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

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TEXTO PARA DISCUSSÃO

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

Carlos Henrique Corseuil  
Miguel Foguel  
Gustavo Gonzaga



# APPRENTICESHIP AS A STEPPING STONE TO BETTER JOBS: EVIDENCE FROM BRAZILIAN MATCHED EMPLOYER-EMPLOYEE DATA<sup>1</sup>

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KEY WORDS: APPRENTICESHIP; YOUTH-TARGETED TRAINING PROGRAM; ADJUSTED MATCHING

**Abstract:** The objective of this paper is to evaluate the Brazilian Apprenticeship program (*Lei do Aprendiz*). This program is a youth-targeted ALMP that has been adopted at a large scale since 2000 in Brazil. 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 very rich longitudinal matched employee-employer dataset covering the universe of formally employed workers in Brazil, including apprentices. Our identification strategy exploits a discontinuity by age in the eligibility to enter the program in the early 2000's, when 17 was the age limit to take part in the program. We examine the impacts on employability, wage growth and attachment to the formal labor market using other temporary workers as a control group. We find that the program increases the probability of employment in permanent jobs in 2-3- and 4-5-year horizons. We also find a positive impact on real wages that increases over time. These results hold when we isolate the effects of the training dimension of the program by using an alternative control group composed of subsidized temporary workers. We show evidence that the positive effects of the program are much larger for less-educated workers and for workers who had their first jobs 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. 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).

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.

Several Latin American countries, in particular, have recently implemented youth-targeted training programs. 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 a youth-targeted training program even more appealing in these countries for at least two reasons. First, expected returns to

human capital investments are larger in low-schooling countries.<sup>2</sup> Second, 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).<sup>3</sup>

Although there is some evidence of positive returns of youth-targeted training programs in Latin American countries (Attanasio *et al.*, 2011; Card *et al.*, 2011; and Betcherman *et al.*, 2007), little is known about what really helps youths find good jobs. Typically, these programs combine in-classroom and/or on-the-job training methods with different contents (life-skills, technical and vocational material) and sometimes also include job-search assistance and payroll subsidies. However, few studies have tried to separate out the impact of these several programs' dimensions on youths' labor market outcomes.

The main goal of the paper is to evaluate a Brazilian multidimensional 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 training at the firm.

The contribution of this paper is threefold. 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

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<sup>2</sup> It has been argued that non-cognitive skills (also known as life-skills) are also relatively scarce in low- or middle-income countries with deficient schooling systems. Therefore, training devoted either to cognitive or non-cognitive content should yield larger returns in these countries.

<sup>3</sup> 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).

market outcomes for youths, such as employment, accumulated experience, turnover, and wages. 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). Second, because we are able to disentangle the effect of the training dimension from the wage subsidy component of the program, the paper contributes to the literature on the effectiveness of ALMPs that combine multiple components into a single intervention. Third, the results contribute to the scarce literature on the effects of skill-enhancing programs for youths in developing countries. 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 in a developing country and to follow them over time.<sup>4</sup> 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 conduct the empirical analysis.

We examine how the Apprenticeship program affects the career prospects of these young workers in terms of degree of attachment to the formal labor market and wage growth. Note that prospects are not good for a young worker who gets a precarious low-productivity job, even in the formal sector. Thus, the choice of outcome variables must

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<sup>4</sup> 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.*, 2012).

go beyond the probability of getting any formal job. More specifically, we estimate the impacts of the program on the probability of employment in formal jobs with open-ended contracts—which can be considered a measure of the quality of the job—on real wages, on measures of turnover, and on measures of formal labor market experience.

Because the decision to participate in the Apprenticeship program probably depends on unobservable characteristics of workers and firms that are likely 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 a standard matching estimator by allowing selection based on unobservables.<sup>5</sup> The adjusted matching method requires an instrument with zero probability of participation in at least one value of its domain. This is provided by the eligibility rule of the Brazilian apprenticeship program, which precludes the participation of individuals over 18 years old.

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.

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<sup>5</sup> This use of an IV to address selection based on unobservables in the matching framework is analogous to the more standard use in the regression framework. 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.

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 8.1% after 2-3 years and by 6.4% 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 on the reservation utilities of workers with respect to subsequent jobs.

Our estimates also show a substantial real wage increase. We find that real hourly wages of apprentices, controlling for selection on observable and unobservable characteristics, evolved from a lower level during the apprenticeship period and the following 2-3 years after the program to a larger level 4-5 years after the program. This suggests that apprentices shared part of the costs of the program in the form of lower wages during the apprenticeship but reaped the returns of training with significantly larger wages in the medium run.

To improve understanding of how these effects take place, we use an alternative control group of workers hired by another type of temporary contract existent in Brazil that offers wage subsidies slightly larger than in the apprenticeship contract but does not have any training provision. By this procedure, we are able to separate out the effect of the training dimension (in-classroom and on-the-job components combined) of the apprenticeship program. The results of this exercise show that the positive impacts of the Apprenticeship program come mainly through its training dimension. When compared with workers with other subsidized temporary contracts, the impact of the Apprenticeship program remains large and becomes more important as time goes by.

We also investigate whether the impact of the program is heterogeneous by initial level of schooling and firm size. We find that the positive effects of the program, such as



increasing employability in permanent jobs and real wages, are much more pronounced for less-educated workers—defined here as workers with less than complete primary school. This result is in line with evidence from the literature that tends to find larger returns of training in developing countries with particularly low levels of schooling.

We also find much larger effects of the program on employability in permanent jobs and real wages in the medium run for workers who had their first job in large firms. This suggests that the positive effects of the program in the medium run are boosted when training occurs in large firms.

