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ABSTRACT

The Effects of Youth Labor Market Reforms: Evidence from Italian Apprenticeships*

This paper estimates the causal effects of the 2003 reform of the Italian apprenticeship contract which aimed at introducing the “dual system” in Italy by allowing on-the-job training. The reform also increased the age eligibility of the apprenticeship contract and introduced a minimum floor to apprentices’ wages. Using administrative data and balancing techniques we find that five years after hiring, the new contract improves the chances of moving to a permanent job in the same firm, yet this happens mostly in large firms. There are also sizeable long-run wage effects of the reform, well beyond the legal duration of apprenticeships, compatible with increased human capital accumulation probably due to the training provisions of the reform.

JEL Classification: J24, J41, C21

Keywords: apprenticeship, permanent work, youth employment, covariate balancing, propensity score

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1. Introduction

Many countries facing high youth unemployment rates want to improve their Vocational Education and Training (VET) systems to shorten the transition between school and work (Quintini and Martin, 2006). Among the many alternative types of VET or post-school study, it seems that apprenticeship contracts work better in several countries (see for example Bonnal, Mendes, and Sofer, 2002 for France; McIntosh, 2004, 2007 for the UK; Lee and Coelli, 2010 for Australia; Winkelmann, 1996 for Germany). Apprentices receive vocational training to enhance their professional skills and competences while employers are compensated with payroll tax rebates and lower wage costs. Differently from stand-alone training programmes, the apprenticeship regime is in general heavily regulated by governments and social partners.

The implementation of the apprenticeship regime in Europe has followed different routes (Eichhorst *et al.*, 2015). In some countries such as France the apprenticeship is integrated into the educational system and focuses on theoretically-based training in schools and certificated institutions. In other countries such as Italy, the apprenticeship regime is separated and contiguous to the formal education and the learning is mostly firm-based. In Germanic speaking countries (Germany, Austria and Switzerland but also Denmark) the so-called “dual system” integrates the apprenticeship contract in the educational system but high importance is also reserved for on-the-job training.

The dual system seems to be more effective than other options of the VET system at helping youth transition into employment: with respect to alternative academic or training education, it shows a faster integration into the labour market, although the effects are more evident in big firms (Soskice 1994). Often apprentices do not find work in the training firm, suggesting that firm-specific skills are not so important in the German apprenticeship system (Parey, 2009). Furthermore, no differences in wage returns relative to the academic track are observed in the long run (Fersterer *et al.*, 2008; Pischke and von Wachter, 2008).

Many countries tried to improve their apprenticeship system moving it towards the successful examples of Germany and Switzerland (Woessmann, 2008; Gambin, 2009). The dual system ensures high-quality training and requires a high degree of employer involvement. Apprentices are paid during the apprenticeships and at the end of the experience centralized accreditation of training curricula creates transparency and promotes acceptance among employers (Dustmann and Schönberg, 2012). Not only European countries but also the US and UK tried to develop dual VET programs. For example the 2009 UK reform - the Apprenticeship, Skills, Children and Learning Act - tightened the link between the apprenticeships and employers offering large incentives for employers to increase training activities (Department for Education, Department for Business and

Skills, 2013). In the United States, both the National Youth Apprenticeship Act of 1992 and the School-to-Work Opportunities Act of 1994 were (failed) attempts to implement the dual system (Lerman and Rauner, 2012; Krueger and Kumar 2004).

Traditionally in southern countries (Italy and Spain) the role of apprenticeship (and in general vocational training) is marginal because employers prefer to hire workers on fixed-term contracts - for which there is no need of a formal vocational training - and because families have a strong preference in favour of academic training (Planas, 2005 and Felgueroso, 2010 for Spain; Tattara and Valentini, 2009 for Italy).

In this paper, we evaluate a reform of the Italian apprenticeship which tried to move it towards the German dual system: Law no. 30/2003. The reform raised the age eligibility from 24 to 29, set a minimum wage to the apprentices' compensation and allowed firms to train apprentices on the job rather than with formal education to tighten the link between employers and apprenticeships. Law no. 30/2003 did not uniform the qualifications across regions (a central characteristic of the German system) because in Italy qualifications are a regional competence. Regional governments had to implement the national law before it became effective locally, and this allows us to use regional variation to identify the effect of the reform.

Much of the difficulties in the evaluation of the effects of apprenticeships come from selection. Most of the literature compares the employment outcomes of apprentices with a control group (often students in the academic education track or in other VET tracks or college graduates), however it is likely that controlling for observable characteristics is not enough to avoid an omitted variable bias because youth deciding to study VET may have different unobserved preferences about work from those who choose an alternative education system or no education.

With respect to previous papers, in this study the problem of selection is limited. Moreover, our focus is not on the effects of apprenticeships versus other forms of youth employment contracts, rather on the effect of changing the features of the apprenticeship contract. To estimate the effects of the 2003 reform, we exploit the contemporaneous presence of two different apprenticeship contracts between 2005 and 2011 due to the heterogeneous time of the implementation between regions and sectors. Individuals could not decide the type of apprenticeship, which depended on the region and the sector of work. We compare the employment and wage path of the apprentices in the new regime to the ones in the old regime, and estimate the Average Treatment on the Treated (ATT). We can assess the effect of the reform with a long time horizon following the hiring (seven years), which is important considering that the apprenticeship regime in Italy can last up to 5 years. We deploy balancing techniques (namely the Covariate Balancing Propensity Score estimator) that control for differences on a large set of observable characteristics and past outcomes. Furthermore,

we perform exogeneity checks of treatment eligibility by testing whether variation of migration or commuting flows affected the composition of the eligible youth in the treated regions.

The empirical literature on the transition of the apprentices to open-ended contracts (the so-called transformation rate of a temporary contract) in Italy is scarce and shows ambiguous effects. Berton *et al.* (2011) find that other temporary contracts outperformed the apprenticeship contract in terms of transformation rates during the period 1998-2004. Conversely, Picchio and Staffolani (2013) show opposite results on workers aged 30 (the age threshold for the apprenticeship contract) during the period 2009-2012. In this paper, we aim to understand whether the different features of the apprenticeship contract drive these contrasting results in the two periods, besides other differences in business cycle and workers' composition. Two former papers have analysed the effect of Law no. 30/2003 on the apprenticeship. Focusing on the short-run effects, D'Agostino *et al.* (2015) find a higher retention in the apprenticeship and hence a lower transition into permanent employment. Looking at firm level outcomes, Cappellari *et al.* (2012) show a positive impact of the apprenticeship reform on job reallocation and productivity.

Our estimates indicate that the reform managed to decrease the early dropout of the apprentices by 11.6% in the first year. After an initial lock-in effect, the transformation to open-ended jobs increased by about 14.5% five years after the hiring. Analyses on heterogeneous effects indicate that the reform's impact is larger in firms with more than ten employees: in the first year the dropout rate decreased in large firms by 21.1% and the transformation rate to open-ended jobs in the same firm after four years increased by 39.7%. Consistently with a pattern of higher job stability, we also find long-term effects on wages.

The paper is structured as follows. Section 2 describes the apprenticeship contract in the period of the reform and the changes introduced by the Law 30/2003. In Section 3 we describe the dataset, while in Section 4 tests on the exogeneity of the regional laws are implemented. Section 5 presents the identification strategy. Results and robustness tests are shown in Section 6, while the last section concludes.

2. Features of the Italian apprenticeship contract

The apprenticeship contract is characterized by an obligation for the employer to provide workers' training which is compensated with payroll tax rebates and lower wage costs. Since its introduction in the Fifties the only requirement for eligibility of an apprenticeship contract has been age while previous work experience has never been a pre-condition. In the period of the analysis, the training accounts for 120 hours per year and is divided into basic skills (35% of the hours) and technical competences for the profession. Basic skills include training on labour laws, work organization,

safety and communication while technical competences regard products and services, production processes, tools and materials of production. Only firms in the private sector may use the contract, and the maximum number of apprentices in a firm has to be below the number of employees. However, enterprises with less than three employees may hire up to three apprentices. Employers have to appoint an internal advisor as mentor of the apprentice: he or she attends external preparatory training and may follow at most five apprentices.

Firms choose the training courses from a regional sector catalogue (the content of training course is a regional competence), while lower wage costs (defined by the sector Collective Bargaining Agreements - CBAs) and high tax rebates compensate the employers for the training requirement. Employers' Social Security Contributions (SSC) are reduced at 10% of the apprentices' gross earnings (for open-ended and fixed-term contracts they account for 27% of gross earnings). There are further incentives for firms with less than ten employees, granting almost full tax exemptions for the first two years of apprenticeship: between 2007 and 2011 the SSC were 1.5% (3%) of the gross remuneration for the first (second) year while from 2012 employers receive full exemption for the first three years. Furthermore, the payroll tax rebates are extended for another year in case of conversion to an open-ended contract (Law no. 56/1987). The probationary period is at most of two months, after that period apprentices may be laid off only for just cause. Since 2009 apprentices dismissed for economic reasons with three months of seniority are entitled to unemployment insurance for 90 days (Law 2/2009).

Differences introduced by the 2003 reform

Before the 2003 reform age eligibility at hiring was 25 years (26 in regions entitled to support from European Union funds –so called Objective 1 regions – with the exception of Abruzzi and 29 in small firms); the duration of the contracts was between eighteen months and four years (five in the craft sector); the training could only be provided by external authorities such as local and accredited institutes sponsored by the regions.

