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Apprenticeship as a stepping stone to better jobs: Evidence from brazilian matched employer-employee data

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APPRENTICESHIP AS A STEPPING STONE TO BETTER JOBS: EVIDENCE FROM BRAZILIAN MATCHED EMPLOYER-EMPLOYEE DATA[1]

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Abstract: The objective of this paper is to evaluate the Brazilian Apprenticeship program adopted at a large scale since 2000. The program concedes payroll subsidies to firms that hire and train young workers under special temporary contracts aiming to help them successfully complete the transition from school to work. We make use of a matched employee-employer dataset covering all formal employees in Brazil, including apprentices. Our identification strategy exploits a discontinuity in the eligibility to enter the program in the early 2000's, when 17 was the age limit to take part in the program. This strategy allows us to consider selection based on unobservable characteristics. We find that the program increases the probability of employment in permanent jobs and decreases turnover rates and formal labor market experience in 2-3- and 4-5-year horizons. These results are consistent with a positive effect of the program on reservation utilities of workers and on their efforts to expand skills. This is also confirmed by the data as we find substantial impacts on schooling attainment. We also find much larger effects of the program for workers who had their first job in large firms. These results are robust to other choices of methods to address selection into the program based on unobservables.

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1. Introduction

It is well known that most young workers face major obstacles in the early stages of their professional careers. There is substantial evidence that young workers disproportionately hold both temporary and low-productivity jobs as well as face larger turnover and unemployment rates.[2] A main concern is that such outcomes may harm welfare in the long run. ‘Scarring’ effects of early unemployment experiences are well documented (Gregg and Tominey, 2005; Eliason and Storrie, 2006). The literature also shows that temporary contracts usually do not lead to better jobs in the future (see Booth *et al.*, 2002, and the references therein). More recent evidence about informal jobs, a proxy for low-productivity jobs, point in the same direction (Cruces *et al.*, 2012).

These facts have brought youth employment to the forefront of policy debate, with an increasing number of countries adopting youth-targeted active labor market programs (ALMPs) with a predominant focus on training (OECD, 2010). The claim for a targeted intervention is commonly justified by a vicious cycle for young workers who do not get (good) job offers because of no previous experience, which is difficult to attain due to the shortage of job offers. Indeed, because the productivity signals of young people are imprecise, employers tend to be reluctant to offer contracts to young workers who lack previous experience and referrals from former employers. As a result, low-productivity and/or temporary jobs are a common first step into the labor market for young workers.

It seems thus that showing a good signal to potential employers when entering the labor market is crucial to break this vicious cycle. This could come either from an experience in a good (high-productivity) job or from a reliable vocational training. These are the two

[2] Being unemployed or employed in low productivity/temporary jobs are related states given the high turnover rates of these jobs. In fact, such high turnover rates are perceived as the main determinant of youth high unemployment rates, as shown at least since Clark and Summers (1982), and Leighton and Mincer (1982).

dimensions combined in most Apprenticeship programs. For this reason, Biavaschi *et al*. (2013) claim that this type of program is the preferred form of youth-target ALMP and Eichhorst *et al*. (2015) favor apprenticeship over other forms of vocational education and training (VET). These claims however are grounded on evidence subject to methodological criticism, as we will see in the next section.

Moreover, most of the available evidence on apprenticeship programs is for developed countries. In contrast with developed countries, low- and middle-income countries are characterized by a relative scarcity of skills and deficiencies of the schooling system. This makes the claim for any training program, in particular an apprenticeship program, more appealing in these countries for at least two reasons. First, because the informational content on the productivity of low-educated workers may be even more imprecise to employers, training programs could reduce the barriers unskilled young workers face to access formal sector jobs (Attanasio *et al*., 2011).[3] Second, expected returns to human capital investments are larger in low-schooling countries.[4]

The main goal of the paper is to evaluate a Brazilian youth-targeted program adopted at a large scale since 2000: the apprenticeship labor contract. Its main objective is to place participants in formal first jobs with adequate specialized training and to increase their employability at the outset of their professional careers (*Ministério do Trabalho e Emprego*, 2009). The program concedes payroll subsidies to firms that hire young workers under special temporary contracts that can last up to two years. The main requirement is to enroll workers in intensive in-classroom training courses provided by certified institutions, which are complemented with a concomitant period of on-the-job

[3] In Brazil, there is evidence that informational barriers lead to much longer searches for a first job than for subsequent ones, especially for formal jobs (Reis, 2015).

[4] It has been argued that non-cognitive skills (also known as life-skills) are also relatively scarce in low- and middle-income countries with deficient schooling systems. Therefore, training with either cognitive or non-cognitive content should yield larger returns in these countries.

training at the firm. As the previous discussion suggests an apprenticeship program could not only improve skills but also provide signals that are more reliable to potential employers than other temporary contracts when entering the formal labor market. The main question we thus ask is whether the apprenticeship program is indeed a better stepping stone to more stable and better jobs when compared with other temporary jobs.

The contribution of this paper is twofold. First, the wealth of data we use allows us to add evidence on the impacts of youth-targeted interventions on a rich set of future labor market outcomes for youths, beyond finding any formal employment in the short term. In particular, we are able to assess the effects of the program on these outcomes for both the short term (2 to 3 years after the intervention) and the medium term (4 to 5 years). Although such extension is recognized as very relevant for analyzing the job prospects of youth, few papers are able to provide results for this time horizon (see Attanasio *et al*., 2017; Albanese *et al*., 2017; and Ibarráran *et al*., 2016). Moreover, we are able to extend the list of relevant outcomes, incorporating variables such as finding a formal open-ended contract, and accumulated experience and turnover measures two to five years after apprenticeship.

Second, we also contribute on methodological grounds taking advantage of a partially fuzzy design to deal with non-random participation in the program. As opposed to more conventional fuzzy designs where LATE parameters are estimated, the partially fuzzy design allows the identification and estimation of the ATT parameter. Therefore, we are exempt from criticisms about the potential low value of LATE results for policy purposes. In the next section, we discuss our contributions to the literature in more detail.

Our data are constructed from the Brazilian official registry of workers (from now on, RAIS, from *Relação Anual de Informações Sociais*), a very large administrative dataset that has information on the full history of formal jobs for millions of Brazilian workers.

Collected by the Labor Ministry, RAIS is a longitudinal matched employee-employer dataset covering, by law, the universe of formally employed workers, including apprentices hired under the Apprenticeship program. The use of RAIS provides a rare opportunity to observe young workers at the beginning of their careers and to follow them over time.[5] The wealth of data from RAIS allows us to focus on individuals who had their first jobs when they were 17 or 18 years old and still have a sample large enough to analyze how the Apprenticeship program affects the career prospects of these young workers in terms of degree of attachment to the formal labor market.

Because the decision to participate in the Apprenticeship program is likely to depend on unobservable characteristics of workers and firms that are correlated with labor market outcomes, the challenge is to address non-random selection based on unobservables. Our identification strategy exploits a discontinuity by age in the eligibility to the Apprenticeship program. From 2001 to 2005, only individuals less than 18 years old could participate in the program. Individuals aged 18 years old or more were not eligible.

We employ the adjusted matching method proposed by Dias *et al.* (2013), which uses an instrument to improve the standard matching estimator by allowing selection based on unobservables.[6] The adjusted matching method requires an instrument with at least one value of its domain that drives the probability of participation in the program to zero. This is provided by the eligibility rule of the Brazilian apprenticeship program, which precludes the participation of individuals over 18 years old.

[5] A recent review of the literature stresses the importance of using better data, particularly longitudinal data with a full set of individual characteristics for estimation of the effects of vocational training and related ALMPs (Biavaschi *et al.*, 2013).

[6] For robustness, we also use two other IV estimators: i) a semi-parametric IV estimator applied to the context of a partially fuzzy RDD, motivated by Battistin and Rettore (2008); and ii) a standard IV (2SLS) estimator for binary treatment and binary instrument.

