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

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Abstract

This paper estimates the causal effects of the 2003 reform of the Italian apprenticeship contract which allowed on-the-job training, increased its legal length and introduced a minimum floor to apprentices' wages. Using administrative data, we implemented a covariate balancing propensity score and a difference-in-differences estimator. 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 reform's training provisions.

Keywords: Apprenticeship, Permanent Work, Youth Employment, Covariate Balancing Propensity Score, Difference-in-Differences

JEL codes: J24, J41, C21

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1. Introduction

Many countries facing high youth unemployment want to improve their Vocational Education and Training (VET) systems to give more chances to the youth in the transition between school and work (Quintini & Martin, 2006). The apprenticeship contract is one of the most popular features of VET systems; apprentices receive vocational training to enhance their professional skills while employers are compensated with reduced payroll taxes and lower wage costs. The apprenticeship regime is generally heavily regulated by governments and social partners, and its implementation has followed different routes across Europe (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 German-speaking countries (Germany, Austria, Switzerland, 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. In other countries, like Italy, the apprenticeship regime is contiguous with formal education and is rather considered an employment contract with a strong training requirement, which is highly regulated by collective agreements and regional governments.

In international comparisons, the dual system seems to be more effective than other options in helping youth transition into employment, as it shows a faster integration into the labour market. The dual system ensures high-quality on-the-job training and requires a high degree of employer involvement. Apprentices are paid during the apprenticeships; at the end of the experience centralized accreditation of training curricula promotes transparency and acceptance among employers (Parey, 2009; Fersterer *et al.*, 2008; Pischke & von Wachter, 2008; Dustmann & Schönberg, 2012). Many countries therefore tried to revise their apprenticeship system by moving it toward the successful examples found in German-speaking countries (Woessmann, 2008; Hogarth, 2009).[1]

This paper analyses the effect of an important reform of the apprenticeship system implemented in Italy in 2003. The Law no. 30/2003 revised the training component of the apprenticeship. Before the reform all the training (typically 40 annual hours of basic competencies and 80 hours of technical competencies) was formal and to be undertaken in specific regional institutions. After the reform, technical training could be undertaken also on-the-job. The opportunity for firms to train on-the-job was inspired by the dual system, where training is done

[1] For example, the 2009 UK reform tightened the link between apprenticeships and employers by offering large incentives for employers to increase training activities. 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 & Rauner, 2012; Krueger & Kumar 2004).

part at school and part at work. The goal was to tighten the link between employers and apprentices and reduce the bureaucracy in the administration of external training for firms. Other changes were introduced by the reform such as a minimum wage and a longer legal length of the contract.

In this paper we focus on the effects of this reform on the apprentices' outcomes. Existing research studies the effects of apprenticeships by comparing outcomes between apprentices and a control group that is typically formed by other students or other temporary jobs. In this paper we instead contribute to the literature by basing our estimates on a comparison within apprenticeships; namely, the "new" and the "old" apprenticeship contract, which limits the issue of selection into treatment. The empirical literature on the transition of the apprentices to open-ended contracts in Italy is scarce and shows ambiguous effects. Berton *et al.* (2011) found that other temporary contracts outperformed the apprenticeship contract in terms of transformation rates during the period 1998-2004. Conversely, Picchio and Staffolani (2018) showed opposite results on workers aged 30 (the age threshold for the apprenticeship contract) during the period 2009-2012. The effects of the 2003 reform on individuals' employment prospects have never been studied before, and we contribute to the literature by providing evidence on these effects.[2]

For identification, we exploit the contemporaneous presence of two different apprenticeship contracts for new jobs due to the staggered implementation of reform across regions and industries. Before the law went into effect, local governments had to issue specific regulations and collective bargaining agreements had to define the training contents of apprenticeships. To estimate the Average Treatment on the Treated (ATT) we compare employment and wage trajectories of apprentices hired in different regimes in 2007. Thanks to rich administrative panel data we are able to look at the effects of the reform for seven years, well beyond the legal duration of apprenticeship contracts (usually between three and five years). In order to identify the causal effects of the reform, we deploy two alternative analytical strategies that hinge upon different sets of assumptions. Our main estimating approach relies on a Covariate Balancing Propensity Score estimator (CBPS) and the Conditional Independence Assumption (CIA) to control for differences in composition among the apprentices in the two regimes. We address the validity of this strategy via a sensitivity analysis on confounders (Rosenbaum, 2002), which highlights the relatively large magnitude of a potential confounder needed to invalidate the results. To assess the robustness of findings from the CBPS analysis, as a second estimating approach we resort to a difference-in-differences estimator, which is robust to selection bias under the parallel trend assumption.

[2] Cappellari *et al.* (2012) estimated a positive effect of the reform on job reallocation (the year-to-year job turnover defined as in Autor *et al.*, 2007) and productivity on firm-level data.

We find that the reform reduced the early dropout of apprentices in the first year and increased the rate of transformation of apprentices (i.e. the conversion of the contract) into open-ended contracts. We estimate substantial effects 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%. Consistent with a pattern of higher job stability, we also find positive long-term effects on wages. While we are in general unable to disentangle which of the various elements of the reform were the most effective, we argue that on-the-job training is an important driver of the results.

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

2. Institutional background

In this section, we describe the two Italian apprenticeship regimes available after the reform of Law no 30/2003. In the first part, we mention the characteristics of the apprenticeship contract common to the two regimes, while the second part highlights their differences.

Apprenticeship contracts were introduced in Italy in the 1950s, with eligibility requirements based solely on the worker's age. The probationary period is two months at most, and after that period apprentices may be laid off only for just cause. Beginning in 2009, apprentices dismissed for economic reasons with three months' seniority are entitled to unemployment insurance for 90 days (Law 2/2009). Only private sector firms may use apprenticeship contracts, and the maximum number of apprentices in a firm must be below the number of employees. However, firms with fewer than three employees may hire up to three apprentices.

Traditionally in southern European countries, the role of apprenticeships (and in general, vocational training) in the education-labour market transition of youths has been marginal because employers prefer to hire workers on fixed-term contracts – for which there is no need for formal vocational training – and because families have a strong preference in favour of academic education (e.g. Eichhorst *et al.* 2015). Moreover, different from the German model, apprenticeship in Italy is recognised as a full employment contract that usually does not originate at school but is completely separate from it.[3] Through the apprenticeship contract, employers provide training to employees in exchange for payroll tax rebates and lower wages.

Employers' Social Security Contributions (SSCs) are reduced to 10% of the apprentices' gross earnings, rather than the standard 27% for the regular employees. There are further incentives for firms with fewer than ten employees, granting almost full tax exemptions for the first two years of the apprenticeship.[4] Furthermore, the payroll tax rebates are extended for another year in the case of a transformation to an open-ended contract (Law no. 56/1987). Lower wage costs are defined by the collective bargaining agreements (CBAs), which are typically industry-specific at the national level. Employers have to appoint an internal advisor as a mentor (and sometimes trainer) for the apprentice; he or she may counsel up to five apprentices. Firms choose the training courses from a region-specific list. In the period of our analysis, the training component of the contract amounted to 120 hours per year, divided between basic skills for 40 hours and job-specific technical competencies for the remaining part. The basic competencies include labour laws, work organization, and safety regulations and are usually financed by the regions. The remaining hours of technical competencies are paid by employers.

