Employment effects of a social and labor inclusion program

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Resumen

En este trabajo examinamos los efectos de un programa de inclusión social y laboral, Uruguay Trabaja (UT), en varios desempeños del mercado laboral y en el bienestar subjetivo de los participantes. Usando registros administrativos y una encuesta diseñada a tal fin, estimamos los efectos causales del programa explotando la asignación aleatoria de los beneficiarios. Nuestros hallazgos indican que UT aumenta en un 40% la probabilidad de que un beneficiario tenga un trabajo formal entre dos y tres años después de finalizar su participación en el programa, y mejora la satisfacción en diferentes dimensiones del empleo, aunque no afecta la probabilidad de estar empleado en general. Además, proporcionamos evidencia que sugiere que estos efectos persisten en el tiempo, hasta tres años después de la finalización de la intervención. Este estudio destaca la importancia de implementar programas integrales para poblaciones vulnerables y de considerar los efectos a largo plazo al evaluar su efectividad.

Palabras clave: efectos en el empleo, política pública, programa de inclusión
Código JEL: C9, H53, I38, J08

Abstract

In this paper, we examine the effects of a social and labor inclusion program called Uruguay Trabaja (UT) on various labor market outcomes and subjective well-being in Uruguay. Using administrative data and a custom survey, we estimate the program's causal effects by exploiting the random assignment of the beneficiaries. Our findings indicate that the UT program increases by 40% the probability of a beneficiary having a formal job between two and three years after the end of their participation in the program, and it improves satisfaction with different dimensions of employment, but does not affect the probability of being employed overall. Additionally, we provide evidence suggesting that these effects persist over time, up to three years after the intervention ended. This study highlights the importance of implementing comprehensive programs for vulnerable populations and of considering long-term effects when evaluating their effectiveness.

Keywords: employment effects, public policy, inclusion program
JEL Classification: C9, H53, I38, J08

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1   Introduction

Social inclusion implies improving the terms under which individuals and groups take part in society and enhancing the ability, opportunity, and dignity with which people disadvantaged on the basis of their identity take part in society (Bank, 2013). There is a close relationship between poverty and social and labour exclusion. In recent years, programs aimed at the inclusion of the most vulnerable populations in Latin American countries (LAC) have featured tools that seek to increase the employability of these population (Abramo et al., 2019). The main proposal of such programs is that to overcome poverty and reduce inequalities, it is necessary to simultaneously advance both social and labour inclusion. In this paper, we evaluate the impact of Uruguay Trabaja (UT, a social inclusion program carried out in Uruguay since 2008) on labour market outcomes. The main objective of UT is to generate opportunities for social inclusion through socio-educational strategies and work, by combining many tools (temporary jobs, formal education, on-the-job training, and a health component). We exploit the random assignment to the UT program to identify its causal impacts on employment and job quality through objective and subjective measures of the 2016 and 2017 editions.

Previous evaluations of the program have shown a negative effect on the probability of having a formal job immediately after completing the program (Nogueira, 2018) and a positive effect on the probability of having a formal job 12 months after the end of the program (DINEM, 2018). Reviews of the results of this type of policy for other countries (Card et al., 2018; Lammers and Kok, 2021; Escudero et al., 2019) show that the time frame of the evaluation matters. Negative short-run effects can be explained by the lock-in effect, that is, a reduction in job search efforts while the participants are attending training, which causes an initial drop in their probability of employment afterwards (Lammers and Kok, 2021). However, results become positive (sufficient) years after completion of the programs.

Additionally, recent evidence from a meta-analysis of impact evaluations of active labour market policies (ALMP) in LAC finds that they are particularly effective in increasing the probability of having a formal job, compared to other outcomes (Escudero et al., 2019). As McKenzie (2017) suggests, studies that have measured both employment and formal employment have tended to find slightly larger impacts on formal employment, indicating that some programs, such as those that include training, help shift workers toward higher quality jobs. For example, Attanasio et al. (2017) and Ibarrarán et al. (2019), provide evidence that youth training programs that have short-term effects on formal employment show persistent long-term effects in the LAC context. Using administrative data similar to ours, the former study evaluates the long-run effects of the program Jóvenes en Acción (designed for disadvantaged youth in Colombia) and finds large short-run effects on the probability of contributing

to social security, persistent in the long run, for both men and women. Likewise, Ibarrarán et al. (2019), considering a program aimed at disadvantaged youth in Costa Rica (Juventud y Empleo), find significant effects on the probability of having a formal job 6 years after the program.

Our data come from social security administrative records and a follow-up survey of a sample of applicants to the 2016 and 2017 editions, conducted during 2020 and 2021 (Survey of Living Conditions -Encovi-). We find a positive effect of UT on the quality of employment: the beneficiaries' probability of having a formal job in the medium term (two to three years after program completion) is 8.5 pp, 40% higher than the control group. Additionally, we find positive effects on other measures of employment quality (such as social benefits and satisfaction with many dimensions of employment). However, we don't find significant effects on activity, employment, or hours worked.

We analyze two possible mechanisms for the program´s effect on the quality of employment. First, whether the specific training component allows participants to obtain formal employment in sectors that demand those specific skills. Second, whether the form of access to their current employment is different (market search or pre-existing social capital) for treated individuals. Our results show that the program generates tools for job searching beyond their pre-existing social capital, and this impacts the quality of employment. In addition, the sectors in which they are trained are related to jobs of higher quality than those in which the controls are trained.

We make four main contributions to the literature. First, we identify effects on *both* objective and subjective measures of employment quality, which is made possible by a combination of data sources and adds to a literature that usually focuses only on one type of measure (mostly hard outcomes). While individual well-being depends on many circumstances, one of the most important is job quality (Clark, 2015). Second, even within hard labour market outcomes, the combination of data sources allows us to analyze and check for heterogeneity in the effects of the program, including different measures of being employed, employment quality, and being economically active. Third, exploiting both sources of data allows us to study the program effect in different time frames (short, medium, long term) on formal employment and other objective measures of employment quality. Last but not least, the survey allows us to delve deeper into the channels driving previous results, an aspect which is usually absent in similar studies. In particular, we tests for differences in the means of access to the current job, effects of the specific training component, and differential insertion into employment in terms of sectors of activity.

The remainder of this paper is organized as follows. Section 2 presents the main characteristics of the UT program. Section 3 describes the data used in the analysis and presents descriptive statistics. Section 4 discusses the methodology and identification strategy. Sections 5 and 6 present the main

results. Section 7 explores possible channels, while final comments are presented in Section 8.

2 Program

The UT program was created in 2008 to contribute, through socio-educational strategies, to the social integration and improved employability of people in conditions of high social vulnerability and long-term unemployment (Law No. 18,240). Specifically, the program's target population are residents of Uruguay between 18 and 64 years of age who have not completed compulsory basic education (third year of secondary school) and who have not been formally employed for more than three months in the two years before applying to the program. UT seeks to provide a multidimensional intervention for participants in order to improve their knowledge of rights, transversal competency, skill-specific training, and formal education level. It does so by combining the contributions of different levels of government (central, departmental, and municipal) with civil society (non-governmental organizations, unions, and worker cooperatives) throughout the country.
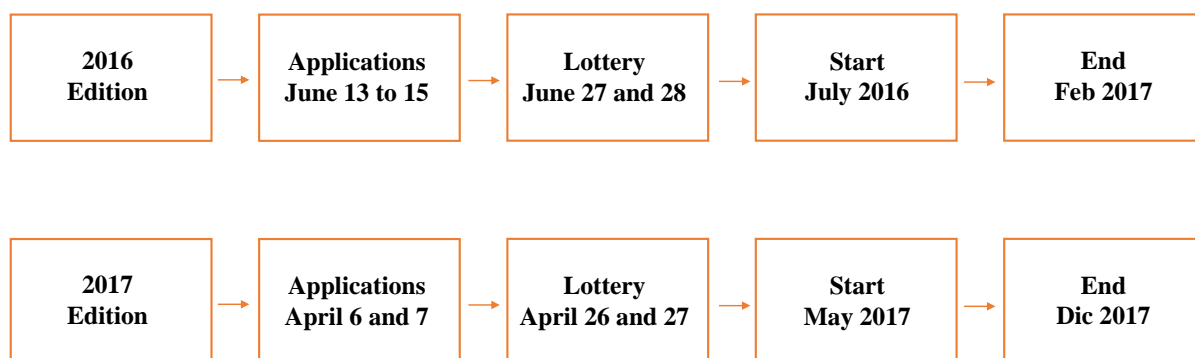
To enter the program, individuals must apply for themselves and meet a series of requirements. Enrollment in the program is done at the points defined in each locality or city. A relevant aspect of the program is that the selection of participants culminates in a lottery that determines the order in which the beneficiaries are summoned at each enrollment site (including a list of alternates)[1]. In 2016, 15,900 people enrolled in the program for 2,000 available slots; in 2017 another 13,450 did so, in this case for 3.000 slots (See Figure 1). Given excess demand, the access route to the program is a lottery, making an experimental impact assessment strategy feasible.

First of all, we follow Le Barbanchon et al. (2021) and take the application as our unit of analysis throughout the paper. This applies to descriptive statistics, main estimation, robustness checks, etc. To be more precise, an 'applicant' is a combination of individual and program edition; hence, if one individual applies to the program on two occasions, it will show up in our paper as two observations.[2] After this clarification, we use applicant and individual as interchangeable terms, given that for better comprehension and intuition, sometimes one term is better suited than the other. Moreover, this issue affects a minor number of actual individuals: in the Appendix A (Figure A.4) we show that results

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[1]Likewise, since 2013 special quotas are used for people with a higher degree of vulnerability: 8% for people of African descent, 4% for people with disabilities, 2% for trans people. Referrals for Priority Programs are created - Cercanías, Uruguay grows with you (UCC), Young People on the Net (JeR), people living on the streets, and with problematic use of psychoactive substances-

[2]We refer the reader to the discussion in Le Barbanchon et al. (2021) for a proper justification of our choice, which we consider to be the best. As an example of the consequences, note that if two men and two women apply to the program in year T and two different women but the same men apply in $T + 1$, using individuals as observations yields 50% male applicants (total of 8 observations), while using applicants as observations yields 75% women (since the men are counted only once, a total of 6 observations).