The treatment group in our exercise is composed of young workers who started their careers in the formal sector as apprentices. Following the line of reasoning above, we use as a control group workers in the same age group (17 or 18 years old) who had other formal temporary contracts as first jobs over the same periods.[7]

Our findings suggest that apprenticeship is a much better stepping stone to permanent and better jobs when compared with other temporary jobs. We find that the program increases the chances of apprentices getting a non-temporary formal sector job by 7.9% after 2-3 years and by 6.9% after 4-5 years, relative to other temporary contracts. We also find a negative impact on accumulated formal labor market experience, which is compatible with a positive effect of the program either on the reservation utilities of workers with respect to subsequent jobs or on their intention to acquire further skills through higher levels of formal education.

Further results confirm both possibilities. We find substantial impacts of the program on the probability of increasing schooling, with the highest impact on the probability of completing secondary education, which was not required by the program in the period of analysis.[8] We also find much larger effects of the program on increasing employability in permanent jobs and decreasing turnover for workers who had their first job in large firms. This group of workers is more likely to have their reservation utilities raised after an experience as an apprenticeship.

Finally, we provide two sets of evidence in support of our identification hypothesis that the 18-year-old group of labor market entrants resembles the 17-year-old group in the absence of the program. First, a placebo exercise shows no difference in subsequent labor

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[7] As apprenticeship contracts, temporary contracts in Brazil can also last up to two years and do not involve firing costs if termination occurs by the end of the contract.

[8] As discussed in more detail in Section 3, an apprentice is required to enroll in primary school until completion of this cycle.

market outcomes for these two groups in a pre-program period. Second, we restrict our sample to 2001, the first year of the program. Selection could not have played a role then, as the 18-year-old group in 2001 could not have taken part in the program when they were 17 years old, because this happened in 2000, a year before the launching of the program. This exercise shows impacts qualitatively similar to our main set of results.

The remainder of the paper is organized as follows. In Section 2, we provide a brief review of the related literature. In Section 3, we describe the Apprenticeship program and the data set used in the study. Section 4 discusses the identification strategy and estimation methods. Section 5 presents the empirical results, including heterogeneity and robustness analyses. Section 6 presents some concluding remarks.

2. Related Literature

Our paper is connected to three strands of the literature. The first and most directly connected strand focuses on the effectiveness of apprenticeship programs for future labor market outcomes of youths, an issue that has received a great deal of attention in European countries. Earlier impact evaluations on apprenticeship, surveyed in Wolter and Ryan (2011), suffered from important limitations on identifying causal effects of apprenticeship. More recent papers attempt to properly address the nonrandom selection of youth individuals into the program. Bonnal *et al.* (2002) rely on normality assumptions for non-observed characteristics to jointly model and estimate the probability of being allocated to an apprenticeship program and future labor market outcomes.

Festerer *et al.* (2007) use apprenticeship contracts interrupted by the closure of firms as exogenous shocks to identify the wage returns of apprenticeship. This strategy restricts their sample to apprentices employed in dying firms, which may comprise a set of firms with particular characteristics such as smaller size or even lower quality of training provided to apprentices. Goggel and Zwick (2012) use mass layoffs to identify the wage returns of apprentices who change jobs. They rely on longitudinal data to take into account individual (time-invariant) unobserved heterogeneity. Concerns on external validity also apply here, as mass layoff episodes are likely to restrict the sample to non-random firms. Gunderson and Krashinsky (2015) aim to identify the impact of apprenticeship contracts by using as an instrument the regional supply of potential supervisors for apprentices' on-the-job training. The use of a single cross-section from the Canadian Census, however, prevents them from estimating the impact on a richer set of outcome variables, as we do in the present study. Parey (2016) relies on a similar strategy, using regional variation on vacancies for apprenticeship programs in Germany as an instrumental variable. Concerns arise about the validity of the exclusion restriction in this setting.[9]

Perhaps the paper that shares the most similarities with ours is Picchio and Staffolani (2013). The authors use Italian data to evaluate the effects of apprenticeship contracts. As in our paper, the authors contrast the outcomes of apprentices relative to a comparison group formed by workers with other temporary labor contracts. They use an identification strategy similar to ours that exploits discontinuities in the age eligibility of apprenticeship contracts. There are two main differences between our paper and theirs. First, they restrict their evaluation to a single outcome variable, namely, the time span until the youth gets (if they do) a permanent contract. The richness of our data allows us to exploit a much larger set of outcome variables, including access to permanent contracts, labor market experience and turnover. Second, they do not observe previous labor market experience. This may induce imbalances between treatment and control groups, as employers may use the previous labor market experience of applicants to allocate them into apprentice

[9] The author justifies this restriction through a theoretical model prediction.

and non-apprentice jobs. The potential bias coming from this imbalance is more likely to be present in their case, as the analysis is conducted for 30-year-old workers, who tend to have different work experience trajectories across groups. In our case, this is not a problem because we are able to restrict the sample to 17- and 18-year-old individuals without any previous formal job experience.[10]

The second strand of the literature connected to our paper investigates whether temporary jobs are a stepping stone to permanent jobs, acting mainly as a screening device, or whether they constitute a bad-job trap.[11] The literature shows that temporary contracts usually do not lead to better jobs in the future (see Booth *et al.*, 2002, and the references therein). As shown by Berton *et al.* (2011) there is considerable heterogeneity among distinct types of temporary contracts. One of the main concerns with fixed-term contracts is the lack of on-the-job training of unskilled workers (Cabrales *et al.*, 2014). In fact Berton *et al.* (2011) report that the best results come from temporary contracts that contain training activities.

This relates to the third strand of the literature connected to our paper, which deals with the evaluation of training programs for youths in developing countries (Latin America). We are not aware of any evaluation of formal apprenticeship programs for the job prospects of youths in these countries.[12] The closest evaluation conducted in this context is in Attanasio *et al.* (2011). The authors evaluate a youth-targeted training program in

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[10] Albanese *et al.* (2017) also analyze the Italian apprenticeship program. However, they do not contrast apprenticeship with an alternative entryway to the labor market nor with another VET. Instead, they compare a new apprenticeship program introduced in 2003 with the previous version of the Italian apprenticeship program.

[11] For Latin America, there is an analogous debate on the role of informal jobs as an entryway to young workers. Although there is evidence of scarring effects of first jobs in informality (Cruces *et al.*, 2012), some authors have argued that an informal first job in this context may help provide training and productivity signals to formal employers (Cunningham and Salvagno, 2011).

[12] Ospino (2016) evaluates some aspects of the Colombian apprenticeship program, but he targets firm performance and other broader outcomes, as opposed to individual job prospects as emphasized here.

Colombia that combines three months of in-classroom training followed by three months of on-the-job training. Selection problems are avoided by exploiting a randomized allocation between treated and non-treated individuals. They find significant impacts of the program in the short run, especially on formal employment and wages. Such results are confirmed for a longer time horizon in Attanasio *et al.* (2017).

In a broader context, Urzua and Puentes (2010) and Betcherman *et al.* (2007) present evidence that youth-training programs have, on average, higher positive impacts in Latin America than in developed countries. This is consistent with the view that training programs should be more effective in low- and middle-income countries where returns to training are larger, as skills are scarcer there (see González-Velosa *et al*., 2012, for a review of youth-targeted programs in six Latin American countries). Other studies that exploit randomized experiments in Latin American countries (Colombia, Dominican Republic and Brazil) have also found positive impacts of in-classroom training programs on some youth labor market outcomes (Card *et al.*, 2011; Ibarrarán *et al.*, 2014; Calero *et al.*, 2017).