Differences introduced by Law 30/2003

Before the 2003 reform, age eligibility at hiring was 16-24 years,[5] the duration of the apprenticeship contracts was between 18 months and four years (five in the craft sector), and training could be provided only by external authorities certified by the regions. Law no. 30/2003 introduced several changes to the regime, which was then labelled "*Vocational Apprenticeship*" (see Table 1). First, it liberalized the training component and firms satisfying specific requirements on training competencies, tutoring, and place of training to perform on-the-job training. Usually, on-the-job training mostly covered the technical-professional competencies; however, some regions and CBAs permitted, under stricter rules, basic training to be performed on-the-job (ISFOL, 2010).[6] Second, the law introduced a minimum wage level for apprentices, establishing that their compensation

[3] This paper looks at the so-called "second-level apprenticeship" which is a labour contract, whose numbers total approximately 400,000 contracts (or 14% of the employed population aged 15-29 in 2011, see ISFOL, 2015). In Italy there is a different "first-level apprenticeship" that partly involves schools, but its numbers are small (even less popular is the "third-level apprenticeship" targeted at secondary school graduates).
[4] Between 2007 and 2011, SSCs were 1.5% (3%) of the gross remuneration for the first (second) year; since 2012, employers have received a full exemption for the first three years.
[5] The age limit was 26 in regions entitled to support from European Union funds – so called Objective 1 regions – with the exception of Abruzzo, and 29 in small firms.
[6] Although the policymaker created a system of administrative sanctions, it was always difficult for the authorities to verify firms' compliance with training requirements. Employers not complying with the training requirements had to pay back twice the tax exemption they had received, and potentially transform the apprenticeship to an open-ended contract.

should have not been lower than two grades on the pay scale of the occupation that the apprentices were qualifying for. Third, it set the contract duration between two and six years, although the CBAs could specify a shorter range within this limit.[7] Finally, it raised the age limit to 29 to facilitate the use of the contracts.[8]

The reform was not immediately effective. The law was enacted with Legislative Decree no. 276/2003 of 10 September 2003. However, since the Italian regions have exclusive competence in the field of vocational training, regional governments retained a high degree of autonomy in its implementation. Regional governments started implementing the law through specific regional regulations only in 2005, as they were not ready to immediately revise the menu of training courses offered. Several regions preferred implementing pilot tests in specific sectors before fully implementing the reform such as Retail Trade in Lazio, Lombardy, Umbria, Marche, and Tourism in Lazio. As shown in Figure 1, the regions enacting regional regulations officially implementing the reform 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.[9]

However, the implementation of the reform often happened through CBAs, rather than regional laws, because in practice the new regime was effective only after the specific CBA also updated the training programmes in the sector (ISFOL, 2007). Furthermore, to speed up the implementation process the government also allowed the CBAs to officially enact the new regime in the absence of a regional regulation (Law 80/2005). The most important CBAs renewing the apprenticeship training since 2005 were Retail and Wholesale Trade, Chemicals, Construction, Tourism, Transport, Financial Services, Energy, Rubber, Textile, and Metal Manufacturing and Metallurgy.

The implementation of the reform varied by region, sector, and time (see Table 3A of Online Appendix A). Due to this staggered implementation, two different apprenticeship contracts (the "new" and the "old") co-existed between 2005 and 2011;[10] however, firms could not freely decide

[7] 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). Compared with the pre-reform regime, the average duration range marginally increased; at the end of 2008, the average maximum and minimum lengths in the CBAs was 33 and 56 months, respectively (ISFOL, 2010).

[8] Underage individuals were excluded from the "vocational apprenticeship" and could only participate in the old regime or in the marginal "first-level apprenticeship", whose implementation was however postponed until 2011 (ISFOL, 2013). In the evaluation we isolate this channel of the reform by focusing on individuals between 19 and 24 years old.

[9] Figure 1 summarises the dates of regional laws.

[10] The Legislative Decree N.167/2011 reformed the contract for the new jobs. As shown in Figure 3A of Online Appendix A, the diffusion of new contracts in the old regime dropped at the end of 2011. In June 2012, Law 92/2012 reformed the apprenticeship regime in a different way for firms below and above 15 employees. These changes are unlikely to affect our sample as at that date only 1% were still in the apprenticeship.

which contract to use as this depended on the sectors and the region of activity at the time of hiring.[11] Importantly, the reform regarded only newly signed contracts and changes of old contracts into new ones were not allowed. Specific limitations were explicitly set to avoid firms dismissing an old-regime apprentice and replacing her with a new one. Although the rules were clear in principle, there was still a degree of uncertainty for employers as to which of the two apprenticeship contracts should be applied (for example, for the many firms with multiple plants in different regions or under different CBAs), resulting in an incomplete overlap between firm eligibility and apprentices' assignment to the new regime.

3. Data and descriptive statistics

To estimate the effect of the reform on apprentices' outcomes, we use administrative data derived from social security registers made available by the Italian Social Security Institute (INPS). The overall administrative sample available for research purposes has a longitudinal structure covering 6.5% of all individuals registered with INPS. The data report individual employment histories in the salaried private sector (e.g. remuneration, working days, type of contract, starting and ending date) and unemployment benefit receipts. The data contain information on firm and individual characteristics (e.g. firm size, sector, age, gender, region of work), with the exception of education. Starting in 2007, the INPS data also record the regime under which new apprentices were 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 subsequent seven years until the end of 2014. 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 persons were eligible for the old regime only in special cases. This allows us to exclude the channel of higher age eligibility in the interpretation of our results. This selection rule generates a sample of 17,948 individuals. Of those, 10,744 and 7,204 apprentices were hired in the new and the old regimes, respectively.[12]

Since apprenticeships have usually a maximum duration of five years, we observe individual trajectories at least two years after the end of the apprenticeship contract. Of course, not all apprentices complete the maximum duration and many terminate the contract earlier to move to

[11] While in our identification strategy we do not require random assignment to treatment, in Online Appendix C we check whether the labour market of the early implementing regions was systematically different before the introduction of the reform. The only observed significant difference is the higher diffusion of apprenticeship contracts, which probably incentivized these regions to adopt the reform faster.

[12] According to population registries of INPS, 277,000 individuals aged between 19 and 24 started an apprenticeship in 2007. Of those, 40% was hired in the old apprenticeship and 60% in the new apprenticeship regime.

other forms of employment (or non-employment). Figure 2 plots 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 first two months of the probationary period; 51% terminate the contract after the third month and before the second year of contract; and 30% have a longer duration. All apprenticeship contracts are terminated at the end of our observation period and, as shown in Figure 2, the hazard rate displays two spikes at the 36th and 48th months (which are the normal duration of the contact according to most CBAs).

Table 2 describes employment transitions differentiated by year and destination for the apprentices hired in 2007. The statuses we consider are the share of youth 1) remaining in the apprenticeship in the initial firm, transiting to 2) an apprenticeship in another firm, 3) a permanent job in the initial firm or 4) another firm, 5) a temporary contract, 6) a collaborator contract, 7) insured unemployment or 8) exiting our database (i.e. not in salaried employment in the private sector and insured unemployment).

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 the last row of columns 1 to 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, and 1% are external collaborators. Finally, 4% of the individuals earn unemployment benefits and almost 40% are no longer in our database.[13] If we split the sample by apprenticeship regime we observe noteworthy differences. As shown in Figure 1A in Online Appendix A, apprentices in the new regime tend to transit to open-ended contracts from the fourth year onward, especially within the same firm. Apprentices in the old regime move more to temporary contracts and other apprenticeships. An important share of apprentices in the old regime moves out of our database in the first few months. For these youths, we observe a cyclical pattern for both the share of out-of-database youth 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.

4. Analytical framework

[13] The database does not include periods in self-employment, public employment, inactivity and uninsured unemployment. Similar descriptive statistics are found in ISFOL (2013), where it is also shown that the share of apprentices ending up in self-employment (including external collaborators) or the public sector seven years after hiring is 9.4% (2005-2012 time period). According to LFS data, the vast majority of apprentices who leave employment enter into unemployment spells (60%) or inactivity (29%), while only 11% of them go into self-employment.

Expected effects of the reform on transformation

We use basic economic intuitions to outline the expected effects of the reform on the apprentices' transformation rate. Employers hire apprentices if the wage / labour cost gains are larger than the sum of direct (e.g. external authorities' fees, organization of class, material) and indirect training costs (e.g. opportunity cost in terms of production for the apprentice and the internal tutor). In this calculation, firms take also into account the increase of apprentice productivity by the end of the training. We expect that when the apprenticeship contract expires, firms transform the contract to a permanent one if the productivity gain of the apprentice has become larger than the sum of the increase in labour costs and the (potential and discounted) firing cost due to the more stringent employment protection legislation (EPL). If this is not the case, the apprenticeship contract could encourage a churning behaviour of firms (i.e. substituting an apprentice with another without promoting the first one to a permanent job). In this case, firms prefer hiring apprentices just as a form of cheap and flexible labour.[14]

By setting a minimum wage for apprentices, the 2003 reform should increase transformations as the cost-saving advantage of churning is reduced. The reform allows substituting external training with on-the-job training, with uncertain effects on the firms' training costs. On the one hand, it introduces organizational costs for employers willing to deliver on-the-job training. On the other hand, organizing internal training may be cheaper than the external authorities' fee and reduce the bureaucracy of their administration. Finally, on-the-job training might positively affect the productivity gain of the youth at the end of the apprenticeship due to more firm-specific human capital accumulation. Thus, we expect more transformations of apprenticeship contracts at the end of the period.

