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The Effects of a Youth Training Program on Youth Turnover in Brazil[1]

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Abstract

Large youth unemployment rates are a structural characteristic of most economies. In Brazil this problem is accompanied by very high youth labor turnover rates, which suggests that the job destruction margin plays an important role in explaining high unemployment rates for this age group. Our main objective in this paper is to provide a first evaluation of a large subsidized training program (*Lei do Aprendiz,* Apprentice's Act) targeted to young workers. The program was introduced in 2000 in Brazil with the intention to help the placement of young workers and their attachment to formal jobs. We make use of a huge longitudinal dataset (*Rais, Relatório Annual de Informações Sociais*), based on administrative data collected by the Labor Ministry, that contains information on the employment histories of all formal workers in Brazil from 2001 to 2006. We attempt to measure the impact of the program on five different outcomes that represent formal labor market attachment and remuneration, using other temporary workers as a control group. We employ three distinct estimation procedures to deal with self-selection in program participation that exploit a discontinuity in its age requirement: 18 year olds could not participate in the program between 2000 and 2005. We use a standard 2SLS, which we denote as parametric IV and two recently proposed estimators: i) a semi-parametric IV due to Battistin and Rettore (2008), and ii) the adjusted matching estimator proposed by Dias *et al.* (2010) that corrects the standard matching approach with an IV estimated correction term based on a sharp observed cutoff criterium. Our results indicate that, compared to other temporary workers, apprentices have a higher probability of getting a formal job in the years after the program and a higher probability of getting a non-temporary contract. On the other hand, our estimates suggest that treated workers get jobs with lower tenure than other temporary workers. We also find positive (and significant) effects on wages but very small in magnitude. These results are robust, holding for the whole set of estimation procedures.

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1. Introduction

One of the main distinguishing features of the Brazilian labor market is its impressively high job and worker turnover rates (World Bank, 2002; Gonzaga, 2003; Corseuil *et al.*, 2003). We know that the contribution of some demographic groups, especially young workers, is quite significant to high turnover (see Maloney, 2003, and more recent evidence in a companion paper).

On the other hand, one of the most worrisome and widespread stylized facts in Labor Economics is the observation of very low employment rates for young workers, usually resulting in very high unemployment rates.[2] In all countries, there is evidence that unemployment rates for the 16-24 year-old age bracket are much higher than for other age groups.[3] In many countries, this is the age group that is taking most of the burden in the current recession (Bell and Blanchflower, 2010), which brings the issue to the forefront of policy debate (OECD, 2010). In Brazil, according to PNAD-2009, the unemployment rate for 15-24 year olds reached 18.9%, while the rates observed for ages 25-49 and 50+, were respectively, 7.1% and 3.7%.[4] Hence understanding what drives the attachment of young workers to formal jobs seems to be a promising path to reduce both youth turnover and unemployment rates.

The main goal of the paper is to evaluate a very large youth program in Brazil, the Apprenticeship program (*Lei do Aprendiz,* Apprentice's Act). This is a targeted active labor market program conducted by the Labor Ministry, which concedes payroll subsidies to firms that hire and train young workers under temporary contracts. The program intends to provide training to young workers and help them to successfully complete the transition from school to work. One of its main objectives is to place participants in good (formal) first jobs and, hopefully, help them to stay in the formal segment of the labor market. In this paper, we investigate whether the apprenticeship program increased the labor market attachment of participants.

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[2] In general, high unemployment rates for young workers can occur either because of a low exit flow from unemployment or from a high entry flow into unemployment. We suspect that a high entry flow into unemployment resulting from large turnover rates is the main determinant of high unemployment rates for young workers, especially in a developing country like Brazil.

[3] In 2009, the ratio of young workers' unemployment rates (15-24 year old workers) over aggregate unemployment rates for a large sample of countries was around 2. The following ratios were observed in 2009: 2.04, on average, for the OECD countries; 2.14, on average, for UE-15; 1.9 in the U.S.; 2.09 in Brazil; 1.93 in Mexico; 2.3 in Chile; 1.8 in Colombia.

[4] This seems to be a structural phenomenon in Brazil. The respective averages for the three age groups for the 1992-2009 period were 17.1%, 6.6%, and 3.6%.

For a developing country like Brazil, the relevance of a job training program targeted to the young population is compounded by the fact that the average level of education of the young labor force is very low. For most people, the decision of remaining at school by the age 16, for example, depends on the trade-off between working and continuing formal education in low-quality schools. For poor families, the opportunity cost of going to school is too high.

Moreover, getting a job, especially a formal job with a working card, typically depends on previous experience and references from former employers. Although the labor legislation foretastes a probationary period of three months without firing costs, most employers are reluctant to formalize contracts with young workers. This creates a vicious circle for these workers who do not get good job offers because of no previous experience, which is hard to attain because they do not get job offers.

This is a well-known phenomenon, but in a country like Brazil it usually results in a bad equilibrium, with large rates of school dropouts and very low formal employment rates. The low levels of education tend to perpetuate this problem, acting as a barrier to investment in training by eventual employers given its very low expected returns. The result is a labor market with a large number of workers trapped in low-paying jobs, mostly informal, with a very little chance of promotion and future real wage increases.