Figure 1: Temporary implementation

| 2016 Edition | → | Applications June 13 to 15 | → | Lottery June 27 and 28 | → | Start July 2016 | → | End Feb 2017 |

| 2017 Edition | → | Applications April 6 and 7 | → | Lottery April 26 and 27 | → | Start May 2017 | → | End Dic 2017 |

Notes. This figure shows the temporary implementation of the program for 2016 and 2017 editions. The program's end date for each edition corresponds to the longest duration modality.

are robust to using only the first application per individual for the case of individuals that applied to both editions.[3]
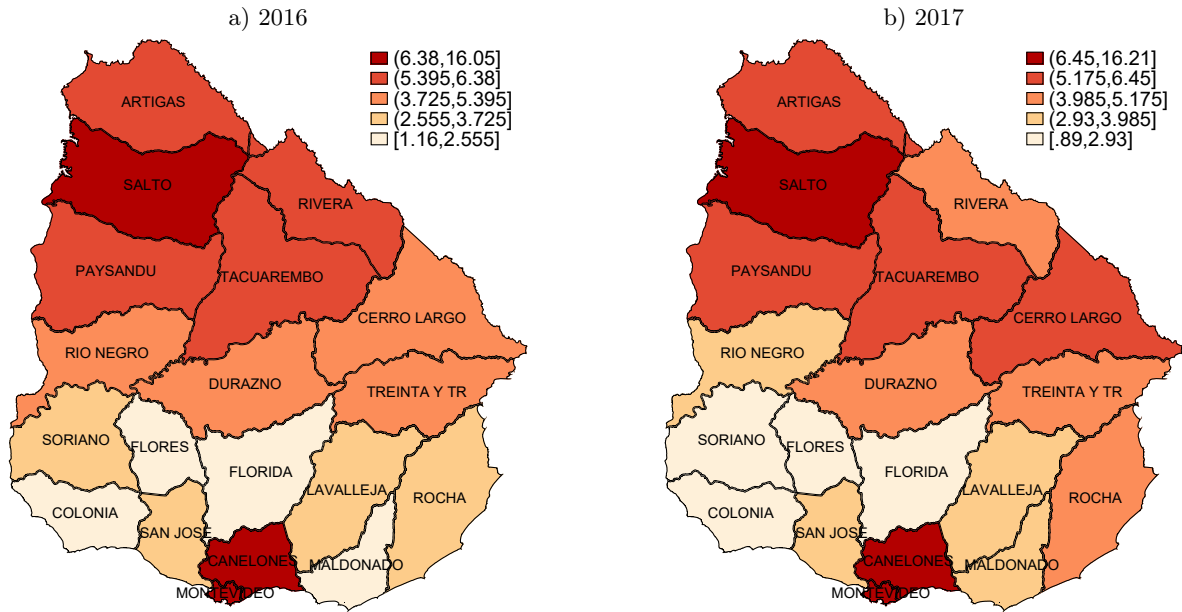
The distribution of the slots in the program has undergone changes in successive editions, with an increase in the number of slots assigned to beneficiaries outside the country's capital (Table A.1). In this paper, we consider the results of the 2016 and 2017 editions of the UT program, and for these, the geographic distribution of beneficiaries is shown in Figure 2. We observe that between 6.4% and 16% of the beneficiaries are located in the country's capital (Montevideo), and 36% in metropolitan regions (Montevideo, Canelones, and San José). Considering that the metropolitan area concentrates 60% of the country's population, this implies an over-representation of the rest of the country among the program's beneficiaries, especially from the northern and northeastern regions. It is possible that the greater representation of these regions is explained by their poorer performance in labour indicators (Carrasco et al., 2018).

The program operates in three modalities. In the country's capital, it works in groups of 25 participants for up to 8 months, while in the rest of the country, it is divided into small towns and departmental capitals. In the small towns, the duration of the program is shorter, up to 6 months, and the groups are smaller (15 participants), and in the department capitals, the duration is up to 8 months and each group has 35 participants.

UT consists of a social support intervention intended to encourage participants' integration into the labor market, and, among the activities carried out, the principal one is providing transitory jobs of public value (labor component). The operational activities are carried out at public institutions that

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[3]Of all individuals in the dataset, 11.3% applied to both editions of the program. As an example, we would have 1000 individuals but 887 applicants.

Figure 2: Geographic distribution of participants

a) 2016

■	(6.38,16.05]
■	(5.395,6.38]
■	(3.725,5.395]
■	(2.555,3.725]
□	[1.16,2.555]

b) 2017

■	(6.45,16.21]
■	(5.175,6.45]
■	(3.985,5.175]
■	(2.93,3.985]
□	[.89,2.93]

Notes. These figures show the geographic distribution of participants by department.

apply to the program, with the Ministry of Social Development (MIDES) promoting and receiving applications from public bodies and local governments (at the departmental or municipal level). The work provided to participants includes tasks considered to be of community value, including light masonry, painting, and other repairs in schools, high schools, hospitals, other public buildings, parks, and squares. To implement this component, an agreement was made with the construction labor union, which provides qualified workers to support learning and specific skills to perform the work.

The other five components of the program are i) Educational- transversal competency workshops and specific training, ii) Health (access to oral health and ophthalmologist treatments), iii) Economic benefit; iv) Facilitating the acquisition of basic documentation (identification card, health card), and v) Educational-recreational outings.

The specific training focuses on knowledge linked to a particular job, while the transversal competency components consist of skills and habits essential for employment and social integration. Although the acquisition of skills related to work is a central axis of the program, it is important to point out that the training is broader, covering topics related to gender roles, the construction of networks, and violence.

In the editions considered, basic care training was incorporated as a mandatory part of the transversal competency training. This was done as a way of directing the training of the participants towards a growing sector as a potential source of employment, in view of the implementation of the Integrated

National Care System (SNIC) in Uruguay [4].

Additionally, the beneficiaries receive a monthly monetary benefit, called *"labour insertion assistance"*, while participating in the program [5]. Despite not being a salary, the benefit generates retirement contributions and maternity and sickness allowances.

The program has a weekly workload of 30 hours from Monday to Friday, distributed between operational activities (24) and training (6), in addition to time spent going over basic documentation and attending health controls.

3 Data sources and descriptive statistics

This section discusses the data sources and variables used in the analysis. This paper uses two sources of information: administrative records of formal employees from the Ministry of Labor and Social Security (*Ministerio de Trabajo y Seguridad Social* - MTSS) and the Living Conditions Survey (*Encuesta de Condiciones de Vida* - Encovi), conducted on a set of individuals who applied to participate in the UT program. Additionally, we present in this section a brief discussion of the summary statistics.

Administrative records

We use the administrative records of formal employees provided by the MTSS. The main database is a monthly unbalanced panel of Uruguayan employees between 2012 and 2021. We have the records of all employees who contributed to Social Security for at least one month between January 2012 and July 2021 (1.6 million observations per month). This database contains monthly information on the activity, the size of the firm, seniority, hours worked, monthly income, employment relationship (public or private salaried worker, or independent worker), and whether they participated in the program.

Since we know the activity record of all Uruguayan employees, we can be sure that if we do not observe someone, it is because he/she does not have a formal job at that moment. When we do not observe a worker in our records, we do not distinguish whether it is because the individual is in informal employment at that moment or because she/he does not have any employment at all. This is an important point in our analysis, and the advantage of the survey is that it reveals whether the person is unemployed or in the informal sector.

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[4]To be a certified caregiver who can be hired using the benefits that the SNIC proposed for dependent adults, training is required by the National Institute of Professional Training (INEFOP). The training provided in the UT program was recognized as part of the required training, thus creating a bridge between the two policies.

[5]The monetary benefit is equivalent to approximately US$300, 70% of the national minimum wage.

Living Conditions Survey

The Encovi survey was carried out on a sample of those enrolled in UT during its 2016 and 2017 editions. The survey was administered to a set of program beneficiaries and applicants who qualified for benefits but were not drawn in the lottery. It was conducted between November 2020 and May 2021. The survey was administered to a sample of households chosen to allow evaluation of the program's impact using a random design. The sample for the Encovi was built in two stages. First, there was a lottery at the locality level. Second, from each locality, a certain number of treated and controls individuals were randomly drawn to be surveyed. The sample in the first step is stratified by the capital city (Montevideo), other cities, and minor localities, taking as a reference the total number of people treated.

A sample of 4,109 individuals who applied UT was obtained. The sample is divided almost equally between the two editions. Among the individuals in the sample who applied to the 2016 edition, 40.5% were assigned from a lottery to the treated group, compared with 59.5% who were drawn as controls. In the 2017 edition, the distribution between groups was similar; 40.4% were assigned to treatment, while 59.6% were selected as controls (the individuals assigned as controls are drawn in order and make up a list of substitutes if any person assigned to treatment desists from participating). From the 4,109 cases originally selected, 2,078 were finally surveyed (59% originally assigned as controls and the remaining 41% treated). Of the total surveys carried out, 21% (431 cases) were by telephone during the months of April and May 2021.

The percentage of rejections was low. The main causes for not conducting the surveys were that the person was not located, the number of surveys projected for the department was reached, or, in many cases, due to the interruption of field work caused by the advance of the pandemic (Table A.2).

Despite the difficulties in conducting surveys due to health emergency measures and the progress of COVID-19, 73% of the projected number of people were surveyed. As shown in Table A.3, the final sample of surveyed individuals does not show significant differences between controls and treated in the baseline characteristics, which allows us to obtain causal estimates.

Descriptive statistics

As established in the previous section, the program's target population has certain characteristics (age, education level, and link to the labour market) that are different from the rest of the residents of Uruguay. Based on the information from the survey and the Continuous Survey of Households in Uruguay (ECH), we present descriptive information on the population that applied for the UT program compared with the entire population of Uruguay.

Table A.4 shows some population characteristics. Specifically, the table shows information for the individuals who were actually surveyed for this study separately from those who made up the original sample. Additionally, the last column presents the characteristics of the set of all individuals residing in Uruguay. Although the proportion of women between 18 and 64 years old living in Uruguay is slightly higher than that of men (53%), this proportion is considerably higher among the population applying for the program (70%) and even higher among those who were actually respondents (77%). Regarding age, among those applying to the program, there is an over-representation of the youngest age group, which ranges from 18 to 33 years (60%), followed by 34 to 48 years (27%), and with a low share of 49 to 64 year olds (14%). For the total population, the proportion of adult individuals in each group is practically identical. In other words, compared with the rest of the Uruguayan population, those who applied to the program belonged to younger age groups. Alternatively, as a requirement of the program, applicants should not have completed the second cycle of secondary education. For this reason, we expected that a greater proportion of individuals with primary education (complete-*pric*- or incomplete-*prii*-) will be observed as the highest level reached among the applicants to the program compared to the total population. In the same way, while 44% of Uruguayans between 18 and 64 years of age have completed high school (*secc or more*), among the applicants, no individual reached that level. Finally, while among the population applying to the program, the proportion of residents of Montevideo does not reach 16%, almost 50% of all Uruguayans reside in that region. This imbalance between Montevideo and the rest of the country is even greater when we look at the beneficiaries of the program. Therefore, there are differences in the characteristics of the target population of the UT program and the adult population of Uruguay.

4 Methodology

In the main analysis, we estimated the local average treatment effect (LATE) on formality. We consider that individuals are treated if they were accepted into the program and participate in it at least initially ($treatment = 1$). Table 1 shows the number of cases that start treatment but do not complete it (Discharges) and the number of cases that start and finish it (Egress), highlighting that the number of discharges is small (158 cases of total treated). Additionally, note that 68 of the individuals interviewed who were originally assigned for treatment (OAT) did not begin their participation in the program, thus producing the entry into the program of a substitute (person originally assigned to control, OAC). We define the *lottery* variable as equal to one for individuals who are randomly selected to participate in the program (regardless of whether they actually participate later). To obtain the causal treatment effect, we exploit the lottery design and instrument the treatment dummy variable with the *lottery*

variable.

Table 1: CLASSIFICATION OF THE SAMPLE

	Effectively Treated			ITT		
	Control	Treated	Total	OAC	OAT	Total
Discharges	0	158	158	14	144	158
Egress	0	695	695	61	634	695
Accepted	1,225	0	1,225	1,157	68	1,225
Total	1,225	853	2,078	1,232	846	2,078

Notes. Source: Own elaboration based on administrative records of the program and Encovi.

