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Alessandra Casarico
Irene Ferrari
Caterina Pavese

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Rebalancing power asymmetries within firms: Evidence from illegal resignations

Alessandra Casarico
(Bocconi University and Dondena Research Center, Milan and CESIfo, Munich)

Irene Ferrari
(Marche Polytechnic University, Ancona and NETSPAR)

Caterina Pavese
(Ifo Institute, University of Munich and CESIfo, Munich)

Rebalancing power asymmetries within firms: Evidence from illegal resignations*

Alessandra Casarico† Irene Ferrari‡ Caterina Pavese§

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Abstract

We document the extent of employer abuse of power and characterise the employers most likely to engage in abusive relationships with their employees. We leverage an Italian reform that changed the process for submitting voluntary resignations from a paper-based system to an online one. This reform aimed to curb the illegal practice of requiring workers to sign undated resignation letters, which employers could later use at their discretion to avoid the costs associated with dismissals - a clear manifestation of power abuse. Using difference-in-differences estimation, we document that resignations declined more in firms with higher shares of vulnerable workers, those operating in weaker local labour markets, and those with lower productivity. Both firms and workers adjusted their behaviour in response to the reform. Firms reduced overall hiring, with no evidence of differential effects by worker demographics, suggesting that cost-saving motives rather than taste-based discrimination drove their response. At the worker-level, we observe a decrease in workplace injuries, indicating that the reform strengthened workers' bargaining power and improved overall workplace safety.

Keywords: Resignations; Power asymmetries; Employer power abuse; Informality; Public policy.

JEL Classification: J18; J46; J81; J83.

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†Bocconi University and Dondena Research Center, Milan and CESIfo, Munich. Email: alessandra.casarico@unibocconi.it

‡Marche Polytechnic University, Ancona and NETSPAR. Email: i.ferrari@univpm.it

§Ifo Institute, University of Munich and CESIfo, Munich. Email: pavese@ifo.de

Riequilibrare le asimmetrie di potere all'interno delle imprese: evidenza dalle dimissioni illegali

Alessandra Casarico[1] **Irene Ferrari**[2] **Caterina Pavese**[3]

[1] Bocconi University e Dondena Research Center, Milano; CESIfo, Monaco

[2] Università Politecnica delle Marche, Ancona; NETSPAR

[3] Ifo Institute, Università di Monaco; CESIfo, Monaco

Sommario

In questo studio documentiamo quanto siano diffuse le pratiche di abuso di potere da parte dei datori di lavoro e caratterizziamo i datori di lavoro più inclini ad instaurare rapporti abusivi con i propri dipendenti. Ci avvaliamo di una riforma italiana che ha modificato il processo di presentazione delle dimissioni volontarie, passando da un sistema cartaceo a uno online. Tale riforma mirava a frenare la pratica illegale di richiedere ai lavoratori di firmare lettere di dimissioni senza data, che i datori di lavoro potevano successivamente utilizzare a loro discrezione per evitare i costi associati ai licenziamenti: una chiara manifestazione di abuso di potere. Utilizzando una stima della differenza nelle differenze, mostriamo che le dimissioni sono diminuite maggiormente nelle aziende con una percentuale più elevata di lavoratori vulnerabili, in quelle che operano in mercati del lavoro locali più deboli e in quelle con una produttività inferiore. Sia le imprese che i lavoratori hanno modificato il loro comportamento in risposta alla riforma. Le imprese hanno ridotto le assunzioni complessive, senza che vi fossero prove di effetti differenziali in base alle caratteristiche demografiche dei lavoratori, il che suggerisce che la loro risposta è stata motivata da ragioni di risparmio sui costi piuttosto che da una discriminazione basata sulle preferenze. A livello dei lavoratori, osserviamo una diminuzione degli infortuni sul lavoro, il che indica che la riforma ha rafforzato il potere contrattuale dei lavoratori e migliorato la sicurezza complessiva sul posto di lavoro.

Parole chiave: Dimissioni; Asimmetrie di potere; Abuso di potere del datore di lavoro; Informalità; Politiche pubbliche.

