

Youth training programs beyond employment. Evidence from a randomized controlled trial.*

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Abstract

Youth unemployment is a pervasive phenomenon in Latin America and the Caribbean. Governments have widely used training programs in order to mitigate such problem. This paper documents the effects of a training program for low income youths, which comprises vocational training, life skills and work experience. Results show large gains in employment, with effects that remain more than two years after the intervention. The program shows also substantial effects on access to credit. Program participants exhibit a higher probability of having requested formal consumer credit, and a higher probability of having bank debts in good standing. The evidence suggests that our results are driven mainly by men and younger participants, who have higher gains in terms of outcomes, contrary to previous evidence from Latin America.

JEL: J08, J68

Keywords: random assignment, labor training programs, youth unemployment.

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1 Introduction

Youth unemployment is a pervasive phenomenon in Latin American and the Caribbean (henceforth LAC). Youth unemployment rates triple those of adults and labor informality is the rule (Gasparini et al., 2011). Young people have little work experience and insertion in the labor market is difficult (e.g. Pallais, 2013). In the past, governments and aid agencies have explored training programs as a potential solution to this problem, following the long tradition of active labor market policies carried out in OECD countries. There is a long history of training programs' evaluations, including randomized controlled trials like *National Supported Work* (NSW) and *Job Training Partnership Act* (JTPA).

Card's (Card et al., 2010) meta-study finds a modest impact at best for programs in Europe and the US. Generally, better results are found for women, and for older workers. Also, firm-based training is often more effective than classroom training. Moreover, programs that have links to work experience in the private sector tend to be more effective than public sector-based programs. The literature of evaluation of training programs in Latin America shows larger effects than in other regions. Unfortunately, such effects are sometime driven by weak methodological approaches and the quality of the data used.

As pointed out in Attanasio et al. (2011), average level of skills in developing countries is lower than in developed countries, so returns to skills may be higher. Also, specialized skills tend to be very valuable, since access to good jobs in the formal sector is generally limited to educated workers. González-Velosa et al. (2012) review the evidence available for LAC, highlighting the fact that many programs have quasi-experimental evaluations. There are few experimental evaluations available, and they show little or no effects.

In the case of Colombia the program *Jóvenes en Acción*, implemented in 2005, was evaluated in Attanasio et al. (2011), this program was aimed to unemployed young people between 18 and 25, from poor households. The training was offered randomly to men and women and it consisted of six months of vocational training. The evidence suggests that the program had positive effects in women' wages (an increase of 19.8%), and is more likely to have a job in the formal sector or one with written contract, for men the effects of the program on the quality of employment are small.

In the Dominican Republic exists experimental evaluations of the program *Juventud y Empleo* (cohort 2004). This program was aimed to young people with less than secondary education, between 18 and 29 years. It's main goal was to increase the employability of vulnerable young through basic training and

technical/vocational courses. The evaluation of the program was conducted in Card et al. (2011),¹ the results indicate no significant impact on employment, a modest impact on wages, conditional on having a job, which is a result coincident with those obtained in developed countries.

This paper attempts to provide more experimental evidence for job training programs in LAC for disadvantaged young, evaluating *entra21* a program carried in Córdoba, Argentina, but administered in several LAC countries. *entra21* differs from the above mentioned training programs in the sense that it is smaller, usually run by NGOs (usually business associations) and, while governments can participate in the implementation, it is not a country-wide program. Also, the cost of the program tends to be higher than the cost of government programs. Finally, the private sector has a higher involvement with the program.

Most existing literature looks at employment and wages as expected outcomes of training programs. However, we explore an additional link that goes from employment to credit access. Credit access is a also relevant outcome, since it helps to reduce risk, which is a central feature in the life of vulnerable groups. If *entra21* had a positive impact on employment and employment increases access to credit, then the effects of training go beyond the ones currently explored by the literature. The expected outcomes of this program are in line with all training programs, i.e. an increase in the probability of employment, but also we are interested in exploit this experiment beyond its stated purposes, we estimate the impact of the program on access to credit and welfare dependency.

The contributions from this paper are threefold. Firstly, it provides more evidence on the impact of youth training program in developing countries, which adds to the literature of active labour market policies (ALMP) in general. Secondly, we have a small sample size, but we use administrative records that imply large periods and precision in measurement. This is not commonly used in developing countries. Finally, we exploit experiment random assignment beyond its stated purposes exploring other related outcomes such as welfare dependency for women and access to credit. For the specific case of acquiring training, credit constraints would reduce demand for training, and thus motivate interventions. Most studies looking at access to credit have focused on the credit employment channel, via entrepreneurship and micro-finance. Here, the channel seems to be through more stable employment, specially for the younger group. However, we cannot rule out other channels through which the program may have an impact on credit.

This paper is organized as follows: Section 2 describes the program, Section 3 describes the random

¹Also see Ibararán et al. (2012).

assignment process and the data used. Section 4 presents the results, Section 5 shows a cost-benefit analysis of the program and finally the Section 6 concludes.

2 Program Description

entra21 is a program that targets specifically vulnerable, unemployed youths who have finished high school. *entra21* differs from the traditional training programs implemented in LAC countries in the sense that it is smaller, usually run by NGOs (mainly business associations) and, while governments can participate in the implementation, it is not a country-wide program. Furthermore, the number of training hours is greater than those of most training programs in LAC. Finally, one of the hallmarks of this type of program is the private sector's very active involvement in its various project components. Also, program administrators highlight that *entra21* increases the probability of finding "good quality" jobs in the formal sector, helping to reduce informality, which is very high among young and vulnerable individuals. The main disadvantage is the relative high cost *vis à vis* other training programs.

One of the *entra21* projects was conducted in Córdoba (Argentina) and the agencies in charge of the implementation were ADEC² and the *Secretaría de Desarrollo Social* (SDS). ADEC and the municipal and provincial governments of Córdoba worked alongside civil society organizations to implement Phase II of *entra21* program in the province. The program was financed by the Multilateral Investment Fund in the US. An experimental impact assessment strategy was designed to measure the ways in which the training influenced a number of different socioeconomic variables.³

The criteria for becoming eligible was set by the SDS. In order to become eligible, individuals must be unemployed within the ages 18-30, have a high school diploma and a total family income below the poverty line. The population eligible for the cohort comprised 407 young people. They were selected after a personal visit of the municipality/secretariat, which administered a survey (baseline). SDS gathered household socio-economic information for each participant. A total of 220 would be randomly assigned to treatment in a public lottery. Only the first cohort that participated from the program was assigned randomly and the individuals assigned to the control group could not participate of further calls.

