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Labor market reforms and allocative efficiency in Italy[☆]

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Abstract

This paper examines the extent to which labour market reforms of temporary contracts introduced in Italy at the beginning of the century influenced aggregate productivity via their effects on the efficiency of resource allocation. Using firm-level data from the Italian manufacturing sector, we measure resource misallocation by computing the covariance between firm size and productivity at the sectoral-regional level. We then implement a difference-in-differences approach to study the impact of the reforms, exploiting the cross-region and sector differences in the timing of adoption. Our results suggest that the effect on allocative efficiency depends on the policy considered. While the reform of apprenticeship contracts made the reallocation of resources across heterogeneous firms more efficiency enhancing, the deregulation of the use of fixed-term contracts did not have, on average, the intended results. The apprenticeship reform might have induced more productive firms, in particular, to invest in human capital by hiring workers to whom they provided job training, gaining market shares in so doing. In contrast, the uncertainty related to the newly lawful motives under which firms were allowed to temporarily hire workers might have reduced the incentive to use fixed-term contracts.

Keywords: Allocative efficiency, Resource allocation, Labor market reforms, Italy. *JEL codes:* F10, F14, F36, G20, G32, L25

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1. Introduction

A growing strand of literature has sought to ascertain to what extent differences in aggregate productivity depend on the misallocation of factors of production across firms. The aggregate productivity of an industry or country is related not only to firms' average productivity but also to the allocation of resources (capital and labor) towards the most productive firms (Olley and Pakes, 1996; Hsieh and Klenow, 2009; Bartelsman et al., 2013). Allocative efficiency depends, in turn, on the implementation of appropriate policies that encourage productive inputs to flow to more efficient firms. Some works explicitly focus on possible policy-induced friction, resulting in the reallocation of factors from more to less productive firms, that can be the source of the observed variation in aggregate economic outcomes (Hopenhayn and Rogerson, 1993; Andrews and Cingano, 2014; Petrin and Sivadasan, 2013).

In line with this recent literature, this paper investigates the role of labor market policies in influencing the allocation of inputs across heterogeneous production units. Since the launch of the euro area, many reforms have been adopted across European countries, with the aim of improving the flexibility of the labor market and easing employment protection legislation for workers with temporary contracts (Boeri, 2011). Similarly to in other European countries, a battery of labor reforms was implemented in Italy at the end of the 1990s and the beginning of the 21st century, from the so-called Pacchetto Treu introduced in 1997 to the Biagi law issued in 2003.¹ Our work focuses on the institutional changes in two different types of temporary contracts: the fixed-term, regulated by Decree 368/2001, and the apprenticeship, regulated by the Biagi Law. The implementation of both measures varies in time and across regions and sectors, allowing exploitation of the exogenous variation to identify the impact of these labor market policies through a difference-in-differences approach.

Using data for all Italian limited liability manufacturing firms over the period 1998-2010, we quantify allocative efficiency by decomposing the sectoral productivity into an unweighted average term and a size-productivity covariance term (OP covariance hereafter), following the methodology proposed by Olley and Pakes (1996). The analysis provides evidence that regulations affecting labor markets do influence the OP covariance, i.e., the extent to which resources are efficiently allocated across production units, although the specific effects are very heterogeneous across sectors and geographical areas and strongly depend on the policy considered. In particular, as suggested by the simple theoretical framework sketched in this paper, and inspired by the search and matching literature², screening, training and uncertainty are important elements that need to be taken into account to assess the roles that different types of temporary contracts have in the allocation of inputs across heterogeneous production units.

Regarding the apprenticeship reform, our results suggest that there is an economically and statistically robust positive relationship between the implementation of the law and allocative efficiency: the OP covariance increased, on average, approximately 5% in the period following the reform.³ By implementing a dynamic econometric model that allows us to explore the timing of the effects, we observe that the impact of the reform has not been instantaneous but rather increasing over time. Moreover, we observe some heterogeneous effects across sectors and regions:

¹See Cappellari et al. (2012), Boeri (2011), and Tiraboschi (2004) for a detailed description of the labor market reforms implemented in Italy.

²This literature was pioneered by the works by Diamond (1982), Mortensen (1982), and Pissarides (1985).

³Given that on average across regions and sectors the OP covariance is about 0.681, the estimated 0.05 corresponds to an increase of about 7% in the mean of our dependent variable.

a greater marginal impact is observed among industries and areas where there is a relatively greater share of white-collar workers. In the framework we propose, which shares some features with the model in Acemoglu and Pischke (1999), firms are ex-ante uncertain as to the efficiency of a hired worker, and can only determine it after having paid her wage and a policy-dependent training cost in the first period; moreover, we allow more productive firms to gain more from a trained worker after the initial period. Given this context, the two main mechanisms at work are screening and training. Both productive and unproductive firms can gain from training, but the necessity to screen workers to determine their efficiency results in more productive firms having more incentive to hire apprentices and provide training, generating a positive covariance between size and productivity at the aggregate level. A policy intervention aimed at reducing the training cost would imply more hiring, especially among productive firms.

In contrast to the apprenticeship reform, the new legislation for fixed-term contracts did not generate the intended results: the estimated coefficients for both the static and the dynamic difference-in-difference models are not statistically significant. However, we observe that the average effect hides a great heterogeneity among geographical areas. In particular, our empirical analysis detects that in regions where labor court disputes are longer, and therefore legal expenses higher for the firms, the reform had a negative and statistically significant effect in terms of covariance, while the effect was positive among those regions where court disputes are less lengthy. This finding, which is in line with the evidence provided by Cappellari et al. (2012) on firms' productivity, can be rationalized within our conceptual framework by introducing the possibility that a firm can incur a sanction with some probability, when it decides to let a fixed-term contract expire. As occurred within the Italian context, this sanction might arise when fixed-term legislation makes the requirements for the use of these contracts too generic and, in the case of court disputes, too dependent on judges' interpretations of the norm. The probabilistic nature of the sanction is therefore due to the uncertainty related to judges' discretion. The two main mechanisms at work for this type of contract are, then, screening and risk of incurring a sanction. Because of the uncertainty, the effect of hiring by means of fixed-term contracts in terms of allocative efficiency turns out to be ambiguous.

Our paper relates to various strands within the economic literature. First, it contributes to the literature that links cross-country differences in aggregate-level productivity performance to the misallocation of resources across firms within each country or industry (Hsieh and Klenow, 2009; Bartelsman et al., 2009, 2013).⁴ In a seminal paper, Hsieh and Klenow (2009) assess the role of misallocation in accounting for cross-sectional gaps in productivity among China, India and the United States. Their estimates suggest that if China and India were able to align the efficiency of their resource allocation to that observed in the United States, manufacturing productivity could rise by 30-50% in China and 40-60% in India. Following Hsieh and Klenow (2009), several other empirical analyses provide convincing evidence of the important role played by reallocation in explaining aggregate productivity differences between countries or industries (See Bartelsman et al., 2009, 2013; Andrews and Cingano, 2014, among others).⁵ For Italy, the works by Calligaris (2015), Linarello and Petrella (2017), and Bugamelli et al. (2018) highlight the relevant role of allocative efficiency in shaping productivity dynamics.⁶

⁴See Restuccia and Rogerson (2013) for a detailed survey of this literature.

⁵More recent studies analyze the role of allocative efficiency as a possible determinant behind the productivity slowdown experienced by many advanced economies (Cette et al., 2016; Gopinath et al., 2017).

⁶According to Linarello and Petrella (2017) resource allocation, as measured by the OP covariance between size and productivity, accounts for about 35 percent of aggregate labor productivity in 2005 and its relevance has

While most of these papers take an agnostic view as to the specific source of such distortions, a parallel line of research focuses more explicitly on the effect of specific policies that can promote or inhibit the reallocation of factors from less to more productive firms. Frictions in product, labor, and credit markets have all been considered as possible sources of misallocation (Hopenhayn and Rogerson, 1993; Banerjee and Duflo, 2005; Barseghyan and DiCecio, 2011; Boedo and Mukoyama, 2012; Buera et al., 2011). Looking at labor market imperfections, employment protection (EP) regulation is one feature that has attracted much attention.⁷ While there is a large literature that looks at the impact of EP on employment rates and workers' conditions, the knowledge base for assessing the links among employment protection, labor mobility and productivity growth is still limited and less clear cut.⁸ Hopenhayn and Rogerson (1993) show that firing taxes distort the allocation of labor across establishments and estimate total factor productivity (TFP) losses from this tax on the order of about 5%. Lagos (2006) uses a matching model and shows the effect that stringent EP might have on average productivity via selection effects. Petrin and Sivadasan (2013) and González and Miles-Touya (2012) assess the degree of resource misallocation at the firm level and look at the effect of a change in severance payments on allocative efficiency. The existing theoretical and empirical literature focuses almost exclusively on the role of dismissal restrictions, with little attention given to rules for temporary contracts. As far as temporary contracts are concerned, existing empirical analyses mainly consider the consequences of such type of work arrangements in terms of employment and earnings.⁹ More recent papers focus on the effects of such type of work arrangements on firms' productivity, observing that firms' efficiency might be either positively or negatively related to work on a temporary basis.¹⁰ Our paper contributes to this line of research by considering the effects that the labor market reforms on temporary contracts applied in Italy at the beginning of the century have had on aggregate productivity through their impact on resource allocation.

The rest of the paper is structured as follows. Section 2 provides a broad picture of the atypical contractual arrangements within the Italian labor market, describes the two main reforms that were implemented at the beginning of the century, and sketches a conceptual framework useful for rationalizing the empirical analysis. Section 3 describes the dataset used in the empirical analysis and the approach we follow to measure the extent of allocative efficiency. Section 4 presents some preliminary evidence on the effects of the two reforms on firms' size and productivity. Section 5 focuses on the analysis of the relationship between the degree of allocative efficiency and the implementation of the labor market reforms. Section 6 concludes.

increased to 42 percent between 2005 and 2013.

⁷The early literature on EP and legal restrictions against dismissals has been reviewed in several chapters of the Handbook of Labor Economics (Bertola, 1999; Nickell and Layard, 1999; Boeri, 2011). Among more recent papers, see Boeri and Garibaldi (2007); Garibaldi and Violante (2005); Cingano et al. (2016).

⁸See Martin and Scarpetta (2012) for a critical review of the recent empirical evidence on the links between regulations affecting the hiring and firing of workers, labor reallocation and productivity growth.