1 Introduction

Globally, more than one in five workers report having experienced at least one form of violence and harassment at work during their working lives (ILO, International Labour Organization , 2022). One of the most widespread sources of violence and harassment at work stems from power abuse vertically perpetrated by the employer. Power abuse encompasses a spectrum of harmful and coercive behaviours, including economic and psychological abuse. Abuse of power by employers can have significant negative economic consequences for both individuals and firms, ranging from reduced productivity to lower well being of workers. Despite its pervasiveness, the economic literature has only just begun to explore the effects of power abuse on workers and firms, and even less is known about the effects of regulations that policymakers have put in place to curb the phenomenon.

In this paper we document the extent of abuse of power by employers and characterise those employers who are more likely to entertain an abusive relationship with their employees, taking advantage of a policy designed to combat power abuse in the labour market. We then explore the reasons and mechanisms behind abusive relationships with employees by investigating how some relevant labour market outcomes for workers and firms change after the introduction of the policy.

Measuring abuse of power and causally identifying its direct and indirect impact, as well as that of regulations that address it, is challenging. The first challenge involves severe data limitations. Datasets reporting working conditions along these dimensions are generally limited to self-reported surveys. More broadly, abuse of power often involves illegal practices that are inherently difficult to observe or are systematically underreported (Dahl and Knepper, 2021). A second challenge is identification: toxic firms and their workers might have worse outcomes even in the absence of employers' abuse of power. This paper overcomes these challenges using a unique policy setting in Italy and matched employer-employee data, allowing us to characterise employers prone to power abuse and the groups of workers most vulnerable to such practices by analyzing firm- and worker-level responses to the introduction of a regulation aimed at promoting formality in employment relationships.

Our analysis leverages a reform enacted in Italy in 2007 (Law No. 188 of 17 October 2007), which primarily aimed to revise the administrative procedure for workers to resign from their jobs. The reform was designed to combat the illegal practice of "dimissioni in bianco" (literally, blank resignations), an extreme manifestation of employers' abuse of power. This practice involves forcing employees to sign an undated resignation letter alongside their employment contract. Employers can use this letter at a later stage to bypass legal restrictions regarding individual redundancies, effectively converting terminations into resignations. Through this practice, employers can, for instance, avoid the costs associated with workers' safety, maternity leave and employees' screening, as well as gain greater flexibility in dealing with economic fluctuations, such as unexpected shifts in demand. While the narratives at the time of the reform were that the phenomenon was rare in the public sector, the evidence available, which provided the background for the reform, suggested that small and medium-sized enterprises in the private sector were likely to have made use of illegal resignations to dismiss workers.

The 2007 reform introduced new ex-ante and ex-post requirements for workers to quit voluntarily: rather than handing in a resignation letter on paper to the employer, after the reform resignations have to be submitted in writing using a mandatory online form only valid for 15 days after the date of issue and they have to be approved by the provincial labour inspectorate. These requirements have been unanimously recognised as challenging for the employer to circumvent.

Our identification strategy leverages a key feature of the Italian employment protection legislation: firms with more than 15 employees face higher firing costs compared to firms with fewer than 15 employees. According to Law No. 300/1970, the latter are exempted from the obligation to provide legitimate reasons for dismissal (such as serious fault and serious misconduct). For firms which do not comply with the ban on discriminatory dismissals, there is a fine and they are forced to pay severance pay worth up to 2.5-6 months of gross earnings. Under no circumstances they can be forced to reinstate the dismissed worker. As the exemption does not apply to firms with more than 15 employees, firing costs are higher for them, making it more likely that these firms will have greater incentives than small firms to resort to illegal resignations to force employees to leave as an alternative to dismissals.

Using employer-employee register monthly data from the Italian National Institute of Social Security (INPS), we exploit the different firing costs faced by firms of different size in a difference-in-differences estimation design comparing outcomes of firms with more than 15 employees (i.e., the group more exposed to the reform, which we label *treated* group) to firms with less than 15 employees (i.e., the group less exposed to the reform, which we label *control* group), before and after the 2007 reform and investigate firm-level outcomes. In our preferred specification, we include firm linear trends to account for potential different trends in the dynamics of voluntary resignations in larger and smaller firms. Moreover, we analyze the dynamics of the effect in an event-study framework. This will allow us to understand whether the effect is persistent over time and at the same time to transparently test the parallel trend assumption underlying the difference-in-differences strategy.

