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Original Article

Heterogeneous paths to stability

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Abstract

In this paper, we show that incentives to convert temporary contracts into permanent ones might be less effective when the employment protection legislation associated with temporary contracts is lax. Drawing upon rich administrative data and using a difference-in-differences methodology, we estimate that workers at their first work experience hired on more flexible contracts undergo a reduction in the conversion rate to permanent employment of 6.3 percentage points after the implementation of the incentives (and of 3.2 percentage points over a year), compared to peers hired on more rigid contracts. This reduced conversion rate, which results in a 17% wage penalty even 2 years into their professional journey, points to a significant negative impact of flexible temporary jobs on the future prospects of young workers.

Keywords: employment protection legislation, flexibility, institutional reforms, temporary contracts, young workers

JEL codes: J41, J63, J64

1 Introduction

In Europe, young workers are facing difficult times. The youth unemployment rate is at an alarmingly high level: during the Great Recession it increased sharply, and never went back to the pre-crisis levels. The COVID-19 pandemic worsened an already concerning situation regarding the labour force participation of young workers: in the UK in 2021, the number of young NEET (not in employment, education, or training) rose by the highest quarterly figure in a decade; in Italy, it passed the 2 million threshold, leading policy makers to call for a 'NEET emergency'. Even when young people find a job after a long time, the instability of employment is concerning, as temporary and non-standard employment have become the norm. In Italy in the past decade, the share of young workers who are stepping for the first time on the labour market and are hired on a temporary contract increased sharply from 35% in 2009 to over 55% in 2019 (Figure 1).

Recent evidence points to young workers as those who have experienced the sharpest deterioration in their employment and earnings over the last three decades (Bentolila et al., 2021). While temporary contracts can be a stepping stone in the transition from education into work (Booth et al., 2002; Eichhorst & Neder, 2014), they can also trap young people in insecure jobs or knock them into a worse career path (Casquel & Cunyat, 2008; Daruich et al., 2023). To reduce labour market segmentation, particularly among young people, in the past decade, several Mediterranean countries implemented a number of policies, which introduced incentives for firms to hire workers on a permanent basis. However, the outcome of these reforms could depend on specific institutional settings, such as the flexibility of temporary contracts, which might compromise the effectiveness of such interventions. In this paper, we study how the degree of flexibility of temporary

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Figure 1. Share of young workers entering for the first time on the labour market with a temporary contract.

Note: The graph reports the share of workers aged 16–35 entering for the first time on the Italian labour market between 2009 and 2019 who were hired on a temporary contract. *Source*: Sample of the Comunicazioni Obbligatorie (CICO) dataset as provided by the Italian Ministry of Labour.

contracts affects the success of institutional and fiscal reforms aiming at reducing labour market segmentation. Specifically, we investigate whether starting the career with a more flexible temporary contract may impact the career progression of young people, when incentives to the transformation into permanent contracts are in place. This is a rather important issue, which has been overlooked so far. Our analysis aims to demonstrate that incentives meant to push towards an increment in the number of permanent workers might be less effective whenever the employment protection legislation of temporary contract is lax, i.e. the contracts are more flexible.

To test if the degree of flexibility of temporary contracts matters for the career of young workers, we link several sources of administrative data, which provide information on the population of Italian workers in the private sector. The Italian case is ideal because in the time span of 1 year a series of reforms were implemented which (a) increased the flexibility of temporary contracts, (b) introduced fiscal incentives for hirings workers on permanent contracts, and (c) reduced firing costs associated with permanent contracts. Moreover, the Italian labour market has been heavily segmented in permanent and temporary workers since the 90s (Barbieri & Scherer, 2009); the youth unemployment rate and the NEET rate in 2020 were among the highest in Europe, at 29.4% and 25.1%, respectively; the share of young people (age 15–24) in temporary contracts has been also alarmingly high: in 2019, Italy ranked second in Europe at 63.3%, after Spain at 69% (OECD).

To evaluate the impact of the flexibility of temporary contracts on the effectiveness of incentives aimed at facilitating the transition to permanent employment, we select young workers at their first work experience hired on a temporary contract. We identify control and treated individuals on the basis of their first contract starting immediately before or after the date of a reform which increased the flexibility of temporary contracts (*Poletti Decree*). We follow these individuals over time and we show that when fiscal incentives to the transformation into permanent contracts are introduced (through the *Budget Law*), the gap in the probability of conversion opens up by approximatively 6 percentage points, in favour of individuals with less flexible temporary contracts. Later, when firing costs are reduced for workers on permanent contracts (through the *Jobs Act*), the probability of conversion converges between the two groups, but the gap remains stable at around 3%. This effect is demonstrated to remain consistent regardless of the attributes of the workers and of the firms that employ them. These results are particularly striking because they are estimated on a sample of workers (treated and control) who are young, share similar characteristics and are hired by firms with very similar features. Treated and control workers in our sample enter for the first time in the labour market with a temporary contract with a maximum time distance of 6 weeks. The only difference between the two groups is the flexibility of the temporary contract they are hired on when they start their careers. The flexibility of their first contract is therefore as good as randomly assigned. In the last part of the paper, we test for the presence of persistent effects; we estimate that the decrease in the conversion rate among young workers hired by chance on a more flexible temporary contract leads to a 17% wage penalty 2 years down their career paths.

These results are consistent with a search and matching framework à la Blanchard Landier (2001) in which workers get hired on temporary contracts and when their productivity is revealed it is decided whether they can get upgraded to a permanent contract.[1] Depending on the degree of flexibility of the initial contract, when firing costs associated with permanent contracts are lowered, temporary workers hired on more flexible contracts might experience a lower conversion rate, due to the lower turnover costs (hiring and firing costs).[2]

This paper contributes to the literature in two ways. First, it investigates for the first time the interaction between institutional settings, i.e. contract features, and policy interventions. From our results we learn that policies aiming at stimulating the conversion into permanent contracts might be less effective if temporary contracts are too flexible. Second, it shows that the characteristics of the first contract strongly affects the evolution of the future professional journey. Although some papers have looked at this issue before, they either compare workers starting their careers with standard vs. non-standard employment contracts (Gebel, 2010; Stuth & Jahn, 2020) or focus on specific group of individuals, such as high-school dropouts (García-Pérez et al., 2019). In this work, we use a natural experiment to compare the careers of all individuals at their first job experience, who are hired on temporary contracts with different degrees of flexibility, thus allowing us to evaluate the scarring effect of temporary contract regulations.

The paper is organized as follows. Section 2 describes the institutional background and the details of the reforms, while Section 3 summarizes the literature on the topic. Section 4 describes the data. Section 5 discusses the identification strategy, while Section 6 illustrates the empirical analysis. Section 7 provides descriptive statistics for the sample of selected workers and Section 8 discusses the results. Finally, Section 9 concludes the paper.

2 Institutional background

Three major labour policies have been implemented between 2014 and 2015 in Italy affecting the flexibility of temporary contracts and the transformation of temporary contracts into permanent contracts: the *Poletti Decree* in 2014 and in 2015 the *Budget Law* and the *Jobs Act*.

2.1 *Poletti Decree*

The *Poletti Decree*, which was approved on 21 March 2014, removed across all temporary contracts, the obligation for employers to justify their choice of hiring a worker with a temporary contract rather than with a permanent contract. Although it might appear as a marginal change, it is relevant for employers, as if the justification is not reported correctly an employee is entitled to sue the employer and eventually obtain from the labour court the conversion of the temporary contract into a permanent contract. The reform also increased the number of possible extensions of the contract from one to five, within the maximum duration of 3 years within the same company. The first change brought by the decree, i.e. the removal of the justification requirement, is the relevant change we want to focus on. We believe the second change, i.e. the increase in the number of extensions, to be marginally relevant as, looking at statistics for the Veneto region, even after the reform was implemented, more than 90% of temporary contracts had been extended at most once

[1] We show that in this framework a reduction in the hiring costs of temporary workers has two opposite effects on the threshold above which workers are upgraded to permanent contracts, making the final impact undetermined. However, when incentives for the transformation to permanent contracts are subsequently introduced, we might observe a lower conversion rates for workers hired on more flexible contracts.

[2] In fact, firms might be more willing to hire new workers, and see how they perform and they might not have enough incentives to upgrade them to permanent jobs. Even if a match turns out to be quite productive, firms might still prefer to fire the worker while the firing cost is low, and take a chance with a new worker, particularly when she is very cheap to hire.

(Table A1 in Appendix). In summary, a worker hired on a temporary basis after the *Poletti Decree* signs a relatively more flexible contract.

2.2 *Budget Law*

The 2015 *Budget Law* (effective in January 2015) introduced a sizeable hiring subsidy for new hires in open-ended contracts. The subsidy covered all new permanent workers hired by any firm from January to December 2015, provided the worker did not have a permanent contract in the previous 6 months and did not have a permanent contract with the same firm in the previous 3 months. The subsidy was a 3-year exemption from social security contributions up to a threshold of 8,060 per year, which was quite high compared with the average contributions typically paid by firms for workers (Sestito & Viviano, 2018). Conversions from temporary to permanent job contracts within a given firm were also subsidized. The hiring subsidy applied uniformly in larger and smaller firms and there was no firm size threshold associated to this policy (Boeri & Garibaldi, 2019). Thus, workers hired or transformed to a permanent contract after the *Budget Law* cost relatively less to their employers.

2.3 *Jobs Act*

The *Jobs Act* approved in March 2015 changed permanent contracts significantly. The new permanent contract for all new open-ended jobs is based on graded security, with severance payments steadily increasing with tenure. The severance payments are flat at 4 months for the first 2 years, and then increase with tenure up to a maximum of 24 monthly wages at a 12-year tenure. The *Jobs Act* also introduced a new form of out-of-court procedure, according to which the employer can pay the worker an indemnity equal to 2 monthly wages in the first 2 years of tenure and then an additional monthly wage per year of service, with a maximum amount of 18 monthly wages after 18 years of service. The acceptance of this transaction prevents any further dispute by the worker, i.e. appealing to courts for a unfair dismissal. Both parties have a strong incentive to settle the dispute through this procedure, since the sum paid is not subject to social contributions or taxation. The new graded security contract also replaced the worker reinstatement with a monetary compensation for economic unfair dismissals. The new dismissal rules apply to all new hires on an open-ended basis, and do not affect workers hired on permanent contracts before the implementation of the reform in firms with more than 15 employees, who continue to be protected by the reinstatement clause. Thus, workers hired or transformed to a permanent contract after the *Jobs Act* are subject to lower firing costs, in case of dismissal.