It is in this general context that we evaluate the Apprenticeship Program. In fact, the objective of the program is to allow a significant number of young workers to exit the trap of bad (informal) jobs / low human capital described above.[5]

Our identification strategy exploits a discontinuity by age in the eligibility to the apprenticeship program. From 2000 to 2005, only individuals aged 14 to 17 years old could participate in the program. Individuals aged 18 years old or more were not eligible. This corresponds to the partially-fuzzy regression discontinuity setting discussed in Battistin and Rettore (2008), in which workers aged above the cutoff value cannot, and in fact do not, participate in the program, yet there is imperfect compliance for those below the cutoff. We use three different estimators in the literature that exploit this design: the adjusted matching estimator proposed by Dias *et. al.* (2011), the semi-

[5] Note that the design of the program focuses on human capital investment, which seems to matter most for a developing country with low levels of schooling. By contrast, an apparently successful UK young workers program, the *New Deal for Young People*, focuses on job search assistance and subsidized job placement (Blundell *et al*., 2003).

parametric IV estimator introduced by Battistin and Rettore (2008), and a standard parametric IV (2SLS) estimator. In our context, all three estimators use information on those aged 18 years old that help identify the average impact of the program on the 17 years old youths that choose (or are chosen) to become apprentices in the formal labor market.

To accomplish this objective, we use a huge longitudinal administrative dataset that has information on the whole history of formal jobs for each Brazilian worker: *Relatório Anual de Informações Sociais (RAIS)*, collected by the Labor Ministry. The use of RAIS provides a rare opportunity to observe careers of young workers from the starting point.[6]

The paper is organized as follows. In Section 2, we provide a short literature review on youth-targeted programs. In Section 3, we describe the dataset used in the study, the *Relatório Anual de Informações Sociais (RAIS)*. Section 4 describes the Apprenticeship Program. Section 5 presents and discusses the estimation methods. Section 6 presents preliminary results for evaluating the Apprentice's Act. Section 7 concludes.

2. A Short Literature Review on Youth-Targeted Programs

The literature on labor turnover for young workers has been growing fast. It is well known since at least Farber (1998) that job separations decline with labor market experience and job tenure.[7]

Our paper is more closely related to a strand of the recent literature, which study whether youth-targeted programs affect the career prospects of young workers, in terms of either wage growth or a higher degree of labor market attachment. Card *et al.* (2010) and Kluve (2010) summarized the findings of the evaluation of several active labor market policies (ALMP's) in a large list of countries, through a meta-analytical framework. Both studies concluded that youth-targeted programs are less successful than other types of ALMP's. They also found that ALMP's could have different impacts depending on the time horizon studied. Many training programs, for instance, have

[6] Von Wachter and Bender (2006) and Adda *et al.* (2009) used similar data to study the career paths of young workers in Germany.

[7] Recent evidence for Latin America has been provided by Cunningham and Salvagno (2011), who show that turnover is highest for young workers in Brazil, Argentina and Mexico.

positive effects only 2 or 3 years after the program, which underlines the advantage of using data that allows one to follow workers for a long period after the intervention. In another recent review of ALMP's, Urzua and Puentes (2010) discussed evidence that youth-targeted programs tend to have better results in Latin American countries than in developed countries.

The literature on youth-targeted programs is also increasingly taking advantage of better microdata and methods. Larsson (2003) used propensity score methods and found negative effects of two youth programs (practice and training) in Sweden on earnings and employment one year after the intervention, with most coefficients becoming insignificant two years after the program.

De Giorgi (2005) used a regression discontinuity design exploiting an eligibility rule to evaluate the New Deal for Young People (NDYP), a major youth-targeted intervention in the UK that combines different aspects of ALMP’s (training, subsidized-employment and job-search assistance). He found that the program significantly increased employability of participants. Dorsett (2006) evaluated which of the different aspects of the NDYP program was most effective in reducing unemployment of participants. He found that subsidized employment was the most effective means of exiting unemployment compared to the other options of NYDP.[8]

Centeno *et al.* (2009) evaluated a youth-targeted program implemented in Portugal (*InserJovem*) in the late 1990s. They used a diff-in-diff estimation, exploiting the fact that, apparently for exogenous reasons, the program was introduced only in some regions of the country. They found a very small effect of the program in reducing unemployment duration.

Caliendo *et al.* (2011) investigated the effectiveness of several youth-targeted programs implemented in Germany, based on a matching-on-observables method (Inverse Probability Weighting) applied to administrative data from 2002 to 2008. In general, they found positive effects of most of the several programs on employment probabilities of participants. Wage subsidies were found to have the largest effects in the long term.[9]

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[8] See also Blundell *et al.* (2003) for an early evaluation of the NDYP program, in which a positive effect of the program on reducing unemployment in pilot areas was found.

[9] As in other studies, like Dorsett (2006) for the British NDYP, they found that public-sector job creation programs were harmful to the employment prospects of participants.

Finally, Card *et al.* (2011) investigated the impact of a youth training program recently implemented in the Dominican Republic, based on a random experiment. They found "little indication of a positive effect on employment outcomes and some evidence of a modest effect on earnings".

3. Data Description - *Relação Anual de Informações Sociais (RAIS)*

RAIS is an administrative file maintained by the Brazilian Ministry of Employment and Labor (*Ministério do Trabalho e Emprego - MTE*). All registered tax-paying establishments (plants) must send every year to the Ministry information about every single worker who had been employed by the establishment anytime during the previous year. So, apart from tax/social security compliance the data has no coverage limitation, as opposed to other similar databases that are limited by geographical region, size, or industry.

The RAIS information provides a matched employer-employee longitudinal data set, similar to those available in developed countries. With the establishment identification number (CNPJ) it is possible to follow all establishments that file the RAIS survey over time. Moreover, with the worker's national insurance number (PIS), it is also possible to follow all workers that remain in the formal sector over time and to match the workers' characteristics with those of the establishments, through RAIS. Therefore, we can form a panel that matches workers to their establishments and follows each of them over time.