Our analysis provides two main sets of results. First, we show that the reform leads to a significant firm-level reduction in voluntary resignations, our main variable of interest. Since illegal resignations cannot be directly observed, we argue that the reform's causal effect on voluntary resignations serves as an indicator of their prevalence in treated firms relative to control firms.

The size of the effect significantly differs by pre-reform characteristics of workers, type of contracts, sectors and labour markets. We document a stronger decrease of resignations in firms characterised by (i) low productivity; (ii) higher share of vulnerable workers: women, migrant workers and those below 40 years of age; (iii) lower share of workers with full-time contracts and with long experience; (iv) presence in low value-added sectors, such as hospitality and retail, and (v) presence in labour markets with low employment rates, a limited number of large firms and high level of informality.

These findings indicate that the workers most vulnerable to employer exploitation are also among the most disadvantaged in the labour market. With limited job opportunities and lower credentials, they often have little alternative but to accept informal employment. These workers might be willing to sign blank resignation letters because, in this context, even such employment relationships may be perceived as an improvement over informal work.

Second, after identifying the contexts where power-abusive relationships are more likely to occur, we explore whether firms and workers adjust to the reform on different margins besides voluntary resignations. At the firm-level, our findings suggest that, in response to the reform, treated firms reduced hiring, with the decline concentrated among workers with open-ended contracts. This supports the interpretation that illegal resignations were used by employers primarily as a cost-saving strategy rather than being motivated by taste-based discrimination, as we find no evidence of differential hiring patterns across demographic groups or worker credentials after the reform's implementation. Since blank resignations were primarily used by firms with low productivity, operating in stagnant labour markets, the reaction that we document on the firm side is in line with blank resignations serving as a means to circumvent normal competitive pressures.

We then turn to the effect of the reform on worker-level outcomes, focusing on the number of injuries. The frequency of workplace injuries serves as an important indicator of overall workplace quality, as well as of workforce training and upskilling. Following the reform, workers may have become more inclined to avoid dangerous situations that pose a heightened risk of serious injury, because they perceive a greater stability in the job relation and are less subject to the employer threat of being fired. At the same time, firms may no longer bypass costs related to worker safety and invest more in it. Consistent with these hypotheses, we find a negative and significant reduction in the number of injuries at the firm-level. This evidence suggests that workers' bargaining power and access to rights may have improved, in line with the goal of the reform.

To support our analysis we provide a set of robustness checks corroborating the validity of our design and results. In particular, we show that results are robust to: (i) the inclusion of controls for firm size (measured in $(t-1)$) and group linear trends; (ii) removing observations close to the cut-off, to address potential sorting of firms; (iii) alternative clustering; (iv) alternative definitions of the outcome variable; (v) alternative firm size bounds used to define treated and control groups. Moreover, we show that we obtain estimates centered around zero when randomizing the treatment assignment and statistically insignificant coefficients when conducting placebo regressions assigning "fake" treatment dates. Finally, the reform may have led to compositional changes among both firms and workers. To address this, we show that firms do not sort strategically below or above the 15-employee threshold. We further show that the reform does not affect the likelihood of survival differently between treated and control firms, nor does it prompt firms to adjust on the layoff margin.

Our paper contributes to the small but growing literature on power imbalances in the labour market that documents the consequences of power asymmetries within firms on female workers (Dahl and Knepper, 2021; Folke and Rickne, 2022; Adams et al., 2024; Adams-Prassl et al., 2024). We complement it with causal evidence that provides insights into other groups of vulnerable workers, such as migrants and young workers, and characterise the firm-level aggregate responses to a regulation aimed at redressing power asymmetries within the firm.

Our findings also contribute to the literature investigating the relationship between informality and flexibility in rigid labour markets (e.g., Ulyssea 2020, Di Porto et al. 2022, and Schneider and Enste 2000). Tighter regulations favour incumbent workers in terms of access to rights, but may change the composition of the labour force, reducing the probability to be hired of those workers with lower labour market credentials.

