Working Paper 2026-02

Dignity by Decree?
The Employment and Wage Effects of Restricting Fixed-Term Contracts

Marco G. Palladino
Matteo Sartori
Giuseppe Grasso

La Chaire de Sécurisation des Parcours Professionnels est gérée par la Fondation du Risque (Fondation de recherche reconnue d'utilité publique). Elle rassemble des chercheurs de Sciences Po et du Groupe des Écoles Nationales d'Économie et Statistique (GENES) qui comprend notamment l'Ecole Nationale de la Statistique et de l'Administration Economique (ENSAE) et le Centre de Recherche en Economie et Statistique (CREST). Les travaux réalisés dans le cadre de la Chaire « Sécurisation des Parcours Professionnels » sont sous l'égide de la Fondation du Risque.

INPS.0072.27/01/2026.0000178 E

Dignity by Decree?
The Employment and Wage Effects of Restricting Fixed-Term Contracts

Marco G. Palladino* Matteo Sartori† Giuseppe Grasso‡

January 7, 2026

Abstract

The *Dignity Decree* was a 2018 reform aimed at restricting the use of fixed-term contracts in the Italian labor market. We examine how this regulatory intervention affected firm-level employment dynamics and worker wages. We find negligible disemployment effects and a significant shift in contract composition: firms more exposed to the reform substantially reduced their use of fixed-term contracts, offsetting the decrease with an increase in permanent employment, primarily through the conversion of existing temporary jobs. We further document a sizeable decline in the post-conversion wage for workers transitioning from fixed-term to permanent contracts compared to pre-reform levels.

We thank Samuel Bentolila and Pierre Cahuc for their careful and detailed feedback. We are also grateful for helpful comments and suggestions to: Bruno Anastasia, Manuel Arellano, Giulia Bovini, Francesco Chiocchio, Giulia Giupponi, Kerstin Holzheu, Diego Puga, Steven Raphael, Michael Reich, Raffaele Saggio, Benjamin Schoefer, Konstantinos Tatsiramos, Eliana Viviano; seminar participants at Sciences Po, Cemfi, INSEAD, OECD, Institute of Economic Sciences Belgrade, Collegio Carlo Alberto, Lisbon Micro Group, Banca d'Italia, INPS, and UC Berkeley IRLE; presentation attendees and discussants at the 36th AIEL Annual Conference, at COMPIE 2022 and at EALE 2022. The realization of this paper was made possible thanks to sponsorships and donations in favor of the VisitINPS Scholars' program. Marco Palladino gratefully acknowledges funding by the *Chaire de recherche sur la sécurisation des parcours professionnels*. Giuseppe Grasso gratefully acknowledges funding by the *Fundação para a Ciência e a Tecnologia* (ref. 2022.01269.PTDC).
The views expressed in the article are those of the authors only and do not involve the responsibility of the Bank of Italy, the Bank of France or the Eurosystem.

*Banque de France
†Banca d'Italia
‡ICS-ULisboa

Introduction

Temporary jobs play an important role in the labor markets of several European countries, accounting for a large proportion of entries into employment (Cahuc, Charlot, and Malherbet 2016). However, they remain controversial among economists and policymakers. Proponents argue that fixed-term contracts provide considerable employment gains and serve as stepping stones to more stable jobs. Critics, on the other hand, contend that they generate precarious employment, leading to excessive worker turnover and dead-end jobs.

After the 2008 financial crisis and the recessions that ensued, concerns about job instability have prompted several countries to reintroduce restrictions on fixed-term contracts, marking a significant shift from previous policies. Few studies have examined how the labor market responds to such reforms through adjustments in labor demand, employment, occupational flows, and job skill requirements (Cahuc et al. 2023; Grasso and Tatsiramos 2023). Even less evidence exists on whether these changes are accompanied by adjustments in wages. This paper provides new insights into these issues by examining how firms changed the size, composition and remuneration of their workforce following a reform that restricted the use of fixed-term contracts in Italy in 2018.

We begin by describing the regulatory framework that governs the use of fixed-term contracts in Italy. Next, we introduce the *Dignity Decree*, a 2018 reform that imposed stricter legal constraints on the use of fixed-term contracts in Italy. Acting through different policy levers, involving both mandatory restrictions and increases in administrative and legal compliance costs, this reform aimed at discouraging prolonged use of temporary jobs as a replacement for permanent positions. Its introduction transformed Italy's policy approach to temporary employment, shifting its regulatory regime from the most flexible to the most rigid among large European economies.

For our empirical investigation, we rely on two rich administrative data sources that offer complementary strengths for examining the dynamics of the Italian labor market around the reform. First, we use a comprehensive matched employer-employee dataset derived from the mandatory communications collected by the Ministry of Labor and Social Policies (*Comunicazioni Obbligatorie*), where each record corresponds to one employment spell. Second, we leverage an extensive administrative dataset from the Italian Social Security Institute (*Rapporti di lavoro mensili Uniemens*), which provides monthly information on the contributory history of each job. Together, the two datasets

provide an exhaustive and detailed picture of the working histories of Italian employees. The first allows us to precisely measure the starting and ending date of each fixed-term contract, its (potential) conversion to a permanent position and — a unique feature for this kind of data — its initially specified duration ("*stated duration*"). The second dataset allows us to track workers' wages and observe firm and individual characteristics over their entire employment histories.

The empirical analysis presented in the paper is organized into two parts. In the first part, we study firm-level employment dynamics around the reform. First, we illustrate a marked shift in the distribution of fixed-term contracts' duration after July 2018, with a large share of contracts clustering just below the 12-month threshold at which regulatory costs became more severe. Then, we construct a firm-level exposure measure that combines reliance on fixed-term contracts before the reform with the share of long-duration fixed-term contracts that were subject to the strictest penalties of the reform. Next, we leverage it within an event-study design to analyze the dynamics of several employment outcomes for different degrees of exposure. We observe a significant change in workforce composition, with a one standard deviation increase in exposure inducing a replacement of temporary jobs for permanent positions amounting to 2.5 percentage points relative to the stock of total employment days. Furthermore, we find that this restructuring of employment relationships was largely driven by within-firm contract conversions, and was accompanied by minimal and only transitory employment losses.

The second part of the analysis examines wage changes experienced by workers who transition from a fixed-term to an open-ended contract within the same firm. We construct a dataset of conversion events, where each entry records a new permanent job deriving from a within-firm contract conversion, and use it to fit a regression model that compares the wage dynamics for similar workers before and after the reform. Our main finding is that contract conversions become less rewarding in the wake of the reform: after July 2018, we observe a significant decline in the starting wage of open-ended contracts that are generated from a conversion. We estimate this drop in the post-conversion starting wage to range between 5 and 2 percentage points, the latter estimate deriving from our richest model specification. Interestingly, this effect is not present for externally-hired permanent workers, whose starting wage remains unaffected after the reform.

Taken together, these results suggest the presence of a trade off between employment security and remuneration for workers. Following the reform, firms mostly keep their workforce size unchanged, shift their contract composition, but compensate for the

increased risk deriving from larger permanent employment with lower starting wages for employees whose contract has been converted.

This paper makes two main contributions to the vast literature on employment protection duality in two-tier labor markets (for an overview, see Bentolila, Dolado, and Jimeno 2020; Boeri 2011; and Boeri and Garibaldi 2024). First, we contribute to the research evaluating the effects of temporary jobs regulation on employment. This strand of the literature examines whether and how regulatory interventions can guarantee enough flexibility to promote employment growth while limiting worker exposure to precarious and unstable employment conditions. Daruich, Di Addario, and Saggio (2023) investigate the effects of a 2001 Italian reform that relaxed constraints on fixed-term contract hiring while maintaining protections for permanent contracts. They find that the reform did not increase overall employment but led to a substantial rise in the share of temporary jobs. Coherent with other papers on temporary employment liberalization (Di Porto and Tealdi 2024; Silva, Martins, and Lopes 2018; Martins 2021), they show that this reorganization occurred mainly through a lower share of conversions from fixed-term to open-ended positions. Focusing on a reform with an opposite regulatory stance, our analyses show that a post-intervention drop in fixed-term contract hiring was counterbalanced by a contemporaneous increase in permanent hiring. Our results thus confirm that regulatory changes mostly produce substitution between contract types with almost no effect on overall aggregate employment. This finding is consistent with Grasso and Tatsiramos (2023), who show that fewer temporary and more permanent positions were advertised in online job postings after the *Dignity Decree*, and with Kabátek, Liang, and Zheng (2023), who report that a Dutch reform reducing the maximum duration of fixed-term contracts increased the probability of transitioning to a permanent job. Moreover, we provide evidence of a reduction in the duration of fixed-term contracts, with a spike around the new one-year legal limit, consistent with the theoretical predictions and empirical results of Güell and Petrongolo (2007). Additional related findings come from Cahuc et al. (2023), who evaluate a Portuguese reform introducing restrictions on fixed-term contract hiring for large firms. Their reduced-form estimates reveal a decrease in temporary employment without a corresponding increase in permanent employment, contrasting with our substitution findings. However, their structural estimates accounting for spillover effects point to negligible aggregate employment effects. Furthermore, Conde-Ruiz et al. (2024) examine a recent Spanish reform that drastically limited fixed-term contracts while introducing a new flexible form of intermittent permanent contracts. Using daily aggregate employment data, they

show that the reform reduced contractual temporary employment rates only nominally but had minimal impact on de facto job stability.

Our paper also adds to the restricted set of works that have analyzed the interaction between temporary employment regulation and earnings dynamics. The empirical evidence indicates that laxer fixed-term contract regimes decrease total earnings through reductions in both wages and days worked (García-Pérez, Marinescu, and Castello 2018; Daruich, Di Addario, and Saggio 2023; Di Porto and Tealdi 2024). Moreover, it shows that workers transitioning from temporary to permanent jobs experience a wage increase at the time of conversion, which reflects a gain in bargaining power as they benefit from higher employment protection (Silva, Martins, and Lopes 2018; Daruich, Di Addario, and Saggio 2023) consistent with previous work on the permanent contract wage premium (Booth, Francesconi, and Frank 2002; Boeri 2011; Kahn 2016). We contribute to this literature by providing the first empirical evidence on the effect of restricting fixed-term contracts on the permanent contract wage premium upon conversion. Our results show that workers who get upgraded to a permanent contract after the reform are subject to a substantial compression of the conversion premium compared to the one obtained by workers converted before the reform. These findings suggest that workers are willing to accept a trade-off between pay and stability, consistent with evidence provided by Berton and Migheli (2015). Firms, in turn, take advantage of this and adapt to the new stricter regime by delaying wage increases, thus preserving their planned wage growth schedules and externalizing the cost of higher screening uncertainty due to anticipated conversions, in line with work by Faccini (2014).

The remainder of the paper proceeds as follows: Section 1 presents the institutional background and provides a detailed account of the legal provisions introduced by the reform. Section 2 illustrates the features of the datasets used for each of the two parts of our empirical analysis. Sections 3.1 and 3.2 present the empirical framework and the results of our analyses of the reform's effects on firm-level employment stocks and flows, and on the wages of converted workers, respectively. Lastly, Section 4 provides the conclusion.

1. Institutional Background

1.1. Temporary Jobs in Italy

In Italy, as in other European countries, the most typical way to formalize an employment relationship is the permanent contract: according to Eurostat's Labor Force Survey,

between 2014 and 2018 the average share of open-ended employment relationships in Italy was 88%. The remaining share of employees work in temporary jobs, governed by contractual arrangements that envisage an employment spell with a pre-determined (usually short) duration. Despite their limited weight in total employment stocks, reliance on temporary jobs has been growing steadily over the last three decades and today they account for approximately 45% of total hires.

The standard way to govern a temporary employment relationship in Italy is the fixed-term contract (*Contratto a tempo determinato*). This type of contract was first introduced in 1962 to address specific instances in which labor demand would be temporary in nature: (i) seasonal activities; (ii) substitution of absentee permanent workers for a limited time-span; (iii) specific and predetermined needs deriving from extraordinary activities.[1] Over time, the rules regulating the use of the fixed-term contracts were subject to numerous modifications (see Appendix Section A1.2), transforming them into a catch-all contractual tool for addressing diverse short-term employment needs (Sartori, Palladino, and Viviano 2025).