⁹The effect of temporary jobs on employment rates have been investigated, among others, by Booth et al. (2002); Blanchard and Landier (2002); Bover and Gomez (2004); Alonso-Borrego et al. (2005); Ichino et al. (2008); de Graaf-Zijl et al. (2011).

¹⁰See, among others, Booth et al. (2002); Engellandt and Riphahn (2005); Ichino and Riphahn (2005); Autor et al. (2007); Cappellari et al. (2012); Dolado et al. (2016).

2. Institutional background and conceptual framework

Temporary contracts have become a regular component of the European labor market policies, and their sustained growth has raised questions about their economic impact. As reviewed in Section 1, while existing studies focus mainly on the effects of such type of work arrangements in terms of employment, earnings, and productivity, an empirical evaluation of the relationship between liberalization of temporary contracts and aggregate productivity through their impact on resource allocation is still lacking in the literature so far. The aim of the next sections is, first, to present the type of atypical contractual arrangements characterizing the Italian labor market (Section 2.1), then introduce the two main reforms implemented in the country at the beginning of the century concerning fixed-term and apprenticeship contracts (Section 2.2) and finally propose a simple theoretical framework to rationalize how the implemented reforms may have affected the allocation of resources across firms (Section 2.3).

2.1. *Atypical contractual arrangements in Italy*

Italy is a country in which a large number of atypical contractual arrangements, including fixed-term contracts, apprenticeships, collaborators and agency work, coexist with standard employment contracts characterized by high social security.

Fixed-term contracts differ from open-ended contracts because they have a pre-determined duration, enabling the firm to terminate a contract at will and without cost, albeit on a given date. The legislation provides specific circumstances under which fixed-term contracts could be used in place of open-ended ones; moreover, the law limits the possibility for the employer to renew temporary engagements with a given employee and the cumulative duration of this type of contracts in the same firm (Barbieri and Sestito, 2008). The apprenticeship differs from other types of temporary contracts because it is aimed at increasing human capital through firm-provided training. Because of this training purpose, these contracts are characterized by longer durations than those in standard temporary contracts, and they are typically associated with greater attachment of workers to firms (Picchio and Staffolani, 2019; Albanese et al., 2017; Viviano, 2014). Firms can use these contracts only for younger workers, and they transmit professional competences to youths by practical on-the-job training. The training accounts for both basic skills and technical competences, it is mostly firm-based, and it focuses on the practical activities on-the-job. The obligation for the employer to provide workers' with training is compensated by lower social security contributions.¹¹ In principle, since apprentices constitute a cheap labor force, firms might use them just as a more flexible form of employment contract. On the other hand, training young workers during the apprenticeship period might represent a long-term investment, and a way of screening workers' efficiency before offering them permanent contracts. This argument is stronger in countries, such as Italy, with a high EPL for permanent workers, where it is particularly important for firms to assess workers' quality before becoming locked in an open-ended job relationship (Picchio and Staffolani, 2019). The empirical literature confirms that in Italy the apprenticeship contract has long-run positive effects on workers' wages and employment probabilities, especially within large companies (Albanese et al., 2017; Picchio and Staffolani, 2019), thus suggesting that apprenticeships truly involve meaningful amounts of training, resulting in higher human capital accumulation.

¹¹Employers not complying with the requirement of providing training have to pay back twice the tax exemption received, and possibly convert the apprenticeship to an open-end contract (Albanese et al., 2017).

Together with fixed-term and apprenticeship contracts, which are the focus of our empirical analysis, there are other types of arrangements that can be implemented. For example, firms can use external workers supplied by temporary agencies; these workers must receive the same pay and the same working conditions as formal employees of the same firm. The main advantage to a firm in using this type of contract is the flexibility it entails, and the main disadvantage that it is more costly, as intermediating agencies themselves must be remunerated. Additionally, firms can rely on collaboration contracts, which also feature a flexible termination clause. Although they share many characteristics with formal employees (often being engaged by just one firm, working on the firm’s premises, etc.), it must be stressed that collaborators are not, formally, employees of the firm.

2.2. *Fixed-term and apprenticeship contract reforms*

In Italy, several reforms have progressively increased the so-called “flexibility at the margin;” i.e., they have eased regulation of the utilization of non-permanent contracts, while leaving the terms of standard employment unchanged. Our empirical analysis focuses on the two main reforms of the Italian labor market implemented at the beginning of the 2000s: Legislative Decree 368/2001, which reformed fixed-term contracts, and Law 30/2003 (the so-called “Biagi” reform), which reformed the apprenticeship contract.¹²

The reform of fixed-term contracts entailed three main changes with respect to the prior legislation. First, it expanded the scope of occupations allowed under fixed-term contracts: while the previous legislation required firms to provide very specific circumstances under which fixed-term contracts could be used (e.g., production peaks or sick leave replacements), under the new law firms were henceforth required only to provide very general “technical, organizational, production or replacement” reasons for using this type of contract (Article 1). This new legislation made the requirements for the use of these contracts too generic and, in the case of court disputes, too dependent on judges’ interpretation of the norm, with the risk of a conversion to a permanent contract. Second, the new law regulated the duration, stipulating that a fixed-term contract with a duration of less than three years could be renewed only once and only when there was an objective reason, and that the total period under which a worker could be hired under this type of contracts did not have to exceed three years (Article 4). Third, the decree reduced unions’ power in deciding the uses of fixed-term contracts; indeed, before the enactment of the decree, expansion of the scope of these types of contracts was allowed only by means of collective agreements; nevertheless, the new law maintained unions’ right to fix the maximum percentage of fixed-term workers of firms’ total employment. Therefore, depending on when collective agreements were renegotiated, the Decree implied different applications across sectors over years. In some sectors, the new fixed-term contract laws were implemented in 2005, after collective bargaining, while in other sectors the adoption of the law was postponed. The top panel of Table 1 shows the sectors in which the reforms have been applied starting from 2005.

Overall and despite its intention, Legislative Decree 368/2001 did not promote employers’ use of fixed-term contracts. Indeed, the decree maintained a large number of protective measures on the use of fixed-term contracts in terms of duration, workers’ rights, and employers’ obligations. Moreover, because the law was too reliant on judges’ interpretations of firms’ justifications for employing fixed-term workers, the resulting uncertainty reduced the incentives to adopt these forms of employment (Aimo, 2006; Cappellari et al., 2012).

¹²In the following description of the reforms we mostly refer to Cappellari et al. (2012).

Table 1: The Italian labor market reforms

Starting from	Applied to sectors	Applied to regions
<i>Fixed term contract Reform</i>		
2005	Food and Beverages, Textiles, Wearing Apparel, Leather Wood and Furniture, Chemicals	All All
<i>Apprenticeship contract Reform</i>		
2005	All	Emilia Romagna, Toscana
2006	All	Alto Adige, Sardegna, Puglia, Friuli Venezia Giulia, Marche
2006	Food and Beverages, Textiles, Wearing Apparel, Leather Wood and Furniture, Printing Chemicals, Metal Manufacturing	Other regions
2007	All	Lazio
2008	All	Piemonte
2010	All	Umbria, Campania

As for the reform of the apprenticeship contract, it was aimed at introducing additional liberalization to a type of contract that already entailed lower labor costs to firms (as compensation for incurred training costs). First, the new legislation extended the upper age limit of the contract from 25 to 30. Second, it further reduced the cost of apprenticeships to firms, by introducing the option of performing part of the training at the workplace instead of apprentices taking external training courses, and it abolished the requirement to certify the obtained qualifications. As a consequence of the reform, there was a shift from a scheme based on general and specific training (the old apprenticeship) to a scheme based only on specific training (the new apprenticeship) (d’Agostino et al., 2019).¹³ Finally, the Biagi reform allowed each Italian region to regulate the amount and quality of the training. Therefore, for the new apprenticeship contract law to become effective, local authorities had to issue regional regulations. From 2003 to the end of 2010, only 11 of 20 regions adopted the guidelines on the training content of the new apprenticeship contracts. The regions that first implemented the new rules were Emilia-Romagna and Toscana in 2005; Friuli Venezia Giulia, Marche, Sardegna, Puglia and Alto Adige in 2006; Lazio in 2007; Piemonte in 2008; and Umbria and Campania in 2010. Where there was no regional legislation, Law no. 80/2005 stated that industry-specific collective agreements could set rules for the regions, implying variability not only across localities but also across industries. The bottom panel of Table 1 summarizes the sectors and regions in which the reform has been applied.

2.3. Effects of fixed-term and apprenticeship contracts on allocative efficiency

While there is a wide literature on the impact of severance pay or, more broadly, firing costs on allocative efficiency, little attention has been given to the effect of the adoption of specific types of temporary contracts.¹⁴ As far as employment protection is concerned, several theoretical models suggest that to the extent EP raises labor adjustment costs, it is likely to have a negative impact

¹³It might be difficult for the authorities to monitor firms’ compliance with the training requirements. However, non-compliance also occurred for the external training in the previous scheme due to lack of funding (Albanese et al., 2017). As noted in Section 2.1, firms have an incentive to invest in training, both to improve workers’ efficiency and to screen workers’ quality.

¹⁴For example, Caggese and Cuñat (2008) consider the interaction between financial constraints and firms’ use of permanent and temporary contracts, but model permanent contracts as being different from temporary ones only in entailing firing costs, differently from the latter.

on efficient workforce adjustments and on the allocation of resources, with negative implications for aggregate economic outcomes (Bentolila and Bertola, 1990; Bertola, 1990; Hopenhayn and Rogerson, 1993; Poschke, 2009). From an empirical point of view, a number of studies have found consistent evidence of a negative effect of EP on allocative efficiency (Bassanini et al., 2009; Andrews and Cingano, 2014; González and Miles-Touya, 2012; Petrin and Sivadasan, 2013).

Turning to the issue of temporary contracts, it has been argued (see OECD, 2010) that asymmetric liberalization that leaves in place stringent regulations on permanent contracts while relaxing those for temporary ones (so-called “flexibility on the margin”) may distort the optimal composition of employment and reduce workers’ involvement in training (Bassanini et al., 2007) and their work commitment (Dolado et al., 2016) and that this, in turn, could have a negative impact on allocative efficiency and productivity.¹⁵ At the same time, however, by providing firms with more flexibility in choosing the arrangement that better fits their labor force needs, temporary contracts might favour those reallocation processes triggered by shocks in technology or demand that call for job change.¹⁶ Moreover, whenever temporary contracts are related to the provision of job training, they can stimulate firms’ investment in human capital and their overall efficiency, especially among the most productive firms that can gain market share by hiring highly motivated and career-oriented workers (Acemoglu and Pischke, 1999; Dearden et al., 2006; Konings and Vanormelingen, 2015).¹⁷ This type of labor provision can indeed improve aggregate productivity by favoring reallocation mechanisms.