Finally, our paper speaks to the extensive literature analysing firms' responses to changes in firing costs (see for example Sestito and Viviano 2018, Kugler and Pica 2008, and Boeri and Jimeno 2005). Differently from standard workers' regulations like employment protection legislation or minimum wage, the change in regulation we analyse removes potential instances of illegal working relationships and may involve different margins of adjustment on the firm side.

Overall, by assessing the causal effect of the reform, the paper provides a novel characterisation of power abusing employers and a better understanding of the firm- and worker-level response to a redressing of power asymmetries within firms. Such asymmetries impose substantial costs on workers and societies. Our findings suggest that regulations fostering formality can substantially reduce these costs.

The remainder of this paper proceeds as follows. In Section 2 we describe the institutional context and the reform. Section 3 introduces the data and sample restrictions. Section 4 outlines our identification strategy. In Section 5 we present our main empirical results and in Section 6 we examine firms' and workers' response to the reform introduction. Section 7 concludes.

2 Institutional context and the reform

In this section we describe employment protection in Italy and the 2007 reform introducing online resignations.

2.1 Key institutional features of the Italian labour market

According to the OECD, in the 2000s Italy was characterised by rigid employment protection legislation (EPL), much stricter than the United States and comparable to countries such as Germany (see Figura A1). The Italian labour market institutions are mostly derived from the *Statuto dei Lavoratori* (Workers' Statute, Law No. 300, 20 May 1970), that introduced the concept of reinstatement in cases of unfair dismissal. This is one of the primary and longstanding forms of protection granted to employees with an open-ended contract, applying to firms exceeding a specific size threshold, namely 15 employees.[1] Reinstatement entailed the employer's obligation to reinstate the employee in the same position as before the unfair dismissal, in addition to compensation of at least five months' pay. The reinstatement clause, despite being rarely used, posed a significant deterrent to hiring a worker with a permanent contract, also due to the substantial costs associated with the potential risks of prolonged trial periods that strenghtened the EPL stringency in firms above the 15-employee threshold (Gianfreda and Vallanti, 2017). In firms with fewer than 15 employees, the employee unfairly dismissed had no right to reinstatement but was entitled to severance payments ranging from 2.5 to 6 months' pay.

With regard to temporary workers, the legislation in force since 1987 (Law no. 56, 28 February 1987) provided that, in the event of unfair dismissal, workers were entitled to compensation covering lost wages from the date of dismissal until the scheduled end of the temporary contract. However, unlike workers with open-ended contracts, temporary workers were not entitled to job reinstatement. Additionally, there was no variation in firing costs for unfair dismissal of temporary workers based on firm size. All in all, the law prescribed a significantly higher cost of unfair dismissal for firms exceeding the 15-employee threshold, as such firms are required to both reinstate dismissed workers and compensate them for lost wages (Article 18 of the Workers' Statute).

2.2 Resignations

Under Italian legislation (Articles 2112, 2118, and 2119 of the Civil Code), workers with open-ended contracts are entitled to resign freely, provided they adhere to the notice

[1] The legislation was changed in 2012 (Fornero Law), well after the period of interest for our analysis.

period specified in their collective agreements, which typically amounts to one month. Resignations must be a voluntary act by the worker; any impairment of the employee's free will renders the resignation null and void. In contrast, fixed-term employment contracts cannot be terminated before their natural expiration. Workers on fixed-term contracts are permitted to resign early only if there is just cause.

2.3 The reform

In October 2007 the Italian Parliament approved Law No. 188/2007, which established that resignations must be submitted in writing using a mandatory online form, marked with a progressive identification code, issued by the Ministry of labour and valid only for 15 days after the date of issue.[2] Prior to the reform, a letter of resignation (on paper) had to be handed in to the employer following a default form. The choice to move to an online procedure and to include a progressive code in the online form was meant to limit the practice of illegal resignations. Under this practice, employees are required to sign an undated resignation letter together with the employment contract, which can be filled in at the employer's discretion. Although this illegal practice is obviously difficult to observe and even harder to measure, the debate surrounding the drafting of the law, which was passed in 2007, focused on its use in the private sector as a means of dismissing workers, particularly pregnant women. While the debate has primarily focused on the use of illegal resignations in cases of pregnancy, this practice may not be exclusively a gender-related issue. Firms might employ illegal resignations as a broader strategy to reduce labour costs. For example, they could seek to minimise expenditures not only on maternity or parental leave, but also try to bypass costs related to worker safety, including expenses for training programs and safety courses. Additionally, firms might opt to save resources by forgoing thorough pre-hiring screening, choosing instead to observe workers on the job and dismiss them in case of a bad match. Illegal resignations might also serve as a low-cost mechanism for firms to adjust their workforce in response to fluctuations in demand.

