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Maurizio Franzini

Effectiveness of Targeted Payroll Tax Reductions *

Aggiornamento WorkINPS papers n. 41* - ottobre 2021

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* la cui pubblicazione è stata autorizzata dal "Comitato Scientifico WorkINPS Papers" in data 6 agosto 2022

L'efficacia delle Riduzioni dei Contributi Sociali *

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Abstract

Questi paper usa dati amministrativi italiani per valutare una riduzione dei contributi sociali che ha temporaneamente incentivato l'assunzione di apprendisti. Sfruttando la variazione introdotta dalla Legge Finanziaria del 2007 troviamo che questa politica sia efficace in termini di costi: tale riduzione nei contributi sociali genera un incremento delle posizioni di apprendistato a fronte di un costo di circa €2,000 a contratto. Questa politica ha effetti di lungo periodo e genera un incremento dei posti a tempo indeterminato a fronte di un esborso di €11,500 a contratto pur non incentivando direttamente questa forma contrattuale. I nostri risultati suggeriscono che le riduzioni nei contributi sociali possano essere una politica efficace e possano mitifare gli effetti delle recessioni per le fasce deboli dei lavoratori.

Classificazione JEL: H25, H32, J13, J23, J31, M51
Parole Chiave: riduzioni nei contributi sociali, apprendistato, imprese, costi di manodopera, domanda di lavoro

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The Effectiveness of Targeted Payroll Tax Reductions *

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Abstract

This paper uses rich Italian administrative data to assess Italian payroll tax cuts that temporarily incentivize firms to hire apprentices. Exploiting variation driven by the 2007 policy's eligibility criteria, a difference-in-differences analysis finds that the policy is cost-effective, generating an increase in subsidized apprenticeship jobs at an implied cost of €2,000 per job. However, the temporary incentives have long-term effects: the policy generates persistent increases on unsubsidized full-time jobs, at an implied cost of €11,500 per job. The results suggest that payroll tax cuts are cost effective and may mitigate scarring effects of recessions for marginalized workers.

JEL classification: H25, H32, J13, J23, J31, M51
Keywords: payroll tax cut, apprenticeship, firms, labor costs, labor demand

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1 Introduction

Policymakers often turn to targeted payroll tax reductions to combat high unemployment rates among the young, the low-skilled, and the long-term unemployed (OECD, 2003, 2011). However, critics argue that payroll tax reductions are not cost effective because they subsidize inframarginal employment that would exist even in the absence of the reform. If firms pocket subsidies by offsetting changes to workers' wages, then the cost to expanding employment is high.

Directly measuring the effectiveness of payroll tax reductions in boosting hiring of marginalized groups has two empirical requirements. First, the incentives must be targeted toward marginalized workers. Second, the variation in incentives across firms must be unrelated to unobservable determinants of firm composition. Owing to the lack of policy variation that satisfies both requirements, there has been little direct observation of whether and how firms capture the surplus associated with targeted payroll tax reductions. Cross-regional studies do not directly target marginalized workers (Bennmarker et al., 2009; Benzarti and Harju, 2021a; Bohm and Lind, 1993; Korkeamaki and Uusitalo, 2006), and national studies that use variation driven by targeted and untargeted workers do not have variation in incentives across firms (Bozio et al., 2020; Egebark and Kaunitz, 2013; Huttunen et al., 2013; Saez et al., 2012, 2019, 2021; Rubolino, 2021). Correspondingly, empirical conclusions about the effectiveness of payroll tax reductions are mixed.

This paper analyzes changes in firm behavior to a targeted, temporary reduction in payroll taxes. In contrast to other studies, the policy targets specific hiring at specific firms. In 2007, Italian firms with at most 9 employees were given relief from increases to required social security contributions (SSCs) for apprenticeship contracts. The relief was equivalent to roughly two months of earnings per apprentice, 10% of a typical 2-year apprenticeship, and phased out over time.

We measure outcomes using the confidential matched employer-employee dataset collected by the Italian Social Security Institute (INPS) between 2004 and 2011. These data include i) the registry of all private non-agricultural firms with at least one employee, and ii) the employment histories of all private-sector non-agricultural workers. The latter contains rich information on job spells, contract types, and SSCs. We combine these data with a firm-level balance sheet dataset collected by the *Cerved Group*.

Exploiting the discontinuous change in SSCs, we compare changes in firm outcomes across the 9-employee threshold in a difference-in-differences design. Specifically, we compare small firms (5–9 employees) to medium firms (10–14), classifying them based on their baseline size

in 2006. Lending credibility to the validity of our research design, there is no evidence of differential pre-trends over a variety of outcomes.

We find that reducing payroll taxes increases demand for apprenticeship contracts. Subsequently, a substantial share of apprentices become permanent workers.

Because our policy generates variation across firms, we examine whether the reform led to undesirable and unintended firm responses. First, we find no evidence of firms opportunistically re-labeling existing contracts, shortening contract duration, strategically separating from incumbent apprentices, or compromising on the quality of the new hires. Second, because our design does not rely on using untargeted workers as comparison group, we are able to examine whether increased demand for apprentices is offset by decreased demand for other workers. We find no evidence that apprenticeship and other contracts are net substitutes. This is especially notable because making eligibility contingent on firm size creates an incentive to limit growth in order to maintain eligibility, a distortion that has materialized empirically in other studies (Caicedo et al., 2020; Garicano et al., 2016). We find no evidence that eligible firms limit growth.

Finally, we evaluate the cost-effectiveness of the policy. The policy is more cost-effective if the employment effects are relatively large; it is less cost-effective if the tax cuts primarily subsidize inframarginal behavior. We find that the policy's cost-effectiveness is driven by two factors. First, the incentive was not automatically applied so take-up was incomplete, eliminating the possibility that muted employment effects reflects low salience of the policy. 14.5% of eligible firms receive some reduction in SSCs in January 2007, at the onset of the reform, reflecting both an extensive margin (i.e. not all firms hire apprentices) but also an intensive margin (i.e. not all firms that hire apprentices take up the discount). Incomplete take-up suggests that poor salience of automatic subsidies may decrease their cost-effectiveness. Second, the presence of wage floors imposed by the CBAs prevent firms from pocketing the subsidy by offsetting changes in payroll taxes to workers' wages. Scaling our reduced-form intent-to-treat effects by the intensive-margin compliance rate using instrumental variables, each complier firm that takes-up the subsidy hires approximately 1 additional apprentice.[1]

Accounting for non-compliance, we estimate that the government has to provide a discount of approximately €2,000 per additional apprenticeship contract. Since only some apprentices gain permanent employment, the implicit cost of a permanent contract is €11,500. These figures only account for the direct costs of the program and not indirect cost savings from decreased utilization of social welfare programs of gainfully employed workers. These

[1]We also find evidence that suggest that compliance is not one-sided and that some ineligible firms receive the discount.

estimates thus represent an upper bound on the *net* cost per job created and compare favorably with other studies that focused on disadvantaged workers (Cahuc et al., 2019; Egebark and Kaunitz, 2013; Neumark, 2013). We conclude that this policy is highly cost-effective.

Finally, our results are robust to the inclusion of trends in baseline firm characteristics and sector and region linear trends. We also show that our results are not driven by differences in firm size between small and medium firms and differential exposure to the Great Recession or liquidity constraints.

This paper relates to three strands of literature. First, it fits in the broader literature on the effectiveness of wage subsidies (Cahuc et al., 2019; Katz, 1998; Neumark, 2013; Sestito and Viviano, 2018; Zurla, 2021). Although wage subsidies have often been used to sustain employment during periods of economic crisis, there is less evidence on their effectiveness during a recessions (Benzarti and Harju, 2021a; Cahuc et al., 2019). Moreover, there are some concerns that their effectiveness is strongly diminished when they target disadvantaged groups (Neumark, 2013; Stanley et al., 1998). We show that payroll reductions sustain the employment of a disadvantaged group even in times of crisis. The short-run effects of our policy greatly underestimate its overall impact. Furthermore, our rich administrative data allow us to study transitions to permanent jobs. We provide evidence that temporary subsidies to temporary jobs can be a cost-effective method for creating permanent, open-ended employment. Importantly, the cost-per-job created of this policy is at the lower range of estimates in the previous literature on incentives for disadvantaged workers (Cahuc et al., 2019; Egebark and Kaunitz, 2013; Neumark, 2013).

Second, our work closely relates to the literature that focuses more specifically on the effects of payroll tax cuts. Research has analyzed changes in payroll taxes that either target a subset of workers or specific regions (Bohm and Lind, 1993; Bennmarker et al., 2009; Benzarti and Harju, 2021a; Bozio et al., 2020; Egebark and Kaunitz, 2013; Huttunen et al., 2013; Korkeamaki and Uusitalo, 2006; Saez et al., 2012, 2019, 2021; Rubolino, 2021). Evaluating the former yields only relative employment effects, and effects from the latter may not be driven by workers at the margins of the labor market. Thus, neither of these strategies can directly measure substitution when specific types of jobs are subsidized. Moreover, with only a single notable exception (Rubolino, 2021), previous studies examine *permanent* changes in payroll taxes (Bohm and Lind, 1993; Benzarti and Harju, 2021a,b; Bennmarker et al., 2009; Egebark and Kaunitz, 2013; Korkeamaki and Uusitalo, 2006; Huttunen et al., 2013; Saez et al., 2012, 2019, 2021). Limitations notwithstanding, employment and earnings results across studies are mixed. Our contribution is twofold. First, unlike previous studies, our paper exploits unique policy variation that targets specific workers only at specific firms. This gives rise to a

clean and straightforward design to study firm responses. Second, we show that a *temporary* reduction in SSCs increases the employment of the targeted group and that this does not crowd-out other contract types.

Third, our paper speaks to the literature on training programs. While a large body of work has studied the returns to apprenticeships for *workers* (Acemoglu and Pischke, 1998; Albanese et al., 2017; Cavaglia et al., 2020; Fersterer et al., 2008; Lodovici et al., 2013; Maida and Sonedda, 2021; Picchio and Staffolani, 2019), much less is known about how firms respond to these type of policies (Alfonsi et al., 2020; Caicedo et al., 2020; Cappellari et al., 2012). Understanding firm responses is key to designing policies that are both cost-effective and that minimize unintended consequences such as firms' opportunistic behavior and firm-size distortions. We contribute to this literature by evaluating several margins that are indicative of firms engaging in opportunistic behavior: relabeling existing contracts, changing contract duration, strategic separations from incumbent apprentices, limited transformations, changes in quality of new hires. We find that payroll tax reductions can increase the employment of the targeted group without generating costly firm-size distortions or unintended opportunistic firm behavior.

The paper is structured as follows. Section 2 describes the institutional setting. Section 3 and 4 present the data and the stylized facts, respectively. Section 5 develops the empirical strategy and discusses the identifying assumption. Sections 6 and 7 discuss the main results. Section 8 reports the robustness checks. Section 9 evaluates the cost-effectiveness of the policy. Finally, Section 10 concludes.

2 Institutional setting

In this section we describe the legal framework for apprentices contracts in Italy and the reform we study.

2.a Apprenticeship Contracts in Italy

Apprenticeships are typically labor contracts that give workers the opportunity to earn a professional qualification and a salary in exchange for labor services (Snell, 1996; Ryan, 2012). Because training is socially valuable, governments often allow firms to pay apprentices lower wages and SSCs (Kuczera, 2017). In what follows we describe the most important features of Italian apprenticeships.[2]

[2]In Italy there are three types of apprenticeship contracts – two of which are quite rare. In this section, we describe the "apprenticeship for job qualification" (*apprendistato professionalizzante*), which covers approximately 95% of apprentices in the country (D'Arcangelo et al., 2019). Refer to Appendix A for the details on the other

In Italy the law mandates at least 120 hours of training per year, of which at least 80 hours are devoted to occupation-specific training and 40 hours devoted to general training (job safety regulation, psychology of labor, and team working). The CBAs regulate the content of the training. In principle, training should be a combination of on-the-job training and formal education, which can be offered either by the firm or by government-funded third parties (see Albanese et al. (2017) for more details).[3].

During the period of study, only private-sector workers aged 18-29 are eligible to work as apprentices. Newly hired apprentices go through a short probation period (maximum 2 months) after which they can be laid off only for just cause. Apprenticeship contracts can last at most 6 years. At the end of the contract, the firm can decide whether to convert the apprenticeship contract into an open-ended contract or to let the worker go at no additional cost.

In return for the training they provide, firms can pay apprentices up to 2 levels below the pay grade negotiated by the CBAs. Firms also benefit from lower employer SSCs (starting from 2007 employer SSCs on apprenticeship contracts amount to 10% of the apprentice earnings, while employer SSCs on open-ended contracts amount to 27% of the worker's earnings – see Section 2.b). Firms also pay lower SSCs in the first year in which the apprenticeship contract is converted into an open-ended contract (Law 56/1987).

The law imposes limitations on the use of these contracts. Firms cannot employ more apprentices than regular workers. An exception to this rule is that firms with at most three workers can employ up to three apprentices. To incentivize the use of apprenticeship contracts as a stepping stone toward open-ended contracts, many CBAs set quotas on the transformation rate of apprenticeship contracts. To hire any additional apprentice, firms have to offer an open-ended contract to a given fraction of the apprentices they have had in the previous one to two years. Quotas vary by CBA and can be as high as 70% (D'Agostino et al., 2010).

In the next section, we discuss the reform we study.

two contract types.

[3]In Italy there is a debate about whether such training is really valuable for workers. Michele Tiraboschi, a legal scholar, goes as far as saying "Although a number of legal provisions establish compulsory training during apprenticeship, reality is often very distant from the ideal apprenticeship model, and this tool becomes a mere instrument of exploitation of a flexible and cheaper labour force" (Tiraboschi, 2014). Some evidence on the returns to apprenticeships contracts on Italian young workers is provided in Citino (2020).

2.b The 2007 Budget Bill

The 2007 Budget Bill (Law n.296/2006) was passed on December 27, 2006 and became effective on January 1, 2007. The bill contained a provision that increased employers' SSCs on apprenticeship contracts to finance the introduction of their paid sick leave.[4]

Figure 1 illustrates how SSCs changed in response to the 2007 Budget Bill. We call "small firms" those with sizes between 5 (included) and 9 (included), and "medium firms" those with sizes between 9 (excluded) and 14 (included). Before 2007, all firms paid a fixed weekly fee of €2.85 per apprenticeship contract.[5] This amounted to roughly €148 per year irrespective of the apprentice's tenure (green triangles in Figure 1). Starting from January 1, 2007 the employer SSCs changed discontinuously for firms above and below the 9-employee threshold. Under the new regime, medium firms paid 10% of the apprentice's earnings in social contributions (hollow blue circles in Figure 1). Small firms paid 1.5% of the apprentice's earnings in the first year of the contract, 3% in the second year, and 10% in all the following years (orange circles in Figure 1).

The discontinuity in the change in labor costs around the 9 employees threshold provides a clean empirical setting to study the impact of a change in payroll taxes on firm outcomes.

The increase in SSCs applied to both existing apprenticeship contracts and those signed after January 1, 2007. Firms with full-time equivalent employment of at most 9 (excluding apprentices, temporary agency workers, workers on leave, and workers with an on-the-job training contract) qualified for the discount. We construct firm size based on the definition provided in the official INPS documents that define eligibility criteria (see Appendix B for more details).[6] For contracts signed after January 1, 2007, eligibility was determined by the firm size at the time of hiring. For pre-existing contracts, the eligibility was determined based on the average firm size in 2006. Firms were not able to manipulate their average firm size in 2006 because it was pre-determined at the onset of the policy.[7]

The eligibility for the SSC discount was not conditional on firms having net employment growth over the period nor on converting a given share of apprentices to open-ended con-

[4]The 2007 Budget Bill also introduced other policies aimed at fighting informal employment, reducing the tax wedge on labor, tweaked the eligibility rules for unemployment insurance, and short-time work. The full text of the law can be accessed at http://www.parlamento.it/parlam/leggi/06296l.htm

[5]The weekly fixed contribution amounted to €2.95 for the subset of contracts eligible for occupational injury insurance. A 2.85 (2.95) weekly fee translates into $2.85 \times 52 = 148.2$ ($2.95 \times 52 = 153.4$) euros per year.

[6]A detailed description of the eligibility criteria can be found in the *circolare n. 22, 2007* by INPS. Our rich administrative data allow us to follow this definition closely.

[7]The 2007 Budget Bill also introduced some changes to the SSCs for other contract types but none of these changes exhibited a discontinuity in firm size.

tracts. No other pre-existing or concurrent policy was discontinuous at nine employees.[8]

Next, we illustrate the extent to which the 2007 Budget Bill changed SSCs through a numerical example. Table 1 reports the implied changes in SSCs for the average apprenticeship contract at baseline (yearly earnings: €12,000). Columns 1 and 2 report the employer SSCs for small and medium firms respectively; columns 3 and 4 report the SSCs under the new regime; finally, columns 5 and 6 report the change in SSCs in the new regime relative to the old one. Medium firms experienced an increase in SSC of €1,052 per apprentice per year (column 5), while firms with 9 employees or less experienced an increase of €32, €212, and €1052 per apprentice in the first, the second, and the third year of the contract, respectively (column 6). In this stylized example, all firms experience an increase in the employer SSCs and the increase is greater for firms above the size threshold. Under the new regime, small firms save the equivalent of almost two months of an apprentice's earnings in the first two years of the contract relative to medium firms. This is not a negligible discount given that the average apprenticeship contract lasts 20 months.

3 Data

In this section, we describe the data that form the basis of our empirical analysis and how we construct our sample.