Law no. 30/2003 introduced several changes to the regime, which was renamed “*Vocational Apprenticeship*”.¹ First, to facilitate the use of the apprenticeship contract, it raised the age ceiling to 29. Second, it liberalised the training component and allowed part of it to be performed on-the-job. Third, it set the contract duration between two years and six years, although the CBAs could specify a shorter range within this limit.² Finally, the Law introduced a minimum wage level for

¹ Other two forms of apprenticeship were introduced, covering a minority of the contracts (1% in 2007 - ISFOL, 2010).

² In general, this remained in the lower part of the range, sometimes even setting a minimum length under the two years (e.g. specific CBAs in the retail trade and banking sectors). The minimum length requirement was later abolished in 2008 (Law no. 112/2008). Compared to the pre-reform regime, the average duration range marginally increased as at the end of 2008, the average maximum and minimum length in the CBAs was 33 and 56 months (ISFOL, 2010).

apprentices: at least two levels below the wage of a similarly qualified worker (i.e. position reached by the end of the contract).

The reform allowed firms satisfying specific requirements on training competences, tutoring and place of training, to perform training on-the-job. Usually training mostly covered the technical-professional competences, however some regions and CBAs allowed, under stricter rules, that also the basic training may be performed on-the-job (ISFOL, 2010). Although the policymaker created a system of administrative sanctions, it was always difficult for the authorities to verify firms' compliance with the training requirements.³ However, non-compliance also occurred for the external training as, in fact, many apprentices did not do any training due to lack of funding. For example, in 2004 only 25% of the apprentices participated in training and just 17% completed the compulsory 120 hours (ISFOL, 2006).

The reform was not immediately effective. The Law was enacted with the Legislative Decree no. 276/2003 of September 10th 2003. However, since the Italian regions have exclusive competence in the field of vocational training, regional governments had a high degree of autonomy in its actual implementation. Regional government started to approve regional laws only from 2005 onwards and the actual implementation was heterogeneous in time as it required agreements among many actors (regional governments, trade and firms' unions). Furthermore, most of the regions were not ready to revise immediately the training offer. Several regions preferred implementing pilot tests in specific sectors before fully implementing the reform. This is the case of sectors such as the Retail Trade (in Lombardy, Umbria, Marche, Lazio, Sicily), Banking (in Piedmont, Lombardy, Marche, Sicily), Metal Manufacturing, Construction and Textile (in Marche), and Tourism (in Lazio). The regions passing regional laws enacting the reformed apprenticeship were Tuscany, Emilia, Marche, Friuli Venetia Giulia and Sardinia in 2005, Lazio, Trentino Alto Adige, Puglia and Basilicata in 2006, Piedmont, Umbria and Lombardy in 2007, Molise in 2008, Campania, Veneto, Liguria and Abruzzi in 2009. Figure 1 summarizes the dates of regional laws.

To speed up the implementation process the government allowed that, in the absence of a regional law, sector-specific CBAs could revise the training content and start the new regime (Law 80/2005). Since 2005 firms covered by such CBAs could start using the new contract. The most important sectors implementing the reform were Retail and Wholesale trade, Chemicals, Construction, Tourism, Transport, Financial services, Energy, Rubber, Textile, Metal Manufacturing and Metallurgy. Because of the heterogeneous time of the implementation, two different apprenticeship contracts existed until a further reform of 2011 (Legislative Decree N.167

³ Employers not complying with the training requirements had to pay back twice the tax exemption received and possibly convert the apprenticeship to an open-end contract (INPS circular No. 40/2004).

of September 2011). Notably, the two contracts implied a different level of compensation for the apprentices and a different training regime (only external or both internal and external). In general, regional authorities organised the same external training for both regimes (ISFOL, 2010). In principle, firms could not decide which regime to use as this depended on the sectors and the region of activity at the moment of hiring. However, because institutional changes occurred through combined legal provisions from multiple sources (regional laws and collective agreements), firms' eligibility did not necessarily translate into actual treatment at the level of the individual worker. There was a degree of uncertainty for employers as to which of the two apprenticeship contracts should have been applied, resulting in an incomplete overlap between firm eligibility and workers' assignment to treatment. Transformations of old contracts into new ones were not allowed and limitations were explicitly set to avoid firms dismissing an old-regime apprentice and re-hiring her with the new one.

3. Data

To estimate the effect of the reform on the apprenticeship transitions to other contracts, we use administrative data derived from social security registers made available by the Italian Social Security Institute (INPS, Istituto Nazionale di Previdenza Sociale). The overall administrative sample available for research purposes has a longitudinal structure and covers 6.5 percent of all individuals registered with INPS. The data report individual employment histories in the salaried private sector inclusive of semi-subordinate employees (also known as 'collaborators', formally consultants that work regularly with a given firm as external staff) and unemployment benefit receipts. The data contain information on firm and individual characteristics, with the exception of education. Starting in 2007, the INPS data also record the regime under which new apprentices are hired, whether the "old apprenticeship" or the "new" one introduced with Law 30/2003.

We select an inflow sample of individuals starting a spell of apprenticeship in 2007 and follow them at a monthly frequency for the following seven years until the end of 2014 (the last data-point available to us). We retain individuals aged 19-24 at the beginning of the apprenticeship spell, because younger individuals were not eligible for the new apprenticeship and older ones were eligible for the old regime only in special cases. This selection rule generates a sample of 17,958 individuals. Since apprenticeships have a maximum duration of five years we can observe individual trajectories at least two years after the end of the apprenticeship. Of course not all apprentices complete the maximum duration and many terminate the contract earlier to move to other forms of employment (or non employment). In Figure 2 we plot the rate of survival in the apprenticeship contract for our inflow sample of 2007. We observe that 19% of apprentices exit the

contract within the two initial months of the probationary period; 51% terminate the contract after the third month and before the second year of contract while 30% have a longer duration.

The administrative data record the reason for the contract's termination: 60% of terminations occurring in the first year of the apprenticeship are due to quits, while 13% are lay-offs; other causes of termination are transformation into an employment contract (8%) and contract expiration (6%), while in 13% of cases the reason for termination is unknown. The high incidence of workers' resignations should be carefully interpreted. Empirical studies in other countries suggest that firms may hire "cheap" apprentices to adjust their workforce to the business cycle (e.g. Merrilees, 1983; Askilden and Nilsen, 2005). Besides, employers might encourage workers' resignation to get around high firing costs induced by EPL. If we look at the exit destination of the early leaver in the first year, we see that 72% of them exit our database the month after the end of the contract. These youths are not employees in the salaried private sector, collaborators, or unemployed (receiving benefits).

In Table 2 we describe the employment patterns differentiated by type of contracts for the apprentices hired in 2007. All apprenticeship contracts are terminated at the end of our observation period and, as shown in Figure 2, the hazard rate shows two spikes at the end of the third and the fourth year. This implies a drop of the survival rate in the initial apprenticeship contract of 6 percentage points (p.p.) and of 4 p.p. respectively. The proportion of youths remaining employed in the salaried private sector decreases over time and at the end of the seventh year 56% are still employed (the sum of columns 1-6). At the end of the seventh year about 39% of apprentices have an open-ended contract (14% within the same firm, 25% in another firm), 5% have a different apprenticeship contract in another firm, 11% have a temporary contract, 1% are external collaborators. Finally, 4% of the individuals are in unemployment benefits and almost 40% are not in our database anymore. As the apprenticeship regime grants unemployment benefits only in special cases, most of the latter are likely uninsured unemployed (we refer to this category as "out-of-database").⁴

Firm size is an important variable for apprentices. Early dropout is much more pronounced in small firms with less than ten employees: within the first year 54% of the apprentices hired in these firms exit compared to 45% of the apprentices hired in large firms. One of the possible explanations could be the looser dismissal rules for firms below fifteen employees in Italy and the larger incentive to churn due to higher tax rebates. Furthermore, after seven years apprentices in large firms are much more likely to have a permanent job in the same firm (20% versus 11% in small firms) and just slightly more in other firms (27% versus 24%). Large firms seem to be able to offer

⁴ The database does not contain self-employed, public employees, inactive and uninsured unemployed.

better career opportunities to the apprentices although two-thirds of the apprentices are hired in firms with less than ten employees. An explanation for the high popularity among small firms is the larger tax rebate that these firms enjoy.

As explained in Section 2, two apprenticeship regimes coexisted in 2007 depending on the geographic region and the sector of activities of the firm at the moment of hiring. In our inflow sample 10,744 and 7,204 apprentices were hired with the new and the old regime respectively. If we split the sample by apprenticeship regime we observe noteworthy differences. As shown in Figure 1A in the Appendix, apprentices in the new regime tend to transit more to open-ended contracts from the fourth year onwards, especially within the same firm. Apprentices in the old regime move more to temporary contracts and other apprenticeships. Besides, an important share of apprentices in the old regime moves out of our database already in the first months. For these youths we observe a cyclical pattern for both the share of youth out-of-database and the share of temporary contracts, indicating some sort of seasonal work. This is likely caused by the implementation of the reform by CBAs, which saw some sectors such as tourism postponing the reform.

As shown in Table 2A in Appendix, the type of apprenticeship regime is indeed correlated with several characteristics. Because of the implementation via CBAs, the most noticeable difference is the concentration in sectors such as Wholesale, Retail Trade, Business services and Construction for the new regime and Food, Tourism and Personal Services for the old regime. New regime apprentices are characterised by a higher probability of being hired by larger firms and a better employment history such as higher past remuneration and higher chances of having already experienced an open-ended contract in the past.

It is clear that the apprentices in the two regimes are different. Furthermore, the regions which implemented the reform earlier than others might differ in labour market characteristics which affect the future employment rate of the apprentices. Both these reasons prevent us from interpreting the observed differences as the causal effect of the reform. In the next section, we explain how we control for selection on observable characteristics to estimate the causal effect of the reform on the apprenticeship transition.