The sparse data available (for instance, Istat 2011), indicate that in 2009, more than 800,000 women had been forced to resign after they informed their employer of their pregnancy, which corresponds to 8.7 percent of working mothers and mothers who

[2]See Figura A3 for an example of the online resignation procedure.

had worked in the past. The data attached to the bill proposal no. 1538, later enacted as law no. 188/2007, provided a similar picture. According to the data provided by the dispute office of CGIL - the largest Italian union - around the time of the reform approximately 1,800 women were seeking legal assistance each year for extortion through illegal resignations. Only a few dozen cases translated into (written or testimonial) evidence key to make the termination of employment invalid.[3]

It is worth highlighting that workers —whether employed in firms with fewer than or more than 15 employees— had no incentives to sign contracts containing blank resignations. For instance, workers gain no additional flexibility or rights from signing such clauses, since workers can always voluntarily resign. The foreseeable benefits of blank resignations appear to go entirely to firms. We further explore firms' incentives to use blank resignations in section 6.

The Law was passed by the Parliament on October 17, 2007; it was published in the Official Gazette on November 8, 2007, and came into effect on November 23, 2007 - fifteen days after its publication. The law was in place for a short period of time, until the end of June 2008, when it was abolished by the new right-wing government (Legislative decree 112/2008), that reinstated the previous administrative process to resign (i.e., resignations on paper).

While the law was in place, failure to comply with the new procedure or using an out of date form implied that the resignation was null and void. The law applied to all subordinate employment relationships, both in the private and public sectors, with any type and duration of contract.

Two other legislative provisions were enforced in 2012 (Law 92/2012 - Fornero law) and in 2015 (Law 23/2015 - Jobs Act) with the motivation of fighting illegal resignations. Differently from Law 188/2007, these provisions were part of a package of other structural labour market reforms, limiting the possibility to single out the impact of the change regarding workers' resignations.

2.4 International evidence on the use of illegal dismissals

One might wonder whether the phenomenon we study and the results we find may extend to other contexts beyond Italy. In their analysis of EU countries, Masselot et al.

[3]The widespread use of blank resignations was also extensively covered by the national press (see for instance Maria Novella De Luca in "La Repubblica", 20th January 2012).

(2012) report that the blank resignation phenomenon has been common practice also in Croatia, Greece and Portugal. Illegal practices that pressure workers - mostly pregnant women - to resign have been largely used also in Romania, Spain and Lithuania. The debate on the practice of blank resignations has also recently flared up in France, when a journalistic investigation revealed in September 2023 how one of the largest chains in large-scale retail distribution in the country was using this tool to hire workers on permanent contracts, thus avoiding the heavier taxes associated with fixed-term contracts.[4] Overall, our paper provides evidence on a uniquely designed policy targeting an elusive illegal practice that is widespread across multiple countries.

Importantly, our results can also be plausibly extended to other types of power abuse by firms and inform about their effects on firm and worker outcomes. According to the World Risk Poll administered by the Lloyd's Foundation (Lloyd's Register Foundation, 2021), Italy is a fairly representative context in terms of the reported violence and harassment experienced at work both by women and men (see Figura A2). In Italy, about 25% (28%) of male (female) workers have experienced at least a form of violence and harassment compared to the 22% (20%) experienced by workers globally. Italy is a good benchmark also when looking at specific forms of harassment such as psychological violence at work : 43% (61%) of female (male) workers were reported to have been exposed to psychological abuse compared to 47% (49%) of a representative sample of global workers.

3 Data and Sample Restrictions

In this section we describe our data sources and the sample restrictions made to obtain our working sample.

3.1 Data

Social Security Data Our analysis mainly draws on data from the Italian National Institute of Social Security (INPS, Istituto Nazionale della Previdenza Sociale), a linked employer-employee dataset covering the universe of formal workers in the non-agricultural private sector. We use monthly data for the period 2006-2008.