3.a Data and Sample Selection

Social Security Records. Our main source of data is the confidential matched employer-employee dataset collected by the Italian Social Security Institute (*Istituto Nazionale di Previdenza Sociale*—INPS hereafter) known as UNIEMENS. These data cover the universe of all private non-agricultural firms with at least one employee from 1983 to today. Firms are identified by a unique tax number and workers are identified by their social security number. As for firms, these data include location, detailed industry codes, juridical status, and opening and closing dates. For each job spell, we observe the beginning and end dates, earnings net of SSCs, detailed information about whether the contract is covered by specific policies, part- versus full-time status, coarse occupation categories (apprentice, blue-collar, white-collar, or manager), and worker's demographic information (e.g., gender and age). The social security records also contain detailed information on employer SSCs and whether the firm claimed

[8] In line with the fact that no other policy was implemented discontinuously at the 9 employee threshold, Figures 9a and 9b show that the distribution of firm size in the two years leading up to the reform is smooth at 9 employees.

the SSC discount. Appendix C provides more information on how we construct the variables used in the empirical analysis. The UNIEMENS database does not contain information on self-employed workers, the unemployed, the informally employed, or those employed in the public sector.

In our analysis, we primarily use data between 2004 and 2011. However, we also use the full length of our panel to construct the complete workers' histories (e.g., previous employment status and previous earnings) and their contract duration. As another reform changed the employer SSCs for apprentices contracts in 2012, we do not examine outcomes past 2011 to avoid picking up the effect of the 2012 reform.

We also have access to a more disaggregated dataset where earnings are reported at a much finer monthly frequency starting from 2005. This is important as it allows us to precisely distinguish salaries that apprentices accumulate during their first year of tenure against subsequent years, as well as compute the exact monetary amounts that the firm receives because of SSC discounts.

Balance Sheet Records. We match the social security records with the balance sheet information collected by the *Cerved Group*. This dataset contains information on balance sheets and income statements of all Italian incorporated companies (*società di capitali*). We use these data to construct financial indicators such as the labor share, liquid assets over assets, investment over assets and cash flow over assets, which we use in our robustness tests.

Sample selection: We impose two sample restrictions. First, we select firms with average full-time equivalent employment (excluding apprentices, temporary agency workers, workers on leave, and workers with an on-the-job training contract) between 5 and 14 in 2006. Second, we require firms to be active during all years from 2004 to 2006. This selection procedure yields a sample of 193,297 firms (full sample). Our full sample is skewed toward small firms by construction. However, because in Italy a disproportionate share of apprenticeship contracts take place at small firms, this is a good setting to study how changes in SSCs impact apprenticeship contracts.

Since we observe balance sheet data for incorporated businesses only, our matched INPS-*Cerved* data contains 98,084 firms (matched Cerved sample). We use this smaller sample in some of the robustness checks.

The next section describes the characteristics of our sample.

3.b Descriptive Statistics

Table 2 displays the summary statistics for firm characteristics at baseline (i.e., in 2006). Column 1 reports the characteristics for the full sample; columns 2 and 3 display the statistics for small and medium firms, respectively. In our empirical analysis, we classify firms on their 2006 average firm size.

Panel A shows that, although worker composition is fairly similar between small and medium firms, small firms employ a slightly larger share of workers below age 30 (27% vs 25%) and of female workers (36% vs. 34%). Both small and medium firms are well established and the typical firm in our sample is 14 years old on average. Medium firms are one year older than small firms on average. Panels B and C show that the two groups are well balanced on industry composition and geographic location. The only notable difference is that medium firms are 8 percentage points more likely than small firms to operate in the manufacturing sector.

Next, we discuss the summary statistics of the outcomes of interest. Column 1 of Table 2 reports the descriptive statistics for the full sample; columns 2 and 3 report the statistics for small and medium firms, respectively. All statistics are computed across firm-year observations (2004-2011). The average firm in our sample has 9.03 full-time equivalent employees and small firms employ fewer employees than medium ones by construction (7.41 vs 12.16 workers). Full-time equivalent employment is marginally lower than the average number of employees, reflecting that most of the workers are employed full-time. Although the average firm is our sample is quite small, its survival rate is extremely high (92%) and there is no evidence of differential attrition of small vs medium firms (92% vs 93%).

Apprenticeship contracts are less prevalent in Italy than in Germany or Austria, the poster children for apprentices. In Italy, apprentices make up 1.8% of the labor force, compared to 3.6% in Germany and Austria, and 0.2% in the US (G20-OECD-EC Conference, 2014). In the full sample 4.4% (0.41/9.41) of workers have an apprenticeship contract. Apprentices make up 4.6% (0.36/7.75) and 3.9% (0.49/12.61) of workers in small and medium firms, respectively, reflecting the fact that this contract type is more prevalent in smaller businesses. Anecdotally, while some firms use this contract type unsparingly, many firms do not use it at all.

Apprenticeship contracts last on average about 20 months and apprentices experience a substantial amount of turnover. The average firm hires 0.31 apprentices and separates from 0.34 apprentices each year. One out of four apprentices is offered a full-time position at the end of their contract (“transformations" hereafter).

4 Stylized Facts

In this section, we discuss two stylized facts that are important for the interpretation of our reduced form estimates: the prevalence of take-up rates and the pass-through of the reduction in SSCs to workers' net earnings.

Take-Up Rates

Figure 2 illustrates the relationship between the share of firms that claim the subsidy in January 2007 (take-up rate) and the average firm size in 2006. The take-up rate is approximately 14.5% for firms below the 9-employees threshold, sharply decrease around 9-employees, and converges to 3.4% for firms above the threshold.

The fact that take-up does not drop to zero past the threshold is consistent with two non-mutually exclusive explanations. First, there could be some measurement error in firm size. We do not observe the flag for the on-the-job training contracts established by the ex D.lgs. 251/2004 and for workers on a temporary leave (see Appendix B). Because these workers ought to be excluded when calculating firm size, this may artificially inflate firm size for firms that use such arrangements. However, this specific on-the-job training contract type is very rare in these years and the prevalence of temporary leave is quite limited, therefore, these sources of measurement error are likely to be small. Moreover, the eligibility for the SSCs discount is determined by the firm size at the time of hiring as opposed to the average 2006 firm size. This is likely to increase the noise in the relationship between pre-determined firm size and take-up rates. Second, there may be non compliance with policy rules such that some firms get the payroll tax reduction despite being ineligible. As we discuss in Section 5, under this form of non-compliance, our intention-to-treat (ITT) estimates will identify a lower bound on the true effect.

The take-up of the policy is affected on the extensive margin—not all firms hire apprentices—and the intensive margin—not all firms that hire apprentices receive the discount. Whereas Figure 2 shows the extensive and intensive margin combined, next we examine the latter. The probability of receiving the discount conditional on hiring at least an apprentice ("attention rate") is decreasing in firm size. The attention rate is 80% for firms with 5 employees, 50% for firms with 9 employees, and to 20% for firms with 14 employees.

This implies that between 20% and 50% of small firms leave some money on the table. This may be driven by either firms being unaware of the policy or by the hassle cost of applying for the subsidy being higher than the savings. The attention rates in our setting compare favorably with those in similar studies (Cahuc et al., 2019), suggesting that the hassle cost

and the informational frictions may not be very severe.

In the next Section we examine the extent to which firms shift the changes in SSCs to workers.

Earnings and pass-through

Critics of payroll tax reductions argue that these policies may have limited effects on employment if firms can shift changes in labor costs to workers net earnings. Therefore, we begin our empirical analysis by examining the impact of the reform on apprentices' earnings.

In a perfectly competitive labor market the tax incidence depends on the relative elasticity of labor demand and labor supply. Workers and firms share the burden of the tax provided that the labor supply has a positive slope and that the labor demand is less than perfectly elastic. Let w and τ be the pre-tax wages and the payroll tax, respectively. Under the old regime, the apprentice's net (after-tax) equilibrium wage is $\widetilde{w}^* = w - \tau^0$ (where τ^0 the payroll tax). In response to an increase in the employers payroll taxes, firms adjust workers' net wages downward to compensate for the increase in taxes. After January 2007, the apprentice's net wage would become $\widetilde{w}_S = w - \tau^S$ at a small firms and $\widetilde{w}_M = w - \tau^M$ at a medium firm. τ^S and τ^M are the payroll taxes at small and medium firms, respectively, and $\tau^S < \tau^M$. In a frictionless perfectly competitive labor market, apprentices bid down net wages at small firms until a market *net* wage $\widetilde{w}^{**} < \widetilde{w}^*$ emerges. A perfectly competitive labor market model with positively sloped labor supply and less than perfectly elastic labor demand predicts that an increase in payroll taxes leads to a reduction of apprentices' net wages, but no differential effect for small and medium firms.

Our setting departs from a perfectly competitive market in two ways. First, apprentices' wages are subject to wage floors set by the CBAs. This may prevent firms from adjusting wages in response to an increase in payroll taxes and may force them to bear the increase in the labor costs. Second, workers may not be able to freely move across firms due to the presence of hiring and firing costs. This may prevent wages from equalizing across firms in equilibrium.

Figure 3 illustrates the evolution over time of average real *net* apprentice monthly earnings in small (red circles) and medium firms (blue circles), respectively. Firms are classified on their 2006 average firm size. Real earnings increase at a relatively constant rate over time suggesting that the ability of firms to lower apprentice wages is limited in this setting. Moreover, there is virtually no difference in the earnings of apprentices at small and medium firms before the reform, and earnings at small and large firm track each other quite closely over time. Setting lower earnings for new hires may be easier than renegotiating the compensation of

incumbent workers. However, the patterns described in Figure 3 are robust to the exclusion of incumbent apprentices (see Appendix Figure D.1).

We conclude that the increase in payroll taxes had no appreciable impact on apprentices' earnings. This is inconsistent with the standard perfectly competitive market model, but it is consistent with the presence of binding wage floors set by the CBAs. Our results are in line with previous studies showing that firms do not (fully) adjust net wages in response to changes in payroll taxes (Benzarti and Harju, 2021b; Bozio et al., 2020; Rubolino, 2021; Saez et al., 2012). In the next section we discuss the empirical strategy and examine the impact of the policy on employment and workers' flows.

5 Empirical strategy

This section presents our empirical strategy. We exploit the change in SSCs introduced by the 2007 Budget bill in a difference-in-differences framework. We begin our empirical analysis by comparing the evolution of outcomes of small and medium firms (intention-to-treat approach). Then, we estimate the impact of the policy for complier firms using an instrumental variable approach.

5.a Intention-to-Treat Approach

Our approach leverages the discontinuous change in the costs of apprenticeship contracts introduced by the 2007 Budget Bill. As documented in Section 3.b, take-up rates do not exhibit a sharp discontinuity at the threshold, thus we cannot use a regression discontinuity design. We follow Cahuc et al. (2019) – who face the same challenge – and we opt for a difference-in-differences design that compares firms close to the threshold. Concretely, we compare the evolution of the outcomes of small firms (between 5 and 9 employees) and medium firms (between 10 and 14 employees) from 2004 to 2011. We estimate the following model

$$y_{it} = \alpha_i + \tau_t + \sum_{k \neq 2006} \beta_k \cdot \mathbf{1}(\text{year}_t = k) \cdot T_i + \epsilon_{it}, \tag{1}$$

where y_{it} represents the outcome for firm i in year i. α_i and τ_t are the firm and time fixed effects, respectively. The firm fixed effects control for time-invariant heterogeneity across firms, while time fixed effects absorb shocks that affect all firms equally. T_i is a dummy variable that takes value one for firms whose average size in 2006 was at most 9 employees.

Because firm size at the time of hiring may be affected by the reform, we choose to classify firms on their *baseline* firm size. At the time of the reform, baseline firm size is a pre-determined variable relative and cannot be manipulated. Standard errors are clustered at the firm level. Our results are robust to clustering at the 2-digit sector level or the local labor market level (see Appendix Figure D.9).

Our empirical strategy is predicated on the fact that 2006 firm size is predictive of firm size in other years. Figures 4a-9f report the scatter plot of firm size in each year and baseline firm size. This Figure shows that 2006 firm size is highly predictive of firm size in other years (slope between 0.88 and 0.97).

The identifying assumption is that 2006 firm size is not predictive of *changes* in the potential outcomes (i.e., parallel trend assumption). While we cannot test this assumption directly, we can probe the validity of our design by testing whether the β_k coefficients in the years leading up to the reform are statistically significant.

Under the identifying assumption, β_k identifies the impact of the policy on small firms in year k (intention-to-treat – ITT hereafter). As shown in Figure 2, some ineligible firms receive the SSC discount. This type of non-compliance biases our results toward zero; thus, our point estimates can be interpreted as a lower bound on the true effects. In Section 5.b we correct for non-compliance using an instrumental variable strategy.

Next, we discuss some threats to our empirical strategy and foreshadow some of the robustness tests we present in Section 8. First, small and medium firms may differ in characteristics that predict changes in potential outcomes. In Section 8.a, we show that our results are robust to controlling for a rich set of firm baseline covariates interacted with year fixed effects and region $\times$ sector linear time trends.

Second, small and medium firms have a different baseline sizes by construction. Section 8.b shows that our results are robust to restricting our sample to firms between 8 and 11 employees, suggesting that differences in firm size do not act as a confounding factor.

Third, our estimates may be confounded by differential exposure to the Great Recession or liquidity contraints. In Section 8.c, we show that our results are not heterogeneous across labor markets that are differentially exposed to the Great Recession or across firms that have different liquity contraints.

The intention-to-treat approach presented in this section is transparent, but the magnitude of the estimates may be hard to interpret. We address this issue in the next section.

5.b Instrumental Variable Approach

The magnitude of the intention-to-treat estimates may be hard to interpret due to the fact that not all firms hire apprentices and the presence of non-compliance. In this section, we present an instrumental variable (IV) strategy that allows us to recover the impact of the policy on complier firms by implicitly rescaling our ITT estimates by the difference in take-up rates between small and medium firms over time. We estimate the following model:

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=2007}^{2011} \lambda_k \cdot \mathbf{1}(\text{take-up}_{it} = 1) \cdot \mathbf{1}(\text{year}_t = k) + \nu_{it}, \tag{2}$$

where $\mathbf{1}(\text{take-up}_{it} = 1)$ is a dummy that takes value 1 if the firm receives a reduction in SSC for *at least one* apprenticeship contract in year t. This includes reductions in SSC on either the incumbent apprentices or the new hires. Since the policy did not exist before 2007, this dummy is mechanically zero in all years from 2004 to 2006. We then exclude $\mathbf{1}(\text{take-up}_{it} = 1) \cdot \mathbf{1}(\text{year}_t = k)$ for $k = 2004, 2005, 2006$ to avoid perfect multicollinearity.

Because the policy take up is endogenous, we estimate (2) via two-stage least squares (2SLS) and instrument each $\mathbf{1}(\text{take-up}_{it} = 1) \cdot \mathbf{1}(\text{year}_t = k)$ with the set of dummies with a $T_i \cdot \mathbf{1}(\text{year}_t = k)$. λ_k identifies the impact of the reform for *complier* firms in year k.

6 Main Results

This section presents our main results. First, we examine the impact of the reduction in SSCs on workers' flows, employment, and firm size by comparing small and medium firms (intention-to-treat approach). Then, we estimate the impact of this policy for complier firms and discuss the magnitude of our estimates (IV approach).

6.a Intention-to-Treat Results

This section reports the intention-to-treat impact of the policy on firm-level outcomes. Each figure reports the point estimates and the associated 95% confidence intervals for the β_k's coefficients from equation (1). We begin our empirical analysis by examining apprenticeships flows. Figure 5 reports the estimated impact of the policy for new apprentice hires (blue circles), separations (red diamonds), and transformations into open-ended contracts (black triangles), respectively. For all three outcomes of interest the coefficients leading up to the year of the reform are not statistically significant, corroborating the identifying assumption

that firm size is not predictive of changes in the potential outcomes.

The impact of the reduction in the employer SSCs on the number of new hires is positive but small and not statistically significant in 2007. Starting from 2008, the point estimate increases gradually, peaks in 2009, and then stabilizes around 0.06 in 2011 (a 21% increase relative to the average number of apprenticeship hires in our sample).

The reduction in employer SSCs does not affect separations in the first two years, implying that firms do not terminate incumbent apprentices and substitute them with cheaper new hires. The impact on separations starts increasing in 2009 and remains positive and statistically significant for the rest of the sample period. The increase in hiring results in a two-year *lagged* increase in separations, consistent with the fact that the average apprenticeship contract in our sample lasts about 20 months. Interestingly, the number of transformations increases on impact (albeit it is only marginally statistically significant in 2007) and remains positive and statistically significant throughout the period. This finding is likely to be driven by the quotas on transformation rates of apprentices to open-ended contracts mandated by the CBAs. As described in Section 2.b, many CBAs require firms to offer an open-ended position to a quota of their incumbent apprentices before being able to hire any additional apprentices. We show in Section 7.b that the increase in the number of transformations is not driven by firms reducing the duration of apprentices contracts.

After having examined the impact of the policy on apprentices' flows, we turn to the stock of apprentices and firm size. These measures summarize the impact of the policy on workers' flows and firm growth. Figure 6 reports the estimated impact of the policy on the stock of apprentices. Unlike workers' flows, we measure the stock of apprentices *pro rata temporis* and in full-time equivalent units. The stock of apprentices reflects both the number of months in a year each apprentice is employed at a given firm and whether the employee works full-time or part-time.[9] The coefficients in the years leading up to the reform are not statistically different from zero and the pattern looks rather flat, supporting our identifying assumption. The stock of apprentices initially decreases in response to the reduction in SSCs. This moderate decline is consistent with the increase in the number of newly hired apprentices not being large enough to compensate for the increase in transformations and separations (Figure 5). Moreover, if the transformation of apprenticeships to open-ended contracts occurs earlier in the year than hiring, this can cause a temporary decrease in the

[9]For example, if an apprentice works full-time from March to December, they count as 0.75 full-time equivalent units. Similarly, if they work part-time from January to December, they count as 0.5 full-time equivalent units. One implication is that, while the month when a worker is hired or let go does not impact the analysis of the worker flows presented in Figure 5, it will be reflected in the analysis of the stock of apprentices discussed in Figure 6.

stock of apprentices measured *pro rata temporis*. The stock of apprentices in small firms relative to medium firms starts picking up in 2009 and increases steadily throughout our sample period. Both coefficients in 2010 and 2011 are positive and statistically significant. By the end of our sample period, the policy increases the stock of apprentices by 0.042 (a 12% increase relative to the average stock of apprentices in the sample).