3. Institutional Background and Data

3.1. The Brazilian Apprenticeship Program

The Apprentice Act was implemented in 2000 and constitutes the main youth-targeted ALMP in Brazil.[13] Training is provided by official professional qualification agencies (mostly by *Senai, Serviço Nacional de Aprendizagem Industrial*, the Brazilian main training institution run by the National Confederation of Industry, but also by similar sectoral training agencies) or by training institutions certified by the Labor Ministry.

[13] The program shares some similarities with other youth-targeted programs introduced in other Latin American countries in the 1990s, usually referred to as *Jóvenes Programs* (Ibarrarán *et al*., 2014).

Firms are responsible for enrolling young workers hired under the Apprenticeship program in these training courses. If an apprentice has not yet completed primary school, she is required to attend school.[14]

The program combines theory and practice with regard to training.[15] It emphasizes the on-the-job training dimension, but the importance of the in-classroom component is substantial. In-classroom training courses in certified institutions are much longer than in most other developing countries, with a minimum of hours varying by occupation from 400 to 800 hours and a maximum number that can reach 1,960 hours for some occupations. All courses have detailed life-skill modules of classes on citizenship, worker rights, worker safety and health, alcohol and drug prevention, and consumption education. As emphasized by Albanese *et al.* (2017) for European countries, apprenticeship programs are heavily regulated by governments and social partners to assure the quality of training. This is also a feature of the Brazilian program.

The Apprenticeship program has been part of the Brazilian labor legislation code *CLT* (*Consolidação das Leis Trabalhistas*) since 1943. It had a very small scope until December 2000, when the Apprentice Act (Law 11,180) was enacted. The program was initially designed for individuals aged 14 to 17 years old. It was regulated in December 2005 by more-detailed legislation (*Decreto-Lei 5598*), when the maximum age for participation increased from 17 to 23 years old. The number of apprentices substantially increased in Brazil from 7,411 workers hired under this type of contract in 2000 to 59,365 in 2005, 192,426 in 2010, and more than 250,000 in 2016.

[14] Since 2008, the requirement to attend school is for any apprentice that has not completed secondary school.

[15] Although much less integrated into the education system than the so-called "dual system" of Germanic speaking countries, the Brazilian program shares similarities with these programs as it combines in-classroom with on-the-job training.

An apprentice cannot work more than six hours per day if she is still enrolled at primary school or eight hours per day if she has completed primary school. Payments must be at least the hourly minimum wage. There is a payroll subsidy in the form of a lower requirement of 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 monthly wage on this fund instead of the rate of either 8% or 8.5% that prevailed for other workers during that period.[16]

Apprentices are hired under non-renewable fixed-term contracts with a maximum length of two years. Contracts must be terminated when the apprentice reaches the age limit (18 years old between 2000 and 2005 and 24 years old after 2005). As in other fixed-term contracts, there are no firing costs for job separations by the end of the contract. By contrast, firing costs for unjustified separations induced by firms in open-ended contracts comprise 1-month advance notice and a fine equivalent to approximately 50% of 1-month wage per year of tenure.[17]

Establishments should hire at least 5% (and at most 15%) of their employees who work in occupations requiring formal training as apprentices.[18] These thresholds should be enforced by the inspection division of the Ministry of Labor. Enforcement, however, was very limited in the early 2000's, when firms could easily claim a lack of training agencies in their region/occupation for not being penalized for employing fewer apprentices than

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[16] Firms had to deposit 8% of the monthly wage on the worker's FGTS account from 1966 to October 2001, when the government introduced a 5-year temporary increase of 0.5 p.p.'s (Gonzaga, 2003).

[17] The fine for unjustified dismissals in non-temporary contracts corresponds to 50% of the accumulated amount deposited in the FGTS account during the employment relationship: 40% paid to the worker and, since October 2001, 10% paid to the government. Because the FGTS fund approximately accumulates at the rate of one monthly wage per year, the firing costs are approximately 50% of one monthly wage for each year of tenure. More than 99% of firm-induced separations in Brazil are unjustified.

[18] Micro and small firms are exempted from this requirement. Firms are classified as micro or small based on their most recent annual revenue. The threshold in the early 2000s was R$1.2 million, approximately US$ 510.6 thousand in 2001.

the 5% minimum. In practice, therefore, the threshold requirements were not binding in the period analyzed in this study.

3.2. Data

In this paper, we use a very large restricted-access administrative file collected by the Brazilian Ministry of Labor (*Ministério do Trabalho e Emprego*), the *Relação Anual de Informações Sociais* (RAIS). RAIS is a longitudinal matched employee-employer dataset covering, by law, the universe of formally employed workers in Brazil. All tax-registered firms have to report every worker formally employed at some point during the previous calendar year.[19] Apart from tax/social security compliance, the data have no coverage limitation. Because our empirical strategy relies on the age-requirement changes implemented in 2000, we use data from 2001 to 2008. Over this period, RAIS contains an average of 40 million worker-establishment records per year.

Each observation in the dataset consists of a contract-worker-establishment triplet in a given year. Each worker is identified by a unique national social insurance number (*PIS, Programa de Integração Social*). Each establishment has a unique identification number (*CNPJ*) given by the federal tax authority. Firm and worker identification numbers allow us to construct a matched employer-employee longitudinal dataset.

We have data on worker characteristics (age, schooling, gender) and establishment characteristics (sector, size, legal form, location at the municipality level) as well as detailed information for each employer-employee contract, such as wages, hours, type of contract (permanent, apprenticeship, and other temporary contracts), tenure, month of admission, month of separation, reason for separation, and occupation. We exclude agriculture and the public sector.

[19] There are incentives for truthful reporting, as the main purpose of RAIS is to administer a federal wage supplement (*Abono Salarial*) to formal workers.

To exploit the age discontinuity of eligibility rules, we restrict the sample to workers who had their first job in the formal labor market at the ages of 17 or 18 years old in each of the first three years after the implementation of the Apprentice Act (from 2001 to 2003). The restriction for youths entering the labor market for the first time avoids confounding sources of heterogeneity due to distinct past experience in the labor market - see Card and Sullivan (1988) for the importance of controlling for previous labor market trajectories when evaluating active labor market programs. To construct an even more homogeneous sample, we only keep information for those youths who were hired for a fixed-term (temporary) job. Apprentices hired under the Apprenticeship program constitute the treatment group, while other temporary workers are in the control group. In total, we have information on 11,377 apprentices (treatment) and 26,738 non-apprentices (control) who had their first jobs at the ages of 17 and 18 between 2001 and 2003.[20]

We follow all workers in our sample for five years (in addition to the entrance year). This allows us to compute average program impacts for the short run (arbitrarily defined as 2 to 3 years after the first formal job) and the medium run (4-5 years after the first formal job). We search for each worker in the sample in all formal (temporary and non-temporary) jobs in subsequent years and collect all information for each matched employee-employer pair.[21]

4. Methodology and estimation procedures

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[20] In principle we could have also explored an analogous fuzzy discontinuity design using workers aged 24 and 25 years after 2005. The main reason we decided not to do so is due to the small number of apprentices aged 24. In 2006 we have 85.5 thousand apprentices, among which only 6 are 24 years old. This number of observations for the treatment group could be even lower as we keep only those hired for their first job.

[21] Attrition is a reason for concern, as in any other study that relies on longitudinal data. On average, the RAIS attrition rate in our sample period is approximately 5%. Attrition is defined as the share of workers not reported as employed in a given year that were employed on the last day of the previous year. One of the main sources of attrition in RAIS is occasional non-reporting by complier establishments. We exclude episodes of spurious establishment "births" and "deaths", in which all employees in some establishments "disappear" from RAIS in a particular year and eventually return in subsequent years.

4.1. On the relevance of unobservables for selection into the program

Selection into the program is a choice of firms and workers and, hence, may be driven by unobservable characteristics. For instance, only a particular "type" of worker may apply for vacancies in apprentice positions. Or, given a pool of applicants, firms may allocate different types of individuals for apprentice or non-apprentice positions. These mechanisms may interact, as firms may invite only certain types of applicants to apprenticeship positions. Guasch and Weiss (1981) provide theoretical support for this, showing that such separating equilibrium is attained under plausible conditions.