Apart from worker characteristics (age, education, gender) and establishment characteristics (industry, location at the municipality level) we also have detailed information about employment conditions, such as wage, hours, tenure, month of admission, month of separation, type of separation (retirement, death, among other categories), occupation, type of contract (temporary or not, including whether it was an apprenticeship contract). Concerning wages, there are two distinct pieces of information provided by RAIS: the average value over the year (or the part of the year when the worker was employed) and the December wage.

Throughout the paper we make use of longitudinal data built from RAIS. Therefore attrition is a crucial issue for our analysis. On average, RAIS attrition rate in any two consecutive years from our sample period is approximately 5%.[10] One of the main sources of attrition in RAIS is due to occasional non-declaration by complier establishments. We identified several cases in which all employees from some establishments “disappear” from RAIS in a particular year and eventually return later on. We excluded these episodes of spurious establishments “births” and “deaths” from our sample.

We use data from 2001 to 2006. Over this period RAIS contains an average of 50 million worker-establishment records per year. In our analysis, we restrict attention to workers aged 17 or 18 who entered the formal labor market between 2001 and 2003. We only collect information for those youths that were in a temporary job in the first job and marked those that were under an apprenticeship contract. We followed all workers in our sample for three years (excluding the first), so we are able to compute average program impacts for the medium/long term.

4. The Apprenticeship Program

Although the Apprenticeship program has been part of CLT (*Consolidação das Leis Trabalhistas*, the Brazilian labor legislation code) since 1943, it had a very small scope until December 2000, when Law 11,180, the Apprentice Law, was enacted. The program was initially designed for individuals 14 to 17 year olds. It was regulated in December 2005 through a more detailed legislation (*Decreto-Lei 5598*), when the maximum age for participation was increased from 17 to 23 year olds. By mid-2010, around 250,000 thousand workers had jobs under the program. The current Dilma Rousseff government works with a target of expanding it to 1 million young workers.

Young workers hired under the Apprenticeship program are required to enroll at school if still at primary school, and to take formal training, either through official qualification agencies, the so-called *Sistema S (Senai, Senac, etc.),* or at ONGs that specialize in such training that are certified by the Labor Ministry.

The maximum number of working hours allowed for young workers hired under the program is six hours per day. Payments should be at least the hourly minimum wage.

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[10] This is the share of workers not found in the second year despite been registered as employed on the last day of the first year.

There is a payroll subsidy in the form of a lower deposit on the worker's FGTS account (*Fundo de Garantia por Tempo de Serviço*, a job-separation fund). Firms should deposit only 2% of the basic wage on this fund, instead of the rate of either 8% or 8.5% that prevails for other workers.[11] Contracts are temporary with a maximum length of two years. There are no firing costs for job separations by (and before) the end of the contract. This is possibly the main benefit for firms to use this type of contract, since the standard procedure in cases of separations induced by firms is to pay a fine equivalent to 40% or 50% of the accumulated amount of money in the FGTS account during the respective employment relation.[12]

Firms' choices regarding the use of apprenticeship contracts are restricted by the following rule. A minimum of 5% (and a maximum of 15%) of the labor force employed in occupations requiring formal training should be composed of apprentices. In the early 2000s, though, firms could claim a lack of training agencies in the region/occupation they operate so as not to be punished for employing less than the minimum amount required. Therefore, in practice the minimum threshold was not binding and, especially, small firms tend not to hire workers under the program[13].

5. Methodology and estimation procedures

The impact of the apprenticeship program on youth employability is not trivially identified, since selection into the program is defined by firms and workers, and hence may be driven by non-observable characteristics. If these non-observable characteristics are not balanced among treated and non-treated workers, then methods relying solely on the contrast of the outcome between the two groups will produce misleading estimates.

[11] The standard deposit rate for FGTS used to be 8% until October 2001, when the government introduced a "temporary" increase of 0.5 percentage points that lasted until the end of our sample period.

[12] The fine (to be paid to the worker) was 40% until October 2001, when it was permanently increased to 50%, with the additional 10 percentage points to be paid to the government. It should be noted that (rarely issued) just-cause firings bear no costs for firms.

[13] Note that small firms tend to be overrepresented in remote places with lower supply of training agencies and lower enforcement of labor laws.

In order to get consistent estimates of the effect of the apprenticeship program we make use of a set of three somewhat related estimators. In all three cases we exploit the fact that the eligibility to the program switches as age crosses a threshold value.

The first is an estimator recently proposed by Dias *et al.* (2010), which combines the idea of matching on observables with exogenous variation provided by an instrument. The second is a semi-parametric version of the IV estimator applied to the context of a partially-fuzzy design as discussed by Battistin and Rettore (2008). The third is the traditional IV estimator, or 2SLS, also applied in a fuzzy design as discussed in Hahn, Todd and Van Der Klaawu – HTV (2001).[14]

We are always able to identify and estimate a version of the ATT parameter regardless of the procedure we choose. This is the case even when using the IV estimators, which is usually associated to the LATE parameter in program evaluation. The reason is that by design those above the age-threshold cannot and do not participate in the program. In this situation the group of always-takers does not exist, implying that the treated group coincides with the complier group, the one for whom the effect is identified in the LATE parameter.[15] In what follows we describe these estimators.