We conclude this section by discussing the impact of the reduction in SSCs on firm size. Appendix Figure 7 reports the results. Firm size increases steadily between 2007 and 2011 in small firms relative to medium firms. The increase in firm size ($0.04 \times 9.41 = 0.4$) is roughly equivalent to the increase in the stock of apprentices, suggesting that firms do not hire apprentices at the expense of other contract types.

The results presented in this section show that a *temporary* reduction in payroll taxes increases the employment of young apprentices. This result is especially important in light of previous studies showing that wage subsidies are ineffective when they target disadvantaged groups (Neumark, 2013) and training programs have a hard time boosting employment of the disadvantaged youth (Stanley et al., 1998). It is important to stress, however, that our identification strategy allows us to estimate the impact of the policy at small relative to medium firms. Therefore, the increase in employment we document may not reflect a *net* increase in employment in the economy.

In the next section, we estimate the impact of the policy on complier firms and discuss the magnitude of our estimates relative to previous studies.

6.b Instrumental Variable Results

Our intention-to-treat estimates identify a lower bound on the impact of the policy because not all firms hire apprentices and some medium-size firms receive the reduction in SSCs despite being ineligible. This section addresses these issues and examines the impact of the policy on complier firms using an instrumental variable strategy.

We begin our analysis on workers' flows. Each figure displays the point estimates and the associated 95% confidence intervals for the λ_k's coefficients from equation (2). Figure 8a and 8b report the estimated impact of the policy on apprentice hires and transformations, respectively. Consistent with our reduced-form results, we find that the impact of the policy on new hires is very modest in 2007, increases in 2008 and 2009, and stays relatively constant afterwards. Over five years, the average complier firm hires 2.86 additional apprentices. On a yearly basis, this effect size represents a twofold increase in hiring relative to the number

of apprentices hires in the average firm in our sample.[10]

Since apprenticeships are designed to be a stepping stone toward a permanent contract, we examine how many of these contracts are transformed into open-ended ones. The impact of this policy ranges between 0.08 and 0.17 additional transformations per year. Over five years, the average complier firm retains 0.52 additional apprentices as permanent employees. On a yearly basis, this effect size is approximately as large as the average number of transformations in our sample.[11]

We conclude this section by studying the impact of the policy on the stock of apprentices. Figure 8c reports the results. Similarly to the reduced form pattern, the stock of apprentices decreases slightly in 2007 and 2008 and then rises steadily in all the following years. At the end of our sample period the policy complier firms employ 0.7 additional apprentices on average. Over five years, complier firms increase their stock of apprentices by 1.17 full-time equivalent employees. On a yearly basis, this effect size is approximately 60% as large as the average number of apprentices in our sample.[12]

Overall, the reduction in SSCs brought about by the 2007 Budget Bill had a considerable impact on hiring, transformations, and stock of apprentices in complier firms. In Section 9, we develop this analysis further and estimate the effectiveness of this policy in terms of cost per job created.

Although it is challenging to directly compare point estimates across designs and countries, we benchmark the magnitude of these effects with those in earlier studies. Several studies found that reductions in payroll taxes have *no impact* on employment, though firms operate in a setting where they can adjust wages (Bohm and Lind, 1993; Bennmarker et al., 2009; Egebark and Kaunitz, 2013; Korkeamaki and Uusitalo, 2006; Huttunen et al., 2013). Our results show that a reduction in SSCs has little to no impact on earnings and substantially *raises* employment. In line with economic theory, our findings suggest that the ability of firms to adjust workers' wages is an important determinant of the effectiveness of payroll tax cuts.

[10] On an annual basis, compliers firm hire on average 0.57 (2.86/5) additional apprentices; this figure is almost twice as large as the average number of apprentice hires in our sample (0.31 – column 1 of Table 3).

[11] On an annual basis, compliers firm transform on average 0.10 (0.52/5) additional apprentices; this figure is roughly the same magnitude as the average number of transformations in our sample (0.09 – column 1 of Table 3).

[12] On an annual basis, compliers firm employ on average 0.23 (1.17/5) additional apprentices; this figure is roughly 60% as the average number of apprentices in our sample (0.41 – column 1 of Table 3).

7 Unintended Firm Responses

In this section, we examine whether the reduction in payroll taxes generated any unintended responses in terms of firm-size distortions or opportunistic behavior.

7.a Firm-Size Distortions

Previous studies documented that policies that rely on firm size cut-offs often generate costly firm size distortions (Caicedo et al., 2020; Garicano et al., 2016). When only small firms qualify for a policy, firms may have an incentive to limit their growth to maintain eligibility. In the setting we study, firms that expect to employ apprentices may choose not to grow past 9 full-time equivalent employees to remain eligible for the reduction in SSCs.

Figure 9 examines whether firms limit their growth by plotting the distribution of firm size by year. Consistent with the fact that at the time there was no discontinuous policy at 9 employees, Figures 9a and 9b show that the distribution of firm size is smooth at the threshold in 2005 and 2006. Figures 9c through 9f show that the distribution is equally smooth between 2007 and 2011. The lack of bunching at the threshold is likely to be attributable to the fact that the SSCs discount may be too modest to induce firms to manipulate their size.

7.b Opportunistic Firm Responses

A concern that is often brought up in relation to apprenticeship contracts is that, although this is a training contract, some firms may hire apprentices as a mere source of cheap labor and fail to provide an adequate level of training (Tiraboschi, 2014). By making apprenticeship contracts cheaper, the policy may exacerbate this problem.

We can not investigate this issue directly, as we do not have a measure of training. However, we can evaluate several margins that are indicative of firms engaging in opportunistic behavior and employing apprentices for the only purpose of reducing costs. These margins are: the "re-labeling" of existing contracts, the reduction in the duration of apprenticeship contracts, the strategic separations from more expensive apprentices, the limited transformations to open-ended contracts, and changes in the quality of new hires.

Contract re-labeling: In response to the reform firms may "re-label" existing contracts as apprenticeships to take advantage of the lower SSCs. To evaluate this, we construct the number of newly hired apprentices including and excluding those who were previously employed at the same firm. Figure 10 reports the estimated impact of the policy on these two outcomes (red and blue circles, respectively). Reassuringly, the point estimates are virtually

identical, implying that the vast majority of new hires are hired from outside the firm. We do not find evidence of firms re-labeling existing contracts in response to the reform.

Contract duration: Because the reduction in SSCs lasts for only two years, firms may have an incentive to churn through more apprentices with shorter contract duration. We evaluate whether this is the case by constructing the average duration of apprenticeship contracts signed in year t.[13] Figure 11 reports the results and shows that the policy did not impact contract duration.[14] We conclude that we find no evidence that firms strategically shift toward shorter contracts in a response to the policy.

Strategic separations: If apprentices were perfectly substitutable, eligible firms may have an incentive to substitute incumbents apprentices with cheaper new hires. Figure 5 shows that the policy does not impact separations in the first two years. Rather, the impact on separations tracks the impact on hiring with a two-year lag, in line with the fact that the average apprenticeship lasts 20 months. We do not find evidence that the increase in hiring is driven by strategic separations from incumbent apprentices.

Transformation into open-ended contracts: If firms hired apprentices as a source of cheap labor and planned to let them go at the end of their contract, we would expect the policy to have no impact on transformations. However, Figure 5 documents an immediate and persistent increase in the number of transformations to open-ended contracts. This result pushes against the interpretation that firms hire apprentices as a mere source of cheap labor and implies that the policy generated some long-lasting firm-worker matches.

Apprentices' selection: Finally, if firms hire apprentices with no intention of keeping them for long, they may choose to invest less effort in searching for talent and compromise on the quality of new hires. We test whether the quality of new hires changes as a response to the policy along two dimensions: previous salary and previous experience of newly hired apprentices. Figure 12 reports the point estimates and the associated 95% confidence intervals. We find no evidence that firms hire a different type of apprentices along these two dimensions (albeit the point estimates for the previous wage are negative).

We conclude that, while we cannot evaluate whether the amount of training that firms provide changes in response to the reform, we find no indication of that firms hire apprentices to merely reduce costs.

[13] An explicit formula can be found in Appendix C, together with the definition of the other variables used in the analysis.

[14] While this regression fits naturally in the framework we developed so far, the dependent variable is defined only for firms that hire apprentices in a given year. This analysis thus exploits the variation in contract duration for firms that hire apprentices both before and after the reform.

8 Robustness Checks

In this section, we address some concerns relative to our empirical strategy. We show that our results are not confounded by differences in baseline firm characteristics and industry-specific trends, differences in size between small and medium firms, and exposure to the Great Recession or to liquidity constraints. We also show that our results are robust to alternative levels of clustering of standard errors.

8.a Firm-Level Controls

Small and medium firms may differ in characteristics that predict changes in apprenticeship use. We address this concern by controlling for a rich set of baseline firm covariates ($\mathbf{x}_i'$) interacted with year fixed effects. We estimate the following model:

$$y_{it} = \alpha_i + \tau_t + \sum_{k \neq 2006} \gamma_k \cdot \mathbf{1}(\text{year}_t = k) \cdot T_i + \sum_{k \neq 2006} \mathbf{x}_i' \boldsymbol{\delta}_k \cdot \mathbf{1}(\text{year}_t = k) + \epsilon_{it}. \tag{3}$$

where the set of controls $\mathbf{x}_i'$ includes the share of workers 29 years old or younger, the share of workers between 30-49 years old, the share of workers 50 years old or older, the share of female workers, the share of apprentices, the share of blue-collar workers, the share of white-collar workers, the share of managers, the labor share, liquid assets over total assets, investment over assets, and cash flow over total assets. All controls are measured at baseline. We estimate this model on the matched-Cerved sample (98,084 firms).[15] Figure D.2 compares our baseline estimates (blue circles) with those obtained controlling for this rich set of covariates (red circles). Our findings are robust to the inclusion of this rich set of controls.

We also present another set of robustness checks where we control for sector- and region-linear time trends. Our specification is:

$$y_{it} = \alpha_i + \tau_t + \sum_{k \neq 2006} \eta_k \cdot \mathbf{1}(\text{year}_t = k) \cdot T_i + \sum_{k \neq 2006} \mathbf{s}_i' \boldsymbol{\zeta}_k \cdot t + \epsilon_{it}. \tag{4}$$

where $\mathbf{s}_i'$ includes either (i) two-digit-sector linear trends, or (ii) region linear trends, or (iii) two-digit-sector $\times$ region trends.[16] Appendix Figure D.3 compares our baseline estimates

[15] The baseline results are robust to restricting the sample to the matched-Cerved sample. Results are available upon request.

[16] We use *linear* trends instead of non-parametric ones purely for computational convenience due to the high

(blue circles) with those obtained controlling for regional trends (red diamonds), sector trends (green triangles), and regional and sector trends (yellow crosses). Our point estimates are not sensitive to the inclusion of the linear time trends.

8.b Firm Size

Although our identification strategy does not require the characteristics of small and medium firms to be balanced at baseline, one may be concerned that medium firms may not be a good counterfactual for small firms because they are larger than small firms by construction. To address this concern, we restrict our sample to firms that had between 8 and 9 employees in 2006 (restricted sample). By design, small and medium firms in the restricted sample exhibit much smaller differences in baseline firm size than those in the full sample.[17]

Appendix Figure D.4 reports the point estimates and associated 95% confidence interval for the β_k from equation (1) estimated on full sample (blue circles) and on the restricted sample (red circles). The point estimates are exceptionally stable across samples and their difference is not statistically significant. The number of hires is the only notable exception; in this case, the point estimates are slightly smaller in magnitude in the restricted sample than in the main sample. Yet, the confidence intervals overlap for the vast majority of point estimates. We conclude that we find no evidence that differences in sizes act as a confounding factor in our setting.

8.c The Great Recession and Liquidity Constraints

The Great Recession may potentially have a differential impact on firms above and below the 9-employee threshold. To address this concern, we evaluate whether our estimated treatment effects are heterogeneous across labor markets exposed more or less severely to the Great Recession. If the crisis was a confounder, we would expect our estimates to be heterogeneous across labor markets that were hit more or less severely. To test this, we run the following specification

$$y_{it} = \alpha_i + \tau_t + \sum_{k \neq 2006} \beta_k^L \mathbf{1}(\text{year}_t = k) \cdot T_i \cdot \left(1 - H_{i(l)}\right) + \sum_{k \neq 2006} \beta_k^H \mathbf{1}(\text{year}_t = k) \cdot T_i \cdot H_{i(l)} + u_{it}. \quad (5)$$

number of fixed effects.

[17] The ideal strategy would involve using a regression discontinuity design to evaluate the impact of the reform at the threshold. As we discussed in Section 5, the absence of a sharp discontinuity in the take-up rate documented in Figure 2 prevents us from pursuing this strategy.

All the variables are defined as in equation (1) and $H_{l(i)}$ is an indicator that takes value one if the local labor market l experienced an above-median change in the unemployment rate between 2007 and 2010.[18]

Appendix Figure D.5 reports the estimated impact of the policy for firms located in local labor markets with above-median (red circles) and below-median (blue circles) exposure to the Great Recession. This analysis shows that the estimated impacts of the reform are not heterogeneous across local labor markets that have been affected more or less severely by the Great Recession.

While the unemployment rate is our preferred proxy for the Great Recession, we repeat this analysis using other proxies of financial constraints at the firm level. Previous research showed that payroll tax cuts help firm by relaxing the credit constraints (Benzarti and Harju, 2021a; Saez et al., 2019). Benzarti and Harju (2021a) finds that lower payroll taxes benefit credit constrained firms during recessions but not during an economic boom. We follow Saez et al. (2019) and use three proxies for financial constrains: (i) liquid assets over total assets, (ii) cash-flow over total assets, and (iii) total revenues. We estimate model 5 substituting $H_{i(l)}$ with a dummy for whether the proxy for firm i is above the median of the distribution of the proxy at baseline. Appendix Figures D.6,D.7, and D.8 report the results. Unlike previous studies, we do not find evidence that a reduction in payroll taxes benefited liquidity constrained firms more than other businesses either during a recession or during booms. We conclude that we do not find evidence that the Great Recession or liquidity constraints act as confounders in this setting. Moreover, our results push against the interpretation that relaxing credit constraints is a quantitatively meaningful mechanism in the setting we study.

8.d Clustering

Finally, we evaluate whether our results are robust to alternative clustering levels. Appendix Figure D.9 compares the standard errors clustered at the firm level (blue circles) with those clustered at the two-digit sector (red circles) and those clustered at the local labor market level (yellow triangles). Our results are not sensitive to the level of clustering.

9 Cost per job created

In this section, we evaluate the cost-effectiveness of the policy. We first estimate the cost-per-job created. Then, we perform a back-of-the-envelope calculation to estimate the number of

[18] We use the 2001 definition of local labor market by the Italian National Institute of Statistics (Istat).

jobs generated by this policy.

Cost-per-job created: The policy is more cost-effective if the employment effects are relatively large; it is less cost-effective if the tax cut primarily subsidize inframarginal workers. Because the reform changes the payroll taxes for apprenticeship contracts, we are primarily interested in understanding how much it costed the government to generate each additional apprenticeship contract. However, apprenticeship contracts are designed to be the stepping stone toward an open-ended position. Therefore, although this policy does not directly incentivize the use of permanent contracts, it is also important to estimate the cost per open-ended job.

We begin by computing the gross cost of the reform as the sum of forgone payroll taxes. This is a gross costs because it does not take into account the savings from unemployment benefits and welfare transfers that would have been paid in the absence of the policy and the taxes that workers and employers pay on the jobs generated by this policy. Let M_{it} be the gross cost of foregone payroll taxes for firm i in year t relative to a setting where payroll taxes increased for all firms equally.

$$M_{it} = \begin{cases} 0, & \text{if take-up}_{it} = 0 \quad (6) \\ 0.085 \cdot W_{it}^1 + 0.07 \cdot W_{it}^2, & \text{if take-up}_{it} = 1, \quad (7) \end{cases}$$

where W_{it}^j is the wage bill for apprentices with tenure j employed by firm i in year t (measured in thousands €). The foregone payroll taxes amount to 8.5% and 7% of apprentices' earnings in the first and second year of tenure, respectively.[19] For simplicity, we assume that the labor choices are static and that firms do not optimize over the future stream of reductions in payroll taxes. We deem this assumption reasonable for small cash-constrained firms. We relax this assumption in Appendix Figure D.10 and we show that our results are largely uncharged if we construct M_{it} as the net present value of such transfers.[20]

To estimate the cost-per-job created, we estimate the following model

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=2007}^{2011} \theta_k \cdot M_{it} \cdot \mathbf{1}(\text{year} = k) + \nu_{it}, \quad (8)$$

where y_{it} can be one of three outcomes: the number of apprentice hires, the number of

[19] Our rich administrative data includes monthly information on the amount of SSCs paid on each contract.

[20] This alternative measure assumes a discount factor of zero. Therefore, to compute the net present value, we sum all present and future transfer transfers.

transformations, or the stock of apprentices. Because firms can choose whether to take up the policy, we instrument M_{it} with T_i. θ_k identifies the number of jobs created in year k per €1,000. Therefore, we compute the cost-per-job-created as

$$\text{Cost per job} = \frac{€1,000}{\frac{1}{5}\sum_{k=2007}^{2011}\theta_k}. \tag{9}$$

Figure 13 reports the coefficients and the 95% confidence intervals from (8). Based on these estimates, it costs approximately €2,000 to create an apprenticeship contract, €5,000 to generate a full-time apprentice position for a year, and €11,500 to create an open-ended position. These estimates are an upper bound on the *net* cost-per-job created because they do not take into account the revenues that the government collects on these jobs and the savings from the unemployment benefits and welfare transfers that would have been paid out in the absence of the policy.