Program administrators highlighted the fact that *entra21* was different from most traditional ALMP for the youth. First of all, the private sector was involved in the implementation. Secondly, the training had several components. It started with classroom training in an "*oficio*" (skill) which resulted from actual

²Agencia para el Desarrollo de Córdoba, www.adec.org

³The program finished in 2012.

firms' demand. It also had some hours of training ICTs and life skills. Finally, the participants would do an internship to acquire on the job skills. Courses in different "oficios" were offered.⁴ Courses lasted for 884 hours divided in: 100 hours of technical in-class training, 64 hours of life skills training, 704 hours of internship and 16 extra hours which varied from basic skills to extra in-class technical training according to each type of course. Classroom training took place between mid-November and February 2011, and then were followed by an internship.

3 Data Description

This section explains how the random assignment was done, then describes the baseline information that was used in order to characterize individuals pre treatment. To estimate the impact of the program we use administrative records, because of in the Southern hemisphere December (when the follow up questionnaire was gathered) coincides with the start of the summer holidays and the capacity of SDS was limited and they collected little information.

3.1 Random Assignment

entra21 had an evaluation component that was conceived since the inception of the intervention. During its initial phase, the original eligible cohorts would be randomized into treatment and control groups in order to conduct a proper impact evaluation. All the participants entered a lottery and only the lottery winners would participate from the program. The 220 lottery winners and the 187 losers would be informed of the results⁵ and training would start soon after that.

The lottery took place on November 9th, 2010. All the process was conducted in a very "transparent" manner, with public officials and a notary present. Eligible individuals who participated from the lottery were aware of the assignment into treatment mechanism and all agreed. We define as pre treatment period before third quarter 2010 (Q3-2010) and post treatment since Q2-2011 to Q4-2013.

Out of a total of 220 assigned to treatment, only 178 participated in the program. The 42 remaining either declined participation at the time of the beginning of the training or was not reachable by phone.

3.2 Baseline

Baseline, pre treatment information can be obtained from the program's application form, which was

⁴Courses were offered in the following fields: cookery, sales and administration and factory workers ("operarios").

⁵Potential beneficiaries could all be present in the lottery. However, none of them was present at the time of the lottery.

administered by the SDS. While the baseline was being collected, participants gave consent to tracking. Table 1⁶ shows descriptive statistics and balance between treatment and control groups. In Panel A we observe all individual characteristics are balanced between treatment and control groups, which is an expected result in a successful randomization. As it can be observed, around one third of the program participants are male, average age is 23, most of them are single (approximately 70%) and 70% have complete high school education or less.

3.3 Administrative Data

In order to measure the impact of the program on the expected outcomes, we resort to three different sources of information. For employment information we have administrative data. Such data consists of monthly registered employment (from January 2003 to November 2013) and gross labor earnings (from January 2003 to November 2013) from SIPA (Sistema Integrado Previsional Argentino). Welfare participation, measured by being a beneficiary of *Asignación Universal por Hijo* (AUH), the Argentine Conditional Cash Transfer is available from ANSES (Administración Nacional de Seguridad Social). Credit in good/bad standing status is registered in the Central Bank of Argentina (Central de Deudores del Banco Central de la República Argentina) and the considered source is NOSIS, a credit rating agency. Credit inquiries were obtained from EQUIFAX (VERAZ).

In Table 1 (Panel B) when pre-treatment outcomes are analyzed, we looked at employment at the 11 quarters before the program started. For 8 out of 11 quarters the means of the treatment and control group are not statistically different. For the last 2 quarters of 2009 and the first quarter of 2010 employment is higher in the treatment group. In terms of receiving welfare programs, there is between 10 and 15% of recipients of *AUH* in both groups, there is no difference between the two groups either. To observe credit use we define two measures: if individuals have credit in good standing (regular credit) and the number of credit inquiries that companies (financials or not) request to a rating agency to know the credit situation when an individual wants a credit. For these variables there are no differences statistically significant pre-treatment. Finally, we take into account some aggregated variables of real wages (with January 2011 as the base month). As can be seen there is some evidence of significant differences, but barely at 10%.

In terms of power calculations, such small sample would only allow us to detect effects above 8 percentage points in employment, which amounts to an effect size of 0.30.

⁶Appendix A explains how the variables were constructed. Appendix B shows tables.

4 Estimated Effects

We conducted several estimations to look at a comprehensive set of outcomes. First, we estimated OLS regressions to obtain the estimator *Intention to Treat* (ITT) with and without covariates. Also, we calculated the estimator *Treatment on the Treated* (TOT) for the main outcomes using the offer of training as instrument for training⁷ (Angrist et al., 1996; Kling et al., 2007).

The estimated equation to obtain the ITT is the following:

$$Y_i = \alpha + \beta Z_i + \delta X_i + \varepsilon_i$$

Where Y_i indicates an outcome (employment, earnings, credit status) for each observation i , α is the constant, β is the parameter of interest because indicates the effect of Z_i , an indicator for those individuals that have been offered the program (value 1 in that case). X_i is a vector of pre treatment characteristics and includes demographic data (such as age, sex, educational achievement, marital status) and pre treatment achievement of the dependant variable Y_i . Finally, ε_i is an error term.

To estimate the estimator TOT we use the following specification:

$$Y_i = \alpha + \gamma D_i + \delta X_i + \varepsilon_i$$

Here the variable D_i is an indicator of compliance, completed training in this case, and we use the offering of participate Z_i as an instrument for that endogenous variable. We are interested in estimate the effect of the program on the treated not a local average treatment effect. The program was not offered to the control group. The principal assumption of this estimation is there is not average effect of being offering to participate in the program for those who did not participate in the program (Kling et al., 2007).

Also we estimate a dynamic model only for employment because one goal of entra21 was increase the employability of participants. We try to estimate this following Card et al. (2011). The employability is capture by a treatment effect if a person was employed in previous period and a treatment effect if not employed in previous period.

$$Y_{it} = \alpha + \beta Treatment * Y_{i,t-1} + \rho Treatment * (1 - Y_{i,t-1}) + \theta Y_{i,t-1} + \phi_t + \varepsilon_{it}$$

Treatment indicates *Treatment group*, our relevant variable is employment (Y_{it}), the interaction between treatment and if i was employed in the previous period indicates *Persistence*, the interaction between

⁷Random assignment as IV for training completion; n=106 out of 220, p=0.481.

treatment and a variable that indicates i was not employed in the previous period indicates *Access*. We also include a indicator of *Dependence* of previous period and dummies for each considered months.

4.1 Employment and Earnings

The main limitation of the administrative data available for employment is that it only registers formal employment. In this sense, results are not directly comparable to other evaluations in LAC. However, the main objective of the program was increase formal employment. Also, formal employment will be the most likely source of credit market effects.