Empirical strategy

We are interested in the effects of the new apprenticeship contract on the labour market outcomes of individuals who started in 2007 a new regime apprenticeship relative to the counterfactual case in which they would be hired under the old regime. We estimate the average treatment effect on the treated in a specific month *t* after hiring to be:

$$ATT_t = E[Y_{it}(1) - Y_{it}(0)|D_i = 1]$$

[14] This is more likely to occur in firms 1) where human capital is a secondary factor and the productivity gain at the end of the training is low; 2) with high probability of job destruction, which decreases the expected return of a trained worker and increases the expected cost of EPL; and 3) where the financial incentives for using the apprenticeship contract are higher (e.g., in Italy this is the case of firms with fewer than ten employees).

1)

where D_i is a binary treatment dummy indicating whether in *t* = 0 the apprentice is hired with the new apprenticeship contract (D_i=1) rather than with the old contract (D_i=0), and $Y_{it}(1)$ and $Y_{it}(0)$ represent the potential outcomes of apprentice *i* at time *t* in case of treatment or without treatment, respectively. The ATT is the parameter that measures the causal impact of the policy for those who have been "treated" relative to the hypothetical case in which they did not receive the treatment and were hired under the old scheme.

Covariate balancing propensity score

As mentioned in the introduction, comparing two different apprenticeship 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. However, as shown in Table 1A in Online Appendix A, the apprentices hired in different regimes differ in several characteristics. Because of the implementation via CBAs, the most noticeable difference is the concentration in larger firms and in certain sectors such as Wholesale, Retail Trade, Business Services, and Construction for the new regime and Food, Tourism, and Personal Services for the old regime.

To limit remaining selectivity issues, we rely on the Conditional Independence Assumption (CIA) and 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 relevant characteristics affecting the outcome in the absence of the treatment. We use the apprentices hired with the old scheme to form the control group, and implement an estimator on the observables to ensure that treated and controls are comparable. More specifically, 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. This is represented as:

$$ATT_t^{IPW} = \frac{\sum_i D_i Y_{it}}{N_1} - \frac{\sum_i (1 - D_i) w_i Y_{it}}{N_0} \tag{2}$$

where N_1 and N_0 are the number of treated and control units in our sample, respectively, $w_i = \frac{\pi(X_i)}{\sum_i \pi(X_i)(1-D_i)/N_0}$ is the (normalised) weight for control units, $\pi(X_i) = \frac{p(X_i)}{1-p(X_i)}$ is the odds ratio of the treatment given the covariates X_i, and $p(X_i)$ is the propensity score. 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. To mitigate potential consistency issues arising from model misspecification in the estimation of the propensity score (which is common when the number of X covariates is large), $p(X_i)$ is estimated by implementing the covariate balancing propensity score estimator (CBPS) proposed by Imai and Ratkovic (2014). According to the empirical simulations of Frölich *et al.* (2017), the CBPS estimator was overall the best performing semi-parametric estimator on the observables. This estimator works like the IPW but instead of estimating $p(X_i)$ by using a logit or probit model, it uses a Generalised Method of Moments (GMM) estimator, having the objective function of both the first-order conditions (i.e., expected score equal to zero) and the balancing of the covariates. More details on the CBPS estimator are available in Online Appendix B.

The validity of identification rests on the assumption of *conditional independence*. In other words, after controlling for the propensity score the potential outcome in the absence of treatment ($Y_t(0)$) should be orthogonal to the treatment assignment:[15]

$$Y_t(0) \perp D \mid p(X) \tag{3}$$

with the set of covariates X_i used to predict the propensity score being key in this respect. We include in the model a long list of covariates related to labour market outcomes. First, as the availability of the new apprenticeship regime depended on the firm characteristics, we control for detailed information such as region, industry, size, belonging to a corporate group, and calendar quarter of hiring.

Second, new rules for the apprenticeship about the minimum wage, minimum duration, and training could induce firms to change the type of individuals hired in an apprenticeship. This could make the treated and the controls different in terms of potential outcome $Y_t(0)$. To ensure a similar composition between the two groups, we control for demographic characteristics (age and gender) and for a long list of labour market characteristics related to individual productivity. Therefore, we include information on the whole employment history in the salaried private sector (experience, age in the first job, average full-time remuneration, share of working time by occupation, contract and firm size, part-time experience, number of jobs, length of the non-employment spell before the apprenticeship, and a dummy equal one if the individual had experienced in the past a period of insured unemployment) and further details for the most recent job (type of contract, gross

[15] Another key assumption is the Stable- Unit- Treatment- Value Assumption (SUTVA). Since the intervention target group is quite small (apprenticeships are a small part of contracts) and the apprentices in the two regimes do not directly compete in the same labour market, as they tend to work in different regions or sectors, the magnitude of the bias coming from a failure of the SUTVA is probably small.

remuneration, reason for ending the contract, part-time, length of contract, and industry).[16] While we cannot exclude that the composition of the two groups might differ in other dimensions, we argue that remaining differences are of a second order. For example, our administrative data have no information on educational attainment but previous salaries, qualification (blue or white collar), experience, and age in the first job can provide a good approximation for the stock of human capital of the apprentices. Furthermore, as we condition on detailed labour market histories, which in our setting can be seen as lagged dependent variables, our covariates can account for time-invariant, unobserved heterogeneity related to the outcomes of interest (Imbens & Wooldridge, 2009).

5. Results

5.1 Covariate balancing propensity score

Estimates of the propensity score show that the likelihood of being hired on the new apprenticeship contract varies with the characteristics of the firm (industry and size), but less so with the characteristics of the worker (e.g., type of last job and past employment history), suggesting that the composition of individuals hired into an apprenticeship did not significantly change after the reform's implementation (Table 2A in Online Appendix A). The lack of significance of the regional dummies confirms that the reform's main implementation channels were collective bargaining agreements, rather than regional regulations.

Since lack of overlap of the propensity score can also bias the estimates and increase the variance (e.g., Lechner & Strittmatter, 2017), we trim the treated units with a propensity score above the 99.9 percentile of the control units, leaving us with about 98% of the treated units. As shown in Figure 2A in Online Appendix A, the trimming removes the thinnest part of the distribution.[17] Diagnostic tests show that the estimator behaves well in balancing covariates across treated and control units. Despite the many covariates, the CBPS performs remarkably well in balancing their distribution. The median Standardized Bias (SB) is as low as 0.7%, the highest SB is 3.2%; the Pseudo R-squared of the reweighted sample is 0.001, and the log-likelihood ratio test for the joint significance of the variables after the reweighting produces a p-value of 1.[18] The balancing tests are better than the ones obtained by the standard logit model (IPW). IPW weights generate a median SB of 2.2%, the highest SB is 11.3%, the pseudo R2 of the reweighted sample is 0.012, and

[16] We exclude the 30 days before starting the apprenticeship to isolate possible anticipation effects on the covariates.
[17] Huber *et al.* (2013) also proposed removing the control units with a weight higher than 4% of the total. However, this additional trimming is not required in our sample as the highest relative weight is only 0.2% of the total sample.
[18] If we trim by the max-rule, or do not trim, the balancing tests are slightly worse (the highest SB is 5.0% or 6.1%).

the p-value of the log-likelihood ratio test is 0. We show the full list of balancing tests by CBPS weights in Table 1A in Online Appendix A.[19]

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[20] labour market statuses in each month (Y_{it}) 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; and out of the database (which includes self-employment, public sector, education, uninsured unemployment, and inactivity). We show the estimated effects graphically in Figure 3.[21]

Overall, the new apprenticeship has a positive effect on employment of about 2 percentage points (p.p.) throughout the seven-year timeframe considered. Results show that the policy has been very successful in curbing the early drop-out of apprentices; the share of individuals 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 an 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 five years in the old regime); the effect eventually converges to zero after five years. It is important to note that the reduction of the dropout rate is already reached within the first year of the spell therefore, the estimated effect is unlikely to come from the extended minimum duration of the reformed apprenticeship (the pre-reform apprenticeship had a minimum duration of 1.5 years, and the "new" contract a minimum of 2 years). The dropout reduction likely results from the combined effect of the minimum wage and on-the-job training. The provisions may encourage firms to see apprenticeships as long run investments. Indeed, substituting a new apprentice for the trained youth implies a larger loss in terms of production, due to an enhanced firm-specific human capital of the trained youth, and a lower wage cost gains, due to the higher wage cost of apprentices.