If unobservable characteristics do affect selection into the program, they would not be balanced among treated and eligible non-treated workers. We investigate this by checking imbalances in observable characteristics across these two groups in our sample.

Table 1 compares averages for the observable characteristics of eligible workers hired under an apprenticeship contract (treated) and those hired under any other type of temporary contract (non-treated). The sample comprises temporary workers who had their first jobs when they were 17 years old between 2001 and 2003, excluding those employed in agriculture and in the public sector.

The first two columns in table 1 show comparison among groups for fifteen mean statistics. The last column reveals that these observable characteristics differ substantially between apprentices and non-apprentices across fourteen (out of fifteen) dimensions. In particular, the schooling distribution of apprentices spikes at 9 to 11 years, contrasting with a relatively more uniform distribution for non-apprentices. Another noticeable difference between these two groups is the relatively larger average size of the firm for apprentices.

Table 1 – Observable Characteristics: Temporary Workers, 1st Job at age 17

Characteristics	Age 17 Nonapprentice	Apprentice	P-Value
Male	0.630	0.644	0.044
Schooling			
Less than 5 years of schooling	0.070	0.004	0.000
6 to 8 years of schooling	0.145	0.107	0.000
9 to 11 years of schooling	0.654	0.844	0.000
More than 12 years of schooling	0.130	0.045	0.000
Industry			
Construction	0.018	0.012	0.000
Manufacturing	0.176	0.229	0.000
Trade	0.127	0.172	0.000
Services	0.678	0.587	0.000
Establishment size	438.1	469.4	0.037
Region			
North	0.031	0.037	0.018
Northeast	0.099	0.096	0.506
Southeast	0.617	0.690	0.000
South	0.216	0.117	0.000
Mid-West	0.038	0.060	0.000
Sample size	8,048	11,377	

Notes: Establishment size is the average number of employees of the firms where each group worked in their 1st formal job. The last column reports the p-value for the test of equality of the estimates for each group. Constructed by the authors based on microdata from RAIS.

The differences in observable characteristics suggest that there may also be differences in unobservables, which could play an important role in the selection scheme. This calls for an identification strategy that deals not only with selection on observables but also with selection on unobservables, as the one we use in this paper. We describe our identification strategy in the next sub-section.

4.2. Identification and estimation: overview

If unobservable characteristics are not balanced among treated and non-treated workers, then methods relying on the comparison of outcomes between the two groups, conditioning solely on observables, may produce misleading estimates of the program effects.

Our identification strategy explores the discontinuity in the eligibility rule to participate in the program between 2000 and 2005. As described in the previous section, workers who were 17 years old were eligible and could select to participate or not. For this group, unobservables may have driven selection into the Apprenticeship program. On the other hand, 18-year-old workers were ineligible, and this restriction was binding until 2005, creating the partially fuzzy RD setting, as discussed in Battistin and Rettore (2008).

To obtain consistent estimates of the effect of the Apprenticeship program, we make use of the adjusted matching estimator proposed by Dias *et al.* (2013), which combines the idea of matching on observables with exogenous variation provided by an instrument. This choice fits well in the ideal setting discussed by Dias *et al.* (2013), as the eligibility rules of the Apprenticeship program impose a restriction on the maximum age for participation.[22] As described above, the maximum age to participate in the program was 17 years old until September 2005, when the age restriction rose to 23. Recalling that the program is not compulsory, we thus have 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 age cutoff condition for eligibility in the Apprenticeship program also fits directly within a framework of regression discontinuity design (RDD). For robustness, we also use two other related estimators: i) a semi-parametric version of the IV estimator applied to the context of a partially fuzzy RDD, as discussed by Battistin and Rettore (2008); and ii) the traditional IV estimator, or 2SLS, also applied in a fuzzy design, as discussed in

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[22] The empirical application in Dias *et al.* (2013) is also implemented under a similar RD setting that uses age as the instrument.

Hahn *et al.* (2001).[23] In all three cases, we exploit the fact that the eligibility to the program switches as age crosses a threshold value.

We are 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 with the LATE parameter in program evaluation. The reason is that, by design, those above the age-threshold could not and did 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. The next sub-section describes the adjusted matching estimator.

4.3. The Adjusted Matching Identification and Estimation Procedure

Dias *et al.*'s (2013) estimator uses an instrument that 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 allow partial compliance for other values. The idea is that by moving individuals in and out of the program, the variation in the instrument can correct for possible imbalances in unobservables due to self-selection into the program. Note that the standard matching (on observables) method does not take care of such imbalance. In fact, the method proposed by Dias *et al.* (2013) is intended to adjust the conventional matching estimator for such imbalances.

Using standard notation from the potential outcome literature, we are interested in estimating the Average Treatment on the Treated (ATT) parameter:

$$\alpha = E[Y_1|D = 1] - E[Y_0|D = 1] =$$

$$E_{X|D=1}E[Y_1|X, D = 1] - E_{X|D=1}E[Y_0|X, D = 1], \quad (1)$$

[23] Hahn *et al.* (2001) 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 of these topics can be found in Angrist and Pischke (2009).

where Y_1 and Y_0 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 $E_{X|D=1}E[Y_1|X, D = 1]$ 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 $E_{X|D=1}E[Y_0|X, D = 1]$ is not directly available in the data, so it needs to be identified through the use of some assumptions. Dias *et al.* (2013) 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:

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

The first assumption is an exclusion restriction imposing that the variable Z is not correlated with the counterfactual outcome Y_0 after conditioning on the covariates in X.[24] 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 any selective participation into the program. Indeed, they allow D to be correlated with Y_0 when Z takes on the value z** (after conditioning on X).

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

Using A1 and A2, Dias *et al.* (2013) propose a constructive proof for the identification of the mean counterfactual outcome $E[Y_0|X, D = 1]$. Hence, they are able to identify the conditioned (on X) version of the ATT parameter, which can be written as:

$$\alpha(\mathrm{x}) = E[Y_1|X, D = 1] - E[Y_0|X, D = 0] - \frac{E[Y_0 \mid X, Z = z^*, D = 0] - E[Y_0|X, D = 0]}{1 - P[D = 0|X]}$$

This expression shows that ATT can be written as the conditional difference between treated and control populations, further adjusted by a correction term given by the last term on the right hand side (RHS) of the equation. Note that all elements that compose this last term can be identified from the data, where $E[Y_0 \mid X, Z = z^*, D = 0]$ is the mean observed outcome for ineligible controls at a given X and $\{1 - P[D = 0|X]\}$ is the propensity score. The object of interest, the unconditional (in X) ATT parameter, is finally identified from $\alpha(\mathrm{x})$ by averaging the latter over the distribution of X for the treated group (D = 1).

We follow this identification strategy using age as the Z variable. This choice fits well in the ideal setting discussed by Dias *et al.* (2013), as the eligibility rules of the Apprenticeship program impose a binding restriction on the maximum age for participation, which was 17 years old until September 2005. Recalling that the program is not compulsory, we thus have an appropriate setting in which the age of workers can be used as the instrument: while those aged above the cutoff value do not participate, there is imperfect compliance for those below the cutoff.

