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ABSTRACT

Heterogeneous Paths to Stability*

We investigate how the flexibility of temporary contracts affects the probability of young workers to be upgraded into permanent employment. Theoretically, we explore the workers' career development in response to the change in flexibility within a search and matching model; empirically, we exploit an Italian labour market reform which increased flexibility in a difference in differences framework. We find that new entrants in the labour market who have been affected by the reform experienced a decrease in the conversion rate of approximately 12.5 percentage points in the first months after the reform, and of 5.1 percentage points over a year, compared to unaffected peers. This effect is particularly strong among women and low-educated workers employed in low productive firms in the Center/South of Italy. Worryingly, the lower conversion rate leads to a 25% wage penalty even two years down the workers' career paths.

JEL Classification: J41, J63, J64

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

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1 Introduction

Young workers are facing very difficult times. In most Mediterranean countries 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. Compared to the unemployment rate of the over 25-year-old, across Europe the youth unemployment rate is more than twice as high (Eichhorst and Neder, 2014). The COVID-19 pandemic has also boosted the percentage of young NEET (not in employment, education or training) who left the labour market because of discouragement effects due to poor employment perspectives (Aina et al., 2021). In 2021, the number of young NEET in the UK 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'. The high youth unemployment rates and the NEET emergency, however, are only the tip of the iceberg: even when young people find a job after a long time, the quality of employment available is concerning, as temporary and other non-standard forms of employment are increasingly becoming the norm. Young people in Europe are over-represented in temporary work, with 42% aged 24 and under hired on a temporary basis compared to just 10% of workers aged 25–64. While temporary contracts can be a stepping stone in the transition from education into work (Booth et al., 2002), they can also trap young people in insecure jobs or knock them into a worse career path (Casquel and Cunyat, 2008; Daruich et al., 2020), depending on the characteristics of the job. To improve such situation, it is therefore paramount to ensure that temporary contracts are regulated to facilitate a smooth transition to employment stability.

In this paper, we aim to understand how the flexibility of temporary contracts affects the labour market careers of young individuals who step for the first time in the labour market. Specifically, we investigate whether starting the career with a more or less flexible temporary contract may affect the career progression of young people. Theoretically, using a simple search and matching framework (Blanchard and Landier, 2001), we show that the increased flexibility of temporary contracts has an ambiguous impact on the probability of conversion

into permanent contracts. In our framework, workers start their career with a temporary job and when firms find out about the quality of the matches, they decide whether to upgrade the workers to a permanent job. We model the increased flexibility of temporary contracts as the reduction of the hiring costs and we show analytically that when hiring costs are lower, two main effects are at play: firms are more inclined to hire new workers on temporary contracts to find out how they perform, but they are also more reluctant to upgrade them into regular jobs. Even when a match turns out to be quite productive, a firm may still prefer to take a chance with a new worker, due to the lower hiring costs. As the two mechanisms have an opposite effect on the threshold productivity level above which the workers get upgraded, the overall impact is undetermined.

We test empirically for the prevalence of one of the two effects using Social Security data on the population of Italian workers, taking advantage of a reform which reduced hiring costs associated with temporary contracts. We use Italy as a case study for many reasons. The Italian labour market is dual and has been heavily segmented in permanent and temporary workers since the 90s (Barbieri and 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 15-14) in temporary contracts has been also alarmingly high: in 2019 Italy ranked second in Europe at 63.3%, after Spain at 69% (OECD). Lastly, we can take advantage of a number of reforms implemented in the period 2014-2015 to identify the effect of the increased flexibility of temporary contracts on the probability of conversion and to study the impact of policies which introduced incentives for firms to transform temporary contracts into permanent contracts. Specifically, in March 2014 the *Poletti Decree* significantly increased the flexibility of temporary contracts by reducing the requirement to declare the reason for using temporary contracts instead of permanent ones. In January 2015 the *Budget Law* introduced a sizeable subsidy for firms hiring workers on open-ended contracts and upgrading workers from temporary contracts. Finally, in March 2015 the *Jobs Act* reform introduced a new type of open-ended contract with firing costs increasing with

tenure, significantly reducing the costs to be paid by firms in case of unfair dismissals (Boeri and Garibaldi, 2019).