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 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 four 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 Ryan (2001), suffered from important limitations on identifying causal effects of apprenticeship. Few recent papers attempt to properly address the nonrandom selection of youth individuals into the program. 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.

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. Apart from differences in data and country of analysis, 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, and wages. 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 and non-apprentice jobs. The potential bias coming from this imbalance is more likely 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, as we are able to restrict the sample to 17- and 18-year-old individuals without any previous formal job experience.

The second strand of the literature connected to our paper focuses on identifying the relative effectiveness of the different types of ALMP programs offered to young workers. Larsson (2003), Dorsett (2006), and Caliendo *et al.* (2011) provide important contributions on this ground, analyzing the relative impact of distinct ALMPs within the same countries. The three papers point to a higher effectiveness of wage subsidies schemes when compared with in-classroom training. Their results, however, could be biased since they ignore the role of unobservables in the selection into the programs. This may be particularly relevant when different programs rely on different selection criteria. For instance, one may suspect that firms positively select individuals for wage subsidy schemes relative to those who stay in training schemes outside the firm (hence, not selected by the employers themselves). It is worth noting that in all these papers, the

compared interventions are either different programs or different (and exclusive) routes of the same program to wage subsidies or training schemes.

Our paper contributes to this strand of the literature, as the apprenticeship program in Brazil combines these two dimensions within the same program, and we are able to isolate the effects of one of them. In one exercise, we evaluate the impact of the training dimension relative to the wage subsidy component of the program by using a control group composed of individuals employed under a special type of temporary contract with slightly larger wage subsidies but without a training requirement. We believe this feature tends to minimize differences in selection mechanisms across individuals subject to distinct dimensions of the program. Our results show that the effects of the apprenticeship program come mainly from its training component.

Attanasio *et al.* (2011) evaluates a youth-targeted training program in 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. However, because the treated individuals took the two modules of the program, they could not isolate the effect of each component. They find significant impacts of the program, especially on formal employment and wages.

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). Recent reviews by 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.*, 2014, for a review of youth-targeted programs in six Latin American countries). Other studies that

exploit randomized experiments in Latin American countries (Dominican Republic and Brazil) have also found positive impacts of in-classroom training programs on some youth labor market outcomes (Card *et al.*, 2011; Ibararán *et al.* 2014; Calero *et al.* 2014).

Finally, our findings contribute to a fourth strand in the literature that 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.<sup>6</sup> One of the main concerns with fixed-term contracts is the lack of on-the-job training of unskilled workers (Cabralés *et al.*, 2014). In fact, some authors have argued that apprenticeships are better arrangements than other fixed-term contracts, precisely because systematic training could improve the prospects for subsequent job promotion, higher wages, and employment stability (Biavaschi *et al.*, 2012). We find positive effects of the apprenticeship program on several labor market outcomes of youths (including employability in open-ended formal jobs in the medium term), suggesting that apprenticeship initiatives seem to be a good stepping stone in the labor market, at least for youths in developing countries. We also find evidence that the training component of the apprenticeship program is the main factor sustaining the good prospects of apprentices in the labor market.

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<sup>6</sup> 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).

### **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.<sup>7</sup> 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. 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 enroll at school.

Like other youth-targeted programs, the Brazilian Apprenticeship program combines different types of ALMPs into a single program. It is predominantly a professional training program, but it also concedes employment subsidies through a reduction in payroll and firing costs, and it facilitates participants' job search through a network of formal sector firms that access data on apprentices.

The program combines theory and practice with regard to training. Although it emphasizes the on-the-job training dimension, the importance of the in-classroom component is substantial. The in-classroom training courses are much longer than in most other developing countries, with a minimum of hours varying by occupation from 400 to 800 and a maximum number that can reach 1,960 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.

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

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<sup>7</sup> 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).

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 2015.

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.<sup>8</sup>

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.<sup>9</sup>

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<sup>8</sup> 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).

<sup>9</sup> 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.

Establishments should hire at least 5% (and at most 15%) of their employees who work in occupations requiring formal training as apprentices.<sup>10</sup> 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 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.<sup>11</sup> 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.

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<sup>10</sup> 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.

<sup>11</sup> There are incentives for truthful reporting, as the main purpose of RAIS is to administer a federal wage supplement (*Abono Salarial*) to formal workers.



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. There are two measures of wages: the average monthly wage of a worker over the employment spell during the year and the December wage. We exclude agriculture and the public sector.

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). To construct a 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.

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 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.<sup>12</sup>

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<sup>12</sup> 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

## **4. Methodology and estimation procedures**

### *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.

Table 1 shows that the observable characteristics differ substantially between apprentices and non-apprentices across some 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.

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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.

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.<sup>13</sup> 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

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<sup>13</sup> The empirical application in Dias *et al.* (2013) is also implemented under a similar RD setting that uses age as the instrument.

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 Hahn *et al.* (2001).<sup>14</sup> 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

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<sup>14</sup> 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).

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:

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

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:**  $Y_0 \perp Z|X$ ;

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

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

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.<sup>15</sup> 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).

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:

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

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  $E[Y_0 | X, Z = z^*, D = 0]$  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  $\alpha(x)$  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 had been 17 years old until September 2005. Recalling that the program is not compulsory, we thus have an appropriate setting in which the age of

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<sup>15</sup> In fact, that condition could be stated in terms of mean (conditional) independence.

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 (2003), 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. The standard matching estimates were computed using Epanechnikov kernel weights. Only observations in the region of the common support of the propensity score were used for computing standard and adjusted matching estimates.<sup>16</sup> Inference is based on standard errors estimated from bootstrapping with 100 replications.