[19] As for the covariate "apprentices hired in firms with more than 500 employees", we do not have a sufficient number of units under 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) which leave us with 17,010 units.
[20] This explains why the sum of all the effects is not one.
[21] Compared with a duration analysis, where the focus is on the moment of transition, our setting has the advantage of including also "indirect" effects such as the persistence of the status. Analyses on the hazard rate using semiparametric estimators are also related to more complex implementation due to changes in composition over the population.

The higher retention of apprentices suggests that firms' churning behaviour is reduced. This becomes evident when looking at the transition to permanent employment, which follows a time pattern very similar to the attachment to the initial apprenticeship but with an opposite sign. 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. Afterward, there is a positive impact of 4 p.p. over the fifth year, which also carries over to the sixth and seventh years after the initial hiring, though at a slightly lower level (+3 p.p.). The shape of the effect is typical of training programmes, where positive employment effects are preceded by a negative impact at the beginning of the treatment ("lock-in effect" – see e.g. Wunsch, 2016). Distinguishing job transitions within the same firm from those that occur between different firms, the bulk of the effect on permanent employment occurs through promotions, particularly during the fifth year after initial hiring.

The 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, suggesting that the time pattern of the effects on dropout 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 presented 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 effects for transformation within the same firm are similar, women show a positive effect on transitions to open-ended contracts with other firms, which is significantly higher than the effect on men.

To consider the effects of the reform by firm size, we split the sample of treated apprentices depending on whether they are initially hired by firms with more or fewer than ten employees; this is the threshold for eligibility to the higher tax rebate. 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 ten employees. The positive effect on attachment to the apprenticeship is much smaller in firms with fewer 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 in small firms, while in larger firms the impact is +6.3 p.p., corresponding to a 39.7% increase relative to those in open-ended contracts in the same firm at that time. In the subsequent 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 ten-employee 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 with the control group, while there is no such effect in small firms. Overall, for small firms the policy seems to have a limited effect on employment in the salaried private sector.

The most likely explanation for the worse performance of the reform in small firms is the lack of firms’ capabilities to deliver on-the-job training, which might have eventually reduced the overall training opportunities for apprentices. Furthermore, smaller firms have a higher incentive to churn and an incentive not to increase the size of their permanent staff to keep their rights to a higher SSC rebate. Apprentices do not contribute to determining firm size for legal purposes, but if they are transformed into regular employees (either permanent or temporary), they might affect the ten-employee threshold and trigger losing eligibility for lower payroll taxes.[22]

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-year window starting from $t = 0$. To account for earnings attrition over the period, we perform the analysis with and without including zero wages; when zero wages are included the outcome can be seen as an overall measure of compensation that includes non-employment spells.[23]

Results are reported in Figure 6. Including zero wages increases the month-to-month volatility of the estimated effects and reduces the precision of the estimates, but the overall pattern is similar. There is an initial sizeable effect of the new apprentice contract on wages, which are almost 20% higher for apprentices hired with the new regime compared with those in the old one. This increase is in line with the higher minimum wage introduced by the reform. Interestingly, the wage gap with apprentices in the old regime 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.

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

[23] 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.

However, there is a significant long-run wage effect from the reform (about +3 p.p., and roughly stable after the fifth year), which possibly reflects the increase of apprentices' human capital thanks to increased opportunity for training under the new regime.

It is also interesting to consider heterogeneity of remuneration effects by gender. There is a distinctive difference in terms of ATT on entry salaries with women showing an effect that is half that of men.[24] 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 +7 p.p.. Looking at heterogeneity by firm size, firms below the ten-employee threshold show no significant remuneration effect in the long run, which is in line with the insignificant effect on the employment rate.

5.2 Robustness

We perform several robustness tests on the estimates. In a first set of tests we check whether the estimates are sensitive to the type of semiparametric estimator on the observables implemented. First, we estimate the ATT by the standard inverse probability weighting. Second, we estimate the ATT using the max trimming rule, and, third, on the untrimmed sample. Fourth, we use the shrinkage method of Pohlmeier *et al.* (2014) on the IPW estimator by the cross-validation method. This method shrinks the estimated propensity score toward the estimated unconditional mean (i.e., the share of treated) to avoid giving to some units excessive weights. Finally, we replace the covariates observed at hiring (i.e., sector, dimension, firm position) with analogous variables but measured during the last job (at least 30 days before the hiring). As shown in Figure 6A in Online Appendix A, results are not significantly different from the benchmark estimates, apart from the last specification, which shows larger effects for transformations and apprenticeship retention.

In the second test we check if the estimates are robust to the presence of potential unobservable confounders. The CBPS estimates rely on the credibility of the conditional independence assumption (CIA). Although we control for a large number of variables that refer to past employment history of apprentices, administrative data do not include education.[25] In general we cannot control for all unobservable factors that may drive a different selection of apprentices in

[24] Results on heterogeneous ATT on remuneration are reported in Figures 4A and 5A in Online Appendix A.

[25] We checked with the Italian Labour Force Survey (LFS) to see whether the level of education of the apprentices had increased during the period of the reform (2004-2011). As the new apprenticeship has been progressively rolled out in Italy, we may expect to observe an increase in the level of education of apprentices if firms started hiring more educated individuals in this contract. We do not find evidence of a compositional change in the education of the apprentices (Figure 8A in Online Appendix A). Furthermore, we implement a DiD estimator as proposed in Section 5.3 to test whether the average level of education has been changing in the sectors and regions since early implementation of the reform. Estimates are not statistically significant (see Table 5A in Online Appendix A).

the two regimes; we can, however, dispel many doubts on correct identification of the results by implementing the sensitivity analysis proposed by Rosenbaum (2002).

The analysis assumes that the estimates might actually be driven by an unobserved confounding factor u affecting the likelihood of treatment D and the outcome Y. The odds ratio of differential treatment assignment due to X and u can be defined as Γ,

$$\Gamma = \frac{p_i(X_i,u_i)*(1-p_j(X_j,u_j))}{p_j(X_j,u_j)*(1-p_i(X_i,u_i)} = \frac{exp(\beta X_i+\gamma u_i)}{exp(\beta X_j+\gamma u_j)} \quad (4)$$

with i and j the treated and the control units, respectively, p is the propensity score estimated by a logit model, X and u are the observed and unobserved confounding factors, and β and γ are their relative effects on the probability of treatment. It can be shown that for matched unit (X_j=X_i), we obtain $\Gamma = \exp(\gamma(u_i - u_j))$, which is equal to 1 if there is no difference in unobserved factor ($u_i = u_j$) or u_i does not affect the probability of treatment ($\gamma = 0$). The goal of this sensitivity analysis is to determine the magnitude of the bias Γ, which would make the treatment effect insignificant. For example, Γ = 2 means that to undermine the analysis, we would need a confounding factor u that makes treated individuals twice as likely to receive the treatment despite having the same X. Note that this bias is a "worst case scenario" as the relation between u and Y is assumed to perfectly determine whether Y of the treated would be larger or smaller than Y of the matched control. Finally, we follow DiPrete & Gangl (2004) and relate the magnitude of this bias introduced from changing an observed X but without controlling for it.