Although it is challenging to compare estimates across countries, we benchmark our results with the cost-per-job created in previous studies on disadvantaged workers. Our estimated cost for a full-time apprentice position for a year is somewhat smaller but in the same order of magnitude as the one in Cahuc et al. (2019) who find that it costs €8,400 to generate a low-wage job for a year. Out point estimates compare very favorably to those in other studies. Neumark (2013) argues that it costs between $37,500 and $75,000 to generate a job for a disadvantaged worker in the US and Egebark and Kaunitz (2013) find that it costs between $150,000 and $240,000 to generate a job for a young worker in Sweden. Relative to previous studies, the payroll tax cuts on apprenticeship contracts introduced by the 2007 Budget Bill are highly cost-effective. We speculate that the cost-effectiveness is driven by three factors. First, the incentive is not automatically applied to take up is incomplete, eliminating the possibility that muted employment effects reflect the low salience of the policy. Second, wage floors imposed by the CBAs limit the ability of firms to pocket the subsidy by offsetting changes to workers' wages. Third, the reduction in SSCs is temporary and gets phased out over time. This is especially beneficial if the demand for apprentices is more elastic in the first year than in later years due to the greater uncertainty about workers' ability and lower firm-specific training. If this is the case, then the reduction in SSCs provides a larger subsidy for apprenticeship positions when the demand for these jobs is more elastic.

Based on our estimates, it is cost-effective to generate an apprentice position by reducing payroll taxes in the setting we study. Importantly, although the policy did not directly subsidize permanent jobs, many of the apprenticeship jobs generated by the reform are transformed into open-ended positions. This is consistent with the reform subsidizing firms

in screening young workers and discovering talent.

Back-of-the-envelope calculation: We build on the work by Autor et al. (2013) and present a simple back-of-the-envelope calculation for the number of apprenticeship jobs generated by the payroll tax cut for the firms in our sample. In this exercise, we assume that (i) we can extrapolate our estimated treatment on the treated effects to all firms in our sample, and that (ii) differences in employment between small and medium firms reflect absolute employment changes. The latter would be violated if there were spillovers between small and medium firms or if the policy had general equilibrium effects.

We compute the number of jobs generated by the policy in year k multiplying the gross cost of the policy in that year ($\mathbf{M}_k$) by the number of apprenticeship positions generated for €1,000 spent (θ_k) in the same year year. We then sum the number of jobs created across years.

$$\Delta\text{Jobs} = \sum_{k=2007}^{2011} \theta_k \cdot \mathbf{M}_k \tag{10}$$

Table 4 reports the gross cost of the policy in each year ($\mathbf{M}_k$). We find that the reform generated approximately 54,000 apprenticeship positions. An analogous exercise for open-ended contracts yields a total of 11,281 additional open-ended positions.

A major limitation of this back-of-the-envelope calculation is that it rests on fairly strong assumptions. Moreover, these results are calculated on a sample of small and medium firms and should not be extrapolated to the whole economy.

10 Conclusions

Although payroll tax cuts have been used to raise the employment of disadvantaged workers, there is no consensus on whether they are effective.

This paper studies a targeted payroll tax cut in a setting where firms have limited ability to adjust workers' wages. The 2007 Budget Bill provided a temporary discount on SSCs on apprenticeship contracts for small firms relative to medium ones in Italy. We exploit this reform in a difference-in-differences framework where we compare the outcomes of small firms (between 5 and 9 employees) with those of medium firms (between 10 and 14 employees).

Critics of this policy argue that the impact of payroll tax cuts on employment may be limited

in a setting where firms have to raise wages following a cut in their labor costs. We do not find evidence that firms adjust workers' wages in response to the reform. We attribute this to the presence of wage rigidity imposed by the CBAs.

Unlike most previous studies (Bennmarker et al., 2009; Katz, 1998; Egebark and Kaunitz, 2013), we find that the reduction in SSCs raised the employment of the targeted group and that a non-negligible fraction of the apprenticeship contracts generated by the reform are ultimately converted into open-ended positions. Over five years, each complier firm hired almost three additional apprentices and retained 0.52 of them as full-time workers. These effects are sizable in light of the fact that complier firms employ less than 9 workers at baseline. We do not find evidence that this policy generated costly firm-size distortions or opportunistic responses.

To evaluate the cost-effectiveness of the policy, we estimate the gross cost per job created. It costs approximately €2,000 to the Italian government to generate an apprentice position and €11,500 to generate an open-ended contract. These figures are an upper bound for the *net* cost per job created as they do not incorporate the savings from the unemployment benefits and the welfare transfers that would have been paid in the absence of the reform as well as the government revenues on the jobs generated by the reform. Our estimates are lower than those in previous studies on disadvantaged workers (Cahuc et al., 2019; Neumark, 2013; Egebark and Kaunitz, 2013). We speculate that two factors may contribute to the cost-effectiveness of this policy: first, firms have limited ability to adjust workers' wages; second, the reduction in SSCs is not automatically applied to all small firms and gets phased-out over time.

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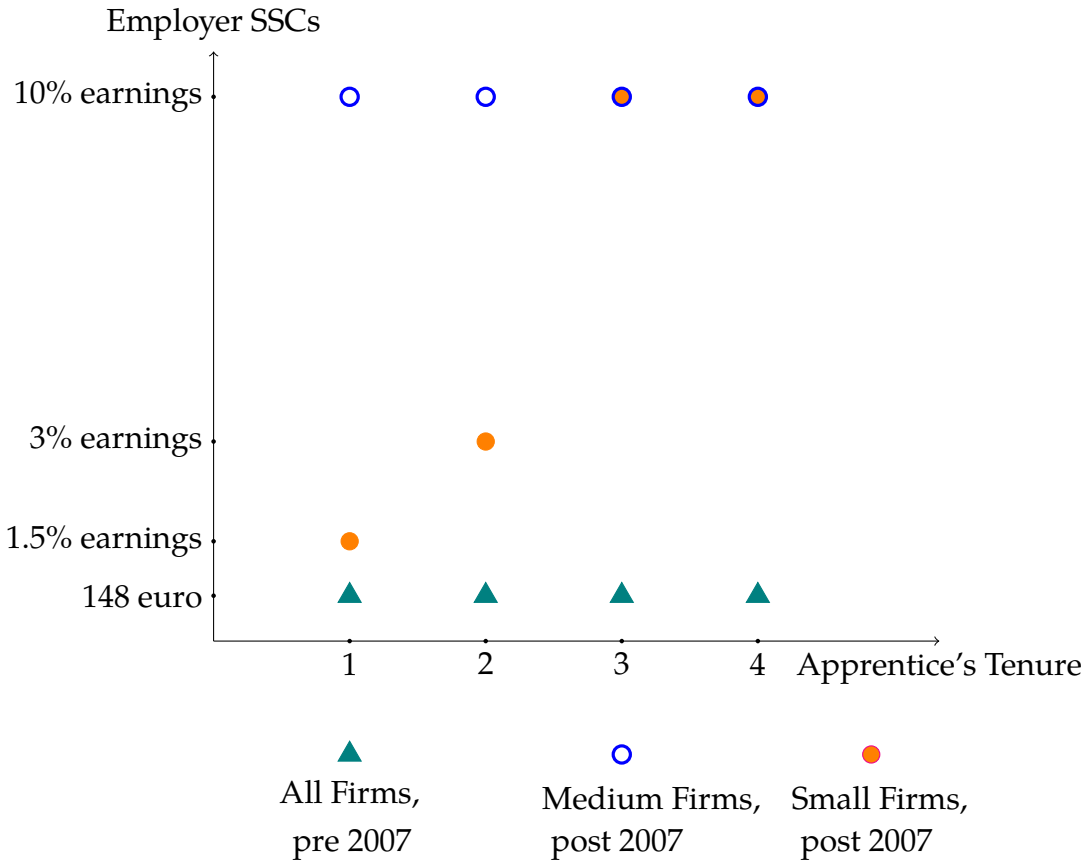
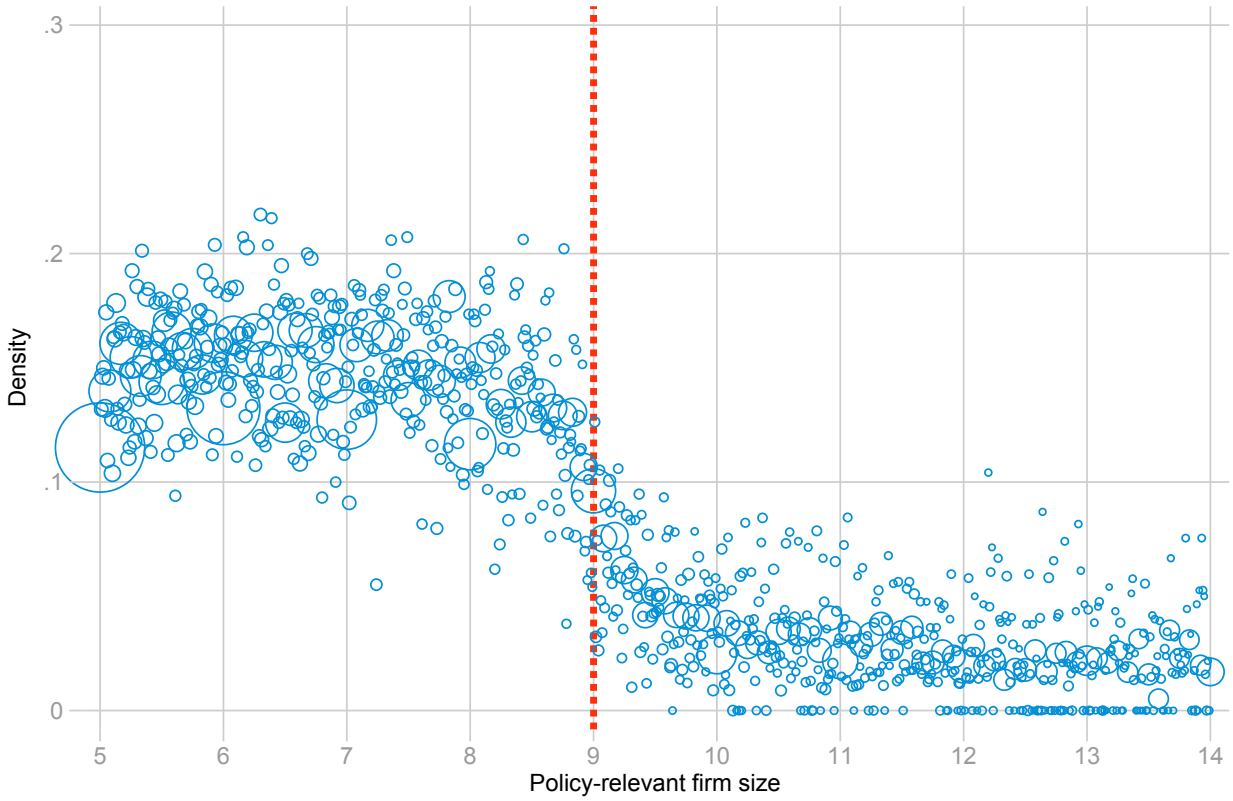
Figures

Figure 1: Employer SSC for Apprenticeship Contracts



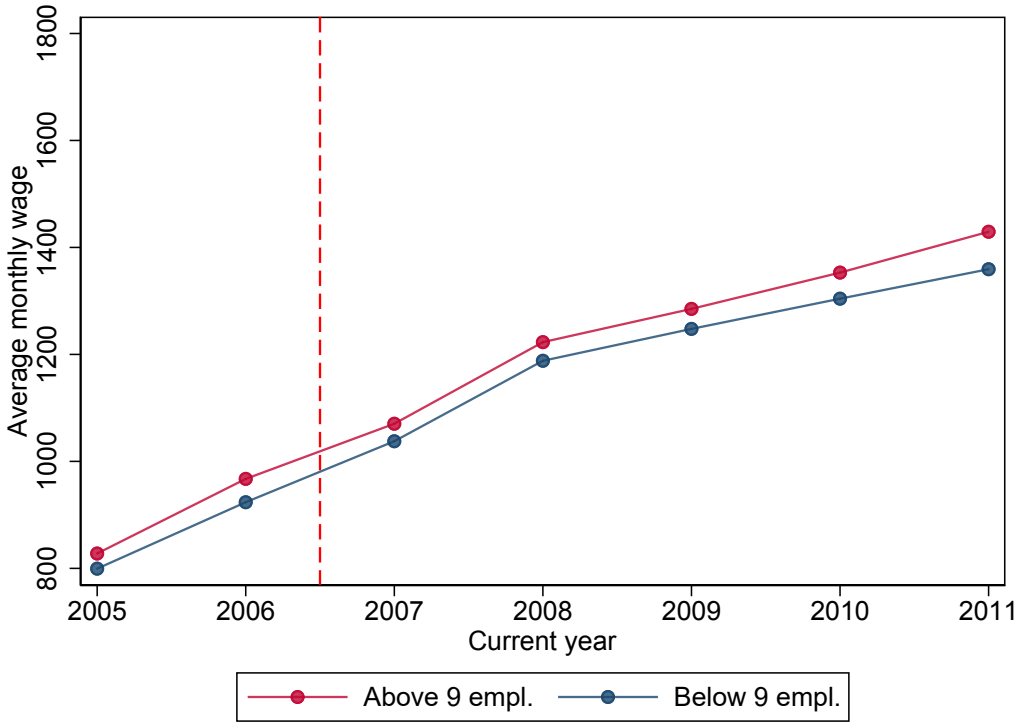
Note: This Figure illustrates how employer SSCs for apprenticeship contracts changed in response to the 2007 Budget Bill. Before 2007 all firms paid a fixed weekly fee of 2.85 euros per apprenticeship contract. This amounted to roughly €148 per year irrespective of the apprentice's tenure (green triangles). Starting from January 1, 2007 the employer SSCs changed discontinuously for firms above and below the 9-employee threshold. Under the new regime, firms with more than 9 employees ("medium" firms) were required to pay 10% of the apprentice's earnings in social contributions (hollow blue circles). Firms with at most 9 employees ("small" firms) were required to pay 1.5% of the apprentice's earnings in the first year of the contract, 3% in the second year, and 10% in all the following years (orange circles)

Figure 2: Take-up rates by firm size



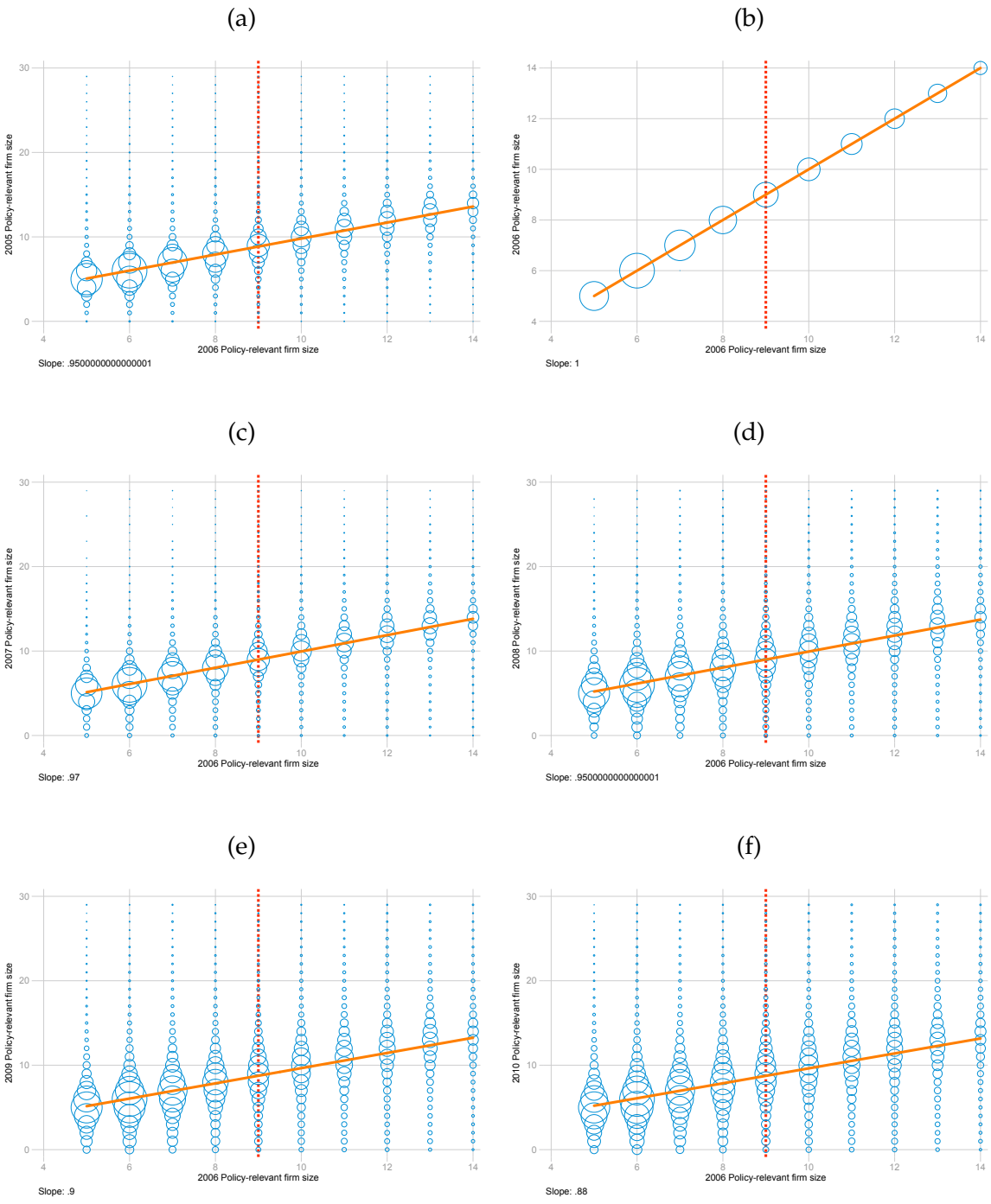
Note: Full sample, INPS data. This figure reports the take-up rate of the SSC discount in January 2007 by small bins of 2006 average firm size. The size of the marker represents the number of firms in each bin. The red dashed line indicates the 9-employee threshold.