In Figure 1, Panel A, we consider only the evolution of employment rates by random assignment, from Q1-2008 to Q4-2013, it can be observed there was an increase in employment rates for both groups at the end of 2010. To calculate the effect of the program for each quarter the ITT estimator was estimated without covariates, the results are shown in Panel B, in this graph there are three quarters statistically significant (as can be seen in Table 1) although the allocation to each group was done randomly.

Table 2 presents the impact of the program on the main outcomes of interest but in aggregated outcomes post treatment. We conducted two estimations, intention to treat (ITT) and treatment on the treated (TOT), instrumenting completed training with random assignment. In this tables we look four outcomes: *formal employment, real earnings, and credit variables: credit in good standing and number of credit inquiries*, and for each of them our interest is analyze the impact of the program through the time. Each panel contains ITT and TOT estimates for each outcome. The columns in the table indicate the reference period, the first one include what we call “short run” and the variable indicates the average of quarters from Q2-2011 to Q3-2012 of the relevant variable en each panel. The second column only represent the variable of interest at the end point of the short term period in our definition, that means the relevant variable in Q3-2012. The third column indicates the “medium run”, this represent the average of quarters from Q4-2012 to Q4-2013 of each outcome variable. Also, we include a column with the quarter Q4-2013. Finally, we include a global measure that represents the overall impact of the program in the post treatment period (Q2-2011 to Q4-2013).

For formal employment (Table 2, Panel A) we observe positive and statistically significant program effects, in all the considered periods the impact is greater for TOT estimates, almost twice as much. In particular there are statistically significant differences in the short term, in the quarter Q3-2012 and in all post treatment period under analysis. This finding is particularly relevant because it seems that the effects

caused by the program tend to fade over time. In the short term there is a positive impact on formal employment of 8 percentage points (statistically significant at 5 percent). Given that the control group has an employment rate of 27 percent, our estimate is equivalent to a 30 percent increase in the formal employment. If we consider only the last quarter of the short-term, we note that the magnitude is greater than the average of all first quarters, reaching an effect of 10 percentage points. The average of quarters of our definition of medium-term and the last quarter of this period, have positive coefficients of the ITT estimator, but not statistically significant, consistent with our hypothesis of dilution of effects as time goes by. Finally we analyze the overall impact of the program on post treatment formal employment, we find positive and statistically significant effects, the magnitude of impact is 7 percentage points (significant only at 10 percent), 1 percentage point lower than in the short-run period.

Also we are interested in the heterogeneous impact (Djebbari and Smith, 2008) of the program in formal employment, because the evidence from previous evaluation indicates disparities between gender and age groups. We considered formal employment and the results are in Table 3. Table 3 shows ITT estimates of impact on employment by gender and age groups using interactions terms for different periods of time. As it can be appraised in Panel A the interaction term treatment group and men is statistically significant. The difference between treated men and women in the control group is 26.86 percentage points in the short run (statistically significant at 1 percent), in the medium run the difference between this groups is 29.191 (statistically significant at percent) and 21.48 percentage points in all the post treatment period. In Panel B we show differential effects by age groups, in the short run the difference between treated younger participants (18 to 24 year-olds) and participants aged 25 and 30 in the control group is 15.39 percentage points and statistically significant at 1 percent, in the medium run the difference is 9.57 but not statistically significant and finally in all post treatment period the difference between these groups is 12.74 percentage points (statistically significant at 5%).

Table 4 shows the results of estimating the impact of random assignment in alternative measures of employment and considering different periods. Panel A includes a measure of employment indicating whether the individual was employed in any month of each period, Panel B indicates whether in each period the participant has entered to a formal employment and Panel C indicates whether has left an employment. Positive and statistically significant effects were found on the variables that indicates if the individual worked in both the short term and medium term, and the all post treatment period with magnitudes of 11, 8 and 10 percentage point, respectively. Coefficients of Treatment on the Treated estimator are greater than ITT estimates.

Table 5 shows estimates of employability in the short run, medium run and in all post treatment period (monthly). In Panel A we observe strong dependence from previous periods and positive and significant effects of *Persistence* in the short run. Panel C includes only men, we observe positive and significant effects in *Persistence* in the short run and in all post treatment period. In Panel D the estimates only include younger individuals between 18 and 24 year-olds, there are positive and significant effects in *Persistence* only in the short run.

Another important result are wages, to analyze the impact of the program on real earnings (with January 2011 as the base month) Figure 2, Panel A shows the evolution of earnings from Q1-2008 to Q4-2013 by random assignment, while Panel B shows the differences between groups, showing differences statistically significant for post treatment quarters.

Panel B, in Table 2, reports ITT estimates for the results on average earnings (by quarter) differences between the treatment and control groups, including the zeros. We observed significant increases in post treatment earnings in the short run and in all post treatment period. This results hold if we consider two additional measures of earnings, in Table 6, Panel A includes the series of earnings with extreme values bounded at the percentile 99 of the earnings in the control group and Panel B include a logarithm transformation of quarterly average earnings, $\log\left(y + (y^2 + 1)^{\frac{1}{2}}\right)$ (Hirshleifer et al., 2014).

Increase in earnings can be caused by increase in employment or by increase in earnings of those employed. Total impact is a combination of productivity gains and changes in employment composition. We calculate bounds for earnings because wages zero are included, this was done in other studies such as Lee (2008), Blanco et al. (2011) and Flores and Chen (2012). Here we follow Attanasio et al. (2011) to estimate the program's impact on productivity.⁸ They divide the sample of individuals in four groups: those who work regardless of the program (or always takers in terms of Angrist and Imbens, 1994), those who would never work, those who are switching to work because of the program (the compliers) and those switching out of work because of the program. Randomization ensures that the size of each group is independent of the assignment to treatment. In first place the monotonicity assumption is used, indicating that individuals who would work without the program would work if they did the training, this allows to decompose the effect of the program on the earnings of compliers plus the always takers, and estimate the productivity gain of the program and the change in composition. Table 7, Panel A shows that *Average* earnings did increase, but we cannot conclude whether it is due to productivity gains or change in employment. The bounds are estimated by estimating the productivity effects and the distribution of

⁸The Appendix B of Attanasio et al. (2011) presents a derivation of such bounds.

wages in the control group (Panel C). The calculated bounds for the average wages are big and the lower bound is negative. But an additional assumption is considered to narrow this lower limit, nonprogram earnings of those who always work are at least as high as the nonprogram earnings of individuals who are no longer unemployed. The bounds with this assumption are limited now between AR\$215 and AR\$791.