We implement the adjusted matching estimator in two steps. First, the correction term is estimated using the analogy principle. In the second step, we implement the procedure proposed by Ichino and Becker (2002), which was also used for the standard matching estimator. The covariates in X used in the propensity score are dummies for gender, schooling, industry, geographical region, and the year in which the worker first entered the formal sector. Only observations in the region of the common support of the propensity score were used for computing standard and adjusted matching estimates.[25] The standard matching estimates were computed using Epanechnikov kernel weights. Inference is based on standard errors estimated from bootstrapping with 100 replications.[26]

The outcomes of interest can be classified into two groups of variables: i) (formal) employability (in permanent or any formal jobs); ii) measures of attachment to labor market (accumulated number of admissions and dismissals; accumulated number of months in formal sector jobs; probability of quits; probability of staying in the same establishment or in the same occupation). [27]

4.4. Comments on identifying assumptions

The empirical strategy we use in the paper is based on the partial participation of youths under 17 years old and the non-participation of youths over 18 years old. To confirm this, Figure 1 shows the participation rate in the Apprenticeship program by age for the period 2001-2003.[28] The figure reveals that the probability of participation becomes virtually zero for youths older than 17. Because the analysis relies on local estimators, we only use information on youths aged 17 and 18 in all estimations.

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[25] Because 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.* (2013), 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.

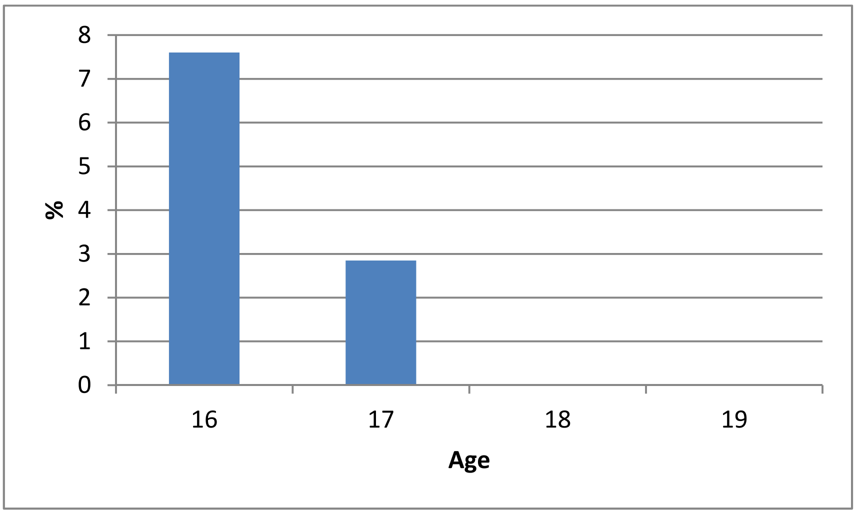
[26] Abadie and Imbens (2008) confirm that bootstrap provides valid inference for kernel-based matching methods.

[27] We follow Card and Sullivan (1988) and abstain from using available data on post-treatment wages due to the higher complexity in accurately estimating the effect of the apprenticeship program on this outcome without having any information on pre-treatment wages.

[28] Participation rates for apprentices are calculated as the ratio between the number of apprentices and the overall number of employed (in the formal sector) individuals in the same age group.

Another important identifying assumption relies on the comparison of unobservable characteristics between individuals who start working with either 18- or 17-year-olds. In what follows, we refer to this assumption as the exclusion restriction. This restriction deserves some further considerations. First, one may argue that the 18th anniversary introduces a discontinuity in employability, as individuals take more legal responsibilities at this age.[29] However, it should be stressed that we are comparing an individual's employability in periods two to five years after the entrance year. Therefore, we should expect that everyone in our sample would have, by that time, already incorporated any discontinuous jump in employability experienced when they turned 18 years old.[30]

Figure 1: Participation rate in the Apprenticeship program by age – 2001/2003



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

To test such an exclusion restriction, we compare subsequent labor market outcomes for individuals entering the job market aged either 17 or 18 years old at a time before the introduction of the program. If the exclusion restriction is valid, we should expect no

[29] In Brazil, 18 years old is a threshold defining criminal responsibilities and permission to drive vehicles.

[30] The outcome variables are compared when the youth who entered the labor market at 17 years old is approximately 19 to 22 years old.

significant differences in subsequent labor market outcomes across the 17- and 18-year-old entrant groups.

Table 2 shows estimated coefficients for a dummy indicating the age of 18 years old (relative to 17) in OLS regressions with subsequent labor market outcomes as dependent variables and a set of control variables, which are those shown in Table 1. The sample is restricted to entrants in the years 1995 or 1996. The subsequent labor market outcomes are measured two to three years later. Thus, the data stretch up to 1999 for labor market outcomes measured three years later than a 1996 entrance. Therefore, everything in these regressions happened before the Apprenticeship program started (December of 2000).

Table 2: Entrance age and subsequent labor market outcomes prior to the program

Outcome	Years t+2 or t+3	
	OLS	**Standard Matching**
Employment		
Employment probability - permanent formal job	-0.007	-0.0047
	(0.0093)	(0.0091)
Employment probability - any formal job	0.0056	0.0045
	(0.0095)	(0.0102)
Turnover		
Accumulated number of admissions	-0.0216	-0.0247
	(0.0200)	(0.0224)
Accumulated number of dismissals	-0.0415**	-0.0509**
	(0.0192)	(0.0218)
Probability of dismissal by quit	0.0401*	0.0272
	(0.0206)	(0.0223)
Experience		
Accumulated number of months	-0.2054*	-0.1888
	(0.1201)	(0.1370)
Prob. of staying same establishment	0.0042	0.0008
	(0.0044)	(0.0044)

Notes: The table presents the estimated coefficients (standard errors in parentheses) for a dummy variable that assumes value 1 (0) if the age of the worker is 17 (18) in separate OLS regressions with the dependent variables indicated in each row. The following covariates are used for matching: dummies for gender, schooling, industry, geographical region, and the year in which the worker first entered the formal sector (either 1995 or 1996).

The results show that for six of the seven subsequent labor market outcomes, we cannot reject the null hypothesis that the 18-year-old dummy coefficient is zero at the 5% significance level, which is an adequate significance level for this high number of observations.[31] Therefore, our overall interpretation is that there is no evidence that the subsequent opportunities in the labor market differ across 17- and 18-year-old entrants in the absence of the program.

One may argue that such a test for the exclusion restriction is insufficient because the introduction of the program may create differences between 17- and 18-year-old entrants that did not exist before. For instance, suppose that before the program, the unobservable characteristics that affect employment were equally distributed in the two groups. Then, after the introduction of the program, some of the would-be 18-year-old entrants decided to enter apprenticeship a year early, hence becoming 17-year-old entrants. If these are individuals with particularly high or low levels of unobservables, the 18-year-old individuals would not be a good proxy for the 17-year-old workers in the absence of the program. In section 5.3.2, we restrict our sample to entrants in the first year of the launching of the program, when this selection could not have taken place. This exercise shows qualitatively similar impacts to our main set of results.

5. Econometric Results

[31] At this significance level, the only rejection of the null comes in a test with the number of separations in a two-year window starting two years after labor market entrance. For six other outcomes, including subsequent employment probability (either in an open-ended contract or not), there is no evidence that the subsequent opportunities in the labor market differ across 17- and 18-year-old entrants. For another outcome, we reject the null at the 10% significance level (number of months employed in a two-year window starting two years after labor market entrance).

In this section, we show the results of the estimation of the impact of the Apprenticeship program on several labor market outcomes derived from applying the identification strategy described in the previous section.

5.1. Main Results

In this sub-section, we present our estimates of the effects of the Apprenticeship program on selected labor market outcomes that measure employability and the degree of attachment to the formal labor market in the subsequent years following the treatment for the main estimation procedure described in Section 4. The analysis is carried out for the short (2-3 years after the program) and medium (4-5 years after the program) run.

Before presenting the econometric results, Table 3 displays the averages of the outcomes for the following two groups in our sample: non-apprentices (aged either 17 or 18 years old) and apprentices. Outcome averages are presented for the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which apprentices and non-apprentices entered the labor market.