Given the various mechanisms that can be at work, the theoretical link between the regulations of temporary workers and allocative efficiency is not straightforward and might depend on several aspects, such as the type of contract issued by the firm, the provision of job training, and the economic context in which firms are operating. To rationalize how the new contract forms introduced by the two specific reforms under scrutiny may have impacted aggregate productivity via resource reallocation, we propose a simple theoretical framework inspired by the search and matching (S&M) literature (Diamond, 1982; Mortensen, 1982; Pissarides, 1985). For the purposes of our discussion, we keep the framework as simple and clear as possible. In particular, to simplify the framework, we focus only on the two forms of contracts that are addressed in the reforms under scrutiny, even though many other contract forms are available to firms in the Italian labor market, as discussed in Section 2.1. Within the context of our simple S&M model, a match is intended to be the result of one firm and one worker starting production; that is, it can also be viewed as a single project started by a multi-worker firm. Projects and workers are randomly matched with each other among a potentially infinite mass of entrepreneurs, and a unit mass of workers. We focus only on the firm side of the matching market; we assume that firms are risk neutral, but differ in terms of how productive a match can result. Formally, firms produce using only (units of efficient) labor (y_j), and workers can be either efficient (y_E) or inefficient (y_I), with $y_E > y_I$. The production function of each firm is given by

$$f_i(y_j) = k_i y_j$$

¹⁵In addition, it could be suggested that allocation should be more efficient with rigid rather than flexible contracts because firms would only hire when they are certain that the match is productive.

¹⁶In the same spirit of the adjustment channels investigated in the context of jobs security provisions by Bentolila and Bertola (1990); Bertola (1990); Hopenhayn and Rogerson (1993).

¹⁷In their study on the relationship between training participation and productivity across different industries, Dearden et al. (2006) find that an increase of 1% in their training measure is associated with an increase in value added per hour of about 0.6%, implying productivity premiums of over 60%. Konings and Vanormelingen (2015) show that the productivity premium for a trained employee is on average 17%.

where k_i is the productivity of either a productive firm (k_P) or an unproductive one (k_U): a productive firm obtains a higher proportion of output from the same units of efficient labor employed, compared to an unproductive one. To model the fact that more productive firms may have better screening capabilities, we assume that there is a higher probability that a productive firm hires an efficient worker

$$P(y = y_E | k = k_P) = \lambda_P > \lambda_U = P(y = y_E | k = k_U).$$

Assuming there is free entry into vacancy posting, the value of a vacancy is taken to be 0 in what follows, i.e. firms earn 0 after a match is broken and before a new match is formed.

When deciding whether to hire a worker to start a new project, firms can employ two kinds of contracts: apprenticeship contracts, or fixed-term contracts. As discussed in Section 2.1, the apprenticeship contract differs from the fixed-term contract because of the training content and the longer duration, which allow workers to accumulate human capital. It follows that we can reasonably assume that each type of contract serves different purposes: e.g., fixed-term contracts might be used for short-term production needs as a buffer to face demand volatility, while apprenticeships might serve as a monitoring and training tool to created high-quality permanent jobs for longer-term projects (Picchio and Staffolani, 2019). We therefore consider separately the decisions whether to hire an apprentice and whether to employ a fixed-term worker.

Apprenticeship contracts involve an additional cost t , other than the wage, in the current period; however, the worker becomes trained and her productivity rises to $a y_j$ ($a > 1$) from the next period onwards. In the initial period, when training occurs, workers produce nothing, yet firms must pay their salaries and the additional training cost t . After the initial training period, when a firm can also gauge the hired worker's productivity, it can decide whether it is worth it to keep the trained worker by offering her a permanent contract or to break the match by letting the contract expire. The (ex-post) value of a match formed by means of an apprenticeship contract is therefore

$$J_{ij}^A = -(w + t) + \beta \max \left\{ \frac{J_{ij}^{A,0}}{1 - \beta}, 0 \right\}$$

where

$$J_{ij}^{A,0} = k_i a y_j - w$$

is the per-period profit a firm obtains from a trained worker.

Fixed-term contracts, unlike apprenticeship contracts, involve no training in the workplace; hence, there are neither additional costs in the current period nor future additional benefits. After one period, a firm can decide whether to offer the worker a permanent contract or let the fixed-term contract expire.¹⁸ At this point, a firm can incur (with probability ϕ) a sanction that, for sake of exposure, we model as generic “unmotivated firings.” This sanction might arise when, as occurred in the Italian context (Section 2.2), the fixed-term legislation makes the requirements for the use of these contracts too generic and, in case of court disputes, too dependent on judges' interpretation of the norm. If a fixed-term contract is not later converted into a permanent one, the employee can indeed sue the employer for not having met one of those generic requirements.

¹⁸Actually, the new legislation reformed the previous one by allowing firms to renew fixed-term contracts once, provided that a total period of three years would not be exceeded. Here, for sake of exposure, we model a firm choice of whether to let the contract expire, or offer the worker a permanent position at the firm, to occur after one period.

The probabilistic nature of the sanction is therefore due to the uncertainty related to judges' discretion.

If the firm incurs a sanction, it must keep the worker in the workplace until the dispute is settled and pay an additional cost c for dispute settlement.¹⁹ Let ψ_r be the probability that the dispute is not settled in the current period, where we allow it to take different values across regions (to allow for different court efficiencies); the (ex-post) value of a match formed by means of a fixed-term contract is therefore

$$J_{ij}^F = J_{ij}^{F,0} + \beta \max\{J_{ij}^{F,H}, \phi J_{ij}^{F,S}\}$$

where

$$J_{ij}^{F,0} = k_i y_j - w$$

is the (ex-post) value to a firm deciding to let the worker go and not sanctioned,

$$J_{ij}^{F,H} = \frac{1}{1 - \beta} J_{ij}^{F,0}$$

that to a firm deciding to permanently hire the worker, and

$$J_{ij}^{F,S} = \psi_r \sum_{t=0}^{\infty} \beta^t \psi_r^t (J_{ij}^{F,0} - c) = \frac{\psi_r}{1 - \beta \psi_r} (J_{ij}^{F,0} - c)$$

that to a firm deciding to let the worker go and receiving a sanction.

Now, to rationalize wage rigidity in the Italian labor market, we assume that wages are Nash bargained each period at a centralized level (e.g., based on the surplus originated by the median worker and the median firm);²⁰ once a match is formed, the wage remains the same until the match is broken. As a result, individual firms and workers take wages as given when deciding whether to form a match, so that we can take w as exogenous (and constant over time from the perspective of a firm) in the value functions above. For the purposes of the following discussion, we can remain agnostic as to which is the actual equilibrium wage of the model; the point to observe is that centralized wage bargaining makes the bargained wage too high for unproductive firms and too low for productive firms (relative to the case where wages would be negotiated at each firm in a decentralized way).

Finally, firms cannot observe a worker's actual efficiency before starting production and make hiring decisions based on the (interim) expected value of a match; i.e., as for apprenticeship contracts,

$$J_i^A = \mathbb{E}(J_{ij}^A) = \lambda_i J_{iE}^A + (1 - \lambda_i) J_{iI}^A$$

and, as for fixed term contracts,

$$J_i^F = \mathbb{E}(J_{ij}^F) = \lambda_i J_{iE}^F + (1 - \lambda_i) J_{iI}^F.$$

Based on our assumptions and on suitable parameter values, different contracts generate different incentives to different types of firms.

¹⁹Complex and time-consuming legal processes can add significantly to the cost of hiring and dismissing this type of workers (Venn, 2009).

²⁰Boeri et al. (2019) show how collective and centralised wage bargaining induces labor misallocation across Italian regions with different average productivity levels.

As far as apprenticeship contracts are concerned, we assume that a productive firm is able to offset the shortcomings of an inefficient match, i.e.

$$k_P y_I < w + t$$

but

$$k_P a y_I > w$$

while unproductive firms are not able to sufficiently train an inefficient worker,

$$k_U a y_I < w$$

and are able to do so only if a worker is efficient, i.e.

$$k_U y_E < w + t$$

but

$$k_U a y_E > w.$$

Given our assumptions, the two main mechanisms at work are *screening* and *training*. Both firm types can gain from training, but the necessity to screen workers to determine their efficiency results in more productive firms having more incentive to hire apprentices and provide training, as they can profitably train both efficient and inefficient workers. In contrast, unproductive firms can profitably train only efficient workers; therefore, the uncertainty as to which worker type they will hire makes them more reluctant to hire by means of apprenticeship contracts and induces them to permanently hire an apprentice only if she turns out to be efficient. Given the above, more workers are hired by means of apprenticeship contracts at more productive firms, generating a positive covariance between size and productivity at the aggregate level. Moreover, a policy intervention aimed at reducing the training cost t , would imply more hirings, initially, at both productive and unproductive firms; however, then only productive firms will permanently keep both worker types, while unproductive firms will lay off inefficient workers.

Focusing now on fixed-term contracts, we assume that efficient matches are profitable for both productive and unproductive firms, while inefficient matches are unprofitable to both productive and unproductive firms.

$$J_{iE}^{F,0} = k_i y_E - w > 0$$

$$J_{iI}^{F,0} = k_i y_I - w < 0$$

$i \in \{P, U\}$. We assume, though, that interim values are always greater than zero, so that both firm types have incentive to hire. Of course, productive firms will always have higher incentives to hire compared to unproductive firms located in the same region. Moreover, in regions where the court system is slower, incentives to hire are reduced for both firm types.

As a result, the two main mechanisms at work for this type of contract are *screening* and *risk* of incurring a sanction (if screening results in hiring an unproductive worker). If the courts are sufficiently efficient (so that ψ_r is low), neither firm type will find it too costly to employ workers and eventually discharge them if they are not efficient enough. In particular, if the hired worker turns out to be efficient, then she will be offered a permanent contract. However, the hired worker might turn out to be inefficient; in this case, many possibilities arise, depending on how productive a firm is and where it is located. In regions where courts are more efficient, a firm is more willing to incur the risk of letting the contract expire, and then keep screening for more efficient workers;

in this respect, these incentives are higher for more productive firms (as they have better screening capabilities and can obtain higher production from any match). However, in regions where courts are slower, a firm may find it more attractive to keep an inefficient worker in the workplace, rather than incurring the additional costs involved in dispute settlement for a long period.