4.2 Credit in good standing and number of credit inquiries

In this literature, credit constraints would reduce demand for training and thus motivate interventions. Studies looking at credit use have focused on the credit-employment channel (entrepreneurship, etc.). The relevance of the credit use as an outcome in this study is related to evidence for LAC that finds effect of training programs on formal employment. If we see a simple model of credit, higher-less volatile-more verifiable earnings in $t=0$ would increase borrowing in $t=1$ even in the absence of credit constraints. In this paper we are also interested in the effects of the program on credit use, for this reason we use two measures that indicates credit use or access to credit:

- credit in good standing: equals 1 if individual has banking credit in good standing, 0 otherwise (bad or no credit).
- credit inquiries: number of credit inquiries to credit rating agency. These inquiries appears when an individual requests some form of banking or consumer credit and the institution requests information about her.

In Figure 3 and 4, Panel A, we consider only the evolution of credit in good standing and number of credit inquiries by random assignment, respectively, from Q1-2008 to Q4-2013. To calculate the effect of the program for each quarter the ITT estimator was estimated for each credit variable, the results are shown in Panel B.

In this section, the interest is to estimate the direct effect of the training offer (ITT), then the effect of employment on credit and finally the effect of earnings on credit. In first place Table 2 Panel C shows the results for credit in good standing post treatment, considering average of quarters in the short, medium term and in all post treatment period. We observe the direct impact of training offer and only there are significant and positive effects in the short term period (5 percentage points). When we use the random assignment as an instrument for *Completed training*, we observe the same result as before, but

the magnitude is almost twice.

In Table 8 for ITT estimations of *credit in good standing* we considered differential effects according gender and age group (using interactions). We observed positive and barely significant effects for treated men with respect to women in the control group in all post treatment period (7.84 percentage points, statistically significant at 10 percent). There are not statistically significant differences between treated younger participants and older participants (aged 25 to 30 year olds).

In Table 2, Panel D we can see the *number of credit inquiries* post treatment (average of quarters) in the short, medium run and in all post treatment period. When we analyze the impact of random assignment we note impacts only in the short run, the coefficient is positive and statistically significant (8 percentage points). TOT estimates are significant too but magnitude is greater.

For this variable, in the second column of Table 8 we also consider heterogeneous impact by estimating ITT and as can be seen the evidence suggests that there are positive effects between treated men and women in the control group (7.07 percentage points) but not statistically significant. With respect to the age group we observed positive differences between the treated younger and the older participants in the control group, but there is not statistically significance.

4.3 Welfare Dependency

Another output of interest is the effect of training on welfare dependency in particular for women, measured by receiving Asignación Universal por Hijo (AUH) benefits. We construct a dummy that takes value 1 if a women is always beneficiary of the AUH, in each considered period, the results are shown in Table 9. In all post treatment period 11 percent of control group received AUH and if we estimate the differential effect between the treated younger women who receive AUH and older women in the control group (aged 25 to 30) the estimate in all post treatment period indicates that the training has a deterrent effect on participation in such benefits, but this difference is not statistically significant.

5 Cost Benefit analysis

One difference between *entra21* and other training program in LAC is its cost. *entra21* is significantly more expensive than programs targeting the same beneficiary groups. The implementing agency states that the quality of the training provided in *entra21* is superior to that of those traditional government

run programs and this justifies its higher costs. Roughly the cost of operation per trainee was US\$ 1722 (more than double the Colombian and the Dominican Republic program with costs of US\$750 and US\$330 respectively). The last column of Table 6 shows an average quarterly gain (due to employment and earnings gains) of AR\$ 312.78, or AR\$ 1251.12 per year. Using a 5% discount rate with no depreciation, we obtain an internal rate of return of 22.31%.

6 Conclusions

Youth unemployment is a pervasive phenomenon in Latin American and the Caribbean and training programs are considered by governments and aid agencies as a potential solution but in the region there is limited causal evidence to conclude about their effectiveness.

This paper is a contribution to the evaluation of training policies in LAC, documenting the effects of a training program for low income youths named *entra21* in the province of Córdoba in Argentina. *entra21* provided a combination of vocational training, life skills training and work experience. Assignment to the program was random, and thus allowing us to obtain causal estimates of training offer on the outcomes of interest. One of the main challenges of the evaluation is the small sample size, resulting from the fact that the program was small too. We made use of administrative data, available before and after the treatment.

We find positive and statistically significant effects on employment, which persist up to two years after the program. Besides these labor market impacts, the evidence also indicates a small reduction in welfare participation for younger women. When we look at gains in earnings, there are positive and significant effects too caused both by productivity gains and increase in employment. Bounds were estimated in order to give some idea of the productivity gain. However, such bounds do include the zero gains, so we cannot conclude that the gain was caused by a productivity increase. Also, there are effects on credit, credit in good standing and credit inquires. For the specific case of acquiring training, credit constraints would reduce demand for training, and thus motivate interventions. Most studies looking at access to credit have focused on the credit employment channel, via entrepreneurship and micro-finance. Here, we conjecture that the channel seems to be through more stable employment, specially for the younger group.

Besides the mentioned before is interesting to note that gains in outcomes analyzed when we split the sample are significant for men and for younger individuals (18-24 versus 25-30), which is a result contrary to previous evidence from Latin America.

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Appendix A

Definition of Variables

Random assignment: indicates the random assignment results to participate in the training (takes value 1 for the treatment group, 0 for the control group).

Completed training: dummy that indicates if the training is complete.

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

-*Average of Q2-2011 to Q3-2012:* average of quarters of the formal employment variable, short term.

-*Average of Q4-2012 to Q4-2013:* average of quarters of the formal employment variable, medium term.

-*Average of Q2-2011 to Q4-2013:* average of quarters of the formal employment variable, all post treatment period.

Earnings: Average of earnings by quarter.

-*Average of Q2-2011 to Q3-2012:* average of quarters of earnings variable, short term.

-*Average of Q4-2012 to Q4-2013:* average of quarters of earnings variable, medium term.

-*Average of Q2-2011 to Q4-2013:* average of quarters of earnings variable, all post treatment period.

Credit in good standing: dummy by quarter that takes value 1 in case that the individual credit status is in good standing (value 1 or 2, according to the classification of Central Bank of Argentina BCRA), 0 in other case.

-*Average of Q2-2011 to Q3-2012:* average of quarters of credit in good standing variable, short term.

-*Average of Q4-2012 to Q4-2013:* average of quarters of credit in good standing, medium term.

-*Average of Q2-2011 to Q4-2013:* average of quarters of credit in good standing, all post treatment period.