Figure 3: Real net monthly earnings at small and medium firms



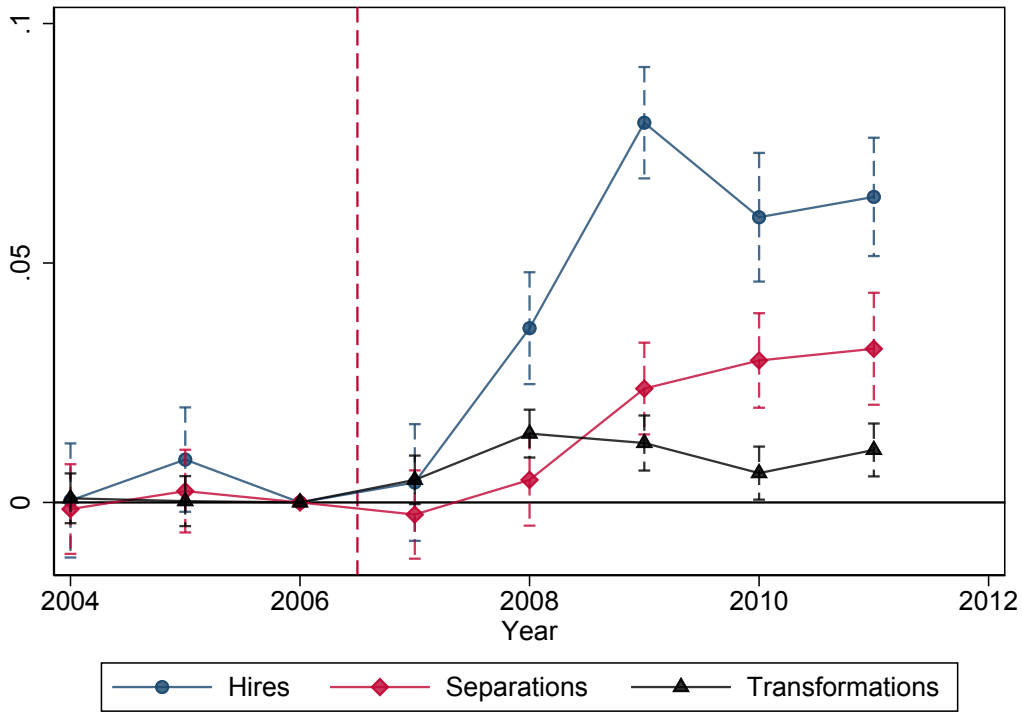
Note: Full sample, INPS data. This figure reports the evolution of real average net monthly earnings for apprentices at small (red circles) and medium firms (blue circles), respectively. Firms are classified on their 2006 average firm size.

Figure 4: Predicting firm size using baseline firm size



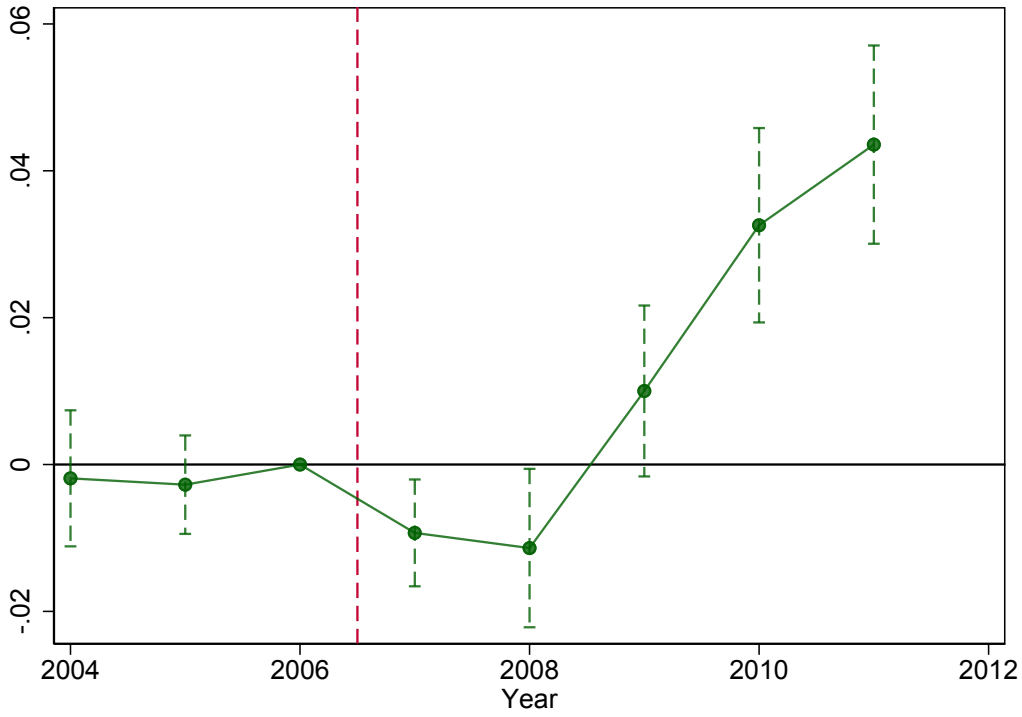
Note: Full sample, INPS data. This figure reports the scatter plot of firm size in each year and 2006 firm size (baseline). The size of the marker represents the number of firms in each cell. Each panel also reports the slope coefficient from a regression of firm size in each year and the 2006 firm size (orange solid line). The slope is equal to 1 in 2006 by construction.

Figure 5: ITT estimates of the reform on apprentice flows



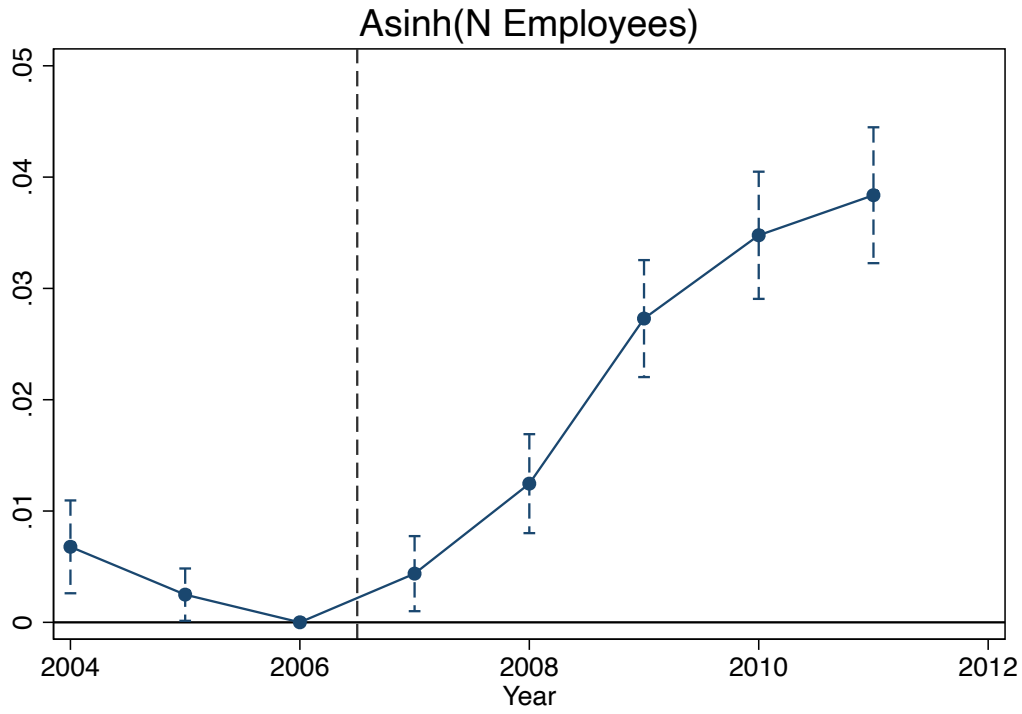
Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). The dependent variables are number of apprenticeship hires (blue circles), number of apprenticeship separations (red diamonds), and number of transformations of apprenticeship contracts to open ended contracts (black triangles). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 6: ITT estimates of the reform on the stock of apprenticeships



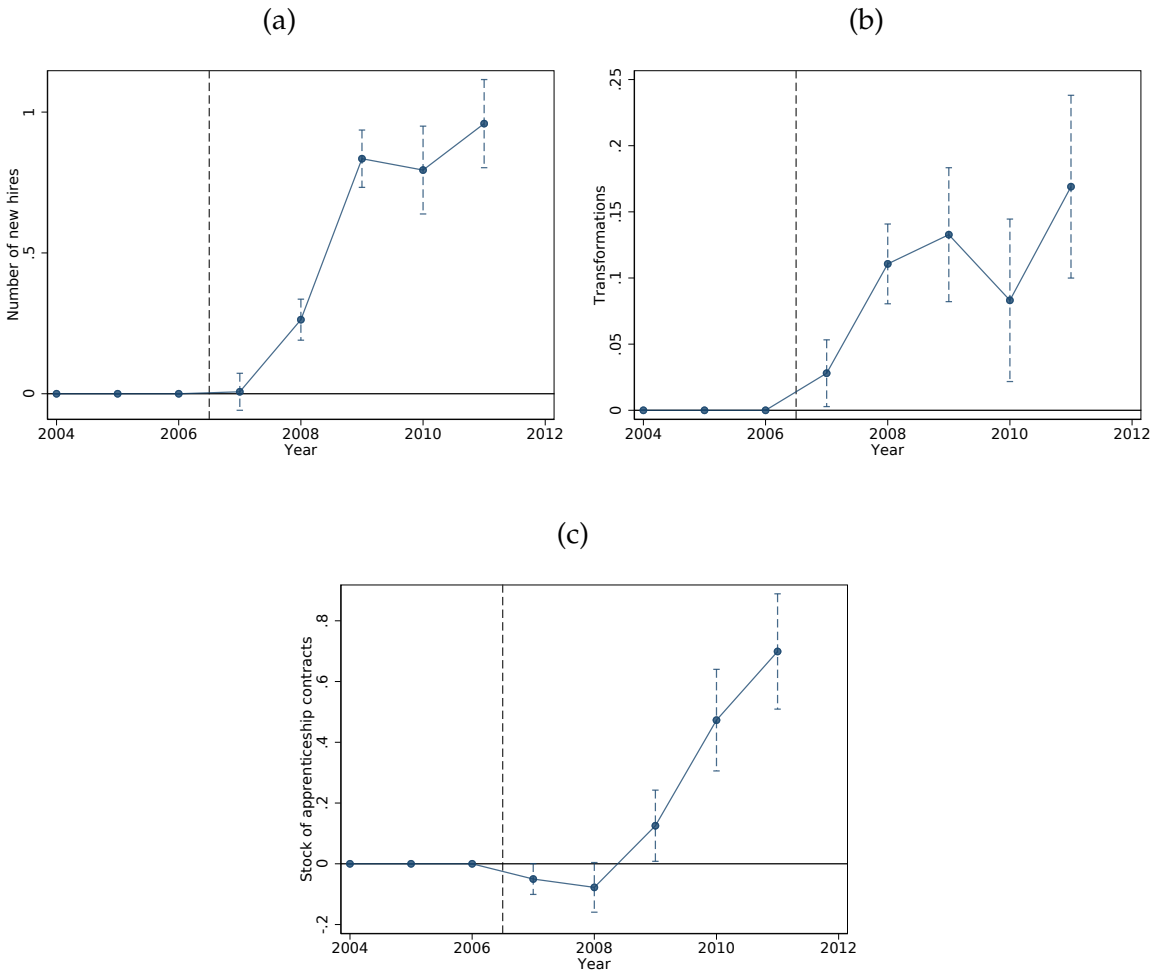
Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). The dependent variable is the stock of apprentices measured *pro rata temporis* and in full-time equivalent terms. Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 7: ITT estimates of the reform on firm size



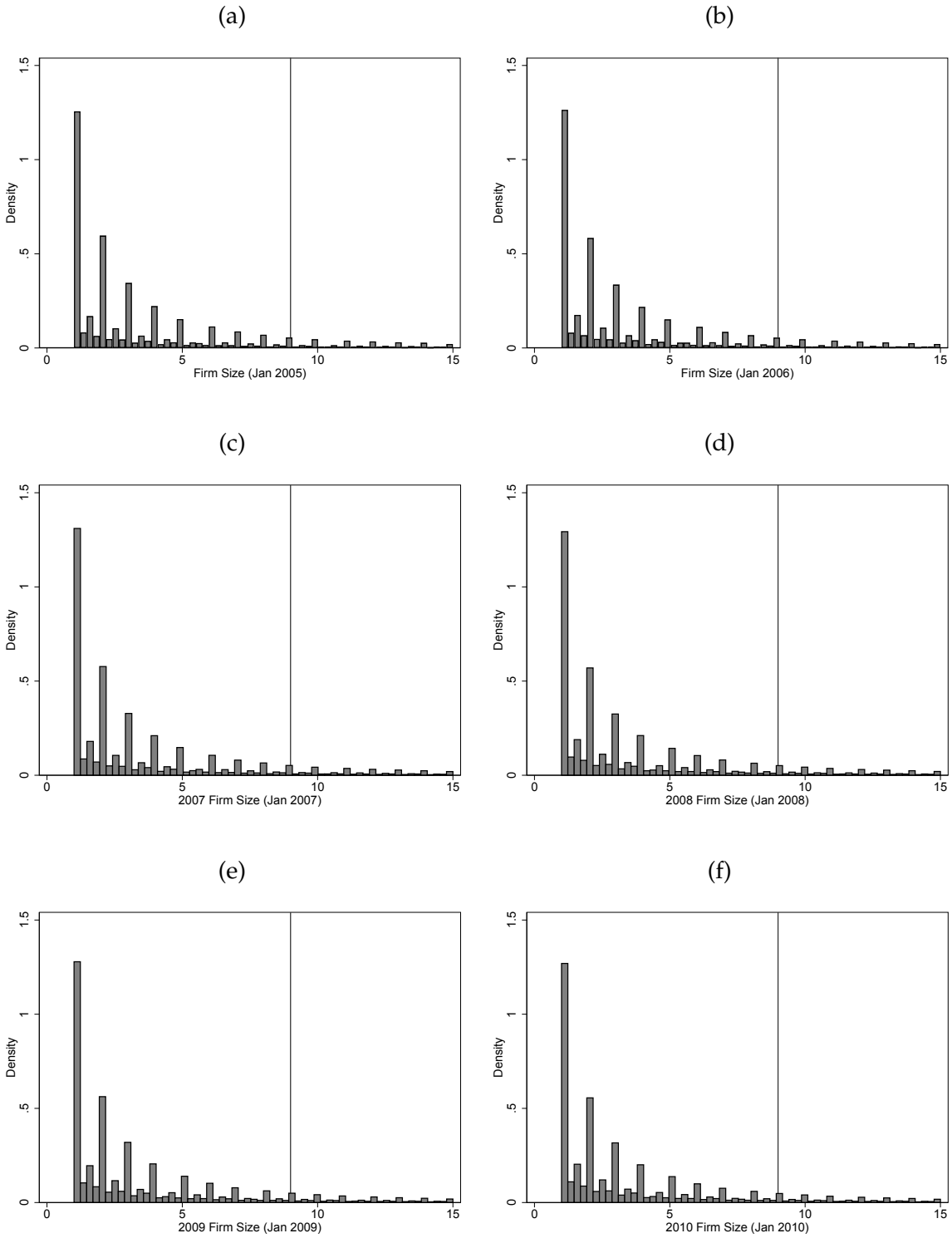
Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). The dependent variable is the inverse hyperbolic sine of firm size. Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 8: IV estimates of the reform



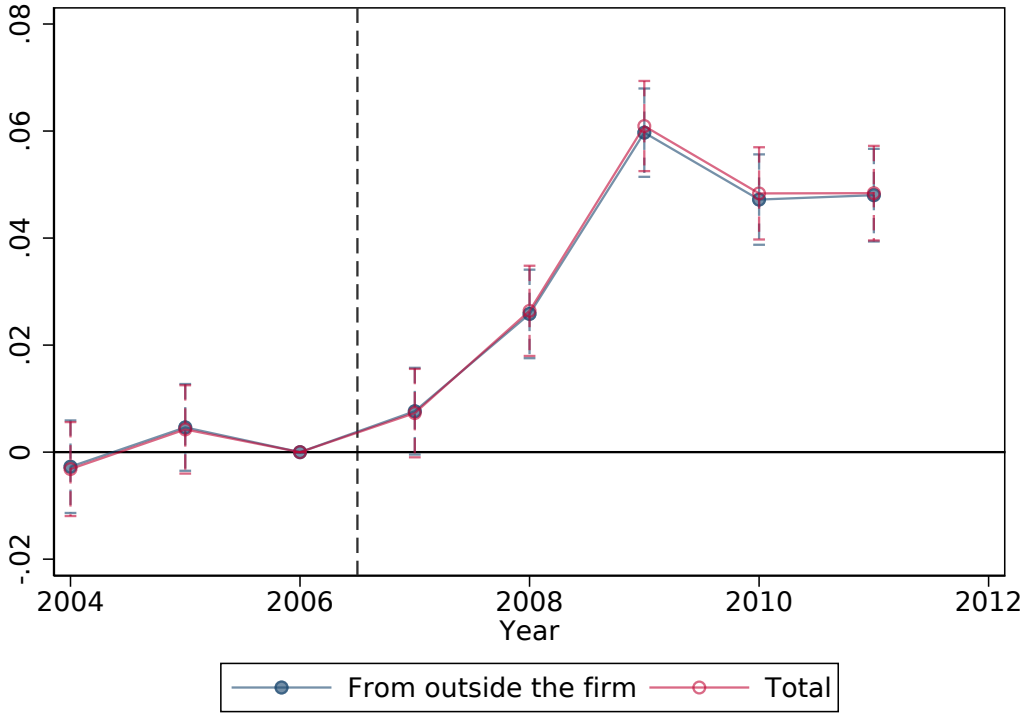
Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the the impact of the reform on complier firms relative to the pre-period (2004-2006), i.e., the λ_k from equation (2). The dependent variables are the number of new apprentices hires (panel a), the number of transformations into open-ended contracts (panel b), and the stock of apprentices (panel c). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 9: Distribution of Firm Size by Year



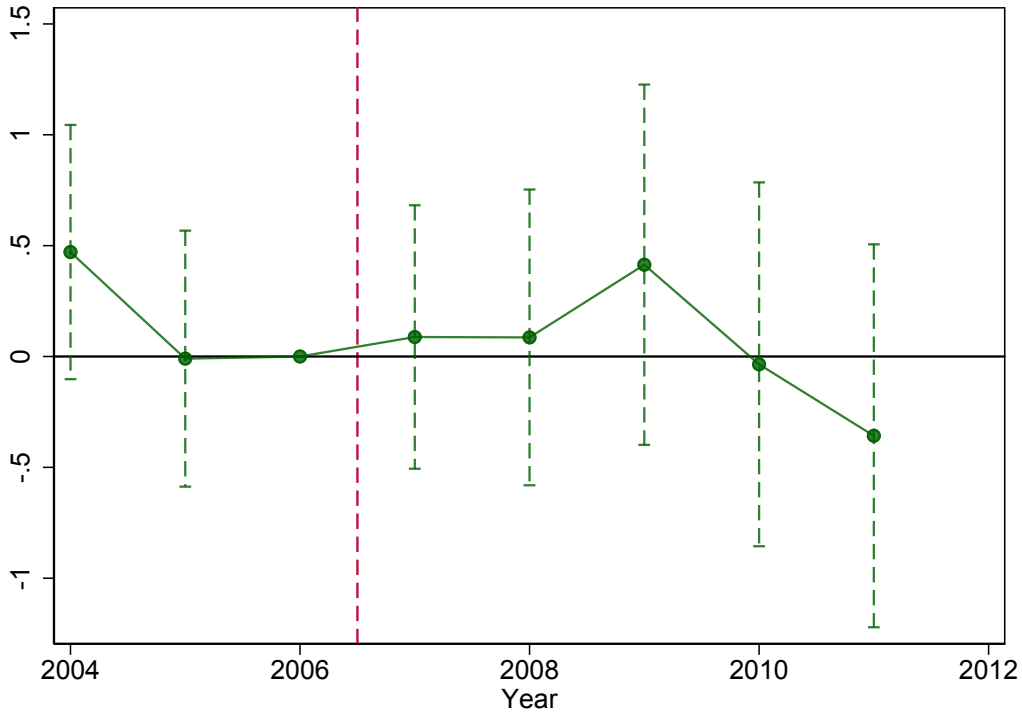
Note: This figure reports the distribution of firm size by year. The vertical line indicates the 9-employee threshold. Panel (a) through (f) report the distribution from 2005 to 2010, respectively.