We first document a lower conversion probability to an open-ended contract for workers at their first work experience hired on a more flexible temporary contract, compared to similar workers hired on a less flexible contract, in the pre-reforms period. Specifically, we identify control and treated individuals on the basis of their first employment contract starting immediately before or after the date of the implementation of the *Poletti Decree* (Eichhorst and Neder, 2014). These two groups should be very similar, as they both include first time entrants in the labour market, who only differ for the starting date of their contract being few months apart. Second, we show that when fiscal incentives to the transformation into permanent contracts are introduced, i.e., through the *Budget Law* in January 2015, the gap in the probability of conversion opens up between the two groups by approximatively 12.5 percentage points in the first three month of 2015. Finally, when lower firing costs for workers on permanent contracts are introduced, i.e., through the *Jobs Act* in March 2015, the probability of conversion decreases between the two groups, but the gap persists and remains stable around 5% until the end of 2015. This effect is shown to be particularly strong for specific groups of workers, such as women and lower educated workers, hired in less productive firms located in the Centre and South of Italy. We then test for the presence of persistent effects in the medium run; we estimate that the decrease in the conversion rate among young workers hired on a more flexible temporary contract leads to a 25% wage penalty, which persists up to two years down their career paths.

The paper is organized as follows. Section 2 summarizes the literature on the topic. Section 3 describes the institutional background and the details of the reforms, while Section 4 illustrates the theoretical framework. Section 5 describes the empirical analysis, including the identification strategy, while Section 6 describes the data and Section 7 discusses the results. Finally, Section 8 concludes the paper.

2 Literature

This paper fits in the literature on labour market segmentation in dual economies, which is abundant (Bentolila et al., 2012; Berton, 2008; Blanchard and Landier, 2001; Booth et al., 2002; Cahuc and 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 US, these jobs are shown to be used as screening devices to more stable jobs (Booth et al., 2002; Holmlund and Storrie, 2002), while in others evidence shows that they have become a source of excessive labour market volatility (Boeri and Garibaldi, 2009; de Graaf-Zijl et al., 2011; Dolado et al., 2013; Tealdi, 2019). The effect of temporary contract duration on the probability of finding a permanent job has been found to be non-linear (Gagliarducci, 2005) and slightly increasing with tenure at the temporary contract, but mainly concentrated at specific durations, i.e., 6, 12, 24 and 36 months (García-Pérez and Muñoz-Bullón, 2011; Güell and Petrongolo, 2007); however, it might be negative for specific categories of workers, such as men and women with children (D'Addio and Rosholm, 2005).

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., 2019; Bovini et al., 2018; Sestito and 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.

Finally, this paper relates to the literature studying how the workers' experience in the

first few years of their careers affects their performance later in life. For the case of Sweden, Nordstrom Skans (2011) provide evidence that the first labor market experience might have an impact on labour market performance up to five years later; Eliason and Storrie (2006) show that displaced workers suffer both earnings losses and worsened labor 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 and Picchio, 2013; Schmillen and Umkehrer, 2017). Scarring effects of temporary contracts have also been found by Mooi-Reci and Dekker (2015) and Gorjón et al. (2021). 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 labor market just after the 1984 liberalization of temporary contracts experienced worse labor market outcomes than cohorts that just preceded them. Specifically, they spent 200 days at work (i.e., a 7% drop) less compared to the control group, whereas their wages dropped by about 22% in the long run. One of the reasons the authors identified is in the higher likelihood of young workers to work under temporary contracts well after their entrance in the labour market.

3 Institutional background

The dualism in the Italian labour market arose at the end of the nineties when the government progressively introduced different types of fixed-term contracts to increase flexibility in the use of labour, without changing the features of the permanent contracts. During the 2000s the share of fixed-term workers increased rapidly to approximately 13 per cent. More than 60 per cent of new hires were fixed-term job contracts, used not only to face labour demand uncertainty, but also as a cheap screening device before hiring workers under a permanent contract (Booth et al., 2002).

Two major labour reforms have been implemented in the last few years in Italy: in 2014 the so-called *Decreto Poletti* and in 2015 the so-called *Jobs Act*. The former further liberalized the utilization of temporary contracts by removing for all temporary contracts, independently on their length, the obligation for employers to declare the precise reason why they would hire a worker with a temporary contract rather than with a permanent one. Even though this might seem like a marginal change, it is quite relevant, as if not reported correctly an employee can sue the employer and eventually obtain from the labour court the conversion of the temporary contract into a permanent one. Moreover, the reform increased the possibility of extension of the contract duration from one to five times, within the maximum duration of three 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 only 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 (Table 7 in Appendix).

The *Jobs Act* approved in March 2015 changed the permanent contract significantly. The new labour 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 two years, and then increase with tenure up to a maximum of 24 months 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 two years of tenure and then an additional monthly wage per year of service, with a maximum amount of 18 monthly after 18 years of service. The acceptance of this transaction prevents any further dispute by the worker, that is, appealing to courts for a dismissal to be unfair or not. 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 applied to all new hires on an open ended basis,

and do not involve workers continuing on permanent contracts in firms with more than 15 employees, who continued to be protected by the reinstatement clause.

In addition, the 2015 *Budget Law* (effective in January 2015) introduced a sizable 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 and Viviano, 2018). Conversions from fixed-term 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 and Garibaldi, 2019).