4. Analytical framework

We are interested in the effects of the new apprenticeship contract on the outcomes of apprentices relative to the counterfactual case in which they would be hired under the old apprenticeship regime. We estimate the average treatment effect on the treated (ATT):

$$ATT = E[Y_i(1) - Y_i(0)|D_i = 1] \tag{1}$$

where D_i is a binary treatment dummy indicating whether the apprentice is hired with the new apprenticeship contract rather than with the old one and $Y_i(1)$ and $Y_i(0)$ represent the counterfactual outcomes of the apprentice with and without treatment. The ATT is the parameter of interest because it answers the question of what has been the causal impact of the policy for those that have been “treated” relative to the hypothetical case in which they did not receive the treatment and were hired in the old scheme.⁵

While conceptually well defined, the ATT (as other causal parameters) involves counterfactual quantities and therefore cannot be directly estimated using equation (1) due to lack of information. In the absence of credible sources of exogenous variation in treatment assignment, we overcome the problem of selection into treatment by replacing counterfactuals with the outcomes of an appropriate control group, whose members are identical to treated units in all the relevant characteristics affecting the outcome. We use the apprentices hired with the old scheme to form the control group, and use balancing techniques to ensure that treated and controls are comparable in terms of the likelihood of receiving the treatment given their observable characteristics. More specifically, given our sample of N units indexed by i , we apply the inverse probability weighting estimator (IPW, see e.g. Hirano *et al.*, 2003) that weights control units based on the odds of receiving treatment given their observable characteristics:

$$ATT_{IPW} = \frac{\sum_i D_i Y_i}{N_1} - \frac{\sum_i (1 - D_i) w_i Y_i}{N_0} \quad (2)$$

where sums run over the entire sample, N_1 and N_0 are the number of treated and control units, $w_i = \frac{\pi(X_i)}{\sum_i \pi(X_i) D_i / N_0}$ is the weight for control units, $\pi(X_i) = \frac{p(X_i)}{1-p(X_i)}$ is the odd ratio of the treatment given the covariates X_i , and $p(X_i)$ is the propensity score.⁶ The IPW uses the outcomes of controls in place of the unobservable outcomes of the treated in the counterfactual scenario of no treatment, and gives more weight to control units that on the basis of their characteristics have a higher predicted odd ratio of being treated. The validity of identification rests on the assumption of *conditional independence*, i.e. after controlling for the propensity score the potential outcome in absence of treatment (Y_0) should be orthogonal to the treatment assignment.

$$Y_0 \perp D \mid p(X)$$

⁵ As mentioned in the introduction, comparing two different regimes reduces the selection problem relative to most of the literature which compares the employment outcomes of apprentices with a control groups of students (in the academic education track or in other VET tracks or college graduates) or workers in other contracts. Previous research has exploited the staggered implementation of the new apprenticeship contract by regions and sectors (Cappellari *et al.*, 2012), focussing on the effect of *firms' eligibility* to the policy on firms' outcomes such as profits or wage costs. This corresponds to estimating the intention-to-treat effect (ITT).

⁶ To mitigate potential consistency issues arising from model misspecification, we estimate the propensity score using the covariate balancing propensity score (CBPS), proposed by Imai and Ratkovic (2014). This estimator results to be overall the best performing semi-parametric estimator in the empirical simulations of Frölich *et al.* (2015).

The set of covariates X_i used to predict the propensity score is key in this respect. We include in the model a long list of covariates related to labour market outcomes: firm characteristics (region, industry, size, belonging to larger establishments); apprentice characteristics (age, gender, quarter of hiring); characteristics of the most recent job in the salaried private sector ended at least 30 days before starting the apprenticeship (type of contract, gross remuneration, reason for ending the contract, part-time, length of contract, industry); previous employment history in the salaried private sector up to 30 days prior to starting the apprenticeship (experience, age in the first job, average full-time remuneration, share of working time by collar, contract and firm size, part-time experience, number of jobs, length of the non-employment spell before the apprenticeship and a dummy for ever being in unemployment subsidies).

We include firms' characteristics because firms are heterogeneous in their churning behaviour (particularly across industries) and omission of these variables might result in inflation of the estimated effects. Consequently, the estimated effect is better interpreted as the net effect of treatment holding constant firms characteristics. Note that even if our administrative data have no information on educational attainment, the set of individual characteristics used to estimate the propensity score provides a good approximation for the stock of human capital of the apprentices. More generally, since we control for detailed labour market histories, following Imbens and Wooldridge (2009) we argue that our conditioning variables control for time-invariant unobserved heterogeneity related to labour market outcomes.

Even controlling for the large set of covariates described above, conditional independence may still fail because of (time varying) unobservable factors that jointly determine treatment and outcomes. As discussed in the preceding sections, policy adoption occurred in a staggered fashion across regions, and we are worried about migrations of future apprentices to adopting regions on the basis of the expected returns in terms of employment or wage prospects. To dispel these doubts about potential threats to identification, we use data from the labour force survey between 2004 and 2008 and estimate difference-in-difference regressions in which individual indicators for either regional migration or daily commuting across regional borders are regressed on a set of individual controls, time dummies and regional dummies, plus a treatment indicator assuming value equal to one whenever a region has adopted the new apprenticeship in a certain year. We run these regressions using different age groups, and allowing for lags in the effects of the policy changes. None of these exercises produced statistically significant estimated effects of the policy change on migration or commuting flows (results are available on request) which rules out endogenous migration or commuting as sources of bias in our estimates.

5. Results

Estimates of the propensity score are available upon request and show that the likelihood of being hired on the new apprenticeship scheme varies with the characteristics of the firm (industry and size) and not so much with the characteristics of the worker (e.g. type of last job and past employment history), suggesting that endogenous selection into treatment by personal characteristics may be less of an issue in our analysis. Diagnostic tests show that the estimator performs rather well in balancing covariates across treated and control units (Table 2A in the Appendix).⁷

Effects of the new apprenticeship on employment

We report results from estimating at a monthly frequency the effect of the policy on labour market trajectories in the seven years after the apprentice is hired. We consider nine non-mutually exclusive destinations for the apprentices hired in 2007: employee; apprentice with the initial firm; apprentice with another firm; permanent employee; permanent employee with the initial firm; permanent employee with another firm; temporary employee; unemployment benefits recipient; out of the database (which includes self-employment, public sector, education, uninsured unemployment and inactivity). We report estimated effects graphically in Figure 3.

Overall, there is a positive effect of the new apprenticeship on employment of about 2 p.p. throughout the seven-year time window considered. Results show that the policy has been extremely successful in curbing the early drop-out of apprentices; the share continuing the apprenticeship increases in the first year by 6.0 p.p. (or 13.8% of the stock of apprentices in that time window), reaching a maximum of 6.8 p.p. at the end of the third year (corresponding to 43.4% of those still in apprenticeship at that time). The effect becomes moderately negative in the fourth year because, while many apprenticeships of the new regime reach their natural termination date, some contracts in the old regime are still effective (for example the craft sector had a duration of 5 years in the old regime); the effect eventually converges to zero after 5 years. Since the reduction of dropout is already observed in the first months of the spell, the estimated effect may not come from the extension of maximum duration of the reformed apprenticeship and is likely the result of the combined effect of the minimum remuneration and the higher (expected) productivity of the apprentices due to the enhanced firm-specific human capital. These policy changes discourage firms from churning.

⁷ As for the covariate “apprentices hired in firms with more than 500 employees” we do not have a sufficient number of units in the old regime to balance the treated group (47 versus 891 units), we remove these individuals before estimating the propensity score (i.e. trimming on covariates). Since lack of overlap of the propensity score can also bias the estimates and increase the variance (Lechner and Strittmatter, 2017), we trim the treated units with a propensity score above the 99.9 percentile of the control units which leaves us with about 98% of the treated units.

Another evident effect of the policy emerges when looking at the transition to permanent employment, which follows a time pattern that is somehow symmetric compared to the one of attachment to the initial apprenticeship. The policy reduces transitions to permanent employment in the first four years after hiring, consistent with the already observed positive effect on attachment to the apprenticeship. Afterwards, there is a positive effect of about 4 p.p. over the fifth year, which carries over also to the sixth and seventh year after the initial hiring, though at slightly lower level (+ 3 p.p.). Distinguishing job transitions within the same firm from those that occur between firms, shows that the bulk of the effect on permanent employment occurs through promotions, particularly during the fifth year after initial hiring.

Remaining panels in Figure 3 show a slightly negative effect on both transitions into another apprenticeship and on exits from the sample, while there is no significant effect on transitions to temporary employment or to unemployment benefits. The negative effect on attrition from the administrative panel is approximately constant throughout the time window, which suggests that the time pattern of the effects on drop out from apprenticeship or transitions to permanent employment are not an artefact of selective attrition.

Heterogeneous effects on employment by gender and firm size

Results obtained by considering men and women separately are in Figure 4. In general, the effects are similar in the two cases, but there are exceptions. Most notably, there is a differential effect on transitions to stable employment at the end of the apprenticeship. While the effect for women is long lasting and evident until the end of the window of observation, the effect for men is short-lived and fades out after one year. Distinguishing transitions by their destination reveals that for men the diminished effects is driven by transitions outside the firm that initially hired the apprentice, for which there is no effect whatsoever.