The Italian fixed-term contract largely conforms to the contractual instruments used to regulate temporary employment in other European countries, sharing with them a number of relevant characteristics. First, fixed-term contracts can be initiated directly by a firm or through an employment agency; in this paper, we focus on the former case, the most common. Second, a fixed-term contract can be extended: an extension occurs when the firm and worker agree to prolong the employment relationship beyond its original expiration date for an additional limited period, maintaining all other contractual terms unchanged.[2] Finally, fixed-term contracts can be converted into permanent ones at any time before their expiration date or during an extension. Contract conversions constitute the primary form of transition from temporary to permanent employment, as they allow to preserve the existing employment relationship without interruptions or bureaucratic burdens.

1.2. The 2018 Reform

The so-called *Dignity Decree* (Law Decree 2018, n.87) was among the first labor market reforms to invert the trend of temporary employment liberalization that characterized Italy and other major European countries starting from the 1990s (see Appendix Section

[1] Law 1962, n. 230.

[2] Firms can also *recall* workers: this occurs whenever a worker experiences two separate temporary employment spells at the same firm, with some time elapsing in between.

A1.1). The decree, introduced in July 2018, severely tightened the set of rules governing the use of fixed-term contracts in Italy through a combination of mandatory limitations, additional contributory costs and increased regulatory requirements. More specifically, following the introduction of the *Dignity Decree*: (i) the maximum duration of a temporary employment relationship (including all extensions) was reduced from 36 to 24 months; (ii) fewer extensions were allowed (up to 4, instead of 5); (iii) a justification for the use of a fixed-term contract became required when the employment relationship exceeded 12 months (including extensions) and for all recalls; (iv) every recall triggered a 0.5% increase in the social security contributions levied on the employer. Other minor changes were included.[3]

The regulatory tightening achieved by this reform is evident in the OECD Employment Protection Legislation Index. While employment protection for permanent contracts remained stable over the past decade (Figure A1B), the strictness of temporary employment regulation in Italy increased sharply between 2018 and 2019, reaching the highest level among the four major European economies since 2008 (Figure A1A).

2. Data

This section introduces the two data sources used in our empirical analysis: the SISCO and INPS datasets. The SISCO dataset provides detailed information on individual employment spells, allowing for precise tracking of employment flows and detailed measurement of employment stocks. We use it in the first part of the analysis to study firm-level employment changes occurring around the enactment of the *Dignity Decree*. The INPS administrative records include information on workers' wages. We exploit this dataset in the second part of our analysis to investigate changes in post-conversion wages for workers transitioning from temporary to permanent contracts before and after the reform.

2.1. Data on Employment Stocks and Flows

2.1.1 Source: SISCO. To measure firm-level employment for temporary and permanent jobs, we rely on the SISCO database (*Sistema Informativo Statistico delle Comunicazioni Obbligatorie*), constructed by Italy's Ministry of Labor and Social Policy. SISCO

[3]The *Dignity Decree* additionally establishes an increase in the statute of limitations to start litigation regarding a fixed-term contract (from 120 to 180 days after its end) and in the compensation due for an unlawful dismissal of a permanent worker (from 4-24 to 6-36 monthly wages).

compiles legally required notifications that employers across both public and private sectors must submit whenever a worker's employment status changes.[4] The database structures these notifications into records where each entry represents a continuous employment spell between a worker and an employer. SISCO allows remarkable temporal precision in tracking employment trajectories. It documents the specific dates when employment contracts begin, are extended, converted, or terminated. For fixed-term contracts, it also reports the duration that is stated by the contract at the moment of signing it. This enables us to precisely measure the duration of both employment spells and non-employment periods. It also allows us to construct a very precise measure of employment stocks based on the employment days for each worker each month.[5]

The database also contains rich contextual information about firms (industry classification), workers (demographic characteristics including sex, age, nationality, educational attainment, and residence), and job-specific details (contract type, workplace location, schedule arrangements, and occupational classification).

2.1.2 Employment Dataset. We select a large sub-sample of the original data focusing on private non agricultural employment. We exclude firms in the agriculture, public administration, education, agency work and domestic work sectors. To ensure a meaningful measure of the incidence of temporary employment at the firm level, we only include firms that recorded at least 140 employment days in each month (roughly equivalent to 5 employees) between January 2013 and June 2019. This criterion ensures a reliable denominator for our calculations.[6] As a result of this selection process, our final sample consists of about 120,000 firms.

We organize the data into a balanced firm-level monthly panel. For each firm, it reports all the monthly hiring, termination and contract-conversion events that occurred between January 2013 and June 2019, as well as total employment days by type of contract, which we use to measure employment stocks. Before aggregating everything at the firm level, we exclude workers under 18 and above 65 years of age at the moment

[4] Mandatory electronic reporting began in 2008, ensuring the database comprehensively covers all dependent employment relationships initiated in Italy since then. For more detailed information on the database, see: *Il sistema delle Comunicazioni Obbligatorie: uno strumento per l'analisi del mercato del lavoro*, 2010; *Il sistema informativo statistico delle Comunicazioni Obbligatorie SISCO*, 2014.

[5] Employment days are obtained by considering: a full month – 28, 30 or 31 days depending on the calendar – if the employment spell starts before the beginning of the given month and ends after its termination; a subset of days if the contract starts or ends in a given month.

[6] Because our measure of exposure, introduced in Section 3.1.1, is based on the share of employment days under fixed-term contracts, any firm-month pair with very few total employment days could produce extreme or volatile ratios that would not accurately reflect the true exposure to the reform.

of hiring. Despite these selection steps, employment dynamics in our sample closely mirror those in the population-level data (see Figure A3).[7]

2.2. Data on Conversion Events and Wages

2.2.1 Source: Uniemens (INPS). To study wages, we use administrative records sourced from the monthly reports known as *Rapporti di lavoro mensili Uniemens*, collected by the Italian Social Security Institute (INPS). The dataset covers all non-agricultural private sector dependent workers since 1974. We use information on the total monthly salary and number of days worked for each employee, the type of contract (permanent vs. fixed-term, full-time vs. part-time) and the total number of days and weeks worked in a given month. To calculate the full-time equivalent (FTE) daily wage, we first adjust the total number of days worked in a month by the percentage of part-time employment.[8] Then, we derive FTE daily earnings by dividing total annual gross earnings by the total number of adjusted days worked in the year.

Additionally, we incorporate information from *Unilav* reports to precisely track individual transitions from fixed-term to permanent contracts. These data also provide information on the employee's occupation and level of education.[9]

2.2.2 Wages Dataset. We structure our dataset so that each observation represents a conversion from fixed-term to permanent employment occurring between 2017 and 2019. For each conversion, we collect information about the worker at the time of transition, including daily wage, sex, experience (months since labor market entry), tenure (months since joining the firm), age, occupation, and education level. We also record the worker's average daily wage during the preceding temporary employment period and identify the firm where the conversion occurred. To avoid distortions related to censoring, we only keep firm-worker matches that can be observed for at least 6 months in their permanent contract employment.

Our analysis is restricted to firms that have reported at least one new permanent worker – either through a conversion or through a direct hire from outside the company

[7] Population-level data on employment dynamics are available on the webpage of the *Osservatorio sul mercato del lavoro (INPS)*.

[8] This percentage is 1 for full-time and vertical part-time employment and less than 1 for horizontal part-time employment. A full-time contract typically involves 8 hours a day over 5 days. Vertical part-time reduces the number of working days while keeping 8-hour days, whereas horizontal part-time reduces daily hours across a standard 5-day week.

[9] The data report 5-digit occupational codes from Istat's CP2011 classification, corresponding to the ILO's ISCO-08 classification.

– both before and after the reform. This allows us to mitigate concerns for potential selection bias in the composition of converting firms after the reform. Figure A5 shows the trend in contract conversions over the selected period using three different data series. The blue series illustrates the official statistics as detailed in the INPS quarterly report,[10] the red series is derived from our initial sample, and the green series represents the subset of firms that recorded at least one conversion both before and after the policy change. The vertical lines mark July 2018 (when the government introduced the decree) and the time of its full implementation after parliamentary approval. The figure shows a significant increase in the number of conversions following the implementation of the reform. Notably, the trends shown by our refined sample closely match the trajectories shown in the other two series, highlighting its accuracy in capturing the shifts that occurred in the wake of the reform.

3. Empirical Analysis

Our empirical analysis is organized into two parts. In the first part, we examine how the reform affected firms' hiring strategies and workforce composition using the data on employment described in Section 2.1. In the second part, we exploit the data described in Section 2.2 to study the effects of the reform on wage dynamics for workers transitioning from fixed-term to permanent contracts, the category most affected by the policy change.

3.1. The Effect of the Reform on Employment

3.1.1 Defining Exposure to the Reform. Unlike other labor market reforms, the rollout of the *Dignity Decree* did not involve distinct policy target groups or a staggered implementation, which would facilitate a straightforward quasi-experimental research design to identify its causal effects. To overcome this limitation, we construct a firm-level measure that captures differential exposure to the reform based on firms' prior usage of fixed-term contracts.

As discussed in Section 1.2, the reform increased the cost of using fixed-term contracts, with longer contracts suffering a larger increase in the regulatory burden. Specifically, the reform introduced particularly stringent requirements for fixed-term contracts lasting more than 12 months. This created a significant disincentive to offer long-lasting

[10] For further information, see the webpage of the *Osservatorio sul mercato del lavoro (INPS)*.

fixed-term contracts or to prolong a temporary employment spell with multiple contract extensions. In line with this, Figure A4 reflects a marked shift in fixed-term contract durations following the reform, illustrating a sharp increase in the concentration of durations just below the 12-month threshold.[11]

To aptly capture these institutional features, we propose an exposure measure that reflects both how extensively a firm relied on fixed-term contracts before the reform and the duration composition of the underlying temporary work relationships, thus accounting for nonlinearity in duration. It is defined as follows:

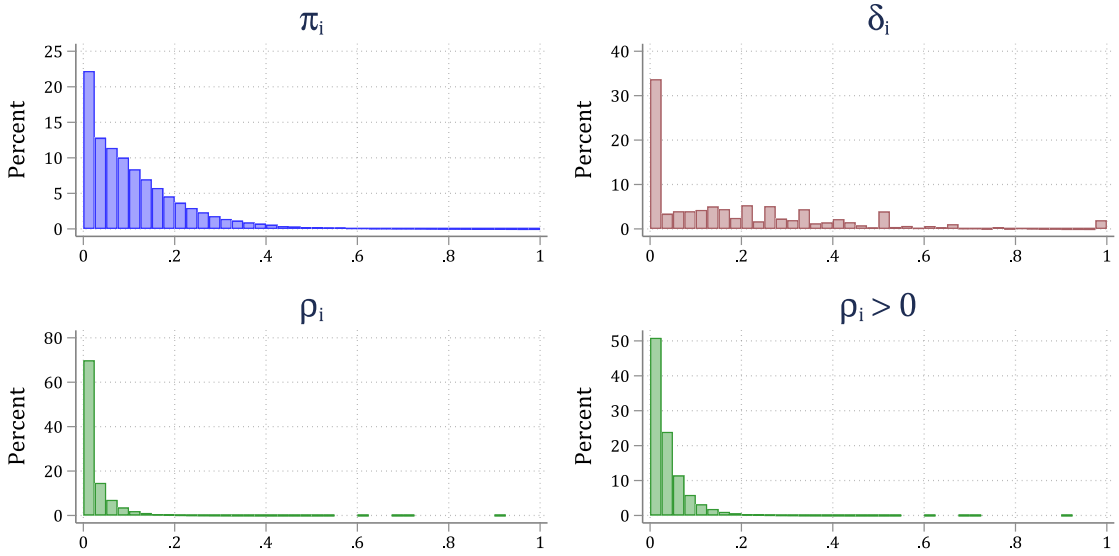
$$\rho_i = \pi_i \times \delta_i \tag{1}$$

where π_i captures the historical reliance of firm i on temporary workers — measured as the average monthly share of employment days on fixed-term contracts relative to the total employment days in the firm during 2013-2017 — and δ_i captures the intensity of the use of long fixed-term contracts — measured as the average monthly share of fixed-term contracts with a total duration (stated duration plus eventual extensions) of 12 months or more over the same period.