The outcomes of interest can be classified into four groups of variables: i) formal employment probability (in permanent and overall formal jobs); ii) real hourly and monthly wages; iii) measures of turnover (accumulated number of admissions and dismissals; probability of quits); and iv) measures of experience in the formal labor market (accumulated number of months in formal sector jobs; probability of staying in the same establishment or in the same occupation).

#### *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

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<sup>16</sup> 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 5<sup>th</sup> percentiles and the minimum of the 99<sup>th</sup> percentiles of the propensity score distributions of the treated and control groups.

period 2001-2003.<sup>17</sup> 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.

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 18<sup>th</sup> anniversary introduces a discontinuity in employability, as individuals take more legal responsibilities at this age.<sup>18</sup> 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.<sup>19</sup>

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 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

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<sup>17</sup> 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.

<sup>18</sup> In Brazil, 18 years old is a threshold defining criminal responsibilities and permission to drive vehicles.

<sup>19</sup> The outcome variables are compared when the youth who entered the labor market at 17 years old is approximately 19 to 22 years old.



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).

The results show that for eight of the nine subsequent labor market outcomes, we cannot reject the null hypothesis that the 18-year-old dummy coefficient is zero at the 5% significance level.<sup>20</sup> 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.

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<sup>20</sup> 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 two outcomes, we reject the null at the 10% significance level (monthly wage variation and number of months employed in a two-year window starting two years after labor market entrance). For all six other outcomes, including subsequent employment probability (either in an open-ended contract or not) and hourly wage variation, there is no evidence that the subsequent opportunities in the labor market differ across 17- and 18-year-old entrants.

## **5. Econometric Results**

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, the degree of attachment to the formal labor market and wage growth 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 four groups of 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.

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 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 trajectories of the real wages of apprentices and non-apprentices are better seen graphically. Figure 2 shows that the real hourly wages of apprentices are considerably lower in period  $t$ , when both enter the labor market. Apprentices, however, have a larger increase in real hourly wages over time. Two years after the program, the real hourly wages of apprentices are already slightly above the real hourly wages of non-apprentices. This gap slightly increases between  $t+2$  and  $t+5$ .

The outcomes presented in Table 3 are raw averages. Table 4 presents our estimation results for the impact of the Apprenticeship program 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 four groups of outcomes of interest: employment probability, real wages, turnover, and experience. 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  $t+5$ ), where period  $t$  corresponds to the year in which apprentices and non-apprentices entered the labor market.

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 8.1% after 2-3 years and by 6.4% after 4-5 years.

The impact of the program on having any formal job was not statistically different from zero in the short and medium run, suggesting that it probably negatively affected the probability of having another temporary job.

We also find a positive and statistically significant effect of the Apprenticeship program on the variation of real hourly and monthly wages in the short and in the medium run. Our estimates show a substantial real wage increase. We find that real hourly wages increased by R\$1.58 after 2-3 years and by R\$1.97 after 4-5 years from a baseline of average real hourly wages of apprentices in year  $t$  of R\$1.95.<sup>21</sup>

These results confirm the visual impression in Figure 2 described above that the real hourly wages of apprentices evolved from a lower level during the apprenticeship period to a larger level 4-5 years after the program, even after controlling for observable and unobservable characteristics. This suggests that workers apparently shared part of the costs of the program in the form of lower wages during the apprenticeship period but reaped the returns of the investment in training with similar wages in the short run and significantly larger wages in the medium run when compared with other workers on temporary contracts. This is consistent with the standard view that firms do not have incentives to finance investment in general human capital, so they lower wages during the training period.

Our estimates also 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 14.8% lower than for other temporary contracts after 2-3 years and 19.6% lower after 4-5 years. The impact on the number of dismissals was an increase of 36.3% after 2-3 years and of

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<sup>21</sup> The average exchange rate along the period of analysis was approximately 1R\$ = 0.4US\$.

20.5% after 4-5 years. The effects on the probability of quitting were not statistically significant in either the short or medium run.

Finally, 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 0.76 months less than workers with other temporary contracts in years t+2 and t+3 and 0.55 months less in years t+4 and t+5. 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 3.9% lower and of working in the same occupation was 3.9% lower after 2-3 years.

Taken together, these findings suggest that the program substantially increased the employability of former apprentices, helping workers successfully complete the transition from school to permanent formal jobs. The results also show that having a first job as an apprentice significantly increases the productivity of young workers when compared to other temporary contracts, as illustrated by the substantial rise in real wages in the medium run. Apprenticeship looks like a much better stepping stone to permanent and better jobs when compared with other temporary jobs.

The findings of a much lower turnover for participants in the program and a negative effect on accumulated formal labor market experience suggest that the program increased the reservation wage (and/or the "reservation job quality") of participants compared with other temporary contracts. As a result, apprentices 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 paid jobs.

## *5.2 Mechanisms and Heterogeneity Analysis*

### *5.2.1 Isolating the Training Component of the Apprenticeship Program*

As discussed above, one of the main shortcomings in the empirical literature of the evaluation of youth-targeted programs is the lack of attempts to decompose the effects of the programs in their many dimensions. The Brazilian Apprenticeship program combines the training dimension with employment payroll subsidies. The results presented in Table 4 use as a control group all other temporary workers, that is, workers aged 17 or 18 in 2001-2003 for whom their first job was under any other type of temporary contract.

In this sub-section, we present estimates of the impact of the program using an alternative control group, which is a subset of the previous one. We use workers aged 17 or 18 in 2001-2003 for whom their first job was under a particular form of temporary contract, established by Law 9601/1998, which provides subsidies slightly larger than those offered in the apprenticeship program. Because this subsidized temporary contract does not require training, this strategy allows us to isolate the impact of the training component in the Apprenticeship Law.