We now consider how the estimated impacts of the reform vary with firm size. Specifically, we split the sample of treated apprentices depending on whether they are initially hired by firms with more or less than ten employees, because this is the threshold for eligibility to the higher tax rebate.⁸ In Figure 5 we plot the two effects, using apprentices in the old regime as the control group for both. The positive effects that we have estimated on the overall treated sample seem to come mostly from the apprentices hired in firms with more than 10 employees. The positive effect on attachment to the apprenticeship is much smaller in firms with less than ten employees. At the beginning of the sixth year, the different lock-in effect translates into very different transitions to permanent employment. In particular, the effect on permanent employment in the same firm is zero

⁸ Apprentices are concentrated in small firms and about 70% are hired in firms with less than 10 employees.

in small firms, while in larger firms the impact is +6.3 p.p., corresponding to a 39.7 percent increase relative to those in open-ended contracts in the same firm at that time. In the following two years the effect remains relatively constant for the apprentices hired in larger firms, while for the smaller firms it decreases becoming slightly negative at the end of the seventh year (-1.4 p.p.). “New” apprentices hired by firms above the 10 employees threshold also have a higher chance of working in a permanent job in other firms at the end of the seventh year (+3.2 p.p.) compared to the control group, while there is no such effect of the policy in small firms. On the other hand, for small firms the policy seems to have a limited effect on increasing attrition from the sample.

The more likely explanation for the worse performance of the reform on small firms is the lack of firms’ capabilities to perform on-the-job training, which might have eventually reduced the overall training opportunities of the apprentices. Furthermore, any potential positive effect might be minimized in smaller firms as they have a higher incentive to churn due to the higher tax rebate and an incentive not to increase the size of their permanent staff. Indeed, because apprentices do not contribute to determining firm size for legal purposes, for firms just below the 10 employees threshold transitions from apprentice to regular employee (either permanent or temporary) trigger the loss of eligibility requirements for higher tax rebates.⁹

Effects on wages

The INPS data contain information on gross pay and the total number of full-time equivalent working days, from which we obtain the full-time equivalent gross daily wage for each month in the seven years window starting on January 1st 2007. To take into account earnings attrition over the period, we perform the analysis with and without including cases with missing wage information, and impute zero wage to missings in the former case; the resulting wage outcome can be seen as an omnibus measure of compensation that includes non-employment spells.¹⁰

Results are reported in Figure 6. Including (left panel) or not (right panel) zero wages increases the month-to-month volatility of the estimated effects and reduces the precision of the estimates, but the overall pattern is not much affected. There is an initial sizeable effect of the new apprentice contract on wages, which are almost 20 percent higher for apprentices hired with the new regime compared to those in the old one. This wage increase is in line with the higher minimum wage introduced by the reform. Interestingly, the wage gap with apprentices in the old regime

⁹ An identification strategy based on a regression discontinuity design (RDD) estimator exploiting the firm size discontinuity is not possible as in the data the firm size is regrouped by class.

¹⁰ The propensity score is estimated on the full sample and used throughout the observation window irrespective of the availability of wage information at any given point in time. As a robustness, we compared treatment effects on wages in the last month of observation re-estimating the propensity score only for cases observed with a valid wage in that month, finding no substantive change in results. Alternatively, we estimated treatment effects on wages limiting the sample to the balanced panel with valid wage information, finding again that results are robust.

shrinks over time, especially during the first two years after hiring, which suggests higher wages to comply with the law may come at the cost of reduced wage growth. However, there is a significant long-run wage effect of the reform (about 3 p.p., and roughly stable after the 5th year), which possibly reflects the increase of apprentices' human capital thanks to better opportunity for training in the new regime.

It is also interesting to consider heterogeneity of wage effects, and we do this in Figure 7 focussing our attention on the sample with valid wage information. There is a distinctive difference between men and women in terms of the effect on entry wages, which is about half in the case of women compared with men. For both men and women there is a decline of the effect during the first two years after hiring, and a long-term effect of about 4 p.p.. Looking at heterogeneity by firm size, the most remarkable result is that for firms above the 10 employees threshold there is no significant wage effect in the long run, though the point estimates for the last month are equivalent (Figure 8). Finally, we have to note that estimates for any months after hiring are potentially affected by changes in the composition of the treated group. As the employment effect is heterogeneous by firm size, different compositional changes may partially explain the different ATT on the wages.

6. Conclusions

In this paper, we have found significant effects of the 2003 reform of the apprenticeship contract on wages and employment probabilities. The aim of the reform was to move the Italian system closer to the German dual system of a paid apprenticeship with on the job training: it increased minimum pay and allowed firms to provide training on the job rather than externally. We minimize the usual problems of selection and omitted variable bias exploiting the heterogeneity of the implementation of the reform between regions and sector which resulted in the co-existence of two apprenticeship regimes during the period 2005-2011. We find that five years after hiring, the new contract improves the chances of moving to a permanent job in the same firm, yet this happens mostly in large firms. There are also sizeable long-run wage effects of the reform, well beyond the legal duration of apprenticeships, compatible with increased human capital accumulation thanks to the training provisions of the reform. These results are helpful for the many countries which implement reforms designed to bring the apprenticeship system closer to the German “dual system”.

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Table 1: Changes in the Apprenticeship Regime Introduced by the 2003 Reform

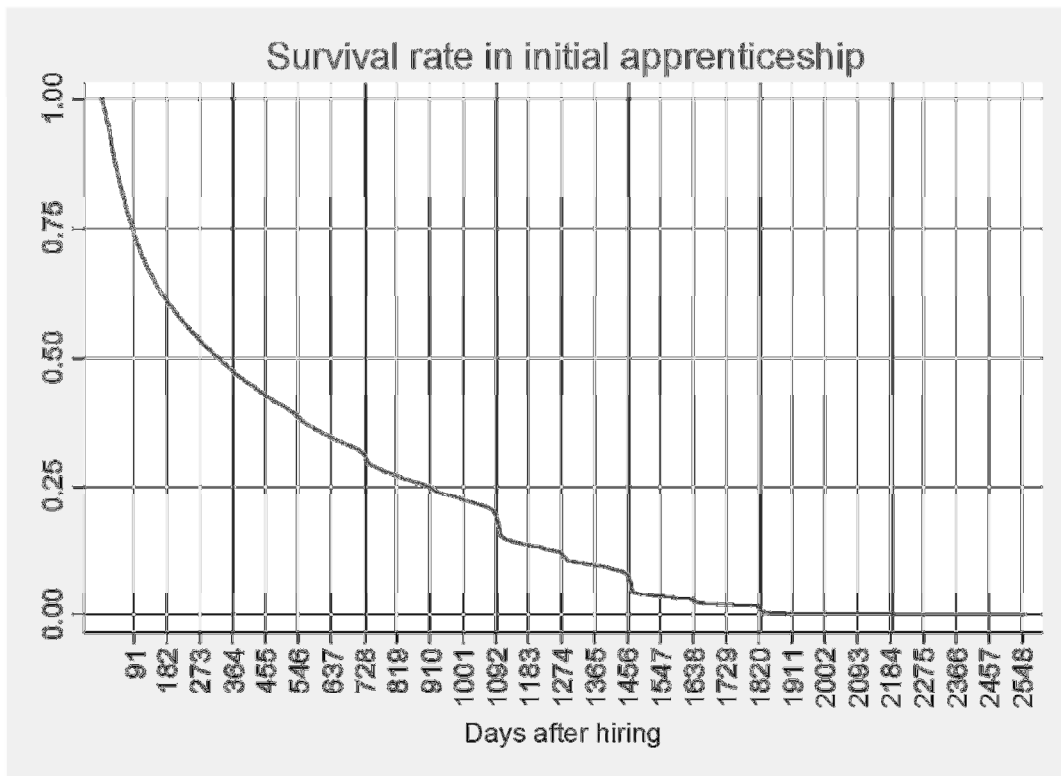
	Pre-reform	Post-reform
Age at hiring	< 25 (30 in some exceptions)	< 30
Training	External authorities	External and Internal (if firm declares training capabilities)
Length	1.5 – 4 / 5 years	Usually 2.75 - 4.5 years (2008 CBAs)
Lower wage	Set by CBAs	Minimum wage to the remuneration set by CBAs

Table 2: Evolution of the Share of Youth in Several Contracts (%)

Year (end)	Appr. initial firm (1)	Other appr. (2)	Open- ended same firm (3)	Open- ended other firms (4)	Tempo rary (5)	Collabo rator (6)	Unempl oyed (7)	Out-of- database (8)
1	48.7	9.9	1.5	4.1	8.4	1.0	0.1	26.2
2	32.3	13.3	3.8	7.5	9.0	1.2	0.6	32.2
3	20.3	13.3	7.9	11.3	10.3	1.3	1.2	34.3
4	8.4	11.2	14.0	16.4	11.6	1.4	1.8	35.1
5	1.9	8.5	16.6	20.7	11.2	1.4	2.4	37.2
6	0.5	6.5	15.6	23.5	9.9	1.1	3.5	39.4
7	0.3	5.4	14.0	25.0	10.1	1.2	4.4	39.7