Figure 10: ITT estimates of the reform on contract re-labeling



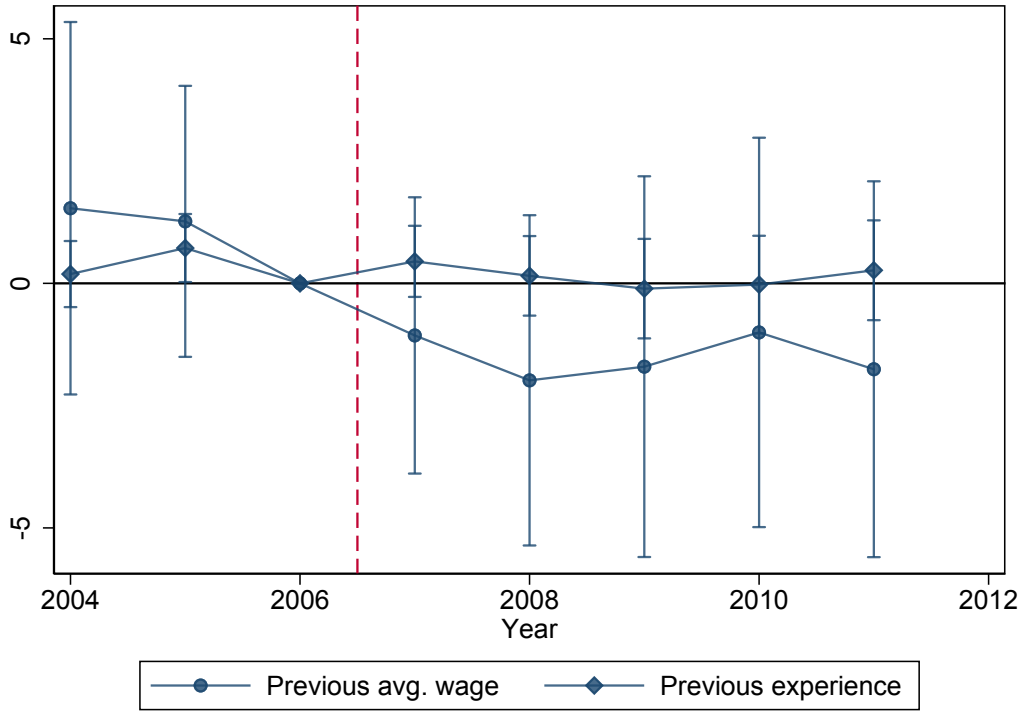
Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). The dependent variables are the number of new apprentice hires including (red circles) and excluding those who were previously employed at the same firm (blue circles), respectively. Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 11: ITT estimates of the reform on apprenticeship contract duration



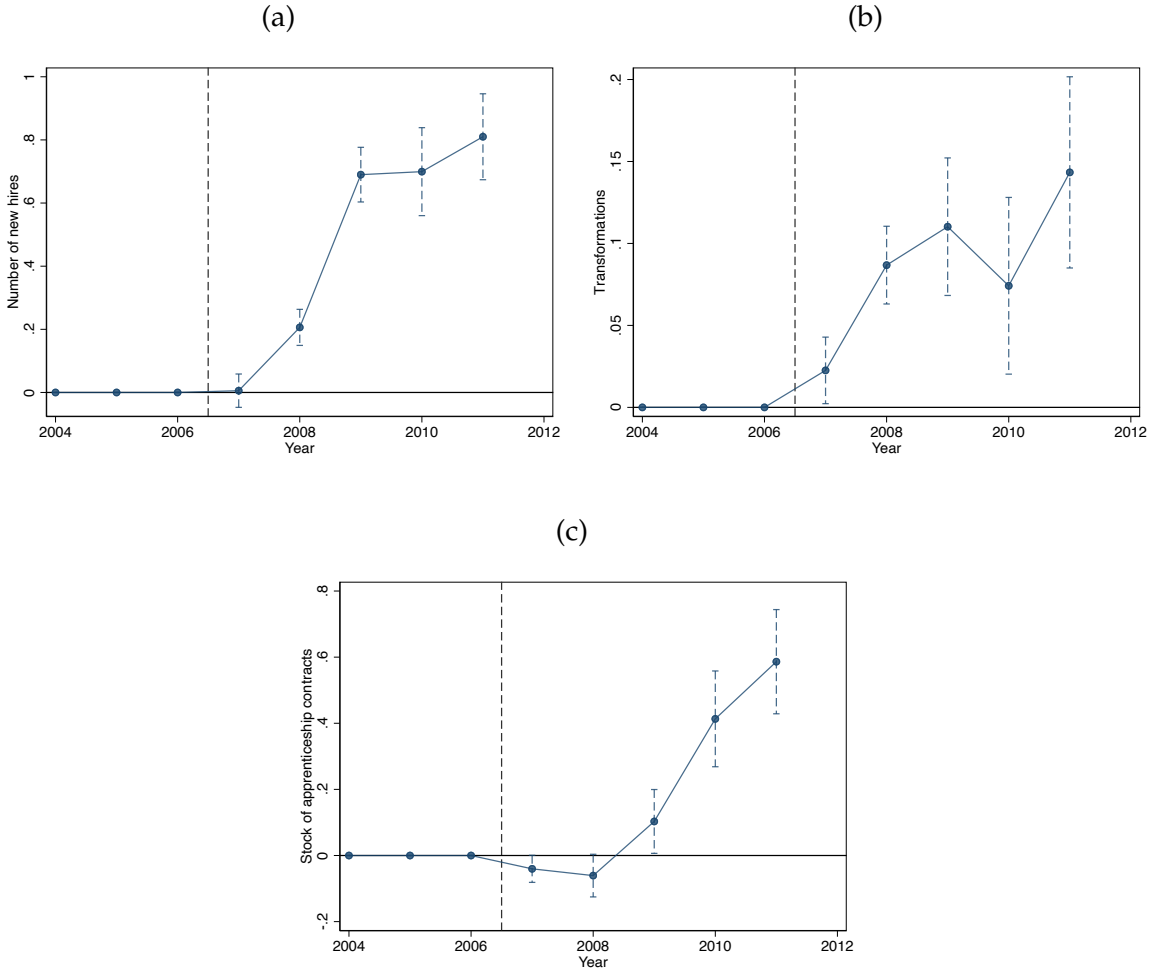
Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). The dependent variable is the average duration (in months) of apprentice contracts signed in year t. Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 12: ITT estimates of the reform on the characteristics of new hires



Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). The two dependent variables are the previous average experience (in months) and the previous average weekly wage (in €) of apprentices hired in year t (blue diamonds). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure 13: Jobs created per €1,000 spent



Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the the impact of €1000 reduction in SSCs on complier firms relative to the pre-period (2004-2006), i.e., the θ_k from equation (8). The dependent variables are the number of new apprentices hires (panel a), the number of transformations into open-ended contracts (panel b), and the stock of apprentices (panel c). M_{it} is constructed as sum of concurrent foregone payroll taxes. Standard errors are clustered at the firm level. The x-axis indexes time.

11 Tables

Table 1: Employer SSC for the Average Apprenticeship Contract

	(1)	(2)	(3)	(4)	(5)	(6)
Apprentice's	Before Jan 1, 2007		After Jan 1, 2007		$\Delta_{After-Before}$	
Tenure	Medium	Small	Medium	Small	Medium	Small
1	148	148	1200	180	1052	32
2	148	148	1200	360	1052	212
3	148	148	1200	1200	1052	1052

Note: This Table illustrates how the employer SSCs for apprenticeship contracts changed in response to the 2007 Budget Bill. In this stylized numerical example, we compute the change in SSCs for the average apprenticeship contract at baseline (yearly earnings: €12,000). Columns 1 and 2 report the SSCs under the old regime for medium and small firms, respectively. We call "small firms" those with at most 9 employees, and "medium firms" those with more than 9 employees. Before 2007 all firms paid a fixed weekly fee of 2.85 euros per apprenticeship contract. This amounted to roughly €148 per year irrespective of the apprentice's tenure. Columns 3 and 4 report the SSCs under the new regime for small and medium firms, respectively. Starting from January 1, 2007 medium firms paid 10% of the apprentice's earnings in social contributions. Small firms paid 1.5% of the apprentice's earnings in the first year of the contract, 3% in the second year, and 10% in all the following years (column 4). Columns 5 and 6 report the change in the employer SSCs between the new and the old regime for small and medium firms, respectively.

Table 2: Firm Characteristics at Baseline (2006)

	(1) Full Sample	(2) Small Firms	(3) Medium Firms
Panel A: Worker Composition and Firm Age			
Share of Workers Aged 0-29	0.262	0.269	0.25
Share of Workers Aged 30-49	0.598	0.593	0.607
Share of Workers Aged 50-100	0.14	0.139	0.143
Share Female	0.353	0.358	0.342
Firm age	14.876	14.566	15.483
Panel B: Industry Composition			
Agric., Hunt, and Forestry	0.005	0.005	0.004
Fishery	0.002	0.001	0.002
Extraction	0.005	0.004	0.006
Manufacturing	0.345	0.318	0.398
Energy	0.001	0.001	0.001
Construction	0.163	0.171	0.148
Commerce	0.194	0.203	0.178
Hospitality	0.072	0.078	0.06
Transport and Comm.	0.053	0.053	0.053
Finance	0.01	0.01	0.008
Real Eastate and Other Prof. Activ.	0.092	0.096	0.084
Other	0.059	0.059	0.058
Panel C: Geography			
Abruzzo	0.02	0.02	0.021
Basilicata	0.007	0.007	0.006
Calabria	0.019	0.02	0.017
Campania	0.067	0.068	0.065
Emilia Romagna	0.09	0.089	0.092
Friuli Venezia Giulia	0.023	0.023	0.023
Lazio	0.076	0.076	0.076
Liguria	0.024	0.025	0.024
Lombardy	0.217	0.213	0.224
Marche	0.032	0.032	0.033
Molise	0.004	0.004	0.004
Piedmont	0.073	0.073	0.074
Apulia	0.047	0.048	0.044
Sardinia	0.024	0.025	0.022
Sicily	0.05	0.052	0.046
Tuscany	0.076	0.076	0.077
Trentino Alto Adige	0.024	0.025	0.023
Umbria	0.016	0.016	0.124
Valle d'Aosta	0.002	0.003	0.002
Veneto	0.108	0.107	0.111
Observations	193296	127857	65439

Notes: Full sample, INPS data. The table reports the summary statistics for firm characteristics at baseline (i.e., 2006). The statistics are computed over the full sample of firms in column 1, and over the samples of small and medium firms in columns 2 and 3, respectively. The full sample includes incumbent firms with the average 2006 firm size between 5 (included) and 14 (included). Small firms include all incumbent firms with size between 5 (included) and 9 (included), while medium firms include all incumbent firms with size between 9 (excluded) and 14 (included). The number of observations for Panel B is 193018, 127641, and 65377, respectively.

Table 3: Firm Outcomes (2004-2011)

	(1) Full Sample	(2) Small Firms	(3) Medium Firms
Avg. N Full-time eq. employees	9.03	7.41	12.16
Avg. N Employees	9.41	7.75	12.61
N Apprentices	0.41	0.36	0.49
N Transformations	0.09	0.08	0.12
N Separations	0.34	0.31	0.40
N Hires	0.31	0.28	0.36
Duration	20.13	19.89	20.55
N	1,422,023	937,184	484,839

Notes: Full sample, INPS data. The table reports the summary statistics for firm outcomes. All statistics are calculated across firm-year observations (2004-2011). The statistics are computed over the full sample of firms in column 1, and over the samples of small and medium firms in columns 2 and 3, respectively. The full sample includes incumbent firms with the average 2006 firm size between 5 (included) and 14 (included). Small firms include all incumbent firms with size between 5 (included) and 9 (included), while medium firms include all incumbent firms with size between 9 (excluded) and 14 (included).

Table 4: Gross cost of the policy

	Gross Cost (€ mln)
2007	37,411
2008	35,060
2009	26,014
2010	20,831
2011	19,974
Total	139,290

Notes: Full sample, INPS data. This table reports the gross cost of the policy calculated over the firms in our sample.

Appendix

Appendix A Appenticeship Contracts

The Italian apprenticeship system is composed of three distinct types of contracts: "apprenticeship for job qualification" (*apprendistato professionalizzante*), "apprenticeship aimed at acquiring a 3-4 year vocational qualification" (*apprendistato per l'espletamento del diritto/dovere di istruzione*) and "apprenticeship for further education and research" (*apprendistato di alta formazione e ricerca*). Apprentices can be hired in all industries and occupations in the private sector, but not in the public sector.

The "apprenticeship for job qualification" is by far the most popular contract type and, as of 2017, covers roughly 95% of all apprentices in the country (D'Arcangelo et al., 2019). The "apprenticeship aimed at acquiring a 3-4 year vocational qualification" is part of the vocational school curriculum and targets individuals aged 15-18. This allows young workers to also fulfil compulsory education obligations. Finally, the "apprenticeship for further education and research" gives the opportunity to young (18-29) individuals with some college education to carry out a specific research project at the firm premises.

Appendix B The Policy-Relevant Firm Size

The 2007 Budget Bill does not define how to compute the policy-relevant firm size and delegates this task to INPS. INPS details how to compute firm size in a provision issued in January 2007 (*circolare n. 22, 2007*). We follow this definition closely.

The firm size that determines the eligibility for the SSC discount is full-time equivalent employment and it excludes apprentices, temporary agency workers, workers who are on leave (unless the firm hires a substitute), and workers who have been hired with an on-the-job training contract. The types of job training contracts that are excluded from the computation of firm size are those created under the following provisions: exD. lgs. 251/2004, D. lgs. n.276/2003, law n. 223/1991.

Our rich administrative data contains detailed information on workers' contracts and allows us to construct a fairly accurate proxy for the policy-relevant firm size. In this context there are two sources of potential measurement error. First, INPS data does not contain a flag for the on-the-job training contracts created under the exD.lgs.251/2004. Anecdotally, this contractual arrangement is very rare and it is unlikely to generate substantial measurement error. Second, our proxy does not account for workers who are on temporary leave (e.g., sick

leave or maternity leave).

Appendix C Variables Definition

In this section, we define the variables we use in the empirical analysis and provide further details about the institutional background related to these variables.

Small firm indicator: the definition of the indicator for small firms is time invariant and is based on the average policy-relevant firm size in 2006. The assignment rule therefore is:

$$T_i = \begin{cases} 0 & \text{if } \mathbf{1}(\text{size}_{i,2006} > 9) \\ 1 & \text{if } \mathbf{1}(\text{size}_{i,2006} \leq 9) \end{cases} \tag{11}$$

This definition gives us 65,439 control firms and 127,858 treated firms.

Hires, separation and transformations: We define the variable "hires" as the number of newly established apprenticeship contracts at firm i in year t. When the contract comes to an end, the worker can either leave the firm, which we term "separation", or she may see her contract converted into an open-ended contract at the same firm, which we call "transformation".