Given these contrasting possibilities, the aggregate effect of hiring by means of fixed-term contracts turns out to be ambiguous. Moreover, policy interventions that have the unintended consequence of increasing the risk of incurring a sanction (ϕ), will reduce the ex-ante value of a match to all firms, discouraging them from hiring workers by means of this contract arrangement in the first place. Given that the ex-ante value of a match is increasing in k_j and decreasing in ψ_r , less productive firms located in regions with relatively slower courts will be particularly discouraged in that respect. As a result, one can expect more projects to be activated by means of fixed-term contracts only at more productive firms and only in regions where courts are relatively faster: at the aggregate level, this would imply a positive covariance between size and productivity only in those regions.

3. Allocative efficiency and descriptive statistics

In this section, we first present the data used and some preliminary statistics; we then describe the approach we follow to measure the extent of allocative efficiency.

3.1. Data and descriptive statistics

Our empirical analysis draws upon two different sources of micro-level data: the Italian Company Account Data Service (Centrale dei Bilanci, CB) which collects annual administrative reports for all Italian limited-liability firms,²¹ and the Italian National Social Security Institute (INPS), which consists of the universe of firms with at least one employee. To avoid confounding effects with other overlapping labor market reforms that took place beginning in 2011, the time period considered in the empirical analysis is 1998 to 2010. Indeed, the apprenticeship was again affected by legislative changes in 2011 (D. Lgs. n.167/2011, the so-called Consolidated Law on apprenticeship) and in 2012 (Law 92/2012).²² The merged dataset contains information on firms' location, industry classification (Nace rev.2), number of employees, occupational classification (blue-collar workers, apprentices, managers and office workers),²³ value added, intermediate input costs and

²¹Italy offers a wide range of legal forms for setting up companies, depending on the company's organization model, its business objectives, the level of capital to be committed, the extent of liability of the founders, as well as tax and accounting implications. Italy's corporate law primarily differentiates between limited liability companies, one of the most popular business types in the country, and partnerships. There are two main types of limited liability companies: private limited companies by quotas (or S.r.l) and public limited companies by shares (or S.p.A). Limited liability companies are firms where each owner's liability is limited to the cash or assets he/she has contributed to the company, and they are characterized by the separation of ownership and managing powers. Differently from limited liability companies, partnerships do not have legal personality, meaning that founders' identities are not separate from that of the partnership. The commitment of the partners, their shared goals, and their unified experience, contribute to the success of the collaboration and the business.

²²As documented by Viviano (2014), the objective of the 2011 Consolidation Law was to define the professional and training standards of the apprenticeship. While providing for the possibility of dismissal, the law has also established that the apprenticeship is a permanent contract. Also within this law the discipline of the different forms of apprenticeship and the identification of the relative training plans are delegated to the agreement between different subjects such as the trade unions of category and the regions. The law 92/2012 has established the minimum duration of the contract and the numerical limits to the use of apprentices, which vary depending on the size of the company.

²³This is the finest level of disaggregation available within the dataset.

capital assets. Capital is proxied by tangible fixed assets at book value (net of depreciation). Given our focus on within-industry allocative efficiency across manufacturing sectors, all non-manufacturing firms are excluded from our final sample. Moreover, observations with negative values for one of the key variables (i.e., output, capital, labor, value added) are dropped from the sample.²⁴ Real value added and real capital stocks are computed using 2-digit industry-level deflators provided by the Italian Statistical Office (ISTAT).

To measure firms' efficiency, we use total factor productivity (TFP) computed by estimating, for each two-digit sector,²⁵ input elasticities of the following Cobb-Douglas production function (in logs)

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_{sw} sw_{it} + \beta_{app} app_{it} + \beta_{bc} bc_{it} + \omega_{it} + \eta_{it} \quad (1)$$

where y_{it} is value added of firm i at time t and k_{it} the real capital stock employed in production. Labor is disaggregated into skilled workers, sw_{it} (including managers and office workers), apprentices (app_{it}), and blue-collar workers (bc_{it}). Because the labor market reforms under scrutiny might well affect the labor composition within each firm and the relative costs of different factors, failing to distinguish between worker types might lead to biased productivity estimates.²⁶ The measures of labor inputs are given by the logarithm of each variable plus one, to keep in the dataset firms reporting no worker of any type. Capital is proxied by tangible fixed assets. As a robustness check, reported in the Appendix, we also measure the capital stock using a perpetual inventory method, obtaining an elasticity of capital that is approximately 20% on average. However, by using the perpetual inventory method, we lose approximately 13% of observations due to missing values in the information on firms' annual investment and accumulated depreciation. Finally, the error term in equation (1) can be decomposed into a productivity shock, ω_{it} , observable to firms but not to the econometrician, and an i.i.d. component η_{it} .

As surveyed by Van Beveren (2012), estimating firm-level TFP requires contending with a number of econometric issues: a simultaneity bias (as input choices may be influenced by productivity changes observed by a firm but not by the econometrician), a potential omitted variable bias, and a sample selection bias (because firms with higher capital stock can better withstand negative productivity shocks without exiting). Following the recent literature, we compute firm-level TFP using Wooldridge's (2009) approach, which builds upon Levinsohn and Petrin (2003) to address the simultaneity bias problem. Similarly to Olley and Pakes (1996), Levinsohn and Petrin's (2003) approach has the advantage of controlling for the simultaneity bias without having to rely on instruments.²⁷ Moreover, Wooldridge's (2009) correction is able to overcome the

²⁴We exclude from the analysis sectors 12 (Tobacco), 19 (Manufacture of coke and refined petroleum products), 21 (Pharmaceutical), 32 (other manufacturing) and 33 (repair machinery). While the economic trend of coke and petroleum products is strongly related to material prices, that of pharmaceutical goods is affected by health budgets. Residual and marginal manufacturing activities (32-33) are excluded because their trend is difficult to interpret. See also Ciani et al. (2018) for the choice of excluding these sectors.

²⁵Although our analysis focuses on resource misallocation within industries and regions, we estimate the production function at industry level only. Moreover, production function coefficients are estimated at the sector rather than sector-year level. These choices are driven by the fact that there are not enough companies in all regions for all sectors, and in all sectors within each year. Moreover, there are no reasons to assume that production functions differ across regions for the same industry.

²⁶We wish to thank an anonymous referee for suggesting this improvement in the estimation of production functions.

²⁷Levinsohn and Petrin (2003) consider labor inputs as flexible variables, capital as state variable, and materials as a proxy for productivity shocks ω_{it} . They assume that productivity evolves exogenously following a first order

Figure 1: Firms' productivity distribution normalized by sector, computed using the Wooldridge (2009) methodology and labor productivity.

identification problem highlighted by Akerberg et al. (2015). It should be noted that, to properly measure firms' productivity, one would ideally observe the quantity of output produced by a firm. Because this information is not reported in balance sheet data, to partially solve this problem the empirical literature has used deflated sales (or value added) as a proxy for firm production, assuming that goods produced by firms in a given industry are homogeneous. Using deflated quantities to calculate firm-level TFP can, however, lead to confounding higher prices with higher productivity. The productivity obtained as a residual from an estimated production function has therefore been considered as a measure combining real productivity and pricing strategies.

Figure 1 compares the estimated (log) TFP distribution with the (log) labor productivity (computed as value added per employee). Although our preferred measure is the Wooldridge (2009) version of Levinsohn and Petrin (2003), the figure confirms that the shape of firms' productivity distribution does not change much by using these two different approaches. This is fairly expected, as the literature has often found more productive firms to appear so, regardless of the methodology used (Syverson, 2011; Van Beveren, 2012).

Table 2 provides descriptive statistics from our firm-level sample, distinguishing among firms exposed to the apprenticeship reform, those exposed to the fixed-term reform, and those not exposed to any treatment.²⁸ After cleaning the data and keeping only manufacturing firms, we end up with an unbalanced panel that increases in size from 69,523 firms in 1998 to 84,534 in 2010. Our dataset covers approximately 20% of all Italian manufacturing firms, representing almost 80% of total manufacturing value added, and accounting for approximately 60% of total employment.²⁹ This picture is explained by the well known abundance of micro and small firms in

Markov process and that firm's intermediate inputs demand depends on the observed productivity and on the current stock of capital. Moreover they demonstrate that, under perfect competition in output markets and competitive inputs markets, this demand is monotonically increasing in productivity, conditional on capital. Therefore, it is possible to use a firm's intermediate inputs to proxy ω_{it} . The procedure is outlined in more detail in the Appendix.

²⁸Treated firms are those operating in those sectors and in those regions affected by the two reforms. Since a firm may be exposed to both regulations, the sum of the numbers in the three groups exceeds the number in the full sample.

²⁹To assess the reliability of our dataset, Figure A1 in the Appendix compares the average annual labor pro-

Table 2: Data and statistics - firm-level

	Full sample	Never treated	Reform of apprenticeship	Reform of fixed term
N. observations	1,043,226	621,456	398,687	176,621
1998	69,523	69,523	0	0
1999	71,847	71,847	0	0
2000	74,420	74,420	0	0
2001	76,787	76,787	0	0
2002	79,039	79,039	0	0
2003	80,694	80,694	0	0
2004	82,996	82,996	0	0
2005	83,826	44,025	16,718	29,706
2006	85,068	10,190	74,878	29,917
2007	85,516	9,248	76,268	29,930
2008	84,323	7,988	76,335	29,066
2009	84,653	7,999	76,654	29,070
2010	84,534	6,700	77,834	28,932
Total employment	31.2	33.3	28.0	25.5
% White collars	26.6	26.2	27.4	25.6
% Blue collars	68.1	68.7	67.1	69.4
% Apprenticeships	5.3	5.1	5.5	5.0
Value added	1829.7	1902.0	1731.0	1527.0
Tangible fixed assets	1739.6	1865.9	1542.8	1597.2
(log) TFP	4.0	4.0	4.0	3.9
% North	64.2	65.8	62.1	52.1
% Central	18.9	17.3	21.6	25.0
% South	16.9	16.8	16.3	22.9

Notes: This table reports average values for some variables between 1998-2010, grouping firms on the basis of their exposure to the apprenticeship and fixed-term reforms. Value Added and Tangible fixed assets are in thousands of Euros. The TFP is estimated using the Wooldridge's (2009) approach and disaggregating the labor inputs into skilled workers (managers and office workers), apprentices and blue-collar workers. Nominal values are deflated using 2-digit sector specific deflators.