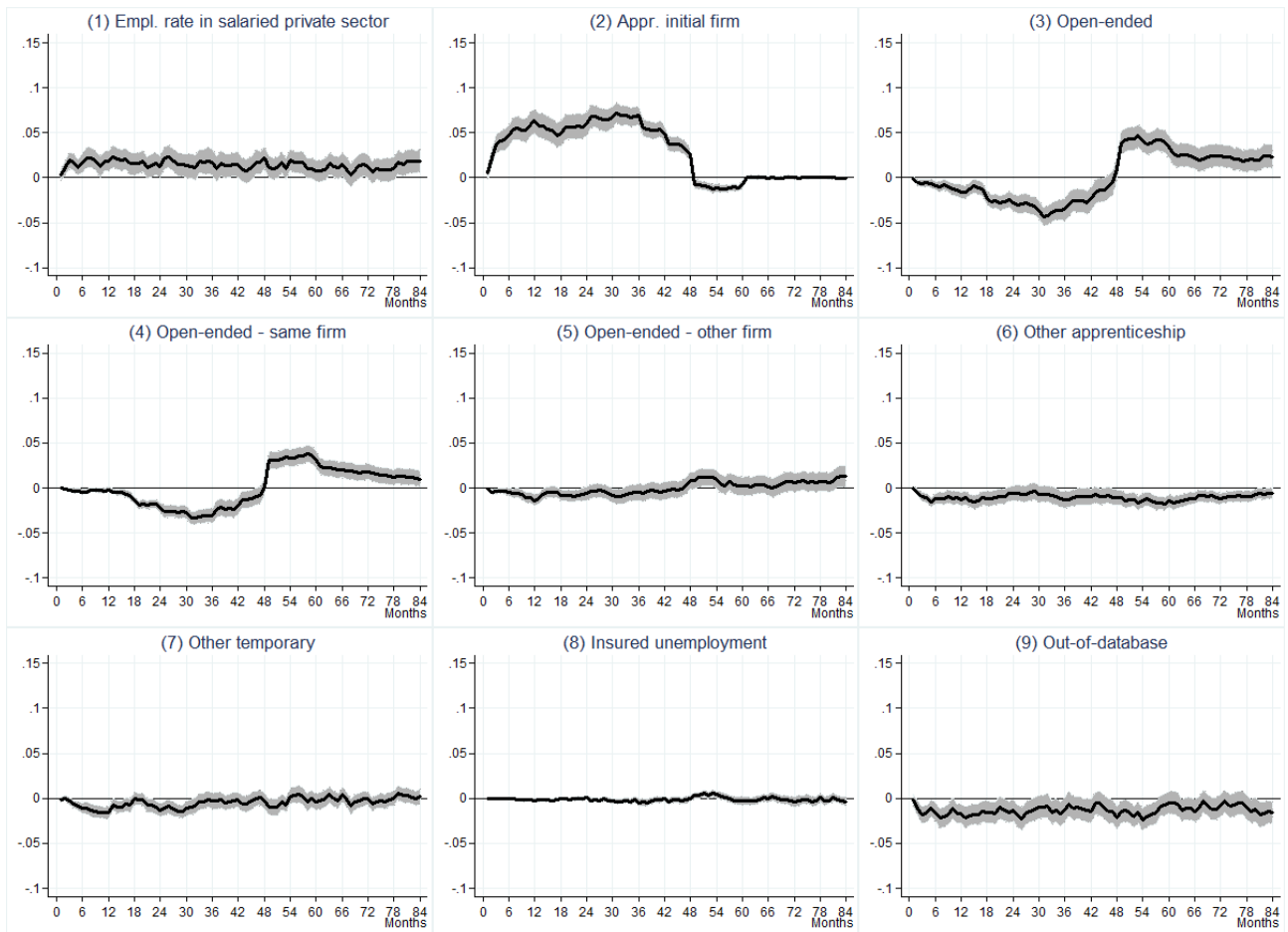
Inflow sample of 17,958 apprentices hired in 2007 aged 19-24. Status at the end of the year after hiring: (1) apprenticeship in the first firm, (2) other apprenticeship, (3) open-ended contract in the same firm, (4) open-ended contract in another firm, (5) other temporary contract, (6) collaborator, (7) insured unemployed, (8) not in salaried employment in the private sector. Individuals with more jobs are considered only in one position following this order: initial apprenticeship, open-ended contract (same, other firms), other apprenticeship, other temporary contract, collaborator, insured unemployment and not in salaried private sector employment.

Figure 2: Survivor Function - Exit from the Initial Apprenticeship – Whole Sample



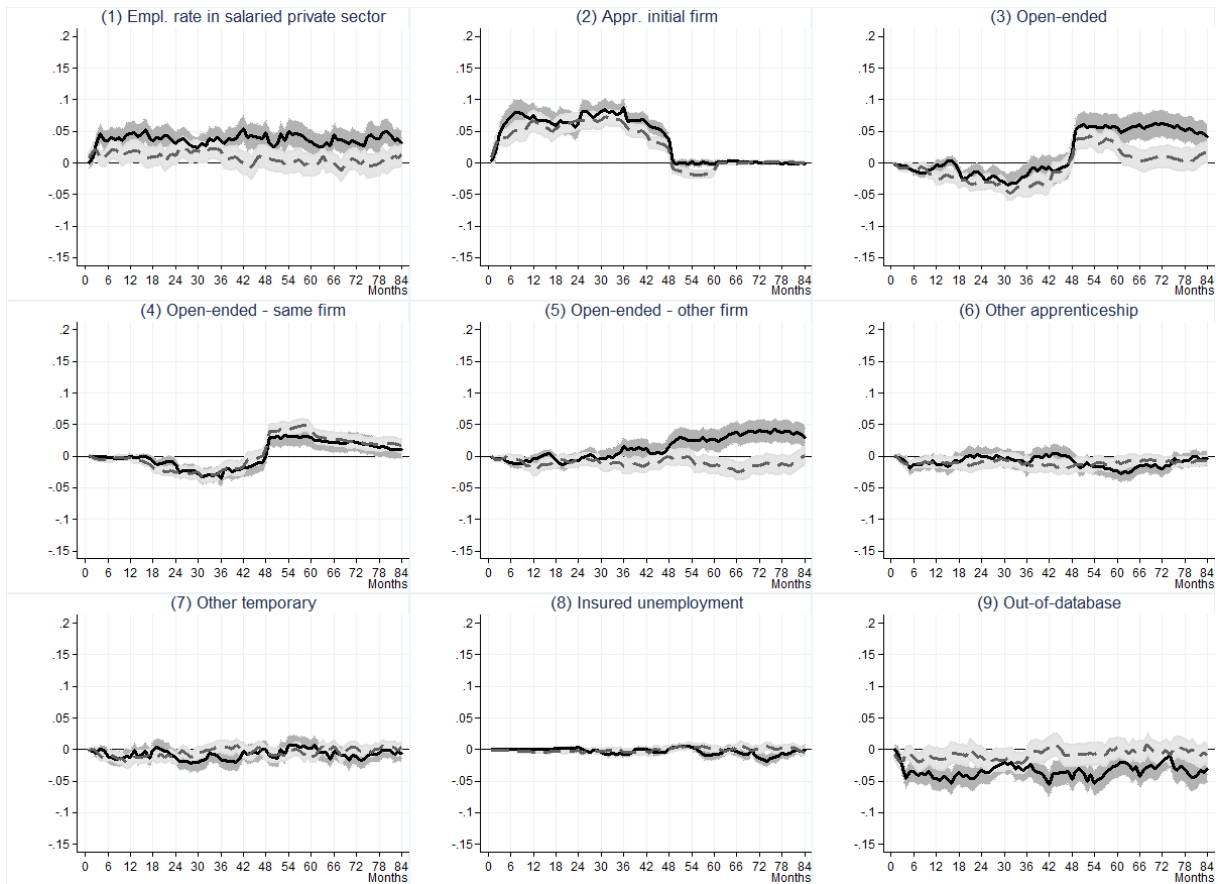
Note: Inflow sample of 17,958 apprentices hired in 2007 aged 19-24.

Figure 3: ATT on the Apprentices in the Next Seven Years



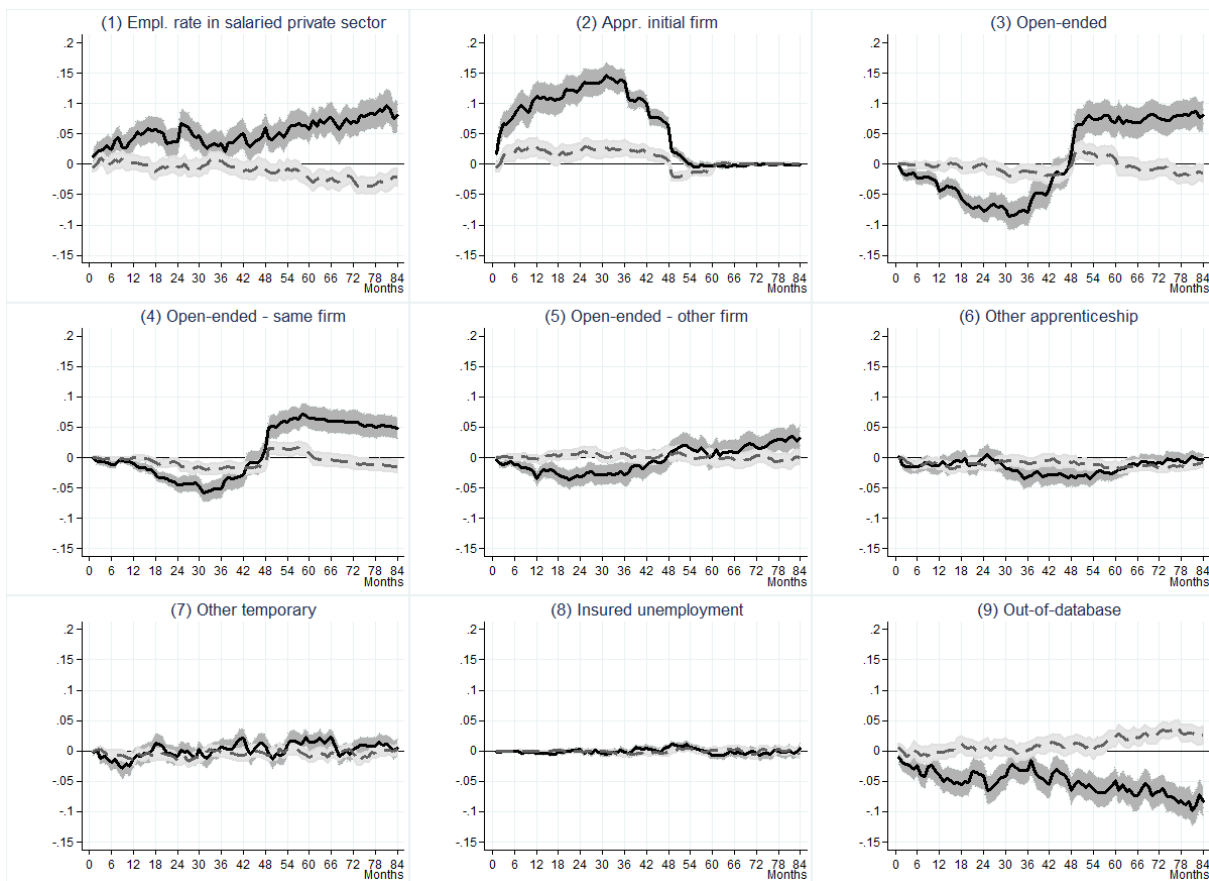
ATT estimated by CBPS estimator of the reformed apprenticeship versus the old apprenticeship on a sample of 16,805 apprentices hired in 2007 aged 19-24 (after trimming). Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-ended contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position using the order mentioned above. Bootstrapped Standard Errors (199 repetitions) clustered by individual to take into account serial correlation.

