

Temporary Employment, Job Flows and Productivity: A Tale of two Reforms

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Abstract

We investigate the effects of two reforms of temporary employment using panel data on Italian firms. We exploit variation in their implementation across regions and sectors for identification. We find that the reform of apprenticeship contracts increased job turnover and induced the substitution of external staff with firms' apprentices, with an overall productivity-enhancing effect. The reform of fixed-term contracts instead did not produce the intended results: it induced a substitution of temporary employees in favour of permanent ones and reduced capital intensity, generating productivity losses. We estimate substitution elasticities across types of temporary contracts that are consistent with these interpretations.

JEL-Code: J240, J410.

Keywords: employment contracts, productivity, institutional changes.

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1 Introduction

In the past two decades the major policy response to high unemployment rates in Europe has been the reduction of Employment Protection Legislation (henceforth EPL) through the liberalisation of temporary contracts.¹ A large literature has established the importance of temporary contracts in affecting job flows by increasing both the hiring and the firing of workers. Although much less researched in theory and in practice, it is plausible that temporary contracts also have a bearing on firms' capital investment decisions, on the capital–labour ratio and, eventually, on productivity. While the effects of EPL on productivity have been assessed in the past (see Autor et al., 2007, Bassanini et al., 2009, and Cingano et al., 2010), the productivity impact of temporary contracts liberalisation has never been evaluated using firm-level data and we are the first to provide this type of evidence.

The purpose of this paper is to evaluate the effects of the institutional changes of two different types of temporary contracts which constitute the core of recent labour market policy in Italy. We analyse the effects of these changes on job flows, employment, capital–labour substitution and productivity. The first institutional change has to do with the implementation of a national law (legislated in 2001) which eased the use of fixed-term contracts by cancelling the need of giving a justification for the use of these contracts. While the law set out nationally a general framework for the use of fixed-term contracts, the actual implementation of its provisions required their approval through the rounds of collective bargaining that took place sector-wise in the subsequent years (starting in 2005, much later than the national law). The actual way in which each sector of the economy implemented the law was therefore different, and the timing of the implementa-

¹Among the countries in the European Union, France, Italy, Germany, Spain and Portugal liberalised temporary contracts over the 1980s and 1990s.

tion varied according to the staggered structure of collective bargaining rounds. This feature generates variation across sector and over time in firms' exposure to the new provisions, which we exploit in estimation. The second reform concerns apprenticeship contracts for young workers. It was meant to stimulate the use of these contractual arrangements mainly by weakening the need of training certifications and extending the scope of their applicability to individuals up to 30 years of age. The relevant law was legislated in 2003 but required regional governments to issue implementation guidelines, which happened differentially by region in the subsequent years (also starting in 2005). This feature of the legislative process generates variation across regions and time in firms' ability to use the new contracts.

A further contribution of this paper is that we estimate the elasticity of substitution between different types of temporary contracts. Economic models necessarily simplify the actual use of temporary and permanent contracts and consider one single type of temporary contract. However in practice in all countries there exist different types of temporary contracts, typically the result of repeated attempts at making the labour market more flexible leaving the open-end contracts untouched. Italian employers can use four types of temporary contracts with different characteristics: apprenticeships (*Apprendistato*), fixed-term (*Tempo Determinato*), collaboration workers (*Collaborazioni Coordinate e Continuative*, the so called *co.co.co*, a sort of consultant hired on a temporary basis) and temporary agency jobs (*Interinali*). These contracts differ along various dimensions such as illness provisions, minimum wages, age restrictions, temporal limits and number of allowed repetitions of the same contract. The perception among labour lawyers and entrepreneurs is that they are highly substitutable but so far there is no hard evidence of this.

We have firm-level data on the use of the four different types of temporary

labour contracts and on permanent ones and we show that the effect of the reform of one type of labour contract may work also through the substitution with other types. This is the first paper, as far as we know, which studies the substitutability across different types of temporary contracts and highlights the potential consequences of a high elasticity of substitution.

We find that the reform of apprenticeship contracts has been successful because it increased the turnover of workers easing the adjustment process of firms, encouraged the substitution of external staff with apprentices and eventually increased productivity. The reform of fixed-term contracts, instead, does not seem to have had the intended results. The fact that the implementation of the national law required approval through collective bargaining rounds and that the new contract itself was subject to high degree of uncertainty may have altered the original spirit of the law and made the use of fixed-term contracts less costly but more "risky". This reform reduced overall job turnover, induced substitution with permanent workers and a lower use of capital per worker, which eventually reduced all measures of firm-level productivity (value added per worker, revenues per worker, TFP).

The paper proceeds as follows: in Section 2 we review the literature, in Section 3 we describe the institutional changes, in Section 4 we describe the data, in Sections 5 and 6 we present, respectively, the estimation framework and the results and we conclude in Section 7.

2 Related literature

Temporary contracts are typically used for different reasons: for screening purposes, to temporarily fill in for staff who are absent or leave, or to accommodate fluctuations in demand. In many cases employers also save in labor costs and

social security benefits using temporary contracts (Houseman, 2001). Temporary jobs inhibit labor market advancement if these jobs displace more productive employment activities. However they may also increase employment and earnings if they substitute for spells of unemployment. The effect of temporary contracts on productivity depends on whether temporary positions on average complement or displace permanent jobs. In this respect the literature on temporary jobs - which mostly looks at the employment effects rather than wage effects - is very heterogeneous and does not draw a general conclusion.

Using a natural experiment Autor and Houseman (2010) show that temporary help positions reduce earnings and employment probabilities in the US. Kvasnicka (2008) finds that holding a temporary contract after a spell of unemployment has at best a null effect on the probability of finding a permanent job. With an eye to European labour markets, Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002) model temporary contracts as churning policies that affect negatively wage setting and may generate higher unemployment and lower productivity. Contrary to this pessimistic vision, if fixed-term contracts are used as a buffer-stock to boost the number of hirings in a boom, employment and productivity may go up at least temporarily (Bentolila and Saint-Paul, 1992, and Boeri and Garibaldi, 2007). Some papers show that being assigned to a temporary contract has a causal effect on the probability of finding a permanent match (for example Ichino et al., 2008, and Booth et al., 2002). According to this view temporary contracts are good screening devices and stepping stones into permanent jobs and therefore increase productivity. While existing studies on temporary employment have been considering all these aspects, an evaluation of the relationship between temporary contracts and firms productivity is still missing in the literature so far.

The specific literature on temporary contracts is also strictly related to the more general one on EPL. In continental Europe many countries relaxed the rules

about the use of temporary contracts with the aim of reducing adjustment costs for firms facing high EPL for standard open-ended contracts. Therefore the reforms designed to make the use of temporary contracts easier constitute a reduction in EPL but also create dual or two-tier labour markets (see Dolado et al., 2002, for Spain, and Holmlund and Storrie, 2002, for Sweden).² EPL raises the cost of employing workers and reduces labour demand unless wages fall to cover exactly the cost of the benefit (Lazear, 1990).³ Because part of EPL constitutes a tax that goes to third parties - lawyers and administrative costs - EPL is by all means an adjustment cost and there is overwhelming evidence that it reduces the volatility of employment (Bentolila and Bertola, 1990). In the following we do not review the huge literature on EPL and job flows and we concentrate on the literature that looks at the relationship between EPL and investment and between EPL and productivity. The effects on productivity and investment are theoretically ambiguous (Ljungqvist, 2002) but we use the insights of this literature to interpret our results.