Figure 1 presents the distribution of the exposure measure and its components across firms. The top-left panel displays π_i, showing that the average firm has about 11.5% of workdays on fixed-term contracts, although this varies considerably across the economy. The top-right panel displays δ_i, revealing that approximately 30% of firms that relied on temporary work during 2013-2017 never offered any fixed-term contract exceeding 12 months in duration, the critical threshold targeted by the reform. The bottom-left panel shows the compound exposure measure ρ_i. The bottom-right panel depicts the exposure measure conditional on positive values, highlighting substantial variation among firms with at least some exposure to the policy change.

[11]The duration distributions shown in Figure A4 are derived from administrative data covering all workers in the Veneto region, which accounts for approximately 10% of the national workforce. The advantage of using this regional subset is that it includes all labor flows in Veneto up to 2021, thereby avoiding the right-censoring that affects our main dataset for events occurring after the second quarter of 2019.

FIGURE 1. Distribution of the Exposure Measure and its Components



Notes: The figure presents the distribution of the exposure measure and its components. The top-left panel shows π_i, the average monthly share of employment days on fixed-term contracts relative to total employment days during 2013-2017. The top-right panel shows δ_i, the average share of fixed-term contracts with a total duration (stated duration plus eventual extensions) of twelve months or more over 2013-2017. The bottom-left panel displays ρ_i, computed as the product of π_i and δ_i. The bottom-right panel shows ρ_i conditional on positive values.
Source: SISCO, see Section 2.1.

Table 1 compares non-exposed firms ($\rho_i = 0$) with exposed firms ($\rho_i > 0$). Exposed firms represent 61.3% of the sample and show higher fixed-term contract utilization as expected. However, the two groups exhibit similar patterns in permanent contract dynamics. While exposed firms have significantly more employment days in total, the proportion of employment days on permanent contracts remains sizeable and fairly similar for both groups (82% for non-exposed vs 73% for exposed), indicating comparable core workforce structures despite different temporary employment practices.

TABLE 1. Pre-reform Outcomes – Non-exposed vs. Exposed Firms

		Non-exposed ($\rho_i = 0$)	**Exposed** ($\rho_i > 0$)
N. of firms		46,148 (38.7%)	73,057 (61.3%)
Monthly Flows			
Outcome	**Contract Type**		
Hirings	Permanent	0.114 (0.747)	0.321 (2.750)
Hirings	Fixed-term	0.364 (8.349)	1.789 (21.582)
Conversions	F.T. $\rightarrow$ P.	0.024 (0.215)	0.178 (1.301)
Hirings	Other contracts	0.267 (7.476)	0.566 (11.512)
Terminations	Permanent	0.127 (1.218)	0.361 (3.659)
Terminations	Fixed-term	0.275 (8.235)	1.486 (21.190)
Monthly Stocks			
Outcome	**Contract Type**		
Employment Days	Permanent	382.5 (875.2)	1004.2 (5620.3)
Employment Days	Fixed-term	23.5 (121.7)	246.8 (1343.6)
Employment Days	Other contracts	57.0 (543.4)	120.9 (1286.5)
Employment Days	Total	463.0 (1064.2)	1371.8 (7002.5)

Notes: This table presents monthly averages and standard deviations (in parentheses) for several employment indicators measured from January 2018 to June 2018. Firms are categorized into two groups based on their exposure to the reform. Employment flows are measured as the average monthly number of events (hirings, conversions, or terminations) per firm, while stocks are measured as average monthly employment days.
Source: SISCO, see Section 2.1.

3.1.2 Empirical Strategy. We rely on an event-study design that leverages our exposure measure coupled with the fine-grained time variability of the data while controlling for unobservable time-invariant and time-specific factors. Our regression specification can be written as follows:

$$y_{it} = \sum_{\substack{k=-5 \\ k \neq 0}}^{12} \beta_k \times \rho_i \times \mathbf{1}\{t = t_0 + k\} + \alpha_i + \tau_t + \iota_{st} + \epsilon_{it}, \tag{2}$$

where y_{it} is a firm-level occupational outcome in year-month t, t_0 indicates June 2018, and the terms α_i, τ_t and ι_{st} denote firm, time, and industry-by-time fixed effects, respectively. This type of identification strategy has been used extensively in the policy evaluation literature (Carry 2022; Harasztosi and Lindner 2019; Saez, Schoefer, and Seim 2019), and the identification assumption is that in the absence of the reform, firms with different exposures would have followed similar employment trajectories after controlling for firm-specific and time-varying factors.

A concern that may arise from our use of monthly data is the potential presence of exposure-specific seasonal patterns, which cannot be controlled for directly in our main specification due to collinearity with the treatment effects. To address this, we first residualize our outcome variables by estimating:

$$y_{it} = \sum_{m(t)=1}^{12} \gamma_{m(t)} \times \rho_i \times \mathbb{1}\{month = m(t)\} + \alpha_i + \tau_t + \iota_{st} + \nu_{it} \tag{3}$$

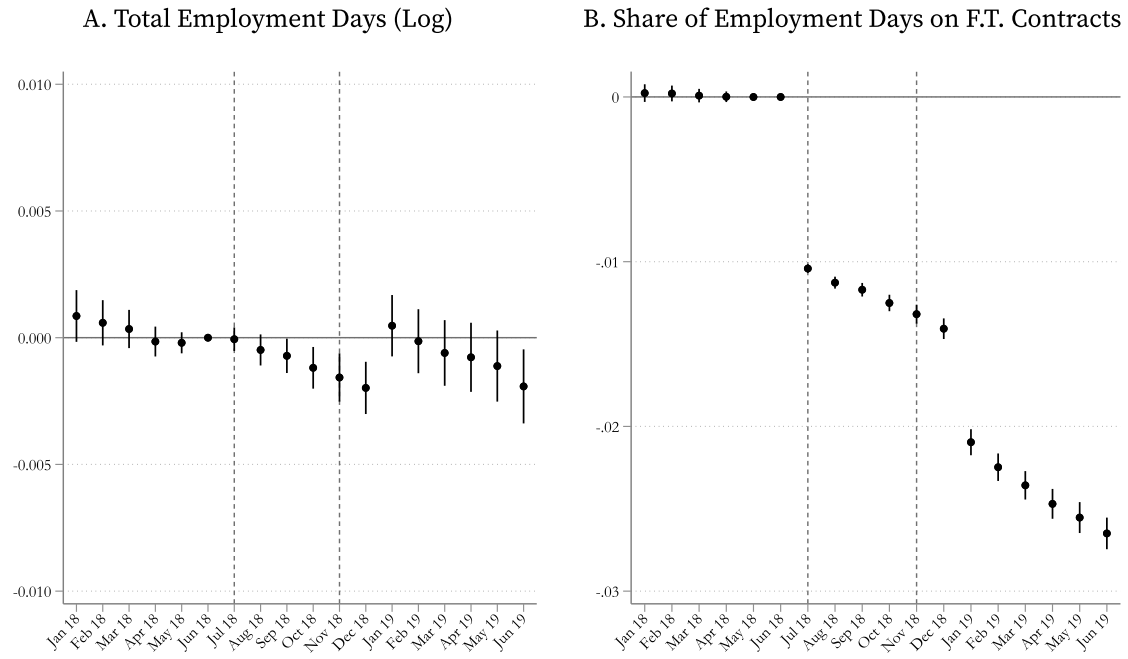
where $\mathbb{1}\{month = m(t)\}$ represents calendar month indicators. We restrict this estimation to the period between January 2017 and June 2019 to ensure that each firm is observed at least twice in each calendar month (across different years), while maintaining a reasonable time window around the reform and avoiding considering 2015 and 2016, two years in which the Italian labour market was flooded with hiring subsidies. Only the predicted values from the exposure-specific seasonality ($\sum_{m=1}^{12} \gamma_m \times \rho_i \times \mathbf{1}_{month=m}$) are then subtracted from the original outcome, leaving firm, time, and industry-by-time fixed effects in the data. We use this seasonally-adjusted variable as our dependent variable in the main event-study specification. This two-step procedure allows us to control for exposure-specific seasonality while maintaining the ability to identify the reform's effects through the event-study framework.

3.1.3 Results Estimation results from equation (2) for several outcomes are presented in a series of figures showing parameter estimates and 95% confidence intervals for the coefficients β_k, which are identified with respect to June 2018 (the last pre-reform period). In each figure, the first vertical line indicates July 2018, when the government presented the *Dignity Decree*, and the second line indicates the date of its full enactment after parliamentary approval.

Effect on Employment Stocks. We start by investigating the dynamics of employment stocks around the reform's enactment. Figure 2 displays the results for total employment

days and for the share of employment days on fixed-term contracts.

FIGURE 2. Employment Stocks



Notes: The graph reports coefficient estimates and 95% confidence intervals for $\{\beta_k\}_{k=\text{June 2018}}$ from Equation (2), showing the relationship between firm exposure intensity ρ and employment outcomes. In Panel A, the outcome is monthly total employment days (in log terms), where a coefficient of -0.01 indicates that a one standard deviation increase in ρ is associated with a 1% decrease in employment days. Panel B shows coefficients for the monthly share of employment days on fixed-term (F.T.) contracts, where a coefficient of -0.02 indicates that a one standard deviation increase in ρ is associated with a 2 percentage point decrease in the fixed-term share. All outcomes are first residualized with respect to calendar-month specific exposure effects following Equation (3). Standard errors are clustered at the firm level. The first vertical line indicates the month when the government introduced the reform, and the second line indicates the date of its full enactment after parliamentary approval.
Source: SISCO, see Section 2.1.

The coefficients in Figure 2A show that, on average, the reform had minimal employment effects once fully implemented, with only a small transitory decline between July and December 2018 (at most -0.2% for a one standard deviation increase in exposure). However, Figure 2B shows a significant shift in the composition of the workforce of exposed firms. After the decree was announced, and even more dramatically after it fully took effect, these firms experienced a significant decline in their use of fixed-term contracts. By mid-2019, the share of employment days on fixed-term contracts had fallen by about 2.5 percentage points for every standard deviation increase in exposure. This represents a substantial 10% reduction from the pre-reform average of 24.7% for exposed firms, suggesting a significant restructuring of employment relationships in response to increased regulatory costs.

Figure A6 provides additional evidence by presenting the regression estimates ob-

tained using the share of employment days under permanent contracts and other contract types as dependent variables.[12] The results show a clear increase in the share of permanent employment, which exactly mirrors the decline in the use of fixed-term contracts documented in Figure 2B, suggesting a direct substitution between contract types. Meanwhile, the share of employment days under other contract types remains essentially unchanged throughout the period, suggesting that firms responded to the reform primarily by converting temporary positions into permanent ones rather than shifting to alternative flexible arrangements.