In this way, we obtain the LATE of the program, by estimating 2SLS. That is, the average effect on individuals who complied with their original treatment assignment. The LATE is of particular interest in targeted programs since it considers the effect that is obtained specifically on the population that complies with the allocation. Analyzing the intention to treat (ITT) effects and LATE together is pertinent and complementary. In the appendix, we present estimates of the ITT effect, where the general effect of the intervention is observed considering only the variable of offering the program. Our results are consistent with our main estimate.[6]

Following de Chaisemartin and Behaghel (2020) and Le Barbanchon et al. (2021), considering the *lottery* variable as an instrument is a reasonable estimation strategy in the context of random waiting lists when the offer rate is small. We estimated the following regression model:

$$y_{i(e)t} = \alpha_1 + \beta_t Treated_{i(e)} + waveFE_i + \epsilon_{i(e)t} \tag{1}$$

$$Treated_{i(e)t} = \alpha_2 + \sigma_t lottery_{i(e)} + waveFE_i + \mu_{i(e)t} \tag{2}$$

where $y_{i(e)t}$ is the performance of interest the individual i in edition e at moment t. $Treated_i(e)$ indicates whether individual i takes the program job offered in edition e. $lottery_{i(e)}$ indicates whether individual i was chosen for treatment in a given edition. To control by lottery design, we include fixed effects for edition ($waveFE$). This takes care of the variation in the probability of being randomly selected through lotteries depending on the number of places offered. Standard errors are clustered at the individual (i) level. Our parameters of interest are β_t, which we estimate using two-stage least squares as explained above; it captures the LATE t periods after the application. Table A.5 of the

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[6]Although the take up to the program is important, the analysis must consider all individuals regardless of their behaviour with respect to the intervention (Duflo et al., 2007; Angrist and Imbens, 1995; Imbens and Rubin, 2015). This can be achieved by distinguishing between individuals actually treated (effectively treated) and those initially selected for treatment (ITT).

appendix presents the regression of the first stage of the dummy variable *Treated* on the variable *lottery* by program edition.
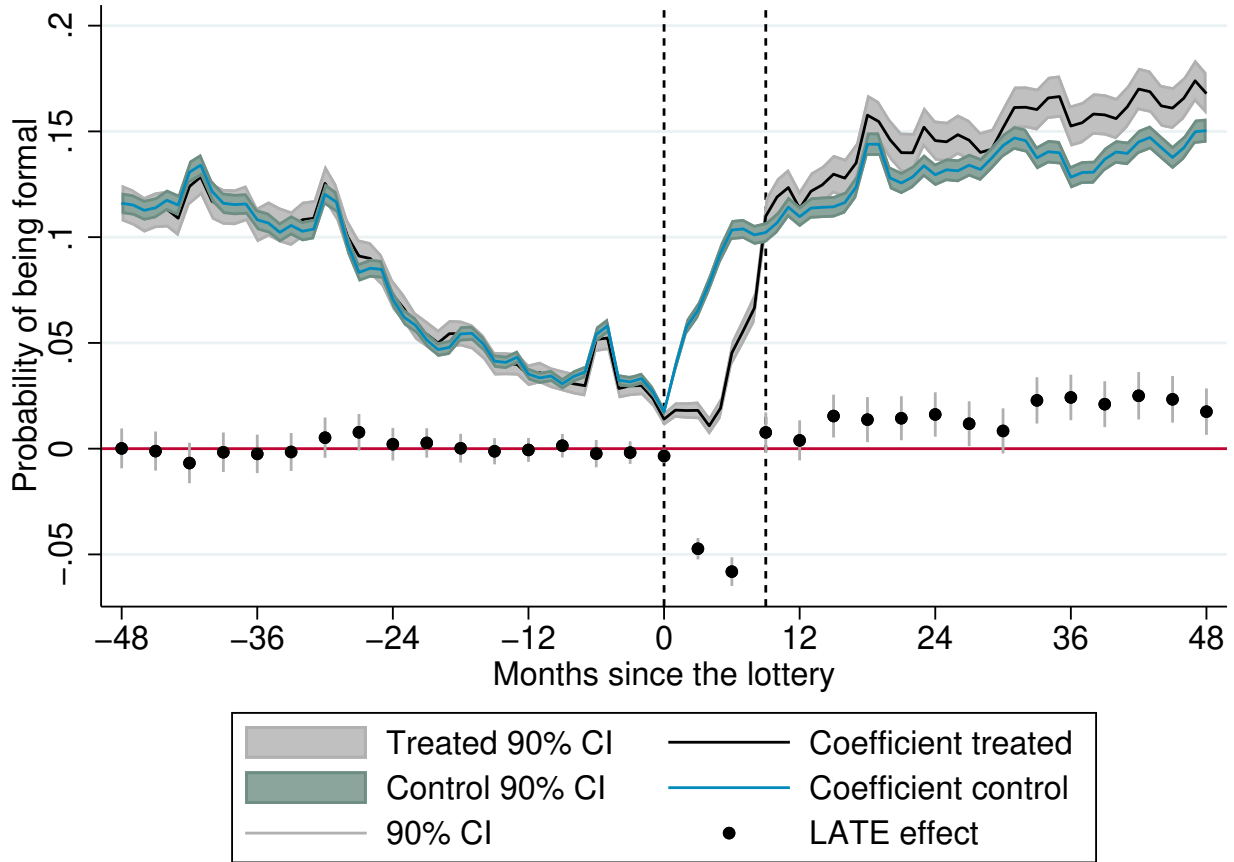
5 Main results

This section describes the main results of the article. We use administrative records and pool the two editions of the program, following the individuals for four years before and after the program. The wealth of data from administrative records allows us to analyze the evolution of the groups assigned to treatment and control over a long period before and after treatment.

Figure 3 shows the effects of the policy on job formality by time (months). We can distinguish three different moments in this figure. First, the pre-treatment period, when both groups show similar behaviour and the probability of being in formal employment steadily decreases, being very close to zero in the month of the intervention. Second, the intervention period, when the rates of re-attachment to formal employment of those who were controls are always higher than those who were treated, an effect that continues until approximately one year after the start of the intervention. Third, one year after the beginning of the program, a statistically significant gap begins to be observed in favour of the treated, which is maintained in the next three years, with a slight tendency to increase [7].

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[7]As mentioned in the program description, despite not providing a salary, participation in the program generates retirement contributions and maternity and sickness allowances. However, to implement the regression, those who are receiving these benefits as a result of their participation in the program are nonetheless considered non-formal. In Figure A.1 of the appendix, we show the results including these individuals as formal, where it can be seen that during the treatment there is a jump in formality among the treated that reaches 80%, which is consistent with what is expected to be observed while the program is running.

Figure 3: EVOLUTION OF PROBABILITY OF BEING FORMAL BY GROUP

Notes: This figure shows the results of monthly IV regressions with treatment instrumented by ITT and a dummy that takes the value 1 if the worker was formal that month as the dependent variable. The sample includes the universe of individuals registered for the lottery who met the conditions. The solid black and light blue lines report the treatment and control mean. The dots report LATE estimates, with their confidence intervals (vertical lines). Source: own elaboration based on administrative records.

The pre-program period (until $t = 0$) shows that there are no statistical differences between the two groups before the program begins. This provides additional evidence of the randomization process of the treated and control groups.

While the program is going on ($0 \leq t \leq 9$), we observe a difference in formality rates between controls and treated in favour of former. These significant differences conform both to the literature (Card et al., 2018) and to previous studies of the program (Nogueira, 2018). There are multiple reasons for this finding. First, the controls have further incentive to get a formal job once they are not selected as program beneficiaries. Second, the increase in the formality rates of the controls would respond to a mechanical effect for the moment in which the conditionalities are controlled. This effect is due to the normal cycle of unemployment. As time passes, they are more likely to get a formal job. Figure

A.2 of the Appendix shows an almost automatic behaviour of workers: at any time it is required to be out of formal employment during the previous 24 months, a jump in formality rates can be observed.[8] On the other hand, the policy's beneficiaries have fewer incentives to look for a formal job during the program´s training period.

Once the training program is finished $(t > 9)$, we estimate positive effects on formal employment among beneficiaries. The dots in Figure 3 report the LATE from equation (1) with their confidence intervals (vertical lines).[9] Additionally, we estimate the average effects by year. The results in Table 2 show that, beginning year 1 after the program is finished, the treatment effects on formality are positive. In fact, the effect on the formality increases yearly until it reaches 2.3 pp in year 3.

Table 2: LATE OF UT ON FORMAL EMPLOYMENT, EXCLUDING UT FUNCTIONAL LINK

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Year -1	Year 0	Year 1	Year 2	Year 3	Ys 1-3
Treated	-0.002	-0.030***	0.012**	0.015***	0.023***	0.017***
	(0.002)	(0.003)	(0.005)	(0.006)	(0.006)	(0.005)
Constant	0.037***	0.082***	0.125***	0.137***	0.139***	0.134***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	352,380	352,380	352,380	352,380	352,380	1,057,140
Individuals	26,387	26,387	26,387	26,387	26,387	26,387
Applicants	29,365	29,365	29,365	29,365	29,365	29,365

Clustered at individuals level standard errors in parentheses
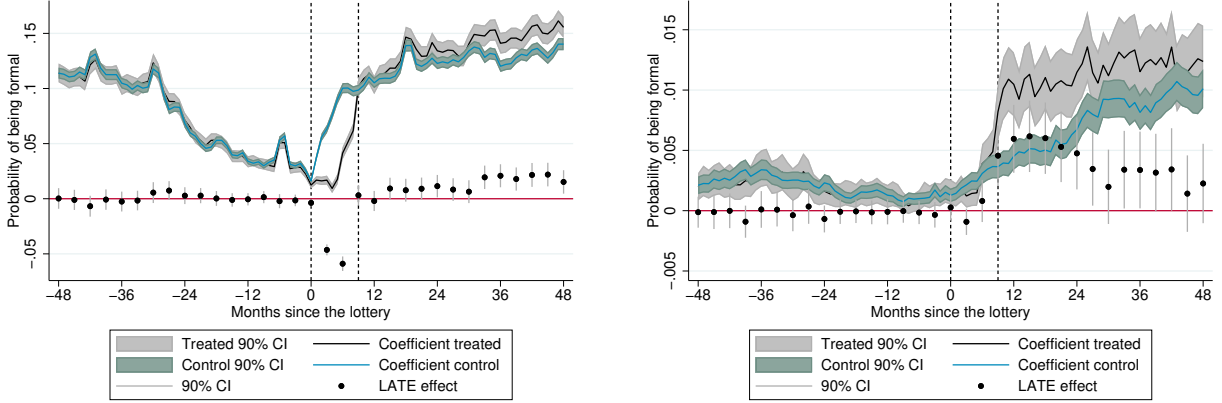*** p<0.01, ** p<0.05, * p<0.1

Figure 4 shows the effects of the policy on formality by time (months), separated by type of employment relationship: panel a) shows the results for salaried workers, while panel b) does the same for the self-employed. It is interesting to compare the evolution over time of both types of employment relationships: while the effect of treatment on salaried workers increases over time, becoming significant in the third year, the effect on the self-employed follows the reverse pattern, beginning with a significant and large effect the first year after the end of the program and then reducing, ceasing to be significant in the third year.

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[8]For the placebos, we use individuals who participated in the lottery for the 2017 edition and check the eligibility conditions in 2014 (at least two years before that the real eligibility conditions are checked), so this exercise should give similar results for other populations.

[9]In the Appendix we present a series of robustness checks. Results are robust to analyzing each edition separately (Figure A.3), restricting the sample to one application per individual (Figure A.4), applying the double re-weighted ever offer estimator of de Chaisemartin and Behaghel (2020) (Table A.6), and using the ITT estimator (Figure A.5).

Figure 4: EVOLUTION OF PROBABILITY OF BEING FORMAL BY EMPLOYMENT RELATIONSHIP

(a) Salaried   (b) Self-employed

Notes.   This figure replicates Figure 3, but for each dependency link separately. Source: own elaboration based on administrative records.

Finally, Figure A.6 in the appendix summarizes the treatment effects on monthly wages. The program has no effect on the formal wage of individuals originally assigned to treatment.

6   General situation in the labour market

In this section, we analyze the general situation in the labour market and both objective and subjective characteristics of current employment. For this, we use a survey performed during 2020 and 2021.

Here we estimate the following regression model:

$$y_{i(e)} = \alpha_1 + \beta Treated_{i(e)} + waveFE_i + \epsilon_{i(e)} \tag{3}$$

$$Treated_{i(e)} = \alpha_2 + \sigma_2 lottery_{i(e)} + waveFE_i + \mu_{i(e)} \tag{4}$$

where $y_{i(e)}$ is the performance of interest for individual i in edition e at the time of the survey. $Treated_i(e)$ indicates whether individual i adopt the program job offered in edition e. $lottery_{i(e)}$ indicates whether individual i was selected at random in a given edition. To control by lottery design, we include fixed effects for edition ($waveFE$). Standard errors are clustered at the individual i level. Our parameter of interest is β, which we estimate using two-stage least squares as explained above and which captures the LATE at the time of the survey. For each set of outcomes, we include a specification that incorporates control variables such as age, sex, education level, region of residence, and year of enrollment in the program.