Number of credit inquiries: variable that indicates the number of EQUIFAX (VERAZ) reports that were required (by financial sector, non-financial and others) to find out the individual credit situation.

-*Average of Q2-2011 to Q3-2012:* average of quarters of number of credit inquiries variable, short term.

-*Average of Q4-2012 to Q4-2013:* average of quarters of number of credit inquiries, medium term.

-*Average of Q2-2011 to Q4-2013:* average of quarters of number of credit inquiries, all post treatment period.

Asignación Universal por Hijo-AUH: dummy by quarter that takes value 1 if a woman perceived social assistance.

- *Always Q2-2011 to Q3-2012:* dummy that takes value 1 if a woman perceives the AUH during the short term.

- *Always Q4-2012 to Q4-2013:* dummy that takes value 1 if a woman perceives the AUH during the medium term.

- *Always Q2-2011 to Q4-2013:* dummy that takes value 1 if a woman perceives the AUH during all the considered period.

Tables

Table 1: Pre treatment summary statistics: Individual characteristics and pre treatment outcomes.

Variables	Treatment		Control		Difference		
	Mean (1)	SE	Mean (2)	SE	(1)-(2)	SE	P-value
<i>1) Baseline survey data</i>							
Male	0.29	0.03	0.34	0.04	-0.04	0.05	0.37
Age	23.55	0.24	23.80	0.26	-0.25	0.35	0.48
Incomplete elementary school	0.04	0.01	0.03	0.01	0.01	0.02	0.58
Complete elementary school	0.08	0.02	0.05	0.02	0.02	0.02	0.34
Incomplete high school	0.28	0.03	0.33	0.03	-0.05	0.05	0.28
Complete high school	0.33	0.03	0.33	0.04	-0.00	0.05	0.93
Incompl tertiary level/college	0.18	0.03	0.16	0.03	0.02	0.04	0.65
Complete tertiary level/college	0.07	0.02	0.06	0.02	0.01	0.02	0.70
Children in the household	0.19	0.03	0.25	0.03	-0.06	0.04	0.18
Single/Widower	0.69	0.03	0.67	0.04	0.03	0.05	0.56
Married/Cohabiting	0.23	0.03	0.27	0.03	-0.04	0.04	0.34
Divorced/Separated	0.04	0.01	0.04	0.01	0.01	0.02	0.69
<i>2) Administrative data</i>							
Avg employment Q1 2003-Q4 2007	0.07	0.01	0.05	0.01	0.02	0.02	0.13
Avg employment Q1 2008-Q3 2010	0.16	0.02	0.12	0.02	0.05	0.03	0.07*
Avg earnings Q1 2003-Q4 2007	1,463.96	88.44	1,351.15	106.36	112.81	140.32	0.42
Avg earnings Q1 2008-Q3 2010	1,599.48	83.75	1,513.60	105.94	85.88	134.37	0.52
Always AUH Q1 2010-Q3 2010	0.10	0.02	0.10	0.02	0.00	0.03	0.93
<i>3) Credit agencies data</i>							
Avg credit good Q2 2010-Q3 2010	0.09	0.02	0.06	0.02	0.03	0.03	0.31
Avg inquiries Q1 2008-Q3 2010	0.11	0.01	0.12	0.01	-0.01	0.02	0.73

Notes: The total number of observations in the Treatment group is 220 and 187 in the Control group. Panel 2) *Avg employment* average of quarters of formal employment. *Avg earnings* indicates the mean of quarterly real salaries with January 2011 as the base month and we adjust for the thirteenth month pay. AUH means Asignación Universal por Hijo, dummy that indicates if a woman perceived social assistance. *Always AUH* dummy that takes value 1 if a woman perceives the AUH during all the considered period. Panel 3) Credit in good standing, dummy by quarter that takes value 1 when individual credit status is in good standing. *Avg credit good* means average of quarters of the Regular Credit variable. Credit inquiries indicates the number of EQUIFAX (VERAZ) reports that were required to find out the individual credit situation. *Avg inquiries* is the mean of quarters of Number of Credit Inquiries variable. Sources: entra21, Certificación Negativa-ANSES, Ministerio de Trabajo de la Nación, NOSIS and EQUIFAX (VERAZ).

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: Impact of random assignment. Post treatment outcomes.

	Average Q2-2011 to Q3-2012	Q3-2012	Average Q4-2012 to Q4-2013	Q4-2013	Average Q2-2011 to Q4-2013
<i>A. Formal employment</i>					
Treatment group - ITT	0.0796** [0.0371]	0.1020** [0.0465]	0.0434 [0.0403]	0.0382 [0.0450]	0.0691* [0.0371]
Completed training - TOT	0.1641** [0.0762]	0.2104** [0.0962]	0.0895 [0.0831]	0.0787 [0.0925]	0.1433* [0.0769]
Control group mean	0.2504	0.2460	0.3048	0.2888	0.2752
<i>B. Earnings</i>					
Treatment group - ITT	384.15*** [143.20]	357.15** [158.20]	234.81 [163.06]	124.24 [160.33]	311.30** [147.33]
Completed training - TOT	796.71*** [298.36]	740.72** [327.97]	486.99 [336.87]	257.68 [332.60]	646.09** [306.17]
Control group mean	837.13	678.96	1025.29	818.52	1136.95
<i>C. Credit in good standing</i>					
Treatment group - ITT	0.0455** [0.0227]	0.0674* [0.0348]	0.0247 [0.0315]	0.0350 [0.0358]	0.0387 [0.0251]
Completed training - TOT	0.0937** [0.0469]	0.1389* [0.0714]	0.0508 [0.0646]	0.0722 [0.0736]	0.0803 [0.0521]
Control group mean	0.0882	0.1070	0.1326	0.1230	0.1084
<i>D. Credit inquiries</i>					
Treatment group - ITT	0.0776** [0.0300]	0.0006 [0.0663]	-0.0212 [0.0359]	-0.0062 [0.0797]	0.0295 [0.0255]
Completed training - TOT	0.1582*** [0.0601]	0.0012 [0.1351]	-0.0432 [0.0734]	-0.0126 [0.1625]	0.0611 [0.0528]
Control group mean	0.1881	0.2513	0.2877	0.3957	0.2333
<i>Observations</i>	407	407	407	407	407

Notes: IV-Completed training instrumented by treatment group. Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. All panels also include pre treatment formal employment Q1-2008 to Q3-2010. Panel B, C and D also include measures of pre treatment earnings, credit in good standing and number of credit inquiries (quarterly), respectively. Robust standard errors in brackets. Sources: entra21, Certificación Negativa-ANSES, EQUIFAX (VERAZ), NOSIS and Ministerio de Trabajo de la Nación.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Effects on formal employment ITT (OLS). Heterogeneous impact.