4 The Model

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. However, while they study the impact of a reduction in the layoff costs, we focus on the effect of a reduction in the hiring 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 layoffs of permanent workers. Separations due to retirement are not subject to firing costs.

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. 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).

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:

$$rW^U = x\left[W^T(y_0) - W^U\right] - sW^U. \tag{1}$$

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

$$rW^T(y_0) = w(y_0) + \lambda \int_{y^*}^{+\infty} \left[W^P(y) - W^T(y_0)\right], dF(y) + \lambda F(y^*) \left[W^U - W^T(y_0)\right] - sW^U, \quad (2)$$

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:

$$rW^P(y) = w(y) - sW^P(y), \quad (3)$$

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 4), 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 worker (Equation 5), the firm gets flow profit $y - w^P(y)$ and might lose the worker at rate s due to retirement.[1]

$$\begin{aligned} rJ^T(y_0) &= y_0 - w(y_0) + \lambda \int_{y^*}^{+\infty} \left[J^P(y) - J^T(y_0)\right], dF(y), \quad (4) \\ rJ^P(y) &= y - w(y) + s\left[J^T(y_0) - J^P(y)\right]. \quad (5) \end{aligned}$$

[1]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 layoffs.

4.1 Equilibrium

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. As explained above, wages are the outcome of Nash bargaining. The model imposes four equilibrium conditions.

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

$$J^T(y_0) = k. \tag{6}$$

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.,

$$J^P(y^*) = J^T(y_0). \tag{7}$$

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

$$W^T(y_0) - W^U = 0, \tag{8}$$

$$W^P(y) - W^U = J^P(y) + c - J^T(y_0), \tag{9}$$

where the left end side is the change in utility for the worker and the right hand side the change in utility for the firm. Note that in Equation (8), 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.

The layoff condition. The first step in the characterization of the equilibrium is the derivation of the layoff condition, using Equation (6) and Equation (7):

$$\frac{y^* + sk}{r+s} - W^U - k = c. \tag{10}$$

The layoff 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 and k we get:

$$\frac{dy^*}{dW^U} = (r+s), \tag{11}$$

$$\frac{dy^*}{dk} = r. \tag{12}$$

The higher the value of being unemployed W^U, the higher the productivity of the marginal match. The higher the hiring cost for permanent jobs, k, the higher the productivity threshold.

The hiring condition. We can compute the threshold $\hat{y}$ 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 (7):

$$y_0 + sk + \lambda \int_{y^*}^{+\infty} \left[\frac{y + sk}{r+s}\right], dF(y) = \left[r + s + \lambda(1 - F(y^*)\right](k + W^U). \tag{13}$$

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:

$$\left[r + s + \lambda(1 - F(y^*))\right] \frac{dW^U}{dy^*} = \lambda f(y^*) \left[-\frac{y^* + sk}{r+s} + (k + W^U)\right], \tag{14}$$

which has an ambiguous sign. However, at the equilibrium, i.e., at the intersection with the job layoff condition (Equation 10), the derivative is given by:

$$[r + s + \lambda(1 - F(y^*))] \frac{dW^U}{dy^*} = -\lambda f(y^*)c < 0, \tag{15}$$

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

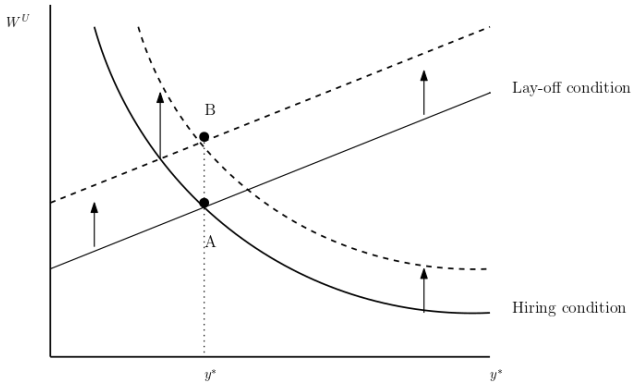
We then take the derivative of W^U with respect to the hiring cost k to get:

$$\frac{dW^U}{dk} = -\frac{r}{r+s} < 0. \tag{16}$$

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

Combining all the results together, we find 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 increase layoffs due to the lower cost of hiring new workers to replace existing ones. The new equilibrium is given by point B in Figure 1. The impact of a reduction in k on y^* is therefore overall ambiguous and would depend on which of the two effects prevail. In our empirical analysis we will quantify the impact of the reduction in hiring costs on the threshold productivity level and specifically on the probability of conversion into a permanent contract taking advantage of a quasi-experimental setting.

Figure 1. Effect of a decrease in hiring costs on the threshold productivity value.



