0.1166* |
| | [0.0709] | [0.0873] | [0.0792] | [0.0878] | [0.0840] | [0.0909] | [0.0675] |
| Younger/Older difference (controls) | -0.0351 | 0.0091 | 0.0062 | 0.0318 | -0.0219 | -0.0017 | -0.0181 |
| | [0.0547] | [0.0690] | [0.0621] | [0.0692] | [0.0664] | [0.0717] | [0.0526] |
| Observations | 407 | 407 | 407 | 407 | 407 | 407 | 407 |
| Control group mean | 0.2933 | 0.2692 | 0.3385 | 0.3269 | 0.3635 | 0.3462 | 0.3293 |

Notes: Regression controls as described in the notes to Table 2, with the exception of the regressions in panel B in which the year of birth categories were replaced with age range categories. Sources: *entra21*, and Sistema Integrado Previsional Argentino (SIPA). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Program effects on monthly earnings: Heterogeneous impact and bounds

| | Average Q2-2011/Q3-2012 | Q3-2012 | Average Q4-2012/Q4-2013 | Q4-2013 | Average Q2-2011/Q4-2013 |
|---|---|---|---|---|---|
| *Panel A. By gender* | | | | | |
| Treatment effect: Men | 1,110.67*** | 1,268.33*** | 983.40*** | 758.00** | 1,054.99*** |
| | [276.17] | [328.20] | [339.43] | [372.37] | [280.00] |
| T.E. Difference: Women/Men | -1,097.68*** | -1,325.70*** | -1,134.28*** | -929.31** | -1,113.69*** |
| | [282.44] | [343.45] | [353.90] | [390.18] | [288.97] |
| Women/Men difference (controls) | -35.97 | -54.84 | 199.69 | 250.10 | 67.13 |
| | [157.79] | [217.41] | [217.88] | [250.42] | [171.19] |
| Observations | 407 | 407 | 407 | 407 | 407 |
| Control group mean | 442.0882 | 542.6750 | 641.6870 | 666.0570 | 529.4127 |
| *Panel B. Age groups 18-24/25-30* | | | | | |
| Treatment effect: Age 18-24 | 370.51** | 400.35** | 319.20 | 206.89 | 348.06** |
| | [160.52] | [198.81] | [197.91] | [216.19] | [167.95] |
| T.E. Difference: Younger/Older | -74.30 | -168.66 | -288.15 | -182.15 | -167.86 |
| | [221.55] | [268.71] | [267.75] | [287.23] | [226.94] |
| Younger/Older difference (controls) | 52.89 | 101.52 | -11.16 | -66.17 | 24.87 |
| | [152.14] | [194.04] | [192.62] | [213.11] | [160.20] |
| Observations | 407 | 407 | 407 | 407 | 407 |
| Control group mean | 578.7615 | 672.2666 | 876.1388 | 976.2771 | 708.8640 |
| *Panel C. Bounds* | | | | | |
| Lower Bound (monotonicity only) | -467.14 | -1,084.30 | -247.03 | -369.44 | -24.33 |
| Lower Bound (Attanasio et al) | 506.16 | 219.85 | 427.14 | 265.00 | 576.75 |
| Upper Bound | 1,479.46 | 1,524.00 | 1,101.30 | 899.43 | 1,177.82 |

Notes: Regression controls as described in the notes to Table 2, with the exception of the regressions in panel B in which the year of birth categories were replaced with age range categories. See Section 4 for details about the construction of earning bounds. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Program impact on employment aggregates over time: Ever formally employed, ever entered and ever left formal employment

| | Q2-2011/Q3-2012 | Q4-2012/Q4-2013 | Q1-2014/Q1-2015 | Q2-2011/Q1-2015 |
|---|---|---|---|---|
| *A. Ever formally employed* | | | | |
| Treatment group | 0.1134** | 0.0793* | 0.0114 | 0.0805* |
| | [0.0487] | [0.0473] | [0.0495] | [0.0463] |
| Control group mean | 0.4011 | 0.4011 | 0.4278 | 0.5829 |
| *B. Ever entered formal employment* | | | | |
| Treatment group | 0.0422 | 0.0043 | 0.0044 | 0.0436 |
| | [0.0466] | [0.0417] | [0.0360] | [0.0486] |
| Control group mean | 0.2727 | 0.2193 | 0.1497 | 0.4866 |
| *C. Ever left formal employment* | | | | |
| Treatment group | 0.0069 | 0.0483 | 0.0125 | 0.0540 |
| | [0.0440] | [0.0409] | [0.0370] | [0.0499] |
| Control group mean | 0.2567 | 0.1711 | 0.1551 | 0.4278 |
| Observations | 407 | 407 | 407 | 407 |