We consider the outcomes where we found the largest effect, i.e., the lock-in in the initial apprenticeship and the transformation to an open-ended job in the same firm during the third, fifth, and seventh years. The Rosenbaum (2002) sensitivity test indicates that to reverse the conclusion we would need a sizable "worst case" confounding factor on top of our covariates. For the lock-in in the initial apprenticeship, the odds of receiving the treatment have to be increased by 54% (100% for apprentices in larger firms). The transformation to a permanent contract requires instead an increase of at least 30% in the fifth year and 15% in the seventh year (35% in both years for larger firms). Results are reported in Table 4A of Online Appendix A. The equivalent bias of 54% in terms of odds ratio required to reverse the lock-in effects (or 15% for the effect on transformations) would be obtained if we increased the total weeks of experience of the treated units by 3.5 times (2.4 for the transformations) or the past full time salary by 5.5 times (or 2.4 times). In the case of larger firms, the induced bias for the lock-in of 100% (35% for transformations) would be reached if we increased these covariates by 5 (2.7) or 8.1 (2.4) times. In addition, let us keep in mind that this is a "worst case" confounding factor as it should also perfectly determine whether Y of the treated is larger or smaller than the Y of the matched control. Overall, the evidence points to the robustness of our results.

5.3 A DiD analysis

To provide further evidence on the robustness of the analysis, in this Section we estimate the effects of the reform relying on a set of assumptions different from the ones behind the CBPS. The policy adoption occurred in a staggered fashion across regions and sectors, which allows us to implement a DiD estimator with multiple groups and time periods.[26] This estimator does not directly control for the sources of selection, but removes time invariant unobserved heterogeneity by taking the double differences in the outcomes between the two groups before and after the reforms; this assumes that counterfactual trends in outcomes are the same for the treated and control groups. The DiD estimator is implemented by the following regression model:

$$\mathrm{Y_{it}} = \sum_{t=1}^{T-1}(\vartheta_t \mathrm{dT}_{it}) + \sum_{r=1}^{R-1}(\eta_r * \mathrm{dREGION}_{ir}) + \sum_{c=1}^{C-1}(\eta_c * \mathrm{CBA}_{ic}) + \sum_{s=1}^{84}(\beta_s * \mathrm{H}_{is}) + \sum_{s=1}^{84}(\delta_s * \mathrm{TREATMENT}_{it} * \mathrm{H}_{is}) + \epsilon_{it} \qquad (5)$$

where dT_{it} are monthly time dummies for the moment of hiring, $dREGION_{ir}$ are regional dummies for the place of work, CBA_{ic} is the industrial collective bargain agreement where worked, and H_{is} are dummies for each *s* month since the start of the apprenticeship. Finally, $TREATMENT_i$ takes value one if at the moment of hiring the sector or the region had already implemented the apprenticeship reform. The coefficient of the interaction between $TREATMENT_i$ and H_{is} (δ_s), represents the treatment effect for each *s* month after hiring.

As mentioned in Section 2, eligibility for the treatment did not automatically translate in its actual take-up. For example, in 2007 the variable $TREATMENT_i$ (constructed by using the CBA) and the observed treatment status of the apprentice show a correlation coefficient for binary variables of 0.794.[27] In this setting of imperfect compliance, δ_{a} represent the reform's intention to treat (ITT), which is a downward estimate of the true ATT estimated by the CBPS estimator exploiting the actual treatment status.

[26] To have sufficient variation in the introduction of the reform over time we enlarge the time window of the inflow sample from 2005 to 2008. The final sample is composed of 69,584 fresh spells.
[27] If we consider also the regional laws the correlation coefficient decreases to 0.546, which confirms that the reform was implemented only in renewed CBA, even in regions implementing the regional law.

If we are willing to assume that the effect of the treatment is stable over calendar time and is the same for the treated and control groups, we can retrieve the local average treatment effect (LATE) on the compliers by implementing a fuzzy difference-in-differences estimator (e.g., Chaisemartin & D'Haultfœuille, 2017). As in a Wald estimator, the LATE can be estimated by dividing the ITT by the effect of the reform on the actual take-up of the treatment. The latter effect is estimated as in equation 5, but replacing Y with the actual treatment status D.[28] Standard errors are cluster robust and calculated by bootstrapping.

The DiD estimator finds similar results to the CBPS estimator. The effect is again insignificant in small firms and of a larger magnitude in other firms. The Wald estimator also confirms the finding of the CBPS estimator (Figure 7). Finally, the DiD estimator is unbiased if the potential outcome in the absence of the treatment follows a parallel path across different industries. We therefore implement a placebo test estimating the ITT on the apprentices hired four months before the actual implementation of the reform. Estimates are small and not significantly different from zero, which confirms the estimates' credibility (Figure 7A in Online Appendix A).[29]

A potential threat to identification comes from endogenous migration, which is the migration of future apprentices to adopting regions on the basis of the expected returns in terms of employment; To dispel these doubts about potential threats to identification, we use data from the Labour Force Survey between 2004 and 2008, and estimate a similar difference-in-differences 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 (see Online Appendix C - full results available upon request), which rules out endogenous migration or commuting as sources of bias in our estimates.

6. Conclusions

We found significant positive effects of the 2003 reform of the apprenticeship contract on wages and transformations to permanent contracts. The reform allowed firms to provide part of the

[28] Note that as we have the actual take-up only after 2007, we can only estimate the effect on the actual take-up in 2008. Therefore, our LATE relies on the additional assumption of constant effect on the take-up in the period 2005-2008.

[29] Different from the CBPS estimator, the long run effect on full-time daily remuneration is insignificant in the DiD. However, as shown in Figure 7A in Online Appendix A, placebo test on the longer run effect of this outcome is rejected.

training on-site rather than only externally, introduced a minimum pay and extended the legal length of the contract. This feature of the reform was introduced with the purpose of encouraging apprentices' learning-by-doing and simplifying firms' administrative burden regarding external training.

To estimate the average treatment effect on the treated, we exploited the contemporaneous presence of two different apprenticeship regimes due to the staggered implementation between regions and sectors. We found robust evidence that, compared to the old apprenticeship regime, the new contract improved the chances of moving to a permanent job in the same firm five years after hiring. However, this happened mostly in large firms. There are also sizeable long-run wage effects of the reform, well beyond the legal duration of apprenticeships. It is hard to say which one of the many changes may explain the results, however it is likely that the possibility of doing training on-site rather than externally may have induced more firms to promote their apprentices into permanent contracts.

The possibility of delivering internal training was "imported" from the German dual system where the training is done part at school and part on-the-job. The Italian and German systems remain however very different systems: to the contrary of Germany, apprenticeships in Italy are full employment contracts and do not originate at school. Furthermore, the accreditation of any training curricula in Italy is a regional competence rather than a centralized feature, thus it has limited acceptance among employers. On top of that, in Germany often apprentices do not find work in the training firm, suggesting that general-human capital skills is also a key component of the German apprenticeship system (Parey, 2009). Yet, in the Italian system where the apprenticeships are entry contracts in the labour market, implementing the German idea that training can be done on-the-job has been good for improving the chances of apprentices to be transformed into permanent workers.