On the one hand there are multiple mechanisms that may induce a negative effect of an increase in EPL on productivity. High EPL hampers the reallocation of workers and jobs across industries and firms by inducing substitution of specific for general skills (Samaniego, 2006, and Wasmer, 2006); reduces workers effort (Ichino and Riphahn, 2005, Riphahn and Engellandt, 2005, Dolado and Stucchi, 2008); reduces the undertaking of highly productive but risky activities (Bartelsman and Hinloopen, 2005). On the other hand other mechanisms indicate a positive rela-

²The OECD produces different indices of employment protection, including one related to the regulation of temporary contracts only. When the index is built considering only the legal treatment of fixed-term contracts, the negative correlation between EPL and job flows is significantly stronger (Martin and Scarpetta, 2011). Temporary contracts and EPL are related also in the US: Autor (2003) showed that higher EPL induced a higher use of temporary agency jobs in the US.

³Analyzing the 1990 Italian reform of EPL, Leonardi and Pica (2010) show that the fall in wages is not a perfect offset of the increased severance costs and conclude therefore that EPL imposes efficiency costs in the competitive model.

tionship between EPL and productivity. More stringent EPL provides insurance and may promote specific investments (Belot et al., 2007); selects the most productive firms which withstand the costs of EPL (Poschke, 2009 and 2010); makes firms become more selective with workers and less productive matches are not realised (Lagos, 2006).

There are theoretical reasons to expect also an ambiguous effect of temporary contracts and EPL on the capital–labour ratio. In labour markets with no frictions an increase in the cost of labour will in general imply substitution of labour with more capital and therefore a positive relationship between EPL and capital–labour ratios. A related case arises in the long run: higher EPL means that labour is more costly and when adopting new technologies firms will choose more capital intensive technologies (see among others Caballero and Hammour, 1998, Alesina and Zeira, 2006 and Koeniger and Leonardi, 2007). Models with wage bargaining between workers and firms instead point to a negative relationship between EPL and capital–labour ratios. When there is wage bargaining, workers will use the protection of EPL - which strengthens the outside option of workers - to claim higher wages (Bentolila and Dolado 1994, and Garibaldi and Violante 2005). As a result, firms may reduce their investment ex-ante to avoid workers capturing part of the investment returns (the so called ‘hold up’ problem).

The empirical part of most of the existing papers on EPL and productivity is based on cross-country and/or cross-industry regressions which usually find negative relationship between EPL and productivity (Micco and Pagés, 2008, Cingano et al., 2010). The approach based on country or industry data potentially suffers from well-known severe problems: reverse causality and omitted variables and most studies do not distinguish between EPL provisions for fixed-term and permanent contracts. In using firm-level data, our work is close to Autor et al. (2007) who study the impact of adoption of wrongful-discharge protection norms in the

US using cross-state differences in the timing of adoption. Differently from them we use a change in EPL that concerns the relaxation of rules about the use of temporary contracts which is typical of many European countries.

3 Institutional background

Italian employers may chose to utilize labour inputs under a variety of employment contracts. The most typical form of contract is the permanent one, which has no termination date and has the highest wedge between workers take home gross pay and labour costs, caused by taxes and social security contributions. Depending upon firms characteristics (mainly their size) these contracts are characterised by relatively stringent EPL and, consequently, high firing costs. A second type of contract is represented by fixed-term contracts. The only difference between these and permanent ones is the presence of a fixed-term: they typically last for two years and can be renewed only once within a given firm-worker match. All other working conditions such as wages, working times, pension rights and probation periods, are identical to the ones of permanent contracts. Apprenticeships represent another form of temporary employment contracts. Differently from fixed-term ones, firms can use these contracts only for younger workers (details on age limits are given later in this Section), they must provide certified training to workers, and pay lower social security contributions. Workers under these three contracts are employees of the firm. There exist other contractual arrangements through which firms can use the labour services of external staff without actually hiring workers. As in many other countries there are temporary help agencies which supply labour services upon the payment of agency fees. Finally, and this is mostly an Italian peculiarity, firms can use collaboration contracts. This contractual arrangements are in place since the early 1970s and were regulated in 1997. They provide a

contractual framework for individuals who are not employed by the firm but individually provide their working services to the firm, either immaterial (consultants) or material. The labour costs associated with these contracts are low thanks to a reduced regime of compulsory pension contributions, which induced many firms to adopt them even in cases in which the worker was in all effects an employee.

Similarly to other European countries, labour market flexibility has increased in Italy over the last decades as a result of a series of reforms which introduced various types of temporary contracts without changing the legislation on permanent, open-ended, contracts. The most important legislation was:

1. Law no. 196/1997 (the so called ‘Treu-Package’, named after the then minister of labour) which legalised temporary work agencies, regulated collaboration contracts and liberalised both apprenticeship and fixed-term contracts;

2. Decree Law no. 368/2001 which eased restrictions on fixed-term contracts further;

3. Law no. 30/2003 (the so called ‘Biagi Law’, named after the legal expert killed by terrorists) which introduced a number of new contracts in the national legislation and reformed the apprenticeship contract.

Our analysis considers data for the period 2004–2007 and focuses on the second and third of these reforms. These two measures were implemented at different times in different regions and in different sectors of the economy, generating variation in the institutional setting that allows us to use a difference-in-differences approach. Both measures—although legislated at the national level in 2001 and 2003—were implemented starting only in 2005 and therefore can be evaluated using the available data from 2004 to 2007. We discuss each of the two measures in turn.

3.1 The ‘new’ fixed-term contract

Legislative Decree no. 368/2001 introduced important changes to fixed-term employment contracts. They included two changes of particular importance for the purposes of this study. The first and definitely most important modification concerned what are termed the ‘reasons’, i.e. the circumstances in which this type of contract may be used. Prior to 2001 the law regulating fixed-term contracts provided a very specific list of circumstances under which firms could use those contracts, for example peaks in production or replacement of workers on sick leave. The new decree liberalised the contract by abolishing the detailed list of specific reasons and introducing the following single general reason: ‘reasons of a technical, organisational, production or replacement nature’. While this part of the decree was intended to allow employers greater flexibility in the use of fixed-term contracts, in practice it made the requirements for the use of these contracts too generic, which inevitably produced uncertainty over the contents of the legislation and how to apply it (Aimo, 2006). Uncertainty over the contents has generated different interpretations of the decree, in particular on whether or not employers could recruit workers on fixed-term contracts without necessarily demonstrating the temporary nature of the work performed by those employed on those contracts. As noted by experts in labour law, this uncertainty may have reversed the originally intended effect of the reform, making the use of this type of contracts more costly, rather than less costly, to firms.

The second change introduced by the decree, which is of particular interest here, is that it has restrained the scope for unions to affect the implementation of national law provisions through collective bargaining that takes place at the industry level. Under the previous legislation, collective bargaining agreements could list additional ‘reasons’ for the use of fixed-term contracts over and above those contained in the national legislation. Given that unions enjoy broad powers

within collective bargaining agreements, they could—and actually did—make the application of fixed-term contracts within a given industry more restrictive than what was established at the national level. The decree abolished the possibility of including additional ‘reasons’ through collective bargaining, thereby reducing union power and increasing the freedom of employers to use fixed-term contracts.

We evaluate the effects of this reform using a difference-in-differences research design. The case of the new fixed-term contracts lends itself to this type of analysis since in order to become applicable in a given industry, the new decree needed to be implemented through the national contracts for that industry. Therefore, only industries with national contracts negotiated after the decree was legislated, could apply the new fixed-term contracts. In Italy, collective bargaining is staggered by industry, so that not all industries bargain at the same time. In particular, after 2001 the renegotiation of collective bargaining agreements that implemented the new contracts at the sectoral level only occurred in some industries (Textiles, Wood production, Chemicals, Construction, Transportation, Retail trade, Food production and Telecommunication), with contracts signed mostly in 2005 and 2006. Our analysis exploits such variation across industries over time. Other important sectors of the economy such as Metal Manufacturing and Banking reached collective agreements during the period, but those agreements did not contain provisions about the use of the new fixed-term contract.