3 Literature

This paper relates to three main strands of the literature: (a) research on dual market segmentation and the employment protection gap between temporary and permanent contracts; (b) work on the potential scarring effects of the first job experience; and (c) papers analysing the effect of recent Italian labour market reforms.

The literature on labour market segmentation in dual economies is abundant (Bentolila, Cahuc, et al., 2012; Berton, 2008; Blanchard & Landier, 2001; Booth et al., 2002; Cahuc & Postel-Vinay, 2002). A large body of the empirical literature focuses on the probability to be upgraded to a permanent contract, with the aim of identifying the role of temporary contracts as stepping stones towards open-ended contracts or dead ends. In some countries such as Austria, Denmark, Sweden, UK, and USA, these jobs are shown to be used as screening devices to more stable jobs (Booth et al., 2002; Holmlund & Storrie, 2002), while in others evidence shows that they have become a source of excessive labour market volatility (Boeri & Garibaldi, 2009; de Graaf-Zijl et al., 2011; Dolado et al., 2013; Hoffmann et al., 2022; Tealdi, 2019). In this paper, although we focus on the probability of conversion of temporary contracts into permanent ones, we are particularly interested in the effectiveness of incentives to the conversion depending on the degree of flexibility of temporary contracts, which is novel in the literature.

This paper also relates to the literature studying how the workers' experience in the first few years of their careers affects their performance later in life. Within this literature, the paper which is closest to our work is by García-Pérez et al. (2019). Using Spanish social security data, the authors find that cohorts of native male high-school dropouts who entered the labour market just after the 1984 liberalization of temporary contracts experienced worse labour market outcomes than cohorts that just preceded them. Specifically, they spent 200 days at work less compared to the control group (a 7% drop), while their wages dropped by about 22% in the long run. One of the reasons is in the higher likelihood of young workers to work on temporary contracts long after their entrance in the labour market. Our paper differs along two main dimensions: (a) its broader context, i.e. it does not focus only on high-school dropouts and (b) the investigation of the role of incentives to the conversion to permanent contracts. Scarring effects for the case of Sweden are described by Nordstrom Skans (2011) who provide evidence on the impact of the first labour market experience on labour market performance up to 5 years later. Eliason and Storrie (2006) also show that displaced workers suffer both earnings losses and worsened labour market positions not only during a transitory period of adjustment but also in the longer run. Other contributions focus on young individuals at the beginning of their career and find that the effect of unemployment spells on the probability of finding a job and on wages is negative and long-lasting (Cockx & Picchio, 2013; Schmillen & Umkehrer, 2017). Scarring effects of temporary contracts have also been reported by Mooi-Reci and Dekker (2015) and Gorjón et al. (2021).

Finally, this paper is also related to the literature which studies the effect of the open-ended contract reforms implemented in Italy (Jobs Act and the 2015 Budget Law) on the creation of new open-ended jobs and the transformation of temporary contracts. Boeri and Garibaldi (2019) find evidence of a causal increase in permanent hirings in larger firms and an increase in the transformation from temporary to permanent contracts as large as 100 percent. Other papers (Ardito et al., 2023; Bovini & Viviano, 2018; Sestito & Viviano, 2018) complement the previous findings by showing that gross permanent hires and conversions of fixed-term positions have significantly benefited from the 2015–2016 hiring subsidies across all types of firms and, more smoothly, from the new regulation of dismissals introduced by the 2015 *Jobs Act* for medium-large firms.

4 Data

We link four distinct sources of administrative data. Our final analytical sample is therefore a unique source of information on the career of young workers.

The first database *Comunicazioni Obbligatorie* collects information on the universe of openings, terminations, and extensions or conversions of employment contracts. Importantly for the aim of this paper, it provides details on the hirings, firings, conversions and extensions of temporary contracts occurred in Italy during our period of observation (between January 2014 and December 2015). For each event recorded in this dataset, on top of the relevant pseudonymous identifiers (firm and worker), we also observe individual characteristics, such as gender, age, education level and nationality. In Section 8.1.2, we also use a publicly available representative sample of the *Comunicazioni Obbligatorie* to perform some robustness tests, as we do not have access to the population dataset for the years before 2014. In this sample, we observe per each individual the type of contract and the conversion date to a permanent contract; however, we do not know when the workers entered for the first time in the labour market.

The second data source is the *Italian Social Security Institute administrative dataset*, which is an employer–employee panel, which collects the working histories of the universe of Italian workers in the private sector (excluding agriculture). These data contain information on earnings, weeks worked, contract type, occupation, detailed industry codes, part-time vs. full-time status, etc.

The third source is the *firm level data* available at the Italian Social Security Institute which contains information on average wages, sectors and additional firm characteristics related to the composition of the workforce.

Finally, the firm database *CERVED* includes the full financial record of over 800,000 incorporated Italian companies. The data provide information about indicators of performance such as sales, revenues, liquidity, value added, etc.

For our analysis, we select young individuals who entered for the first time in the labour market between February 1 and April 30 of 2014 on a temporary contract, which lasts at least until December 2014. We identify as control those individuals whose contract started between 1 February and 21 March 21 and as treated those individuals whose contract started between 22 March and 30 April.

5 Identification strategy

Before the approval of the *Poletti Decree* in 2014, a firm wanting to hire a worker on a temporary contract was required to declare the reason behind this choice.[3] By reducing the hiring cost faced by employers, the removal of the obligation to report the reason for hiring on a temporary basis could potentially affect the conversion rate to permanent contracts and therefore the careers of new entrants in the labour market, particularly when incentives to the conversion are in place. The effect of the reduction in hiring costs on the probability of conversion to permanent employment is ambiguous ex ante as both job creation and job destruction are affected. However, when firing costs are reduced in a context in which more and less flexible temporary contracts share the same probability of conversion, workers hired on more flexible contracts are more likely to experience a lower conversion probability (Appendix D).

To empirically verify this theoretical prediction, we identify a period of time around the date of 21 March (date of the publication of the *Poletti Decree*) to set-up a natural experimental setting. We focus on young workers who step for the first time on the labour market. Specifically, we consider workers hired for the first time on a temporary contract between 1 February 2014 and 21 March 2014 (with a termination date after December 2014) as control group. Workers hired for the first time on a temporary contract between 22 March 2014 and 30 April 2014 (with a termination date after December 2014) represent, instead, our treated group. These two groups are very similar to each other, as they both include first-time entrants in the labour market, who only differ in their time of entrance being few weeks apart. With this choice, we implicitly assume that starting a temporary job in, say February 2014, or April 2014 for a new entrant it is a matter of chance and does not depend on individual characteristics. We test this assumption in Section 4 by showing that the normalized differences of observable characteristics in the two groups are lower than the critical value of 0.25. Moreover, we show that the characteristics of the hiring firms are very similar, and we provide evidence that the implementation of the *Poletti* reform did not affect neither the timing nor the hiring decisions of employers (Section 8.1.2). In fact, employers neither brought forward nor pushed backwards their hirings around the time of the *Poletti* reform. We follow new entrants until the end of 2015, thus observing their careers over three different stages: the first, between September and December 2014, in what we can call the pre-reform period; the second, between January and March 2015, when strong fiscal incentives were introduced to convert temporary contracts into permanent ones (*Budget Law*); and the third, between April and December 2015, when the firing costs associated with permanent contracts were severely reduced (*Jobs Act*), thus favouring the conversion of temporary contracts. In what follows we assume that these events (the implementation of the *Budget Law* and the *Jobs Act*) exogenously affected the conversion rate of workers in our analytical sample. To validate our assumption, in Section 6 we report evidence of parallel trends among treated and control individuals before the reforms for a number of outcomes.

A possible drawback of our strategy is that we do not know how long the individuals have been searching before finding the first job. This could potentially hide a problem of self-selection, i.e. better workers may have found a job faster, thus ending up in the control group. We believe this to be a minor issue in our set-up for several reasons. First, we show that observable characteristics are very similar between the two groups (Table 1); second, our results are unaffected when we include individual fixed effects to capture time-invariant individual characteristics, such as specific job searching abilities. Third, the very short difference in the time of entrance (few weeks) between individuals in the two groups is more likely to be attributed to chance rather than selection. Finally, we show that the parallel trend assumption, which is crucial for the validity of our identification strategy, holds (Figure 3).

Although there is no significant difference between employees' and firms' observable characteristics, as we show in Tables 1 and 2, a possible concern is that employers might have exerted more effort in selecting temporary workers before the implementation of the *Poletti Decree*. However, this does not seem to be problematic as our identification strategy is based on the fact that treated and control workers are hired few weeks apart (in Table B3, we also report a series of robustness

[3] Moreover, the temporary contract could be extended only once within the maximum length of 36 months. However, the normative change in the number of possible extensions had minimal take-up (Section 2).