Law 9601 was promulgated in January 1998. It allowed firms to hire from 20 to 50% of their workers under subsidized special temporary contracts, with this proportion decreasing with the size of the firm. As in the apprenticeship program, workers hired under this contract would also have only 2% of their wages deposited into the worker's job-separation fund (FGTS) account instead of the usual 8% rate (or the 8.5% rate between 2001 and 2006), which is a 6 p.p. (6.5 p.p between 2001 and 2006) reduction in payroll taxes.<sup>22</sup>

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<sup>22</sup> Law 9601/1998 also established a decrease of approximately 3.5 percentage points, on average, in other payroll taxes for workers hired under these special temporary contracts. Payroll subsidies for workers hired under this regime are thus larger than for apprentices. Estimates of the impact of training when

Columns 2 and 4 in Table 5 shows the results of the impact of the program when we use this alternative control group of temporary workers. For comparison, the first and third columns of the table reproduce the base estimates in Table 4.

The table shows that the effects of the program come mainly through its training dimension. When compared with other subsidized temporary contracts, the Apprenticeship program increased the probability of having a non-temporary formal job by 6.5% after 2-3 years and by 6.9% after 4-5 years. These estimates are slightly lower in the short run (6.5% compared with 8.1%) and larger in the medium run (6.9% compared with 6.5%) than those found when we use all other temporary contracts as a control group. That is, the impact of the program remains large and becomes more important as time goes by, as expected, when we isolate its training dimension.

The impact of the program on having any formal job (permanent or temporary) is now significant after 4-5 years, when subsidized temporary workers are used in the control group. This suggests that workers hired under subsidized contracts probably had a difficult time finding other (non-subsidized) temporary contracts, which is apparently not the case for apprentices.

The effects of the program on wage growth are lower when subsidized temporary workers are used in the control group, but they are still substantial. Indeed, the estimates show that, relative to this control group, the real hourly wages of the apprentices increased by R\$1.08 after 2-3 years and by R\$1.21 after 4-5 years from a baseline of average real hourly wages of apprentices in year  $t$  of R\$1.95.

The impacts of the program on turnover and experience variables are no longer significant in the medium run. In the short run, we find that the program negatively affects the number of admissions (by 15.7%), increases the probability of quitting (by

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these workers are used as a control group should therefore be viewed as a lower bound for the true impact.

13.2%), and decreases the probability of remaining at the same establishment (by 3.1%).

### *5.2.2 Heterogeneity: Workers with Complete Primary Schooling*

An important component of the Apprenticeship program is the requirement of enrolling in school if they have not completed their primary school education. For these workers, the effects of the program are thus a combination of receiving training and completing primary school, which should have larger returns given the lower level of general human capital. On the other hand, the effects of the program could be less effective given decreasing returns from the work-school double shift, with possible negative impacts on learning capacity. In this sub-section, we assess which of these two effects prevails by analyzing the differences in our estimates of the impact of the program by level of primary education.

Table 6 presents the estimates of the impact of the program for samples of workers with less than complete primary school (“less-educated” workers, columns 1 and 3) and workers with at least complete primary school (“more-educated” workers, columns 2 and 4). The results show that the effects of the program are much more pronounced for less-educated workers. Because the coefficients are less precisely estimated for workers without complete primary school, we cannot say that the program had a larger effect on the probability of non-temporary employment for these workers. After 2-3 years, the program increased the probability of having a non-temporary formal job by 7.2% (with a p-value of 11.1%) for less-educated workers and by 6.4% for more-educated workers. After 4-5 years, the coefficients are 8.8% (p-value of 5.5%) and 4.7%, respectively. The program did have a negative and statistically significant (at 10%) effect on the probability of having any formal job for less-educated workers in the short run, meaning



that it negatively affected the probability of having another temporary job in the short run.

We find larger effects of the Apprenticeship program on the variation of real hourly and monthly wages in the short and medium run for less-educated workers. For real hourly wages, for example, our estimates show an increase of R\$1.79 for less-educated workers after 2-3 years compared with R\$1.26 for more-educated workers. After 4-5 years, the estimates show an increase of R\$2.03 for less-educated workers and R\$1.46 for more-educated workers.

The table also shows that the results for all coefficients related to turnover and experience variables are also much larger, in absolute values, for the sample of less-educated workers.

We interpret this as evidence that the positive effects of the program, in terms of increasing the probability of employment in permanent jobs and real wages and in terms of decreasing turnover, are more pronounced for less-educated workers. This is compatible with the notion of expected larger returns of providing training for less-educated workers, which is especially important in developing countries with particularly low levels of schooling. The effects of the program for these workers are not only a combination of training and completing primary school but also an opportunity for these workers to engage in a formal labor market experience, which is usually not available to them at this age and level of education.

### *5.2.3 Heterogeneity: Large Firms*

Our estimates of the positive impacts of the apprenticeship program and its training dimension on the probability of employment in non-temporary jobs and real wages bring additional questions regarding exactly through which mechanisms the program delivers its positive results. One could argue that the essential dimension of the program

works through the opportunity given to young workers to get their first jobs in large formal firms, something that would be otherwise very difficult to achieve.

In this sub-section, we assess this possibility by dividing our sample by firm size. We estimate our model for large firms (defined here as firms with more than 250 employees) and small and medium firms (firms with less than 250 employees).<sup>23</sup> Table 7 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 employability are approximately the same in the short run for large and small firms: 7.2% and 7.3%, respectively. However, the effect in the medium run (after 4-5 years) is much larger for large firms: 7.1% compared with 4.5% for small and medium firms.