3.2 The ‘new’ apprenticeship contract

Legislation to regulate apprenticeship contracts has existed for a long time and has also been reformed several times. The lower labour costs associated with these contracts make them particularly convenient to employers. These lower labour costs are intended to compensate firms for the training costs that they incur. Firms are required to share training costs by giving apprentices time off work (for a mini-

mum number of paid hours) to attend external training courses that are provided by local authorities or accredited training institutes (and sponsored by the regions) outside the premises of the firm. At the end of the training period, apprentices should receive a certificate for the qualification they have attained. There are, nevertheless, limitations on this formal training activity: lack of public funding for training, a lack of infrastructures for training courses and little control over compliance with compulsory training obligations by firms using these contracts. As a consequence most of the training is in the form of the on-the-job type.

The ‘Biagi Law’ liberalised this contract further. A new form of apprenticeship was introduced (*apprendistato professionalizzante*, literally ‘apprenticeship leading to a job’) with the same reduced labour costs as before. The new legislation abolished the certification of qualifications and extended the scope of the contract to include persons up to the age of 30 (the previous age limit was 25). The option of performing training at the workplace as a substitute, at least in part, for external training courses was also introduced. This last amendment made it even more difficult to monitor compliance with this obligation by firms. Before the new law could be implemented, regional governments – who have exclusive power to legislate over vocational training, including the training content of the new apprenticeships – had to issue regional regulations. The regions were, nevertheless, very slow in issuing these regulations, partly because they lacked the funds needed to organise the external training for apprentices (despite the reduction in the quantity of this type of training by the national legislation). Although slow to act, some regions passed legislation earlier than others. Some regions also enacted experimental projects for the new contract in specific economic sectors (mainly Retail trade, Banking and Hotel and restaurants). These experimental projects were implemented in 2005.

No regions passed any guidelines in 2003 and 2004. In addition to those re-

gions which introduced sector-specific experimental schemes, in 2005 two regions, Emilia Romagna and Tuscany, enacted regional regulations to enable the use of the new contract by all firms. Another four regions followed suit in 2006: Friuli, Marche, Sardinia and the autonomous province of Bolzano. Finally, regulations were issued in Lazio in 2007. We exploit this variation over regions and time in a difference-in-differences framework. Additional institutional variation in the implementation of the new contracts was generated by guidelines issued by the Ministry of Labour in July 2005 (Circolare no. 30), specifying that in the absence of regional regulations, sector-specific national collective agreements could specify the training content of the new contracts. Thence, firms in those sectors that signed bargaining agreements after the guidelines were issued could use the new apprenticeship contract. Collective agreements were reached in: Textiles, Wood production, Chemicals, Construction, Transportation, Retail trade, Food production and Telecommunication, Energy, Banking, and Metal manufacturing. To sum up, institutional variation in firms' exposure to the new apprenticeship contract comes from three sources: regional, sectoral, and regional-sectoral (the later deriving from the experimental projects of 2005).

4 Data and descriptive statistics

The data set used in this paper is a balanced panel of about 13,000 firms in the private sector observed over the years 2004—2007, representative of the universe of corporate firms in the private sector. Firm-level information on the types of employment contracts used within the firm is derived from the Excelsior database, a survey conducted by Unioncamere (the Association of Italian Chambers of Commerce) with the aim of providing information on firms' occupational needs, in particular the skill requirement of prospective hires. It contains detailed infor-

mation on the number of workers in the firm, distinguishing across all the various contractual arrangements that firms may adopt for utilising labour services: permanent employment contracts, fixed-term employment contracts, apprenticeships, agency workers and collaborators. The data also provide details on the industry (3-digit) and geographical location of the firm, which is essential in constructing the treatment indicators discussed in the institutional section. The other relevant piece of information used in the paper is the balance sheet information which is derived from the ASIA database, the archive of firm data maintained by the National Statistical Institute. In particular, ASIA provides information on firms' value added, revenues and net physical capital stock.

Based on this information we can construct two treatment dummies capturing the exposure of firms to the two reforms discussed in the previous Section. We identify exposure to the reform of fixed-term contracts using firms' sectoral affiliation. Treated sectors are the ones whose national collective agreements were signed after the nation-wide legislation was passed in 2001, and whose national agreements explicitly implemented the new legislation. These sectors were Textiles, Wood production, Chemicals, Construction, Transportation, Retail trade and Food production, whose collective agreements were signed in 2005, and Telecommunication, with agreements reached in 2006. Exposure to the reform of apprenticeships occurred mostly through firms geographical location due to the staggered adoption of regional regulations implementing the national legislation. The first regulations were introduced in Emilia-Romagna and Tuscany in 2005, followed by Friuli, Marche, Sardinia and the autonomous province of Bolzano in 2006 and by Lazio in 2007. Other regions (Piedmont, Lombardy, Umbria, Abruzzo, Campania, Veneto, Liguria, Marche, Lazio) introduced experimental regulations only in some sectors (such as Retail trade, Banking and Hotel and restaurants), therefore we include firms operating in those sectors and in those regions in the treatment group. Fi-

nally, as explained in Section 3, guidelines issued by the Ministry of Labour in July 2005 allowed the possibility to use the new contracts to firms in sectors that would have reached national agreements afterwards, and we also include these firms in the treatment group.

In Table 1 we provide a description of the data. We begin by grouping firms on the basis of their exposures to the reforms, distinguishing never treated firms from firms treated by the apprenticeships reform and those exposed to the reform of fixed-term contract. Since a firm may be exposed to both reforms, the sum of the numbers in the three groups exceed the number in the full sample. There are no firms in the treatment group in 2004. The number of firms treated by the reform of apprenticeships is relatively small in 2005 (when only two regions adopted the regulations, while some others introduced experimental regulations in some sectors) but grows considerably in 2006 as a consequence of the adoption of regulations by several more regions and of the ministerial guidelines allowing using the new contracts in additional sectors. The number of treated firms still grows in 2007 as a consequence of Lazio joining the group of regions issuing regulations. Exposure to the reform of the fixed-term contract follows a different pattern: most firms enter the treatment groups in 2005, and the remaining few cases which join in 2006 are firms in Telecommunication. We compute firm size including also external staff (agency workers and collaborators) and obtain an average of about 200 workers (about 190 if excluding external staff), slightly higher among the never treated and lower among firms in the treatment group of the apprenticeship reform. The workforce composition is rather stable across groups, but treated firms tend to have a higher share of fixed-term and apprenticeships contracts, whereas never treated ones use relatively more collaboration contracts. Looking at job turnover computed as the mean absolute employment change, we can observe that there are essentially no differences across treatment groups: average turnover is about

11 percentage points in each groups, virtually identical to the values reported by Autor et al. (2007) on plant level data. The percentage of positive changes is 44, about 5 points lower than the figures of Autor et al. (2007), and there is little variation across the groups. The geographical distribution of the firms reflects the well known regional heterogeneity of economic activity in Italy, with most firms located in the North-west and the North-east. The largest share of firms in the sample operates in manufacturing. The sectoral distribution by treatment status is sparse in the case of fixed-term contract reform because only firms in some sectors were treated.