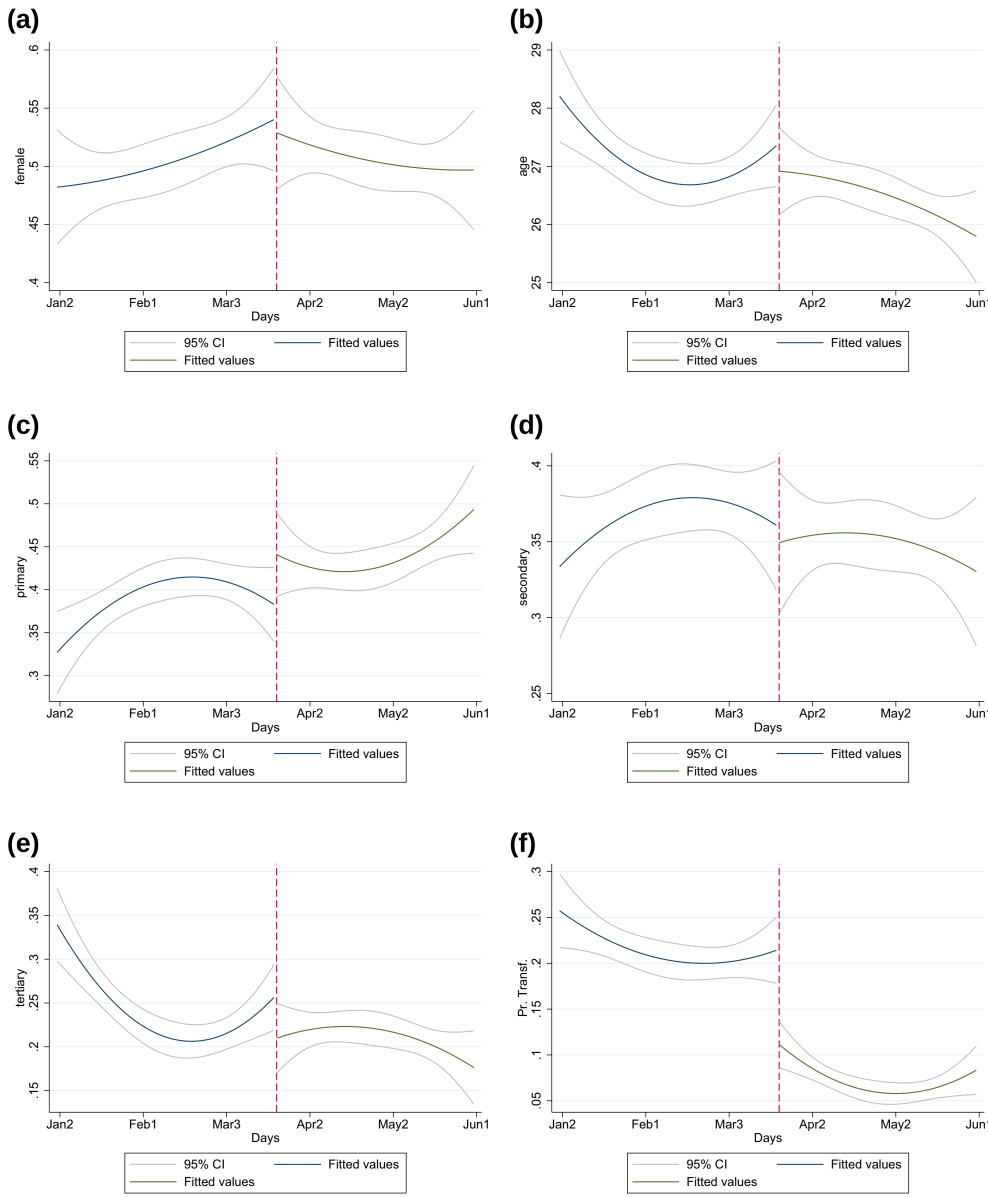

Figure 2. Average characteristics of workers by the starting date of the temporary contract: (a) female, (b) age, (c) primary education, (d) secondary education, (e) tertiary education, and (f) probability of conversion (as of March 2015).

Note: On the horizontal axis, the individual's date of hiring on the temporary contract is reported. The vertical dashed line indicates the implementation date of the Poletti Decree (21 March 2014). The fitted values are reported together with the 95% confidence intervals.

exercises, where we shrink this period to few days) and it is likely that the screening process lasted for weeks. This implies that workers in the treated group were screened before the implementation of the *Poletti Decree* too and most likely with similar effort. Bureau of Labor statistics (BLS) data for the USA show that the average vacancy duration in 2017 in the manufacturing sector, which is the largest sector in our data, was 30.7 days. The likely longer vacancy duration in a country like Italy, which is characterized by high levels of bureaucracy, reassures about the negligible chance of heterogeneous effort exerted by employers in selecting treated and control workers.

Table 1. Descriptive statistics of workers in treated and control groups

	Total	Control	Treated	Normalized difference
Age	26.81	26.92	26.67	−0.031
	(7.98)	(8.08)	(7.85)	
Under 25	0.568	0.568	0.570	0.004
	(0.50)	(0.50)	(0.50)	
Female	0.516	0.511	0.522	0.022
	(0.50)	(0.50)	(0.50)	
Primary education	0.412	0.399	0.428	0.059
	(0.49)	(0.49)	(0.50)	
Secondary education	0.373	0.381	0.364	−0.035
	(0.48)	(0.49)	(0.48)	
Tertiary education	0.215	0.220	0.208	−0.029
	(0.41)	(0.41)	(0.41)	
Wage (Sept 2014)	1224	1263	1189	0.037
	(1055)	(1156)	(953)	
Observations	4,465	2,438	2,027	

Note: Standard deviations in parenthesis. A rule of thumb is to worry about covariate imbalance if several covariates demonstrate a normalized difference of above 0.25.

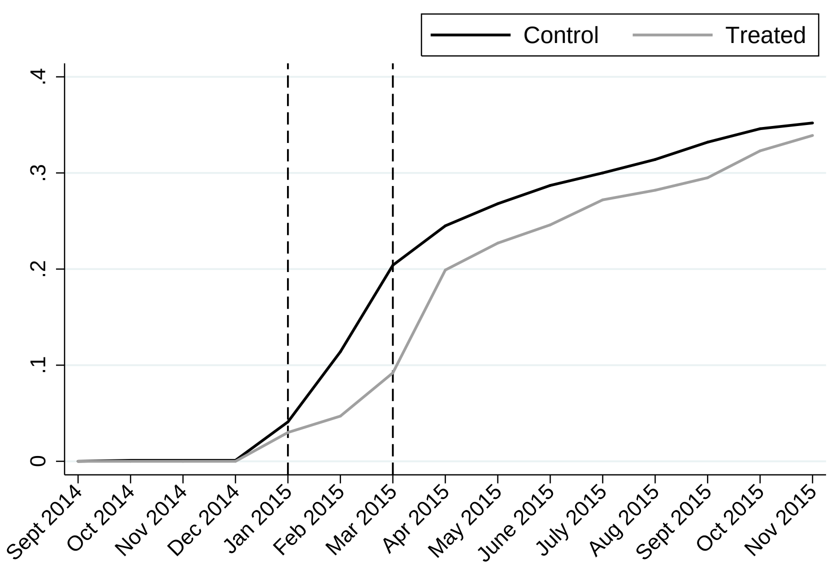

Figure 3. Average conversion probability in treated and control groups.

Note: The average conversion probability for workers in treated and control groups in each month between September 2014 and November 2015 is reported. The black line refers to workers in the control group, while the grey line refers to workers in the treated group.

6 Empirical strategy

6.1 Short-term effect on conversion rates

Let Y_{it} be a dummy indicator for each worker i in our sample which takes value one if the worker's contract is converted to permanent in each month t between September 2014 and December 2015.

Table 2. Descriptive statistics of firms in treated and control groups

	Total	Control	Treated	Normalized difference
Firm size	835	863	800	0.014
	(4223)	(2451)	(5679)	
Firm age	13.5	14.0	12.8	−0.087
	(13.5)	(13.8)	(13.15)	
Wage	16,022	16,299	15,684	−0.049
	(12,610)	(11,972)	(13,340)	
Share part-time	0.45	0.44	0.47	0.079
	(0.38)	(0.38)	(0.38)	
Share temporary	0.42	0.41	0.43	0.053
	(0.38)	(0.37)	(0.38)	
Observations	4,444	2,426	2,018	
Fixed assets	57,125	69,314	42,325	−0.085
	(324,829)	(385,609)	(229,708)	
Value of production	242,162	272,365	205,492	−0.065
	(1,038,103)	(1,067,691)	(1,000,206)	
Revenues	241,308	271,275	204,925	−0.064
	(1,037,214)	(1,065,597)	(1,000,893)	
Value added	49,567	50,129	48,885	−0.005
	(233,111)	(158,476)	(299,788)	
Observations	2,997	1,636	1,360	

Note: Standard deviations in parenthesis. A rule of thumb is to worry about covariate imbalance if several covariates demonstrate a normalized difference of above 0.25.

We estimate the following generalized difference in differences regression through Ordinary Least Squares (OLS):

$$Y_{it} = \alpha + \sum_{t=1}^{T} \sigma_t * \mathrm{Month}_t + \sum_{t=1}^{T} \gamma_t * (\mathrm{Month}_t \times \mathrm{Treated}_i) + \eta_i + \epsilon_{it}, \tag{1}$$

where Treated $_i$ is a dummy variable which takes value one if the worker is in the treated group and zero otherwise. Month_t, $\forall t = 1, \ldots, T$, are a set of dummies identifying months starting from September 2014 to December 2015 (where the baseline month is November 2014). The coefficients $\{\gamma_1, \gamma_2, \ldots, \gamma_T\}$ of the interactions between the $\mathrm{Treated}_i$ and the Month_t variables identify a set of average treatment effects on treated individuals under the standard parallel trend assumption. η_i captures individual fixed effects and ϵ_i is an idiosyncratic error term. It is worth to notice that our policy is non-staggered. This implies that we can use standard econometrics techniques and specify two-way fixed-effect estimation to retrieve the causal effect of interest, with errors clustered at individual level. The baseline regression is reported in a dynamic plot, showing all the estimated coefficients for the average treatment effects (Figure 4). Later, to ease the reading of our results, we report in Tables 4–7 the estimates obtained by pooling together both interactions and time dummies. *Post*1 identifies the period January–March 2015, immediately after the introduction of the *Budget Law*. *Post*2 identifies the period April–December 2015, after the *Jobs Act* reform.