As for real wages, the estimated impacts are similar by firm size in the short run, but the effect on hourly wage changes in the medium run is much larger for large firms (R\$2.43 compared with R\$1.15 for small-medium 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. The program's impact on the probability of remaining in the same establishment is negative and much lower in absolute terms for large firms in the short run (-4.7% compared with -2.1% for small-medium firms). Apprentices in large firms also spent much less time in the formal sector in the short run (-1.2 months) and in the medium run (-0.9 month).

Altogether, these results indicate that in the medium run, the apprentices who had their first job in large firms became more productive (as reflected in their wages and probability of finding an open-ended job) and probably more picky, searching more for

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<sup>23</sup> The median size of Brazilian firms in our matched formal employer-employee data is 14 workers (Corseuil *et al.*, 2013).

other jobs and staying less in jobs they do not like, especially in the short run. The trade-off is a lower probability of finding a permanent job in the short run.

These results suggest that part of the positive effects of the program in the medium run are 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 or even from the motivating aspects of working in a more professional environment. A policy implication would be to adopt measures that would make it more attractive for large firms to hire apprentices or, in general, to offer more job entry positions for young workers.

### 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.<sup>24</sup> The second and fourth columns in Table 8 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.

All coefficients in Table 8 have the same sign in both estimation procedures when statistically significant. The magnitudes of the estimated coefficients, however, are much 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 11.3% after 2-3 years and 9.3% after 4-

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<sup>24</sup> 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.

5 years compared to 8.1% and 6.4%, respectively, when the adjusted matching estimation is used. Real hourly wages are estimated to increase by R\$1.97 after 2-3 years and by R\$2.24 after 4-5 years from a baseline of average real hourly wages of apprentices in year  $t$  of R\$1.95 compared with the adjusted matching estimates of R\$1.58 and R\$1.98, respectively.

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 10.5% in the short run. According to the semi-parametric IV estimators, the program significantly reduced the number of admissions (by 18.4% after 2-3 years and 23.7% after 4-5 years), dismissals (by 50.1% after 2-3 years and 25.1% after 4-5 years), accumulated number of months (by 1.04 months after 2-3 years and 0.68 months after 4-5 years), and the probability of staying in the same establishment (by 5.9% after 2-3 years) and occupation (by 4.7% after 2-3 years).

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

### *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 work at the age of 18 may have been (self-)selected out of the program when they were 17 years old. If this was 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 9 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.

The results in Table 9 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<sup>25</sup>, 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. Two out of ten coefficients either flip from significantly different from zero at 5% to non-significant, or they flip the other way around.<sup>26</sup>

## **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 combines

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<sup>25</sup> The only point estimate with a different sign is not significantly different from zero in either sample.

<sup>26</sup> A third flip appears if we consider the 10% significance level.

subsidized job experience under a temporary contract with concomitant in-classroom training.

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), wage growth, 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 and higher wage growth for apprentices both in the short and the medium run. These results are compatible with an increase in reservation utility for apprentices while they keep searching for a subsequent job.

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.

Concerning the training dimension of the program, we evince two important results. First, we find that the impact is much larger for workers with incomplete primary school. This result is in line with the view that training should display higher impacts for workers with low human capital. Second, the apprenticeship contract performs better, especially in terms of future employment in open-ended (formal) jobs and wage growth than another type of temporary contract with slightly larger wage subsidies but with no training provision. This piece of evidence contributes to policy discussions on the effectiveness of ALMPs that combine different instruments into one single intervention.

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

**Table A1: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes - Standard 2SLS Estimator, Full Sample**

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Adjusted Matching	Standard 2SLS	Adjusted Matching	Standard 2SLS
<b>Employment</b>				
Employment probability - permanent formal job	0.0813*** (0.0127)	0.0792*** (0.0113)	0.0641*** (0.0119)	0.0696*** (0.0113)
Employment probability - any formal job	-0.019* (0.0114)	-0.0097 (0.011)	-0.0144 (0.0118)	0.0021 (0.0111)
<b>Wages</b>				
Variation in hourly wages	1.5795*** (0.1826)	1.3161*** (0.1362)	1.9741*** (0.2918)	1.3214*** (0.2617)
Variation in monthly wages	363.3922*** (26.3854)	344.4668*** (19.2163)	433.5919*** (31.9418)	370.3686*** (31.0698)
<b>Turnover</b>				
Accumulated number of admissions	-0.1477*** (0.028)	-0.1334*** (0.0251)	-0.1963*** (0.0309)	-0.1842*** (0.0256)
Accumulated number of dismissals	-0.3627*** (0.027)	-0.3788*** (0.0241)	-0.2048*** (0.0275)	-0.1871*** (0.0249)
Accumulated number of quits	0.0478 (0.0297)	0.0606** (0.0301)	0.0275 (0.0349)	0.0033 (0.0334)
<b>Experience</b>				
Accumulated number of months	-0.764*** (0.1752)	-0.9166*** (0.1475)	-0.5451*** (0.1783)	-0.5134*** (0.1548)
Prob. of staying same establishment	-0.0391*** (0.0043)	-0.0452*** (0.004)	-	-
Prob. of staying same occupation	-0.0386*** (0.0087)	-0.0405*** (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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Table 1 – Observable Characteristics: Temporary Workers, 1<sup>st</sup> Job at age 17

<b>Characteristics</b>	<b>Age 17</b>	
	<b>Nonapprentice</b>	<b>Apprentice</b>
Male	0.630	0.644
<b>Schooling</b>		
Less than 5 years of schooling	0.070	0.004
6 to 8 years of schooling	0.145	0.107
9 to 11 years of schooling	0.654	0.844
More than 12 years of schooling	0.130	0.045
<b>Industry</b>		
Construction	0.018	0.012
Manufacturing	0.176	0.229
Trade	0.127	0.172
Services	0.678	0.587
<b>Establishment size</b>	438.1	469.4
<b>Region</b>		
North	0.031	0.037
Northeast	0.099	0.096
Southeast	0.617	0.690
South	0.216	0.117
Mid-West	0.038	0.060
Sample size	8.048	11.377