5 Estimation framework

We are interested in assessing the impact of the two reforms on measures of job turnover, production inputs and productivity. As documented in the previous Sections, we can exploit in a difference-in differences set-up the differential variation in exposure to the reforms across regions and sectors over time. Let d_{it}^F be a dummy capturing the exposure of firm i in time t to the reform of fixed-term contracts, and d_{it}^A a dummy capturing the exposure of firm i in time t to the reform of apprenticeships, with $t = 2004, \dots, 2007$. In order to ensure that our comparisons across treatment groups over time do not reflect group-specific characteristics, we control for time, region and sector fixed effects, plus region-specific and sector-specific time trends. The latter require that identification comes from the discontinuity surrounding the passage of the reforms. These specifications can provide reassurance that estimated reforms effects are not reflecting smoothly-trending omitted variables that are potentially correlated with the adoption of the reforms. Our main estimating equation takes the following form:

$$Y_{it} = \gamma_F d_{it}^F + \gamma_A d_{it}^A + \delta_t + \sum_s (\theta_s + \lambda_s t) Z_i^s + \sum_r (\theta_r + \lambda_r t) Z_i^r + \beta' X_{it} + \varepsilon_{it}, \quad (1)$$

where Y is an outcome measure, the γ coefficients measure the effects of the two reforms on the outcome, δ_t is a time fixed effect, Z_i^r and Z_i^s are dummy variables for regions and sectors, so that the θ coefficients capture regional (r) and sectoral (s) fixed effects, while the λ coefficients capture region- and sector-specific time trends, X_{it} is a vector of controls and ε_{it} is an error term. Whenever the outcomes of interest are in levels we also include firms fixed effects, whereas in the case for variables derived from differencing levels, like job turnover, we control for the presence of repeated observation by firms using a robust variance estimator.

We start by looking at job flows as the outcome of interest. Specifically, we consider the year-to-year job turnover defined as in Davis et al. (1996) and Autor et al (2007) : $JT_{it} = \frac{|E_{it} - E_{it-1}|}{\frac{1}{2}(E_{it} + E_{it-1})}$ where E_{it} is firm i employment in year t .

This measure accounts for the absolute year-to-year employment change by recording annual net employment flows. Since we have detailed information on the type of employment contracts, we are able to estimate the reforms' impact on employment flows considering either total employment and employment in each contract type. This exercise enables an indirect assessment of the degree of substitutability between different types of employment contracts. In other words, the effectiveness of reforms in one type of employment contract greatly depends on the extent to which firms are able to substitute between contract types. Estimating the impact of reforming one type of contract on job flows of another contract type is a way to assess the existence of substitution effects across contracts.

Next, our investigation will proceed by applying the estimating framework of equation (1) to other margins of firms decision, namely employment levels (overall

and by contract type), capital (total and per worker), investments (total and per worker) and the skill ratio defined as the ratio between non-manual and manual workers in the firm. Applying equation (1) to this set of outcomes will offer a rather complete picture of the effects of the two reforms on firms production choices. Finally, we will focus our attention on the results of firms activity, namely on various measures of productivity. Specifically we will consider value added per worker, revenues per worker and total factor productivity.

5.1 Assessing the validity of identification

The validity of the identification of (1) rests on the exogeneity of the reforms. In the ideal case, the reform adoption decisions (by the regions and the sectoral bargaining rounds) would be independent random events that varied in timing and had no spillover effects to non-adopting regions or sectors. While firm migration across sectors and regions to take advantage of the rules is highly unlikely, one possible concern is that the regions which had higher or lower than average employment growth in temporary contracts were also the same to adopt the reforms of the apprenticeship contract or of the fixed-term contract.

To dispel this doubt we use data from the Italian Labour Force Survey (LFS) from 1996–2007. We cannot use our firm-level data because we need to observe several years of data prior to the reforms to control for pre-dating trends in employment in temporary contracts, whereas in the Excelsior database 2004 is the only pre-reform year. Therefore we use LFS data which, although based on individuals and not on firms, are a representative sample of the Italian labour market. Figure 1 top panel compares the log employment in (all types of) temporary contracts in the regions adopting the apprenticeship contract reform (treated sample) and in the non-adopting regions (control sample). The bottom panel does the same for adopting and non-adopting sectors of the fixed-term contract reform. Both panels

show a similar movement in the two series before the adoption of the two reforms in 2005 thus supporting the validity of our identification strategy which is based on the assumption that the outcomes of interest would have otherwise evolved similarly in adopting and non-adopting regions and sectors.⁴

To further prove that preceding trends in temporary employment do not predict the adoption of the reforms, we regress the two treatment dummies defined at the sectoral (for the fixed-term reform) or regional (for the apprenticeship reform) level on leads and lags of log employment in temporary contracts computed from the LFS. The coefficients on the lags are relative to the period four years prior to the reform, and their pattern indicates whether the coefficients associated with the reform in equation (1) are consistent with a causal interpretation. In particular, we would be concerned if there are large and statistically significant coefficients on the lag indicators, regardless of whether they are positive or negative. The first two columns of Table 2 show the effect of log temporary employment on the adoption of the apprenticeship contract reform. The results show that past temporary employment has no significant effect on the adoption of the reform. In the same way the third and fourth columns show that past temporary employment has no effect on the adoption of the fixed-term contract reform. Overall, the evidence from both Figure 1 and Table 2 is consistent with a causal interpretation of the effects that we are going to discuss in the next Section.

⁴Clearly the exercise for the apprenticeship reform is incomplete because part of firms exposure occurred at the sectoral, not regional, level while our exercise only considers the regional dimension. However, as explained in Section 3, the sectoral dimension of exposure to the treatment was mostly due to governmental guidelines that were valid throughout the country, without any element of choice on the part of sectors. The spirit of this comparison is to check whether adopting regions or sectors did so on the basis of trends in temporary employment, so that the comparison between treated and non treated regions in the graph represents a good approximation of the complete treated-control comparison. Similar remarks apply to the regression of Table 2.

6 Results

We begin by assessing the impact of the two reforms on the level of job reallocation. If the reforms decreased the costs of using certain types of temporary contracts, then we should expect an increase in the hiring and dismissal of workers with those same contracts, which in turn should result in an increase of employment fluctuations.

We next consider the effects of the reforms on firms employment, both at the aggregate level and by contract types. Furthermore, we also investigate the effects of the reforms on some other margins of firm adjustment along which theory does not give clear predictions and prior research has obtained mixed results: capital (total and per worker), investment (total and per worker) and the skill mix, defined as the ratio between non-manual and manual workers in the firm.

After considering the impact of the reform on various dimensions of inputs to the production process, our analysis moves on to consider effects on productivity, looking at both labour productivity and total factor productivity (TFP). Finally, we provide evidence on the interplay between firms' production function and the various forms of labour contracts by estimating a the elasticity of substitution among temporary employment contracts and between these and permanent contracts.

6.1 Job reallocation

Table 3 provides results on job turnover. Panel A of the Table considers overall job turnover. The reform of apprenticeship contracts had a positive effect on apprentices' turnover, producing a statistically significant increase of about 3 percentage points (p.p.). The reform, on the other hand, had no significant effects on the use of other types of employment contracts, nor on turnover in total employment.

The reform of fixed-term contracts had a positive effect on job reallocation within this type of employment contracts, which is similar in size to the effect of the apprenticeship reform on turnover in apprenticeship, 3 p.p.. Moreover, a somewhat unexpected and smaller effect of this latter reform can be observed on turnover in permanent contracts, 1 p.p.. We can also observe a negative significant effect on total employment turnover, although of limited size, 0.5 p.p..