6.2 Longer term effect on wages

In the second part of our empirical analysis we are interested in estimating the impact of the slowdown in the conversion rate to permanent employment on future wages (12 and 24 months later). We expect that delayed conversions would have a negative impact on future wages.

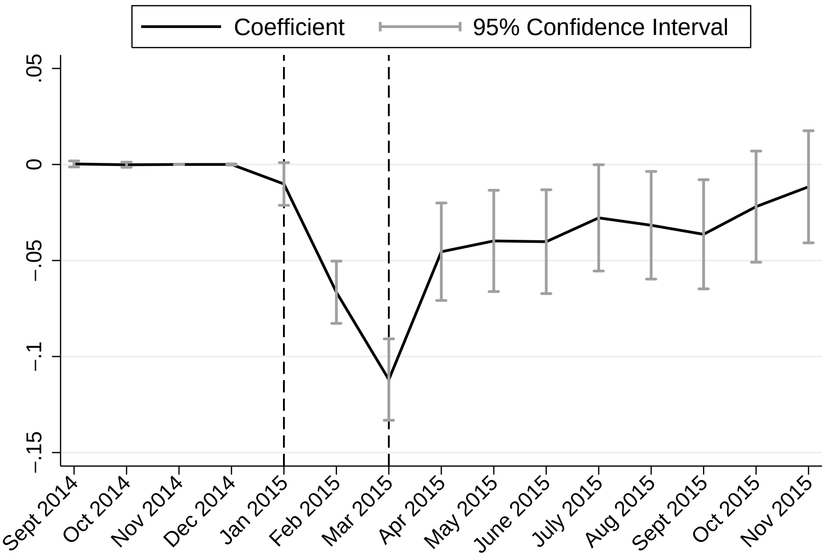

Figure 4. Treatment effects over months.

Note: The coefficients of the interaction of the treated variable and each month from September 2014 to November 2015 are reported, together with the 95% confidence intervals. The vertical dashed lines indicate the times of the two reforms which introduced fiscal incentives to the conversion to permanent contracts: the grey line refers to the Budget Law (January 2015) and the black line to the Jobs Act (March 2015).

Table 3. Estimation results

	Probability of transformation
Treated × Post1	−0.063***
	(0.000)
Treated × Post2	−0.032**
	(0.016)
Post1	0.119***
	(0.000)
Post2	0.304***
	(0.000)
Constant	0.001
	(0.839)
Workers	4,465
Observations	66,598

Note: The dependent variable is the individual probability of being upgraded to a permanent contract. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

We adopt an instrumental variable approach and we use the exogenous variation of being hired on a more flexible temporary contract interacted with the time period after the implementation of the reforms (Budget Law and Jobs Act) as an instrument. For consistency, in this analysis, we use the same sample as in Section 8.1. To ease the economic interpretation of the coefficients, in the first stage we use as dependent variable the probability of not being converted to a permanent contract, $C_{it} = 1 - Y_{it}$, where C_{it} is a dummy variable equal to zero if the contract is permanent. C_{it} is also the endogenous variable in the second stage, where Wage_{it+12} or Wage_{it+24} are the outcome variables of interest.

Table 4. Estimation results by individual characteristics

	Sex	Age	Education
Treated × Post1	−0.076***	−0.077***	−0.062***
	(0.000)	(0.000)	(0.000)
Treated × Post2	−0.024	−0.010	−0.005
	(0.230)	(0.658)	(0.761)
Treated × Post1 × Females	0.025*		
	(0.071)		
Treated × Post2 × Females	0.044		
	(0.121)		
Treated × Post1 × Under 25		0.026	
		(0.227)	
Treated × Post2 × Under 25		0.014	
		(0.642)	
Treated × Post1 × Tertiary			−0.002
			(0.925)
Treated × Post2 × Tertiary			0.016
			(0.637)
Workers	4,465	4,465	4,465
Observations	296,915	296,915	296,915

Note: The dependent variable is the individual probability of being upgraded to a permanent contract. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

In doing so, we compute a naive Wald-IV estimate in two stages, the second stage being:

$$\text{Wage}_{it+\tau} = \alpha + \beta * \hat{C}_{it} + \eta_i + \epsilon_{it}, \tag{2}$$

where $\tau = \{12, 24\}$. The first stage is

$$C_{it} = \phi + \delta\,\text{Post}_t + \gamma\,\text{Post}_t \times \text{Treated}_i + \eta_i + u_{it}. \tag{3}$$

By using this strategy we are implicitly assuming that the effect on future wages occurs only through the different conversion probability to permanent employment between treated and control individuals. This assumption seems plausible especially after conditioning for individual fixed effects. The coefficient of interest, β, identifies the local average treatment effect of the probability of not being converted to a permanent contract on future wages. Within this set-up, compliers are all those individuals who have experienced a lower conversion probability due to the fact that they have been hired for the the first time in their career on a more flexible temporary contract after the implementation of the *Poletti Decree*. Once again, to ease the reading of our results we pool together time dummies in the period *Pre* reforms (before January 2015) and *Post* reforms (January–December 2015).[4] We expect the sign of the coefficient in the first stage to be positive, and the effect on wages in the second stage to be negative, due to the different evolution of the working careers of treated and control workers.

[4] In January 2015 the Budget Law was implemented, and in March 2015 the Jobs Act was signed into law.The *Post* dummy covers the period after which both reforms which introduced incentives for the conversion of temporary contracts into permanent ones were implemented.

Table 5. Estimation results by firm's characteristics

	Size	Age	Part-time share	Temporary share
Treated × Post1	−0.062***	−0.063***	−0.074***	−0.077***
	(0.000)	(0.000)	(0.000)	(0.000)
Treated × Post2	−0.023	−0.038**	−0.056***	−0.055***
	(0.212)	(0.030)	(0.002)	(0.002)
Treated × Post1 × Large size	(0.003)			
	(0.828)			
Treated × Post2 × Large size	(0.022)			
	(0.379)			
Treated × Post1 × Old		(0.000)		
		(0.999)		
Treated × Post2 × Old		(0.014)		
		(0.576)		
Treated × Post1 × High Part-Time			0.022	
			(0.109)	
Treated × Post2 × High Part-Time			0.050**	
			(0.048)	
Treated × Post1 × High Temporary				0.026**
				(0.049)
Treated × Post2 × High Temporary				0.048**
				(0.049)
Workers	4,444	4,444	4,444	4,444
Observations	65,236	65,236	65,236	65,236

Note: The dependent variable is the individual probability of being upgraded to a permanent contract. Firms are split above and below the median value of each variable. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

7 Descriptive statistics

In Table 1, we report descriptive statistics for the sample of selected workers. In assessing covariate balance, our goal is to gauge whether the covariates of treatment and control groups demonstrate sufficient overlap, which is confirmed by the values of the normalized differences being below 0.25 for all the characteristics reported (Imbens & Wooldridge, 2009).

Our sample includes 4,465 workers, of which 2,438 in the control group and 2,027 in the treated group. The individual characteristics are very similar across the two groups. The average age is approximately 26 years old, the sample is split evenly among men and women, and in both samples approximately 41% of workers have a primary level of education, 37% have a secondary level of education and the remaining 22% a tertiary level of education. Finally, approximately 57% of workers in both groups are less than 25 years old. When we link our sample with the INPS firm level data, we lose 53 observations, bringing our sample to 4,444 observations. The average size of the firms in which treated and control workers are employed is approximately 835 employees[5] and the average firm age is 13 years (Table 2). On average, 45% of employees work part-time and 42% are hired on a temporary contract; their average annual wage is close to 16.000.

We then link our sample with the CERVED dataset, and we lose additional 1,447 observations, bringing our sample to 2,997 observations. The firms in which treated and control workers are hired show also comparable financial structures and measures of productivity, as shown by their

[5] The size distribution of treated and control firms is very skewed and averages are not very informative; the median of the overall sample is 45 employees.

Table 6. Estimation results by firms' characteristics

	Fixed assets	Average wage	Liquidity	Geographical location
Treated × Post1	−0.082***	−0.054***	−0.086***	−0.047***
	(0.000)	(0.000)	(0.000)	(0.003)
Treated × Post2	−0.053**	−0.015	−0.049**	−0.030
	(0.030)	(0.376)	(0.048)	(0.286)
Treated × Post1 × High K	−0.024			
	(0.198)			
Treated × Post2 × High K	−0.016			
	(0.640)			
Treated × Post1 × High wage		−0.022		
		(0.116)		
Treated × Post2 × High wage		−0.037		
		(0.177)		
Treated × Post1 × High cash			−0.031*	
			(0.090)	
Treated × Post2 × High cash			−0.006	
			(0.847)	
Treated × Post1 × North				−0.025
				(0.173)
Treated × Post2 × North				−0.003
				(0.920)
Treated × Post1 × Centre				−0.007
				(0.716)
Treated × Post2 × Centre				−0.003
				(0.947)
Workers	2,997	4,444	2,997	4,444
Observations	41,822	65,236	41,822	65,236

Note: The dependent variable is the individual probability of being upgraded to a permanent contract. Firms are split above and below the median value of each variable. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

similar fixed and liquid assets, production values, revenues and value added. Overall, these statistics confirm that the workers in our sample share similar characteristics and are employed in firms sharing similar features.

In Figure 2, we report descriptive statistics of treated and control workers, per each month in which their temporary contract started. Specifically, we report the percentage of females (Figure 2a), average age (Figure 2b), share of workers by education level (Figure 2c–e) and importantly the probability of conversion into a permanent contract as of March 2015 (Figure 2f). Overall, we observe similar characteristics among workers in the treated and control groups; in other words, the workers' features are smooth over the starting date of the contract. However, we report a striking statistically significant difference in the probability of conversion into permanent contracts as of March 2015, i.e. we show a sharp discontinuity around the time of the implementation of the *Poletti Decree*, with the value being much higher for workers in the control group. This evidence confirms that our treatment assignment can be considered as good as random.