The dataset provides comprehensive information on the main employment relation-

[4] Additional details about the scandal are available at the following link Lettre de demission anticipee.

ships of workers, including detailed data on the start and end dates of each job, job location, contract type (full-time or part-time, permanent or temporary), occupation (apprentice, blue-collar, white-collar, middle managers, executive), injuries and maternal and parental leaves, demographic information of workers (year of birth, gender, citizenship), and their work history. Additionally, it offers detailed information on firm characteristics such as the number of employees, composition of the workforce, and industry sector. Unique firms identifiers allow to match workers to the firm they work in and track firms over time. The monthly data allow us to study firms' dynamics around the reform date.

Crucially for our purpose, we observe the reason behind workers' separations. These include dismissal for just cause, end of contract, other reasons related to firm restructuring, suspension, worker's death, and voluntary resignations. On average, voluntary resignations represent 40% of the total number of separations.

We establish the 15-employee threshold, which will be key in our identification strategy, using the INPS constructed variable "firm labour force" (*forza aziendale*), defined as the firm's full-time equivalent workforce. As clarified by Boeri and Garibaldi (2019), this metric serves as the most reliable proxy for the labour court's criteria in assessing firm size concerning the 15-employee threshold pertinent for the application of Employment Protection Legislation (art. 18 of Law 300/1970). We define new hires based on the start date of the employment contract, and separations based on the end date of the employment contract.

Black economy data Our main result may conceal significant heterogeneity at the local labour market level. To explore this, we conduct an initial heterogeneity analysis focusing on the geographic prevalence of informality. Specifically, we use data from the Italian Statistical Office (*Istat*) on the informality rate among employed individuals (per 100 employed) at the regional-sector level for 2006. This measure is aggregated at the one-digit sectoral level across regions, resulting in a total of 80 cells.

Census data We will also leverage data from the 2011 Census to construct employment rates at the municipal level. Given that the availability of outside options may play a critical role in shaping workers' decisions, we will examine whether our results vary with the local employment rate.

Balance sheet data In our analysis, we additionally exploit information from the

CERVED dataset reporting information on firms' balance sheets. All pieces of information available are reported yearly, limiting their usage in our monthly setting. For instance, we cannot investigate the impact of the reform on firms' productivity. However, we take advantage of the information on firms' pre-determined value added to characterise them in terms of productivity in a heterogeneity analysis.

3.2 Sample restrictions

In our analyses, we will focus on firms that are active in 2006. Furthermore, we keep only firms that are active for at least one quarter each year (which leads to dropping less than 0.2% of observations) and that employ at least one worker on an open-ended contract. We drop employees younger than 19 and older than 65. We further clean the dataset by dropping employees with zero registered paid days for at least six months in a year (about 0.7% of observations dropped). Finally, we exclude from the sample outlier firms that grow beyond 80 workers within the period of analysis, likely as a consequence of mergers or acquisitions.

We narrow our focus on firms with pre-reform employment levels (average firm labour force in the first six months of 2007) within a specific range centered around the 15-employee threshold. Within this framework, we define the treatment group as that comprising firms with pre-reform employment ranging between 16 and 22 employees, while the control group encompasses firms with pre-reform employment ranging between 9 and 15 employees.

The upper bound is established to exclude relatively large firms, which, as outlined in Section 2, are less affected by the practice of illegal resignations. Conversely, the lower bound eliminates very small firms with basic organizational structures that are subject to significant fluctuations. The sample restrictions do not substantially affect the sample representativeness, as 98% of firms in Italy are below 20 employees. We test the sensitivity of the upper and lower bounds in Section 5.2.1. Similar sample selection criteria have been employed in prior studies utilizing the same dataset, facilitating meaningful comparisons of our findings with existing literature (Boeri and Garibaldi (2019), Cingano et al. (2016) and Sauvagnat and Schivardi (2023) among others).

The final sample includes 126,116 firms, corresponding to 2,575,381 firm-year observations. Among these, 98,214 firms (2,004,568 observations) have 15 or fewer employees and form our control group, as detailed in Section 4. The remaining 27,902 firms (570,813 observations) have more than 15 employees and constitute the treated group. Collectively, the firms in our sample account for approximately one-fifth of the Italian workforce in 2007.