Effect on Employment Flows. Next, we look at gross flows, to better understand the adjustment in hiring strategies that drove the observed substitution pattern. Figure 3 shows the results when examining hiring by contract type and entry type for new permanent jobs. The monthly gross flows data have a significant number of zero values and a skewed distribution. To deal with this appropriately, we apply the inverse hyperbolic sine (IHS) transformation. This transformation helps stabilize the variance and allows us to better handle the characteristics of the distribution, leading to more reliable results in our analysis.[13]

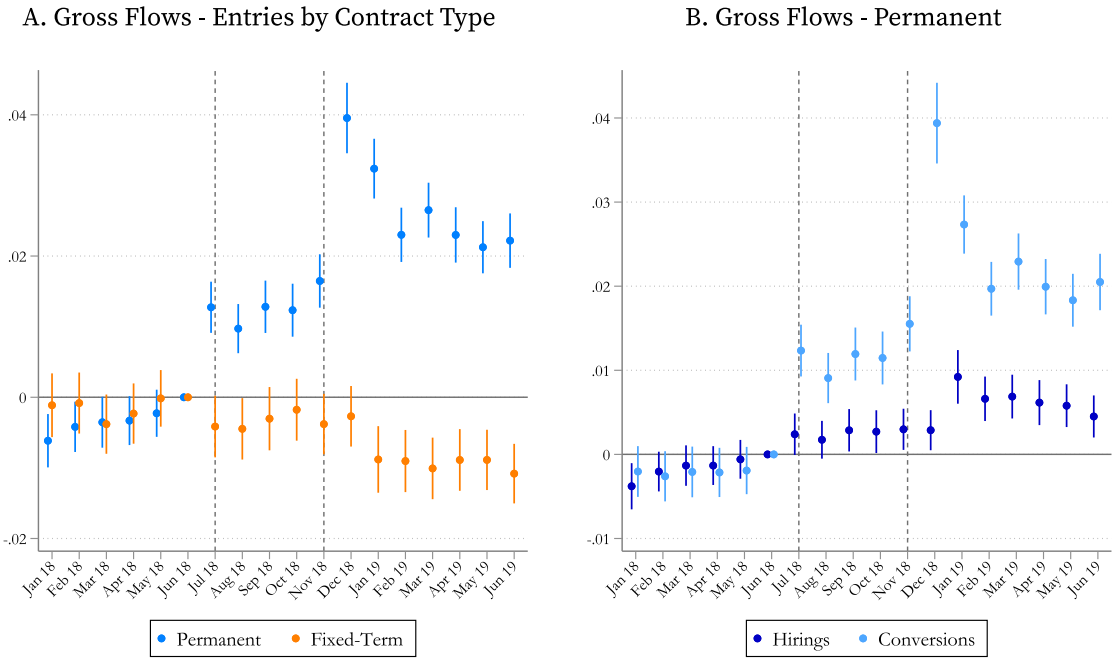
The estimates show a statistically significant relationship between reform exposure and hiring strategies after July 2018: a one standard deviation increase in exposure is associated with a substantial increase of new permanent contracts (Figure 3A). This increase is mainly driven by the conversion of existing fixed-term contracts (Figure 3B).[14] In the post-reform period, greater exposure is also associated with a significant drop in temporary entries.

[12] Other contracts include (i) apprenticeship, (ii) consultancy (so-called *parasubordinato*), (iii) intermittent worker, (iv) internship (so-called *tirocinio*) and (v) seasonal worker.

[13] Based on recent work by Chen and Roth (2023), the interpretation of the coefficients avoids presenting them as percentage differences associated with changes in exposure intensity in the pre-post reform period. This decision is made to avoid arbitrary interpretations that could be influenced by the specific units of the outcome variable, especially when dealing with extensive margin effects, as in the case of monthly gross flows. Here, we are primarily interested in investigating the presence of a discontinuity in firms' hiring behavior around the introduction of the reform.

[14] Note that while hirings + conversions = permanent entries in levels, the coefficients presented do not sum exactly because all variables are transformed using the IHS transformation.

FIGURE 3. Difference in Hiring Patterns



Notes: The graph reports coefficient estimates and 95% confidence intervals for $\{\beta_k\}_{k=\text{June 2018}}$ from Equation (2), showing the relationship between firm exposure intensity ρ and employment outcomes. In Panel A, the outcome are gross flows by contract type. Permanent gross flows include both new permanent employment relationships and conversions of existing temporary contracts. Panel B separates between new permanent employment relationships and conversions. The outcomes are first transformed using the inverse hyperbolic sine (IHS) transformation and then residualized with respect to calendar-month specific exposure effects following Equation (3). Standard errors are clustered at the firm level. The first vertical line indicates the month when the government introduced the reform, and the second line indicates the date of its full enactment after parliamentary approval.
Source: SISCO, see Section 2.1.

Finally, figure A9 shows a small positive effect on permanent contract terminations in the post-reform period. This very modest increase in terminations only partially offsets the increase in permanent hirings,[15] further confirming that the primary mechanism driving the compositional change is the conversion of fixed-term contracts into permanent positions rather than a net expansion of permanent employment through new external hires.

Robustness to Sample Composition. A potential concern with our empirical strategy is that firms which offer mostly short-duration fixed-term contracts are classified as non-exposed ($\delta_i = 0$), but they may still be partially affected by the reform given that they employ temporary workers ($\pi_i > 0$). Table A1 shows that these firms are substantial in number, representing 30.2% of our sample. Although they do not have long-term fixed contracts, they still employ a modest number of temporary workers (accounting,

[15] When estimating the effect on permanent net flows (hirings minus terminations), the significance almost disappears.

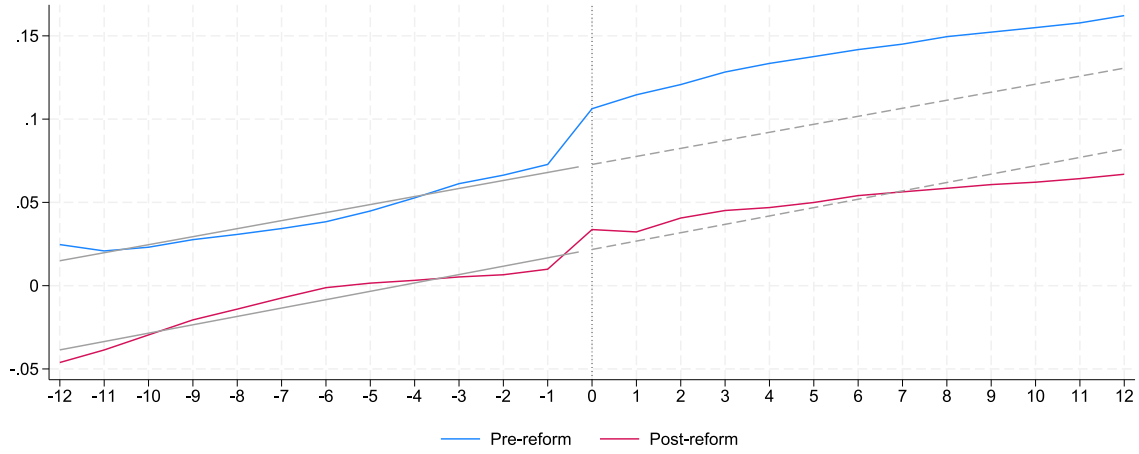
on average, for only 6% of total employment days). If these partially exposed control firms were to adjust their employment practices in response to the reform, our baseline estimates would represent a lower bound on the true effects. To address this concern, we also perform a sensitivity analysis where we estimate the specification in (2) using a more restrictive control group consisting only of firms with no temporary employment at all between 2013 and 2017 ($\pi_i = 0$).

Figures A7, A8, and A10 present the results from this sensitivity check for each outcome. These tests strongly confirm our baseline results, as the estimates obtained using the restricted control group are almost identical to those obtained with the unrestricted one. This consistency between our two sets of results suggests very minimal adjustments for firms that use fixed-term contracts of low and medium duration but do not exceed the twelve-month threshold targeted by the reform. Taken together, these findings support the robustness of our identification approach and strongly suggest that the reform primarily affected firms relying on longer-duration fixed-term contracts, as intended.

3.2. The Effect of the Reform on the Wages of Converted Workers

The first part of our empirical analysis focused on firms' hiring adjustments and changes in workforce composition. In this second part, we investigate how the reform affected the wages of workers moving from temporary to permanent employment.

FIGURE 4. Average Daily Wages Around Conversion Events



Notes: The figure displays average log daily wages around conversion events. Wages are first demeaned at the month-collective bargaining agreement level, so as to account for the generalized wage growth deriving from collective contracts. The average is calculated for the sample of firms that can be observed for at least 12 months before and 12 months after the conversion, so as to minimize composition effects in the estimates.
Source: INPS, see Section 2.2.

As a first step in this direction, we expand the dataset of contract conversion events occurring between July 2017 and July 2019 described in Section 2 to a monthly panel tracking the evolution of converted workers' wages. Figure 4 plots average log daily wages over a 24 month window centered around the conversion event, along with a linear trend estimated on the pre-reform period and projected in the post-reform months. The wages are demeaned each month within groups identified by different collective bargaining agreements.[16] Workers converted after the *Dignity Decree* started their temporary employment period with lower average wages (initial gap between the two lines) compared to the workers converted before the reform. Importantly, no difference in the pace of wage growth (the slope of the two lines) emerges during the months before the shift to a permanent position between the pre- and the post-reform conversion groups. However, wage growth becomes strikingly different upon conversion: before the reform a transition to a permanent job implied a 3.5% increase in remuneration, while this increase is absent for the post-reform period.

Nevertheless, these raw differences may partly reflect changes in the composition of converter firms and/or converted workers following the reform, as already suggested by the lower average wages observed during the temporary period, rather than a genuine shift in wage-setting practices associated with contract conversion within jobs.

[16] In Italy, the majority of the workforce's wages are adjusted through collective bargaining, which is governed by several hundred agreements.

3.2.1 Empirical Strategy In order to better isolate the effect of the reform, we turn to a regression framework that compares within-firm wage dynamics for similar workers obtaining a contract upgrade before and after the reform. Specifically, we use the dataset of conversion events, where each observation corresponds to a single contract conversion at a given firm, and estimate the following regression model:

$$W_{i,j,t}^{T\to P} = \Psi_j + \phi X_i + \gamma \overline{W}_{T,i} + \beta_t + \epsilon_{i,t} \tag{4}$$

where $W_{i,j,t}^{T\to P}$ is the wage of worker i after a transition from a temporary (T) to a permanent (P) contract at firm j in month t. This is computed as the log daily wage smoothed over the time window from the conversion month t until month $t+6$. On the right hand side of the equation we include a set of year-month dummies, β_t, as well as a range of covariates to capture changes in wage dynamics that can be attributed to factors other than the reform: Ψ_j is a firm fixed effect, reflecting differences in compensation policies across employers;[17] X_i is a vector of individual characteristics measured at the time of conversion,[18] accounting for changes in the marginal converted worker; and $\overline{W}_{T,i}$ is the average log daily wage for individual i over the duration of the temporary employment spell before the contract conversion, controlling for job-specific heterogeneity in remunerations. The coefficients of interest, β_t, identify the change in average post-conversion wages for similar workers over time, relative to the last pre-reform month, June 2018. They are estimated over a 24-month window centered around the reform. Table A2 presents summary statistics illustrating differences in a set of key variables before and after the reform. Importantly, the unconditional conversion premium before the reform amounts to about 8 log points, in close alignment with the 6% estimate by Daruich, Di Addario, and Saggio (2023).

3.2.2 Results The regression results are summarized in Figure 5, which shows three series of estimates for the coefficients β_t and their 95% confidence intervals, derived by progressively enriching the specification in equation (4). The first series (illustrated in blue) is estimated using the most parsimonious specification, which includes only firm fixed effects; the second series (in red) is obtained by adding worker-level controls;

[17] We restrict our attention to firms that recorded at least one new permanent worker – either through a conversion or through a direct hire from outside the company – both before and after the implementation of the reform.

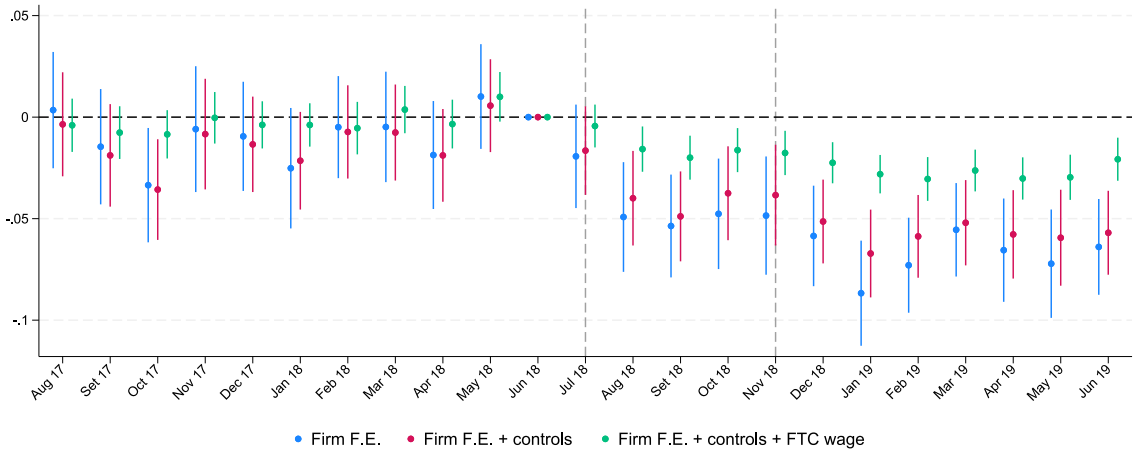
[18] X_i includes a quadratic function of age, experience, and tenure, and interactions of five-digit occupation codes with a seven-level categorical education variable and a sex dummy, yielding 2,878 occupation-education-sex cells.

estimates in the third series (in green) also account for variation in fixed-term contract wage levels. For none of these specifications, the estimates for the β_t coefficients are statistically different from zero in the pre-reform period, indicating no changes in starting wages upon conversion before the reform was implemented.