5.1. Adjusted Matching

In its ideal setting, the Dias *et al*. (2010) estimator uses an instrument which exploits boundary restrictions on eligibility rules based on individual characteristics (e.g., age, education, income). In this context, the instrument should drive participation into the program to zero for certain values of its domain and at the same time allows partial compliance for other values.[16] The idea is that by moving individuals in and out of the

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[14] HTV relates the set of identification conditions in this context with those prevailing for the estimation of the LATE parameter, which in turn was proposed by Angrist and Imbens (1994). A summary on these topics can be found in Angrist and Pischke (2009).

[15] It is worth noting that in the setting of regression discontinuity design, as in the fuzzy designs we are dealing with in two of our estimators, identification is still local not because of the restriction to compliers but because of the validity around an age threshold. So we end up estimating a parameter that may be called a local average treatment on the treated (LATT).

[16] The Dias *et al.* (2010) approach is related to the partially-fuzzy regression discontinuity design proposed by Battistin and Rettore (2008). One of the main differences between the two approaches is that the former explicitly requires an exclusion restriction in the form of an instrument, while the latter is based on an assumption of continuity near the cutoff point. Also, while the latter approach is cast in terms of the LATE (Local Average Treatment Effect), the former is cast in terms of the ATT (Average Treatment on the Treated).

program the variation in the instrument can correct for possible unbalances in unobservables due to self-selection into the program. Note that the standard matching (on observables) method does not take care of such unbalance.

To be more formal, we are interested in estimating the Average Treatment on the Treated (ATT) parameter:

$$\alpha = E[Y1|D = 1] - E[Y0|D = 1] = E_{X|D=1}E[Y1|X, D = 1] - E_{X|D=1}E[Y0|X, D = 1],$$

where Y1 and Y0 represent individual potential outcomes associated with assignment to treatment and non-treatment, respectively, D measures the actual treatment status, with D = 1 (D = 0) corresponding to actual participation (non-participation) in the program, and X is a vector of conditioning covariates. The notation $E_{X|D=1}$ means expectation over the X distribution for the D = 1 population.

The object $E_{X|D=1}E[Y1|X, D = 1]$ can be directly computed from the data through the mean of the outcome of interest among the treated group. However, as usual, the counterfactual object $E_{X|D=1}E[Y0|X, D = 1]$ is not directly available in the data, so it needs to be identified through the use of some assumptions. Dias *et al.* (2010) propose an estimator of the counterfactual object based on the existence of a variable Z for which two features are assumed to apply:

A1: $Y0 \perp Z|X$;

A2: There exists a set of points $\{z^{*}, z^{**}\}$ in the domain of Z where for all X:

$P\big[D = 1\big|X, Z = z^{*}\big] = 0$ and $0 < P\big[D = 0|X, Z = z^{**}\big] < 1$.

The first assumption is an exclusion restriction that imposes that the variable Z is not correlated with the counterfactual outcome Y0 after conditioning on the covariates in X.[17] Assumption 2 requires the existence of at least one value of Z that is capable of driving participation into the program to zero and at least another value for which participation is non-deterministic. It is interesting to note that these assumptions do not impose that there is no selective participation into the program. Indeed, they allow D to be correlated with Y0 when Z takes on the value z** (after conditioning on X).

[17] In fact, that condition could be stated in terms of mean (conditional) independence.

Using A1 and A2, Dias *et al.* (2010) propose a constructive proof for the identification of the mean counterfactual outcome $E[Y0|X, D = 1]$.[18] They show that this object can be written as

$$E[Y0|X, D = 1] = E[Y0|X, D = 0] + \frac{E[Y0 \mid X, Z = z^*, D = 0] - E[Y0|X, D = 0]}{1 - P[D = 0|X]}$$

This expression shows that $E[Y0|X, D = 1]$ is equal to the mean outcome $E[Y0|X, D = 0]$, typically computed in matching estimation, plus what the authors call a correction term, which is given by the second term in the right hand side (RHS) of the equation. Note that all elements that compose this second term can be identified from the data, where $E[Y0 \mid X, Z = z^*, D = 0]$ is the mean observed outcome for ineligibles controls at given X and $\{1 - P[D = 0|X]\}$ is the propensity score. The object of interest $E[Y0|D = 1]$ is finally identified from $E[Y0|X, D = 1]$ by averaging the latter over the distribution of X for the treated group (D = 1).

To implement their estimator, the Z variable is age. This choice fits well in the ideal setting for the application of the Dias *et al.* (2010) estimator, since the eligibility rules of the Apprenticeship program impose a restriction on the maximum age for participation. As described before, the maximum age was 17 years old until September 2005, when the age restriction rose to 23. Recalling that the program is not compulsory, we have thus an appropriate setting in which the age of workers can be used as the instrument: while those aged above the cutoff value cannot participate, there is imperfect compliance for those below the cutoff.

The estimation results will be presented for both the standard propensity score matching estimator and what Dias *et al.* (2010) call the adjusted matching estimator. The covariates in X we use in the propensity score are gender, dummies for the schooling category of the worker, dummies for broad industry, dummies for the occupation of the worker, dummies for geographical region, and dummies for the year in which the worker first entered the formal sector. The standard matching estimates were computed using Epanechnikov kernel weights and only observations in the region of the common support of the propensity score were used for computing the standard and adjusted

[18] The proof can be found in Appendix 1.

matching estimates.[19] Inference is based on standard errors estimated from bootstrap with 100 replications.