	Average Q2-2011 to Q3-2012	Q3-2012	Average Q4-2012 to Q4-2013	Q4-2013	Average Q2-2011 to Q4-2013
<i>Panel A. Interaction Men</i>					
Treatment group*Men	0.2392*** [0.0822]	0.2793*** [0.1010]	0.2258*** [0.0870]	0.2450** [0.1026]	0.2331*** [0.0772]
Treatment group	0.0054 [0.0411]	0.0154 [0.0521]	-0.0266 [0.0456]	-0.0378 [0.0486]	-0.0092 [0.0396]
Men	0.0240 [0.0601]	0.0182 [0.0729]	0.0927 [0.0665]	0.0756 [0.0760]	0.0552 [0.0576]
Observations	407	407	407	407	407
Control group mean	0.2137	0.2177	0.2500	0.2500	0.2302
<i>Panel B. Interaction Age 18-24</i>					
Treatment group*Age 18-24	0.0889 [0.0709]	0.0973 [0.0873]	0.1202 [0.0792]	0.1026 [0.0878]	0.1031 [0.0689]
Treatment group	0.0299 [0.0519]	0.0442 [0.0629]	-0.0183 [0.0578]	-0.0124 [0.0632]	0.0080 [0.0507]
Age 18-24	0.0351 [0.0547]	-0.0091 [0.0690]	-0.0062 [0.0621]	-0.0318 [0.0692]	0.0163 [0.0539]
Observations	407	407	407	407	407
Control group mean	0.1968	0.2169	0.2627	0.2410	0.2267

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010. In Panel B we exclude year of birth (dummies) and we generate *age 18-24*, a dummy that takes value 1 when individual is between 18 to 24 year-olds. Robust standard errors in brackets. Sources: entra21, Certificación Negativa-ANSES and Ministerio de Trabajo de la Nación.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Ever formally employed, ever entered and ever left a formal employment (post treatment).

	Q2-2011 to Q3-2012	Q4-2012 to Q4-2013	Q2-2011 to Q4-2013
<i>Panel A. Ever formally employed</i>			
ITT	0.1134** [0.0487]	0.0793* [0.0473]	0.0983** [0.0471]
TOT	0.2340** [0.1007]	0.1635* [0.0971]	0.2027** [0.0971]
Control group mean	0.4011	0.4011	0.5080
<i>Panel B. Ever entered formal employment</i>			
ITT	0.0422 [0.0466]	0.0043 [0.0417]	0.0239 [0.0487]
TOT	0.0871 [0.0962]	0.0088 [0.0860]	0.0494 [0.1005]
Control group mean	0.2727	0.2193	0.4171
<i>Panel C. Ever left formal employment</i>			
ITT	0.0069 [0.0440]	0.0483 [0.0409]	0.0332 [0.0484]
TOT	0.0143 [0.0908]	0.0996 [0.0839]	0.0684 [0.0996]
Control group mean	0.2567	0.1711	0.3636
<i>Observations</i>	407	407	407

Notes: TOT: IV, Completed training instrumented by treatment group. Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010. Robust standard errors in brackets. Sources: entra21 and Ministerio de Trabajo de la Nación.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Dynamic regressions for Employment.

	Formal employment 04-2011 to 09-2012	Formal employment 10-2012 to 11-2013	Formal employment 04-2011 to 11-2013
<i>Panel A. All sample</i>			
Persistence	0.0346* [0.0183]	-0.0165 [0.0139]	0.0088 [0.0129]
Access	0.0014 [0.0062]	0.0079 [0.0062]	0.0038 [0.0049]
Dependence	0.8522*** [0.0176]	0.9170*** [0.0118]	0.8846*** [0.0112]
Observations	7,326	5,698	13,024
Control group mean	0.2080	0.2735	0.2366
<i>Panel B. Women</i>			
Persistence	-0.0139 [0.0216]	-0.0362* [0.0189]	-0.0246 [0.0162]
Access	-0.0007 [0.0061]	0.0074 [0.0058]	0.0026 [0.0047]
Dependence	0.8966*** [0.0191]	0.9354*** [0.0127]	0.9163*** [0.0126]
Observations	5,022	3,906	8,928
Control group mean	0.1873	0.2321	0.2069
<i>Panel C. Men</i>			
Persistence	0.0996*** [0.0291]	0.0074 [0.0219]	0.0522** [0.0205]
Access	0.0172 [0.0189]	0.0267 [0.0201]	0.0192 [0.0156]
Dependence	0.7690*** [0.0298]	0.8771*** [0.0232]	0.8251*** [0.0193]
Observations	2,304	1,792	4,096
Control group mean	0.2487	0.3549	0.2951
<i>Panel D. Age 18-24</i>			
Persistence	0.0560** [0.0246]	-0.0029 [0.0172]	0.0263 [0.0167]
Access	0.0042 [0.0096]	0.0107 [0.0094]	0.0069 [0.0075]
Dependence	0.8302*** [0.0239]	0.9045*** [0.0168]	0.8675*** [0.0149]
Observations	4,158	3,234	7,392
Control group mean	0.2372	0.2974	0.2635
<i>Panel E. Age 25-30</i>			
Persistence	-0.0024 [0.0254]	-0.0377 [0.0264]	-0.0202 [0.0214]
Access	-0.0011 [0.0080]	0.0007 [0.0081]	-0.0008 [0.0064]
Dependence	0.8856*** [0.0237]	0.9268*** [0.0172]	0.9076*** [0.0165]
Observations	3,168	2,464	5,632
Control group mean	0.1714	0.2435	0.2029

Notes: Covariates included are male, year of birth (dummies) (according to each panel) and average pre treatment formal employment Q1-2008 to Q3-2010 (monthly). Robust standard errors in brackets. Sources: entra21 and Ministerio de Trabajo de la Nación.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Effects on Earnings (no outliers and transformed earnings).

	Average between Q2-2011 to Q3-2012	Q3-2012	Average between Q4-2012 to Q4-2013	Q4-2013	Average between Q2-2011 to Q4-2013
<i>Panel A. No outliers earnings</i>					
Random assignment - ITT	377.11*** [142.31]	350.43** [157.10]	233.50 [160.98]	131.80 [158.73]	252.46* [145.40]
Control group mean	837.01	678.67	1024.33	817.06	1136.45
<i>Panel B. Transformed earnings</i>					
Random assignment - ITT	0.9373** [0.4191]	0.7737* [0.4116]	0.5782 [0.4192]	0.2325 [0.3988]	0.7582* [0.4043]
Control group mean	3.2939	2.0953	3.3635	2.4565	4.1950
<i>Observations</i>	407	407	407	407	407

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment and earnings Q1-2008 to Q3-2010. Robust standard errors in brackets. Sources: entra21 and Ministerio de Trabajo de la Nación.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Earnings bounds (post treatment).