Controlling for firm composition, workers who transitioned to a permanent job after the reform experienced considerably lower post-conversion wages ($\approx$ –5 log points) compared to workers whose conversion occurred before the reform was implemented. No significant difference emerges instead in the pre-reform period. When individual-level controls are added, the observed drop is only slightly reduced, suggesting that the composition of converted workers does not explain much of the variation in permanent contract wages. Even including the fixed-term contract wage as a control, the post-conversion wage penalty for workers who transitioned to a permanent contract after the reform remains large and persistent, although in the post-reform period it is halved to approximately 2 log points. This latter specification serves to control for latent match-specific factors that could be due to unobserved characteristics of the worker or the job.[19] As a placebo test, we run a regression with firm effects and worker covariates (the second specification in Figure 5) including both converted workers and newly hired permanent contract workers who did not go through a temporary employment spell in the same firm (Figure A11). Contrarily to what is observed for converted workers, the starting wages of direct permanent contract hires display no discontinuity when the reform is introduced.

[19] Residual selection on unobservable worker quality that is uncorrelated with job and firm quality remains a concern that our design cannot fully eliminate.

FIGURE 5. Post-Conversion Log Daily Wages



Notes: The figure displays estimates of β_t from different specifications of Equation (4), showing changes in log-daily wages for workers transitioning from a temporary to a permanent contract, relative to June 2018. The estimates in blue are obtained controlling for firm fixed effects only; the estimates in red also account for individual characteristics; the estimates in green further adjust for the wage in the fixed-term contract that gets converted. Standard errors are clustered at the firm level. The first vertical line indicates the month when the government introduced the reform, while the second one the date of its full enactment after parliamentary approval.
Source: INPS, see Section 2.2.

Overall, these results provide evidence that the reform negatively affected the starting wages of similar workers upon their transition from a temporary to a permanent contract. To put this in figures, assuming that an individual works 312 days a year, the wage decrease translates into an annual loss of between €271.5 (according to the last specification) and €589.8 (according to the first) for individuals experiencing a contract conversion after the reform.[20]

These findings indicate that firms may have accepted to convert a larger share of their temporary workforce — or to anticipate their conversion — in response to the reform. While we cannot fully disentangle these two channels, they both suggest that firms retained substantial bargaining power and may have leveraged it to offset the increased rigidity with flatter post-conversion wage trajectories. Both mechanisms are plausible and likely at play. On the one hand, we observe no major shifts in average tenure or

[20]The monetary equivalent of the estimated wage penalty is computed using the formula $(e^{\mu} - e^{\mu - \bar{\hat{\beta}}}) \times D$, where μ is the average log daily wage before the reform, $\bar{\hat{\beta}}$ is the average estimated post-reform drop in log wages based on equation (4), and D is the number of working days in a year. We use $\mu = 3.634$ (see Table A2) and $D = 312$. In the most parsimonious specification (firm fixed effects only), the average post-reform $\bar{\hat{\beta}}$ equals 0.0512, yielding a daily wage difference of approximately €1.89 and an annual loss of €589.8. In our richest specification (which includes worker and fixed-term wage controls), $\bar{\hat{\beta}} = 0.0235$, corresponding to a daily loss of about €0.87 and an annual loss of €271.5. Our observation window ends in 2019 due to the Covid-19 pandemic, preventing us from evaluating whether these gaps persist over time or are eventually recovered.

experience at the time of conversion, and converted workers exhibit lower average wages, suggesting that firms may have started converting lower-wage workers and/or that lower-paying firms may have become more likely to convert. On the other hand, the sharp concentration of overall contract durations just below the 12-month threshold, previously illustrated in Figure A4, points to anticipatory behavior in response to the new legal constraints.

4. Conclusions

The *Dignity Decree* marked a turning point in the regulatory approach to fixed-term contracts in Italy. After several waves of liberalizing reforms, this regulatory intervention tightened the restrictions on fixed-term contracts utilization in Italy.

We show that the reform prompted a significant restructuring of workforce composition within exposed firms. The share of employment days under fixed-term contracts decreased markedly, as companies reduced their reliance on temporary jobs in response to increased regulatory costs and constraints. This reduction was primarily offset by an increase in permanent employment, achieved mainly through the conversion of existing fixed-term contracts rather than external hiring. Furthermore, the reform led to only transient and negligible employment losses, thus reaching its stated objective of discouraging systematic recourse to long-lasting temporary jobs and prompting substitution with permanent positions.

However, the reform also affected wage outcomes for converted workers. Workers transitioning from fixed-term to permanent contracts experienced a pronounced decline in their starting wages after the reform, with an estimated gap between –5 and –2 percentage points compared to the pre-reform period. Such reduction in post-conversion wages may reflect both changes in firms' wage-setting policies and shifts in the composition of converted workers, though our extensive controls in the richest specification suggest the former plays a role. This compression of the conversion premium suggests that firms exerted some degree of bargaining power in response to the increased regulatory burden, effectively passing on the costs of enhanced job security to workers in the form of lower wages. Interestingly, this wage penalty was not observed for externally hired permanent workers, indicating that the adverse impact was concentrated among those directly affected by the new restrictions on fixed-term contracts.

These findings underscore a critical trade-off inherent in the regulation of dual

labor markets. While stricter employment protection can enhance job stability and reduce the incidence of precarious work, it may also come at the expense of workers' earnings. This highlights a broader challenge in labor market regulation: striking the right balance between protecting workers from precarious working conditions and creating economic incentives to sustain wage growth.

Overall, our research contributes to the ongoing debate on the regulation of fixed-term contracts by providing robust evidence on the complex interplay between job security and wage dynamics. The findings illustrate that while tighter regulation of temporary work can reshape employment patterns in line with policy objectives, the accompanying economic costs must be carefully considered to avoid undermining the intended benefits of such reforms.

References

Bentolila, Samuel, Juan J. Dolado, and Juan F. Jimeno. 2020. "Dual Labor Markets Revisited." In *Oxford Research Encyclopedia of Economics and Finance*, 1–34.

Berton, Fabio, and Matteo Migheli. 2015. "Estimating the Marginal Rate of Substitution between Wage and Employment Protection." *Working Paper no. 143 LABORatorio R. Revelli*.

Boeri, Tito. 2011. "Institutional Reforms and Dualism in European Labor Markets." In *Handbook of Labor Economics*, vol. 4, 1173–1236: Elsevier.

Boeri, Tito, and Pietro Garibaldi. 2024. "Temporary Employment in Markets with Frictions." *Journal of Economic Literature* 62 (3): 1143–1185.

Booth, Alison L., Marco Francesconi, and Jeff Frank. 2002. "Temporary Jobs: Stepping Stones or Dead Ends?." *The Economic Journal* 112 (480): F189–F213.

Cahuc, Pierre, Pauline Carry, Franck Malherbet, and Pedro S. Martins. 2023. "Spillover Effects of Employment Protection." *Nova SBE Working Paper Series No. 655*.

Cahuc, Pierre, Olivier Charlot, and Franck Malherbet. 2016. "Explaining the spread of temporary jobs and its impact on labor turnover." *International Economic Review* 57 (2): 533–572.

Carry, Pauline. 2022. "The Effects of the Legal Minimum Working Time on Workers, Firms and the Labor Market." *Working Paper*.

Chen, Jiafeng, and Jonathan Roth. 2023. "Logs with zeros? Some problems and solutions." *Working Paper*.

Conde-Ruiz, J. Ignacio, Manu García, Luis A. Puch, and Jesús Ruiz. 2024. "Reforming Dual Labor Markets: "Empirical" or "Contractual" Temporary Rates?" *Working Paper*.

Daruich, Diego, Sabrina Di Addario, and Raffaele Saggio. 2023. "The Effects of Partial Employment Protection Reforms: Evidence from Italy." *The Review of Economic Studies*.

Di Porto, Edoardo, and Cristina Tealdi. 2024. "Heterogeneous Paths to Stability." *Journal of the Royal Statistical Society Series A: Statistics in Society* 187 (4): 1037–1064.

Faccini, Renato. 2014. "Reassessing labour market reforms: Temporary contracts as a screening device." *The Economic Journal* 124 (575): 167–200.

García-Pérez, J. Ignacio, Ioana Marinescu, and Judit Vall Castello. 2018. "Can Fixed-term Contracts Put Low Skilled Youth on a Better Career Path? Evidence from Spain." *The Economic Journal*.

Grasso, Giuseppe, and Konstantinos Tatsiramos. 2023. "The Impact of Restricting Fixed-Term Contracts on Labor and Skill Demand." *IZA DP No. 16496*.

Güell, Maia, and Barbara Petrongolo. 2007. "How Binding Are Legal Limits? Transitions from Temporary to Permanent Work in Spain." *Labour Economics* 14 (2): 153–183.

Harasztosi, Péter, and Attila Lindner. 2019. "Who Pays for the Minimum Wage?" *American Economic Review* 109 (8): 2693–2727.

Kabátek, Jan, Ying Liang, and Kun Zheng. 2023. "Are Shorter Cumulative Temporary Contracts Worse Stepping Stones? Evidence from a Quasi-Natural Experiment." *Labour Economics* 84: 102427.

Kahn, Lawrence M. 2016. "The Structure of the Permanent Job Wage Premium: Evidence from Europe." *Industrial Relations* 55 (1): 149–178.

Martins, Pedro S. 2021. “Should the Maximum Duration of Fixed-Term Contracts Increase in Recessions? Evidence from a Law Reform.” *International Review of Law and Economics* 68: 106009.

Saez, Emmanuel, Benjamin Schoefer, and David Seim. 2019. “Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers’ Tax Cut in Sweden.” *American Economic Review* 109 (5): 1717–1763.

Sartori, Matteo, Marco Guido Palladino, and Eliana Viviano. 2025. “Why Do Firms Use Fixed-Term Contracts?” *Working Paper*.

Silva, Marta, Luis Filipe Martins, and Helena Lopes. 2018. “Asymmetric Labor Market Reforms: Effects on Wage Growth and Conversion Probability of Fixed-Term Contracts.” *ILR Review* 71 (3): 760–788.

Appendix

A1. Background

A1.1 The international context Fixed-term contracts have enjoyed a great diffusion during the last three decades. When during the 1980s and '90s unemployment rates started to pick up and remain high for prolonged periods in many European countries, part of the blame was ascribed to the very low degree of flexibility prevailing in European labor markets at the time. As a reaction, policymakers in these countries started reforming their body of Employment Protection Legislation (EPL) with interventions aimed at revamping labor market dynamism and leaving the so-called *Eurosclerosis* behind. Rather than embarking on the politically-costly endeavor of reforming the legislation covering permanent employment contracts, however, many governments opted for an alternative: adjusting the degree of labor market flexibility at the margin by facilitating the use of temporary employment contracts, considered a less-binding form of working relationship between employer and employee.

The rationale behind this move was simple: matching the marginal labor demand – for which the very rigid and protected permanent contracts would be too costly – with a new slice of supply, drawn directly out of the ranks of the unemployed. To many policymakers this looked like a "free lunch" policy, which would increase total employment without the need to touch the rights of incumbent permanent workers. In countries such as Italy and Spain, the de-regulation of fixed-term contracts came to be considered a standard tool to boost employment up to the mid-2010s.

Today, after three decades and many reforms abiding by this principle, the era of temporary work liberalizations might have finally come to an end: governments have started to give more weight to the downsides of job precariousness and some of the countries characterized by very high usage of fixed-term contracts – Portugal, Italy and Spain – have recently taken step backs to discourage systematic recourse to these contractual arrangements.