5.2 Semi-parametric IV

The age cutoff condition for eligibility in the Apprenticeship program fits directly into a framework of regression discontinuity design (RDD). In particular, it fits well the framework put forward by Battistin and Rettore (2008), where on one side of the eligibility threshold individuals are precluded from participating, while on the other side eligible individuals may self-select into the program. In fact, the main idea behind their estimator exploits the imposition of the non-participation condition near the threshold for eligibility to solve the selection problem. In our context, this implies that those aged 18 years old will help identify the average impact of the program on the 17 years old youths that choose (or are chosen) to become apprentices in the formal labor market. The fact that their framework is based on a design where on one side of the cutoff point there are ineligibles and on the other side there are eligible participants and eligible non-participants configures what the authors call a partially fuzzy design.[20]

To see how Battistin and Rettore's (2008) estimator operates, let program eligibility be defined by an observable, continuous variable Z[21] (age in our case). Let $\bar{z}$ be the value of Z that defines the eligibility threshold, that is, the discontinuity point in the domain of Z below which individuals can participate in the program. Let $\bar{z}^{-}$ and $\bar{z}^{+}$ refer to the groups of individuals that are marginally below and above the cutoff point of eligibility, respectively. In our estimation, they will be represented by workers with 17 and 18 years old, respectively.

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[19] Since the denominator of the correction term of the adjusted matching estimator is the estimated propensity score, estimates of the correction term can become quite imprecise for low values of the propensity score. Hence, following a suggestion in Dias *et al.* (2010), we asymmetrically trimmed the common support interval to be between the maximum of the 5th percentiles and the minimum of the 99th percentiles of the propensity score distributions of the treated and control groups.

[20] Typically in the RDD literature there are two types of designs: i) the sharp, where the probability of participation in the program changes from zero to one as the value of the eligibility variable crosses the threshold; and ii) the fuzzy design, where the change in the participation probability is less than one at the discontinuity threshold. The partially-fuzzy design combines features of these two designs. Classic recent references in the RDD literature are Hahn *et al.* (2001) and van der Klaauw (2002).

[21] In their paper this variable is denoted by "s". We use "z" to be consistent with the notation in the previous section.

Using the notation from the previous section, if $\alpha = Y1 - Y0$ denotes the impact of the intervention, our interest centers in identifying the average treatment on the treated effect (ATT): $E[\alpha|D = 1]$, where $D = 1$ denotes program participation. Using the usual counterfactual notation, the observed outcome of any individual in the population can be written as

$$Y = Y0 + D(z)\alpha,$$

where $D(z)$ is an indicator function of treatment status which explicitly recognizes that it depends on the variable Z.

Consider the difference in mean outcomes $E[Y|\bar{z}^-] - E[Y|\bar{z}^+]$. Using the previous expression, this difference can be rewritten as

$$E[Y|\bar{z}^-] - E[Y|\bar{z}^+] = E[Y0|\bar{z}^-] - E[Y0|\bar{z}^+] + E[D(s)\alpha|\bar{z}^-] - E[D(s)\alpha|\bar{z}^+].$$

By construction of the program design, those that are marginally ineligibles cannot participate (those who are 18 years old in our context). Hence, $D(\bar{z}^+) = 0$ and the previous expression becomes:

$$E[Y|\bar{z}^-] - E[Y|\bar{z}^+] = E[Y0|\bar{z}^-] - E[Y0|\bar{z}^+] + E[D(z)\alpha|\bar{z}^-].$$

The only condition needed to identify a local version of the parameter of interest is:

C1: $E[Y0|Z]$ is a continuous function of Z at $\bar{z}$.

This assumption, which is the main condition for identification of the mean impact of treatment for those at $\bar{z}^-$ in the usual sharp RDD, simply requires that there is no discontinuity in counterfactual outcomes at the threshold for eligibility. It is typically considered a weak condition.

Noting that $E[D(z)\alpha|D = 0, \bar{z}^-] = 0$, we can write the last term of the previous expression simply as $E[D(z)\alpha|\bar{z}^-] = E[\alpha|D = 1, \bar{z}^-].P[D = 1|\bar{z}^-]$. Now, by condition C1, $E[Y0|\bar{z}^-] = E[Y0|\bar{z}^+]$, so the parameter of interest can be locally identified for individuals with $Z = \bar{z}^-$ (the group of 17 years old) by

$$E[\alpha|D = 1, \bar{z}^-] = \frac{E[Y|\bar{z}^-] - E[Y|\bar{z}^+]}{P[D=1|\bar{z}^-]}$$

Notice that all objects in the RHS of this expression can be computed from the data. In particular, the denominator can be seen as the propensity score for participation for those marginally eligible. In practice, it has been calculated for this group using the same propensity score that was estimated for the adjusted-matching estimator of the previous section.[22] For comparison purposes, inference was based on the same 100 bootstrap replications that were used in the computation of the adjusted-matching estimator. We also computed the partially-fuzzy estimator using the same common support of each replication of the adjusted matching estimator.

5.3 Parametric IV

As P[D=1|$\bar{z}^{+}$] = 0 in our context, expression (1) above can be re-written as:

$$E[\alpha|D=1,\bar{z}^{-}] = \frac{E[Y|\bar{z}^{-}] - E[Y|\bar{z}^{+}]}{P[D=1|\bar{z}^{-}]} = \frac{E[Y|\bar{z}^{-}] - E[Y|\bar{z}^{+}]}{P[D=1|\bar{z}^{-}] - P[D=1|\bar{z}^{+}]}$$

The last term is the traditional formula for the fuzzy regression discontinuity identification strategy, which in turn motivates the use of 2SLS estimation procedures by applied economists. Therefore we also use this estimator for the sake of comparability with a standard framework to deal with self-selection issues. We apply it in a fully parametric 2SLS framework, where a dummy for being 17 years old is used as the instrument for the apprentice's treatment dummy.