Italian manufacturing, together with the observation that the limited-liability legal status tends to be more common across medium-larger firms, and these account for the great bulk of overall activity. The figures reported in Table 2 are very similar to those provided by Cappellari et al. (2012) for a smaller sample.³⁰ Because the reforms started to be implemented in 2005, there were no firms exposed to the new legislation from 1998 to 2004. The number of firms treated by the apprenticeship reform increased considerably from 2005 onwards: as detailed in Table 1, while in 2005 only two regions adopted the regulation, from 2006 the law has been extended to a set of new regions and to specific sectors in other geographical areas. As far as the reform of fixed-term contracts is concerned, this has entered into force in all regions but only a limited number of

ductivity growth rate obtained from our sample with the one observed by Linarello and Petrella (2017) using the FRAME-SBS dataset, an integrated firm-level census that covers all active firms. The evolution of the average productivity growth for the CERVED-INPS data is quantitatively and qualitatively similar to the one observed when using the universe of Italian firms (FRAME-SBS), assuring the goodness of our dataset.

³⁰The work of Cappellari et al. (2012) is based on a survey conducted by Unioncamere (the Association of Italian Chambers of Commerce) on a representative sample of 13,000 firms for the period 2004-2007.

industries since 2005. In this case the number of firms belonging to the treatment group is fairly constant over time.

In terms of average number of employees, Table 2 shows that the average firm size in our sample is approximately 31. As expected, since our sample covers only limited liability firms, this statistic is relatively larger than that reported by Linarello and Petrella (2017) for the universe of Italian manufacturing firms (which is approximately 9.3). Grouping firms on the basis of their exposure to the reforms, we observe that those firms in the two treatment groups are slightly smaller, on average, than those in the never-treated group.³¹ The workforce composition is very similar across treated and non-treated firms, with a marginal difference in the shares of apprentices and skilled workers, which appear to be higher for those firms exposed to the apprenticeship reform. Tangible fixed assets and real valued added are slightly higher among the never-treated firms, while there are essentially no differences in terms of firms' productivity across treatment groups. As for geographical distribution, most firms are located in the North, reflecting the well-known regional heterogeneity of economic activity in Italy.

3.2. Allocative efficiency

Starting from firm-level TFP, we now turn to computing a measure of allocative efficiency. Among various alternative ways to measure the degree of within-industry allocative efficiency,³² this work follows the approach first proposed by Olley and Pakes (1996), focusing on the covariance between firm size and productivity. Bartelsman et al. (2013) argue, both theoretically and empirically, that since more productive firms tend to be larger than less productive ones, the within-industry covariance between size and productivity is a robust measure to assess the extent of misallocation. Olley and Pakes (1996) introduce a simple decomposition of sectoral productivity to distinguish between the two sources of aggregate productivity growth mentioned above: average productivity growth, and the efficient reallocation of factors across firms

$$\Omega_{st} = \sum_{i \in s} \theta_{it} \omega_{it} = \bar{\omega}_{st} + cov_{st}(\theta_{it}, \omega_{it}) = \bar{\omega}_{st} + \sum_{i \in s} (\theta_{it} - \bar{\theta}_{st})(\omega_{it} - \bar{\omega}_{st}) \quad (2)$$

where θ_i is the share of activity for firm i in industry s and time t , ω_i is the log of firm i 's productivity, and bars over variables stand for unweighted industry averages at time t . That is, the aggregate productivity of sector s in year t , Ω_{st} , can be decomposed into the sum of average firm productivity, $\bar{\omega}_{st}$, and the covariance between firm size and productivity, $cov_{st}(\theta_{it}, \omega_{it})$. If negative, the covariance term would indicate that the allocation of resources is negatively contributing to sectoral aggregate productivity, that is resources are allocated towards less productive than average firms. If positive, it would indicate that resources are allocated towards more productive than average firms.

Since the aim of our work is to assess the impact of labor market reforms on allocative efficiency, exploiting the exogenous variation across regions, sectors and years, we do not pool all firms within the same industry at the national level but rather conduct the analysis separately for each industry-region pair.³³ Our aim is, therefore, to assess whether and how the reforms have changed

³¹Differently from Cappellari et al. (2012), we compute firms' size excluding agency workers and collaborators, since we do not have information on external staff.

³²See Baily et al. (1992); Foster et al. (2001a); Hsieh and Klenow (2009); Petrin and Sivadasan (2013); González and Miles-Touya (2012).

³³Performing the decomposition at this level of aggregation is not much problematic for a country like Italy

the allocation of resources within the same industry-region over time.³⁴ Given these considerations, we apply the Olley and Pakes’s (1996) decomposition to industry-region pairs as follows

$$\Omega_{srt} = \sum_{i \in sr} \theta_{it} \omega_{it} = \bar{\omega}_{srt} + cov_{srt}(\theta_{it}, \omega_{it}) = \bar{\omega}_{srt} + \sum_{i \in sr} (\theta_{it} - \bar{\theta}_{srt})(\omega_{it} - \bar{\omega}_{srt}) \quad (3)$$

where θ_i is the share of activity of firm i in industry s and region r at time t , ω_i is (log) firm-level productivity, and bars over variables stand for unweighted averages at the level of each industry-region pair sr .

While our paper applies the static Olley and Pakes (1996) decomposition, therefore investigating the impact of labor market regulations on the reallocation of resources working through expansion and contraction of existing firms, it does not explicitly take into account the “net entry” (i.e. entry less exit) component of reallocation, that accounts for a non-negligible share of aggregate productivity growth. As recently shown by Bottasso et al. (2017), firms’ entry and exit rates can indeed be affected by firing restrictions, particularly in those industries characterized by more flexibility in labor force adjustment. A dynamic version of the Olley and Pakes (1996) decomposition, which takes into account firms’ entry and exit, has recently been proposed by Melitz and Polanec (2015). However, Linarello and Petrella (2017) show that the results of this dynamic decomposition very much depend on the dataset used for the analysis, and stress the importance of having access to data on the universe of firms to properly assess the contribution of the dynamic component. Because the firm-level data at our disposal cover all the Italian limited liability firms rather than the entire universe, and do not provide detailed information on firm demography, the question of the role of labor market reforms in firm entry and exit cannot be addressed properly.³⁵

With this caveat in mind, we now apply Olley and Pakes’s (1996) decomposition to industry-region pairs. As largely discussed by Foster et al. (2001b) and Melitz and Polanec (2015), there are numerous possibilities for the choice of a productivity measure and the associated market share weight. We restrict our analysis to total factor productivity, treating different workers types as different inputs, and using value added shares as weights. Using TFP rather than labor productivity allows us to analyse how resources overall are allocated rather than looking at the labor factor allocation alone. As indicated in equation 3, the OP covariance is computed considering the reallocation of resources within sector-region. Because some combinations of sector-region have only a few observations, we group together related industries and contiguous regions with a similar economic structure and belonging to the same treatment group, ending up with eleven sectors, fifteen regions and thirteen years for a total of 2145 observations.³⁶ To provide a visual

characterized by scarce labor sectoral and regional mobility, despite presenting considerable and persisting cross-regional income differences (Mocetti and Porello, 2010). As reported by Modena and Rondinelli (2016) only 1.1% per year of the youths (aged 15-34), over total residents, in the period 2000-2005 changed their original region. As far as the inter-sector labor mobility is concerned, according to the Labor Force Survey provided by ISTAT, on average over the period 2011-2016 only 2% of total workers in the manufacture moved from a sector (2digit-Ateco) to another one within one year. To make our analysis robust to labor flows we estimate the covariance and run the analysis at a different level of disaggregation (province-sector) and results, available upon request, do not change.

³⁴Using the measure of covariance in comparative terms could be problematic because of the differences in the underlying distribution of firms’ productivity across sectors and regions. As detailed in Section 5, our empirical model includes sector-region fixed effects, and it focuses on the effect of the reform on the change in allocative efficiency over time within the same sector-region.

³⁵Given the limited business churning characterizing the Italian market (Criscuolo et al., 2014), this might be a minor issue in our context.

³⁶See Table A1 in the Appendix for a detailed list of the sectors and regions.

Figure 2: OP covariance index using 1998 as base year. The OP covariance is computed for each sector-region as $\sum_{i \in sr} (\theta_{it} - \bar{\theta}_{srt})(\omega_{it} - \bar{\omega}_{srt})$ where $(\theta_{it} - \bar{\theta}_{srt})$ is deviation of a firm's value added from the average firm share in sector s and region r ; $(\omega_{it} - \bar{\omega}_{srt})$ is deviation of a firm's log TFP from the average log TFP of sector s and region r . The aggregate covariance is obtained by weighting the sectoral-region covariance with the relative importance of the sector-region in terms of value added. The TFP estimated distinguishing between skilled workers, number of apprentices and blue-collars.

depiction of the trend of OP covariance for the Italian manufacturing sectors over the sample period, Figure 2 shows the total covariance as a weighted average of each sector-region covariance, using as weights the relative importance of each unit in terms of value added.

4. Labor market reforms and firm level adjustments

We begin our core analysis by assessing the impact of the two reforms on different margins of firms' adjustments: number of apprentices, number of employees and productivity level. The investigation of the within-firm effects could represent a relevant benchmark for the core underlying mechanisms behind the main aggregate effects. This preliminary investigation is very similar to what has been shown by Cappellari et al. (2012). With respect to their previous findings, we add some further analysis to test whether the effects are heterogeneous across companies. It is indeed possible that the reforms differentially increase size among firms with different levels of productivity, but it is also possible that the regulations raise productivity among the largest firms. Both micro-level mechanisms, which are not mutually exclusive, could explain a rise in allocative efficiency observed at the aggregate level.³⁷

Following Cappellari et al. (2012), we estimate the following specification

$$\ln Y_{f,t} = \alpha + \lambda_1 d_{sr,t}^A + \beta_1 d_{s,t}^F + d_t + d_f + \epsilon_{ft} \quad (4)$$

where the dependent variable, $Y_{f,t}$, stands alternatively for number of apprentices, number of employees, or productivity level at time t . We also include year dummies d_t , capturing shocks common to all firms in a given year, and firm fixed effects d_f , capturing firm-specific and time invariant unobserved characteristics.

³⁷We thank an anonymous referee for this insightful suggestion.