Notes: Regression controls as described in the notes to Table 2. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Employment transitions (monthly data)

| | Formal employment 04-2011/09-2012 | Formal employment 10-2012/11-2013 | Formal employment 01-2014/03-2015 | Formal employment 04-2011/03-2015 |
|---|---|---|---|---|
| *A. Full sample* | | | | |
| Persistence | 0.0346* | -0.0165 | -0.0062 | 0.0032 |
| | [0.0183] | [0.0139] | [0.0125] | [0.0101] |
| Access | 0.0014 | 0.0079 | 0.0016 | 0.0028 |
| | [0.0062] | [0.0062] | [0.0072] | [0.0047] |
| Dependence | 0.8522*** | 0.9170*** | 0.9288*** | 0.8803*** |
| | [0.0176] | [0.0118] | [0.0116] | [0.0088] |
| Observations | 7,326 | 5,698 | 6,105 | 19,536 |
| *B. Women only* | | | | |
| Persistence | -0.0139 | -0.0362* | -0.0265 | -0.0236* |
| | [0.0216] | [0.0189] | [0.0168] | [0.0130] |
| Access | -0.0007 | 0.0074 | -0.0026 | 0.0006 |
| | [0.0061] | [0.0058] | [0.0074] | [0.0046] |
| Dependence | 0.8966*** | 0.9354*** | 0.9353*** | 0.9033*** |
| | [0.0191] | [0.0127] | [0.0134] | [0.0103] |
| Observations | 5,022 | 3,906 | 4,185 | 13,392 |
| *C. Men only* | | | | |
| Persistence | 0.0996*** | 0.0074 | 0.0189 | 0.0384** |
| | [0.0291] | [0.0219] | [0.0201] | [0.0158] |
| Access | 0.0172 | 0.0267 | 0.0191 | 0.0169 |
| | [0.0189] | [0.0201] | [0.0198] | [0.0133] |
| Dependence | 0.7690*** | 0.8771*** | 0.9034*** | 0.8357*** |
| | [0.0298] | [0.0232] | [0.0229] | [0.0149] |
| Observations | 2,304 | 1,792 | 1,920 | 6,144 |
| *D. Age group 18-24* | | | | |
| Persistence | 0.0560** | -0.0029 | 0.0107 | 0.0206 |
| | [0.0246] | [0.0172] | [0.0156] | [0.0129] |
| Access | 0.0042 | 0.0107 | 0.0085 | 0.0066 |
| | [0.0096] | [0.0094] | [0.0107] | [0.0070] |
| Dependence | 0.8302*** | 0.9045*** | 0.9175*** | 0.8651*** |
| | [0.0239] | [0.0168] | [0.0157] | [0.0117] |
| Observations | 4,158 | 3,234 | 3,465 | 11,088 |
| *E. Age group 25-30* | | | | |
| Persistence | -0.0024 | -0.0377 | -0.0403* | -0.0273 |
| | [0.0254] | [0.0264] | [0.0236] | [0.0174] |
| Access | -0.0011 | 0.0007 | -0.0107 | -0.0039 |
| | [0.0080] | [0.0081] | [0.0094] | [0.0061] |
| Dependence | 0.8856*** | 0.9268*** | 0.9405*** | 0.8998*** |
| | [0.0237] | [0.0172] | [0.0166] | [0.0129] |
| Observations | 3,168 | 2,464 | 2,640 | 8,448 |

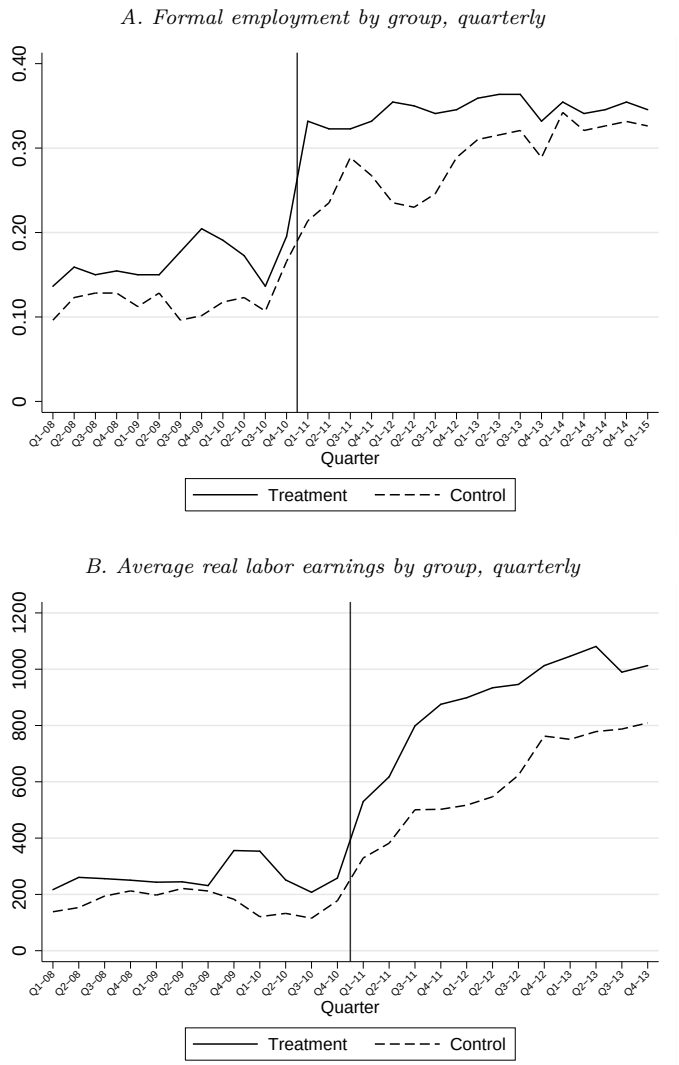
Notes: Regression controls as described in the notes to Table 2. The estimates correspond to the model in Equation 2. The "Persistence" coefficient corresponds to that of the interaction between the treatment group indicator and the outcome in the previous period, and captures the probability of continuing in employment once the individual is employed. The coefficient for "Access" corresponds to the interaction between the treatment group indicator and the transformation $1 - Y_{i,t-1}$ (which indicates whether individual $i$ was not employed in the previous period), and captures the probability of entering employment when the individual is unemployed. The coefficient on "Dependence" captures the overall degree of dependence of the current employment status on that of the previous period for individuals in both the treatment and the control groups. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA). Standard errors clustered by individuals in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Probability of staying with the same employer from one year to the following year