Figure 3 reports the average probability of transformation of control and treated workers in each month in the period considered. In the last 4 months of 2014, before the implementation of the *Budget Law*, the probability for an individual to be converted to a permanent contract was the same among workers in the treated and control groups, providing evidence of a parallel

Table 7. Estimation results by firms' productivity level

	Value of production	Value added	Revenues
Treated × Post1	−0.086***	−0.068***	−0.084***
	(0.002)	(0.000)	(0.000)
Treated × Post2	−0.060**	0.033	−0.056**
	(0.014)	(0.176)	(0.022)
Treated × Post1 × High production	−0.034*		
	(0.068)		
Treated × Post2 × High production	−0.032		
	(0.329)		
Treated × Post1 × High VA		−0.004	
		(0.817)	
Treated × Post2 × High VA		−0.024	
		(0.460)	
Treated × Post1 × High revenues			0.029
			(0.112)
Treated × Post2 × High revenues			0.023
			(0.481)
Workers	2,997	2,997	2,997
Observations	41,822	41,822	41,822

Note: The dependent variable is the individual probability of being upgraded to a permanent contract. Firms are split above and below the median value of each variable. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

trend in the pre-treatment period. When the *Budget Law* is implemented in January 2015, as expected, the average probability of being converted increases significantly among workers in the control group, but only slightly among workers in the treated group. The gap between the two groups keeps growing until March 2015, when it reaches its peak at 20%, approximately. After the *Jobs Act* is implemented (March 2015), we observe a large increase in the probability of conversion among workers in the treated group. Although, the gap in the average probability of conversion between the two groups shrinks, the probability of conversion remains persistently lower (by approximately 5 percentage points) among treated individuals until the end of 2015.

8 Results

8.1 Effects on conversion probability

We plot in Figure 4 the coefficients of the interaction of the treated variable and the time variable, obtained by estimating equation (3).

We observe the absence of any pre-trend, in the months between September and December 2014, which confirms that the parallel trend assumption holds. In the period from January to March 2015, the probability of conversion declines significantly among treated individuals (compared to individuals in the control group), reaching the lowest point in March 2015, with a decline of 11.2 percentage points. After the Jobs Act (March 2015), the probability of conversion increases among treated workers, but remains significantly and persistently lower by 3 percentage points until the end of the observation period.

We quantify the average treatment effect (on the treated) in the period after the *Budget Law*, but before the *Jobs Act* (January–March 2015) and in the period after the *Jobs Act* (April–December 2015). We find a negative and statistically significant effect in both periods after the reforms (Table 3). Specifically, the probability of conversion is 6.3 percentage points lower among treated individuals in the January–March period and 3.2 percentage points lower in the April–December period. Thus, being hired on a more flexible contract significantly reduces the probability of conversion to a permanent contract. This is particularly striking as the workers in the treated and control groups are very similar to each other and work in firms which share similar features. This result is consistent with the theoretical prediction of a search and matching model, in which temporary contracts differ according to the associated hiring costs but, as demonstrated by the parallel trend, share the same conversion probability (Section D in Appendix). In this set-up, when incentives to the conversion to permanent contracts are introduced (through a decrease in the firing cost associated with permanent contracts), the shift in the lay-off curve leads to a lower probability of conversion among workers hired on more flexible temporary contracts.[6]

8.1.1 Heterogeneous effects

To assess whether among treated workers, there are differences in the conversion probability depending on the characteristics of the workers or the firms they are hired on, we perform a series of triple difference-in-differences estimations. First, we interact the treatment status with two dummies identifying the periods post-reforms and a battery of individual characteristics, including age, gender and education. Second, we interact the treatment status with two dummies identifying the periods post-reforms and a set of firm characteristics, including age, size, workforce composition, geographical location, capitalization, liquidity and wages paid. Finally, we interact the treatment status with two dummies identifying the periods post-reforms and three indicators of firm's performance. Results are reported in Tables 4–7. Overall, the very few statistically significant coefficients suggest that the reduction in the conversion rates affected almost homogeneously all treated workers hired on a more flexible temporary contract, independently on their or their firm's features.

Specifically, in terms of individual characteristics (Table 4), we find a marginally higher conversion probability among females, compared to males, but only in the January–March period. When we analyse the difference between workers below and above 25 years old, we do not find any significant difference in the probability of conversion between the two classes. Even when we focus on the workers' level of education, we do not find any statistically significant difference between the groups.

We then estimate equation (3) by differentiating between firms in which treated and control workers are hired, differing along dimensions such as age, size and composition of the workforce (Table 5).[7] We find no significant differences by size and by age. We also find no difference in the probability of conversion between firms with high and low shares of part-time workers in the first period (January–March); however, we find a significant difference in the second period (March–December). Finally, in firms with lower shares of temporary workers the probability of conversion is lower in both periods.

We then distinguish firms according to their level of capitalization (fixed assets), the average salary paid, liquidity, and their geographical location (Table 6). We find no difference in the probability of conversion between firms with different level of capitalization or paying higher wages. We only find a marginally lower effect among firms with higher levels of liquidity, but only in the first period (January–March 2015). We also do not report any significant effect by geographical location: operating in the North, Centre, or South of Italy does not affect the probability of converting temporary contracts to permanent contracts.

We then classify firms according to three different measures of productivity. We consider the value of production, total revenues and value added (Table 7). Our results suggest once again that the probability of conversion is not very different across firms with different levels of productivity. The only marginal effect regards firms with higher levels of production; however, the coefficient is statistically significant only in the first period (January–March 2015).

[6] This is due to the steeper slope of the hiring curve associated with more flexible temporary contracts, i.e. the trade-off between the value of unemployment and the threshold conversion rate is larger. Due to the lower turnover costs, firms have in fact incentives to hire new workers, and observe their performance. Moreover, firms might be reluctant to convert them into permanent jobs. Although the productivity of a match might turn out to be quite high, firms might still prefer to fire the worker, as the firing cost is low, and take a chance with a new worker, as the hiring cost is low.

[7] In each estimation we interact the treatment with the two dummies identifying the two time periods and a dummy variable which identifies whether the firm's value is above or below the median.

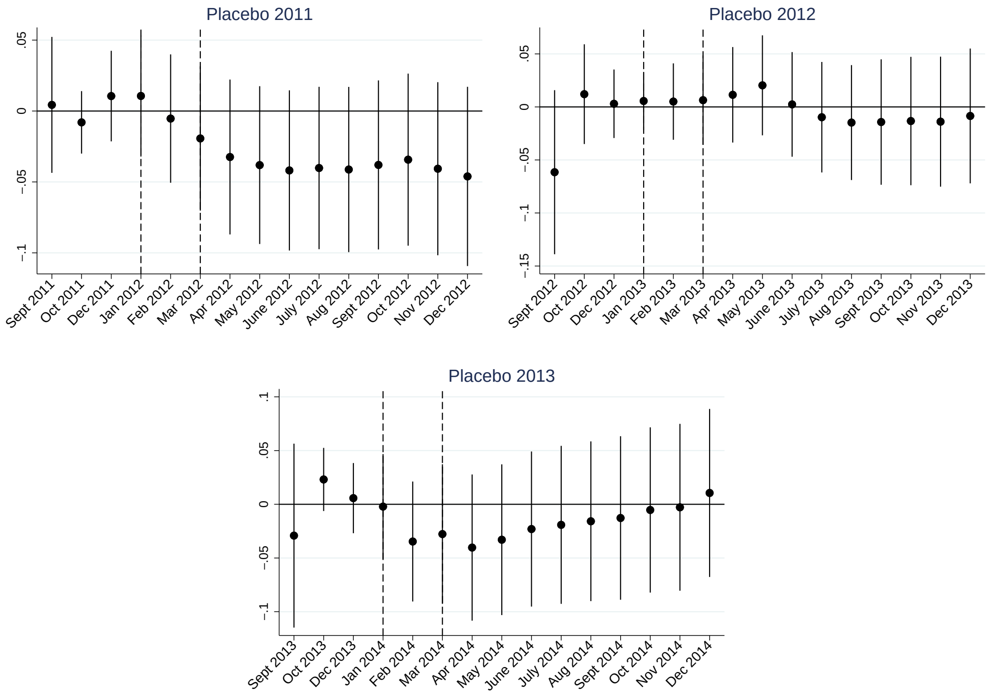

Figure 5. Placebo estimates.

Note: The coefficients of the interaction of the treated variable and each month between September of the placebo year and November of the following year are reported, together with the 95% confidence intervals. The vertical dashed lines indicate the corresponding months of the two reforms which introduced fiscal incentives to the conversion to permanent contracts: the grey line refers to the Budget Law (January of the year following the placebo) and the black line to the Jobs Act (March of the year following the placebo). *Source*: Sample of the Comunicazioni Obbligatorie dataset (CICO) as provided by the Italian Ministry of Labour.

Overall, workers and firms characteristics seem to matter very little in affecting the probability of conversion. Workers hired on more flexible temporary contracts are equally penalized by the increased flexibility of the contracts, independently on who they are and where they work. These results show that the average treatment effect (on the treated) of having a flexible temporary contract on the probability of being hired permanently is strongly homogeneous.

8.1.2 Robustness

To test the validity of our strategy, we first propose a classical placebo test. Using a (publicly available) representative sample of *Comunicazioni Obbligatorie*[8] for the years 2011–2013, we estimate equation (3) over different years (Figure 5). In these regressions, we sample treated and control units as in our original experiment by month of entry (control from 1 February to 21 March and treated from 21 March to 30 April). To recreate a sample which shares similar characteristics with our analytical sample, as we do not observe whether workers are first-time entrants, we limit individuals in our sample to be below 35 years old.[9] In all proposed scenarios there are no statistically significant difference in differences coefficients. These results reassure us about the effect visible in 2014 being due to the reforms (*Budget Law* and *Jobs Act*), which affected asymmetrically similar workers hired on more flexible temporary contracts.