Table 3: Labor market reforms and firm level adjustments

Dep. Var.	# Apprentices _{f,t}	ln # Apprentices _{f,t}	ln # Employees _{f,t}	ln TFP _{f,t}
	Poisson	Linear Regression Model		
	(1)	(2)	(3)	(4)
$d_{s,t}^F$	-0.113*** (0.030)	-0.018*** (0.003)	-0.046*** (0.004)	-0.035*** (0.003)
$d_{rs,t}^A$	-0.112* (0.064)	-0.020*** (0.003)	0.001 (0.003)	-0.013*** (0.003)
N	854625	854625	854625	854625
adj. R^2		0.599	0.922	0.655
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Cluster		Firm		

Notes: This table reports firm-level regressions obtained by using Italian data between 1998 and 2010. The dependent variable, reported at the top of each column, is in levels in column 1 and in log-levels in columns 2-4. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results are reported in Table 3. Considering the number of apprentices as the dependent variable, we propose two different regression models. First, we experiment using Poisson regressions to account for censoring at zero (estimates in column 1 of Table 3), and, second, we run a linear regression model using the log number of apprenticeships plus one (estimates in column 2 of Table 3). Similarly to Cappellari et al. (2012), on average the fixed-term contract reform had negative effects in terms of both employment and productivity, confirming that it has not been successful in promoting firms' overall employment and efficiency. Average negative effects are also observed for the apprenticeship contract reform, in terms of both number of apprenticeships and productivity, whereas there are no effects on the level of employees. These latter results go in opposite directions with respect to the findings of Cappellari et al. (2012), who show that the reform of apprenticeship contracts has been successful in raising firms' productivity and the net employment of apprentices. However, it should be noted that while our sample includes both relatively small and large firms, Cappellari et al. (2012) focus mainly on large firms with an average size of 200 workers (approximately 190 if excluding external staff). By simply looking at the average effect, the estimation framework proposed in equation 4 might not detect possible heterogeneous effects among firms of different sizes or productivity levels. We therefore estimate the effect of the reforms on firms' size and efficiency for different parts of the size and TFP distributions for each sector-region combination. Precisely, we distinguish among low- (from the 1st to the 33rd percentiles), medium- (from the 34th to the 66th percentiles), and high- (from the 67th to the 100th percentiles) productivity firms in the pretreatment period³⁸ within sector-region pairs. We include three dummies — $D1_{f,pre}^{TFP}$ (baseline), $D2_{f,pre}^{TFP}$, and $D3_{f,pre}^{TFP}$ — taking value 1 if a firm is in the first, second or third part of the productivity distribution, respectively, and zero otherwise. We then estimate the following equation

$$\ln Y_{f,t} = \alpha + \lambda_1 d_{sr,t}^A + \lambda_2 d_{sr,t}^A \times D2_{f,pre}^{TFP} + \lambda_3 d_{sr,t}^A \times D3_{f,pre}^{TFP} + \beta_1 d_{s,t}^F + d_t + d_f + \epsilon_{ft} \quad (5)$$

where the dependent variable is either the (log) number of apprentices or employees.

³⁸We consider firms' average productivity between 2003 and 2004 as pre-treatment level of efficiency.

Table 4: Labor market reforms and firm level adjustments: heterogeneous effects

Dep. Var.	ln # Apprentices _{f,t}	ln # Employees _{f,t}	ln TFP _{f,t}	ln # Employees _{f,t}	ln TFP _{f,t}
	(1)	(2)	(3)	(4)	(5)
$d_{rs,t}^A$	-0.016*** (0.004)	-0.035*** (0.005)	-0.025*** (0.004)	0.000 (0.003)	-0.013*** (0.003)
× D2 ^{TFP} _{f,pre}	-0.025*** (0.004)	0.014*** (0.005)			
× D3 ^{TFP} _{f,pre}	0.017*** (0.004)	0.089*** (0.005)			
× D2 ^L _{f,pre}			0.016*** (0.004)		
× D3 ^L _{f,pre}			0.027*** (0.003)		
$d_{s,t}^F$	-0.018*** (0.003)	-0.048*** (0.004)	-0.035*** (0.003)	-0.103*** (0.007)	-0.042*** (0.004)
× D2 ^{TFP} _{f,pre}				0.036*** (0.008)	
× D3 ^{TFP} _{f,pre}				0.120*** (0.008)	
× D2 ^L _{f,pre}					-0.009 (0.008)
× D3 ^L _{f,pre}					0.026*** (0.006)
<i>N</i>	854625	854625	854625	854625	854625
adj. <i>R</i> ²	0.600	0.922	0.655	0.922	0.655
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Cluster			Firm		

Notes: This table reports firm-level regressions obtained by using Italian data between 1998 and 2010. D2^{TFP}_{f,pre} and D3^{TFP}_{f,pre} (D2^L_{f,pre} and D3^L_{f,pre}) are dummies that take value 1 if a firm is in ending up in the second or third part of the productivity (employment) distribution, respectively. Standard errors in parentheses: * p<0.10, ** p<0.05, *** p<0.01.

Columns 1 and 2 of Table 4 identify the differential effects of the apprenticeship reform across firms with a different level of productivity in the pretreatment period. The findings are consistent through the two different specifications. The coefficient on the interaction term reveals that the effect of the apprenticeship reform varies across firms with different productivity levels. We observe that the reform has a positive impact only on the highly productive firms, while the effect is negative among the others. Indeed, this translates into a reduction in the number of apprentices and the number of employees among the less efficient firms, and into an increase in the employment level among the more productive ones. This firm-level mechanism could indeed suggest that the apprenticeship reform has contributed to reallocate labor from the least to the more productive firms. This finding is in line with the argument proposed in our conceptual framework (Section 2.3): highly productive firms have more incentive to hire apprentices since they are more efficient in training and they can profitably train both efficient and inefficient workers.

As shown above, the reforms induced different changes in size among firms with different levels of productivity, but it is also possible that the regulations induced different changes in productivity among firms of different size. To test for this second mechanism, we run a similar regression as in

(5)

$$\ln TFP_{f,t} = \alpha + \lambda_1 d_{sr,t}^A + \lambda_2 d_{sr,t}^A \times D2_{f,pre}^L + \lambda_3 d_{sr,t}^A \times D3_{f,pre}^L + \beta_1 d_{s,t}^F + d_t + d_f + \epsilon_{ft} \quad (6)$$

where now the dependent variable is a firm's (log) TFP, and we distinguish among firms with a low (from the 1st to the 33rd percentile), medium (from the 34th to the 66th percentile), and high (from the 67th to the 100th percentile) level of employment in the pretreatment period within sector-region pairs. Results, reported in column 3 of Table 4, confirm again the presence of heterogeneous effects: the reform induced productivity to increase among the largest firms, while it had a negative effect among the smallest ones.

We can perform a similar set of exercises, but aimed at testing the heterogeneity of the fixed-term reform. We interact the fixed-term reform dummy with dummies for firms' different level of productivity (column 4 of Table 4) and size (column 5 of Table 4). The results show that only high-productivity firms are induced to expand employment by the fixed-term reform, probably because they have better screening capabilities and they can obtain higher production from any employer-employee match (as suggested in Section 2.3).

5. Labor market reforms and allocative efficiency

Our findings so far provide some preliminary evidence of the microeconomic effects of the two reforms in terms of employment and productivity. Given this firm-level evidence, we now turn to the aggregate analysis concerning the effects on allocative efficiency. First, we provide evidence on the relationship between labor market reforms and allocative efficiency by implementing a traditional difference-in-differences model, where we estimate the average treatment effect of the reforms. Second, we extend the static model to detect possible heterogeneous effects among sectors and regions. Finally, we adopt an event study approach that allows us to explore more accurately the timing of the effects of the two reforms.

5.1. Difference-in-differences static model

To study the relationship between the degree of within-industry allocative efficiency and the implementation of the labor market reforms, we use a difference-in-differences (DiD) approach, exploiting the exogenous variation across regions, sectors and years upon which we base our identification strategy. We estimate the following model

$$cov_{sr,t} = \alpha + \lambda d_{sr,t}^A + \beta d_{s,t}^F + \gamma \ln L_{sr,t} + d_t + d_{sr} + \epsilon_{sr,t} \quad (7)$$

where the dependent variable $cov_{sr,t}$ is the OP covariance variable described in the previous section, $d_{sr,t}^A$ is a dummy capturing the exposure of sector s and region r in time t to the reform of apprenticeship contracts, while $d_{s,t}^F$ is a dummy capturing the exposure of sector s in time t to the reform of fixed-term contracts. As reported in Table 1, the two reforms have been applied to different sectors and regions starting from 2005. Specifically, the dummy for the fixed-term contract reform takes value one for all regions but only in some sectors starting from 2005. The dummy for the apprenticeship contract reform takes value one for all sectors in Emilia Romagna and Toscana starting from 2005; for all sectors in Alto Adige, Sardegna, Puglia, Friuli Venezia Giulia, and Marche from 2006; for all sectors in Lazio from 2007; for all sectors in Piemonte from 2008; for all sectors in Umbria and Campania from 2010; and only for some sectors in all the other regions from 2006. Additionally, we also control for the average (log) firm size at the sectoral-regional-year level ($\ln L_{sr,t}$), and to ensure that our comparisons across treatment groups

Figure 3: Test for the common trend assumption, choosing fake reform years for all the periods before the introduction of the two reforms. The left panel reports coefficients, together with standard errors, related to the apprenticeship reform, $d_{sr,t}^A$ while the right panel repeats the same exercise for the fixed-term reform, $d_{s,t}^F$ ().

over time do not reflect group-specific characteristics, we control for time and region-sector fixed effects, d_t and d_{sr} , respectively: including the latter can provide reassurance that the estimated reforms' effects are not reflecting time-invariant omitted variables that are potentially correlated with the adoption of the reforms (Besley and Case, 2000). We report standard errors clustered at the sector and region level, but the results are robust to alternative treatments of the error terms such as sector-time and region-time or sector-region.³⁹

Although we have argued that the implementation of labor market reforms across regions and sectors was to some extent random, we nonetheless must exclude the possibility that regions or sectors that were more productive and displayed a better allocation of resources, were also those that adopted the reforms of the apprenticeship contract or the fixed-term contract in the first place. In this case, we would still observe higher allocative efficiency ex-post, but the causal relation would be questionable. To control for the possibility that the implementation of the reforms is correlated with underlying trends, we first repeat our regressions for the period before the introduction of the law. If our indicators are capturing differences in trends among regions and sectors, we should find that the coefficients for the two labor market reforms are still significant when running the same regressions for the period before the law was passed. Figure 3 supports the conclusion that the two dummies capturing the reforms were not correlated with some pre-existing underlying trends.