Table 3 – Outcomes: Temporary Workers, 1st Job at age 17 or 18

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Nonapprentice	Apprentice	Nonapprentice	Apprentice
Employment				
Employment prob. - permanent formal job	0,5138	0,5655	0,5200	0,5658
Employment prob. - any formal job	0,6264	0,6089	0,6042	0,5992
Turnover and experience				
Accumulated number of admissions	0,9181	0,8300	0,9119	0,8141
Accumulated number of dismissals	0,8271	0,6043	0,8569	0,7500
Accumulated number of months	6,2907	5,8300	6,5033	6,2200
Accumulated number of quits	0,3680	0,4070	0,4454	0,4600
Prob. of staying same establishment	0,0374	0,0210	-	-
Prob. of staying same occupation	0,0510	0,0415	-	-

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

Raw comparisons of outcomes between non-apprentices (first column) and apprentices (second column) reveal that apprentices tend to have larger probabilities of being employed in a permanent formal job in the short and in the medium run and slightly lower probabilities of being employed in any formal job (temporary or permanent) in the short and medium run.

Table 3 also shows that labor turnover is lower for former apprentices, with smaller numbers of accumulated dismissals and admissions over two-year windows in the short and medium run. On the other hand, former apprentices also have a slightly larger probability of quitting in the short run, with no differences in the medium run. Finally, former apprentices have, on average, a lower number of accumulated months worked in formal jobs in the short and medium run; a considerably lower probability of staying in the same firm two years after the entrance in the job market; and a slightly lower probability of staying in the same occupation at the same time.

The outcomes presented in Table 3 are raw averages. Table 4 presents our estimation results for the impact of the Apprenticeship program. Average treatment effects are shown for all selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5). For each outcome, we first show results for the standard matching estimator, followed by results controlling for selection on unobservables, based on the adjusted matching estimator discussed in Section 4. The results in Table 4 are organized as in Table 3 for the three groups of outcomes of interest: formal employment probability, turnover, and experience.

We find that the Apprenticeship program provided a better entryway to the labor market than other temporary contracts. The table shows that the Apprenticeship program had a positive and statistically significant effect on the probability of being employed in a permanent formal job in the short run as well as in the medium run. The program increased the probability of having a non-temporary formal job by 7.9% after 2-3 years and by 6.9% after 4-5 years. Therefore, apprenticeship looks like a much better stepping stone to permanent and better jobs when compared with other temporary jobs.

Table 4: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes Standard and Adjusted Matching Estimators, Full Sample

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Standard	Adjusted	Standard	Adjusted
Employment				
Employment prob. - permanent formal job	0.0425***	0.0791***	0.0269***	0.0687***
	(0.0069)	(0.0119)	(0.0062)	(0.0115)
Employment prob. - any formal job	-0.0126*	-0.0244**	-0.0041	-0.0129
	(0.0067)	(0.0114)	(0.0080)	(0.0117)
Turnover and experience				
Accumulated number of admissions	-0.0649***	-0.1667***	-0.0898***	-0.2061***
	(0.0129)	(0.0277)	(0.0151)	(0.0267)
Accumulated number of dismissals	-0.1932***	-0.3786***	-0.0993***	-0.2091***
	(0.0143)	(0.0257)	(0.0158)	(0.0244)
Accumulated number of months	-0.3437***	-0.7358***	-0.2770***	-0.5114***
	(0.1022)	(0.0087)	(0.0391)	(0.0829)
Accumulated number of quits	0.0604***	0.0518*	-0.0097	0.0308
	(0.0180)	(0.0294)	(0.0214)	(0.0299)
Prob. of staying same establishment	-0.0135***	-0.0381***	-	-
	(0.0002)	(0.0070)		
Prob. of staying same occupation	0.0011	-0.0638***	-	-
	(0.0114)	(0.0144)		

Notes: The table presents the adjusted matching estimator (Dias *et al.*, 2013) of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor market (see text). The following covariates are used for matching: dummies for gender, schooling, industry, geographical region, and the year in which the worker first entered the formal sector. The instrument for all estimates is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Bootstrapped standard errors (100 replications) are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Results for the standard matching estimator are much lower (4.3% after 2-3 years and 2.7% after 4-5 years). This confirms that unobservable characteristics play an important role in the selection into the program. Individuals seem to be negatively selected to the program, that is, the selection scheme is such that individuals with relative lower values for unobservable determinants of employability are allocated into the program.

The impact of the program on having any formal job was not statistically different from zero in the medium run, suggesting that it probably negatively affected the probability of having another temporary job. In the short run, though, the coefficient estimates are negative for this same outcome. In order to better interpret this result we first discuss the estimates on turnover outcomes.

Our estimates show that the program had a negative, large in absolute terms, and statistically significant effect on the accumulated number of dismissals and admissions in the short and medium run. After controlling for selection on observables and unobservables, the number of admissions experienced by apprentices was 16.7% lower than for other temporary contracts after 2-3 years and 20.6% lower after 4-5 years. The impact on the number of dismissals was a decrease of 37.9% after 2-3 years and of 20.9% after 4-5 years.

The effect of the program on the accumulated number of months worked in formal jobs was negative and statistically significant in the short and medium run. The program's impact on apprentices was that they would work, on average, 0.74 months less than workers with other temporary contracts in years t+2 and t+3 and 0.51 months less in years t+4 and t+5.

For all turnover outcomes apprenticeship effects are considerably lower when estimated by standard matching, confirming the pattern of negative selection observed for employability outcomes.

The findings of a much lower turnover for participants in the program and a negative effect on accumulated formal labor market experience, combined with the previous result of boosting the chances of getting a job with a permanent formal contract, suggest two complementary mechanisms. The first is that the program may have increased the reservation wage (and/or the "reservation job quality") of participants compared with entrants employed under other temporary contracts. The other mechanism is that the program possibly increased the awareness about the relevance of skills (or credentials) to obtain a good job, making more likely that apprentices would stay longer in (or return to) the formal education system. As a result of either mechanism, apprentices would tend to spend relatively more time out of the formal labor market in the short run searching for (and eventually succeeding in obtaining) more stable and better jobs. Under this interpretation, these findings suggest that the program helps workers successfully complete the transition from school to permanent formal jobs.

The last three rows of table 4 report results for a complementary subset of outcomes. The effects on the probability of quitting were not statistically significant in either the short or the medium run (considering 5% as an appropriate significance level for our sample size). The impacts on the probabilities of staying in the same firm and working in the same occupation were both negative and statistically significant in the short run. The probability of staying in the same firm was 3.8% lower and of working in the same occupation was 6.4% lower after 2-3 years.

5.2 Complementary results

5.2.1 Education as an outcome

As stated before, one possible mechanism to interpret our main results is that the program may have increased the awareness about the importance of skills (or credentials) to obtain a good job. According to this interpretation, the program could have increased the probability that apprentices would stay longer in (or return to) the formal education system. According to Alet and Bonnal (2011), this could drive at least part of the positive impacts of apprenticeship programs on labor market integration. Moreover, as discussed in Section 3, attending primary school until completion of this cycle is one requirement of the apprenticeship contract.

Table 5 presents results supporting this interpretation, by showing results of estimating our main specification with outcome variables related to the worker education level. The table reports a positive impact of the apprenticeship program on the probability of completing different cycles of the education system.

The chances of completing the primary schooling level 2 to 3 years after entering the labor market are 20.4% higher for apprentices than for non-apprentices. This effect is even more intense for the probability of completing the secondary level, which was not a requirement of the program in that period. The apprenticeship program increases the probability of completing secondary school by 43.8% 2 to 3 years after entering the labor market. Such positive and large effects remain 4 to 5 years after entering the labor market (15.8% and 35.2% for the primary and secondary level, respectively). For this longer time horizon, it is also possible to compute the chances of a 17 or 18 years old individual to complete college. Such effect of the program is also positive, although the magnitudes are much less striking.