5 Empirical strategy

Until the approval of the *Poletti Decree* in 2014, any firm which wanted to hire a worker on a temporary contract was required to declare the reason behind this choice. Moreover, the temporary contract could be extended only once within the maximum length of 36 months. The *Poletti Decree* was first introduced as a Legislative Decree on March 21, 2014. This first decree introduced a maximum of 8 extensions within the same maximum length of 36 months. Two months later, on May 19, 2014 the *Poletti Law* was approved and the number of extensions within the same maximum duration of 36 months was reduced to 5. While the normative change in the number of possible extensions had minimal take-up (Section 3), the removal of the obligation to report the reason for hiring on a temporary basis can potentially affect the careers of new entrants in the labour market, even if the sign of this theoretical prediction is ambiguous ex ante (Section 4). We identify a period of time around the date of March 21 (date of the publication of the Poletti Decree) to setup our natural experimental setting.[2] Specifically, we consider workers hired for the first time on a temporary contract between January 01, 2014 and March 21, 2014 (with a termination date at least after December 2014) as control group. Workers hired for the first time on a temporary contract between March 21, 2014 and May 31, 2014 (with a termination date at least after December 2014) represent, instead, our treated group. These two groups should be very

[2]Technical details about the timing of the policy implementation are reported in Appendix A.2.

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 months apart.[3] With this choice, we implicitly assume that starting a temporary job in, say February 2014, or April 2014 for a new entrants it is a matter of chance and does not depend on individual characteristics. We corroborate our assumption in Section 6 where we show that the observable characteristics of workers in the two groups are very similar. We follow the new entrants until the end of 2015. This allows us to observe 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 favoring the upgrading 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 the workers in our analytical sample.

Let Y_{it} be a dummy indicator for each worker i in our sample which takes value one if the worker's contract is upgraded to permanent in each month t between September 2014 and December 2015. We estimate the following generalized difference in differences regression through OLS:

$$Y_{it} = \alpha + \sum_{t=1}^{T} \sigma_t * \text{Month}_t + \sum_{t=1}^{T} \gamma_t * (\text{Month}_t \times \text{Treated}_i) + \eta_i + \epsilon_{it}, \tag{17}$$

where $Treated_i$ is a dummy variable which takes value one if the worker is in the treated

[3] 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 ending up in the control group. We believe this to be a minor issue for several reasons: first, as we show in Table 1, observable characteristics are very similar between the two groups; second, individual fixed effects capture all 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 attributable to chance rather than selection. Finally our identification strategy relies on the fulfillment of the parallel trend assumption (Figure 2).

group and zero otherwise. $Month_t$, $\forall t = 1, .., T$, are a set of dummies identifying months starting from September 2014 to December 2015. The coefficients $\{\gamma_1, \gamma_2, ..., \gamma_T\}$ of the interactions between the $Treated_i$ and the $Month_t$ variables identify dynamic 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. Errors are clustered at individual level. To ease the reading of our results, we report in Tables 2-5 the estimates obtained by pooling together interactions and time dummies. $Post1$ identifies the period January-March 2015, immediately after the introduction of financial incentives to hire workers on a permanent contract, through the strong reduction in labour costs. $Post2$ identifies the period April-December 2015, after the *Jobs Act* reform which significantly reduced firing costs for workers hired on permanent contracts. All other variables are defined as in Equation (17).

5.1 Longer-term effect on wages

In the second part of our empirical analysis we are interested in estimating the impact of the potentially different conversion rates on future wages (12 and 24 months later). We use the treatment status $Treated_i$ as an exogenous predictor for Y_{it}, the indicator dummy for conversion in a fuzzy difference in differences setup (De Chaisemartin and d'Haultfoeuille, 2018), where $Wage_{it+12}$ and $Wage_{it+24}$ are the outcome variables of interest. The fuzzy design is suitable for our purpose because the treated and control groups, that are identified as G or groups in the De Chaisemartin and d'Haultfoeuille (2018) setup, differ in the average probability of conversion and the treatment group only experiences a higher increase in its mean treatment. We perform the estimation in two stages,[4] where the first stage is the generalized difference in differences (Equation 17). In the second stage (Equation 18), we regress future wages on the predicted probability of conversion in a second difference in

[4]The specification used in the analysis is slightly different from the one proposed by De Chaisemartin and d'Haultfoeuille (2018): it is more traditional but more tractable considering that we are using a panel data estimation.

differences model, where we correct the standard errors according to the two-stage procedure:

$$Wage_{it+\tau} = \alpha + \delta * \text{Post}_t + \beta * \text{Post}_t \times \hat{Y}_i + \eta_i + \epsilon_{it}, \quad (18)$$

where $\tau = \{12, 24\}$. 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 the two groups. 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 conversion on future wages. Within this setup, compliers are all those individuals who have experienced a lower conversion probability due to the fact that they have been hired on their first job in the post-reform period. 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).