A1.2 Labor market reforms in Italy The progressive diffusion of fixed-term contracts in Italy dates back to the 1990s when a number of reforms started a process of fixed-term contracts liberalization that continued throughout the 2000s. As underlined in Daruich, Di Addario, and Saggio (2023), most of these legislative measures were *partial*, meaning that, while the regulation of fixed-term contracts was largely relaxed, the employment protection measures associated with permanent positions remained unchanged. Over

the 2010s, on the contrary, three major reforms significantly intervened in the Italian labor market by acting on the legislation concerning both types of contract.

In 2012, the *Fornero Reform*[21] attempted to reduce the regulatory gap between permanent and fixed-term contracts by acting on both sides: on one hand, it reduced the uncertainty about firing costs associated to permanent positions;[22] on the other hand, it made more costly the renewal of fixed-term contracts.[23]

In 2015, the *Jobs Act* brought even further the reduction of protection for permanent positions[24] but at the same time strongly liberalized the use of fixed-term contracts.[25]

The third of these reforms was the *Dignity Decree*, the object of interest of our study (see Section 1.2 for details). The policy pitch of this reform was clearly stated by its political promoters: "putting a stop to rampant precariousness" and marking a clear discontinuity with respect to previous reforms, which had operated according to "the myth of flexibility and the bugbear of rigidity".[26]

A1.3 Hiring subsidies Despite the period between the introduction of the Jobs Act in 2015 and the signing of the *Dignity Decree* in 2018 was a relatively uneventful one on the side of changes in labor market legislation,[27] the various governments in charge during those years still took a very interventionist stance with respect to the incentivization of permanent employment: four waves of hiring subsidies flooded the Italian labor market, providing monetary benefits to firms hiring workers through permanent employment contracts.[28]

[21] *Law 28 June 2012, n. 92*. Not to be confused with the *Fornero Law* (pension system reform).

[22] The *Fornero Reform* eliminated the automatic reinstatement of a worker in the job in some cases of unlawful dismissal (the judge could opt for monetary compensation).

[23] The *Fornero Reform* introduced the requirement of an explicit justification for the use of temporary employment (except for the first contract) and increased the fiscal contribution for renewals by 1.4%.

[24] The *Jobs Act* introduced the *graded-security* contract, which limits the reinstatement to discriminatory dismissals and fixes a predetermined level for the compensation for unfair dismissals, proportional to job tenure. In September 2018, the Constitutional Court Sentence 194/2018 stroke down this pillar of the Jobs Act by determining that a judge can once again step in to decide the amount of the severance.

[25] The *Poletti Decree* eased entry and renewal processes for temporary positions and removed the requirement of a justification for any temporary employment spell involving up to 36 months or 5 extensions.

[26] Min. of Labor Luigi Di Maio, speech to the Chamber of Deputies, July 30, 2018.

[27] With the only exception of the abolition, in March 2017, of the so-called "vouchers" (*buoni lavoro*), a form of "on call" work relationship akin to the UK's zero-hour contracts.

[28] Untargeted subsidy programs: (i) Total exemption from employer's social security contributions (36 months duration, €8,060 yearly cap); (ii) 40% exemption from employer's social security contributions (24 months duration, €3,250 yearly cap). Targeted subsidy programs: (iii) Firms in the South: total exemption from social security contributions (12 months duration, €8,060 yearly cap); (iv) Under-35 workers: 50% exemption from social security contributions (36 months duration, €3,000 cap).

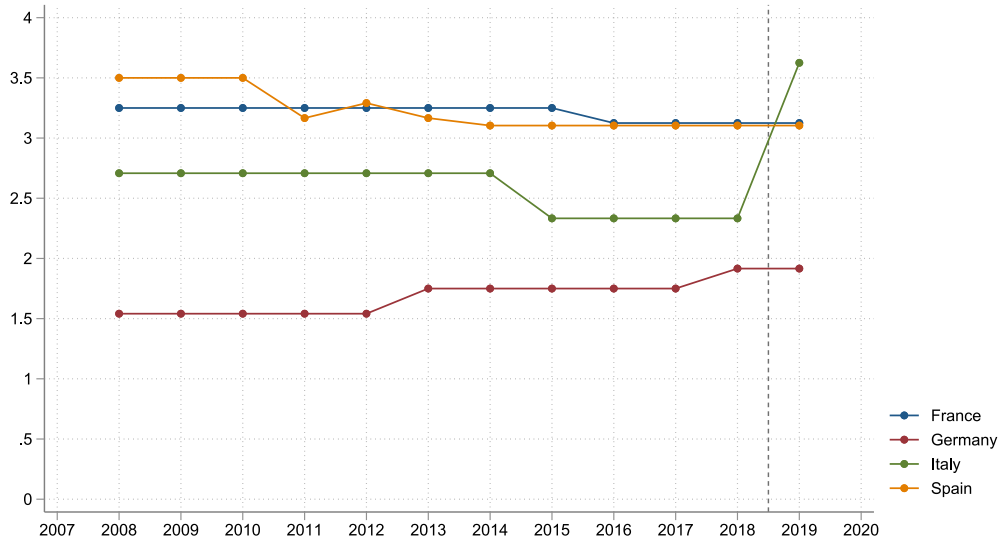
While the first two waves of subsidies — those of 2015 and 2016 — were very large and directed to the entire labor force, the subsequent two waves — starting in 2017 — were much more restricted in size and targeted to specific groups of workers: those working in a firm located in regions of Southern Italy and those under 35 years of age.

To better visualize the whole institutional picture, in Figure A2 we report the timeline of the reforms and interventions undertaken in the Italian labor market starting from 2015.

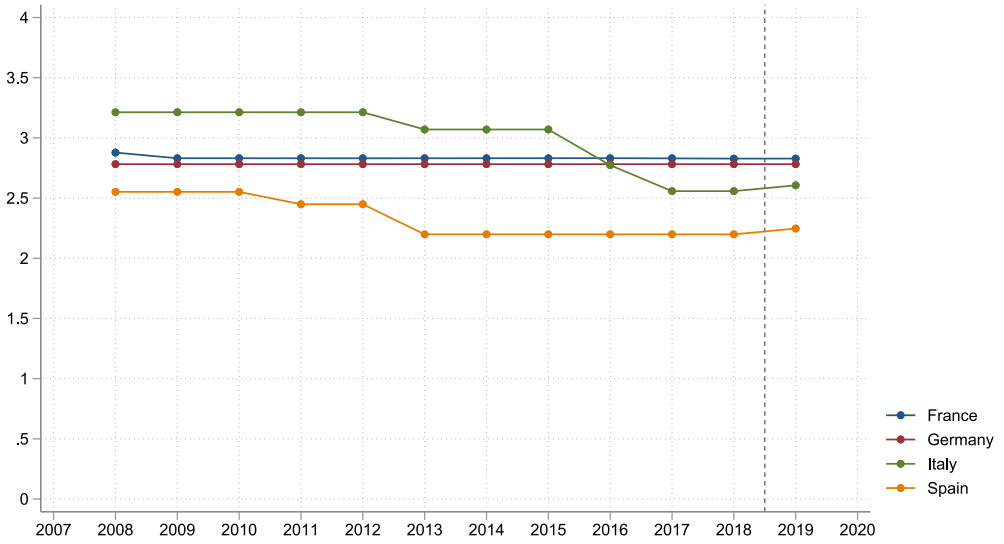
A2. Additional Figures

FIGURE A1. Strictness of EPL in Europe

A. Restrictions on temporary contracts

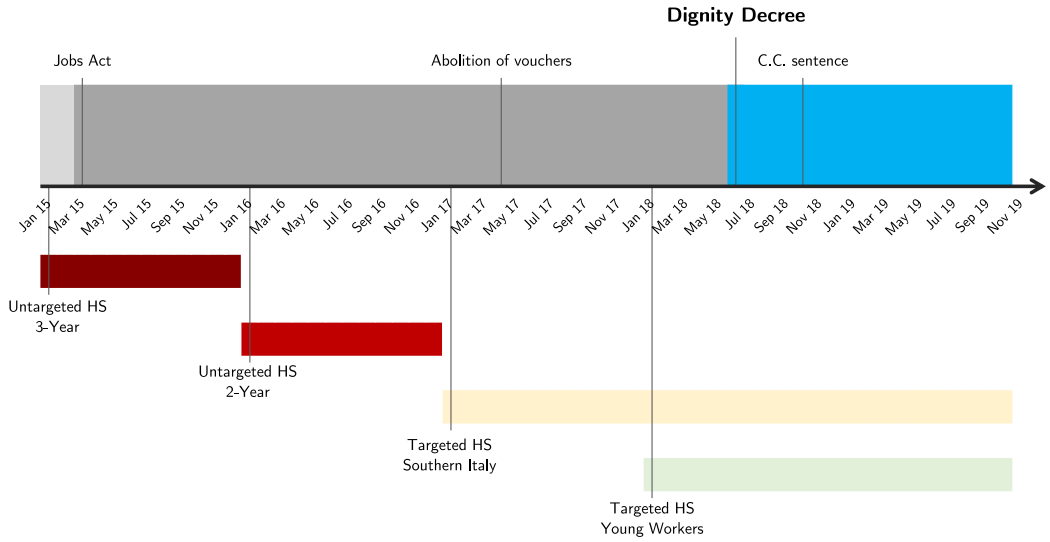


B. Protection for permanent contracts



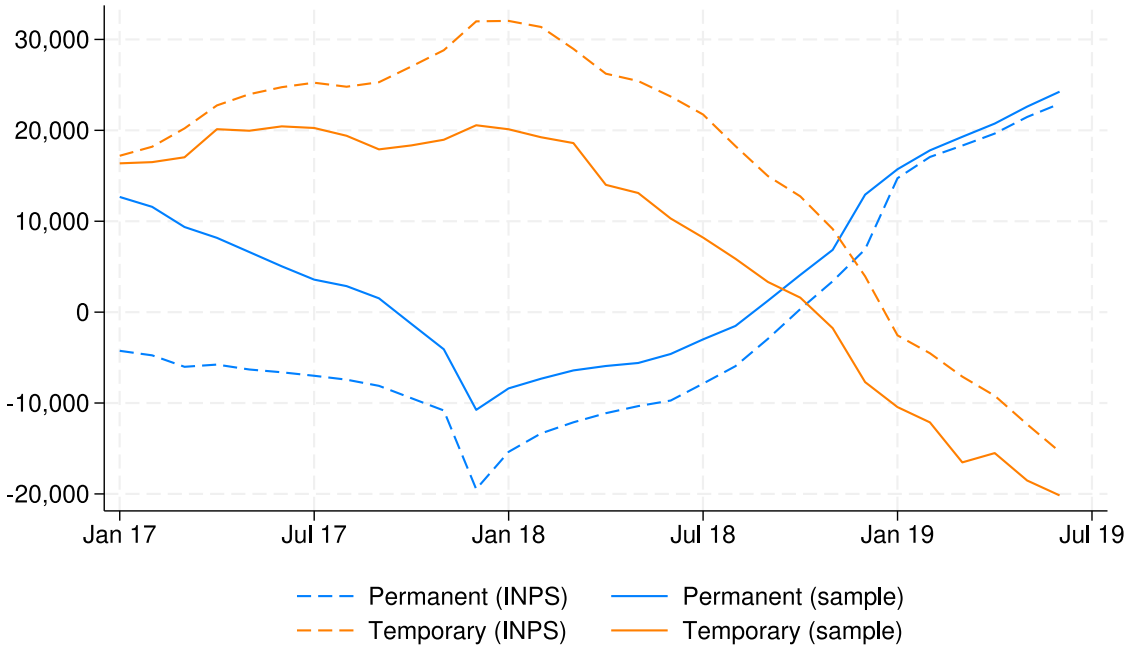
Notes: Strictness of the Employment Protection Legislation regarding permanent and temporary jobs for France, Germany, Italy and Spain. The indicator is a synthetic measure of the level of protection against dismissals for permanent contracts and of the level of regulation for the use of temporary contracts.
Source: OECD.