The measures of turnover cannot distinguish whether the effects come from more hiring or more firing. We provide insights on this point by separating expanding firms (i.e. firms with a positive or null change in employment between 2004 and 2007) from declining ones. The results of Panel B for expanding firms confirm the positive effects of the reform of apprenticeships, i.e. expanding firms took advantage of the new apprenticeship contract. There are also negative –non significant– coefficients estimated on agency and collaborator workers which are consistent with the idea that expanding firms used the new contracts for substituting external workforce with workers employed by the firm. The reform of fixed-term contracts, instead, had a significant effects on the turnover of permanent workers in expanding firms, but not on fixed-term ones. Panel C of the Table shows that the reform of fixed-term contracts had an impact on the turnover of fixed-term workers only in contracting firms. In sum, firms treated by the reform of fixed-term contract increased turnover of fixed-term workers only if they were contracting total employment, whereas they used permanent contracts if they were expanding. Both facts suggest that this reform was not successful in stimulating fixed-term employment.

6.2 Robustness

In Table 4 we assess the robustness of our findings on job turnover, focussing on turnover in total employment (Panel A), turnover in apprenticeships (Panel

B) and turnover in fixed-term contracts (Panel C). In column (1) of the Table we show results obtained after excluding from the sample firms that displayed very low or very high levels of year to year change in capital levels, i.e. above or below the 99th or 1st percentile in the distribution of average capital changes. Column (2) of the Table instead adds sources of firms heterogeneity by controlling for (endogenous) firms characteristics in terms of capital levels, value added and the skill mix. Alternatively, in Column (3) we control for time invariant firm heterogeneity (both observed and unobserved) by using a fixed effects estimator. Finally, in Column (4) rather than considering absolute employment changes, we look at employment growth, i.e. we use the measure defined in the previous Section but without absolute values at the numerator.

Results for turnover in overall employment in Panel A are generally robust in that statistically significant coefficients remain significant and maintain size and sign. In particular, Table 3 pointed towards a 0.5 p.p. penalty associated with the reform of fixed-term contracts, which is still evident in Columns (1) and (2). The effect is smaller in size when the fixed effect estimator is used, whereas it almost doubles in the growth regression (minus 0.9 p.p.).

Results on turnover of apprenticeships in Panel B are also robust across columns. The most evident difference with respect to the benchmark regression of Table 3 (Panel A, Column (4)) arises from the regression with fixed effects, where the estimated coefficient on the apprenticeship reform becomes about 1 p.p. smaller (from 3.1 to 1.8) while its standard error remains stable, resulting in an overall loss of statistical significance. This coefficient is estimated out of within-firm variation over a relatively short time interval, while the dependent variable is derived from differenced employment levels, thence it is not surprising that it loses significance.⁵

⁵Autor et al (2007) encountered similar issues of statistical significance in applying difference-in-differences estimators with plant fixed effects on the same variable over a much longer time span.

Overall, we can interpret fixed effects estimates as corroborative of the evidence on turnover of apprenticeships produced in Table 3.

Results from Panel C also point towards the robustness of the evidence on the reform of fixed-term contracts, the benchmark regression this time is Column (3) of Panel A in Table 3. We can observe again that the effect of the fixed-term reform loses significance in the fixed effects regression (both the point estimate and the standard error gain size, the latter relatively more) and remarks similar to the one for the apprenticeships reform also apply in this case. The effect in Column (4) - where we use employment growth rather than turnover as dependent variable - is close to zero and non significant, which is consistent with the evidence from Column (3) in Panel C of Table 3 that the reform increased turnover of fixed-term contracts only in declining firms.

6.3 Employment levels

The overall effect of the increase in turnover on the level employment is theoretically ambiguous because a higher turnover may imply a higher or lower net employment. To understand the employment effects of the new legislation, in Table 5 we estimate regressions for firms log employment. We consider both aggregate employment and employment in each of the type of contracts. All models include firms fixed effects.⁶ The apprenticeship reform had a positive effect on the net employment of apprentices: firms exposed to this reform experienced an increase in the level of apprenticeship employment of 5.2 p.p.. This confirms the evidence emerged from turnover equations in which expanding firms experienced a higher turnover of apprenticeships. The other effect of this reform is a reduction

⁶We experimented using Tobit regressions to account for censoring at zero in the employment of some type of contracts. We also tried using lagged reform indicators in place of current ones. Results of both robustness checks confirm our benchmark employment regression and therefore we do not report them here.

in the level of employment for collaborator, minus 6.5 p.p.. This is again in line with the findings on turnover in expanding firms, which appear to have used the reform for hiring apprenticeships and reducing the use of external workforce. Often collaborator workers are young individuals in the same age range covered by apprenticeship contracts, 15 to 30, and it may well be that firms consider workers on these contracts as substitutes for apprenticeships. A reason to move away from external workers may be their lower attachment to the firm and higher turnover costs.

Similar positive employment effects, instead, cannot be found for the reform of fixed-term contracts. In this case, there are no significant effects on employment of either permanent, fixed-term or apprentices, whereas there are effects on the levels of agency and collaborator workers that go in opposite directions with respect to each other. The overall employment effect is small (0.9 p.p.) and negative. This evidence confirms that the reform of fixed-term employment has not been successful in promoting the use of this type of contracts by firms.

6.4 Investment, capital and skill ratio

We now explore the consequences of the reforms on other margins of firms' adjustment such as capital, capital-labour substitution and investments: if reforms make the use of temporary workers easier and facilitate adjustment firms may substitute out of capital with new (temporary) workers. Alternatively firms may vary their skill ratio: this may happen if workers with certain skills tend to be concentrated in specific types of contracts, or if the easing of temporary contracts make the use of skills more or less intensive. A higher capital-labour ratio or a higher skill mix should also improve productivity.

In Table 6 Columns (1) and (2) we look at the effects of the reforms on log-capital and the log capital-labour ratio. The reform of fixed-term contracts had a

negative effect on firms' capital and the capital labour ratio, which decreased by 2.6 and 1.6 pp. In terms of the literature discussed in Section 2, the substitution effect between capital and labour prevailed over the 'hold up' effect. In Columns (3) and (4) we consider investment and investment per worker: since in the data we have information on net capital K , we define investment as $I_t = K_t - K_{t-1}$, i.e. we do not apply any depreciation rate. As was the case with capital, the reform of fixed-term contracts impacted negatively on investment, although standard errors are relatively large and the estimated coefficients not statistically significant at conventional levels.

These effects of the fixed-term reform contrast with the ones from the reform of apprenticeships. In this case estimated coefficients are smaller in size and never significant from a statistical point of view. The last set of results in the Table (Column 5) refers to a different substitution margin between skilled and unskilled workers. We find no effects of the two reforms on the skill ratio. This is expected in that temporary contracts are popular among both white and blue collars, particularly of young age.

6.5 Labour productivity

We now turn in Table 7 to productivity measures, which possibly represent the most relevant benchmark to measure the economic implications of institutional changes. We consider three different measures of productivity. The first is labour productivity defined as real value added per worker. The second focuses on firms sales and is defined as revenues per worker. Finally, we partial out the contribution of physical capital and build a measure of TFP as the residual of a regression of log value added on log capital and log employment.⁷

⁷We experimented using as denominator (for labour productivity) or control (for TFP) measures of employment that exclude external staff i.e. agency workers and collaborators. Dey et al. (2006) discuss the issue of computing productivity across sectors when the sectoral affiliation

Panel A of Table 7 shows that the apprenticeship reform has had a positive and significant impact on all measures of productivity, between 0.9 p.p. and 1.6 p.p. in the case of sales per worker and TFP.⁸ Results on the fixed term reform tell a completely different story, all coefficients being negative, sizeable (between 2.4 and 3.5 p.p.) and statistically significant.