[8] Unfortunately we do not have access to the *Comunicazioni Obbligatorie* population dataset before 2014. Therefore we use a publicly available dataset, which includes a representative sample of the *Comunicazioni Obbligatorie*. In this sample, we still observe per each individual the type of contract and the conversion date to a permanent contract; however, we do not know when the workers entered for the first time in the labour market.

[9] Results are unchanged when we vary the age threshold.

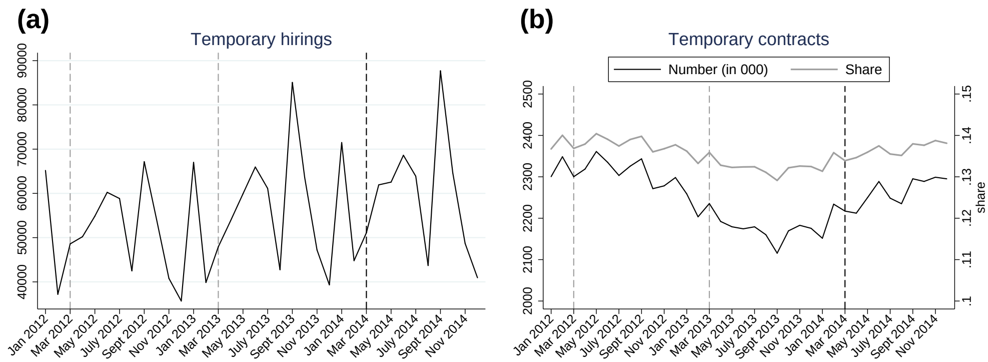

Figure 6. Flow and stock of temporary contracts. (a) Monthly flow of temporary contracts. *Source*: Comunicazioni Obbligatorie (sample). (b) Monthly stock of temporary contracts. *Source*: Italian Institute of Statistics.

Note: In both graphs, the vertical grey dashed lines indicate flow and stock of temporary employment in March 2012 and 2013. The vertical black dashed lines identify flow and stock of temporary employment in March 2014, when the *Poletti Decree* was implemented. The share of temporary employment is computed as the ratio of temporary employees on total number of employees (temporary and permanent).

A possible concern with our strategy is that in anticipation of the *Poletti Decree* being implemented, employers could have postponed the decision of hiring a temporary worker for a non-urgent position. If that would be the case, we would observe a decrease in the number of hirings on a temporary basis before the implementation of the *Poletti Decree* and an increase right after. We provide evidence against this hypothesis, by reporting in Figure 6a the flow of workers hired on temporary contracts per each month in the period between January 2012 and December 2014.[10] Using employment data from the National Institute of Statistics, we report in Figure 6b also the stock of workers hired on temporary contracts per each month in the period between January 2012 and December 2014. Across both statistics, we observe seasonal fluctuations as expected, but we do not detect anomalies around the date of the *Poletti Decree* (March 2014).

In order to provide further evidence on the potential firms' strategic behaviour, we also perform a difference in differences estimation where we compare the probability to be hired on a temporary contract in the months before and after the implementation of the *Poletti Decree* in the 2 years before the reform (2012–2013) and in the year of the reform (2014). We identify treated and control units as in our original experiment, we denote 2014 as the post-treatment period and we use the probability to be hired on a temporary contract as the dependent variable. If temporary hirings had changed in anticipation of the *Poletti Decree*, we would observe a different pattern in the probability of being hired on a temporary basis in the first 5 months of 2014 (before and after the implementation of the *Poletti Decree* reform) compared to the same months in previous years. However, the coefficient of interest is not statistically different from zero, thus confirming the absence of any anticipation effect (Column 1, Table 8). We also report the result of a similar regression, when using as dependent variable the probability to be hired on any employment contract, independently on the type. Even in this case, there is no significantly different pattern in 2014 compared to previous years (Column 2, Table 8).

In Appendix B, we report few additional robustness exercises. First, we add different set of trends to the baseline equation (Table B1). Specifically, we introduce regional and sectoral trends in order to account for possible heterogeneous cyclical effects, and gender specific or age specific trends to account for the possibility that the cycle affects different groups of workers asymmetrically over time. Results remain very similar across the different specifications. Second, we use different definitions of transitions to a permanent contract and we show that our results are not affected by a specific definition of treatment (Table B2). Third, we identify treated and control groups based on different windows of time around the implementation of the *Poletti Decree*. Shrinking the period of selection does not change the sign and the magnitude of the results (Table B3). Finally, we report results when performing survival analysis (Table B4). Also this final

[10] These are computed using the publicly available sample of the *Comunicazioni Obbligatorie* dataset.

Table 8. Probability to be hired

	Temporary contract	Any contract
Treated × Post	−0.034	−0.0043
	(0.993)	(0.874)
Observations	6,959,352	21,688,665

Note: Post is a dummy variable taking value 1 in 2014 and 0 in 2013 and 2012. Treated are workers hired immediately after the Poletti Decree, as in our baseline. In Column 1, the dependent variable is the probability to be hired on a temporary contract; In Column 2, the dependent variable is the probability to be hired on any contract. *p*-Values are reported in parenthesis.

Table 9. Wage estimation results

	Log wage after 12 months			Log wage after 24 months		
	Ordinary least squares	IV		Ordinary least squares	IV	
		First stage	Second stage		First stage	Second stage
	−0.049***	0.052***	−0.107***	−0.054***	0.059***	−0.178***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	47,812	47,812	47,812	44,970	44,970	44,970
Kleibergen-Paap (KP) test			1026.74			941.13

Note: The dependent variable in the first stage is the probability of not being converted. The dependent variable in the second stage is the logarithm of the wage after 12 and 24 months. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

check clearly replicates our results, thus confirming that the effect is identified even under very different (non-linear) econometric specifications.

8.2 The scarring effect on the initial phase of the workers' career

In Table 9, we report the results of the IV specification described in Section 6.2.[11]

Columns 1 and 4 include the OLS estimations of our endogenous variable (C_{it} is equal zero if the contract is permanent) on wages. As expected we find a negative correlation with both wages at 12 and 24 months, and the effect is increasing with time. Specifically, the wages are 4.9% lower 1 year ahead and 5.4% lower 2 years ahead. We then estimate the IV model and, consistently with the OLS results, we find a significant negative effect on both wages (Columns 3 and 6 in Table 9). Specifically, the wages are 10.7% lower after 12 months and 17.8% lower after 24 months, thus larger compared to the OLS estimates. The IV estimates are local average treatment effects (LATE) and the compliers of this natural experiment are those who have a higher probability of not being converted to permanent employment. Results from the first stage (Columns 2 and 5 in Table 9) are positive and significant in both regressions, i.e. being hired on a more flexible contract is associated with a higher probability of not being upgraded to a permanent contract.

Considering that treated and control individuals are very similar and are hired (for the first time in their career) in very similar firms, this can be explained by the lower human capital accumulation, due to the longer period of time spent on a temporary contract (Cabrales et al., 2014; Dolado et al., 2002). Empirical evidence suggests that workers on temporary contracts are less productive as firms tend to invest less in training for temporary employees compared to permanent employees. This is confirmed by a study based on the Survey of Adult Skills for the period 2008–2013 in 21 countries reports that being on an temporary contract reduces the probability of receiving

[11] Reduced-form regressions are reported in Table C1 in Appendix.

employer-sponsored training by 14% (OECD, 2014). This reduced training, which is justified by the lower returns to the training investment made by the firms, might translate into lower productivity and lower wages. A recent study by Garcia-Louzao et al. (2023) shows that limited training provided to temporary workers hampers their human capital accumulation, resulting in lower returns to experience compared to permanent workers. Another potential explanation is around the shopping for better matches of workers who have been upgraded to a permanent contract. This would lead to workers sorting into potentially higher paying firms, thus contributing to the estimated wage gap. Finally, negative duration dependence of temporary employment might affect the probability of workers to be upgraded to a permanent contract, thus reducing their future labour market opportunities. Evidence shows that the duration dependence for workers on temporary contracts is not linear (Gagliarducci, 2005); it might be positive in the first few months, but may become negative afterwards, particularly among specific categories of workers, such as men and women with children (D'Addio & Rosholm, 2005).

9 Conclusions

In this paper, we aim to understand if the flexibility of temporary contracts reduces the effectiveness of policies aiming at stimulating the conversion of workers into permanent contracts. Specifically, we investigate whether starting the career with a more flexible temporary contract may affect the career progression of young people, when such incentives are in place. We find that workers whose first job experience is through a more flexible temporary contract have a lower probability of being upgraded into a permanent contract, when incentives to the transformation are introduced, even if they are very similar to their counterpart and are hired in very similar firms. This result is consistent with the theoretical prediction of a search and matching model, in which two types of temporary contracts coexist (differing only on in terms of the hiring cost) and incentives to the conversion to permanent contracts are introduced through a reduction in firing costs. These lower conversion rates translate into significantly lower wages up to 2 years down their career paths, which might be ascribable to a lower accumulation of human capital. These results highlight the importance of a 'good start' for young workers who step for the first time in the labour market. As most youngsters begin their labour market careers with a temporary job, setting the features of the contract appropriately is paramount in ensuring an easier transition to a permanent position and the achievement of better labour market opportunities, particularly when incentives to the transformation to permanent employment are in place. The evidence found informs policy makers about intrinsic obstacles to the effectiveness of policies which aim at stimulating the conversion of temporary contracts into permanent ones. This is particularly relevant in European countries, where such reforms have been implemented widely, sometimes with disappointing outcomes (Bentolila, Dolado, et al., 2012; Bentolila et al., 2020). As a result, young workers have experienced over the last three decades a significant deterioration in their labour market outcomes (Bentolila et al., 2021). In the current post-COVID time, in which the share of temporary workers is increasing once again, understanding how to best manage the regulations of these contracts in association with policies to promote their conversion is a crucial task to improve the future careers of young individuals.

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Conflict of interest: None declared.

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Data availability

In this article, we use administrative data for Italy, as provided by the Italian Social Security Institute (INPS). The use of these data, which include the working histories of the entire Italian population, is restricted to researchers who have been awarded access (VisitINPS Scholars), as part of a peer-review selection process. For privacy reasons, the data are accessible only from the computers located in the institute (safe havens) and are not publicly available.