These results are evidence that the apprenticeship program may have raised the demand for further qualification, possibly due to an increase of the awareness on the importance of skills (or credentials) to obtain a good job.[32]

Table 5: Estimates of the Impact of the Apprenticeship Program on Education Outcomes: Standard and Adjusted Matching Estimators, Full Sample

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Standard	Adjusted	Standard	Adjusted
Completed Primary Education	0.0729***	0.2043***	0.0350***	0.1577***
	(0.0102)	(0.0140)	(0.0124)	(0.0137)
Completed Secondary Education	0.1159***	0.4376***	0.0932***	0.3515***
	(0,0075)	(0,0188)	(0.0056)	(0.0153)
Completed University			0.0136**	0.0291***
			(0.0064)	(0.0095)
Completed any Education Cycle			0.0755***	0.2919***
			(0.0051)	(0.0148)

Notes: The table presents the adjusted matching estimator (Dias *et al.*, 2013) of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor market (see text). The following covariates are used for matching: dummies for gender, schooling, industry, geographical region, and the year in which the worker first entered the formal sector. The instrument for all estimates is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Bootstrapped standard errors (100 replications) are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

[32] Alet and Bonnal (2011) find similar results for France.

5.2.2 Results by Firm size

The literature on apprenticeship programs usually finds larger transitions to open-ended contracts when the program is taken in large firms (Soskice, 1994). Albanese *et al*. (2017) find similar results for Italy. They argue that larger firms tend to provide better-structured on-the-job training. Therefore, one should expect that the differential content of the on-the-job training for apprentices should be more prevalent in larger firms.

In this sub-section, we assess this possibility by dividing our sample by firm size. We estimate our model stratifying the sample for youths that had their first job in large firms (defined here as firms with more than 250 employees) and in small and medium firms (firms with less than 250 employees).[33] Table 6 presents estimates of the impact of the program for both samples.

The results show that the estimates of the impact of the apprenticeship program on the probability of finding a job with open-ended contract is much larger for those apprentices that took part in the program in large firms. This impact is approximately 8.4% in the short run and 8.8% in the medium run (after 4-5 years) for large firms; and 7.2% and 4.3%, respectively for small firms.

The negative effects of the program on admissions and dismissals are also much larger (in absolute values) for large firms in the short run. The estimated coefficients for turnover are similar by firm size in the medium run. Apprentices in large firms also spend less time in the formal sector relative to non-apprentices in the short run. The estimated coefficients for experience (accumulated number of months in the formal sector) are similar by firm size in the medium run.

[33] The median size of Brazilian firms in our matched formal employer-employee data is 14 workers (Corseuil *et al*., 2014).

Table 6: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes by Firm Size, Adjusted Matching Estimator

Outcome	**Years t+2 or t+3**		**Years t+4 or t+5**	
	Small-Medium	**Large**	**Small-Medium**	**Large**
Employment				
Employment prob. - permanent formal job	0.0723***	0.0838***	0.0429***	0.0884***
	(0.0159)	(0.0177)	(0.0146)	(0.0218)
Employment prob. - any formal job	-0.0095	-0.0469***	-0.0148	-0.0233
	(0.0162)	(0.0169)	(0.0158)	(0.0174)
Turnover and experience				
Accumulated number of admissions	-0.1026***	-0.2224***	-0.2014**	-0.2206***
	(0.0343)	(0.0428)	(0.0360)	(0.0408)
Accumulated number of dismissals	-0.3149***	-0.4331***	-0.1905***	-0.2372***
	(0.0330)	(0.0427)	(0.0374)	(0.0420)
Accumulated number of months	-0.3304***	-1.4502***	-0.7676***	-0.6656***
	(0.1066)	(0.3055)	(0.1963)	(0.0677)
Accumulated number of quits	0.0194	0.1117**	0.0686	-0.0064
	(0.0408)	(0.0524)	(0.0451)	(0.0517)
Prob. of staying same establishment	-0.0321***	-0.0417***	-	-
	(0.0026)	(0.0009)		
Prob. of staying same occupation	-0.0668***	-0.0650***	-	-
	(0.0157)	(0.0161)		

Notes: The table presents the adjusted matching estimator (Dias *et al.*, 2013) of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor market (see text). Columns 'Large Firms' use workers hired by large firms (with more than 250 workers) in period *t*, while columns 'Small-Medium Firms' use a sample of workers in firms with less than 250 employees. The following covariates are used for matching: dummies for gender, schooling, industry, geographical region, and the year in which the worker first entered the formal sector. The instrument for all estimates is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Bootstrapped standard errors (100 replications) are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Altogether, these results suggest that part of the positive effects of the program can be associated with the size of firms during the apprenticeship experience. This could come from a credential effect, as having an apprenticeship from a large firm is probably more valued by other potential employers through better on-the-job training provided by large firms in a broad sense, including the motivating aspects of working in a more organized professional environment.

The program's impact on the probability of remaining in the same establishment is negative and lower in absolute terms for large firms in the short run (-4.1% compared with -3.2% for small-medium firms). Finally, the effect of apprenticeship on remaining in the same occupation is very similar across both firm size categories.

5.3. Robustness

5.3.1 Alternative estimation methods

For robustness, we show in this sub-section the results when we use the first alternative estimation procedure discussed in the methodological section: the semi-parametric IV estimator inspired by the identification strategy developed by Battistin and Rettore (2008) for the partially fuzzy regression discontinuity setting.[34] The second and fourth columns in Table 7 show that we obtain similar results when we use this estimation alternative. For comparison, the first and third columns of the table reproduce the adjusted matching estimates in Table 4.

Table 7: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes- Semi-parametric IV Estimator, Full Sample

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Adjusted Matching	Semi-Parametric IV	Adjusted Matching	Semi-Parametric IV
Employment				
Employment prob. - permanent formal jol	0.0791***	0.1104***	0.0687***	0.0961***
	(0.0119)	(0.0155)	(0.0115)	(0.0140)
Employment prob. - any formal job	-0.0244**	-0.0224	-0.0129	-0.0014
	(0.0114)	(0.0139)	(0.0117)	(0.0162)
Turnover and experience				
Accumulated number of admissions	-0.1667***	-0.2021***	-0.2061***	-0.2442***
	(0.0277)	(0.0330)	(0.0267)	(0.0328)
Accumulated number of dismissals	-0.3786***	-0.5087***	-0.2091***	-0.2485***
	(0.0257)	(0.0321)	(0.0244)	(0.0314)
Accumulated number of months	-0.7358***	-0.9877***	-0.5114***	-0.5910***
	(0.0087)	(0.0532)	(0.0829)	(0.0092)
Accumulated number of quits	0.0518*	0.1097***	0.0308	0.0285
	(0.0294)	(0.0375)	(0.0299)	(0.0404)
Prob. of staying same establishment	-0.0381***	-0.0564***	-	-
	(0.0070)	(0.0084)		
Prob. of staying same occupation	-0.0638***	-0.0794***	-	-
	(0.0144)	(0.0184)		

Notes: The table presents the semi-parametric IV estimator motivated by Battistin and Rettore (2008), together with the adjusted matching results presented in Table 4, of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor market (see text). The instrument is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Bootstrapped standard errors (100 replications) are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

All coefficients in Table 7 have the same sign in both estimation procedures when statistically significant. The magnitudes of the estimated coefficients, however, are much

[34] Results shown in the Appendix using a standard 2SLS estimator with age as an instrument also provide a qualitatively similar set of results, although estimated coefficients vary in magnitude.

larger in absolute terms when using the semi-parametric IV estimator. For instance, the point estimate of the effect of the apprenticeship program on the probability of finding a permanent formal job is 11.0% after 2-3 years and 9.6% after 4-5 years compared to 7.9% and 6.9%, respectively, when the adjusted matching estimation is used.