Taken together with the results on employment turnover and employment levels, the productivity effects confirm that while the apprenticeship reform has been successful, the reform of fixed-term contracts generated effects that were opposite to expectations. To further assess the causal interpretation of our findings, in Panel B of Table 7 we use lagged values of the treatment indicators in place of current values. In this way we avoid picking up any simultaneity between institutional changes at the sectoral or regional level and productivity growth. Results are robust to the use of lagged treatment indicators, the only coefficient which loses significance is the one for the effect of the apprenticeship reform on revenues per worker.

A possible interpretation of these results is the following. The increase in the number of apprenticeships occurred through substitution of external staff, mainly collaboration workers. The rise in productivity that we observe is likely to reflect a compositional shift in labour quality because our labour productivity measures do not adjust for the quality of labour inputs. To the extent that external collaborators have lower attachment to the firm and exert lower effort, the reform of apprenticeship may have induced firms to shed this unproductive labour in exchange for more motivated apprentices. Although we do not have direct evidence of this, higher workers' effort is plausibly the mechanism that may have increased

of external staff is different from the one of the firm, e.g. service sector for external staff and manufacturing for the firm. We obtained virtually identical results in the two cases, and we present only those obtained using the overall number of workers.

⁸This result is similar to Autor et al. (2007) who analyze an increase in EPL and find that TFP is reduced with an average elasticity in the order of 3 to 4 pp..

labour productivity after the reform (Riphahn and Engellandt, 2005, Dolado and Stucchi, 2008).

The negative productivity effects of the reform of fixed-term contracts are less clear-cut. In this case, we know that firms treated by this reform increased job turnover along this margin only if they were declining, while they were more oriented toward hiring permanent workers if they were expanding. This substitution across contract types may have been an unintended consequence of the reform, stemming from the increased uncertainty on the applicability of fixed-term contracts brought about by the reform. In parallel, these firms have also reduced capital intensity, which may have induced the observed productivity decline.

6.6 Substitution effects

The reform of apprenticeship induced substitution of external staff (agency workers and collaborators) with apprentices. The reform of fixed-term contracts increased job flows of fixed-term contracts but reduced significantly total turnover. Non-declining firms increased turnover of permanent workers. These results suggest substitutability between permanent and temporary contracts and among temporary contracts of various types, which is something that has always been known among employers but has never been investigated by economists.

In order to provide a direct assessment of substitution effects across different types of contracts, we also estimate the parameters of a production function in capital and labour. We allow labour inputs to differ according to the contract type, distinguishing between permanent and temporary employment contracts and, within temporary contracts, among the four types of temporary contracts that are available to firms. In other words we estimate a simple production function where the four types of temporary contracts are partial substitutes and the entire group of temporary contracts is substitutable for permanent contracts. We model the

substitution across type of labour contracts using a nested CES technology:

$$Q_{it} = K_{it}^{\alpha} [L_{pit}^{\sigma} + (\sum_{\tau} L_{\tau it}^{\rho})^{\frac{\sigma}{\rho}}]^{\frac{1-\alpha}{\sigma}}, \quad (2)$$

where Q is the value added, K is capital, L_p is permanent labour and L_{τ} represents four types of flexible labour (agency workers, collaborators, apprentices, fixed-term). Using this nested CES specification, parameters σ and ρ govern the substitution process between labour inputs. In particular $\eta_{\rho} = \frac{1}{1-\rho}$ defines the substitution elasticity between varieties of temporary labour, while $\eta_{\sigma} = \frac{1}{1-\sigma}$ defines the substitution elasticity between permanent and temporary labour.

Table 8 shows that the elasticity of substitution between various types of temporary contracts is high and significant, higher than the elasticity of substitution between permanent contracts and temporary contracts. Pooling all years between 2004 and 2007, the elasticity of substitution between temporary contracts is 1.4 (with some variation across years) while the elasticity of substitution between permanent and temporary contracts is stable at around unity. In year 2007 the elasticity of substitution between the four types of temporary contracts is not estimated significantly. The elasticity of 1.4 is high and means that small changes in relative prices between different types of contracts yield big changes in relative quantities. This elasticity is higher than the elasticity between temporary and permanent contracts which have very different characteristics and are harder to substitute.

In the economics literature there are plenty of studies on substitutions elasticities across factors of production. There are no studies though on the substitution across different types of temporary contracts. The most famous studies which look at labour factors of different types, typically different education levels, find elasticities of substitution between college-educated and high-school educated workers in the US in the range of 0.5 to 2.5 (Katz and Murphy, 1992, find a value of 1.4).

These estimates are typically obtained under the assumption of the labor market being on the relative demand curve and using information on wage bill shares. With this method the conditional factor demand also include changes in prices or quantities of other inputs such as capital and energy. We do not have information on relative prices of all inputs (types of temporary contracts) therefore we estimate the elasticity using a simple CES function but we control for capital thus we control for substitution possibilities across other inputs.

7 Discussion and conclusion

The overall picture emerging from our analysis shows that the reform of apprenticeship contracts has been successful because it increased the turnover and the net employment of apprentice workers. These results suggest that the reform actually reduced the cost of apprenticeship contracts and firms were encouraged to substitute external temporary staff with apprentices. Although the capital–labour ratio remained unaffected, the reform increased labour productivity possibly through one of the mechanisms suggested in the literature, for example increasing average worker effort through the employment of more motivated workers (i.e. apprentices facing the prospects of training and wage growth) to replace external less motivated staff. The reform of fixed-term contracts instead does not seem to have had the intended results: the reform reduced labour turnover, reduced the capital–labour ratio and had a strong negative effect on productivity. This suggests that the reform may have made the use of fixed-term contracts more costly rather than less costly as already pointed out by some of the literature on labour law.⁹ If re-

⁹Similarly to us, Autor et al. (2007), show that a substantial component of the economic cost of the employment-at-will exceptions emanates from the uncertainty they introduced into the employment relationship. In this sense the finding that a reform increases uncertainty is not new. In Autor et al. (2007) it is an increase in EPL whose cost is aggravated by the increase in uncertainty, in our case it is a reform that decreases EPL whose effects are offset by the increase in uncertainty.

allocation of labour is important and the reform of fixed-term contracts hampers job reallocation across and within firms (for example because it raises the costs of consultancy for fear of the courts), then productivity falls. Indeed, finding a negative effect of fixed-term contracts on job reallocation is a pre-requisite for claiming that higher costs hamper the optimisation of resources and allocative efficiency (Bertola, 1990).

One possible mechanism that made the reform of apprenticeships effective and the reform of fixed-term contracts ineffective is respectively the substitution within different types of temporary workers and the substitution in favor of permanent contracts in the face of increased uncertainty about the applicability of fixed-term contracts. This interpretation is supported by estimates of high substitution elasticities especially within different types of temporary contracts. Contrary to the recent past, nowadays many countries are thinking of limiting the diffusion of temporary employment by limiting the use of those types of temporary contracts that are deemed the most misused. The evidence of substitutability provided in this paper is to be taken to account when proposing such policies that often do not predict the possible offsetting effects through the use of other types of temporary contracts.