Appendix A

A.1 Share of temporary contracts by number of extensions

Table A1. Shares of temporary contracts by number of extensions over time

Year	# Of extensions (% contracts)					
	0	1	2	3	4	5
2010	74.0	23.5	1.3	0.4	0.2	0.1
2011	74.0	23.4	1.5	0.4	0.2	0.1
2012	75.0	22.3	1.8	0.6	0.2	0.1
2013	75.0	21.9	1.8	0.5	0.2	0.1
2014	73.0	19.3	4.4	1.7	0.8	0.5
2015	73.0	18.0	5.2	2.3	1.0	0.6
2016	70.0	20.5	6.0	2.0	0.7	0.3

Source: Veneto Working Histories.

Appendix B. Robustness

B.1 Inclusion of trends

In Column 1 of Table B1, we report the baseline estimation without the inclusion of trends. In Column 2, we include a regional trend, while in Column 3 we include in addition to the regional trend also the sectoral trend. In Column 4, we also include the square of both trends. Finally, in Column 5, we also add trends for gender and education.

Table B1. Regressions with different trends

	(1)	(2)	(3)	(4)	(5)
Treated × Post1	−0.0629***	−0.0632***	−0.0616***	−0.0620***	−0.0619**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated × Post2	−0.0319**	−0.0328**	−0.0290**	−0.0291**	−0.0289**
	(0.016)	(0.013)	(0.028)	(0.028)	(0.029)
Workers	4,465	4,465	4,465	4,465	4,465
Observations	66,598	66,598	66,598	66,598	66,598
Region trend	No	Yes	Yes	Yes	Yes
Sector trend	No	No	Yes	Yes	Yes
Region trend square	No	No	No	Yes	Yes
Sector trend square	No	No	No	Yes	Yes
Gender trend	No	No	No	No	Yes
Education trend	No	No	No	No	Yes

Note: *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

B.2 Different definitions of transitions

In Column 1 of Table B2, we restrict the transitions to include only the upgrading from temporary to permanent contracts as declared by the employers. In Column 2, we report the baseline, which includes all the transitions from temporary to permanent contracts within the same firm. Finally, in Column 3, we report all transitions from temporary to permanent contracts, including those to a different firm.

Table B2. Estimation results by definition of transitions

	Declared upgrading within firms	Declared upgrading within firms and transitions within firm	Declared upgrading within firms and transitions within and across firms
Treated × Post1	−0.049***	−0.062***	−0.068***
	(0.000)	(0.000)	(0.000)
Treated × Post2	−0.015***	−0.034***	−0.039***
	(0.000)	(0.000)	(0.000)
Workers	4,465	4,465	4,465
Observations	66,598	66,598	66,598

Note: p-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

B.3 Different time windows

In Column 1 of Table B3, we report the estimation when including in our sample all individuals hired on a temporary contract for the first time between 15 January and 15 May. In Column 2, all individuals hired on a temporary contract for the first time between 1 February and 30 April (baseline model). In Column 3, all individuals hired on a temporary contract for the first time between 15 February and 15 April. Finally, in Column 4, the sample includes all individuals hired on a temporary contract for the first time between 1 March and 10 April.

Table B3. Estimation results by definition of treated/control groups

	Sample 1	Sample 2	Sample 3	Sample 4
Treated × Post1	−0.071***	−0.063***	−0.062***	−0.050***
	(0.000)	(0.013)	(0.000)	(0.000)
Treated × Post2	−0.032***	−0.034***	−0.045***	−0.036**
	(0.003)	(0.010)	(0.004)	(0.027)
Workers	6,378	4,465	3,235	2,468
Observations	95,595	66,598	48,493	36,988

Note: p-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

B.4 Survival analysis

In this section, we also perform survival analysis to compute the expected duration of time until transformation. Specifically, we report estimates of the Cox proportional hazard model and the Weibull accelerated failure time (AFT) model. We also test for the best fit. We report the hazard ratios for the two regressions in Table B4). The below-one statistically significant values across both specifications confirm our previous findings that workers in the treated group have a relative lower risk of being transformed into a permanent contract compared to workers in the control group. The Akaike's information criterion (AIC) and Bayesian information criterion (BIC) tests point to the Weibull model as the best fit for our data.

Table B4. Survival analysis

	Cox	Weibull
Treated	0.878***	0.912**
	(0.000)	(0.011)
Age	Yes	Yes
Gender	Yes	Yes
Education	Yes	Yes
Akaike's information criterion (AIC)	54,947	−13,076
Bayesian information criterion (BIC)	54,981	−13,028

Note: Hazard ratios are reported for both the Cox proportional hazard model and the Weibull AFT model. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

Appendix C. Reduced-form regressions

We report the reduced-form regression of equation (2), where we estimate the (log) wage at 12 and 24 months on the variable 'Post' and the interaction between 'Treated' and 'Post'. As expected, the ITT coefficients in this regression have the same sign (negative) of the coefficient in the IV regression, but are lower in magnitude and are estimated less precisely.

The variables 'Post' and the interaction of 'Treated' and 'Post' are the excluded instruments in the IV estimation (Section 6.2). Despite not being a weak instrument test, the results of the joint significance tests are as expected aligned with those of the KP test reported in Table 9.

Table C1. Wage estimation results—reduced form

	Log wage after 12 months	Log wage after 24 months
Treated×Post	−0.011	−0.034*
	(0.474)	(0.059)
Post	−0.064***	−0.001
	(0.000)	(0.919)
Constant	0.024***	0.020***
	(0.000)	(0.000)
Observations	47,812	44,970
Joint significance	106.51	9.56
	(0.000)	(0.008)

The dependent variable is the logarithm of the wage after 12 and 24 months. *p*-Values are reported in parenthesis. *10% significance level, **5% significance level, ***1% significance level.

Appendix D. The search and matching model

D.1 The set-up

This section discusses how the flexibilization of temporary contracts might affect key economic outcomes, and in particular the conversion to permanent contracts. We use the search and matching model developed by Blanchard and Landier (2001), and as in their framework we think of flexibilization as a reduction of employment protection. We focus first on the effect of a reduction in the hiring costs and later on the impact of a reduction in the lay-off costs.

Firms. Firms are value maximizers. Each firm creates a temporary position at a fixed hiring cost k (Pissarides, 2009), and then operates it forever. The firm fills the position instantaneously, by hiring a worker from the pool of unemployed. New matches start producing output with productivity level y_0. Over time, matches sealed with a temporary contract are subject to idiosyncratic productivity shocks with arrival rate $\lambda > 0$. Conditional on λ striking, the value of the match is drawn from the distribution $F(y)$. When the productivity level changes from y_0 to y, the firm can decide either to fire the worker or keep her permanently. The productivity level y remains constant until the worker retires. Firing costs c are to be paid by the firms in case of lay-offs of permanent workers. Separations due to retirement are not subject to firing costs.[12]

Workers. Workers are risk neutral and discount the future at interest rate r. The mass of workers is normalized to unity. There is a constant flow of entrants equal to s, and each individual retires with instantaneous probability s, so the flow of retirees is equal to the flow of entrants. New workers enter the labour market unemployed and start looking for a temporary job, which they find with probability x, where $x = h/u$, with h being the flow of hires and u being the number of unemployed. Their temporary job is hit by a productivity shock and workers can be upgraded to a permanent job at rate λ, if the withdrawn productivity level is high enough. Alternatively, if the productivity level is too low, they join the unemployment pool.

Wages. The instant a vacancy and a worker make contact, they bargain over the division of the surplus. We assume that match specific wages and profits are the outcome of a Nash bargaining between the parties with workers' bargaining share equal to $\beta > 0$. Wage contracts are renegotiated each time new information about the match is revealed (the productivity shock hits the match).

D.1.1 The Bellman's equation

Let W^T be the expected lifetime income of an employee on a temporary job and let W^P be the expected lifetime income of an employee with productivity equal to y on a permanent job. Let y^* be the threshold level of productivity above which the worker is converted to a permanent contract, and below which she joins the unemployment pool. Given the above assumptions, the expected discounted lifetime income when an individual is unemployed, W^U, can be expressed as the solution to the following Bellman's equation:

$$rW^U = x[W^T(y_0) - W^U] - sW^U. \tag{D1}$$

Similarly, the expected lifetime income of an employee on a temporary job solves:

$$rW^T(y_0) = w(y_0) + \lambda \int_{y^*}^{+\infty} [W^P(y) - W^T(y_0)], \, \mathrm{d}F(y) + \lambda F(y^*)[W^U - W^T(y_0)] - sW^U, \tag{D2}$$

where $w(y_0)$ is the wage earned in the entry-level job. The second term is the probability that the productivity shock hits the match and the new productivity level drawn from the $F(y)$ distribution is high enough for the worker to be upgraded to a permanent position. Alternatively, if the productivity level is too low, the worker joins the unemployment pool, as picked up by the third term. The last term is the probability that the worker retires.

Finally, the expected lifetime income of an employee on a permanent job solves:

$$rW^P(y) = w(y) - sW^P(y), \tag{D3}$$

where $w(y)$ is the wage earned in the permanent job, while the second term is the probability that the worker retires.

From the firm's perspective, when the firm hires a temporary worker (equation D4), the firm gets flow profit $y_0 - w^T(y_0)$ and when the match is hit by a productivity shock at rate λ, the firm decides whether to upgrade the worker to a permanent position or replace the worker with a new one. This decision depends on the new productivity level of the worker. When the firm hires a permanent

[12] The firms' Bellman's equations are reported in Appendix D.1.1.

worker (equation D5), the firm gets flow profit $y - w^P(y)$ and might lose the worker at rate s due to retirement.[13]

$$rJ^T(y_0) = y_0 - w(y_0) + \lambda \int_{y^*}^{+\infty} [J^P(y) - J^T(y_0)],\ \mathrm{d}F(y), \tag{D4}$$

$$rJ^P(y) = y - w(y) + s[J^T(y_0) - J^P(y)]. \tag{D5}$$

D.2 The equilibrium conditions

The definition of equilibrium is standard. Competition among entrant firms will bid up the rental price of a match until it equals exactly the flow expected present value of holding a match, bringing the value of a vacancy to zero. Upon meeting, a firm and a worker will agree to create a new match as long as its value is strictly positive, given that being vacant has zero value. The model imposes four equilibrium conditions, which are reported below.