Figure 4: Heterogeneous Effects: ATT on Female (Solid Line) & Male (Dashed Line) Apprentices



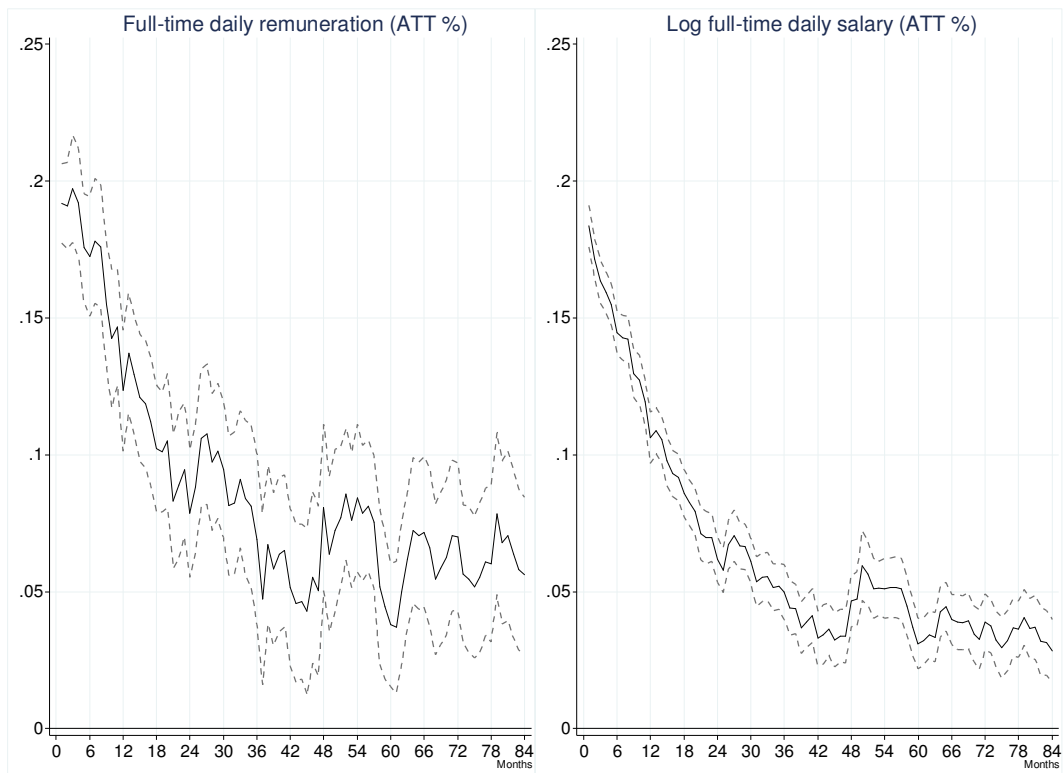
ATT estimated by CBPS estimator of the reformed apprenticeship on men (dashed lines) and women (solid lines). The areas around the point estimates represent the corresponding confidence intervals at 95%. Sample of 9,815 men and 6,949 women - aged 19-24 (after trimming). Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-ended contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position using the order mentioned above. Bootstrapped Standard Errors (199 repetitions) clustered by individual to take into account serial correlation.

Figure 5: ATT on the Apprentices Hired in Small (dashed line) & Other Firms (solid line)



ATT estimated by CBPS estimator of the reformed apprenticeship on apprentices hired in larger firms (solid blue lines) versus hired in smaller firms (orange dashed lines). The areas around the point estimates represent the corresponding confidence intervals at 95%. Sample of 11,653 and 5,197 apprentices hired in small and large firms in 2007 aged 19-24 (after trimming). Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-ended contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position using the order mentioned above. Bootstrapped Standard Errors (199 repetitions) clustered by individual to take into account serial correlation.

Figure 6: ATT on the Full-Time Daily Remuneration (Left) and Full-Time Daily Wage (Right)



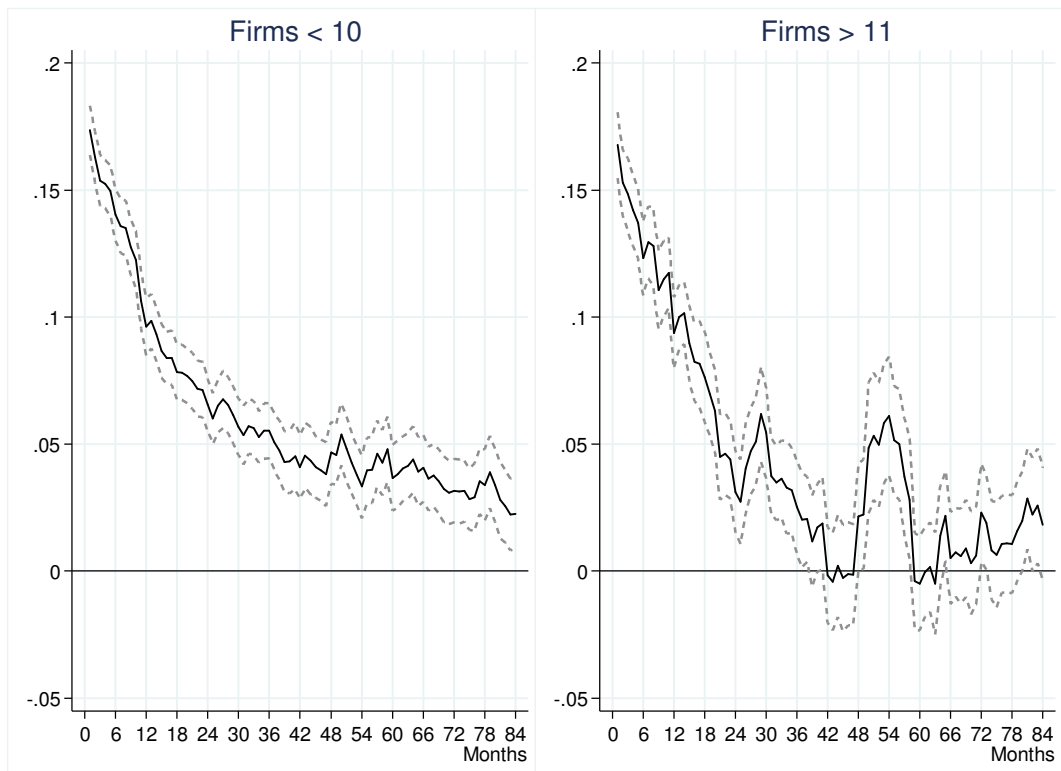
ATT estimated by CBPS estimator of the reformed apprenticeship versus the old apprenticeship on a sample of 16,805 apprentices hired in 2007 aged 19-24 (after trimming). Left panel outcome: full-time daily remuneration (zero if the individual does not work in t). Right panel outcome: full-time daily salary (missing if not working). Bootstrapped Standard Errors (199 repetitions) clustered by individual to take into account serial correlation.

Figure 7: ATT on the log Full-Time Daily Wage - Men and Women



ATT estimated by CBPS estimator of the reformed apprenticeship versus the old apprenticeship on a sample of 9,815 male and 6,949 female apprentices hired in 2007 aged 19-24 (after trimming). Outcome: full-time daily salary (i.e. missing if not working). Men on the left panel and women on the right panel. Bootstrapped Standard Errors (199 repetitions) clustered by individual to take into account serial correlation.

Figure 8: ATT on the log Full-Time Daily Wage – Smaller Firms and Other Firms



ATT estimated by CBPS estimator of the reformed apprenticeship versus the old apprenticeship on a sample of apprentices hired in 2007 and aged 19-24. Left panel: 11,653 apprentices hired in firms with less than 10 employees. Right panel: 5,197 apprentices hired in firms above 11 employees (figures after trimming). Outcome: full-time daily salary (i.e. missing if not working). Bootstrapped Standard Errors (199 repetitions) clustered by individual to take into account serial correlation.