	Q2 2011	Q3 2012	Q4 2013	Sum/1000	Average
<i>Panel A ITT Average Wage</i>					
Random assignment	207.80*	378.62**	221.56	28.41**	312.78**
SE	111.84	151.07	158.41	11.82	140.85
Control group mean	452.22	678.96	818.52	59.20	1,136.95
<i>Panel B ITT Employment</i>					
Random assignment	0.07*	0.10**	0.04	0.08*	0.08*
SE	0.04	0.05	0.04	0.05	0.05
Control group mean	0.24	0.25	0.29	0.53	0.53
<i>Panel C Wage bounds</i>					
Lower Bound (Only monotonicity)	-860.65	-915.23	-265.96	-21.64	-361.20
Lower Bound (Attanasio et al)	217.19	279.08	346.77	30.95	215.03
Upper Bound	1,295.03	1,473.39	959.50	83.54	791.25

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010. Robust standard errors in brackets. Sources: entra21 and Ministerio de Trabajo de la Nación.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Effects on Credit, Q2-2011 to Q4-2013 ITT(OLS). Heterogeneous impact.

	Credit in good standing Average Q2-2011 to Q4-2013	Number credit inquiries Average Q2-2011 to Q4-2013
<i>Panel A. All sample</i>		
Treatment group	0.0387 [0.0251]	0.0295 [0.0255]
Observations	407	407
Control group mean	0.1084	0.2333
<i>Panel B. Interaction Men</i>		
Treatment group*Men	0.1568*** [0.0571]	0.1962*** [0.0558]
Treatment group	-0.0102 [0.0291]	-0.0352 [0.0322]
Men	-0.0681* [0.0354]	-0.0903** [0.0429]
Observations	407	407
Control group mean	0.1188	0.2566
<i>Panel C. Interaction Age 18-24</i>		
Treatment group*Age 18-24	0.0672 [0.0516]	0.1492*** [0.0529]
Treatment group	-0.0001 [0.0416]	-0.0576 [0.0392]
Age 18-24	-0.0606* [0.0354]	-0.0493 [0.0428]
Observations	407	407
Control group mean	0.1325	0.2640

Notes: Covariates included are male, year of birth (dummies), children in the household, complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Also we include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment measures of credit in good standing Q1-2010 to Q3-2010 in the case of *Credit in good standing Average Q2-2011 to Q4-2013*. *Number credit inquiries Average Q2-2011 to Q4-2013* include number of credit inquiries, Q1-2010 to Q3-2010 (quarterly). In Panel C we exclude year of birth (dummies) and we generate *age 18-24*, a dummy that takes value 1 when individual is between 18 to 24 year-olds. Robust standard errors in brackets. Sources: entra21, NOSIS and EQUIFAX (VERAZ).

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Effects on Welfare Program Participation AUH Q2-2011 to Q4-2013 ITT(OLS). Women only.

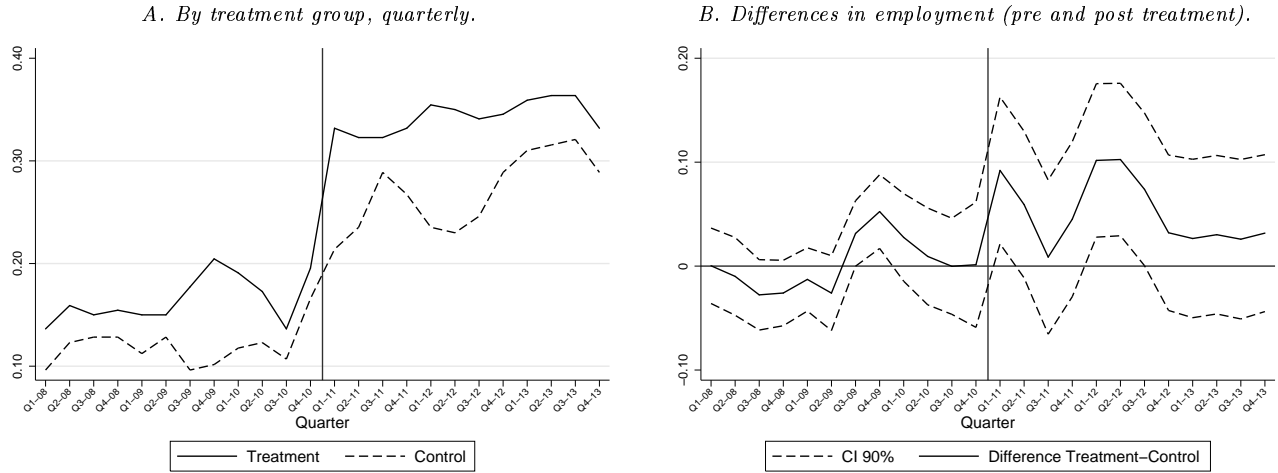
	Always Q2-2011 to Q3-2012	Always Q4-2012 to Q4-2013	Always Q2-2011 to Q4-2013
<i>Panel A. Women</i>			
Treatment group	-0.0080 [0.0312]	0.0015 [0.0393]	0.0077 [0.0315]
Observations	279	279	279
Control group mean	0.1290	0.1694	0.1048
<i>Panel B. Women: Interaction Age 18-24</i>			
Treatment group*Age 18-24	-0.0663 [0.0696]	-0.0706 [0.0837]	-0.1147* [0.0692]
Random assignment	0.0286 [0.0635]	0.0417 [0.0678]	0.0674 [0.0626]
Age 18-24	0.0025 [0.0528]	0.0419 [0.0638]	0.0207 [0.0532]
Observations	279	279	279
Control group mean	0.1905	0.2222	0.1429

Notes: Covariates included are children in the household, year of birth (dummies), complete elementary school, incomplete high school, complete high school, incomplete tertiary level/college, complete tertiary level/college, missing education, married/cohabiting, divorced/separated and missing marital status. Estimations also include pre treatment formal employment Q1-2008 to Q3-2010 and pre treatment AUH Q1-2010 to Q3-2010. In Panel B we exclude year of birth (dummies) and we generate *age 18-24*, a dummy that takes value 1 when individual is between 18 to 24 year-olds. Robust standard errors in brackets. Sources: entra21 and Certificación Negativa-ANSES.

* significant at 10%; ** significant at 5%; *** significant at 1%.