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

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

<b>Outcome</b>	<b>Years t+2 or t+3</b>
<b>Employment</b>	
Employment probability - permanent formal job	-0.00558 (0.00958)
Employment probability - any formal job	-7.70e-05 (0.00936)
<b>Wages</b>	
Variation in hourly wages	-0.0908 (0.0844)
Variation in monthly wages	-19.93* (11.14)
<b>Turnover</b>	
Accumulated number of admissions	0.0255 (0.0203)
Accumulated number of dismissals	0.0437** (0.0195)
Accumulated number of quits	-0.0335 (0.0208)
<b>Experience</b>	
Accumulated number of months	0.213* (0.122)
Prob. of staying same establishment	-0.00272 (0.00443)

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).

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

Outcomes	Nonapprentice	Apprentice
<u>Employment in formal jobs</u>		
Employment probability - permanent job - years t+2 or t+3	0.51	0.57
Employment probability - permanent job - years t+4 or t+5	0.52	0.57
Employment probability - any formal job - years t+2 or t+3	0.63	0.61
Employment probability - any formal job - years t+4 or t+5	0.60	0.60
<u>Wages</u>		
Real hourly wage in year t	R\$ 2.92	R\$ 1.95
Average real hourly wage in years t+2 and t+3	R\$ 3.67	R\$ 3.49
Average real hourly wage in years t+4 and t+5	R\$ 4.48	R\$ 4.39
<u>Turnover</u>		
Accumulated number of dismissals - years t+2 and t+3	0.83	0.60
Accumulated number of dismissals - years t+4 and t+5	0.86	0.75
Accumulated number of admissions - years t+2 and t+3	0.92	0.83
Accumulated number of admissions - years t+4 and t+5	0.91	0.81
Probability of dismissal by quit in years t+2 or t+3	0.37	0.41
Probability of dismissal by quit in years t+4 or t+5	0.45	0.46
<u>Experience</u>		
Accumulated number of months worked - years t+2 and t+3	6.29	5.83
Accumulated number of months worked - years t+4 and t+5	6.50	6.22
Probability of staying in the same establishment - years t+2	0.04	0.02
Probability of staying in the same occupation - years t+2	0.05	0.04
Sample size	26,738	11,377

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

**Table 4: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes -  
Adjusted Matching Estimator, Full Sample**

<b>Outcome</b>	<b>Years t+2 or t+3</b>	<b>Years t+4 or t+5</b>
<b>Employment</b>		
Employment probability - permanent formal job	0.0813*** (0.0127)	0.0641*** (0.0119)
Employment probability - any formal job	-0.019* (0.0114)	-0.0144 (0.0118)
<b>Wages</b>		
Variation in hourly wages	1.5795*** (0.1826)	1.9741*** (0.2918)
Variation in monthly wages	363.3922*** (26.3854)	433.5919*** (31.9418)
<b>Turnover</b>		
Accumulated number of admissions	-0.1477*** (0.028)	-0.1963*** (0.0309)
Accumulated number of dismissals	-0.3627*** (0.027)	-0.2048*** (0.0275)
Accumulated number of quits	0.0478 (0.0297)	0.0275 (0.0349)
<b>Experience</b>		
Accumulated number of months	-0.764*** (0.1752)	-0.5451*** (0.1783)
Prob. of staying same establishment	-0.0391*** (0.0043)	-
Prob. of staying same occupation	-0.0386*** (0.0087)	-

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.

**Table 5: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes -  
Adjusted Matching Estimator, Full Sample, Control = Subsidized Temporary Contracts**

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Control Group: Temporary Contracts		Control Group: Temporary Contracts	
	All	Subsidized	All	Subsidized
<b>Employment</b>				
Employment probability - permanent formal job	0.0813*** (0.0127)	0.0653*** (0.0192)	0.0641*** (0.0119)	0.0689*** (0.0227)
Employment probability - any formal job	-0.019* (0.0114)	0.0182 (0.0222)	-0.0144 (0.0118)	0.0397** (0.02)
<b>Wages</b>				
Variation in hourly wages	1.5795*** (0.1826)	1.0828*** (0.3431)	1.9741*** (0.2918)	1.2109*** (0.3634)
Variation in monthly wages	363.3922*** (26.3854)	265.1757*** (42.8673)	433.5919*** (31.9418)	299.5854*** (29.835)
<b>Turnover</b>				
Accumulated number of admissions	-0.1477*** (0.028)	0.0203 (0.0484)	-0.1963*** (0.0309)	0.0241 (0.0465)
Accumulated number of dismissals	-0.3627*** (0.027)	-0.1568*** (0.043)	-0.2048*** (0.0275)	-0.0177 (0.0425)
Accumulated number of quits	0.0478 (0.0297)	0.1321** (0.055)	0.0275 (0.0349)	0.0566 (0.0475)
<b>Experience</b>				
Accumulated number of months	-0.764*** (0.1752)	0.1425 (0.2669)	-0.5451*** (0.1783)	0.275 (0.2512)
Prob. of staying same establishment	-0.0391*** (0.0043)	-0.0314*** (0.0076)	-	-
Prob. of staying same occupation	-0.0386*** (0.0087)	-	-	-

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 ‘Subsidized’ use workers hired under subsidized temporary contracts in the control group (see text), while columns ‘All’ reproduce the estimates in Table 4 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.