The semi-parametric IV procedure also provides larger estimates (in absolute values) of the effect of the apprenticeship program on turnover and experience outcomes. The estimates are approximately 25% larger than those found using the adjusted matching estimators. The effect of the program on the accumulated number of quits is now statistically significant and estimated at 11.0% in the short run. According to the semi-parametric IV estimator, the program significantly reduced the number of admissions (by 20.2% after 2-3 years and 24.4% after 4-5 years), dismissals (by 50.9% after 2-3 years and 24.9% after 4-5 years), accumulated number of months (by 0.99 months after 2-3 years and 0.59 months after 4-5 years), and the probability of staying in the same establishment (by 5.6% after 2-3 years) and occupation (by 7.9% after 2-3 years).

In sum, the results when we use the semi-parametric IV estimator as an alternative procedure to address selection on unobservables are qualitatively similar to those discussed in section 5.1.

5.3.2 Robustness for selection in the 18-year-old group

Assuming that the program is seen as a good opportunity by young individuals searching for their first job, one may wonder whether those who start working at the age of 18 may have been (self-)selected out of the program when they were 17 years old. If this were the case, assumption A.1 of our empirical identification strategy would be violated.

However, such a selection scheme was not in place for those who started to work in 2001, as this was the first year of the apprenticeship program. Those aged 18 in 2001 could not have been selected to take part in the apprenticeship program when they were 17, because they were 17 in 2000, the year before the enactment of the Apprentice act.

In this sub-section, we estimate our baseline specification with the sample restricted to those young individuals (aged 17 or 18) who had their first job in 2001 instead of 2001-2003, the period used in all previous tables. The trade-off is that we obtain less-precise estimates for the 2001 sample given the much smaller number of observations. Table 8 presents estimates of the impact of the program when we use the sample restricted to 2001 entrants (see columns 2 and 4). For comparison, the first and third columns of the table reproduce the base estimates in Table 4.

Table 8: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes by Year of First Job - Adjusted Matching Estimator

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	2001-03 Sample	2001 Sample	2001-03 Sample	2001 Sample
Employment				
Employment prob. - permanent formal job	0.0791***	0.1027***	0.0687***	0.0125
	(0.0119)	(0.0337)	(0.0115)	(0.0353)
Employment prob. - any formal job	-0.0244**	-0.0718**	-0.0129	-0.1393***
	(0.0114)	(0.0308)	(0.0117)	(0.0362)
Turnover and experience				
Accumulated number of admissions	-0.1667***	-0.4444***	-0.2061***	-0.5629***
	(0.0277)	(0.0766)	(0.0267)	(0.0721)
Accumulated number of dismissals	-0.3786***	-0.6801***	-0.2091***	-0.6212***
	(0.0257)	(0.0677)	(0.0244)	(0.0742)
Accumulated number of months	-0.7358***	-1.8447***	-0.5114***	-2.7905***
	(0.0087)	(0.1049)	(0.0829)	(0.6782)
Accumulated number of quits	0.0518*	0.0061	0.0308	-0.0650
	(0.0294)	(0.0830)	(0.0299)	(0.0841)
Prob. of staying same establishment	-0.0381***	-0.0358***	-	-
	(0.0070)	(0.0023)		
Prob. of staying same occupation	-0.0638***	-	-	-
	(0.0144)			

Notes: The table presents the adjusted matching estimator (Dias *et al.*, 2013) of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor market (see text). Columns '2001 Sample' refer to workers that entered the labor market in 2001, while columns '2001-03 Sample' reproduce the estimates in Table 4 (with workers that entered the labor market in years 2001-2003) for comparison. The following covariates are used for matching: dummies for gender, schooling, industry, geographical region, and the year in which the worker first entered the formal sector. The instrument for all estimates is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Bootstrapped standard errors (100 replications) are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

The results in Table 8 show that we obtain similar qualitative results when we compare the two samples. For the short-run horizon (t+2 and t+3), all coefficients have the same sign[35], although the magnitudes of the coefficients in the 2001 sample tend to be larger (and less precisely estimated). For the medium-run horizon, there are a few changes in the significance status of the estimates. The coefficients on employability in the medium run either flip from significantly different from zero at 5% to non-significant, or the other way around.

6. Concluding comments

The main objective of this paper has been to estimate the impact of the Brazilian Apprenticeship program on subsequent labor market outcomes in the formal sector of youths with no previous experience in the labor market. The program provides subsidized job experience under a temporary contract combining substantial in-classroom training with on-the-job training for up to two years.

We make use of a very rich longitudinal matched employer-employee dataset (*RAIS, Relação Anual de Informações Sociais*) to define a broad set of outcome variables such as formal sector employment (overall and permanent), turnover and experience accumulated in the labor market. The longitudinal aspect of the dataset also allowed estimation of the program's effects over the short run (two and three years after the program) and the medium run (four and five years after the program).

Using methods that address selection on unobservables, we find that the program is effective at increasing the probability of employment in permanent jobs and at decreasing turnover. Both results hold in the short and medium run. We also find a negative effect on accumulated formal labor market experience in the short run. These results are compatible with a relatively higher increase in reservation utility for apprentices that makes them more prone to make investments aiming at getting a good (stable) job. Such

[35] The only point estimate with a different sign is not significantly different from zero in either sample.

investment could take the form of either a more selective search for a subsequent job or the acquisition of more skills.

The investment in the acquisition of more skills is confirmed by results showing that former apprentices have higher chances of increasing their education level than youths in the control group. We also show that the effects of the program are larger for workers who had their first jobs in large firms. This could be due to motivating aspects of working in more-professional environments, which in turn reinforces our interpretation that the intervention boosts the reservation utility for the treated group in subsequent job search episodes. Higher impacts of apprenticeship in larger firms could also be driven by a credential effect, as having a work experience in a large firm is probably more valued by other potential employers. Lastly, it could also be explained by better on-the-job training provided by large firms. A policy implication of this result could be to adopt measures that make hiring apprentices more attractive for large firms.

Overall, these are encouraging results given the barriers faced by youth when entering the labor market in developing countries. The Brazilian type of apprenticeship contract seems to be a better stepping stone to stable formal jobs than other temporary contracts. It is apparently able to break the vicious cycle for (low-skilled) young workers who have difficulties accumulating formal job experience and, hence, tend to end up in low-productivity and high-turnover jobs.

Appendix: Results using a standard 2SLS estimator with age as an instrument

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Adjusted Matching	Standard 2SLS	Adjusted Matching	Standard 2SLS
Employment				
Employment prob. - permanent formal job	0.0791***	0.0792***	0.0687***	0.0696***
	(0.0119)	(0.0113)	(0.0115)	(0.0113)
Employment prob. - any formal job	-0.0244**	-0.0097	-0.0129	0.0021
	(0.0114)	(0.0110)	(0.0117)	(0.0111)
Turnover and experience				
Accumulated number of admissions	-0.1667***	-0.1334***	-0.2061***	-0.1842***
	(0.0277)	(0.0251)	(0.0267)	(0.0256)
Accumulated number of dismissals	-0.3786***	-0.3788***	-0.2091***	-0.1871***
	(0.0257)	(0.0241)	(0.0244)	(0.0249)
Accumulated number of months	-0.7358***	-0.9166***	-0.5114***	-0.5134***
	(0.0087)	(0.1475)	(0.0829)	(0.1548)
Accumulated number of quits	0.0518*	0.0606**	0.0308	0.0033
	(0.0294)	(0.0301)	(0.0299)	(0.0334)
Prob. of staying same establishment	-0.0381***	-0.0452***	-	-
	(0.0070)	(0.0040)		
Prob. of staying same occupation	-0.0638***	-0.0405***	-	-
	(0.0144)	(0.0074)		

Notes: The table presents the standard 2SLS estimator, together with the adjusted matching results presented in Table 4, of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor market (see text). The instrument is a dummy that assumes value 1 (0) if the age of the worker is 17 (18). Bootstrapped standard errors (100 replications) are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

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