6 Data and descriptive statistics

To implement the empirical strategy described above, we use four data sources: (i) the administrative microdata from the *Comunicazioni Obbligatorie*, (ii) the working histories of the Italian population available at the Italian Social Security Institute, (iii) the employer records available at the Italian Social Security Institute and (iv) the firm database CERVED.

In Italy employers are required to electronically file all occurrences concerning a job position to the regional agencies in charge of active labour market policies. Microdata archives, which cover only employees in the private sector and part of the public sector, are collected and organized by each Italian region. The database *Comunicazioni Obbligatorie* collects information on the opening, termination, and extension or conversion of contracts. Our dataset contains information regarding all events (hiring, firing, conversion and fixed-term contract extended duration) that occurred in Italy between January 2014 and December 2015. For each event recorded in our dataset it is possible to identify both the firm and the worker

involved. On top of the relevant anonymized identifiers (firm and worker) and the type of event, we observe some individual characteristics, such as the worker's gender, birth-date, education level and nationality.

The *Comunicazioni Obbligatorie* data are matched at firm level with the CERVED database, which contains the full financial record of over 800,000 incorporated Italian companies. The data include information about the firm size, the sector, the workforce composition, and some indicators of performance (sales, revenues, liquidity, value added, etc.).

The data are then merged at firm level with the record of all firms registered at the Italian Social Security Institute with the aim of getting information about average wages and some additional firm characteristics related to the composition of the workforce. Finally, the data are also merged at individual level with the record of all individuals registered at the Italian Social Security Institute to collect the working histories of all individuals in our sample.

In Table 1, we report descriptive statistics for the sample of selected workers. The sample comprises individuals who enter for the first time in their lives in the labour market and are hired on a temporary contract, which last at least until December 2014.

Our sample includes 2,467 workers, of which 1,189 in the control group and 1,278 in the treated group. The close similarity of individual characteristics between the two groups supports our assumption that the only relevant difference between the two groups lies in the start date of the contract. The average age is approximately 27 years old, the sample is split evenly among men and women, and in both samples approximately one third of workers holds a primary level, one third holds a secondary level and one third holds a tertiary level. Finally, approximately 50% of workers in both groups are less than 25 years old. The average size of the firms in which treated and control workers are employed is approximately 950 employees (the size distribution of treated and control firms is very skewed and averages are not very informative; the median of the overall sample is 52 employees) and the average firm age is 15 years. On average, 37% of employees work part-time and 37% are hired on a temporary contract; their average annual wage is 20.000 €. The firms in which treated and control

Table 1. Descriptive statistics for workers in the treated and control groups.

	Total	**Control**	**Treated**
Workers' characteristics			
Age	27.87	28.05	27.70
	(8.15)	(8.34)	(7.97)
Under 25	0.494	0.498	0.489
	(0.50)	(0.50)	(0.50)
Female	0.489	0.499	0.478
	(0.50)	(0.50)	(0.50)
Primary Education	0.334	0.354	0.312
	(0.47)	(0.46)	(0.48)
Secondary Education	0.313	0.290	0.331
	(0.46)	(0.47)	(0.46)
Tertiary Education	0.350	0.350	0.357
	(0.47)	(0.46)	(0.48)
Firms' characteristics			
Firm size	951	955	946
	(2863)	(3184)	(2523)
Firm age	15.0	15.4	14.6
	(14.3)	(14.9)	(13.7)
Wage	20051	20301	19814
	(15065)	(15162)	(14975)
Share part-time	0.37	0.38	0.36
	(0.38)	(0.38)	(0.37)
Share temporary	0.37	0.37	0.38
	(0.30)	(0.30)	(0.29)
Fixed assets	135365	159712	111928
	(525777)	(644308)	(377072)
Value of production	331401	383529	281219
	(1258162)	(1059948)	(1434277)
Revenues	330167	381787	280475
	(1256903)	(1431141)	(1061141)
Value added	62236	68994	55729
	(187987)	(204605)	(170351)
# Observations	2,467	1,189	1,278

Note: Standard deviations in parenthesis.

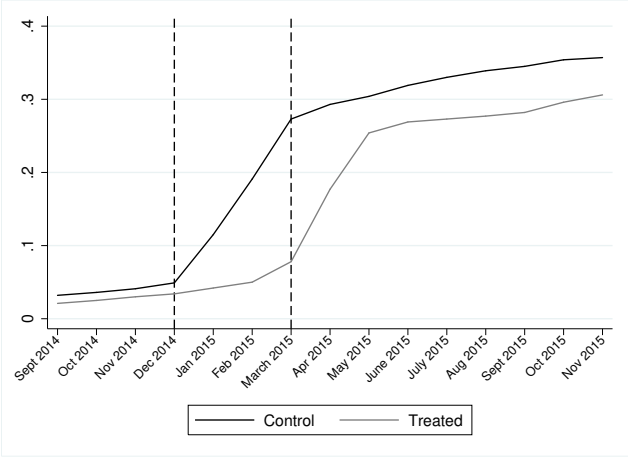
workers are hired show also comparable financial structures and measures of productivity, as shown by their similar fixed and liquid assets, production values, revenues and value added.