**Table 6: Estimates of the Impact of the Apprenticeship Program on Selected Outcomes by Schooling – Adjusted Matching Estimator**

Outcome	Years t+2 or t+3		Years t+4 or t+5	
	Less Educated	More Educated	Less Educated	More Educated
<b>Employment</b>				
Employment probability - permanent formal job	0.0713 (0.0473)	0.0636*** (0.012)	0.0876* (0.0474)	0.0468*** (0.0122)
Employment probability - any formal job	-0.0905* (0.0499)	0.0017 (0.0125)	-0.068 (0.0511)	-0.0074 (0.0116)
<b>Wages</b>				
Variation in hourly wages	1.7908** (0.8125)	1.2555*** (0.1257)	2.0303*** (0.6873)	1.4569*** (0.3051)
Variation in monthly wages	462.5428*** (61.5648)	330.9892*** (21.3462)	467.885*** (99.6022)	358.3899*** (31.3969)
<b>Turnover</b>				
Accumulated number of admissions	-0.4549*** (0.1182)	-0.0747** (0.0295)	-0.4141*** (0.1217)	-0.1502*** (0.0321)
Accumulated number of dismissals	-0.5741*** (0.104)	-0.2985*** (0.03)	-0.4266*** (0.1059)	-0.1531*** (0.0274)
Accumulated number of quits	0.0943 (0.1369)	0.0778** (0.0373)	0.0574 (0.1175)	-0.0073 (0.0304)
<b>Experience</b>				
Accumulated number of months	-1.7307*** (0.6279)	-0.61*** (0.1724)	-1.1476* (0.606)	-0.4515** (0.1908)
Prob. of staying same establishment	-0.0822*** (0.0147)	-0.0248*** (0.0056)	-	-
Prob. of staying same occupation	-0.1084*** (0.0341)	-0.0213** (0.0085)	-	-

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 ‘Less Educated’ refer to workers with less than complete primary school in period *t*, while columns ‘More Educated’ use workers with at least complete primary school. 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.



**Table 7: 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 Firms	Large Firms	Small-Medium Firms	Large Firms
<b>Employment</b>				
Employment probability - permanent formal job	0.0734*** (0.0172)	0.0715*** (0.0192)	0.045** (0.0142)	0.0709*** (0.0187)
Employment probability - any formal job	-0.0024 (0.0143)	-0.043** (0.0186)	-0.0111 (0.0153)	-0.0244 (0.0174)
<b>Wages</b>				
Variation in hourly wages	1.485*** (0.2721)	1.5155*** (0.2196)	1.1535* (0.4829)	2.4342*** (0.4943)
Variation in monthly wages	353.0034*** (39.3748)	341.554*** (31.3717)	386.3803*** (39.1244)	471.5974*** (81.3307)
<b>Turnover</b>				
Accumulated number of admissions	-0.0931** (0.0388)	-0.2036*** (0.0416)	-0.1929** (0.0357)	-0.2155*** (0.0418)
Accumulated number of dismissals	-0.3065*** (0.038)	-0.4109*** (0.046)	-0.1854*** (0.0374)	-0.2257*** (0.0407)
Accumulated number of quits	-0.002 (0.0419)	0.1113* (0.0568)	0.051* (0.0434)	-0.0426 (0.0522)
<b>Experience</b>				
Accumulated number of months	-0.3692* (0.2208)	-1.2003*** (0.2631)	-0.2763 (0.2035)	-0.9121*** (0.2534)
Prob. of staying same establishment	-0.0208*** (0.0057)	-0.0466*** (0.0099)	-	-
Prob. of staying same occupation	-0.0186* (0.0104)	-0.0321** (0.0136)	-	-

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.

**Table 8: 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
<b>Employment</b>				
Employment probability - permanent formal job	0.0813*** (0.0127)	0.1128*** (0.0153)	0.0641*** (0.0119)	0.0925*** (0.0146)
Employment probability - any formal job	-0.019* (0.0114)	-0.0183 (0.0148)	-0.0144 (0.0118)	-0.0036 (0.0165)
<b>Wages</b>				
Variation in hourly wages	1.5795*** (0.1826)	1.9687*** (0.2185)	1.9741*** (0.2918)	2.2441*** (0.3731)
Variation in monthly wages	363.3922*** (26.3854)	474.9485*** (30.3862)	433.5919*** (31.9418)	529.4211*** (40.1854)
<b>Turnover</b>				
Accumulated number of admissions	-0.1477*** (0.028)	-0.1844*** (0.0331)	-0.1963*** (0.0309)	-0.237*** (0.0366)
Accumulated number of dismissals	-0.3627*** (0.027)	-0.5012*** (0.0315)	-0.2048*** (0.0275)	-0.2512*** (0.0326)
Accumulated number of quits	0.0478 (0.0297)	0.1053*** (0.0377)	0.0275 (0.0349)	0.0287 (0.042)
<b>Experience</b>				
Accumulated number of months	-0.764*** (0.1752)	-1.0443*** (0.2176)	-0.5451*** (0.1783)	-0.6797*** (0.2209)
Prob. of staying same establishment	-0.0391*** (0.0043)	-0.0592*** (0.0057)	-	-
Prob. of staying same occupation	-0.0386*** (0.0087)	-0.0469*** (0.0106)	-	-