6. Descriptive Statistics and Econometric Results

6.1 Descriptive Statistics

One way or another, all methods we use in the paper are based on the partial participation of youths under 17 years old and the non-participation of youths over 18 years old. To confirm that this is the case, we present Figure 3, which shows the participation rate in the Apprentice's program by age for the period 2001-2003. As it reveals, although the probability of participation declines for eligibles, it is always positive below the 17 years old cutoff and becomes virtually zero for youths older than

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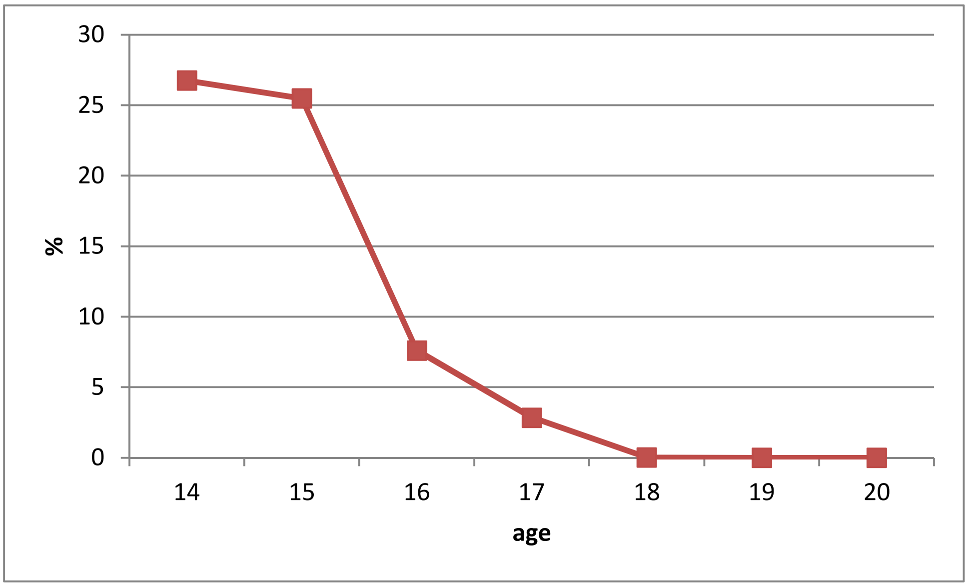
[22] *Mutatis mutandis,* all objects presented in this section could be conditioned on the vector of observable characteristics X without changing the essence of the identification of the object of interest.

this threshold. Since the estimators we use are local, we only used information on youths aged 17 and 18 in estimation.

Figure 3: Participation rate in the Apprentice's program by age – 2001/2003



Source: Constructed by the authors based on microdata from RAIS.

Table 3 presents the actual number of workers registered in RAIS as apprentices for all years from 1998 to 2010. The Table reveals that: i) the number of apprentices substantially increased throughout the 2000s; ii) the majority of apprentices are 16 and 17 year olds; iii) the threshold of 18 year olds was respected between 2000 and 2005; and iv) the discontinuity at 24 years old after 2005 is not relevant with only a small number of apprentices aged 23 until 2008.[23]

[23] An informal conversation with an inspector from the Labor Ministry confirmed our prior that at age 23 an apprenticeship job is not attractive for a 23 year old, which explains the declining number of apprentices as they approach that age.

Table 3: Number of Apprentices by Age

Number of Apprentices by Age

Age	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
14	215	82	99	143	582	803	937	1291	1497	2125	2242	2369	2918
15	1063	984	724	1061	2781	5279	7387	8962	11656	13228	16115	16252	20907
16	1616	3145	2917	2684	5747	12365	20341	26060	29776	35100	39100	41787	50723
17	1262	3182	3617	2156	4547	9117	16266	21122	30160	34376	38871	42393	50654
18	70	34	54	76	48	79	121	1392	7052	12972	16317	21968	27846
19	27	1	0	0	0	0	0	246	2521	6687	9565	13145	17758
20	22	0	0	0	0	0	0	125	1396	3560	5804	8250	10464
21	16	0	0	0	0	0	0	72	832	1970	3307	4902	6470
22	4	0	0	0	0	0	0	63	403	1125	1793	2621	3366
23	0	0	0	0	0	0	0	32	193	439	674	1057	1320
24	3	0	0	0	0	0	0	2	6	25	31	51	77
25	2	0	0	0	0	0	0	0	0	0	10	28	35
26	4	0	0	0	0	0	0	0	0	0	9	24	27
27	2	0	0	0	0	0	0	0	0	0	6	30	43
28	1	0	0	0	0	0	0	0	0	1	6	17	25
29	1	0	0	0	0	0	0	0	0	0	9	23	28
30	0	0	0	0	0	0	0	0	0	0	3	18	22
Total	4295	7428	7411	6120	13705	27643	45052	59365	85486	111582	133788	154744	192426

Source: Constructed by the authors based on microdata from RAIS.

The sample used in the regression analysis is composed of youths aged 17 to 18 years old who were temporary workers and who had obtained their first jobs in the formal labor market between 2001 and 2003. All workers in this group are followed for a period of three years after the year of their first appearance in the data.