More specifically an individual is considered hired as an apprentice in year t at a given firm A if she appears with an apprenticeship contract in such firm in year t but did not hold any apprenticeship contract at the same firm during year $t-1$. Our definition includes workers who had already had a spell at the same firm in the past, just with a different contract type, and excludes individuals who perform consecutive apprenticeships at the same firm. As shown in picture 10, the vast majority of new hires concern individuals who were not working at the same firm in the previous year, so alternative definitions are unlikely to change our results. One drawback with our hiring measure is that we cannot observe if the firm rolls apprenticeship contract over, as we do not observe the exact level the worker is training for. If a firm uses two consecutive apprenticeship contracts with the same worker, we are bound to classify this as a unique contract. An apprentice is considered to be separated from a given training firm A in year t if she was holding an apprenticeship contract in such firm in year t but does not appear in firm A in year $t+1$, with *any* contract. Transformations to open-ended contracts are not subject to measurement error because our administrative data contains a flag that identifies such an event.

Apprenticeship duration: our measure of duration is based on the number of months

between the hiring date of an apprentice, and the last month when they are observed with an apprenticeship contract at the firm.[21] Realized duration can be different from *ex-ante* contractual duration if either the apprentice decides to quit midway through the contract or the firm decides to lay the worker off. In order to aggregate duration to the firm level, we construct the following measure:

$$D_{it} = \frac{1}{N_{it}} \sum_{v=1}^{N_{it}} D_{vit} \tag{12}$$

where N_{it} is the number of apprenticeship contracts *started* by firm i in year t and D_{vit} is the duration of contract v started by firm i in year t. Notice that this measure is defined only for years when the firm starts new apprenticeship contracts.

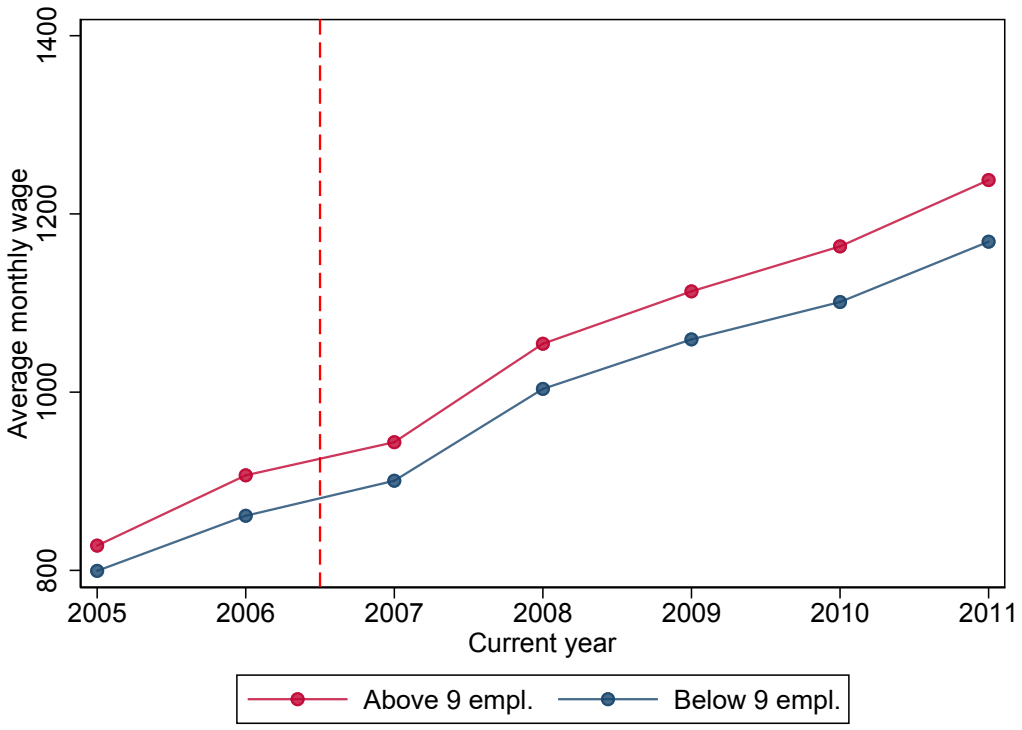
Earnings: Our data contains monthly earnings *net* of SSCs (*imponibile previdenziale*), which in Italy constitutes the basis on which such contributions are computed. Such measure includes all compensation that the firm pays the worker, but excludes all compensation that the worker receives from INPS in case there is an event leading to a temporary reduction in working hours or absence (e.g. sickness leave or maternity leave). If the employer is responsible for paying when these events occur, those amounts are included in our measure. Our earnings measure is not a contractual wage but it reflects how many hours are effectively worked during the year.

Our data also contains a flag that indicates whether the firm received the SSC discount in each month.

[21] Similarly to before, such last date requires that the worker is not observed at the same firm with any contract during the whole of year $t + 1$.

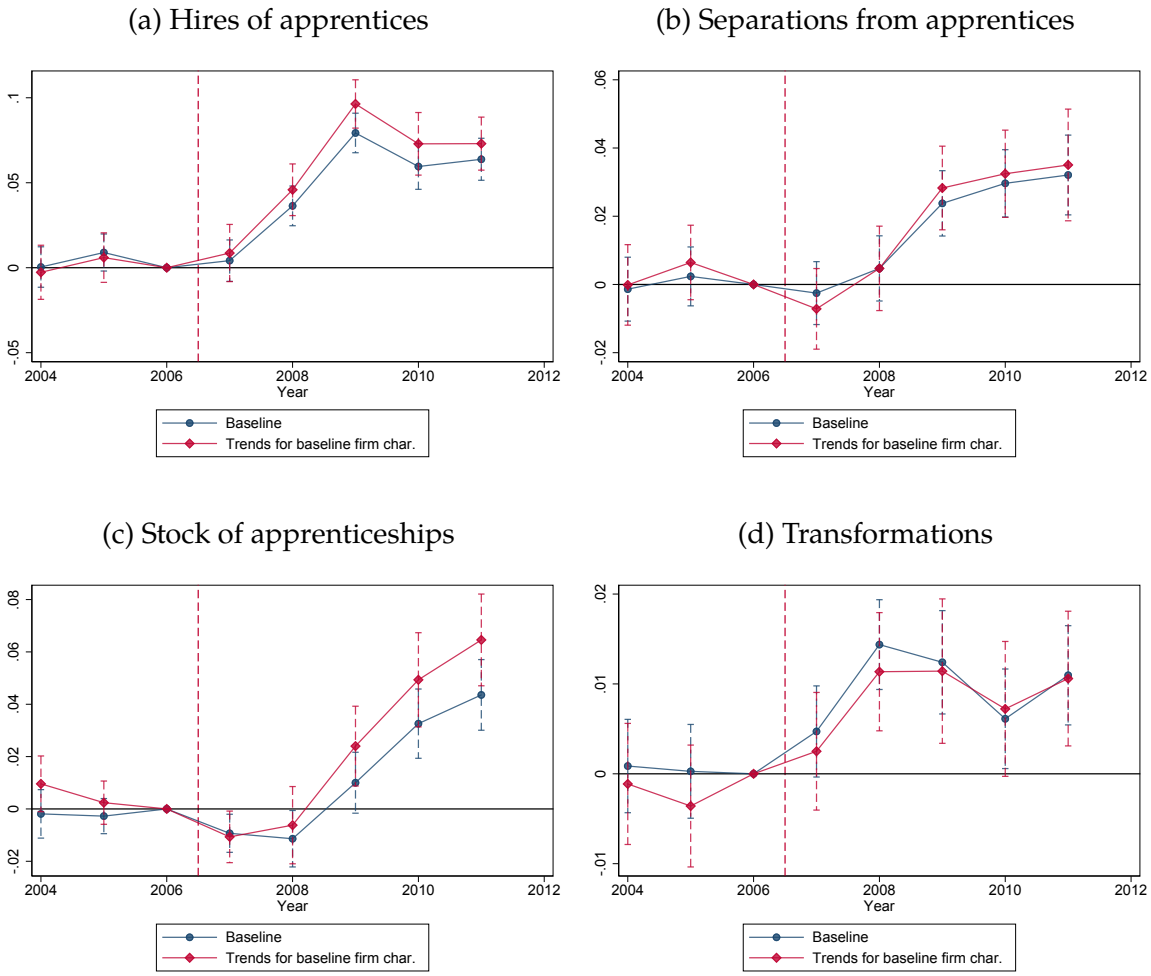
Appendix D Additional Figures

Figure D.1: Real net monthly earnings for newly hired apprentices at small and medium firms



Note: This figure reports the evolution of real average net monthly earnings for newly hired apprentices at small (red circles) and medium firms (blue circles), respectively. Firms are classified on their 2006 average firm size.

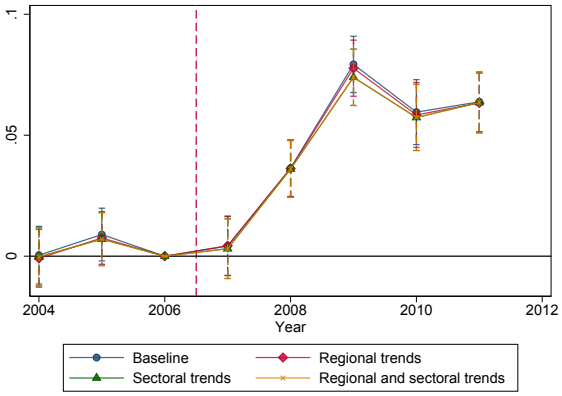
Figure D.2: Robustness: Trends in baseline firm characteristics



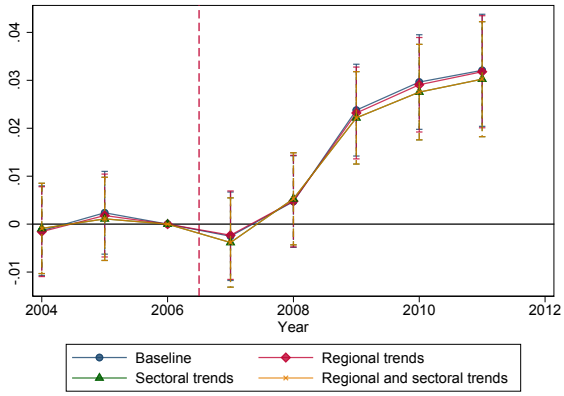
Note: Full sample and Matched-Cerved Sample, INPS and Cerved data. This figure compares our baseline estimates (blue circles), with those obtained controlling for trends in a rich set of baseline covariates (red diamonds). It reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the β_k from (1) for the baseline specification and the $\hat{\gamma}_k$ from (3) for the robustness check. The baseline estimates are obtained from the full sample, while estimates for the robustness check are obtained from the matched-Cerved sample. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure D.3: Robustness: Sector and province-specific trends

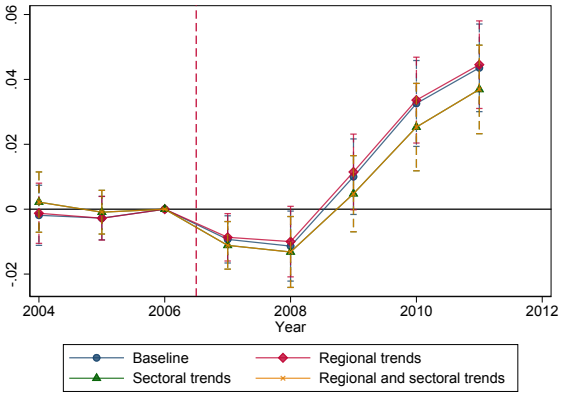
(a) Hires of apprentices



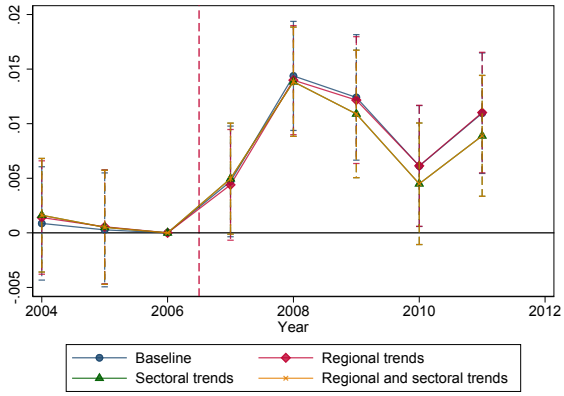
(b) Separations from apprentices



(c) Stock of apprenticeships

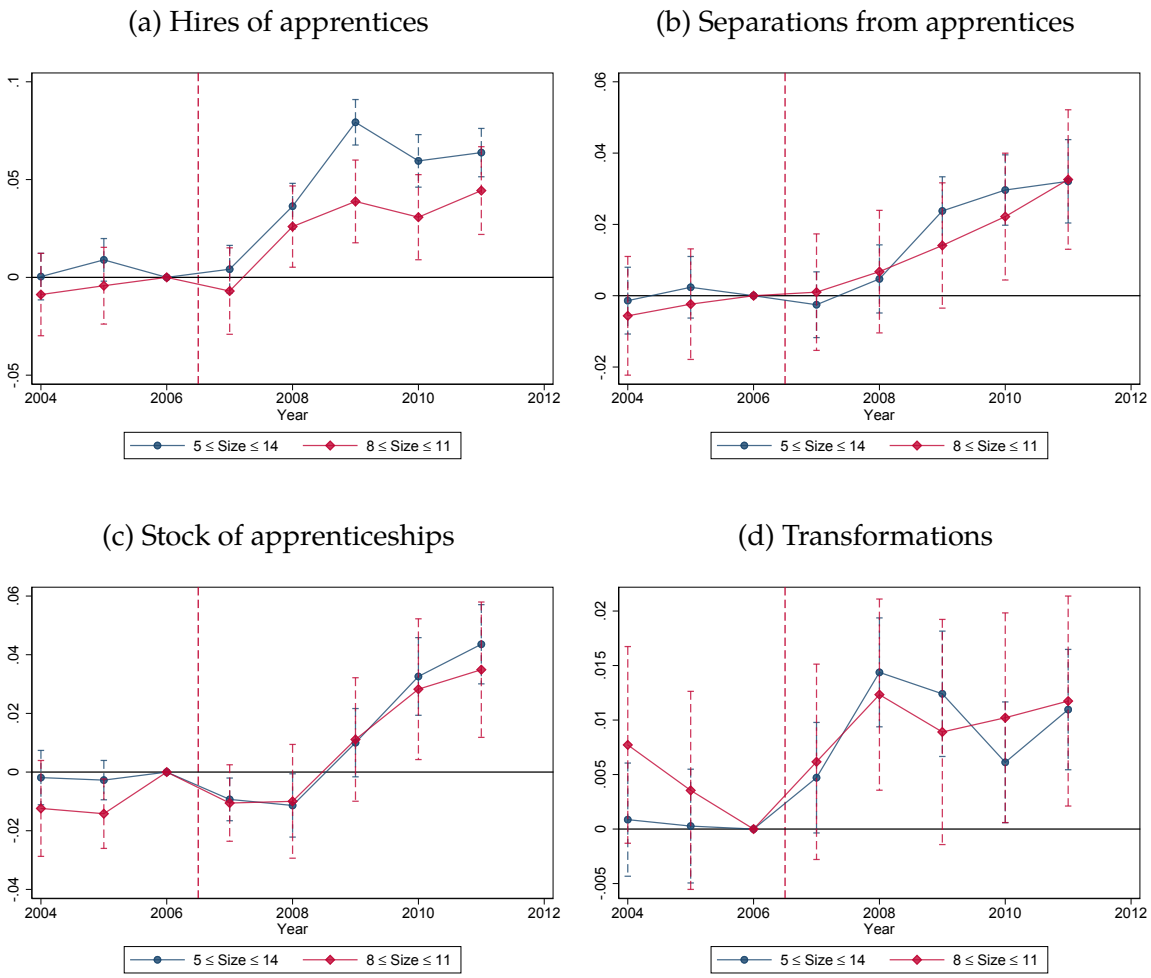


(d) Transformations



Note: Full sample, INPS data. This figure compares our baseline estimates (blue circles), with those obtained controlling for (i) two-digit sector specific linear trends (green triangles) (ii) region specific linear trends (red circles) (ii) two digit sector × region specific linear trends (yellow crosses). This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the β_k from (1) for the baseline specification and the $\hat{\eta}_k$ from (4) for the robustness checks. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

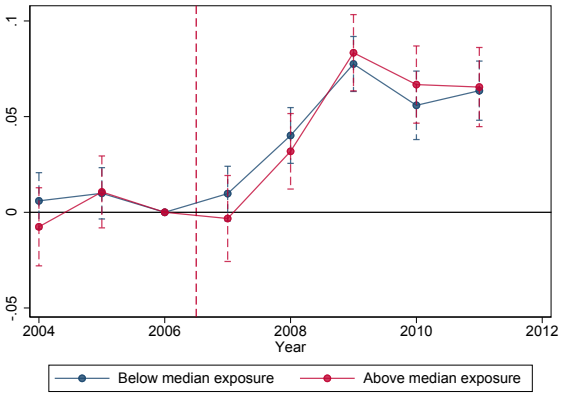
Figure D.4: Robustness: Restricted Sample



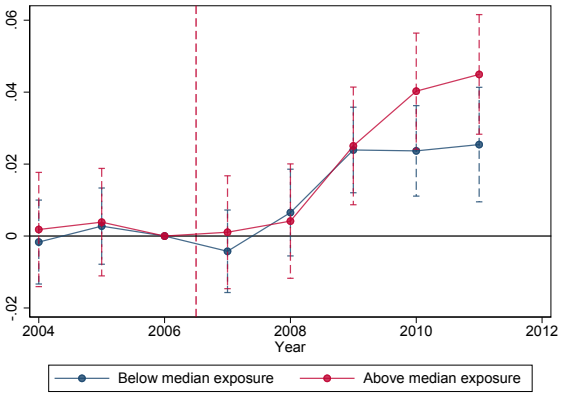
Note: Full sample and restricted sample, INPS data. This Figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006, i.e., the β_k from (1). The baseline estimates are obtained on the full sample (blue circles) and the robustness check estimates are obtained on the restricted sample (red diamonds). The full sample includes all incumbent firms with baseline firm size between 5 and 15 employees, while the restricted sample includes all incumbent firms with baseline firm size between 8 and 11. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure D.5: Robustness: The Great Recession (unemployment rate)