FIGURE A2. Reforms and interventions in the Italian labor market



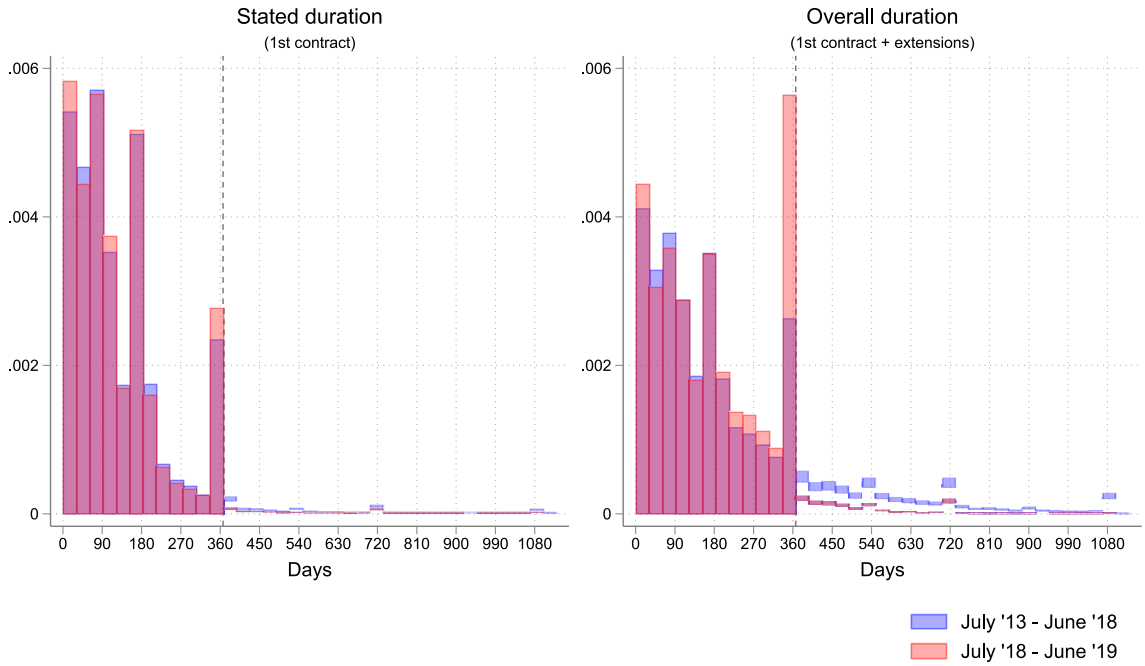
Notes: Timeline of reforms and interventions in the Italian labor market (2015-2019). Structural reforms are reported on top of the timeline while hiring subsidies are at the bottom. For subsidies, filled bars indicate periods in which it is possible to gain entitlement to the subsidy, and pattern-filled bars indicate the benefit duration.

FIGURE A3. Net Labor Flows by Contract Type: Universe (INPS) vs. Sample



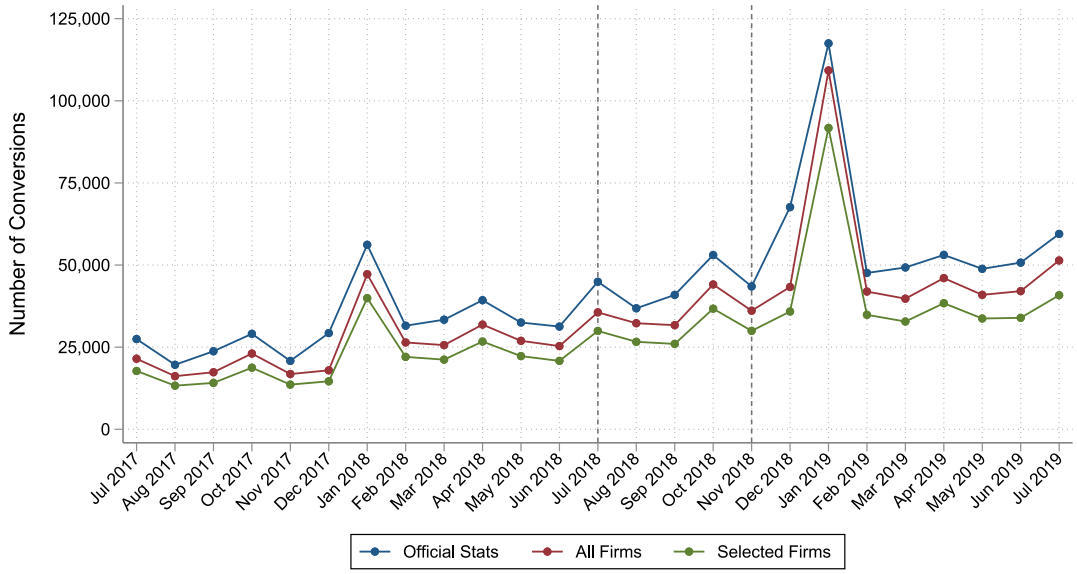
Notes: The graph shows total monthly net labor market flows for permanent and fixed-term contracts, comparing official INPS aggregate data (*Osservatorio sul mercato del lavoro*) and our sample. For permanent contracts, the net flow is (hiring + conversions - terminations); for fixed-term contracts the net flow is (hiring - conversions - terminations). The construction of the sample is described in Section 2.
Source: SISCO (see Section 2.1) and *Osservatorio sul mercato del lavoro (INPS)*.

FIGURE A4. Duration Distribution for Fixed-Term Contracts



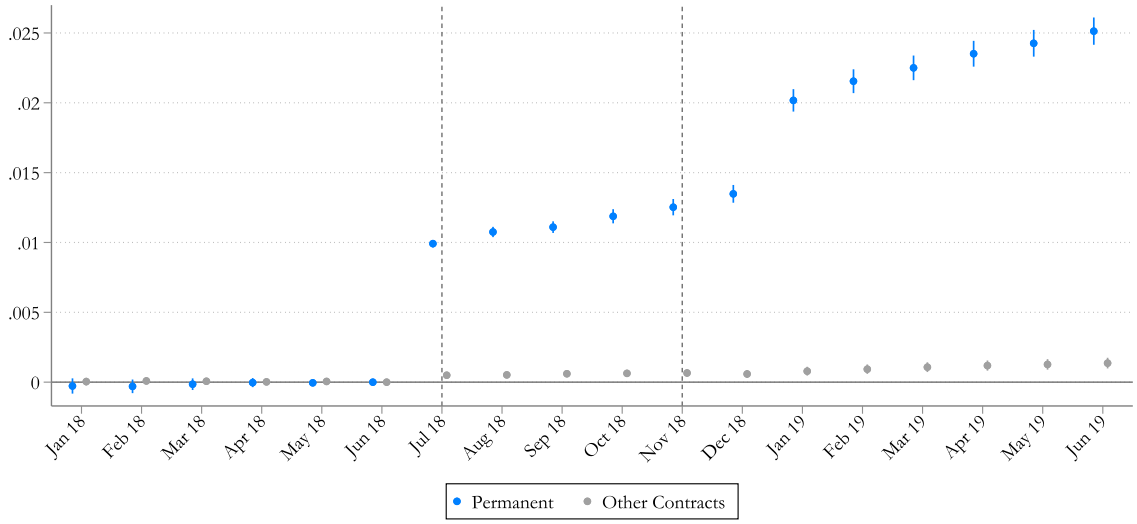
Notes: The figure shows duration distributions for fixed-term contracts signed before the reform (July 2013 to June 2018, in blue) and after the reform (July 2018 to June 2019, in red). The left panel reports the stated duration, the duration that is written on the contract when it is first signed. The right panel shows the overall duration, the duration of the entire temporary employment spell, including contract extensions. The sample is restricted to the first contract between a worker and a firm (i.e. contract renewals after a non employment period are excluded).
Source: Mercurio, administrative data covering labor flows for all workers in the Veneto region up to 2021.

FIGURE A5. Monthly Number of Conversions of Fixed-Term to Permanent Contracts



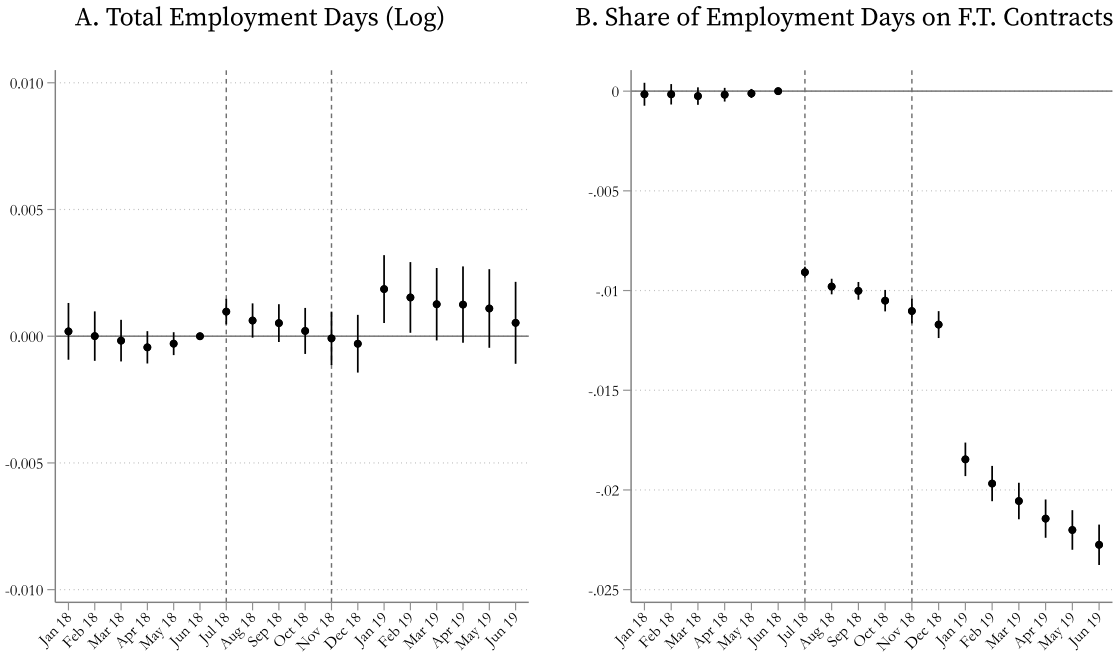
Notes: The figure shows changes in the monthly number of contract conversions over time. The blue line comes from official INPS report data. The red line shows data from our first sample of firms. The green line shows data from a smaller group of companies that had at least one conversion before and after the *Dignity Decree* was introduced. The first vertical line indicates the month when the government introduced the reform, and the second line indicates the date of its full enactment after parliamentary approval. *Source*: INPS (see Section 2.2) and *Osservatorio sul mercato del lavoro (INPS)*.

FIGURE A6. Share of Employment Days



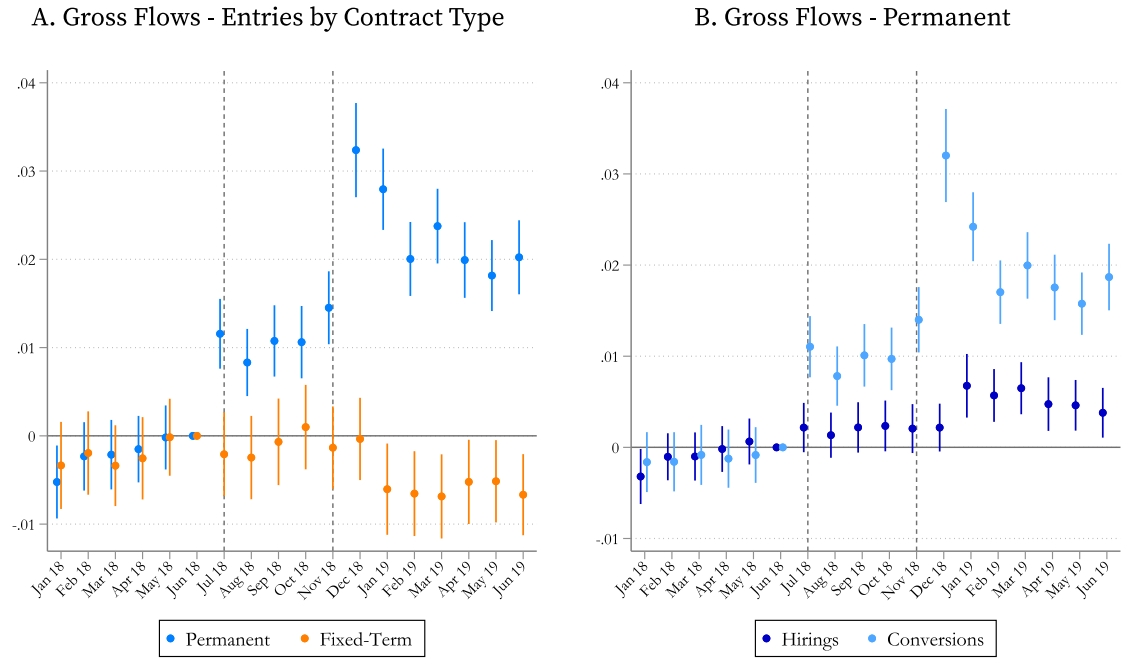
Notes: The graph reports coefficient estimates and 95% confidence intervals for $\{\beta_k\}_{k \neq \text{June 2018}}$ from Equation (2), showing the relationship between firm exposure intensity ρ and the monthly share of employment days on permanent and other contract types. Other contract types include (i) apprenticeship, (ii) consultancy (so-called *parasubordinato*), (iii) intermittent worker, (iv) internship (so-called *tirocinio*) and (v) seasonal worker. All outcomes are first residualized with respect to calendar-month specific exposure effects following Equation 3. Standard errors are clustered at the firm level. The first vertical line indicates the month when the government introduced the reform, and the second line indicates the date of its full enactment after parliamentary approval.
Source: SISCO, see Section 2.1.