Table 4 displays the main characteristics of the workers in the sample, where around ¼ is composed of apprentices (11483 out of 44855). Almost half of the sample are 17 years old, around 2/3 are males, the majority has between 9 and 11 years of schooling (which means more than primary school but less than a secondary degree), over 70% were in the sales or service sectors, and about ¾ were living in the Southeast or the South regions, the richest of the country. The year of youth's entrance in the formal sector was not so equally distributed, with the year of 2002 being the year of entrance with less than 1/3 of the sample.

Table 4: Descriptive statistics of the sample

Characteristic	**Proportion**
Apprentice	0.26
17 years-old	0.49
Male	0.66
Schooling (years)	
Less than 5	0.10
Between 6 and 8	0.18
Between 9 and 11	0.51
More than 12	0.20
Industry	
Agriculture	0.16
Construction	0.02
Manufacturing	0.04
Sales	0.10
Service	0.62
Region	
North	0.03
Northeast	0.13
Southeast	0.62
South	0.15
Midwest	0.06
Year of entrance	
2001	0.35
2002	0.28
2003	0.38
Sample Size	44855

Source: Authors' calculations based on microdata from RAIS.

6.2 Econometric Results

We focus on five outcomes of interest in subsequent years following the treatment period. Specifically, letting *t* represent the year in which treatment started, our response variables of interest are: (i) the probability of obtaining a job between years t+2 and t+3, (ii) the probability of being employed in a non-temporary job between years t+2 and t+3, (iii) the accumulated number of months worked in the formal sector within years t+2 and t+3, and (iv) two wage variables, one for the percentage variation in hourly wages between years t+2 and t, and another for the percentage variation between years t+3 and t. All these outcomes try to capture what can be seen as medium term effects of the program.[24]

Table 5 presents the estimation results for all three estimation procedures. The first two columns display the standard matching (for comparison purposes) and the adjusted matching estimates, respectively. The following three columns present the semi-parametric IV, the parametric IV, and first stage statistics of this last procedure to evaluate instrument weakness.[25]

In the first row we report our results for the impact of the program on the probability of obtaining a formal job within the second or third year after the year of the program. Looking at the standard matching estimate, this impact seems to be positive. The adjusted matching estimate point to the same direction although with a lower magnitude[26]. This suggests that selection biases the standard matching results upwards, meaning that more employable young workers were allocated to apprenticeship contracts. The positive and significant impact of the program on the probability of being employed is also observed for the other two estimators. In fact, the magnitudes of all our estimates for this first outcome are rather similar across estimation procedures, being around 6% on average and falling within the interval between 4% (adjusted matching) and 9% (semi-parametric IV). This means that a random treated youth has an employment rate two or three years after the program which is 5 percentage points

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[24] Because it is possible that treated individuals keep working as apprentices during year t+1, we opted to evaluate the impact of the program on employability from year t+2.

[25] We have also computed the correction term of the adjusted matching method, with its respective standard errors. Results are available upon request.

[26] In this case the correction term was statistically significant, which suggests that there is selection (on unobservables) into the program.

higher than that of a similar non-treated youth that also entered the labor market with a temporary contract.

The evaluation of the first stage estimates lessens the worries of weak instruments. Indeed, the treatment variable seems to be highly correlated with the instrumental age variable, the instrument is significant, the first stage has a reasonable R^2, and the F statistic is quite large. These first stage results are the same for the other employment variables and qualitatively similar for the wage outcomes.[27]

A positive result also comes out when we look at the second outcome variable, which is the probability of having a non-temporary job in the same time period considered for the first outcome variable. The results for this outcome variable are displayed in the second row of table 5. Once again, the adjusted matching estimate indicates a positive and significant impact. The standard matching effect is also positive but now slightly lower in magnitude.[28] Here, again, the semi-parametric and parametric IV estimates point to the same positive effect of the training program on the likelihood of obtaining a non-temporary job between years t+2 and t+3. The magnitudes do not vary too much, fitting in the interval from 0.05 (parametric IV) to 0.11 (semi-parametric IV).

In the third row of table 5, we show the estimates of the program effect on the formal labor market experience accumulated between the second and third year after the program. Now the adjusted matching is negative and statistically significant at conventional levels. Comparing with the standard matching result, one can see that correcting for selection on unobservables changes the magnitude of the result, this time by a large amount. Once again, the other estimation procedures point to quite similar results.

[27] The sample used to estimate the impact for the wage variables is composed only of youths that were employed in years t+2 or t+3.

[28] This was the only outcome for which the correction term was not statistically significant. Therefore, if based in this outcome, we cannot reject the hypothesis of no selection into the apprenticeship contract.

Table 5: Estimates of the Average Treatment on the Treated parameter for the Apprenticeship program

Outcome	Standard matching (1)	Adjusted matching (2)	IV Semi-Parametric (3)	Parametric IV (2SLS) (4)	1st Stage (5)
Employment probability in years t+2 or t+3	0,0597 (0.0048)	0,0356 (0.0085)	0,0826 (0.0097)	0,0501 (0.0083)	0,4391 (0.0034) [0.2663] {0.0000}
Probability of having a non-temporary job in years t+2 or t+3	0,0642 (0.0069)	0,0754 (0.0176)	0,1113 (0.0180)	0,0486 (0.0110)	0,4391 (0.0034) [0.2663] {0.0000}
Accumulated number of months worked between years t+2 and t+3	-0,3505 (0.1105)	-1,8125 (0.3308)	-2,3655 (0.3372)	-2,4778 (0.1996)	0,4391 (0.0034) [0.2663] {0.0000}
Wage variation between years t+2 and t	0,739 (0,0539)	1,5229 (0,1690)	1,9137 (0,1887)	1,1694 (0,1241)	0,4326 (0,0044) [0,2720] {0,0000}
Wage variation between years t+3 and t	0,9026 (0.0977)	1,7022 (0,2539)	2,3837 (0,2894)	1,3759 (0,1429)	0,5416 (0,0060) [0,3328] {0,0000}