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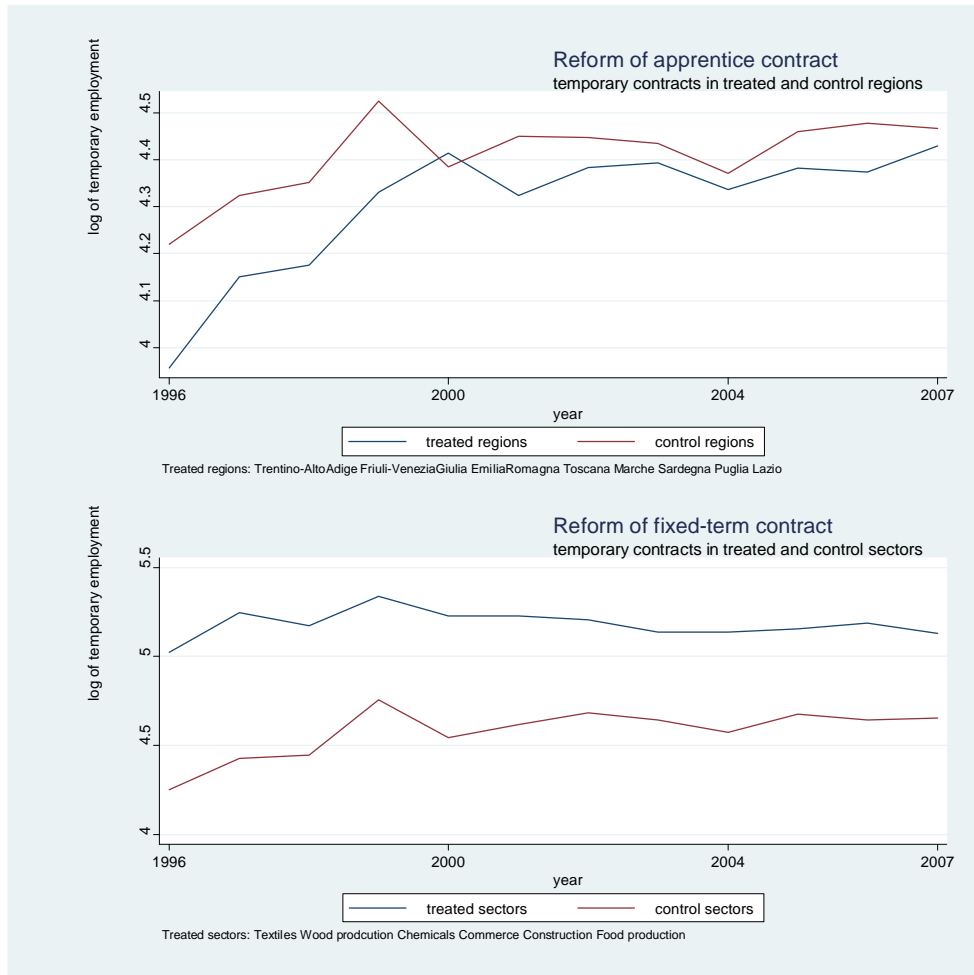


Figure 1: Log employment in temporary contracts in treated and control samples.

Table 1: Descriptive statistics for Excelsior firm panel 2004-2007

	Full sample	Never treated	Reform of apprenticeship	Reform of fixed term
Number of observations	53144	26182	24208	12994
2004	13286	13286	0	0
2005	13286	6922	3671	4316
2006	13286	3170	10095	4339
2007	13286	2804	10442	4339
Total employment	203.21	210.95	193.90	201.30
% Permanent contracts	88.11	87.92	88.05	88.76
% Fixed term contracts	6.02	5.78	6.38	5.99
% Apprenticeships	1.92	1.76	2.09	2.31
% Agency workers	2.32	2.25	2.44	2.10
% Collaborators	2.11	2.57	1.69	1.48
% Blue collars	60.85	60.09	61.12	60.32
% White collars	37.56	38.24	37.38	38.13
% Managers	1.59	1.67	1.50	1.55
Capital per capita (€ 2004)	63653.45	63367.87	64548.91	58655.62
Job turnover	11.33	11.61	11.32	11.01
% positive employment growth	44.01	43.69	44.94	42.63
% Northwest	36.84	44.01	28.46	35.17
% Northeast	31.44	25.95	38.20	30.58
% Central	18.40	15.06	22.60	19.76
% South	13.32	14.98	10.74	14.49
% Extraction	0.72	0.98	0.52	
% Manufacturing	54.55	52.98	55.24	48.39
% Energy	0.69	0.85	0.60	
% Construction	5.87	2.99	7.15	17.97
% Retail trade	10.87	5.49	16.42	33.36
% Hotel and restaurants	2.19	2.68	1.91	
% Transports and communication	6.97	6.32	8.34	0.28
% Finance	0.05	0.08	0.03	
% Real estate	10.55	16.29	5.52	
% Private education	0.47	0.80	0.17	
% Private health	4.92	7.60	2.58	
% Other services	2.14	2.94	1.52	

Table 2: Reform adoption and preceding trends in temporary employment

	Dummy reform of apprenticeship		Dummy reform of fixed term	
% female	-0.497		-0.372	
	(1.174)		(2.529)	
% university graduates	-0.839		-1.622	
	(1.023)		(2.403)	
log temp empl	0.077	0.059	0.383	0.414
	(0.087)	(0.093)	(0.308)	(0.325)
log temp empl t-1	0.018	0.012	-0.213	-0.210
	(0.085)	(0.091)	(0.332)	(0.344)
log temp empl t-2	0.132	0.120	-0.336	-0.336
	(0.089)	(0.095)	(0.235)	(0.252)
log temp empl t-3	0.003	0.005	0.0961	0.146
	(0.084)	(0.085)	(0.312)	(0.354)
log temp empl t-4	0.056	0.0511	0.0324	0.0619
	(0.087)	(0.089)	(0.288)	(0.310)
log temp empl t+1	0.048	0.030	0.108	0.107
	(0.080)	(0.084)	(0.246)	(0.258)
log temp empl t+2	0.093	0.082	-0.322	-0.351
	(0.091)	(0.093)	(0.301)	(0.320)
Constant	-1.952	-1.219	-2.939	-2.385
	(1.612)	(2.018)	(9.084)	(9.729)
Region trends	NO	YES	NO	YES
Sector trends	NO	YES	NO	YES
Observations	95	95	60	60
R-squared	0.387	0.397	0.567	0.584

Notes: Source Labour Force Survey 1996-2007 collapsed by region (reform of apprenticeship contracts) and by sector (reform of fixed term contracts). Dependent variable is reform dummy, additional controls include year, region and sector dummies. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 3: The effect of reforms on job reallocation by type of contract

	Total employment	Permanent contracts	Fixed term contracts	Apprenticeship	Agency workers	Collaborators
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All firms (N=39857)						
Reform of Apprenticeship	-0.0020 (0.0028)	0.0020 (0.0049)	-0.0022 (0.0149)	0.0313** (0.0147)	-0.0094 (0.0151)	-0.0230 (0.0163)
Reform of Fixed term	-0.0054** (0.0023)	0.0119** (0.0050)	0.0309** (0.0146)	-0.0134 (0.0150)	-0.0040 (0.0157)	0.0220 (0.0160)
Constant	0.114*** (0.0146)	0.113*** (0.0248)	0.505*** (0.0805)	0.259*** (0.0673)	0.225*** (0.0549)	0.498*** (0.0830)
R-squared	0.018	0.018	0.004	0.022	0.053	0.014
Panel B: Non-declining firms (N=22835)						
Reform of Apprenticeship	-0.0063* (0.0034)	-0.0008 (0.0068)	0.0191 (0.0194)	0.0357* (0.0197)	-0.0235 (0.0198)	-0.0284 (0.0213)
Reform of Fixed term	-0.0016 (0.0032)	0.0128* (0.0071)	0.0234 (0.0200)	-0.0293 (0.0212)	-0.0046 (0.0215)	0.0279 (0.0224)
Constant	0.115*** (0.0173)	0.122*** (0.0388)	0.601*** (0.116)	0.206*** (0.0790)	0.0840 (0.0609)	0.433*** (0.106)
R-squared	0.019	0.022	0.007	0.027	0.056	0.014
Panel C: Declining firms (N=17022)						
Reform of Apprenticeship	0.0045 (0.0047)	0.0068 (0.0072)	-0.0335 (0.0234)	0.0199 (0.0221)	0.0047 (0.0234)	-0.0156 (0.0253)
Reform of Fixed term	-0.0107*** (0.0034)	0.0105 (0.0070)	0.0410* (0.0216)	0.0146 (0.0212)	-0.0004 (0.0229)	0.0099 (0.0230)
Constant	0.109*** (0.0253)	0.0972*** (0.0268)	0.383*** (0.103)	0.324*** (0.116)	0.400*** (0.0981)	0.565*** (0.130)
R-squared	0.029	0.021	0.007	0.019	0.054	0.020