The first condition implies that the value of a new position is equal to the cost of creating it, i.e.

$$J^T(y_0) = k. \tag{D6}$$

The second condition is that, at the threshold level of productivity y^*, the firm is indifferent between keeping the worker, or let her go and hire a new worker, i.e.

$$J^P(y^*) = J^T(y_0). \tag{D7}$$

The third and fourth conditions are the Nash bargaining equilibrium conditions for temporary and permanent jobs:

$$W^T(y_0) - W^U = 0, \tag{D8}$$

$$W^P(y) - W^U = J^P(y) + c - J^T(y_0), \tag{D9}$$

where the left-hand side is the change in utility for the worker and the right-hand side is the change in utility for the firm. Note that in equation (D8), when a worker loses a temporary job, her utility changes from $W^T(y_0)$ to W^U; however, when the firm loses a temporary worker, the change in utility is zero as the firm can immediately replace the worker with a new one.

D.2.1 The lay-off condition and the hiring condition

In order to characterize the equilibrium, we derive the *lay-off condition* and the *hiring condition*, which identify a relationship between the value of being unemployed W^U and the productivity threshold y^*, at which the firm is indifferent whether to upgrade the worker to a permanent position or to open a new vacancy and hire a new worker on a temporary contract. We then evaluate how the two curves respond to a reduction in the hiring cost k and we assess the impact on the productivity threshold y^*.

The lay-off condition. The first step in the characterization of the equilibrium is the derivation of the lay-off condition, using equations (D6) and (D7):

$$\frac{y^* + sk}{r + s} - W^U - k = c. \tag{D10}$$

[13] Since regular jobs are not subject to productivity shocks the only role of the firing cost c is to affect wage bargaining in permanent jobs, but not lay-offs.

The lay-off condition highlights a clear relationship between y^* and W^U. The left-hand side gives the total gross surplus (i.e. ignoring firing costs) of a match of productivity y^*. Specifically, it is the expected value of output net of the outside options of workers and firms. Taking the derivatives of y^* with respect to W^U we get:

$$\frac{\mathrm{d}y^*}{\mathrm{d}W^U} = (r+s), \tag{D11}$$

The higher the value of being unemployed W^U, the higher the productivity of the marginal match.

The hiring condition. We can compute the threshold y^* by which the firm is indifferent whether to upgrade the temporary worker to a permanent position or whether let her go and hire a new worker. To compute the threshold, we use equation (D7):

$$y_0 + sk + \lambda \int_{y^*}^{+\infty} \left[\frac{y+sk}{r+s}\right], \mathrm{d}F(y) = [r+s+\lambda(1-F(y^*)](k+W^U). \tag{D12}$$

The left-hand side gives the discounted total gross surplus from creating a new job and hiring a worker, while the right-hand side captures the outside options. Taking the derivative of W^U with respect to y^*, we get

$$[r+s+\lambda(1-F(y^*))]\frac{\mathrm{d}W^U}{\mathrm{d}y^*} = \lambda f(y^*)\left[-\frac{y^*+sk}{r+s} + (k+W^U)\right], \tag{D13}$$

which has an ambiguous sign. However, at the equilibrium, i.e. at the intersection with the job lay-off condition (equation D10), the derivative is given by

$$[r+s+\lambda(1-F(y^*))]\frac{\mathrm{d}W^U}{\mathrm{d}y^*} = -\lambda f(y^*)c < 0, \tag{D14}$$

implying that an increase in the threshold productivity value leads to a decrease in the value of unemployment.

D.3 Effect of lower hiring costs

We are interested now in assessing the equilibrium impact of a reduction in the hiring cost. To achieve this, we take the derivatives of the hiring and lay-off conditions with respect to k. From the lay-off condition (equation D10), we get

$$\frac{\mathrm{d}y^*}{\mathrm{d}k} = r, \tag{D15}$$

i.e. the higher the hiring cost k, the higher is the productivity threshold. We then take the derivative of the hiring condition (equation D12) with respect to the hiring cost k to get

$$\frac{\mathrm{d}W^U}{\mathrm{d}k} = -\frac{r}{r+s} < 0. \tag{D16}$$

This relationship is clearly negative, implying that a reduction of the hiring cost would lead to an increase in the value of being unemployed.

Putting the two effects together, we observe that a decrease in the hiring cost k would move the hiring condition to the right, thus increasing the value of being unemployed and the threshold productivity y^*, due to the reduction in costs for the firm. However, lower hiring costs would also lead to a shift of the lay-off condition up, thus reducing the productivity threshold, as firms

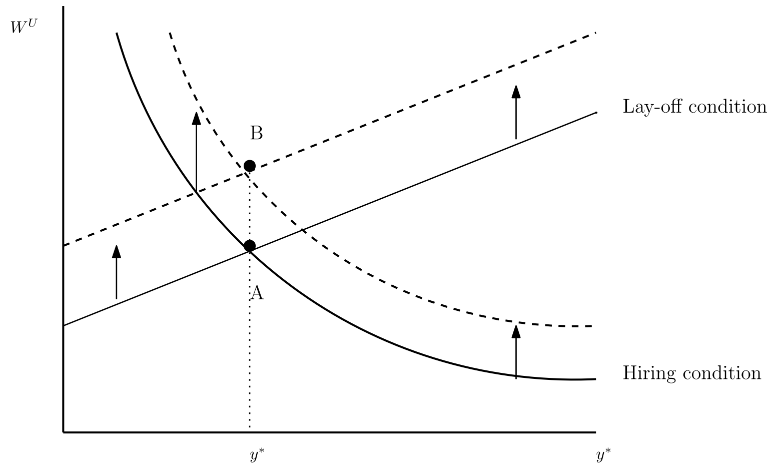

Figure D1. Effect of a decrease in hiring costs on the threshold productivity value.
Note: The dashed lines represent the curves after the reduction in the hiring cost.

increase lay-offs due to the lower cost of hiring new workers to replace existing ones. The new equilibrium is given by point B in Figure D1. The impact of a reduction in the hiring cost k on the value of being unemployed W^U is clearly positive; however, the effect on the productivity threshold y^* is overall ambiguous and would depend on which of the two effects prevail.

D.4 Effect of a subsequent lower firing costs

The scenario described in the previous section is meant to recreate the equilibrium after the implementation of a reform which reduced the hiring costs to k' for some temporary contracts, type B contracts (Point B in Figure D1), while leaving others unaffected, i.e. with the original hiring cost k, type A contracts (Point A in Figure D1). In this section, we are interested in investigating the impact for both types of contracts of a subsequent reduction in the firing costs c associated with permanent jobs. The slope of the hiring condition for the two types of temporary contracts is different due to the different hiring cost:

$$[r+s+\lambda(1-F(y^*))]\frac{\mathrm{d}W^U}{\mathrm{d}y^*}=\lambda f(y^*)\left[-\frac{y^*+sk}{r+s}+(k+W^U)\right],\quad \text{type A contracts}$$
$$[r+s+\lambda(1-F(y^*))]\frac{\mathrm{d}W^U}{\mathrm{d}y^*}=\lambda f(y^*)\left[-\frac{y^*+sk'}{r+s}+(k'+W^U)\right],\quad \text{type B contracts,}$$

where $k' < k$, thus implying a steeper hiring condition for type B temporary contracts. The slope of the lay-off condition, instead, does not depend on k.

We then take derivatives of the hiring and lay-off conditions with respect to the firing cost c. From the lay-off conditions we get:

$$\frac{\mathrm{d}y^*}{\mathrm{d}c}=r+s, \tag{D17}$$

which points to a movement of the firing curve upwards, in the presence of lower firing costs. The hiring condition instead does not include the firing cost.

The reform reducing firing costs would then cause the lay-off condition curve for both types of temporary contracts to move upwards (Point A' and B' in Figure D2). Due to the steeper slope of the hiring condition for type B temporary contracts and the fact that the hiring condition does not move in response to the change in firing costs, we might end up in a situation where $y^A < y^B$, i.e. the

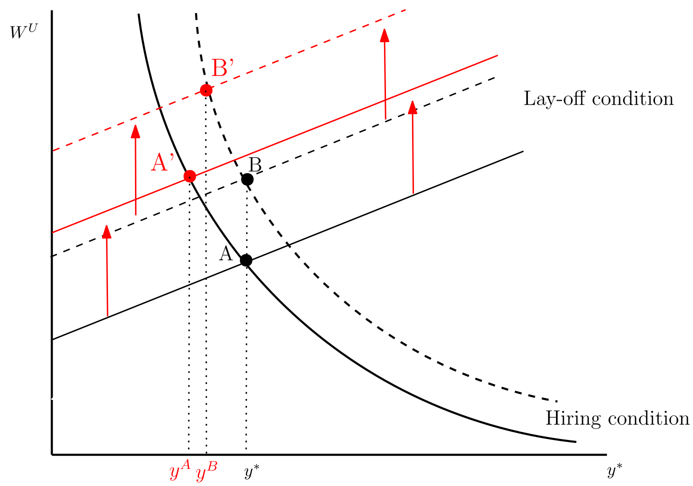

Figure D2. Effect of a decrease in firing costs on the threshold productivity value.

Note: Dashed lines refer to B-type contracts, associated with lower hiring costs; solid lines refer to A-type contracts associated with higher hiring costs. The arrows indicate the movements of the curves due to the reduction in firing costs.

productivity threshold above which temporary contracts are converted into permanent contracts is lower for the original contracts with higher hiring costs.

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