Appendix

Figure 1A: Evolution of the Employment Rate by Type of Apprenticeship – Descriptive Evidence



Inflow sample of 17,958 apprentices hired in 2007 aged 19-24. Status at the end of the month: (1) Open-ended contract in the same firm, (2) open-ended contract in other firms, (3) other apprenticeship, (4) other temporary contract & collaborator, (5) insured unemployment, (6) not in salaried private sector employment. Individuals with more jobs are considered only in one position following the above order. Blue Lines: apprentices hired in the new apprenticeship. Red lines: apprentices hired in the old apprenticeship.

Table 2A: List of Covariates – Balancing Test on Full Sample

Variables	Raw or CBPS	Mean		Standardized Bias	P-value - mean equality	
		Treated	Control			
Hiring information: quarter	1st quarter	RAW	26.6%	30.8%	-9.4	0.000
		CBPS	27.0%	26.9%	0.3	0.864
	2nd quarter	RAW	25.9%	31.3%	-12.1	0.000
		CBPS	26.2%	25.6%	1.2	0.426
	3rd quarter	RAW	23.7%	22.6%	2.4	0.117
		CBPS	23.7%	23.1%	1.3	0.414
	4th quarter	RAW	23.9%	15.2%	22.0	0.000
		CBPS	23.2%	24.4%	-3.0	0.070
Firm characteristics: Sector (Nace rev 1.1)	Other: primary/energy (1-5, 11, 14, 23, 40, 41)	Raw	0.3%	0.4%	-0.9	0.566
		CBPS	0.3%	0.3%	-0.3	0.846
	Food (15)	Raw	2.7%	6.6%	-18.7	0.000
		CBPS	2.7%	2.8%	-0.3	0.806
	Textile, wearing, and leather (17, 18, 19)	Raw	2.1%	4.1%	-11.5	0.000
		CBPS	2.2%	2.2%	-0.1	0.965
	Wood (20)	Raw	0.9%	1.5%	-5.7	0.000
		CBPS	0.9%	0.9%	-0.1	0.967
	Paper, publishing (21, 22)	Raw	1.2%	0.9%	3.0	0.055
		CBPS	1.2%	1.3%	-0.1	0.971
	Chemicals, rubber and plastic (24, 25)	Raw	1.4%	1.1%	2.6	0.092
		CBPS	1.4%	1.3%	1.1	0.518
	Manuf. of (non)-metal minerals (26, 27)	Raw	1.2%	1.7%	-4.4	0.004
		CBPS	1.2%	1.2%	0.0	0.980
	Manufacture of structural metal products (28)	Raw	4.7%	7.5%	-11.7	0.000
		CBPS	4.8%	4.8%	-0.2	0.914
	Manuf. of machinery and equipment (29)	Raw	2.4%	2.3%	1.0	0.503
		CBPS	2.5%	2.4%	0.3	0.835
	Manuf. of office machinery, computers, electrical and apparatus (30, 31)	Raw	1.3%	2.1%	-6.5	0.000
		CBPS	1.3%	1.3%	0.2	0.906
	Manuf. of transport, communication & precision instruments (32, 33, 34, 35)	Raw	1.5%	2.1%	-4.5	0.003
		CBPS	1.6%	1.6%	-0.2	0.893
	Other manufacturing (36, 37)	Raw	1.4%	1.8%	-3.3	0.032
		CBPS	1.4%	1.5%	-0.2	0.893
	Construction (45)	Raw	23.1%	14.9%	21.0	0.000
		CBPS	23.6%	23.8%	-0.6	0.735
	Sale, maintenance and repair of motor vehicles (50)	Raw	3.6%	3.5%	0.6	0.692
		CBPS	3.7%	3.7%	-0.3	0.871
	Wholesale trade (51)	Raw	5.9%	0.9%	27.5	0.000
		CBPS	5.1%	4.8%	2.0	0.280
	Retail trade (52)	Raw	16.6%	4.3%	41.1	0.000
		CBPS	15.7%	15.1%	2.1	0.262
	Hotels and restaurants (55)	Raw	11.9%	27.6%	-40.3	0.000
		CBPS	12.1%	12.4%	-0.8	0.530
	Transport and communication (60, 61, 63, 64)	Raw	2.4%	1.1%	9.4	0.000
		CBPS	2.4%	2.5%	-0.7	0.688
	Finance and renting (65, 66, 67, 70, 71)	Raw	1.8%	1.3%	4.4	0.005
		CBPS	1.9%	1.9%	-0.2	0.926
	Computer and R&D (72, 73)	Raw	2.1%	0.6%	12.8	0.000
		CBPS	2.1%	2.1%	0.0	0.985
Other business activities (74)	Raw	6.5%	3.0%	16.5	0.000	
	CBPS	6.6%	6.7%	-0.6	0.752	
Education, health and social work (80, 85, 90)	Raw	1.3%	0.8%	5.8	0.000	
	CBPS	1.4%	1.4%	-0.7	0.695	
Organizations, Recreational, cultural, sporting activities (91-92)	Raw	1.0%	1.4%	-3.5	0.024	
	CBPS	1.0%	1.0%	-0.2	0.889	
Other personal services (93)	Raw	2.7%	8.6%	-25.5	0.000	
	CBPS	2.8%	2.9%	-0.3	0.812	
Gender	Women	Raw	39.9%	44.5%	-9.5	0.000
		CBPS	39.5%	39.1%	0.7	0.646
Firm characteristics: Position	Single	Raw	90.6%	92.4%	-6.5	0.000
		CBPS	90.7%	90.9%	-0.7	0.663
	Subsidiary	Raw	3.1%	2.5%	3.4	0.031
		CBPS	3.1%	3.1%	0.2	0.883
	Parent-company	Raw	6.3%	5.1%	5.4	0.001
		CBPS	6.2%	6.1%	0.7	0.675
Job-to-Job transition	Not employed 30 days before the hiring	Raw	72.1%	75.8%	-8.5	0.000
		CBPS	72.6%	72.9%	-0.8	0.602

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Firm characteristics: Region of work	Piedmont, Aosta Valley	Raw	8.9%	8.3%	2.3	0.134
		CBPS	8.9%	9.0%	-0.4	0.818
	Lombardy	Raw	17.1%	15.6%	4.0	0.011
		CBPS	17.0%	17.0%	0.0	0.998
	Liguria	Raw	3.5%	3.7%	-1.4	0.381
		CBPS	3.6%	3.6%	0.1	0.958
	Trentino	Raw	1.6%	2.8%	-8.1	0.000
		CBPS	1.7%	1.6%	0.5	0.681
	Veneto	Raw	13.5%	12.4%	3.4	0.030
		CBPS	13.4%	12.7%	2.1	0.173
	Friuli Venetia Giulia	Raw	2.4%	2.9%	-3.4	0.025
		CBPS	2.4%	2.3%	0.5	0.749
	Emilia-Romagna	Raw	9.6%	13.8%	-13.3	0.000
		CBPS	9.7%	9.1%	1.7	0.243
	Tuscany	Raw	9.1%	9.7%	-2.1	0.182
		CBPS	9.1%	8.7%	1.3	0.410
	Umbria	Raw	2.8%	2.5%	1.8	0.254
		CBPS	2.8%	2.8%	0.0	0.975
	Marche	Raw	4.0%	6.1%	-9.6	0.000
		CBPS	4.1%	3.9%	0.9	0.503
	Lazio	Raw	7.9%	4.8%	12.6	0.000
		CBPS	7.8%	8.6%	-3.2	0.067
	Abruzzi, Molise	Raw	1.9%	3.2%	-7.8	0.000
		CBPS	2.0%	1.9%	0.3	0.826
	Campania	Raw	4.1%	2.3%	10.0	0.000
		CBPS	4.0%	4.5%	-2.7	0.134
	Puglia	Raw	5.5%	5.4%	0.7	0.675
		CBPS	5.6%	5.7%	-0.6	0.720
	Basilicata	Raw	0.8%	0.5%	4.6	0.004
		CBPS	0.9%	0.8%	0.0	0.979
	Calabria	Raw	1.8%	1.1%	6.0	0.000
		CBPS	1.8%	2.0%	-1.3	0.466
Sicily	Raw	3.9%	3.5%	2.3	0.136	
	CBPS	3.9%	4.3%	-2.2	0.186	
Sardinia	Raw	1.6%	1.4%	1.1	0.480	
	CBPS	1.6%	1.6%	0.0	0.975	
Hiring information: Age	19	Raw	18.5%	22.3%	-9.5	0.000
		CBPS	18.6%	18.5%	0.4	0.780
	20	Raw	20.7%	21.9%	-2.9	0.062
		CBPS	20.8%	20.9%	-0.4	0.802
	21	Raw	18.4%	18.4%	-0.2	0.913
		CBPS	18.3%	17.7%	1.6	0.296
	22	Raw	15.8%	14.8%	2.9	0.062
		CBPS	15.8%	16.2%	-1.0	0.516
	23	Raw	14.3%	12.4%	5.6	0.000
		CBPS	14.2%	14.1%	0.1	0.962
	24	Raw	12.3%	10.2%	6.7	0.000
		CBPS	12.3%	12.5%	-0.9	0.570
Firm characteristics: Firm size	0-5 employees	Raw	50.3%	58.2%	-15.9	0.000
		CBPS	51.4%	52.1%	-1.4	0.357
	6-10 employees	Raw	14.2%	17.2%	-8.3	0.000
		CBPS	14.5%	14.7%	-0.6	0.672
	11-15 employees	Raw	8.0%	8.8%	-2.9	0.065
		CBPS	8.2%	8.3%	-0.4	0.808
	16-20 employees	Raw	4.4%	5.2%	-3.9	0.012
		CBPS	4.5%	4.6%	-0.3	0.840
	21-25 employees	Raw	3.0%	2.2%	5.3	0.001
		CBPS	3.1%	3.0%	0.5	0.768
	26-30 employees	Raw	2.2%	1.2%	7.4	0.000
		CBPS	2.1%	2.0%	1.1	0.503
	31-40 employees	Raw	3.6%	1.8%	10.8	0.000
		CBPS	3.4%	3.4%	0.2	0.894
	41-50 employees	Raw	2.6%	1.0%	11.7	0.000
		CBPS	2.3%	2.3%	-0.1	0.967
	51-100 employees	Raw	5.1%	2.1%	16.2	0.000
		CBPS	4.9%	4.5%	1.9	0.278
	101-200 employees	Raw	3.8%	1.3%	15.8	0.000
		CBPS	3.2%	2.9%	1.7	0.307
201-500 employees	Raw	2.7%	0.8%	14.8	0.000	
	CBPS	2.5%	2.2%	1.7	0.332	