Columns 1 and 2 of Table 5 report the results of the DiD model without and with the control for the number of employees at the sector-region-year level, respectively. In both cases the reform of apprenticeship contracts has a positive effect on resource allocation, producing a statistically significant increase of allocative efficiency: the estimated coefficient is 0.05, which represents 7.6% of the mean of the OP covariance term. In contrast, the reform of the fixed-term contracts does not have any effect on the OP covariance. To confirm that our comparisons across treatment groups over time do not reflect group-specific characteristics, in addition to sector-region fixed effects, one should run a specification that also includes region-specific and sector-specific time trends. The inclusion of the latter set of fixed effects will ensure that the identification comes from

³⁹See columns 3 and 4 of Table A2 in the Appendix.

the discontinuity surrounding the passage of the reforms. Because this identification strategy is very demanding, we recover some degrees of freedom by defining the region-time and sector-time fixed effects at a higher level of aggregation.⁴⁰ The results of this specification, shown in column 3 of Table 5, confirm the previous findings.

We now consider a set of exercises aimed at testing the robustness of our results to alternative estimates of a firm’s TFP and to possible biases in the standard errors due to serially correlated outcomes when differences-in-differences estimation is employed (Bertrand et al., 2004). First, to address possible measurement error in the TFP estimates we re-run the analysis by using the perpetual inventory method to measure the capital stock, and by estimating the production function separately for each three-digit industry (Becker and Haltiwanger, 2006; Lenzu and Manaresi, 2018). The results obtained by using these alternative TFP estimation strategies are reported in columns 1 and 2 of Table A2 in the Appendix. In both cases the coefficients of interest are very similar to those reported in Table 5, confirming our previous findings. Second, standard errors of equation (7) may be systematically downward biased if serial autocorrelation is not seriously taken into account. Bertrand et al. (2004) propose a number of methods to address this problem: pairs cluster bootstrapped t-statistics (block bootstrap procedure), different clustering of the error terms, and collapsing the time series information into a pre and post period. Because our post period varies across sectors and regions we can check the robustness of our results by proposing the first two methods. In column 3 of Table A2 in the Appendix, we present the results by clustering the error at sector-time and region-time, while in column 4 in the Appendix we present the results of the block bootstrap estimation with error clustered at the sector-region level.⁴¹

5.2. Heterogeneous effects

To refine our analysis and detect the possible mechanisms behind the effects of the two reforms, we interact the two policy dummies with industry-region characteristics capturing different exposures to the reform. This allows us to assess whether the regulations have boosted allocative efficiency disproportionately more in relatively highly exposed sectors-areas than in less exposed ones.

As a first exercise, we interact the dummy for apprenticeship reform with a dummy $D_{rs,pre}^{ShareSW}$ which takes value 1 for those sectors-regions with a percentage of skilled workers above the median in the pretreatment period⁴² and zero otherwise. According to the arguments proposed in Section 2.3, the most productive companies find the apprenticeship form of contract particularly convenient. Because highly productive firms are also those employing a large fraction of skilled workers, as shown for instance by Haltiwanger et al. (1999); Abowd et al. (2002); Haskel et al. (2005),⁴³

⁴⁰We aggregate the regions in Northern, Central and Southern Italy, according to the NUTS level 1 regions. Sectors are aggregated into groups according to the ISIC/NACE intermediate aggregation.

⁴¹Additional robustness checks, not shown but available upon request, include specifications where we separately run regressions for the two reforms, to check whether the results are somehow mutually reinforcing. Moreover, to better understand the interaction between the two reforms, we run a regression with the two reform dummies and their interaction, to see if the lack of statistically significant effects from the reform of fixed-term contracts is more or less accentuated where the other policy operates. In all cases results do not change with respect to our baseline specification.

⁴²We use the pre-treatment average in order to have a pre-determined figure.

⁴³There is a good deal of empirical research that examines the connection between productivity and human capital at the micro level of the firm. Using matched employer-employee data sets, Abowd et al. (2002) for France, Haltiwanger et al. (1999) for the United States, Haskel et al. (2005) and for the United Kingdom investigate the relation between productivity and workers’ skills, showing the existence of a positive relationship.

Table 5: Labor market reform and allocative efficiency: difference-in-differences model

	$COV_{sr,t}$				
	(1)	(2)	(3)	(4)	(5)
$d_{rs,t}^A$	0.052**	0.050**	0.052**	0.011	0.046**
	(0.018)	(0.018)	(0.020)	(0.023)	(0.017)
$\times D_{rs,pre}^{ShareWhiteCollar}$				0.062*	
				(0.033)	
$d_{s,t}^F$	-0.010	-0.010	-0.010	-0.005	0.051**
	(0.026)	(0.024)	(0.025)	(0.022)	(0.022)
$\times D_r^{CourtUncertainty}$					-0.128***
					(0.023)
$\ln L_{sr,t}$		-0.146		-0.162*	-0.184**
		(0.088)		(0.086)	(0.079)
N	2145	2145	2145	2145	2145
adj. R^2	0.697	0.699	0.697	0.701	0.704
Year FE	Yes	Yes	Yes	Yes	Yes
Sector-Region FE	Yes	Yes	Yes	Yes	Yes
Macro-Region-year and Macro-Sector-year	No	No	Yes	No	No
Cluster	sector and region				

Notes: This table reports sector-region level regressions obtained by using Italian data between 1998 and 2010. The two dummies $d_{rs,t}^A$ and $d_{s,t}^F$ capture the exposure of sector s and region r at time t to the reform of apprenticeship contracts and fixed-term contracts, respectively. $D_{rs,pre}^{ShareSW}$ takes value 1 for those sectors-regions with a percentage of skilled workers above the median in the pre-treatment period. $D_r^{CourtUncertainty}$ is a dummy for those regions with the average duration of labor court disputes above the median. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

we should observe the impact of the apprenticeship reform to be stronger within skill-intensive sectors, which are more likely to be populated by highly productive firms. The results of this specification, shown in column 4 of Table 5, confirm the intuition that the mechanisms characterizing the apprenticeship reform are more likely to be at work among skill-intensive sectors and regions. The coefficient on the interaction term is indeed positive and statistically significant, suggesting a higher marginal impact of the apprenticeship reform where there is a relatively higher share of white collar workers than in industries-regions where the percentage of non-manual workers is low.

As far as the fixed-term contracts reform is concerned, columns 1 to 3 of Table 5 provide evidence that, on average, the deregulation did not have any effect in terms of allocative efficiency. As explained in Section 2.3, and following the argument proposed by Cappellari et al. (2012), a possible positive effect of the reform that liberalized the use of fixed-term contracts might have been offset by the increase in uncertainty related to judges' interpretation of the norm. By removing the requirement of a specific reason for adopting fixed-term contracts, the reform made the use of these contracts too generic and, in case of court disputes, overly reliant on courts' interpretations of specific cases. This uncertainty might have actually reduced the incentives to use fixed-term contracts, and eventually hampered the possible positive effects of the reform.⁴⁴

To test this explanation, we interact the dummy for the fixed-term reform with a dummy

⁴⁴It should be noted that here we refer to uncertainty related to court disputes rather than demand or productivity conditions, as discussed for instance by Bloom et al. (2007), where uncertainty about a firm's value is proxied by the standard deviation of daily stock returns.

variable $D_r^{CourtUncertainty}$, which takes value 1 for those regions with an average duration of labor court disputes above the median, and zero otherwise. Regional court efficiency is computed as the simple average across provinces within a region of the days required to settle a labor law dispute. The results, shown in column 5 of Table 5, suggest that in regions where labor court disputes are longer, and therefore potential legal expenses higher for firms, the reform had a negative and statistically significant effect in terms of OP covariance, while the effect turns out to be positive among those geographical areas where court disputes are less lengthy. The estimated effects of the fixed-term reform for regions with an average duration of labor court disputes above the median is -0.077, which represents approximately 11% of the mean of the dependent variable. This result corroborates the idea that uncertainty in court rulings indeed played a significant role in the effectiveness of the new fixed-term labor legislation.

5.3. Event study model

The difference-in-differences model discussed in Section 5.1 shows the average “post” treatment effect of the reforms. In this section, we apply a dynamic econometric model that allows us to explore the timing of the effects and provide a further useful test for pre-trends, supporting the evidence provided by Figure 3. As in the previous model, the econometric specification exploits the panel structure of our data, by controlling for sector-region specific fixed effects. We use data for sectors and regions that were subject to the reform at some point during the period 1998-2010. Sectors and regions that have not yet had an event, have already had an event, or never experience an event serve as controls for the treated groups (see Sandler and Sandler, 2013, for a useful discussion). We estimate the following equation

$$cov_{sr,t} = \alpha + \sum_{\tau \neq -1} \lambda_{\tau} d_{sr,t+\tau}^A + \sum_{\tau \neq -1} \beta_{\tau} d_{s,t+\tau}^F + \gamma \ln L_{sr,t} + d_t + d_{sr} + \epsilon_{sr,t}$$

where the set of dummy variables $d_{sr,t+\tau}^A$ and $d_{s,t+\tau}^F$ represents relative periods with respect to the event of the apprenticeship and fixed-term reforms, respectively ($\tau = 0$). In particular, λ_{τ} (β_{τ}) is the effect on the sector-region OP covariance following the reform (or, if τ is negative, prior to). Given the time span available, from 1998 to 2010, τ can assume values $-7 \leq \tau \leq 5$, with $\tau \neq -1$. The d_t 's are year fixed effects, aimed at controlling for common time trends affecting the whole economy, and d_{sr} 's are individual fixed effects, meant to control for unobserved time-invariant sector-region characteristics that could be correlated both with the independent variables and the outcomes. We also include the (log) average number of employees at sectoral-regional-year level ($\ln L_{sr,t}$). We expect that, if we have carefully controlled for all the non-ignorable observable and unobservable variables influencing differences in the relevant dependent variables between the control and treated groups, the parameters λ_{τ} and β_{τ} at $\tau < -1$ will not be significantly different from zero.⁴⁵

Column 1 of Table 6 shows the dynamic model for the apprenticeship reform, and column 2 for the fixed-term reform, while columns 3 and 4 report the full-model without and with the control for the number of employees at the sector-region-year level, respectively. The estimated effects of the reforms for the pre-event years are not statistically significant, providing an informal specification test of the model. Looking at the post-reform coefficients, we find that the covariance of the sector-region subjected to the apprenticeships reform is positively affected. The effects of

⁴⁵In general, a bias in the model could occur if the treated group is not a random sample in terms of non-ignorable characteristics we do not control for (Jacobson et al., 1993).