A central aspect of the random selection method for deriving causal effects of a program is that the sample of treated and control individuals must be balanced. An analysis of the balance of the

Encovi information between treated and controls is presented in Table A.3, and there are no significant differences in baseline covariates for the sample of 2,078 individuals effectively surveyed.

6.1 Objective characteristics of current employment

In this section, through equations 3 and 4, we analyze the differences in the current relationship with the labour market generated by participation in the program, based on survey data. Table 3 (and Table A.7 for ITT) summarizes the lack of significant differences attributable to the program in relation to being part of the economically active population, being employed or unemployed, having more than one job in the case of the employed, or seeking employment with certain requirements in the case of the unemployed.

Table 3 shows that formality results found with administrative records are also present in the survey data estimation. It is not possible to make an exact comparison between the results based on these data because the survey was carried out during 2020 and 2021 (between 4 and 5 years after treatment for the 2016 edition and between 3 and 4 for the 2017 edition), which is also during the pandemic period. Note that while the total effect for the survey is higher, the LATE+Controls sum is lower; in other words, the level of total formal employment is lower, which is expected due to the pandemic.

Table 3: Effect of UT on the current general situation in the labour market (LATE)

Variables	(1)	(2)	(3)	Mean T=0
Economically active population	0.025	0.023	0.024	0.731
	(0.023)	(0.023)	(0.023)	
Employed	0.007	0.009	0.005	0.470
	(0.026)	(0.027)	(0.026)	
Formal	0.048	0.045	0.047	0.087
	(0.017)***	(0.017)***	(0.017)***	
More than one job	-0.021	-0.020	-0.015	0.056
	(0.017)	(0.017)	(0.017)	
Unemployed	0.018	0.014	0.019	0.261
	(0.023)	(0.023)	(0.023)	
Search job with requirements	0.044	0.043	0.055	0.066
	(0.029)	(0.029)	(0.029)*	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of the survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. The regressions in the first three rows were carried out on 2078 (column 1) and 1983 observations (columns 2 and 3), the fourth row on 995 and 948 observations in each case, and the last row on 543 and 523 observations in each case. Source: Own elaboration based on Encovi.

Table 4 and Table A.8 present the effects of the program on the characteristics of current employment for those employed at the time of the Encovi. No significant differences are observed in hours worked in the main occupation between controls and treated. Thus, we complete the characterization of the job supply that is conducted for the population studied, since we did not observe differences in the extensive or intensive margin of their employment participation in the labour market. In addition, how this relation is produced, as a salaried worker (private or public employee) or as self-employed worker (with or without investment or employees), does not show differences either.

Table 4: EFFECT OF UT ON THE CURRENT EMPLOYMENT CHARACTERISTICS - EMPLOYED (LATE)

Variables	(1)	(2)	(3)	Mean T=0
Hours worked	1.302	1.237	1.541	24.423
	(1.359)	(1.400)	(1.370)	
Self-employed	0.010	0.006	0.020	0.283
	(0.034)	(0.035)	(0.035)	
Formal	0.102	0.096	0.101	0.200
	(0.034)***	(0.035)***	(0.034)***	
Formal salaried	0.141	0.134	0.141	0.254
	(0.041)***	(0.042)***	(0.041)***	
Formal self-employed	0.001	-0.005	-0.004	0.061
	(0.040)	(0.041)	(0.045)	
Has health insurance	0.092	0.089	0.092	0.164
	(0.031)***	(0.032)***	(0.031)***	
Has paid licence	0.088	0.082	0.087	0.164
	(0.031)***	(0.032)**	(0.031)***	
Hourly wage (log)	0.010	0.006	0.015	5.892
	(0.074)	(0.077)	(0.079)	
Permanent employment	0.064	0.064	0.059	0.405
	(0.038)*	(0.039)	(0.038)	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. All regressions are performed over employed individuals. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. Regressions are performed on 995 (column 1) and 948 observations (columns 2 and 3). Exceptional cases due to missing information are the first row with 929 and 886 observations respectively, and in the penultimate row where 921 and 888 observations are considered. Lastly, in the case of formality for salaried workers, 722 and 686 observations are considered, and for self-employed, 273 and 262. Source: Own elaboration based on Encovi.

It is in the variables related to the quality of the participants' employment more than two and a half years after the end of the program that the greatest effects are found. The effect of the program on the formal employment in the medium run is 10.2 pp, which implies a difference of 51% between treated and controls[10]. This effect is always significant and robust to all the specifications[11]. In particular,

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[10]These formalization rates among the employed (20% for controls) are very low compared to the total employed population for which in February 2020 the same rate amounted to 77%.

[11]Considering the particularity of the period in which the survey was carried out because of the pandemic, and a robustness check, in Appendix B we present results for questions asked regarding the relationship with the labour market before the pandemic's start, specifically in March 2020.

if we distinguish between those who have a relation as salaried or self-employed workers, we verify impacts associated with the program only in the case of the former, where the effect on the formality rate is equivalent to 14.1 pp., bringing the formality gap between treated and controls to 55.5%. This lack of effect in the survey among the self-employed is consistent with the results found 3 years after the end of the program in the administrative records when separated by dependency link.

Beyond formality in the employment relationship, but probably in relation to this result, the program positively affects other dimensions of work quality in the medium run, such as having paid leave and having health insurance (Table 4). Being assigned to treatment on average increases the likelihood of having paid leave by 8.8 pp., which is equivalent to 53.6%, while the probability of having health insurance increases 9.2 pp., indicating a gap with respect to workers originally assigned to controls of 56%.

6.1.1 Heterogeneous effects on formal employment

The two previous impact evaluations conclude that UT is most effective among those who have been excluded from the formal labour market for the longest time (inactive, unemployed, or informally employed), particularly if they are women. Vezza (2014) shows that for LAC programs, effectiveness is closely associated with the specific characteristics and details of each program, target population, and the labour market of implementation. To deepen the understanding of the effects found on the quality of employment, the heterogeneity of the results is analyzed according to three dimensions that were shown to be relevant in previous studies carried out on the program: sex, region of residence, and age. Additionally, we explore the possibility that being a beneficiary of non-contributory social benefits could modify the impact of the UT program on the labour market.

In Table 5 it can be seen that no significant heterogeneous effects are found according to sex or region. Regarding being a beneficiary of a program of non-contributory social benefits, given the specific design that these programs have in Uruguay, they could generate incentives in the opposite direction to the objectives of UT, promoting the permanence of or an increase in informality (Bergolo and Cruces, 2011). The results indicate no heterogeneous effects depending on whether both treated and controls are part of an AFAM beneficiary household. Finally, in the fourth column of the table, the existence of heterogeneities in the effects of the program is tested according to the age of the participants, in particular by differentiating the population under 30 years of age from the rest. Again, we do not find heterogeneous effects associated with age.

Table 5: Effect of UT on formality - employed, heterogeneity

	(1)	(2)	(3)	(4)
Heterogeneity	AFAM	Sex	Region	Age
Treated	0.093*	0.072*	0.084**	0.125***
	(0.054)	(0.043)	(0.037)	(0.042)
Interaction	0.006	0.071	0.096	-0.079
	(0.070)	(0.072)	(0.103)	(0.073)
Treated + interaction	0.099***	0.142***	0.179***	0.047***
	(0.045)	(0.057)	(0.096)	(0.059)
Observations	948	948	948	948
R-squared	0.01	0.01	0.01	0.01
Mean control	0.211	0.211	0.211	0.211

Notes. Each column shows the result of an IV regression with a dummy that takes value 1 if the worker was formal as the dependent variable. Treated and the interaction between the treatment and the variable of interest are instrumented by ITT and the interaction between ITT and the variable of interest. These variables are: a dummy for being beneficiary of AFAM (column 1), male (column 2), Montevideo (column 3) and less than 30 years of age (column 4). Regressions are performed on 948 observations. Source: Own elaboration based on Encovi

6.2 Subjective characteristics of current employment

In relation to the subjective aspects of jobs among the employed, some dimensions of their satisfaction with their jobs are analyzed in Table 6 (and Table A.9 for ITT). To measure satisfaction, a scale of 1 to 5 is used, where 1 means very dissatisfied, 2 is dissatisfied, 3 neither satisfied nor dissatisfied, 4 satisfied, and 5 very satisfied. Within a framework of fairly high general satisfaction, effects of the program are observed on average satisfaction with all dimensions of quality and work environment: i) relationship with peers at work, ii) relationship with their superiors, iii) and the non-salary benefits granted by the job.[12] In particular, the greatest effect significant and robust to the different specifications occurs in *How satisfied are they with non-salary benefits*, which is in line with the results found for objective measures of job quality.

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[12]The survey collected information on the following additional dimensions, for which no significant results were found: satisfaction with the specific task performed, with work schedule, with your income or salary, with the recognition you receive for your work, with the general work environment of the company or establishment, with opportunities for promotion and career development, and with training possibilities in the company.

Table 6: EFFECT OF UT ON SATISFACTION WITH SELECTED DIMENSIONS OF YOUR JOB FOR EMPLOYEES (LATE)

Variables	(1)	(2)	(3)	Mean T=0
How satisfied you are with your relationship with your peers	0.112	0.105	0.105	4.088
	(0.059)*	(0.061)*	(0.062)*	
How satisfied you are with your relationship with your superiors	0.123	0.119	0.114	4.134
	(0.051)**	(0.052)*	(0.052)*	
How satisfied are you with non-salary benefits	0.288	0.332	0.365	3.038
	(0.117)**	(0.122)***	(0.121)***	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. Regressions are performed on 995 (column 1) and 948 observations (columns 2 and 3), and in the remaining rows 862 and 817 observations respectively. In rows 1, 3, and 5 the dependent variable is a dummy that takes the value 1 if the person is satisfied or very satisfied. In rows 2, 4, and 6, the dependent variable takes values from 1 to 5. Source: Own elaboration based on Encovi.

7 Channels

The results described above leave open the question of which channels are operating to explain the insertion in better quality jobs from the objective and subjective points of view of the treated individuals. The richness of the questions asked in the survey, used in conjunction with administrative records, allows us to explore two possible channels. First, we explore whether the means of access to their current employment is different (job market search or pre-existing social capital) for the treated than the controls. An increase in market search as the means of access to their current employment could imply that the program increases the employability of the workers and/or generates changes in the search effort of the workers and in the tools they use for they job search. Unfortunately, the information available does not allow us to know which of these two effects explains this channel. Second, we explore whether the specific training component allows participants to obtain formal employment in sectors that demand specific skills. Our data allow us to match the specific training received by the workers during the program and after passing through it with the sector of activities in which they found a job. This channel agrees with Card et al. (2018), in which larger gains were found for programs that emphasize human capital accumulation, and training programs tended to have more positive average impacts in the medium and longer runs, with a pattern of rising impacts. More interestingly, we can explore the effectiveness of different types of specific training and their relationship with the general trends in the sectors of activity.

7.1 Form of access to current employment

Table 7 (and Table A.10 for ITT) presents information on how the survey respondents accessed their current job. Although the main way to obtain employment for both groups is through their social capital (friends or relatives), this form of access is significantly less relevant within the treated group (23.6%). On the other hand, the treated accessed jobs to a greater extent through searches in the labour market[13], with finding a 22.6% difference in using this search mechanism in favour of the treated. This may be a channel to explain the differences found in various dimensions of the quality of employment accessed by UT beneficiaries: passage through the program generates job search tools beyond the participants' pre-existing social capital.