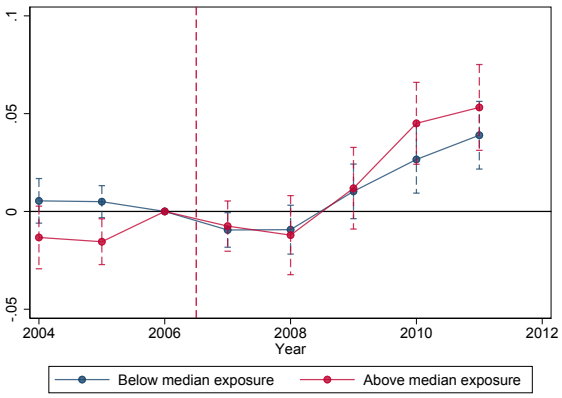
(a) Hires of apprentices



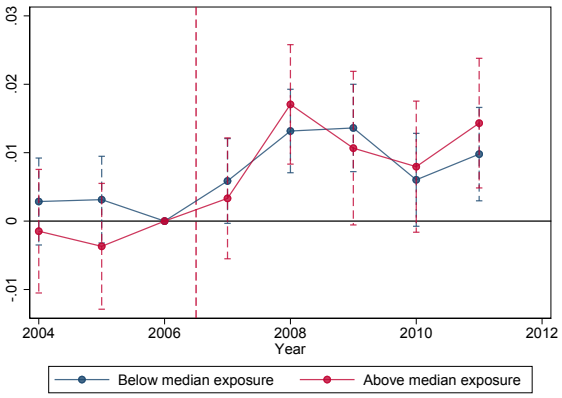
(b) Separations from apprentices



(c) Stock of apprenticeships



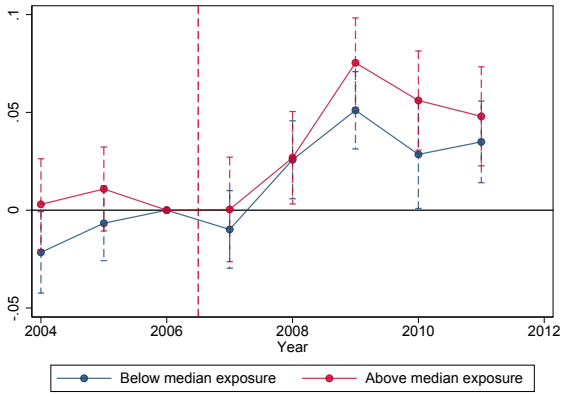
(d) Transformations



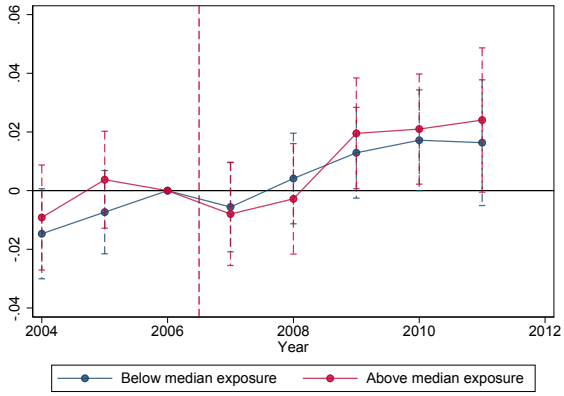
Note: Full Sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006 by exposure to the Great Recession, i.e., the $\hat{\beta}_k^L$ (blue circles) and the $\hat{\beta}_k^H$ (red circles) from equation (5). Exposure is defined as the 2007-2010 change in the unemployment rate of the local labor market where the firm operates. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure D.6: Robustness: The Great Recession (liquid assets over total assets)

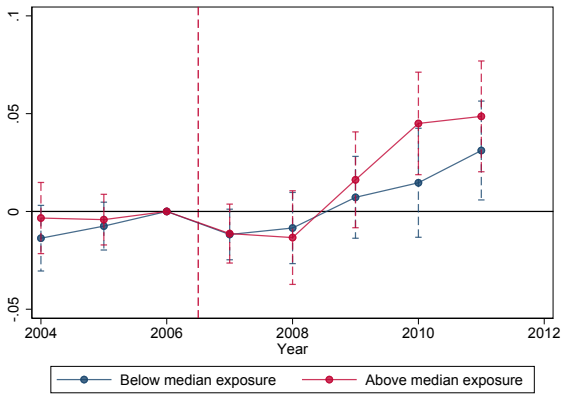
(a) Hires of apprentices



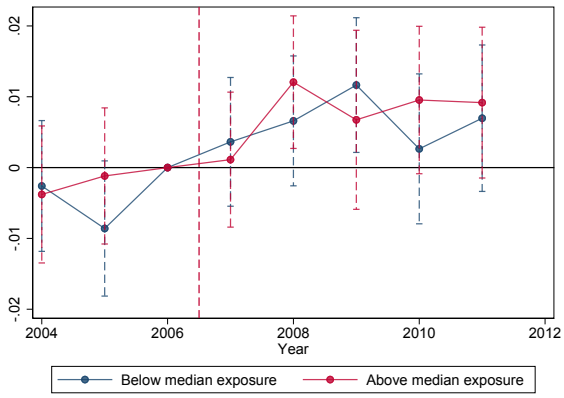
(b) Separations from apprentices



(c) Stock of apprenticeships

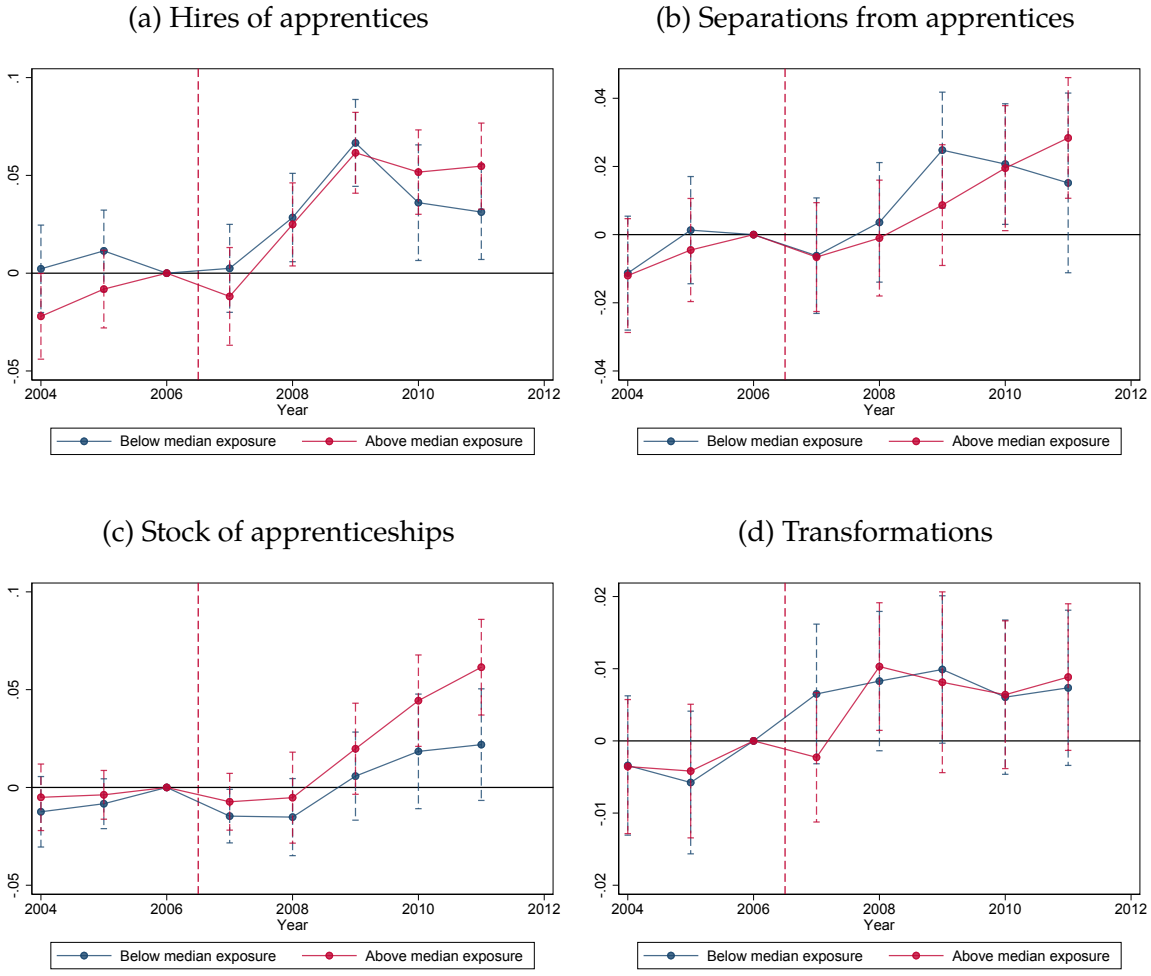


(d) Transformations



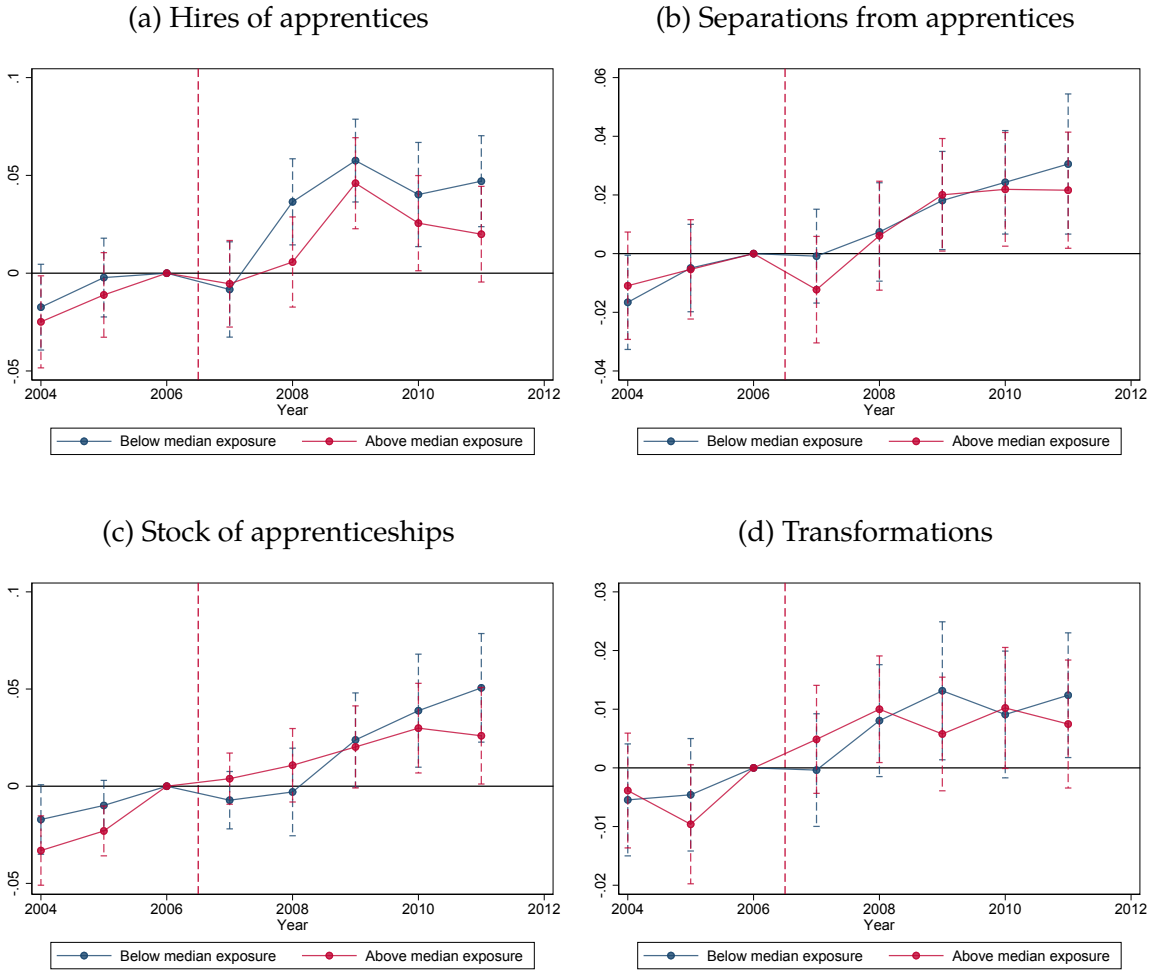
Note: Matched-Cerved sample, INPS and Cerved data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006 by exposure to the Great Recession, i.e., the $\hat{\beta}_k^L$ (blue circles) and the $\hat{\beta}_k^H$ (red circles) from equation (5). Exposure is defined as the ratio of liquid assets over total assets. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure D.7: Robustness: The Great Recession (cash flow over assets)



Note: Matched-Cerved sample, INPS and Cerved data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006 by exposure to the Great Recession, i.e., the $\hat{\beta}_k^L$ (blue circles) and the $\hat{\beta}_k^H$ (red circles) from equation (5). Exposure is defined as the ratio of cash flow over assets. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure D.8: Robustness: The Great Recession (total revenues)

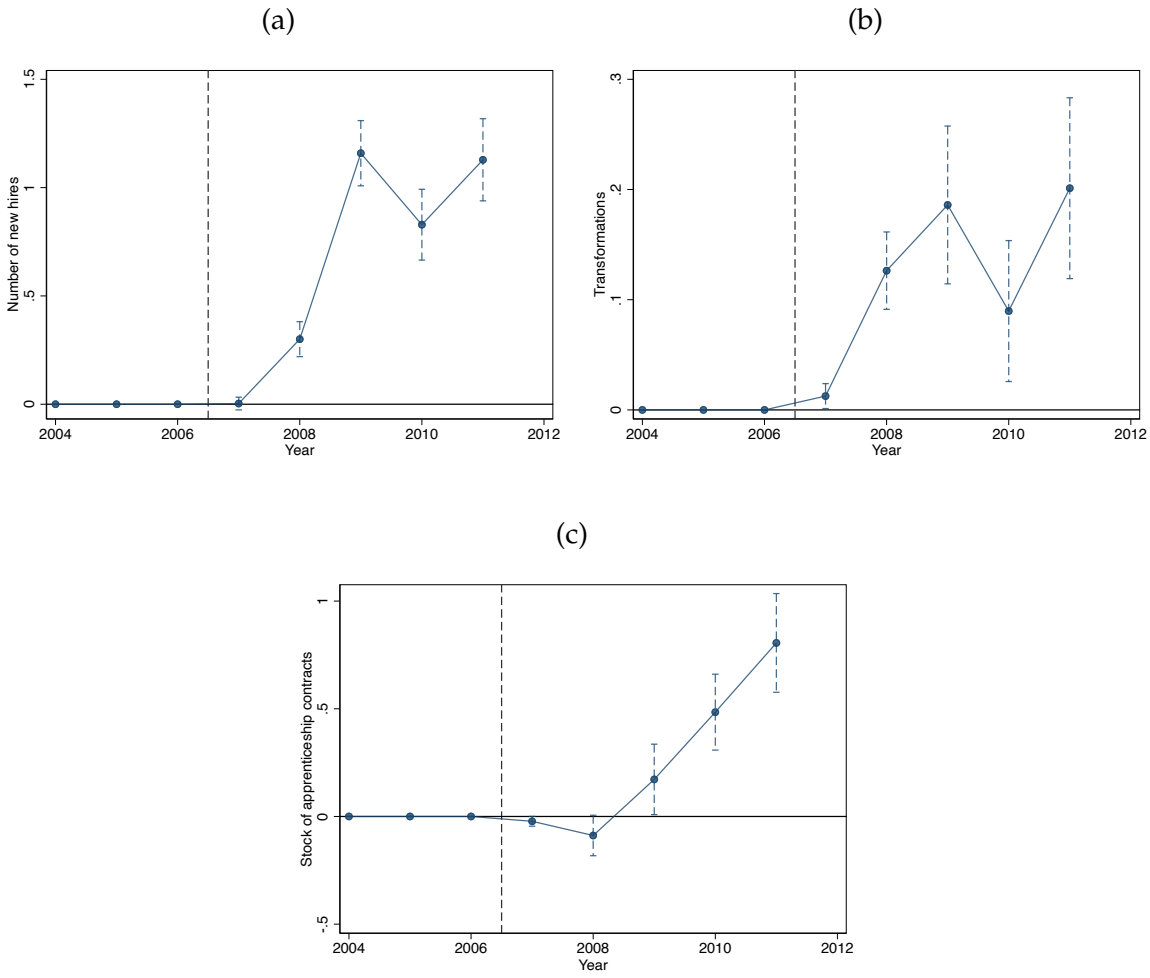


Note: Matched-Cerved sample, INPS and Cerved data. This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between small and medium firms relative to 2006 by exposure to the Great Recession, i.e., the $\hat{\beta}_k^L$ (blue circles) and the $\hat{\beta}_k^H$ (red circles) from equation (5). Exposure is defined as total revenues. The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). Standard errors are clustered at the firm level. The x-axis indexes time.

Figure D.9: Robustness: Clustering

Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals under different clustering schemes for the difference between small and medium firms relative to 2006, i.e., the $\hat{\beta}_k$ from equation (1). This figures compares the standard errors clustered at the firm level (blue circles), with those clustered at the two-digit sector (red circles), and those clustered at the local labor market level (yellow triangles). The dependent variables are number of apprenticeship hires (panel a), number of apprenticeship separations (panel b), stock of apprentices (panel c), and number of transformations of apprenticeship contracts to open ended contracts (panel d). The x-axis indexes time.

Figure D.10: Robustness: Jobs created per €1,000 spent



Note: Full sample, INPS data. This figure reports the regression coefficients and the associated 95% confidence intervals for the the impact of €1000 reduction in SSCs on complier firms relative to the pre-period (2004-2006), i.e., the θ_k from equation (8). The dependent variables are the number of new apprentices hires (panel a), the number of transformations into open-ended contracts (panel b), and the stock of apprentices (panel c). Standard errors are clustered at the firm level. M_{it} is constructed as the net present value of concurrent and future foregone payroll taxes. The x-axis indexes time.