7 Results

7.1 Effects on conversion probability

Figure 2 reports the average probability of transformation of workers hired on a temporary contract for the first time right before (control) and right after (treated) the flexibilization of the temporary contract, in each month of the period considered. Interestingly, in the last four months of 2014, before any incentive for the conversion of temporary contracts into

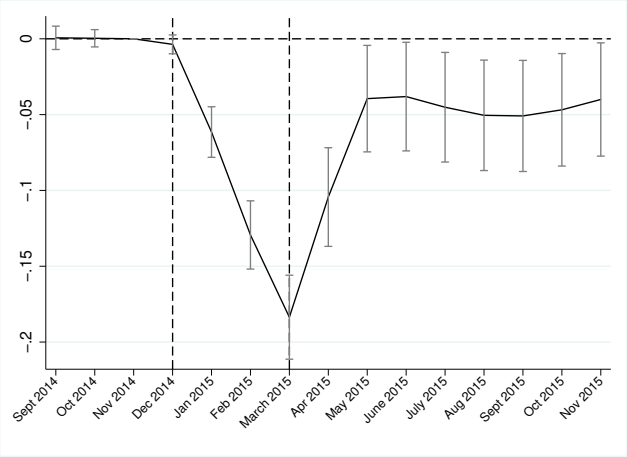
Figure 2. Average conversion probability in treated and control groups.



permanent was in place, the probability for an individual to move to a permanent position was approximately 1% higher among workers in the control group. Importantly, the gap is stable over time, providing evidence of parallel trends between treated and controls in the pre-treatment period. When the *Budget Law* is implemented in January 2015, which significantly reduced the labour cost for firms which upgrade temporary employees into permanent positions, the average probability of being upgraded 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, while only a slight increase among workers in the control group. Although, the gap in the average probability of conversion between the two groups shrinks, this probability remains persistently lower by approximately 5 percentage points among treated individuals until the end of 2015.

We plot in Figure 3 the coefficients of the interaction of the treated variable and the time variable, obtained by estimating Equation (17). We observe the absence of any pre-trend, in the months between September and December 2014, supporting the validity of our approach. In the period from January to March 2015, after the implementation of the Budget Law, the probability of conversion declines significantly among treated individuals (compared to individuals in the control group), reaching the lowest point in March 2015,

Figure 3. Dynamic Treatment Effect



Note: The coefficients of the interaction of the treated variable and the time are reported, together with the 95% confidence intervals.

with a decline of 18.3 percentage points. After the Jobs Act (March 2015), the probability of conversion increases among treated workers, but remains significantly and persistently lower by 5 percentage points until the end of the observation period. We quantify the average outcome 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), as estimated from Equation (17). We find a negative and statistically significant effect in both periods after the reforms (Column 1 of Table 2). Specifically, the probability of conversion is 12.4 percentage points lower among treated individuals in the January-March period period and 5.1 percentage points lower in the April-December period. We then split the sample according to individual characteristics, such as gender (Columns 2 and 3), age (Columns 4 and 5) and education level (Columns 6, 7 and 8).

We find that for both males and females the effect is negative and significant in the January-March period and the magnitude of the effect is comparable across the two categories of workers. The probability of conversion is 12.6 percentage points lower among females and and 12.3 percentage points lower among male workers hired on a more flexible temporary contract. However, in the April-December period the effect is negative and significantly different from zero only among female workers, for whom the probability of upgrading is still 7.8 percentage points lower, compared to workers in the control group. For males,

Table 2. Estimation results by individual characteristics.

	Total	Females	Males	Under 25	Over 25	Primary	Secondary	Tertiary
Treated x Post1	-0.124***	-0.126***	-0.123***	-0.117***	-0.131***	-0.078***	-0.143***	-0.146***
	(0.008)	(0.014)	(0.013)	(0.019)	(0.014)	(0.015)	(0.017)	(0.018)
Treated x Post2	-0.051***	-0.078***	-0.026	-0.376	-0.065***	-0.046*	-0.086***	-0.014
	(0.018)	(0.025)	(0.024)	(0.025)	(0.024)	(0.027)	(0.032)	(0.030)
Constant	0.033***	0.031***	0.034***	0.034***	0.033***	0.034***	0.029***	0.035***
	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)

Note: The dependent variable is the individual probability of being upgraded to a permanent contract.