Table 7: Effect of UT on the form of access to current employment - employed (LATE)

Variables	(1)	(2)	(3)	Mean T=0
Market search	0.064	0.073	0.084	0.283
	(0.037)*	(0.037)*	(0.037)**	
Friends or relatives	-0.134	-0.155	-0.166	0.566
	(0.040)***	(0.041)***	(0.040)***	
Program	0.006	0.009	0.008	0.024
	(0.013)	(0.013)	(0.013)	
Temporary employment	-0.004	-0.005	-0.005	0.004
	(0.003)	(0.003)	(0.003)	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that is worth 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that is worth 1 for those surveyed by telephone, and 3 dummies for education level. Regressions are performed on 935 (column 1) and 891 observations (columns 2 and 3). Source: Own elaboration based on Encovi.

7.2 Sectors of activity and relationship with specific training

As a result of data from the Encovi, we know whether respondents have received any type of job training throughout their lives, and the characteristics of their most recent training. In line with expectations, since the program has a component associated with courses and training, a significant, positive, and highly relevant difference is observed between treated and control individuals in terms of participation in courses or training (Table 8). On average 32% of the controls indicate that they are

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[13]This form of access is derived by grouping the following questions into a single category: 1- Offered his job and they accepted it, 2- Applied for an ad, 3- Entered through a contest, 4- He/She did the paperwork, managed loans; searched local or field to settle.

attending or have attended a job training course, while for the treated, this percentage rises to more than 64% in the specifications[14]. Although this is a "mechanical" effect of the program (it is present and is actually declared by the respondents.

As a result of the UT program features, its participants mention construction, IT/communication, and care for dependent people[15] to a greater extent as their most area of training. Note that the dependent care course is taken by all UT beneficiaries in the years considered, as part of their transversal competency training. Nevertheless, unlike the specific training, which has a total workload of between 50 and 100 hours, the transversal competency training has a workload of only 16 hours. The fact that the question is about the last training course, and during the UT intervention individuals take both specific training and the transversal competency care course, generates a problem in interpreting the answer. For this reason, we ask if the individual did the care system course (beyond the introductory module of UT). In Table 8, we report that treated individuals had a higher probability of doing that course in contrast with the untreated; the training areas to which the controls are linked more intensely than the participants are administration and beauty.

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[14]Note that all UT beneficiaries completed some training course. When the question is restricted to those who complete the program, the percentage rises but is still less than 100%. Therefore, this difference could be due to recall errors because the query is made some years after the end of the program.

[15]This course, in addition to constituting a general training in caring for dependent people, can be useful for a wide variety of activities, is the introductory module of the course for caregivers that is, required to be employed in care system. The care system is a government policy that was developed with the aim of generating a model of shared responsibility between families, the State, the community, and the market.

Table 8: Training in specific areas (LATE)

Variables	(1)	(2)	(3)	Mean T=0
Attends or attended courses or training	0.323	0.353	0.351	0.324
	(0.021)***	(0.021)***	(0.021)***	
Formation area of the last course				
Administration	-0.029	-0.033	-0.032	0.048
	(0.012)**	(0.013)**	(0.012)***	
Masonry/Construction	0.108	0.137	0.127	0.143
	(0.026)***	(0.025)***	(0.027)***	
IT/communication	0.058	0.077	0.080	0.163
	(0.026)**	(0.026)***	(0.027)***	
Hospitality/gastronomy	-0.028	-0.034	-0.036	0.138
	(0.022)	(0.023)	(0.023)	
Beauty/Hairdressing/Manicure/Podiatry	-0.048	-0.058	-0.059	0.068
	(0.014)***	(0.016)***	(0.014)***	
Cares	0.093	0.094	0.093	0.026
	(0.011)***	(0.011)***	(0.013)***	
Did the Care System course	0.035			0.011
	(0.007)***			
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Each cell has an IV regression with the probability of attending or having attended a course of training as the dependent variable and treatment instrumented with ITT as the main explanatory variable. Controls are sex, age, a dummy that is worth 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that is worth 1 for those surveyed by telephone, and 3 dummies for education level. Regressions are performed on 935 (column 1) and 891 observations (columns 2 and 3). Source: Own elaboration based on Encovi.

A possible channel through which the program impacts the quality and formality of the employment of treated individuals is the educational and specific training component.

In Table 9 we show the probability of being formally employed in different sectors of activity, using an industry class (1 digit, ISCI, fourth revision) between 36 and 48 months after the beginning of the intervention. There are three sectors in which individuals assigned to treatment are inserted with greater probability than controls in a statistically significant way: agricultural and forestry activities, household activities, and health and social services.

Administrative records allow us to analyze in greater depth which particular activities these individuals are being inserted into. In Figure 5, we present some specific sectors of interest classified at 5

digits of ISCI, fourth revision. We can see that most of the effect in health is explained by residential care institutions and patient accompaniment services.

In household activities, the sector of interest classified at 5 digits of ISCI in which treated are inserted with higher probability than controls is domestic service. In this sector, it is particularly interesting to analyze what happens with the subsector that contains the care system workers, which is in full expansion in the years under analysis.

Finally, for agriculture and forestry, it can be seen that the effect is not mainly explained by any specific sector.

As for the hypothetical relationship between specific training and employment in related sectors of activity, we find that those treated are more likely to be trained in care activities, and then more likely to be employed in sectors of activity that require this type of skill.

However, this does not happen with the other sector in which this relationship could occur, which is construction[16]. Figure 5 shows clues as to why this occurs: there it can be seen that beyond the differential effect on treated/controls, the care sectors (quadrants a b and c) are increasing their demand for workers, while construction is reducing its demand. This suggests that while specific training is necessary for inserting these workers, it is also required to be in sectors of activity that have current demand for workers with this type of training or skills.

These differences provide a possible explanatory channel for the greater probability of acquiring formal jobs among the individuals assigned to treatment: it is possible that the courses received and skills acquired allow them to enter as formal workers in certain sectors.
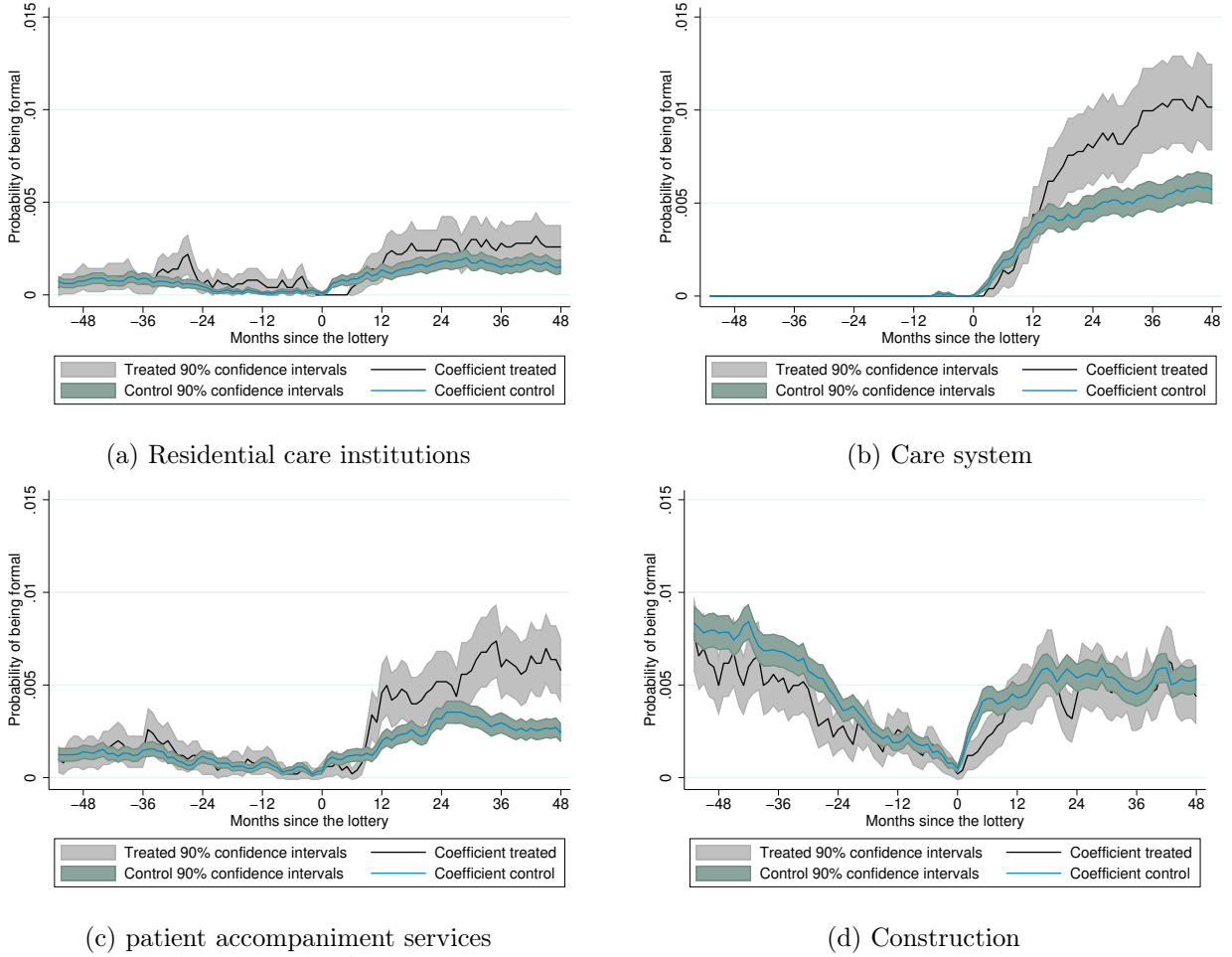
Table 9: Effect of UT on the current employment activity sector (LATE)

	Agric., forestry	Manuf. industry	Constr.	Commerce	Accom. and food services	Admin. activity	Public administration	Health	Household activity
Treated	0.004*	0.002	0.000	0.002	-0.002	0.001	0.002	0.005**	0.007**
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
Constant	0.022***	0.010***	0.005***	0.018***	0.006***	0.019***	0.006***	0.011***	0.020***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

Notes. Each row shows the results of an IV regression with Treated instrumented by ITT as the independent variable and a dummy that takes the value 1 if the worker was formal and employed in a particular sector as the dependent variable. The sample includes the universe of individuals registered for the lottery who met the conditions, observed between 36 and 48 months after the beginning of the intervention. Clustered at individuals level standard errors in parentheses. All regressions are performed on 352,380 observations. Source: own elaboration based on administrative records.

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[16]Thise analysis is not carried out for IT because the courses given in these areas provide transversal competency and basic knowledge that can be used in different sectors.

Figure 5: EVOLUTION OF PROBABILITY OF BEING FORMAL BY ACTIVITY SECTOR

(a) Residential care institutions

(b) Care system

(c) patient accompaniment services

(d) Construction

Notes. This figure replicates Figure A.5, but the independent variable takes the value 1 only if the individual has formal work in a specific sector referred to in each box. Source: own elaboration based on administrative records.

8   Final comments

This paper conducts an impact evaluation of the 2016 and 2017 editions of the Uruguayan program *Uruguay Trabaja*. It is defined as a socio-educational program that seeks to generate tools for social inclusion and is aimed at people with high social vulnerability. Previous evaluations of the same program found no effects on employment in the short term. This suggests, therefore, the existence of a negative effect on the probability of finding formal employment up to one year after leaving the program. In this study, we consider the longer-term effects of the program on different dimensions of employment.

We use two sources of information: an administrative registry of formal employees and a living conditions survey. We found a positive effect of the program on the quality of employment measured in multiple ways: formality, social benefits, and satisfaction with various dimensions of employment. We

do not find program effects on employment rates (at the extensive or the intensive margin). A primary effect on quality rather than employment level is within expectations given the existing literature, at least for young people (Attanasio et al., 2017; Ibarrarán et al., 2019). We did not find heterogeneous effects according to sex, region of residence, age, or receiving additional non-contributory social benefits on the likelihood of formality. When distinguishing between salaried and self-employed workers, it is interesting to note that the self-employed experience a significant and positive effect immediately after the end of the program, which then fades away, while the opposite occurs for salaried workers. This is also consistent with the finding of no significant effects for the self-employed in the survey.