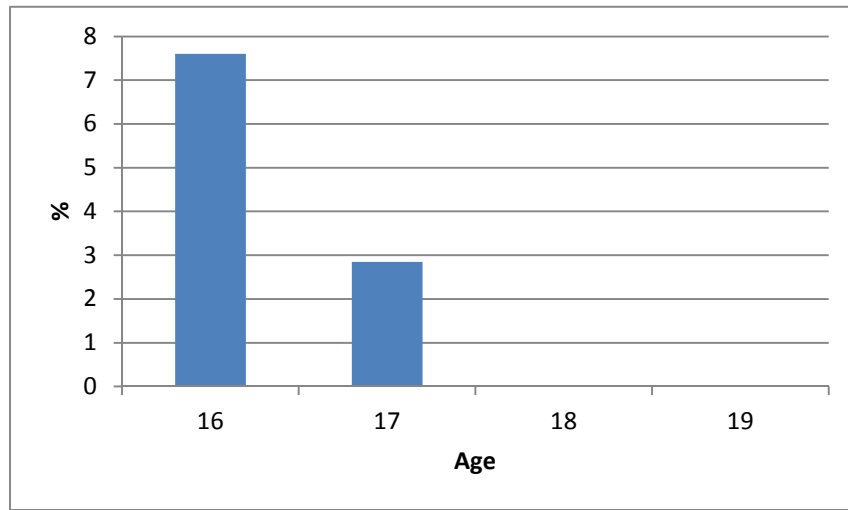
Notes: The table presents the semi-parametric IV estimator motivated by Battistin and Rettore (2008), together with the adjusted matching results presented in Table 4, of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods 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; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 9: 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
<b>Employment</b>				
Employment probability - permanent formal job	0.0813*** (0.0127)	0.0856** (0.0377)	0.0641*** (0.0119)	0.0064 (0.0351)
Employment probability - any formal job	-0.019* (0.0114)	-0.0817** (0.0371)	-0.0144 (0.0118)	-0.1452*** (0.0457)
<b>Wages</b>				
Variation in hourly wages	1.5795*** (0.1826)	1.6671*** (0.4456)	1.9741*** (0.2918)	1.825*** (0.4824)
Variation in monthly wages	363.3922*** (26.3854)	363.1124*** (67.6796)	433.5919*** (31.9418)	443.4533*** (92.5976)
<b>Turnover</b>				
Accumulated number of admissions	-0.1477*** (0.028)	-0.4384*** (0.0883)	-0.1963*** (0.0309)	-0.5465*** (0.0802)
Accumulated number of dismissals	-0.3627*** (0.027)	-0.6981*** (0.0746)	-0.2048*** (0.0275)	-0.6173*** (0.0828)
Accumulated number of quits	0.0478 (0.0297)	-0.027 (0.1057)	0.0275 (0.0349)	-0.0944 (0.1092)
<b>Experience</b>				
Accumulated number of months	-0.764*** (0.1752)	-2.0486*** (0.5885)	-0.5451*** (0.1783)	-2.324*** (0.4646)
Prob. of staying same establishment	-0.0391*** (0.0043)	-0.0279** (0.0121)	-	-
Prob. of staying same occupation	-0.0386*** (0.0087)	-	-	-

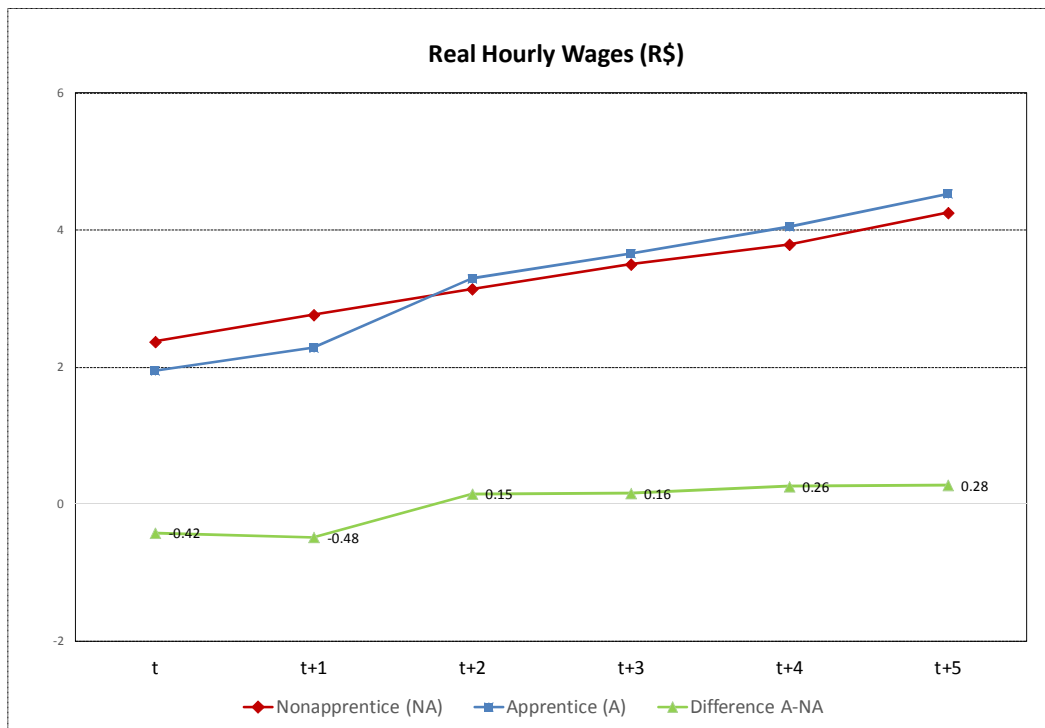
Notes: The table presents the adjusted matching estimator (Dias *et al.*, 2013) of the impact of the apprenticeship program for selected outcomes in the short run (periods t+2 and t+3) and the medium run (periods t+4 and t+5), where period *t* corresponds to the year in which workers entered the labor 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.

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



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

Figure 2: Evolution of Real Wages of Apprentices and Non-Apprentices



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