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

	Pre-reform	**Post-reform**
Maximum age at hiring	≤ 24 (29 in some exceptions)	≤ 29
Training	External authorities	External but also internal if the firm has training capabilities
Maximum & minimum contract length	1.5–4 years (5 in craft sector)	2-6 years (De facto 33-56 months)
Wage	Set by the collective bargain agreements	Minimum wage to the remuneration the collective bargain agreements could set

Figure 1: Timing of Regional Implementation of the Reform Until the 1st Quarter of 2011

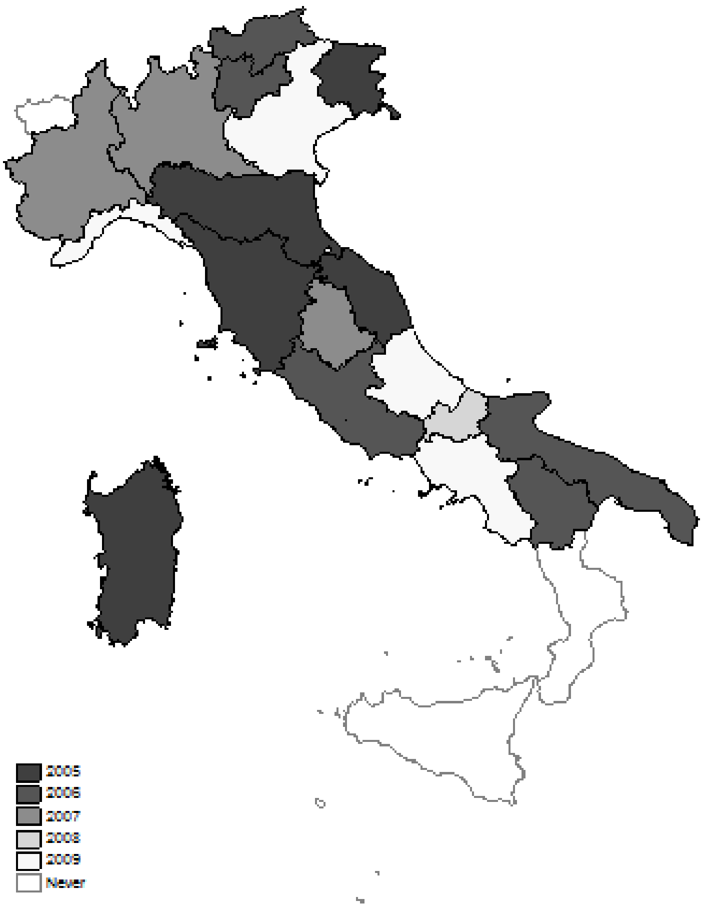
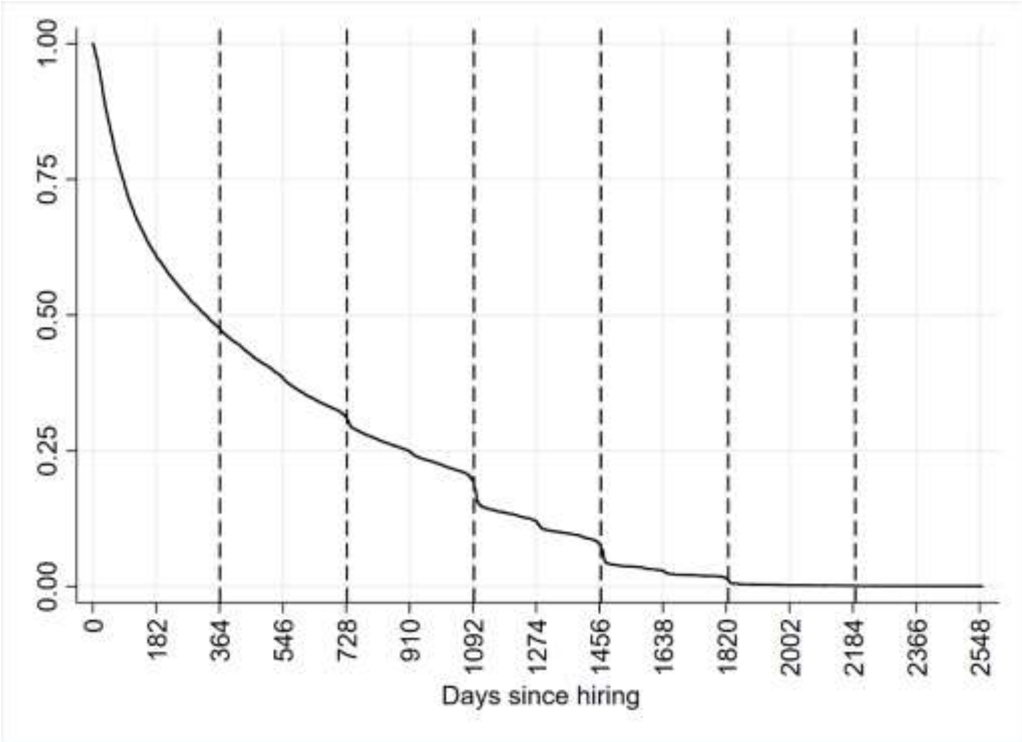


Table 2: Labour Market Status over Time (%)

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

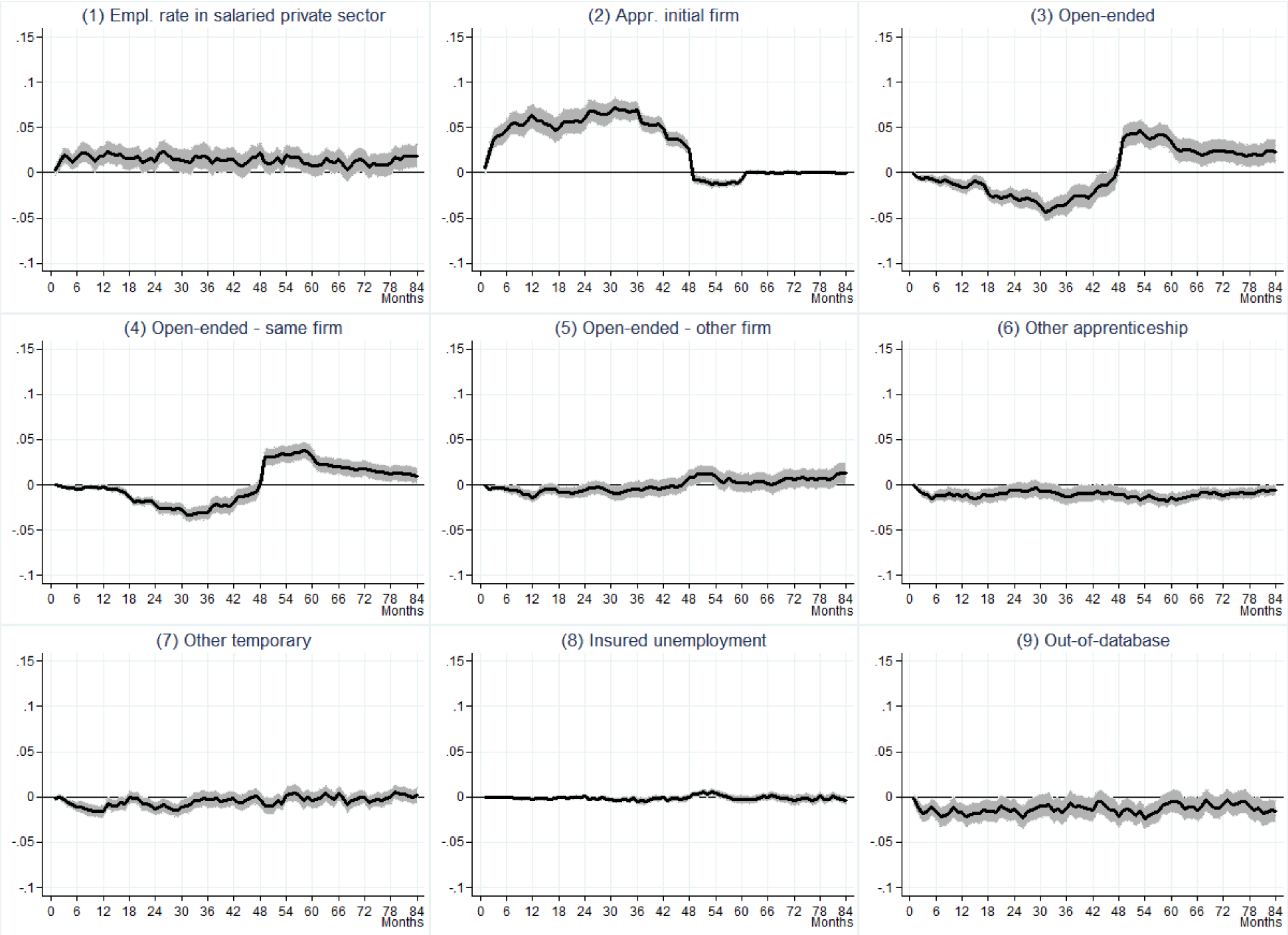
Inflow sample of 17,948 apprentices hired in 2007 aged 19-24. Status at the end of the year after hiring in %: (1) apprenticeship in the first firm, (2) other apprenticeship, (3) open-ended contract in the same firm, (4) open-ended contract in another firm, (5) other temporary contract, (6) collaborator, (7) insured unemployed, (8) not in salaried employment in the private sector. Individuals with more jobs are considered only in one position following the columns order.