FIGURE A7. Employment Stocks - Restricted Control Group (Firms with $\pi_i = 0$)



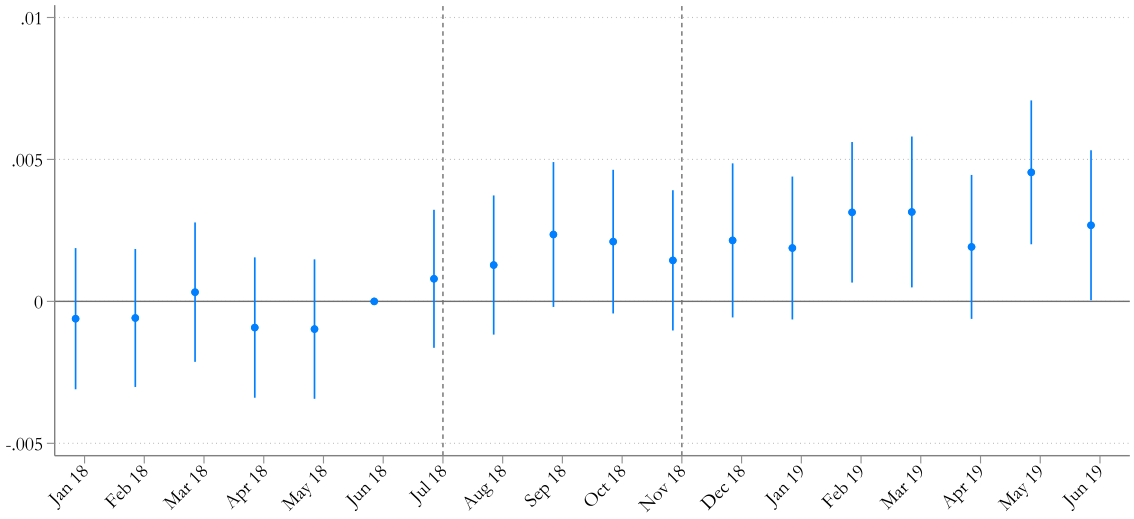
Notes: The graph reports estimates analogous to those shown in Figure 2, obtained by fitting Equation (2) excluding firms with short-duration contracts only between 2013 and 2017 ($\pi_i = 0$).
Source: SISCO, see Section 2.1.

FIGURE A8. Difference in Hiring Patterns - Restricted Control Group (Firms with $\pi_i = 0$)



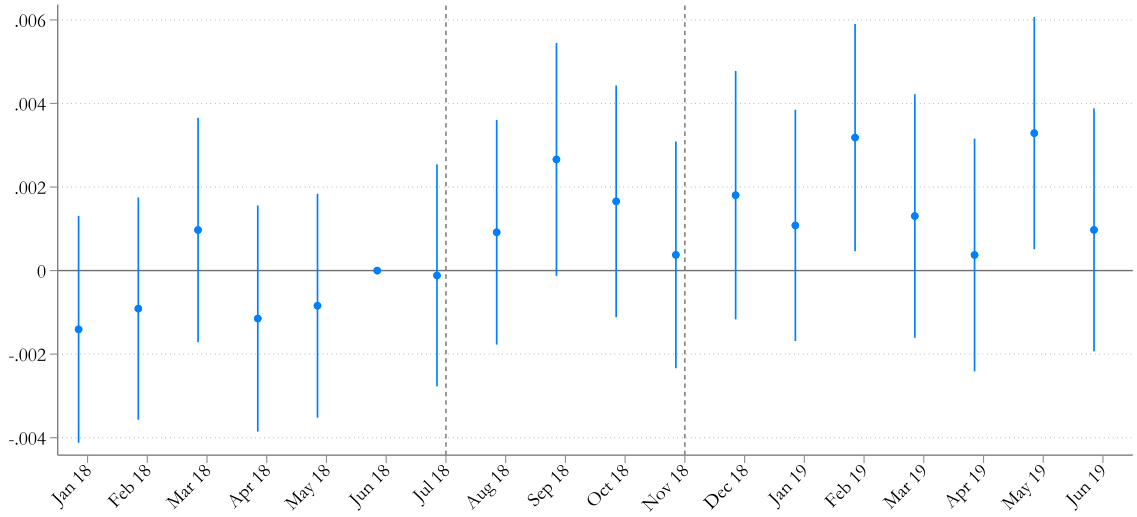
Notes: The graph reports estimates analogous to those shown in Figure 3, obtained by fitting Equation (2) using a more restrictive control group that consists only of firms with no temporary employment at all between 2013 and 2017 ($\pi_i = 0$).
Source: SISCO, see Section 2.1.

FIGURE A9. Permanent Jobs Terminations



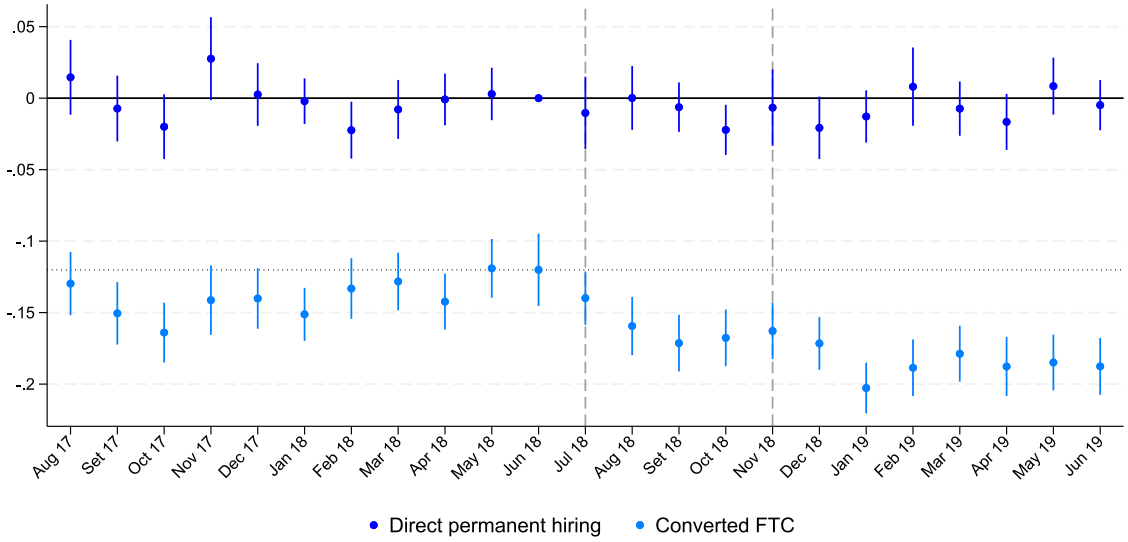
Notes: The graph reports coefficient estimates and 95% confidence intervals for $\{\beta_k\}_{k \neq \text{June 2018}}$ from Equation (2), showing the relationship between firm exposure intensity ρ and permanent job terminations. The outcome is first transformed using the inverse hyperbolic sine (IHS) transformation and then residualized with respect to calendar-month specific exposure effects following Equation (3). Standard errors are clustered at the firm level. The first vertical line indicates the month when the government introduced the reform, and the second line indicates the date of its full enactment after parliamentary approval.
Source: SISCO, see Section 2.1.

FIGURE A10. Permanent Jobs Terminations - Restricted Control Group (Firms with $\pi_i = 0$)



Notes: The graph reports estimates analogous to those shown in Figure A9, obtained by fitting Equation (2) using a more restrictive control group that consists only of firms with no temporary employment at all between 2013 and 2017 ($\pi_i = 0$).
Source: SISCO, see Section 2.1.

FIGURE A11. Log Daily Wages: Converted vs. Direct Permanent Hires



Notes: The figure displays estimates of β_t from the second specification of Equation (4) – which includes a firm fixed-effect and individual controls – showing changes in log-daily wages for new permanent contract workers, relative to June 2018. The estimates in dark blue display represent direct permanent contract hires; the estimates in light blue represent fixed-term contract conversions. Standard errors are clustered at the firm level. The first vertical line indicates the month when the government introduced the reform, while the second one the date of its full enactment after parliamentary approval. The additional horizontal line represents the value for the coefficient on converted workers in June 2018.
Source: INPS, see Section 2.2.

A3. Additional Tables

TABLE A1. Pre-reform Outcomes by Firm Exposure Category

		Non-exposed ($\pi_i = 0$) ($\delta_i = 0$)	**Partially Exposed** ($\pi_i > 0$) ($\delta_i = 0$)	**Exposed** ($\pi_i > 0$) ($\delta_i > 0$)
N. of firms (%)		10,180 (8.54%)	35,968 (30.17%)	73,057 (61.29%)
Flows				
Outcome	**Contract Type**			
Hirings	Permanent	0.066 (0.825)	0.127 (0.723)	0.321 (2.750)
Hirings	Fixed-term	0.022 (0.418)	0.461 (9.452)	1.789 (21.582)
Conversions	F.T. $\rightarrow$ P.	0.001 (0.027)	0.030 (0.243)	0.178 (1.301)
Hirings	Other contracts	0.228 (10.615)	0.278 (6.311)	0.566 (11.512)
Terminations	Permanent	0.075 (0.550)	0.141 (1.348)	0.361 (3.659)
Terminations	Fixed-term	0.006 (0.186)	0.352 (9.326)	1.486 (21.190)
Stocks				
Outcome	**Contract Type**			
Empl. Days	Permanent	361.3 (922.1)	388.5 (861.4)	1004.2 (5620.3)
Empl. Days	Fixed-term	1.6 (19.3)	29.7 (136.8)	246.8 (1343.6)
Empl. Days	Other contracts	49.4 (777.0)	59.2 (456.1)	120.9 (1286.5)
Empl. Days	Total	412.2 (1244.4)	477.4 (1006.9)	1371.8 (7002.5)

Notes: This table presents monthly averages and standard deviations (in parentheses) for several employment indicators measured from January 2018 to June 2018. Firms are categorized into three groups based on their exposure to the reform. Employment flows are measured as the average monthly number of events (hirings, conversions, or terminations) per firm, while stocks are measured as average monthly employment days.
Source: SISCO, see Section 2.1.

TABLE A2. Summary Statistics

	Pre-reform	Post-reform
Log-Daily Wage At Conversion	3.634	3.555
	(1.058)	(1.088)
Log-Daily Wage Before Conversion	3.556	3.491
	(1.070)	(1.088)
Months of Tenure	13.847	14.634
	(11.486)	(10.080)
Years of Experience	14.394	14.694
	(11.107)	(11.409)
Age	36.615	37.101
	(10.043)	(10.384)
Share of Females	0.361	0.379
	(0.480)	(0.485)
Share with University Degree	0.140	0.126
	(0.347)	(0.332)

Notes: The table shows the average value (or share) of several individual characteristics for workers who experienced a transition from a temporary to a permanent contract within the same employer before and after the reform, measured at the time of conversion. Standard deviations are reported in parentheses.
Source: INPS, see Section 2.2.