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Last job as employee	Primary (or unknown)	Raw	2.3%	1.8%	3.4	0.029
		CBPS	2.3%	2.5%	-1.2	0.489
	Manufacturing	Raw	11.4%	13.1%	-5.4	0.000
		CBPS	11.5%	11.4%	0.2	0.905
	Construction	Raw	10.4%	8.5%	6.5	0.000
		CBPS	10.6%	10.7%	-0.4	0.827
	Services	Raw	27.7%	23.9%	8.7	0.000
		CBPS	27.1%	26.7%	1.1	0.506
	Tourism	Raw	9.2%	16.6%	-22.3	0.000
		CBPS	9.3%	9.1%	0.6	0.657
Last job as employee	% full-time (=0 if no job)	Raw	54.1%	57.0%	-6.1	0.000
		CBPS	54.0%	53.4%	1.2	0.438
	Full-time weekly remuneration (2007 €)	Raw	€ 157.6	€ 150.6	4.8	0.002
		CBPS	€ 156.2	€ 154.7	1.0	0.537
Non-employ. spell	Number of days last job-apprenticeship	Raw	111.1	125.0	-5.1	0.001
		CBPS	111.9	110.3	0.6	0.708
Last job: Contract	Apprentices	Raw	26.9%	34.5%	-16.4	0.000
		CBPS	27.3%	27.0%	0.6	0.679
	Open-end	Raw	9.3%	8.2%	3.9	0.013
		CBPS	9.3%	9.3%	0.1	0.928
	Temporary agency worker	Raw	6.8%	5.4%	5.7	0.000
		CBPS	6.6%	6.2%	1.7	0.288
	Seasonal	Raw	1.9%	2.5%	-4.2	0.006
		CBPS	1.9%	1.8%	0.6	0.692
	Fixed-term	Raw	12.2%	10.4%	5.7	0.000
		CBPS	11.9%	12.2%	-0.8	0.604
	Collaborator	Raw	4.3%	3.2%	5.8	0.000
		CBPS	4.2%	4.4%	-1.0	0.534
	Other/Unknown	Raw	1.8%	1.4%	3.4	0.031
		CBPS	1.8%	1.8%	0.2	0.917
Last job as employee	Part-time	Raw	11.8%	13.0%	-3.8	0.014
		CBPS	11.6%	11.5%	0.3	0.847
Last job: Reason end of the contract	No information (or before 2005 or collab.)	Raw	31.2%	29.1%	4.6	0.003
		CBPS	30.9%	30.4%	1.2	0.459
	Dismissal	Raw	3.3%	4.2%	-4.8	0.002
		CBPS	3.3%	3.4%	-0.6	0.679
	Resignation	Raw	16.0%	18.8%	-7.5	0.000
		CBPS	16.1%	16.1%	0.0	0.981
	End of contract	Raw	7.4%	8.9%	-5.4	0.000
		CBPS	7.5%	7.3%	0.7	0.655
	Others	Raw	5.3%	4.6%	3.4	0.031
		CBPS	5.3%	5.5%	-1.1	0.508
Last job	Tenure (in weeks)	Raw	17.8	18.8	-2.9	0.059
		CBPS	17.8	18.0	-0.5	0.722
Employment history : Age at first job	18 or less	Raw	1.1%	0.8%	3.0	0.056
		CBPS	1.0%	1.1%	-0.4	0.804
	19	Raw	19.5%	22.9%	-8.5	0.000
		CBPS	19.6%	19.5%	0.3	0.827
	20	Raw	21.3%	22.3%	-2.4	0.117
		CBPS	21.3%	21.5%	-0.5	0.739
	21	Raw	18.0%	18.2%	-0.4	0.821
		CBPS	18.0%	17.4%	1.5	0.321
	22	Raw	15.0%	14.3%	2.1	0.180
		CBPS	15.1%	15.4%	-0.8	0.599
	23	Raw	13.6%	11.9%	5.1	0.001
		CBPS	13.5%	13.4%	0.2	0.880
	24	Raw	11.5%	9.6%	6.0	0.000
		CBPS	11.5%	11.8%	-0.8	0.604
Employment history : Ever worked with specific contract	Open-term	Raw	18.3%	15.9%	6.3	0.000
		CBPS	18.2%	18.3%	-0.1	0.930
	Temporary	Raw	30.6%	26.4%	9.4	0.000
		CBPS	30.0%	29.9%	0.2	0.904
	Apprentice	Raw	41.9%	48.6%	-13.5	0.000
		CBPS	42.1%	41.7%	0.9	0.561
	Collaborator	Raw	8.2%	6.1%	8.2	0.000
		CBPS	8.0%	8.2%	-0.6	0.699
	Seasonal	Raw	5.9%	9.6%	-13.9	0.000
		CBPS	5.9%	5.8%	0.4	0.774

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Employment history	Average Contract length (days)	Raw	137.7	138.0	-0.2	0.922
		CBPS	136.7	138.0	-0.6	0.693
Employment history as employee: % experience by contract	Open-end	Raw	9.1%	7.6%	6.1	0.000
		CBPS	9.0%	9.0%	0.0	0.976
	Apprentice	Raw	33.7%	41.1%	-16.5	0.000
		CBPS	34.0%	33.7%	0.8	0.613
	Other	Raw	18.2%	15.3%	8.8	0.000
		CBPS	17.8%	17.7%	0.3	0.858
Employment history as employee: % experience by collar	Blue collar	Raw	21.0%	19.3%	4.8	0.002
		CBPS	20.9%	20.7%	0.6	0.702
	Apprentice	Raw	33.7%	41.1%	-16.5	0.000
		CBPS	34.0%	33.7%	0.8	0.613
	White collar	Raw	6.3%	3.6%	13.7	0.000
		CBPS	5.9%	6.0%	-0.6	0.739
Employment history	Average share working time	Raw	55.9%	58.5%	-5.7	0.000
		CBPS	55.8%	55.3%	1.0	0.523
	Av. Full-time weekly remuneration (2007 €)	Raw	€ 157.6	€ 152.7	3.7	0.019
		CBPS	€ 156.5	€ 155.4	0.8	0.632
	N. of previous jobs (with collaborators)	Raw	1.8	1.9	-2.5	0.105
		CBPS	1.8	1.8	0.9	0.543
	Experience as collaborator (months)	Raw	0.5	0.3	7.7	0.000
		CBPS	0.5	0.5	-1.4	0.442
	Experience as employee (weeks)	Raw	41.3	40.6	1.1	0.479
		CBPS	41.0	41.0	0.0	0.985
	Experience as apprentice (weeks)	Raw	27.4	30.0	-5.1	0.001
		CBPS	27.4	27.5	-0.1	0.957
Employment history as employee: % Experience by firm size	Micro (0-5)	Raw	25.1%	30.6%	-13.6	0.000
		CBPS	25.3%	25.2%	0.3	0.867
	Small (6-25)	Raw	17.3%	18.6%	-3.9	0.012
		CBPS	17.4%	17.3%	0.3	0.869
	Medium (26-200)	Raw	8.5%	7.4%	4.7	0.002
		CBPS	8.4%	8.5%	-0.4	0.801
	Large (201+)	Raw	10.1%	7.4%	10.9	0.000
		CBPS	9.7%	9.4%	1.4	0.382
Employment history	No experience (employee/collaborator)	Raw	36.8%	34.4%	5.0	0.001
		CBPS	36.9%	37.3%	-0.8	0.614
	Ever insured unemployed	Raw	1.7%	1.7%	0.0	0.988
		CBPS	1.7%	1.8%	-0.6	0.693
	Experience only as collaborator	Raw	2.3%	1.7%	4.3	0.006
		CBPS	2.2%	2.3%	-0.5	0.761
Sample	Pseudo R2 (1)	LLR test (2) – p-value	Mean SB (3)	Median SB (4)	B (5)	R (6)
RAW	0.148	0.000	7.8%	5.7%	96.9*	1.26
CBPS	0.001	1.000	0.7%	0.6%	9.0	1.41

Balancing tests after reweighting by the CBPS and trimming. Covariates distribution by treatment group/regime and robust t-test of mean equality. (1) Pseudo r-squared, (2) Log-likelihood ratio test on joint significance, (3) Mean Standardized bias, (4) Median Standardized Bias, (5) B: the absolute standardized difference of the means of the linear index of the propensity score in the treated and reweighted controls; (6) R: the ratio of treated to reweighted controls variances of the propensity score index. B should be less than 25 and R between 0.5 and 2 (Rubin, 2001). Note that missing values are given value equal to zero (e.g. last remuneration for person without experience or % working time).