Additionally, we analyze two possible mechanisms behind the positive effect on the quality of employment generated for program participants. First, we explore whether the means of access to their current employment is different (market search or pre-existing social capital) for program beneficiaries. Second, we examine whether the specific training component allows participants to insert themselves into formal jobs in sectors that demand those specific skills. According to our results, the program generates a reduction in social capital as the means of access to current employment and an increase in employment access through market search. Moreover, the treated individuals are more likely to obtain employment in sectors of activity related to care in households and health activities, which require skills and training that the program provides to the workers, and also are that were experiencing expansion.

The UT has been a successful program in improving formal employment levels among beneficiaries. The improvement in cross-cutting skills and the increased training and search capacity of the beneficiaries could have been key to keeping them in formal employment even in the adverse conditions generated by the COVID-19 pandemic. The evidence we obtain measures the value of this type of program in countries with similar labour markets. The impacts on the quality of employment improve the living conditions of the most vulnerable.

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APPENDIX

A Appendix A

Table A.1: DISTRIBUTION OF PROGRAM VACANCIES BY REGION AND YEAR

Departamento	2011	2012	2013	2014	2015	2016	2017
Montevideo	32%	29%	29%	24%	14%	17%	17%
Artigas	6%	6%	6%	6%	7%	7%	7%
Canelones	15%	16%	15%	16%	18%	15%	15%
Cerro Largo	2%	4%	4%	5%	6%	5%	5%
Colonia	2%	3%	2%	2%	2%	1%	3%
Durazno	2%	3%	3%	3%	4%	4%	4%
Flores	1%	1%	1%	1%	1%	1%	1%
Florida	2%	2%	2%	2%	2%	2%	2%
Lavalleja	2%	2%	2%	1%	3%	3%	3%
Maldonado	2%	3%	2%	2%	3%	3%	3%
Paysandú	4%	4%	4%	5%	6%	6%	6%
Río Negro	3%	2%	2%	3%	3%	3%	3%
Rivera	5%	4%	4%	5%	5%	5%	5%
Rocha	3%	3%	3%	3%	4%	4%	4%
Salto	5%	6%	6%	6%	7%	7%	6%
San José	4%	3%	3%	3%	4%	4%	4%
Soriano	2%	3%	3%	3%	3%	4%	3%
Tacuarembó	5%	4%	5%	5%	6%	5%	5%
Treinta y Tres	2%	2%	2%	3%	3%	4%	4%
Total	100%	100%	100%	100%	100%	100%	100%

Table A.2: GENERAL FIELD BALANCE (SAMPLE 4,109 INDIVIDUALS)

Percentage	Balance	Frequency
Undone	1,067	26,0
Done	2,078	50,6
Rejection	181	4,4
Untraceable	685	16,7
Other Undone	89	2,2
Office rejection	9	0
Total	4,109	100

Notes. Rejected cases involve people who did not want to answer the interview either in person or by telephone. The untraceable are those that we confirmed by telephone and in the field that we did not find the case. Finally, the category "other undone" involves those who were contacted, but could not carry out the interview, due to being in a situation of imprisonment or partial rejections, and office rejections are those cases carried out that were eliminated in quality supervision. Source: Own elaboration based on Encovi.

Table A.3: Randomization check per year

	Control	Treat	Difference T-C	DE	p-valor	N
Year 2016						
Women	0,76	0,79	0,03	0,03	0,28	1051
	(0,43)	(0,41)				
Age	32,24	32,47	0,22	0,74	0,76	1050
	(11,43)	(12,36)				
Incomplete primary education	0,12	0,12	0,00	0,02	0,99	1050
	(0,33)	(0,33)				
Complete Primary	0,50	0,46	-0,04	0,03	0,17	1050
	(0,50)	(0,50)				
High school	0,38	0,42	0,04	0,03	0,16	1050
	(0,49)	(0,49)				
Montevideo	0,13	0,14	0,01	0,02	0,62	1051
	(0,34)	(0,35)				
Year 2017						
Women	0,77	0,76	-0,01	0,03	0,65	1027
	(0,42)	(0,43)				
Age	32,83	32,28	-0,55	0,75	0,46	1024
	(11,60)	(11,61)				
Incomplete primary education	0,12	0,11	-0,01	0,02	0,51	1024
	(0,33)	(0,31)				
Complete Primary	0,47	0,47	0,00	0,03	0,99	1024
	(0,50)	(0,50)				
Incomplete high school	0,40	0,42	0,01	0,03	0,65	1024
	(0,49)	(0,49)				
Montevideo	0,14	0,11	-0,03	0,02	0,11	1027
	(0,35	(0,31)				

Notes. Elaborated from the sample actually surveyed. The first two columns present the average value of each variable for the group assigned to control and treatment, respectively. The third column shows the difference in the mean of the two groups. Column 4 (SD) is the standard deviation of the difference, and column 5 shows the p-value of the means test between columns 1 and 2. Finally, N indicates the number of observations for each variable. Source: Own elaboration based on the ENCOVI and RA of the program.

Table A.4: DESCRIPTIVE STATISTICS

	Program applicants (Surveyed sample)	All population (ECH)
Women	0.766	0.526
	(0.423)	(0.499)
Age group		
18 - 33	0.595	0.345
	(0.491)	(0.476)
34 - 48	0.271	0.329
	(0.445)	(0.470)
49 - 64	0.132	0.326
	(0.339)	(0.469)
Education group		
Incomplete primary education	0.120	0.049
	(0.325)	(0.215)
Complete Primary	0.476	0.187
	(0.500)	(0.390)
Incomplete high school	0.402	0.181
	(0.490)	(0.385)
Complete high school or more	0	0.439
		(0.496)
Montevideo	0.133	0.388
	(0.340)	(0.487)

Notes. Source: Own elaboration based on Encovi and Continuous Household Survey.

Table A.5: EFFECT OF UT OFFER ON UT PARTICIPATION (FIRST STAGE)

	(1) Treated	(2) Treated	(3) Treated
ITT (Won lottery)	0.853***	0.840***	0.876***
	(0.004)	(0.007)	(0.006)
Constant	0.056***	0.077***	0.029***
	(0.001)	(0.002)	(0.002)
Edition	Both	2016	2017
Observations	29,365	15,907	13,458
R-squared	0.640	0.517	0.767

Notes. OLS regressions of UT participation on the intention to treat (winning the lottery). Standard errors clustered at the applicant level shown in parenthesis. Source: Own elaboration based on administrative records of the program.

Table A.6: Effect of UT on formal employment, robustness DREO estimator

VARIABLES	(1) Year -1	(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 3	(6) Ys 1-3
Treated	-0.002	-0.032***	0.008**	0.013***	0.015***	0.012***
	(0.0028)	(0.0011)	(0.004)	(0.007)	(0.004)	(0.005)
Observations	352,380	352,380	352,380	352,380	352,380	1,057,140
Individuals	26,387	26,387	26,387	26,387	26,387	26,387
Applicants	29,365	29,365	29,365	29,365	29,365	29,365

Notes. Clustered at individuals level standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.7: Effect of UT on the current general situation in the labor market (ITT)

Variables	(1)	(2)	(3)	Mean T=0
Economically active population	0.022	0.019	0.020	0.731
	(0.019)	(0.020)	(0.020)	
Employed	0.006	0.007	0.004	0.476
	(0.022)	(0.023)	(0.022)	
Formal	0.042	0.040	0.042	0.101
	(0.015)***	(0.015)***	(0.015)***	
More than one job	-0.017	-0.017	-0.012	0.061
	(0.014)	(0.014)	(0.015)	
Unemployed	0.016	0.012	0.016	0.255
	(0.020)	(0.020)	(0.020)	
Search job with certain requirements	0.038	0.037	0.047	0.067
	(0.025)	(0.025)	(0.024)*	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. The regressions of the first three rows are carried out on 2078 (column 1) and 1983 observations (columns 2 and 3), the fourth row on 995 and 948 observations in each case, and the last row on 543 and 523 observations in each case. Source: Own elaboration based on Encovi.

Table A.8: EFFECT OF UT ON THE CURRENT EMPLOYMENT CHARACTERISTICS - EMPLOYED (ITT)

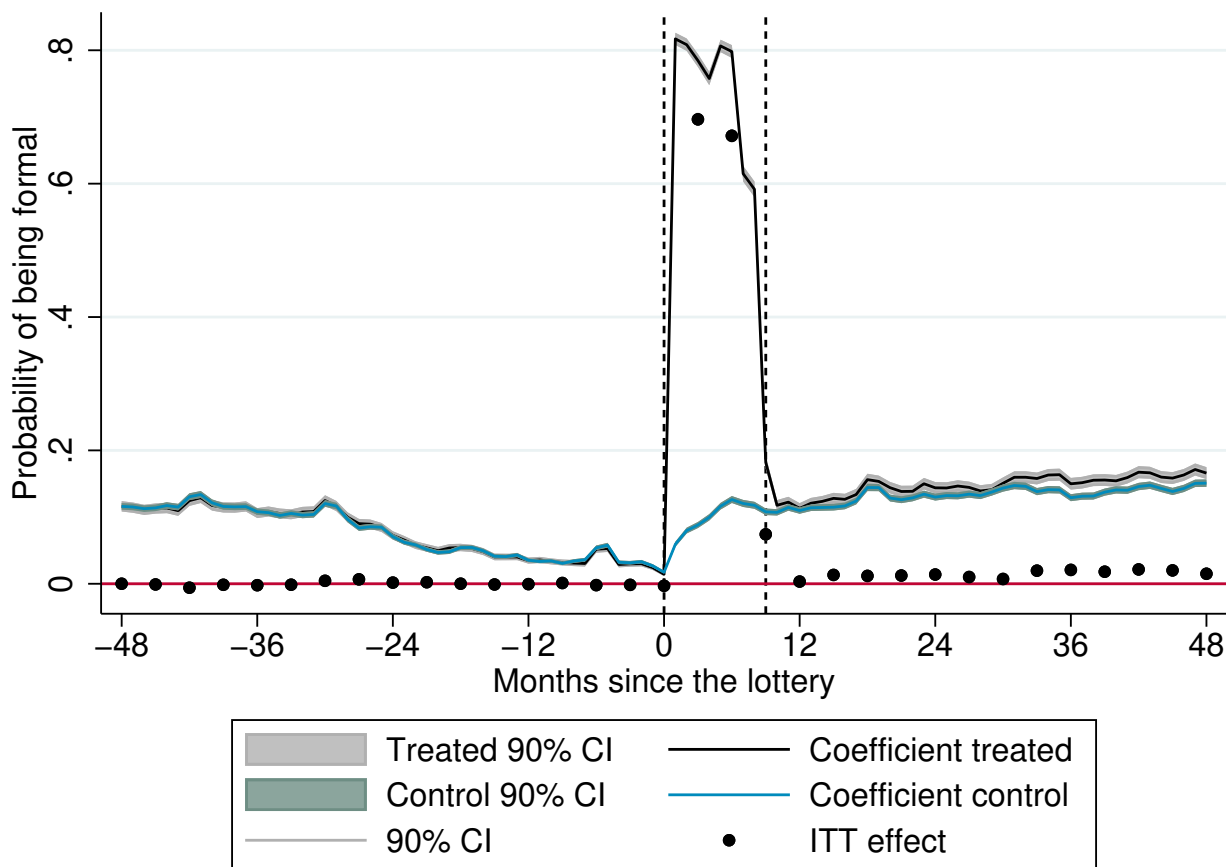| Variables	(1)	(2)	(3)	Mean T=0
Hours worked	1.086	1.023	1.276	24.639
	(1.135)	(1.160)	(1.148)	
Self-employed	0.009	0.005	0.017	0.271
	(0.029)	(0.029)	(0.029)	
Formal	0.085	0.080	0.084	0.211
	(0.028)***	(0.029)***	(0.028)***	
Formal salaried	0.120	0.114	0.121	0.264
	(0.036)***	(0.036)***	(0.036)***	
Formal self-employed	0.001	-0.004	-0.003	0.069
	(0.031)	(0.032)	(0.034)	
Has health insurance	0.077	0.074	0.076	0.164
	(0.026)***	(0.027)***	(0.026)***	
Has paid licence	0.073	0.068	0.072	0.167
	(0.026)***	(0.027)**	(0.026)***	
Hourly wage (log)	0.009	0.005	0.012	5.922
	(0.062)	(0.063)	(0.064)	
Permanent employment	0.054	0.053	0.049	0.429
	(0.032)*	(0.033)	(0.031)	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. Regressions are performed on 995 (column 1) and 948 observations (columns 2 and 3). Exceptional cases due to missing information are the first row with 929 and 886 observations respectively, and the penultimate row where 921 and 888 observations are considered. Lastly, in the case of formality for salaried workers, 722 and 686 observations are considered, and for self-employed workers, 273 and 262. Source: Own elaboration based on Encovi.