Notes: Standard matching refers to propensity score matching based on an Epanechnikov kernel with a bandwidth of 0.02. Adjusted matching employs the correction term proposed in Dias et al (2010). The IV semi-parametric is based on Battistin and Rettore (2008). The instrument for all estimates is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Standard errors for these estimators were computed from bootstrap with 100 replications. Standard errors are displayed in parentheses. For the parametric IV we also show some statistics for the first stage in the fifth column. The first figures in this column are for the coefficient estimate and the corresponding standard error of the instrument. The numbers in square brackets refer to the partial R^2, and the figures in curly brackets are the p-values of the F tests of the first-stage regressions.

Regarding the wage outcomes, the impact of the program is positive and statistically significant for all estimation procedures we use. The effect size is quite small, though, being around 1.5 (1.8) percentage points for the hourly wage variation between years t+2 (t+3) and t. These results are similar across estimation methods for each variable. It is interesting to compare the results for the adjusted matching with the one for the standard matching. For wage outcomes, the first one is higher than the second, suggesting a selection of less productive workers allocated to apprenticeship contracts. This contrasts with the "positive" selection mentioned for the first outcome variable.

Altogether these results suggest a positive effect of the Apprenticeship program on the medium-term perspectives of treated youths to be employed in the formal sector. On the other hand, the results indicate that the program engenders a negative effect on accumulated formal labor market experience. It looks then that the program is capable of increasing the employability of apprentices but it is not inducing them to stick to the jobs they get. As for the impacts on wages, the program's effects are quite small in size.

7. Concluding comments

Young unemployment is a structural characteristic of developed and developing countries. This may create long term problems for young workers if there is path dependence on social issues. In developing countries this is compounded by higher turnover for this age group in absolute terms and compared to other groups. Active labor market programs try to improve inflows into employment for this age group.

We provide a first evaluation of the Apprentice Act, a law that provides incentives for firms to hire young workers by lowering firing costs but requiring workers to attend work skills enhancing courses. We make use of a huge longitudinal dataset (*Rais, Relatório Annual de Informações Sociais*), based on administrative data collected by the Labor Ministry, that contains information on the employment histories of all formal workers in Brazil from 1996 to 2010. We measure the impact of the program on five different outcomes that represent formal labor market attachment and remuneration, using other temporary workers as a control group.

We employ three distinct estimation procedures which deal with self-selection in program participation. Apart from the standard 2SLS, which we denote as parametric IV, we also employ two recently proposed estimators: a semi-parametric IV due to Battistin and Rettore (2008) and the adjusted matching estimator proposed by Dias *et al.* (2010) that corrects the standard matching approach with an IV estimated correction term based on a sharp observed cutoff criterium.

Our results indicate that program workers have a higher probability of getting a job in the years after the program and a higher probability of getting a non-temporary contract. On the other hand, our estimates suggest that apprentices accumulate less formal labor market experience than other temporary workers. According to our estimates, the program had a very small effect on wages, but positive and significant. These results are robust to our choice of methods that deal with selection into the program, holding for the whole set of estimation procedures.

Appendix: Identification Result in Dias *et al.* (2010)

This appendix informs the reader how to use assumptions A1 and A2, described in section 5.1 above and reproduced below, to reach the identification of the counter-factual component of the ATT parameter. The identification conditions are:

A1: $Y0 \perp Z|X$;

A2: There exists a set of points $\{z^*, z^{**}\}$ in the domain of Z where for all X:
$P\big[D = 1\big|X,\, Z = z^*\big] = 0$ and $0 < P\big[D = 0|X,\, Z = z^{**}\big] < 1$.

Following Dias et al. (2010), we first have that

$$\begin{aligned} E[Y0|X] &= E[Y0|X, Z] \\ &= E[Y0|X, Z, D = 0]P[D = 0|X, Z] + E[Y0|X, Z, D = 1]P[D = 1|X, Z] \\ &= E[Y0\,|\,X, Z = z^*, D = 0], \end{aligned}$$

where the first equality comes from A1. The second equality holds for any z, in particular for Z = z*. Hence the third inequality comes from A2 when Z = z*.

Since it is always true that

$E[Y0|X] = \; E[Y0|X, D = 0]P[D = 0|X] + E[Y0|X, D = 1]P[D = 1|X]$, we can write

$$\begin{aligned} E[Y0|X, D = 1] &= \frac{E[Y0|X] - E[Y0|X, D = 0].\,P[D = 0|X]}{P[D = 1|X]} \\ &= \frac{E[Y0\,|\,X, Z = z^*, D = 0] - E[Y0|X, D = 0].\,P[D = 0|X]}{P[D = 1|X]}, \end{aligned}$$

where the last equality comes from the previous result. Now, with some algebraic manipulation of the last expression we obtain that

$$E[Y0|X, D = 1] = E[Y0|X, D = 0] + \frac{E[Y0\,|\,X, Z = z^*, D = 0] - E[Y0|X, D = 0]}{1 - P[D = 0|X]}$$

This expression corresponds to equation (4) in Dias *et al.* (2010).

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