Figure 2: Survivor Function in the Initial Apprenticeship



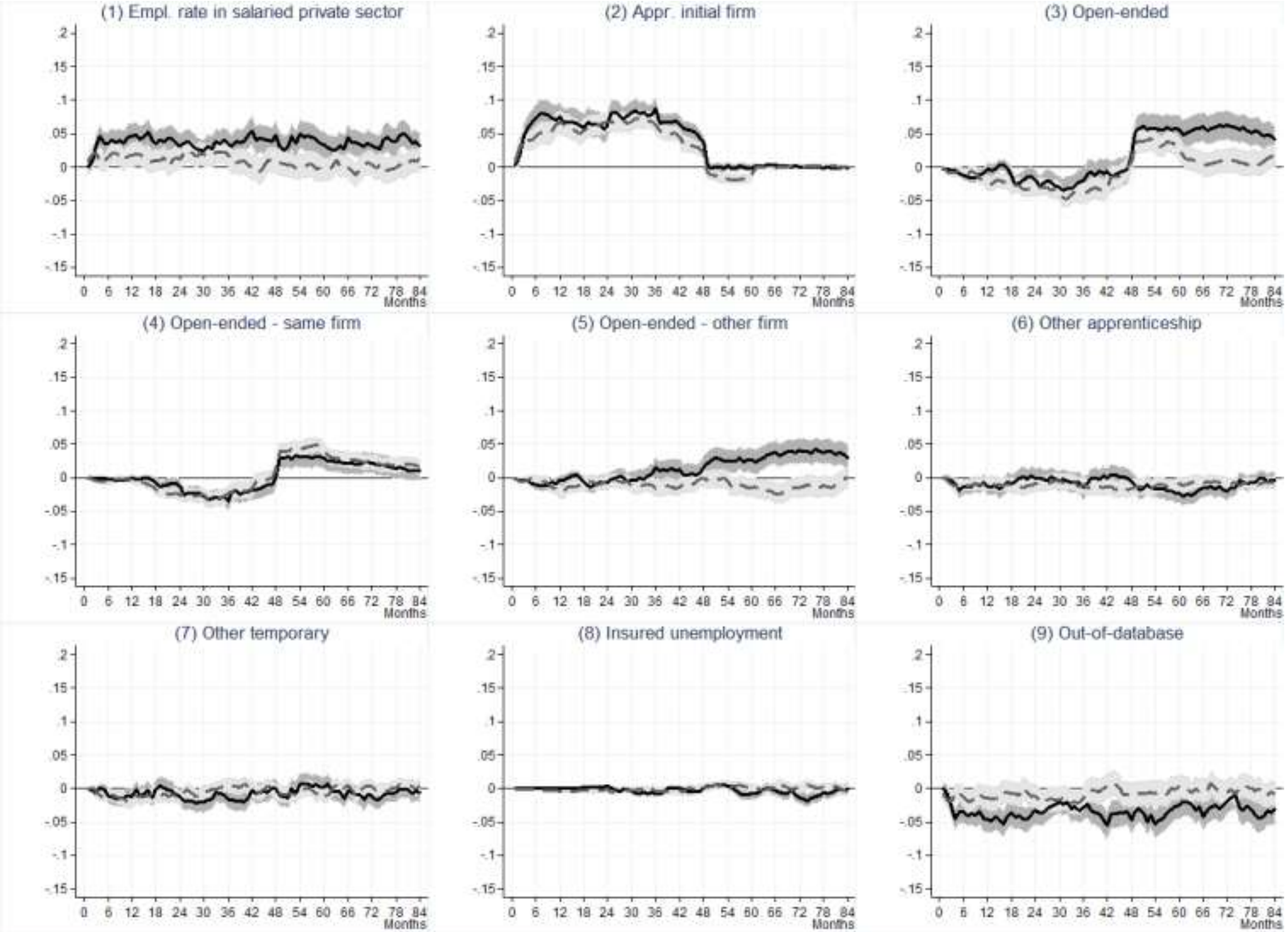
Note: Inflow sample of 17,948 apprentices hired in 2007 aged 19-24.

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



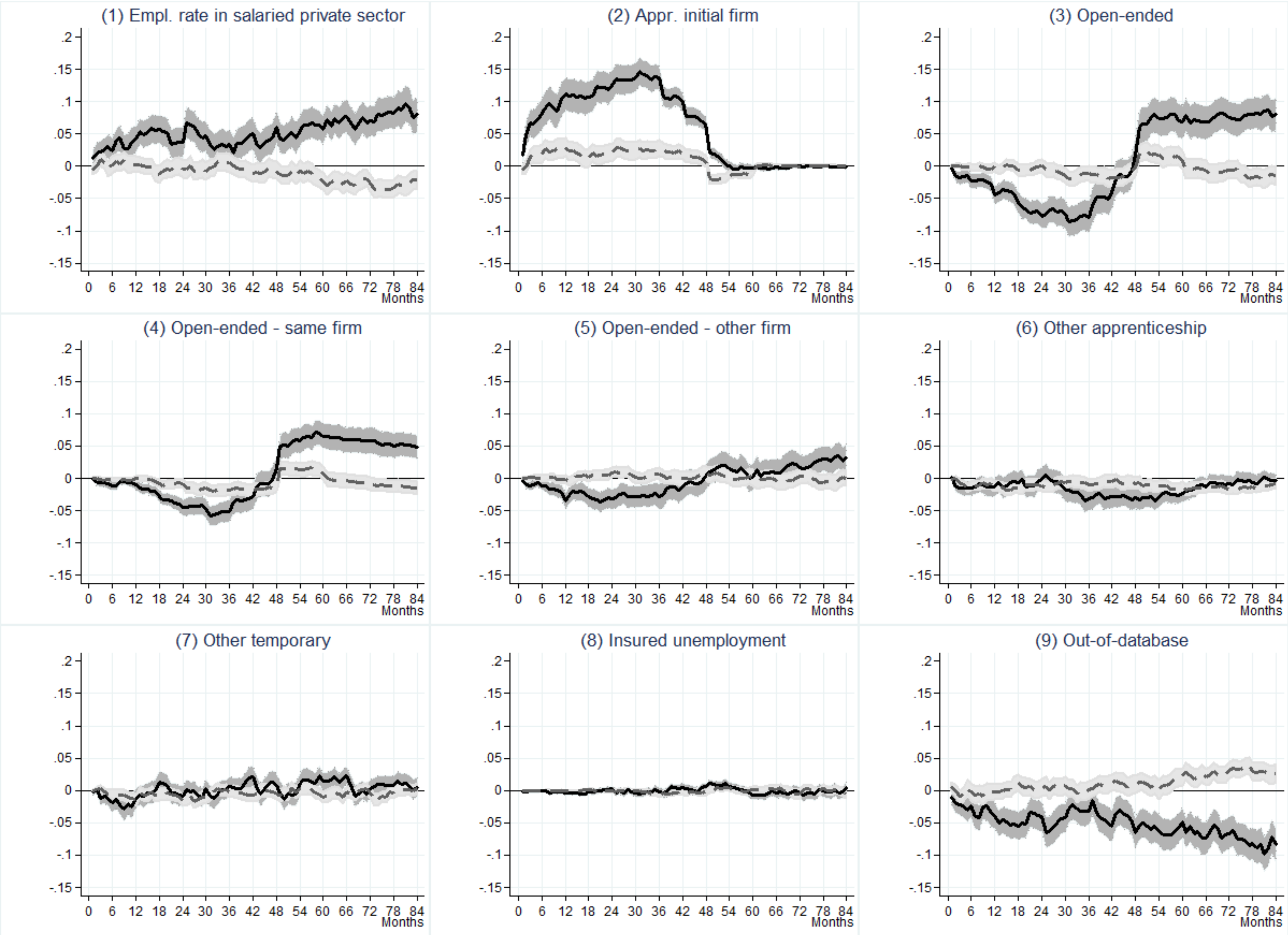
ATT estimated by CBPS estimator of the reformed apprenticeship versus the old apprenticeship on a sample of 16,805 apprentices hired in 2007 aged 19-24 (after trimming). Status at the end of each month after hiring. Bootstrapped standard errors (199 repetitions) clustered by individual to take into account serial correlation.