Note: The dependent variable is the measure of workers flow defined in the text, applied to the overall firm labour force and by type of employment contract. All regressions include controls for time, region and industry dummies and region- and sector-specific trends. Non declining firms of panel (b) have non-negative employment change between 2004 and 2007, declining firms of panel (c) have negative employment change in the same period. Robust variance estimates account for repeated observation on the same firm over time. Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Robustness checks on job turnover

	Trim firms with high capital change (1)	Controls for VA K and skill ratio (2)	Firm Fixed effects (3)	Total employment growth (4)
Panel A: Total employment				
Reform of Apprenticeship	-0.0024 (0.0028)	-0.0022 (0.0028)	0.0021 (0.0034)	0.0034 (0.0032)
Reform of Fixed term	-0.0049** (0.0023)	-0.0051** (0.0023)	-0.0284** (0.0112)	-0.0094*** (0.0024)
Constant	0.113*** (0.0148)	0.118*** (0.0147)	-3.545** (1.490)	-0.0060 (0.0213)
Observations	38893	39857	39857	39857
R-squared	0.019	0.019	0.011	0.008
Panel B: Apprenticeship contracts				
Reform of Apprenticeship	0.0286* (0.0149)	0.0330** (0.0147)	0.0180 (0.0168)	0.0511*** (0.0143)
Reform of Fixed term	-0.0135 (0.0151)	-0.0144 (0.0150)	-0.0161 (0.0541)	-0.0083 (0.0103)
Constant	0.256*** (0.0683)	0.232*** (0.0686)	-15.85** (7.206)	-0.0036 (0.0655)
Observations	38893	39857	39857	39857
R-squared	0.022	0.024	0.003	0.003
Panel C: Fixed-term contracts				
Reform of Apprenticeship	-0.0013 (0.0150)	-0.0012 (0.0149)	0.0162 (0.0178)	0.0079 (0.0172)
Reform of Fixed term	0.0326** (0.0147)	0.0303** (0.0146)	0.0562 (0.0576)	0.0104 (0.0116)
Constant	0.507*** (0.0810)	0.489*** (0.0811)	-2.779 (7.958)	0.166* (0.0857)
Observations	38893	39857	39857	39857
R-squared	0.005	0.005	0.002	0.003

Note: The dependent variable is the measure of workers flow defined in the text. All regressions include controls for time, region and industry dummies and region- and industry-specific time trends. In column 1 we trim firms with annual capital change below the 1st and above the 99th percentile. In column 2 we add value added, capital and the ratio of white collar to blue collar as regressor; in column 4 we use employment growth rather than job turnover as dependent variable. Robust variance estimates account for repeated observation on the same firm over time. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 5: The effect of reforms on employment

	Total employment	Permanent contracts	Fixed term contracts	Apprenticeships	Agency workers	Collaborators
	(1)	(2)	(3)	(4)	(5)	(6)
Reform of Apprenticeship	0.0013 (0.0031)	0.0065 (0.0047)	-0.00136 (0.0170)	0.0521** (0.0218)	0.0356 (0.0244)	-0.0652*** (0.0206)
Reform of Fixed term	-0.0093** (0.0042)	-0.0025 (0.0065)	0.00409 (0.0236)	-0.0119 (0.0290)	-0.0931*** (0.0320)	0.0858*** (0.0280)
Constant	4.246*** (0.0546)	4.122*** (0.0841)	6.033*** (1.010)	3.320 (4.668)	6.307** (2.459)	5.862*** (1.659)
Observations	53144	52932	31490	14623	17834	19434
R-squared	0.013	0.006	0.016	0.028	0.035	0.016
Number of firms	13287	13286	10844	6480	7175	8704

Notes: The dependent variable is the log of the number of employees by different contract. All regressions include controls for time, region and industry dummies plus region- and industry-specific time trends and firm fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 6: The effect of reforms on capital, investment and the skill ratio

	Capital	Capital per worker	Investments	Investments per worker	Skill ratio
	(1)	(2)	(3)	(4)	(5)
Reform of Apprenticeship	-0.0070 (0.0060)	-0.0080 (0.0064)	0.0125 (0.0749)	0.0148 (0.0754)	-0.0193 (0.0607)
Reform of Fixed term	-0.0265*** (0.0082)	-0.0160* (0.0088)	-0.142 (0.259)	-0.171 (0.261)	0.0140 (0.0834)
Constant	13.90*** (0.109)	9.642*** (0.116)	-93.68 (90.18)	-102.1 (90.79)	-1.690 (1.066)
Observations	52147	52147	15440	15440	53144
R-squared	0.007	0.006	0.015	0.014	0.003
Number of firms	13267	13267	9460	9460	13287

Note: All dependent variables are in logs. Investment has 39,857 observations but many zeros. All regressions include controls for time, region and industry dummies plus region- and industry-specific time trends and firm fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 7: The effect of reforms on labor productivity and Total Factor Productivity

	Value added per worker	Sales per worker	TFP
	(1)	(2)	(3)
Panel A: Contemporaneous reforms			
Reform of Apprenticeship	0.0147*** (0.0048)	0.0092** (0.0044)	0.0162*** (0.0046)
Reform of Fixed term	-0.0279*** (0.0066)	-0.0349*** (0.0060)	-0.0238*** (0.0064)
Constant	9.791*** (0.301)	11.61*** (0.0776)	-0.487* (0.291)
Observations	52,840	53,144	52,675
R-squared	0.008	0.023	0.010
Number of firms	13,275	13,287	13,260
Panel B: Lagged reforms			
Reform of Apprenticeship	0.0131** (0.0052)	0.0039 (0.0047)	0.0146*** (0.0050)
Reform of Fixed term	-0.0274*** (0.0061)	-0.0363*** (0.0056)	-0.0248*** (0.0059)
Constant	9.766*** (0.301)	11.60*** (0.0776)	-0.517* (0.290)
Observations	52,840	53,144	52,675
R-squared	0.008	0.023	0.010
Number of firms	13,275	13,287	13,260

Notes: The dependent variables are in logs, TFP is a residual of a log regression (see text for details). All regressions include controls for time, region and industry dummies plus region- and industry-specific time trends and firm fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Elasticity of substitution between temporary contracts and with permanent contracts

Year	2004-2007	2004	2005	2006	2007
η_ρ (across temporary contracts)	1.392*** (0.148)	1.215*** (0.113)	1.802* (1.023)	1.478*** (0.223)	-0.780 (6.898)
η_σ (between temporary and permanent contracts)	1.062*** (0.254)	1.070*** (0.085)	1.058*** (0.040)	1.060*** (0.220)	1.056*** (0.092)
Observations	53145	13287	13286	13286	13286

Notes: Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.