instead, the gap between treated and control workers disappears after the first three months. This means that for women hired on a very flexible temporary contract (treated group) the probability to be converted into a permanent contract is much lower compared to women hired on a less flexible temporary contract (control) and compared to men hired on similar flexible contracts, even twelve months after the hiring date. When considering two age categories, below and above 25 years old, we estimate a negative and significant effect for both groups in the January-March period, slightly larger among the over 25 (13.1 percentage points versus 11.7 percentage points). In the April-December period, instead, the effect is only significantly lower among the over 25, for whom the probability of conversion remains 6.5 percentage points lower compared to the control group. We then split the sample by education levels. The effect is negative and significant across all three individual categories in the January-March period, but the size of the effect is much smaller among primary educated individuals compared to secondary and tertiary educated workers (7.8 percentage points against 14.3 percentage points and 14.6 percentage points, respectively). However, in the April-December period the effect disappears for tertiary educated workers, while it persists for primary and secondary educated workers, who experience a 4.6 percentage points and 8.6 percentage points lower probability of conversion, respectively, compared to workers of the same age category in the control group.

7.1.1 Heterogeneous effects

We estimate Equation 17 by considering different types of firms, in which treated and control workers are hired. Per each variable considered we split the sample of firms with values above

Table 3. Estimation results by firm characteristics.

	Size		Age		Part-time share		Temporary share	
	Low	High	Low	High	Low	High	Low	High
Treated x Post1	-0.115***	-0.134***	-0.107***	-0.147***	-0.144***	-0.106***	-0.147***	-0.101***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated x Post2	-0.047*	-0.046*	-0.030	-0.047*	-0.050**	-0.048**	-0.025	-0.067***
	(0.066)	(0.063)	(0.246)	(0.074)	(0.025)	(0.048)	(0.332)	(0.006)
Constant	0.041***	0.031***	0.046***	0.025***	0.033***	0.043***	0.023***	0.045***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.004)	(0.000)

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.

and below the median. While we find no significant differences by size (Table 3, columns 1-2), we estimate the effect in older firms to be larger in the first period and persistent in the second period; in younger firms, instead the effect, which is smaller in period one, is not persistent in period two (Table 3, columns 3-4). Hence, workers hired in younger firms although are less likely to be upgraded into a permanent contract compared to workers in the control group in the first period, have the same chances in the second period, while the probability remains persistently lower in older firms. Workers in firms with low shares of both part-time workers as well as temporary workers have lower chances to be upgraded to a permanent contract compared to firms with high shares of both in period one (Table 3, columns 5-8), but while the effect is negative and persistent in period two in firms with high shares of temporary workers, it disappears in firms with low shares of temporary employees.

Workers hired in firms paying lower average wages are less likely to be upgraded into a permanent job in period one (Table 4, columns 1-2), but this effect persists in period two, while it disappears in firms paying higher salaries. The effect is also persistent in period two in firms located in the Centre and South of Italy, while no significant effect appears in period two in firms in the North (Table 4, columns 3-5). Also across firms operating in the most popular sectors, such as commerce, construction and manufacturing, which together cover almost 45% of the firms in our sample, with the manufacturing sector being the largest and covering 22% of firms, the effect is significant in the first period, but it is not persistent in the second period (Table 4, columns 6-8).

We then classify firms according to their fixed assets and some measures of productivity.

We find that workers hired in both firms with high and low fixed assets experience a lower probability to be upgraded to a permanent contract in the first period, while no significant effect afterwards (Table 5, columns 1-2). However, when we consider different measures of productivity, such as the value of production, total revenues and value added, we find that, consistent across the three indicators, in less productive firms the probability to be upgraded to a permanent contract remained persistently lower also in period two, while this is not the case for more productive firms (Table 5, columns 3-8).

Table 4. Estimation results by firm characteristics.

	Wage		Geography			Sector		
	Low	High	North	Centre	South	Commerce	Construction	Manufacturing
Treated x Post1	-0.108***	-0.141***	-0.131***	-0.120***	-0.107***	-0.119***	-0.122*	-0.133***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.075)	(0.000)
Treated x Post2	-0.086***	-0.073	-0.035	-0.074*	-0.072*	-0.004	-0.033	-0.006
	(0.000)	(0.781)	(0.127)	(0.053)	(0.074)	(0.919)	(0.785)	(0.867)
Constant	0.041***	0.200**	0.030***	0.030***	0.052***	0.048***	0.029***	0.021***
	(0.000)	(0.013)	(0.000)	(0.010)	(0.000)	(0.003)	(0.443)	(0.079)

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.

Table 5. Estimation results by capital level (fixed assets) and productivity level.

	Fixed assets		Value of production		Revenues		Value added	
	Low	High	Low	High	Low	High	Low	High
Treated x Post1	-0.108***	-0.137***	-0.115***	-0.130***	-0.115***	-0.131***	-0.115***	-0.131***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated x Post2	-0.052	0.013	-0.064*	-0.026	-0.064*	-0.027	-0.056*	0.019
	(0.129)	(0.691)	(0.054)	(0.424)	(0.054)	(0.421)	(0.091)	(0.559)
Constant	0.050***	0.150	0.047***	0.019*	0.047***	0.019*	0.049***	0.016
	(0.000)	(0.141)	(0.000)	(0.063)	(0.000)	(0.063)	(0.000)	(0.105)

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.