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



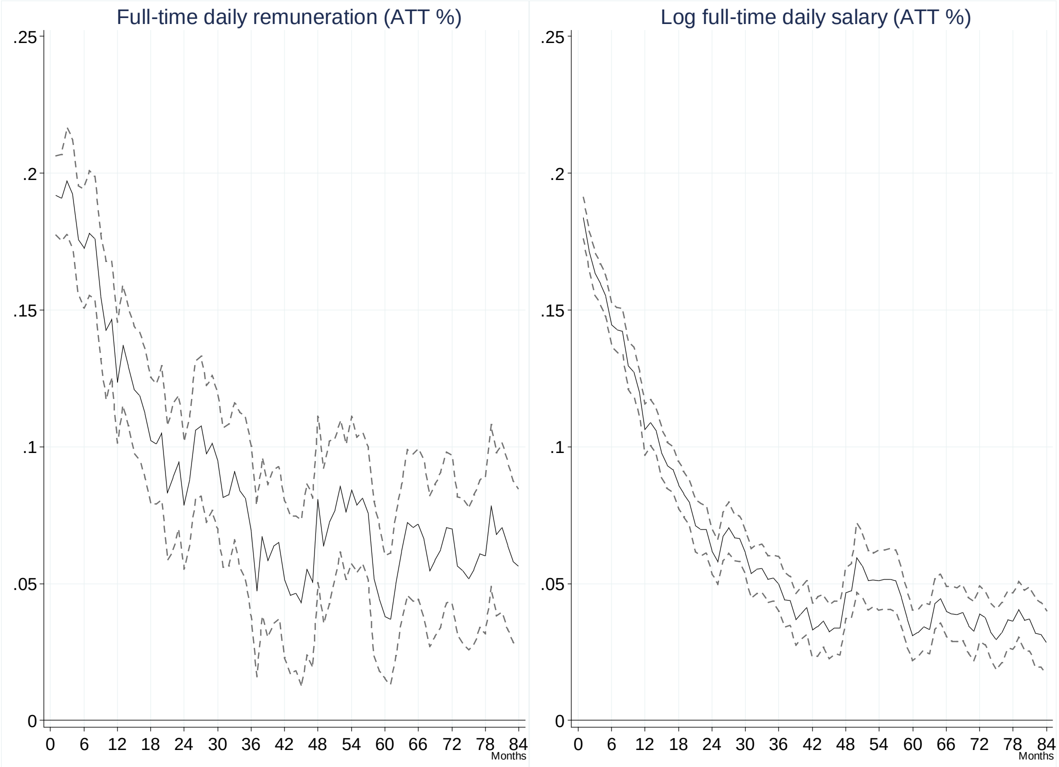
This figure is described as in the note of Figure 3. Results refer to men (dashed lines) or women (solid lines).

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



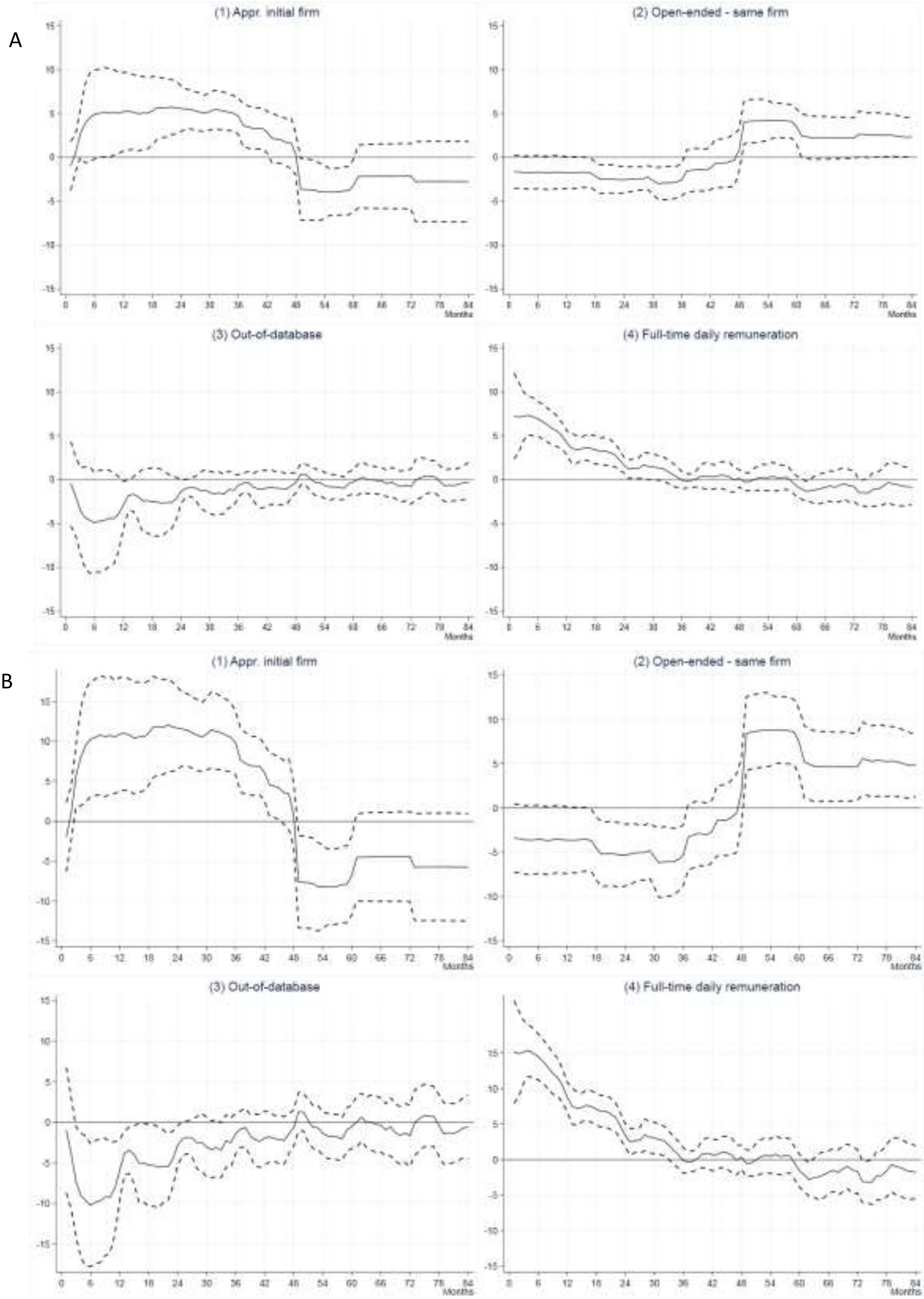
This figure is described as in the note of Figure 3. Results refer to firms below 10 employees (dashed lines) or other firms (solid lines).

Figure 6: ATT on the Full-Time Daily Remuneration: Maintaining (Left) and Removing (Right) Zero Outcomes



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). Effect in %. 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: Event Study on the Apprentices – (A) ITT, (B) LATE (Wald Estimator)



ITT (Panel A) and LATE (Panel B) of the reformed apprenticeship versus the old apprenticeship on a sample of 69,584 *apprentices hired in 2005-2008 and aged 19-24. Panel A is estimated by difference-in-differences estimator, while Panel B is estimated by the Wald Estimator. Treatment status defined as being hired in a sector implementing the reform. Outcomes: status at the end of each month after hiring. The effect on full-time daily remuneration is in absolute terms (and mantaining the zeros). Panel A: robust standard errors, Panel B: boostrapped standard errors with 199 repetitions. Standard errors clustered by collective bargain agreements.*