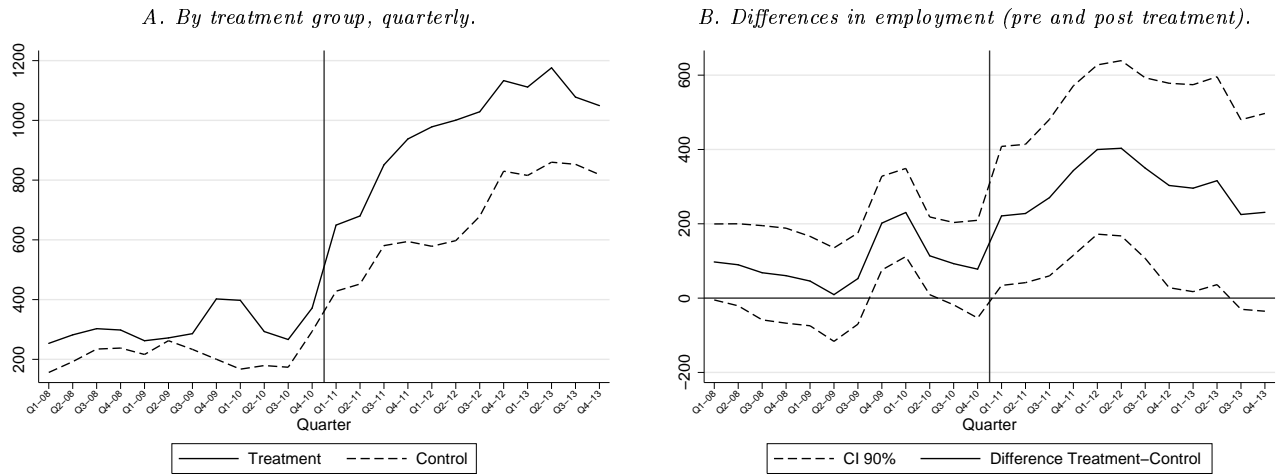
Figures

Figure 1: Formal Employment



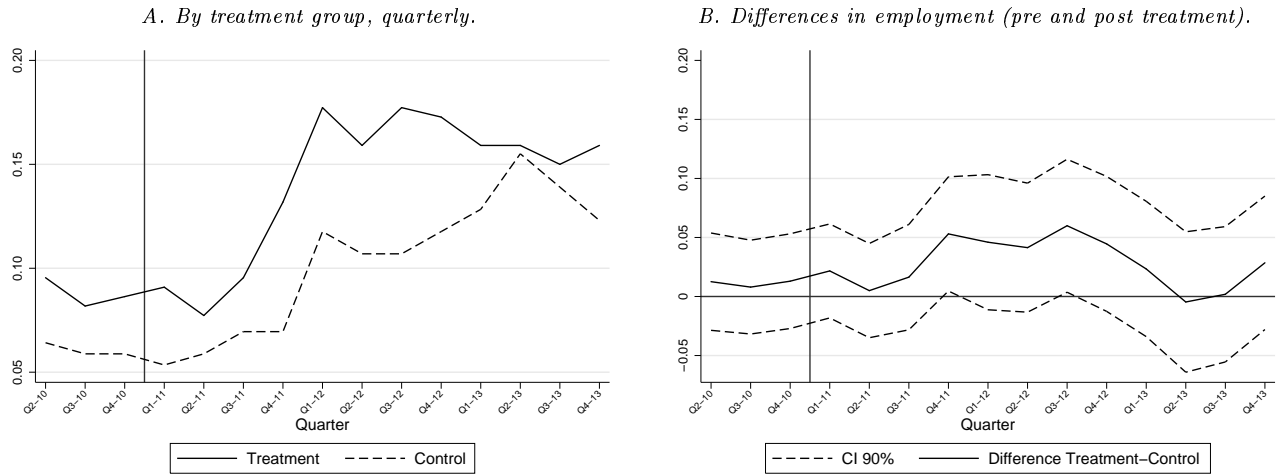
Notes: Vertical line indicates when the program took place. Sources: entra21 and Certificación Negativa-ANSES.

Figure 2: Real earnings (with January 2011 as the base month). With zeros.



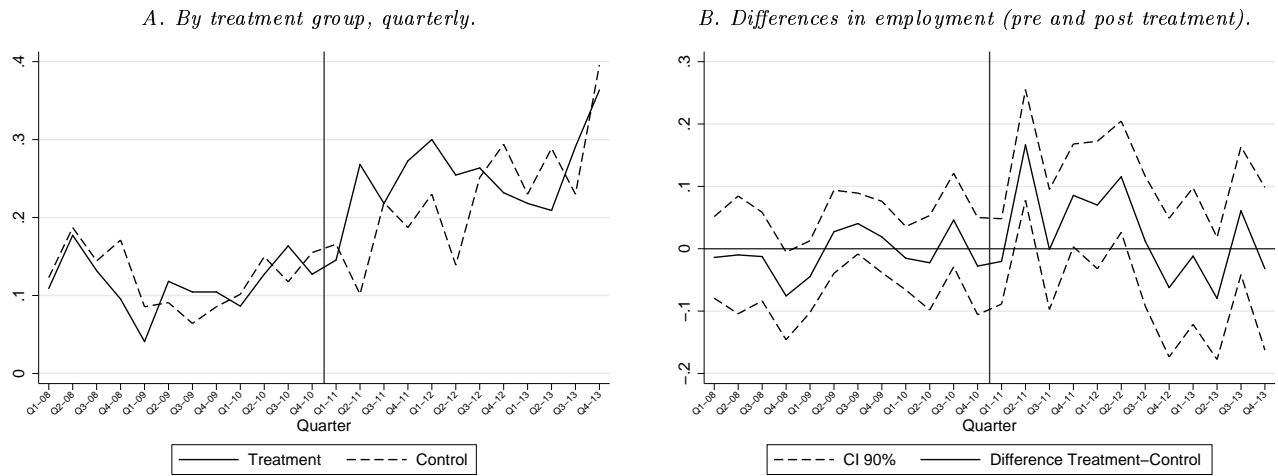
Notes: Vertical line indicates when the program took place. Sources: entra21 and Ministerio de Trabajo de la Nación.

Figure 3: Credit in good standing.



Notes: Credit in good standing, dummy by quarter that takes value 1 in case that the individual credit status is in good standing (value 1 or 2, according to the classification of Central Bank of Argentina BCRA), 0 in other case. Vertical line indicates when the program took place. Sources: entra21 and NOSIS.

Figure 4: Number of credit inquiries.



Notes: Vertical line indicates when the program took place. Sources: entra21 and EQUIFAX (VERAZ).