Overall, we are able to define profiles of workers who are more negatively affected by the increased flexibility of temporary contracts and to identify the profile of firms which are more likely to reduce the conversion whenever contracts are more flexible. In terms of workers, we find that the females, over 25 years old with a low level of education are the ones who are more negatively affected: they are the categories who experience persistently lower probabilities to be upgraded into a permanent contract even after 18 months from the beginning of their contract. In terms of firms, those workers who are more penalised by the increased flexibility of temporary contracts are hired mainly in older, less productive firms,

with a relatively high share of temporary employees, paying lower average wages, located in the Centre or South of Italy.

7.2 The scarring effect on the first stage of the workers' career

We report in Table 6 the results of the estimation on the logarithm of wages after 12 and 24 months, using a fuzzy difference in differences approach, as described in Section 5.[5]

Table 6. Wage estimation results.

	Log wage after 12 months		Log wage after 24 months	
	First stage	Second stage	First stage	Second stage
	-0.0703^{***}	-0.3077^{***}	-0.0673^{***}	-0.2671^{***}
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	28,545	28,545	26,412	26,412
KP test		70.82		434.33

Note: The dependent variable is the logarithm of wage after 12 and 24 months.

We find a significant negative effect on wages both at 12 months and at 24 months (Columns 2 and 4 in Table 6). Specifically, experiencing a lower conversion probability leads to 30% and 26% lower wages after 12 and 24 months, respectively.

This could be the result of 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 than workers on permanent contracts as firms tend to invest less in training for temporary employees compared to permanent employees. For instance, 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 employer-sponsored training by 14% (OECD, 2014). This reduced training, which is justified by the lower returns from the training investment made by the firms, might in turn translate into lower productivity and lower wages. Moreover, workers who are upgraded to a permanent contract might also be able to change employer in search for a better match. This would lead to workers sorting into potentially higher paying firms,

[5]The main assumptions of the Fuzzy DiD are fulfilled in our natural experimental setting; in particular, in our experiment within each group units switch treatment in only one direction between the pre and post period. Moreover, as shown in the first part of the analysis the treated group experiences a larger increase of the treatment rate and we provide evidence of a parallel trend.

thus explaining 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 affecting 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 and Rosholm, 2005).

8 Conclusions

In this paper, we aim to understand the way the flexibility of temporary contracts affects the labour market careers of young individuals who step for the first time in the labour market. Specifically, we investigate whether starting the career with a more or less flexible temporary contract may affect the career progression of young people. 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, even when incentives to the transformation are introduced. These lower conversion rates translate into significantly lower wages up to two years down their career paths. 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.

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A Appendix

A.1 Share of temporary contracts by number of extensions

Table 7. Shares of temporary contracts by number of extensions.

	# of extensions (% contracts)					
Year	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.

A.2 The Poletti reform: technical details

Until the approval of the *Poletti Decree* in 2014, any firm which wanted to hire a worker on a temporary contract was required to declare the reason behind this choice. Moreover, the temporary contract could be extended only once within the maximum length of 36 months. The *Poletti Decree* was first introduced as a Legislative Decree on March 21, 2014. This first decree introduced a maximum number of extensions within the same maximum length of 36 months equal to eight. Two months later, on May 19, 2014 the *Poletti Law* was approved and the number of extensions within the same maximum duration of 36 months was reduced to five.

Due to these changes and the possibility that different temporary contracts could be overlapping across these time periods, a clarification note was released with details on which contracts could be extended and how many times (Figure 4). It turns out that contracts which started before March 21, 2014 and were not extended in the transitory period (21 March and 19 May) could be extended once, while those which started in the same time period but were extended once in the transitory period could not be extended anymore (Group 1). Contracts which started in the transitory period (21 March and 19 May) and were extended in the transitory period less than 5 times could be extended for a maximum of 5 times after May 19 (Group 2). Contracts which started in the transitory period (21 March and 19 May) and were extended in the transitory period at least 5 times could not be extended anymore after May 19 (Group 3). Contracts which started after the transitory period (after May 19) could be extended for a maximum of five times (Group 4).

Taking advantage of these complicated changes, we define as control those workers hired on temporary contracts which could not be extended at all (Group 1) or could be extended at most once after May 19 (Group 3). We instead define as treated those workers hired on temporary contracts which could be extended at most 5 times, as described in the *Poletti Law*, after May 19 (Groups 2 and 4). As it turns out that there are no workers in group 3, the date of March 21, 2014 becomes the date by which there is a clear split between workers

in the control and treated groups.

Figure 4. Poletti decree application guide.