Table A.9: EFFECT OF UT ON SATISFACTION WITH SELECTED DIMENSIONS OF YOUR JOB FOR EMPLOYEES (ITT)

Variables	(1)	(2)	(3)	Mean T=0
How satisfied you are with your relationship with your peers	0.095	0.088	0.088	4.073
	(0.050)*	(0.051)*	(0.052)*	
How satisfied you are with your relationship with your superiors	0.105	0.101	0.097	4.137
	(0.051)**	(0.052)*	(0.052)*	
How satisfied are you with non-salary benefits	0.256	0.293	0.321	3.076
	(0.105)**	(0.108)***	(0.109)***	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for educational level. Regressions are performed on 995 (column 1) and 948 observations (columns 2 and 3). Those of the rest of the rows about 862 and 817 observations in each case. in rows 1, 3, and 5 the dependent variable is a dummy that takes the value 1 if the person is satisfied or very satisfied with their job. in rows 2, 4, and 6, the dependent variable takes values from 1 to 5. Source: Own elaboration based on Encovi.

Table A.10: EFFECT OF UT ON THE FORM OF ACCESS TO CURRENT EMPLOYMENT - EMPLOYED (ITT)

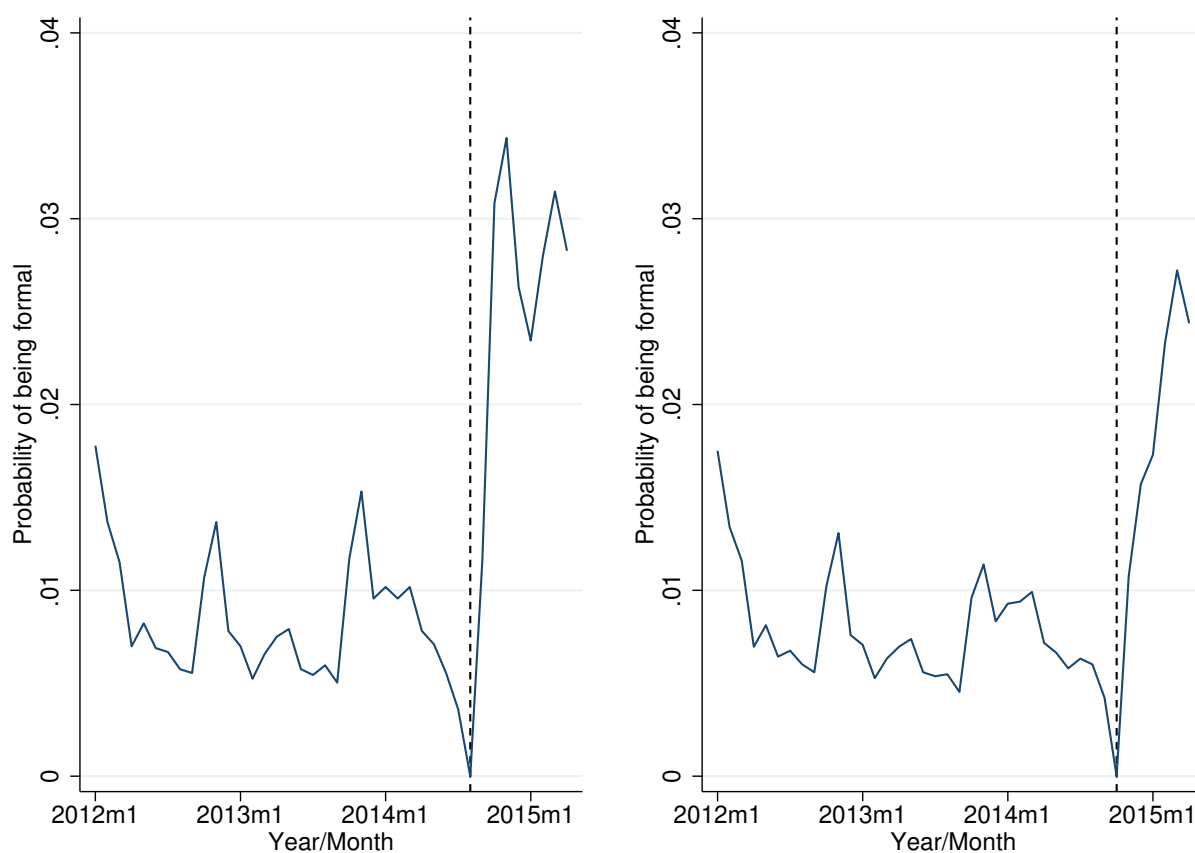| Variables	(1)	(2)	(3)	Mean T=0
Market search	0.054	0.060	0.070	0.289
	(0.031)*	(0.031)*	(0.031)**	
Friends or relatives	-0.112	-0.129	-0.138	0.576
	(0.033)***	(0.034)***	(0.034)***	
Program	0.005	0.007	0.006	0.026
	(0.011)	(0.011)	(0.011)	
Temporary employment	-0.004	-0.004	-0.004	0.004
	(0.003)	(0.003)	(0.003)	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that is worth 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that is worth 1 for those surveyed by telephone, and 3 dummies for educational level. Regressions are performed on 935 (column 1) and 891 observations (columns 2 and 3). Source: Own elaboration based on Encovi.

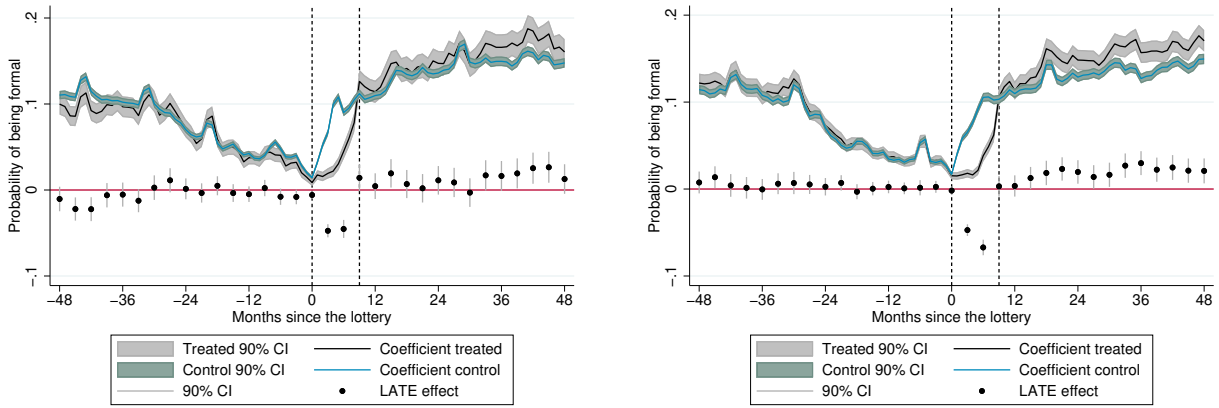Figure A.1: EVOLUTION OF PROBABILITY OF BEING FORMAL BY GROUP INCLUDING UT FUNCTIONAL LINK

Notes. This figure show the results of monthly OLS regressions with ITT as dependent variable and a dummy that takes the value 1 if the worker was formal that month as independent variable. The sample includes the universe of individuals registered for the lottery who met the conditions. The solid black and light blue lines report the treatment and control mean for originally assigned to treatment and control individuals. The dots report ITT effect estimates with their confidence intervals (vertical lines). In this figure, we include individuals participating in the program as formals employees, because their participation generates retirement contributions and maternity and sickness allowances. Source: own elaboration based on administrative records.

Figure A.2: PLACEBO: MECHANICAL EFFECTS OF ELIGIBILITY CONDITIONS ON FORMALITY

Notes. Blue lines are the evolution of the probability of being formal for individuals who participated in the lottery for the 2017 edition. In the first panel, we restrict the sample to individuals who meet the eligibility conditions of the program in August 2014, while in the second panel we do the same but checking the conditions in October 2014. In both cases, we restrict the analysis to a period that ends 2 years before the eligibility conditions are checked in 2017 and that begins at least 2 years before January 2012, which is the first month for which we have information. Source: own elaboration based on administrative records.

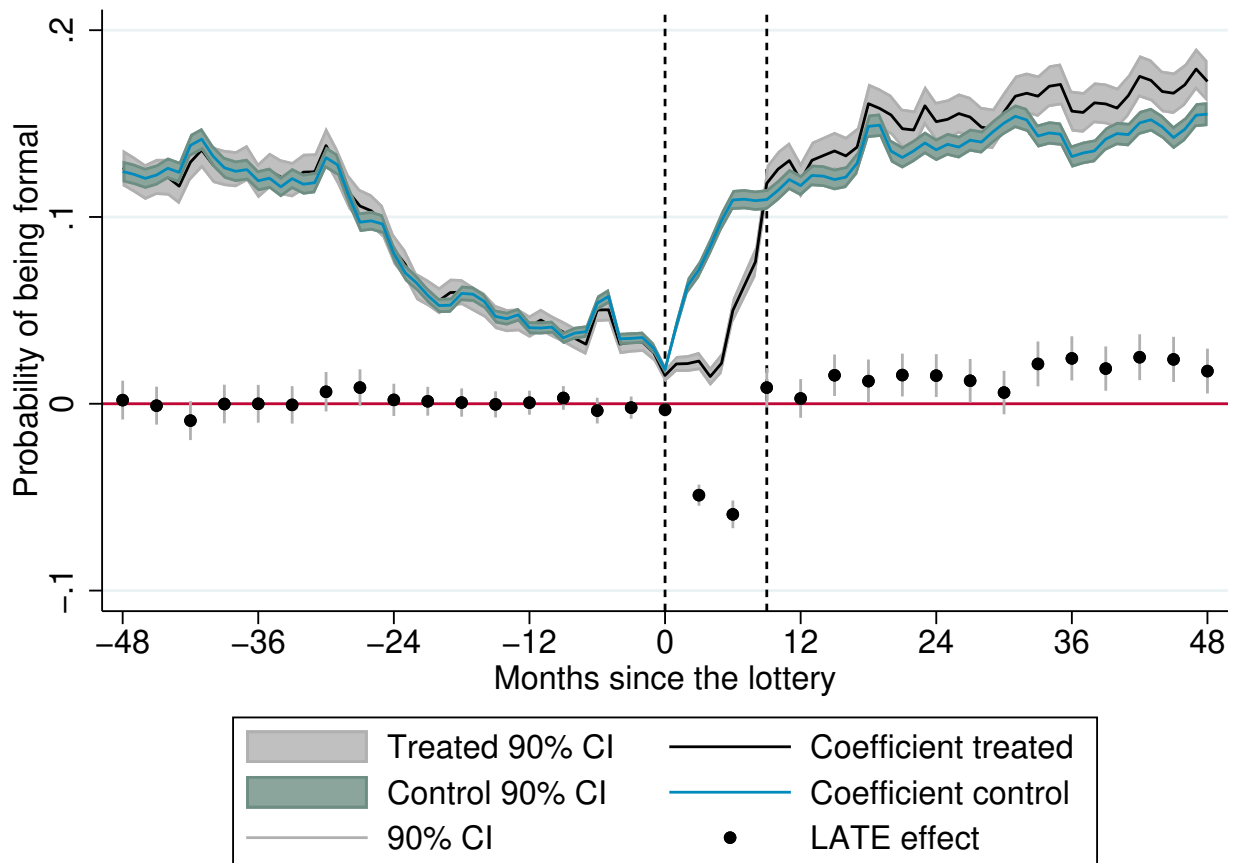Figure A.3: Evolution of probability of being formal by year