| | Same employer Q3-2010&Q3-2011 | Same employer Q4-2011&Q4-2012 | Same employer Q4-2011&Q4-2013 |
|---|---|---|---|
| *A. Full sample* | | | |
| Treatment group | -0.0130 | 0.0413 | 0.0425 |
| | [0.0117] | [0.0356] | [0.0306] |
| Observations | 407 | 407 | 407 |
| Control group mean | 0.0214 | 0.1176 | 0.0749 |
| *B. By gender* | | | |
| Treatment effect: Men | -0.0185 | 0.1523** | 0.0723 |
| | [0.0269] | [0.0716] | [0.0620] |
| T.E. Difference: Women/Men | 0.0086 | -0.1608** | -0.0433 |
| | [0.0304] | [0.0813] | [0.0699] |
| Women/Men difference (controls) | 0.0149 | -0.0463 | -0.0050 |
| | [0.0232] | [0.0565] | [0.0458] |
| Observations | 407 | 407 | 407 |
| Control group mean | 0.0161 | 0.1210 | 0.0726 |
| *C. Age groups 18-24/25-30* | | | |
| Treatment effect: Age 18-24 | -0.0158 | 0.0955* | 0.0716 |
| | [0.0198] | [0.0519] | [0.0435] |
| T.E. Difference: Younger/Older | 0.0025 | -0.1209* | -0.0587 |
| | [0.0237] | [0.0676] | [0.0587] |
| Younger/Older difference (controls) | -0.0145 | -0.0028 | 0.0028 |
| | [0.0227] | [0.0518] | [0.0430] |
| Observations | 407 | 407 | 407 |
| Control group mean | 0.0288 | 0.1346 | 0.0769 |

Notes: Regression controls as described in the notes to Table 2 with the exception of the regressions in panel C in which the year of birth categories were replaced with age range categories. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1: Evolution of formal employment and labor earnings for the treatment and the control groups




Notes: The vertical line indicates when the program took place. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA).

# Appendix B: Main results without controls

Table F.1: Program impact, intention to treat and treatment on the treated. No control variables

| | Average Q2-2011/Q3-2012 | Q3-2012 | Average Q4-2012/Q4-2013 | Q4-2013 | Average Q1-2014/Q1-2015 | Q1-2015 | Average All Post-Treat |
|---|---|---|---|---|---|---|---|
| *A. Formal employment* | | | | | | | |
| Treatment group - ITT | 0.0867** | 0.0949** | 0.0479 | 0.0430 | 0.0188 | 0.0193 | 0.0533 |
| | [0.0383] | [0.0450] | [0.0417] | [0.0460] | [0.0431] | [0.0471] | [0.0361] |
| Completed training - TOT | 0.1799** | 0.1970** | 0.0994 | 0.0893 | 0.0390 | 0.0400 | 0.1107 |
| | [0.0795] | [0.0935] | [0.0864] | [0.0953] | [0.0895] | [0.0977] | [0.0749] |
| Control group mean | 0.2504 | 0.2460 | 0.3048 | 0.2888 | 0.3294 | 0.3262 | 0.2921 |
| *B. Monthly earnings* | | | | | | | |
| Treatment group - ITT | 333.03*** | 322.44** | 254.12* | 203.65 | | | 298.51** |
| | [117.86] | [140.71] | [147.66] | [160.13] | | | [123.16] |
| Completed training - TOT | 691.20*** | 669.22** | 527.42* | 422.68 | - | | 619.55** |
| | [243.32] | [289.85] | [307.13] | [333.23] | | | [255.39] |
| Control group mean | 512.04 | 623.50 | 775.47 | 809.29 | - | - | 627.29 |
| Observations | 407 | 407 | 407 | 407 | 407 | 407 | 407 |

Notes: the TOT coefficients correspond to instrumental variable regressions where the completed training indicator is instrumented by the treatment assignment (lottery) variable. The regressions do not include any control variables. Earnings data not available for the Q1-2014/Q1-2015 period. Sources: *entra21* and Sistema Integrado Previsional Argentino (SIPA). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.