Table 6: Labor market reforms and allocative efficiency: event study mode

	$COV_{sr,t}$			
	(1)	(2)	(3)	(4)
$d_{rs,t-7}^A$	-0.020 (0.025)		-0.024 (0.028)	-0.022 (0.027)
$d_{rs,t-6}^A$	-0.017 (0.026)		-0.022 (0.027)	-0.020 (0.027)
$d_{rs,t-5}^A$	0.002 (0.019)		0.002 (0.024)	0.002 (0.024)
$d_{rs,t-4}^A$	-0.005 (0.021)		-0.012 (0.019)	-0.014 (0.020)
$d_{rs,t-3}^A$	0.013 (0.018)		0.009 (0.018)	0.007 (0.018)
$d_{rs,t-2}^A$	-0.004 (0.008)		-0.005 (0.010)	-0.008 (0.009)
$d_{rs,t}^A$	0.019 (0.018)		0.023 (0.021)	0.022 (0.020)
$d_{rs,t+1}^A$	0.049** (0.022)		0.051* (0.026)	0.048* (0.025)
$d_{rs,t+2}^A$	0.065*** (0.019)		0.065** (0.025)	0.060** (0.025)
$d_{rs,t+3}^A$	0.077** (0.030)		0.080** (0.035)	0.075* (0.035)
$d_{rs,t+4}^A$	0.132*** (0.020)		0.135*** (0.027)	0.132*** (0.028)
$d_{rs,t+5}^A$	0.186*** (0.033)		0.189*** (0.038)	0.193*** (0.040)
$d_{rs,t-7}^F$		-0.000 (0.030)	-0.000 (0.033)	-0.000 (0.032)
$d_{rs,t-6}^F$		0.014 (0.017)	0.020 (0.020)	0.018 (0.021)
$d_{rs,t-5}^F$		0.022 (0.026)	0.027 (0.027)	0.028 (0.028)
$d_{rs,t-4}^F$		-0.012 (0.023)	-0.014 (0.030)	-0.014 (0.032)
$d_{rs,t-3}^F$		0.035 (0.019)	0.037* (0.018)	0.036* (0.018)
$d_{rs,t-2}^F$		0.020 (0.020)	0.017 (0.019)	0.019 (0.022)
$d_{rs,t}^F$		0.002 (0.013)	0.002 (0.013)	-0.001 (0.013)
$d_{rs,t+1}^F$		-0.016 (0.017)	-0.022 (0.023)	-0.023 (0.022)
$d_{rs,t+2}^F$		0.012 (0.027)	0.000 (0.030)	-0.001 (0.027)
$d_{rs,t+3}^F$		0.027 (0.019)	0.013 (0.025)	0.010 (0.025)
$d_{rs,t+4}^F$		0.008 (0.042)	-0.009 (0.044)	-0.008 (0.042)
$d_{rs,t+5}^F$		0.030 (0.044)	0.001 (0.046)	0.005 (0.042)
$\ln L_{sr,t}$				-0.149 (0.088)
N	2145	2145	2145	2145
adj. R^2	0.698	0.695	0.697	0.699
Year FE	Yes	Yes	Yes	Yes
Sector-Region FE	Yes	Yes	Yes	Yes
Cluster		sector and region		

Notes: This table reports sector-region level regressions obtained by using Italian data between 1998 and 2010. The covariance term is regressed on the dummies for the labor market reforms at different point in time. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the reform are not instantaneous but rather increasing over time. The dynamic model confirms the previous results according to which the fixed-term reform did not have any effect on the allocation of resources across firms within sectors.

6. Conclusions

While most of the existing empirical literature mainly focuses on the relationship between employment protection legislation and allocative efficiency, little attention has been given to regulation of temporary contracts. This paper contributes to the existing literature by examining the extent to which regulations of different types of temporary contracts influence aggregate productivity through their impact on the efficiency of resource allocation. We investigate the effects of two reforms of temporary employment applied in Italy at the beginning of the century, by exploiting the exogenous variation in the implementation across regions, sectors and time to identify their effects on the allocation of resources across firms. The empirical analysis is based on the universe of limited liability manufacturing firms over the period 1998-2010. Following Olley and Pakes's (1996) approach, we decompose the sectoral-region aggregate productivity as the sum of firm average productivity, and the covariance between firm size and productivity — a term capturing allocative efficiency.

Our results support the evidence that the type of temporary contract being considered is relevant, since different contracts are characterized by different combinations of screening, training, and incentives for firms (Berton et al., 2011). In particular, we find that there is an economically and statistically robust positive relationship between the reform of apprenticeship contracts and the OP covariance terms. The analysis suggests that the reform has induced highly productive firms to hire apprentices and provide them with training, generating a positive covariance between size and productivity at the aggregate level. In contrast, the analysis reveals that, on average, the reform of fixed-term contracts has not produced the intended results. However, we observe that this overall effect obscures great heterogeneity among geographical areas. In particular, our empirical analysis detects that in regions where labor court disputes are longer, the reform has had a negative and statistically significant effect in terms of covariance, while the effect has been positive among those regions where courts disputes last less. This finding points towards the crucial role played by judicial (un)certainty (and, more broadly, the Rule of Law) in stimulating reallocation mechanisms. More generally, our results suggest that reducing judicial uncertainty and investing in (and testing for) the quality of workers are important issues that must be taken into account in labor market policies.

Establishing the impact of legislation on temporary contracts is relevant for policy purposes, especially in the context of the radical transformation that the European labor markets have experienced in the last two decades, and that is still under way. A new wave of labor market reforms, aimed at reducing labor market dualism, has been implemented in the aftermath of the crisis.⁴⁶ Shedding light on the effect of specific regulations on temporary contracts can better inform policymakers on the likely consequences of the implementation of such reforms. In particular, understanding the impact of these policies on the degree of within-industry allocative efficiency is important for the political debate on what interventions could enhance overall economic efficiency

⁴⁶In Italy, the recent labor market reform ('Jobs Act') has revised employment protection for permanent contracts, lowering firing costs and making firings less prone to court disputes; at the same time, another policy provided generous hiring incentives to firms offering open-ended contracts (see Sestito and Viviano, 2016, for an empirical evaluation of such policy).

and, in turn, aggregate productivity. This appears particularly important in light of the recent literature showing that the allocation of resources across heterogeneous firms is a key determinant of the marked productivity differentials across countries, and in consideration of the key role that productivity gains may play in the preservation of a high-growth trajectory.

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Appendix

TFP Estimation

We estimate firm-level TFP according to the Wooldridge (2009) methodology, which is based on the Levinsohn and Petrin (2003) methodology. We consider labor inputs as flexible variables (considered as a single input here for sake of exposure), and materials as proxy variable for TFP shocks ω_{it} . The estimation procedure relies on two main assumption. First, that the function materials m_{it} are a strictly monotone function of ω_{it} , for any given capital stock k_{it} , so that we have

$$\omega_{it} = g^{-1}(k_{it}, m_{it}) = h(k_{it}, m_{it})$$

Second, that ω_{it} is first-order Markov

$$\omega_{it} = f(\omega_{i,t-1}) + \xi_{it}$$

where ξ_{it} is White noise. Wooldridge (2009) proposes to estimate the following equation

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + f(h(k_{i,t-1}, m_{i,t-1})) + \xi_{it} + \eta_{it}$$

by simultaneously minimizing the two moment conditions

$$E[\eta_{it}|I_{it}] = 0$$

$$E[\xi_{it} + \eta_{it}|I_{i,t-1}] = 0.$$

To proceed with GMM estimation, one must firstly deal with the unspecified function $f(h(k_{i,t-1}, m_{i,t-1}))$ (usually approximated by a second or third order polynomial); second, one must deal with instrument choices: in addition to the exogenous state variable k_{it} , one can use the first lag of labor; one could also use lags of more than one period, but this would mean losing more initial time periods.

Comparison between FRAME-SBS and CERVED-INPS

Figure A1: Comparison between FRAME-SBS and CERVED-INPS in terms of average growth of labor productivity.

List of sectors and regions

Table A1: Sectors and Regions

Regions	Sectors
Piemonte	Food Products, Beverages
V.Aosta,Liguria	Textiles, Wearing Apparel, Leather
Lombardia	Wood, Furniture
Trentino AA*,Friuli	Paper
Veneto	Printing
Emilia Romagna	Chemicals
Toscana	Rubber Plastic, Glass
Umbria	Basic metals, Fabricated metal
Marche	Computer Electronic, Electrical equipment
Lazio	Machinery and equipment
Abruzzo, Molise	Motor vehicles, Other transport equipment
Campania	
Puglia	
Basilicata,Calabria,Sicilia	
Sardegna	

Notes: This table reports the list of sectors and regions used in the empirical analysis.

Robustness checks

Table A2: Labor market reform and allocative efficiency: robustness checks

	Covariance (TFP)			
	(1)	(2)	(3)	(4)
$d_{rs,t}^A$	0.060*** (0.019)	0.044** (0.018)	0.050*** (0.015)	0.050* [0.08]
$d_{s,t}^F$	-0.006 (0.031)	-0.010 (0.022)	-0.010 (0.012)	-0.010 [0.72]
$\ln L_{sr,t}$	-0.088 (0.085)	-0.133 (0.082)	-0.146*** (0.049)	-0.146* [0.07]
N.Obs	2145	2145	2145	2145
R ²	0.723	0.754	0.724	0.741
Year FE	Yes	Yes	Yes	Yes
Sector-Region FE	Yes	Yes	Yes	Yes
Cluster	sector and region	sector-year,region-year	sector-region	

Notes: This table reports sector-region level regressions obtained by using Italian data between 1998 and 2010. The two dummies $d_{rs,t}^A$ and $d_{s,t}^F$ capture the exposure of sector s and region r at time t to the reform of apprenticeship contracts and fixed-term contracts, respectively. In columns 1 the TFP is estimated measuring the capital stock with a perpetual inventory method while in column 2 it is estimated by sector at 3-digit. Columns 3 reports standard errors clustered at sector-time and region-time level. Column 4 reports symmetric pairs cluster bootstrap-t method and with sector-region standard errors. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.