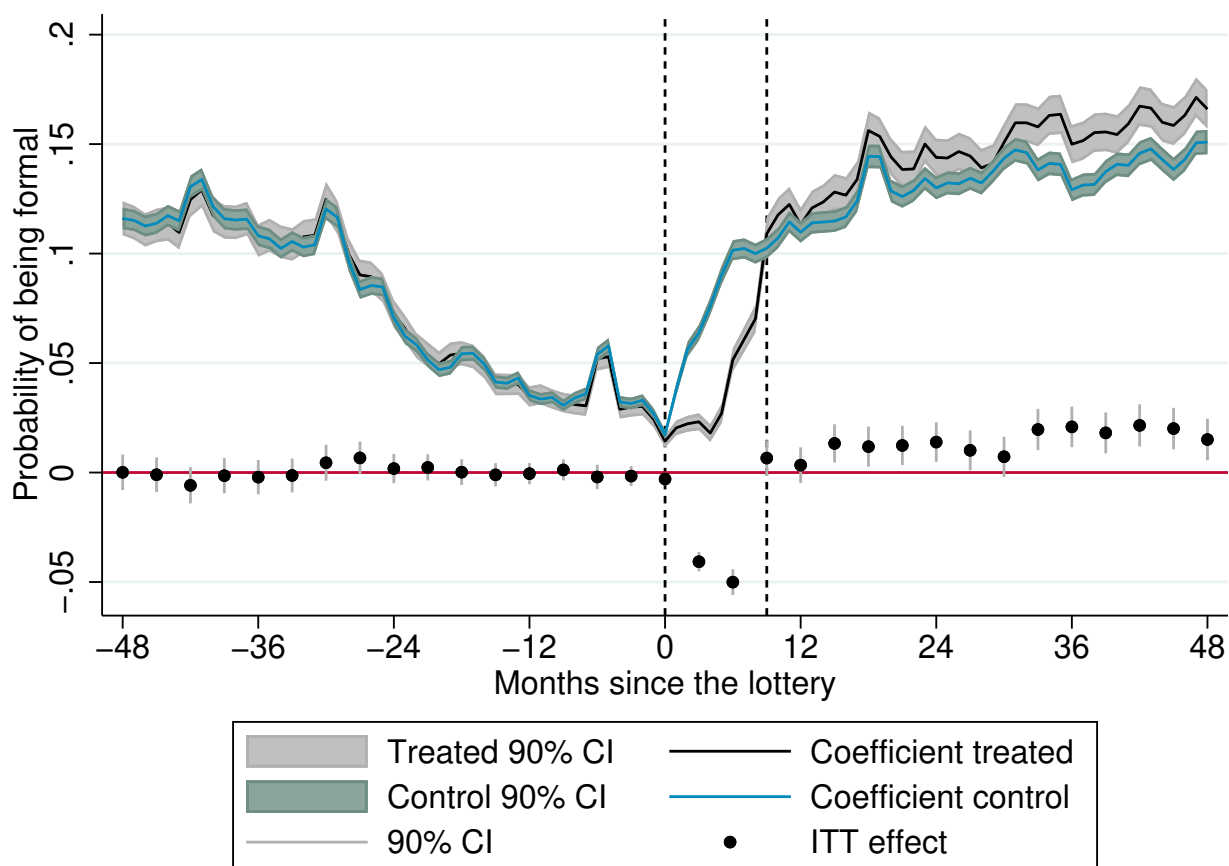(a) Edition 2016   (b) Edition 2017

Notes. This figure replicates Figure 3, but for each edition separately. Source: own elaboration based on administrative records.

Figure A.4: Evolution of probability of being formal by group - one application per individual
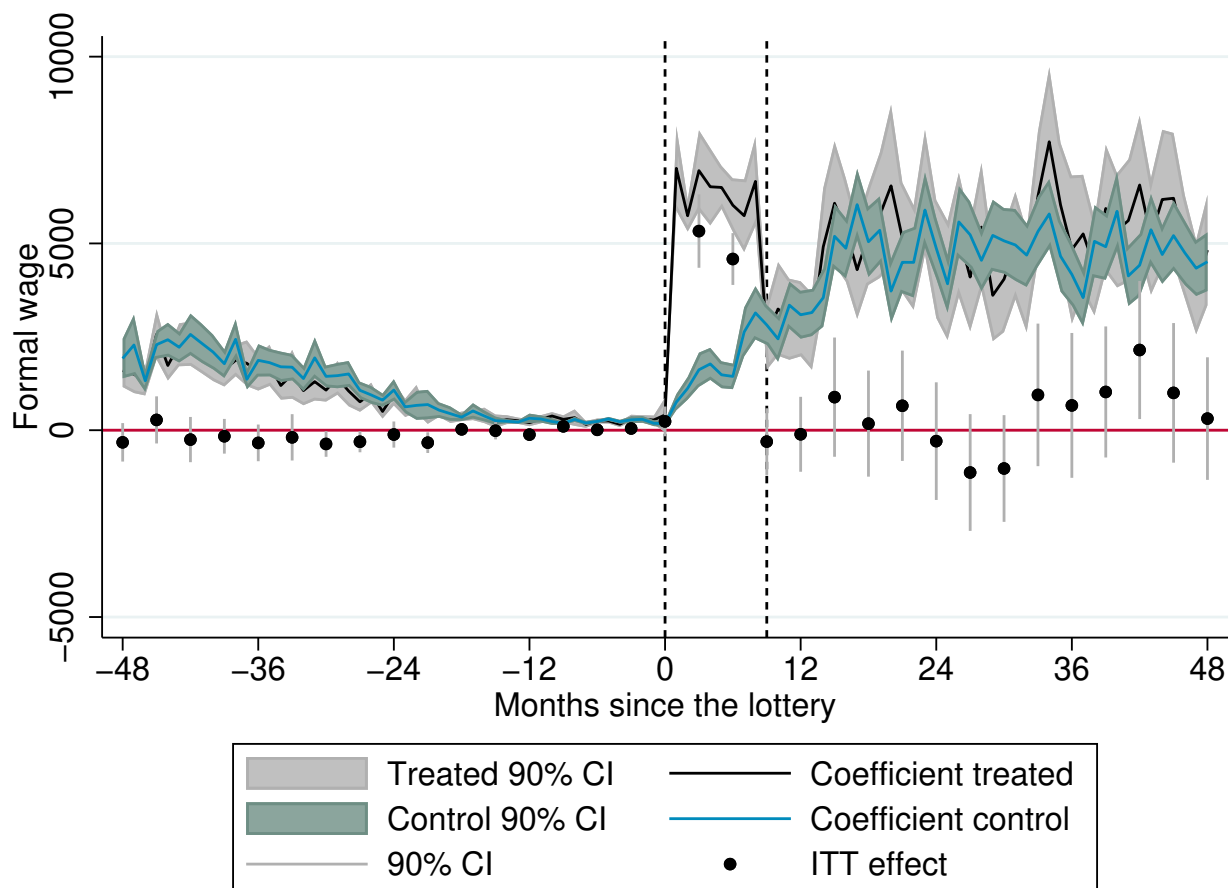

Notes. This figure replicates Figure 3, but eliminating the second application of individuals who applied twice. Source: own elaboration based on administrative records.

Figure A.5: EVOLUTION OF PROBABILITY OF BEING FORMAL BY GROUP

Notes. This figure show the results of monthly OLS regressions with ITT as independent variable and a dummy that takes the value 1 if the worker was formal that month as dependent variable. The sample includes the universe of individuals registered for the lottery who met the conditions. The solid black and light blue lines report the treatment and control mean for originally assigned to treatment and control individuals. The dots report ITT effect estimates with their confidence intervals (vertical lines). Source: own elaboration based on administrative records.

Figure A.6: EVOLUTION OF THE FORMAL WAGE BY GROUP

Notes. This figure show the results of monthly OLS regressions with ITT as independent variable and the formal wage as dependent variable. The sample includes the universe of individuals registered for the lottery who met the conditions. The solid black and light blue lines report the treatment and control mean for originally assigned to treatment and control individuals. The dots report ITT effect estimates, with their confidence intervals (vertical lines). Source: own elaboration based on administrative records.

B Appendix B: Pre-pandemic effects and changes in relation to the time of the survey

Considering the particularity of the period in which the survey was carried out as a result of the pandemic, questions were asked regarding the participants' relationship with the labor market prior to the pandemic´s start, specifically in March 2020.

Table B.1 (and Table B.2 for ITT) shows the main results for survey questions that reflect the respondent's status as of March 2020. The results are consistent with what was found for the main specifications. In fact, the impact on formality is of greater magnitude when looking at March 2020, with the treated group reaching a formality rate 60% higher than for control groups. Looking at the results at the time of the survey and at the pre-pandemic time together, it is interesting to note that the effects of the program on formality persist even after a shock of this magnitude.

Table B.1: Effect of UT on the situation in the labor market prior to the pandemic and changes in relation to the time of the survey (LATE)

Variables	(1)	(2)	(3)	Mean T=0
Employed	0.005	0.011	0.021	0.408
	(0.026)	(0.026)	(0.026)	
Self-employed	-0.060	-0.053	-0.045	0.272
	(0.035)*	(0.036)	(0.036)	
Formal	0.175	0.167	0.171	0.232
	(0.039)***	(0.040)***	(0.039)***	
Keep the same job	0.072	0.077	0.063	0.596
	(0.040)*	(0.041)*	(0.040)	
Stopped being employed	-0.056	-0.049	-0.047	0.212
	(0.032)*	(0.032)	(0.031)	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. The regressions in the first row are performed on 2078 (column 1) and 1983 observations (columns 2 and 3). Source: Own elaboration based on Encovi.

Table B.2: EFFECT OF UT ON THE SITUATION IN THE LABOR MARKET PRIOR TO THE PANDEMIC AND CHANGES IN RELATION TO THE TIME OF THE SURVEY (ITT)

Variables	(1)	(2)	(3)	Mean T=0
Employed	0.004	0.009	0.018	0.413
	(0.022)	(0.022)	(0.022)	
Self-employed	-0.050	-0.043	-0.037	0.265
	(0.029)*	(0.030)	(0.030)	
Formal	0.145	0.137	0.141	0.238
	(0.032)***	(0.033)***	(0.032)***	
Keep the same job	0.060	0.063	0.052	0.609
	(0.033)*	(0.034)*	(0.034)	
Stopped being employed	-0.047	-0.040	-0.039	0.202
	(0.026)*	(0.027)	(0.026)	
Baseline controls	No	No	Yes	
Sample	Total	Net	Net	

Notes. Controls are sex, age, a dummy that takes the value 1 for those from Montevideo, fixed effects for program edition and for month of survey, a dummy that takes the value 1 for those surveyed by telephone, and 3 dummies for education level. The regressions in the first row are performed on 2078 (column 1) and 1983 observations (columns 2 and 3). Source: Own elaboration based on Encovi.