4 Empirical strategy

We study the effect of the 2007 reform introducing online resignations in firms using a difference-in-difference (DiD) design. We assign firms to the treatment and control group based on whether their employment in the first semester of 2007 - the semester before the introduction of the reform - is above or below the 15 threshold. As explained in Section 3, for this purpose we use the INPS variable measuring the firm's full-time equivalent workforce. We estimate the following specification:

$$Y_{i,t} = \alpha + \gamma \cdot 1(Emp_{i,2007} > 15) \cdot RefDate_t + X'_{it}\beta + \epsilon_{i,t} \tag{1}$$

Where $Y_{i,t}$ is the outcome of firm i in month t, $Emp_{i,2007}$ is a dummy equal to 1 if the firm has more than 15 employees the year before the reform enforcement, $RefDate_t$ is a dummy equal to 1 if the observation is from November 2007 onward, and X_{it} is a matrix of controls. In our preferred specification, X_{it} includes month fixed effects, firm fixed effects and firm specific linear trends. Time fixed effects control for any time-varying factors that affect the outcome variable. Firm fixed effects control for time-invariant differences across firms in the treatment and control groups. We include firm-specific linear trends in the specification, allowing firms of different sizes to follow distinct growth trajectories. Firm-specific linear trends control for potential time-varying factors specific to each firm that may not be captured by the firm fixed effects alone. In our setting, they might be particularly indicated to account for gradual changes in the outcome variable that may not be adequately captured by general time trends. Specifically, we can better capture the evolution of firm size over time within each treatment group. This is important because firms below the 15-employee threshold may deliberately limit their growth to avoid crossing the threshold, leading to systematically different trends from those above it. Standard errors are clustered at the firm level.

The coefficient of interest, γ, captures the effect of the new legislation in the post-reform period starting in November 2007 in firms with more than 15 employees compared to firms with less than 15 employees.[5] Firm-level models can suffer from reduced statistical power when firms vary substantially in size. This happens because time fixed effects capture average fluctuations experienced by firms of very different sizes, making them too large or too small depending on firm size, which in turn may generate severe heteroskedasticity. Some studies address this by restricting the sample to very small firms, which can limit external validity; others normalize outcomes by firm size, though this may introduce its own issues. In our setting, this concern does not arise because our identification strategy compares firms that are all small and of similar size, eliminating the heteroskedasticity problem described above. Moreover, any attempt to normalize the outcome by firm size would raise endogeneity concerns, as firm size may itself respond to the treatment. For this reason, we adopt a more transparent approach and keep the outcome variable in levels.

Our main specification comprises a pre-reform period of 12 months and a post-reform period of 9 months. In the post-reform period, we include the months during which the reform was active (up until the repeal at the end of June 2008), plus an additional month. This adjustment accounts for the fact that the impact on resignations can only be observed with a lag, due to the notice period. In section 5.2.1 we provide additional robustness to alternative sets of bandwidths.[6]

Clearly, illegal resignations are not observable by definition. Our main outcome variable at firm-level will therefore be voluntary resignations. The causal interpretation of the coefficient of interest relies on the identification assumption that, in the absence of the reform, the relative number of voluntary resignations of workers in firms above

[5]Several studies in the literature have previously exploited the 15 employees threshold driven by the employment protection legislation as a source of exogenous variation to investigate the relationship of dismissal cost with a range of outcomes such as workers and jobs flows (Bertoni et al. (2023) and Kugler and Pica (2008)), hiring dynamics (Boeri and Garibaldi (2019)), adjustments to health shocks Simonetti et al. (2022) and firms capital deepening and training provision (e.g. (Cingano et al. (2016) and Bratti et al. (2021)).

[6]One might wonder whether a difference-in-discontinuities design exploiting the firm-size threshold at 15 employees could be used instead of the difference-in-differences strategy adopted in this paper. As shown by Grembi et al. (2016), identification in a difference-in-discontinuities framework requires that the treatment does not interact with the confounding policy generating the baseline discontinuity (Assumption 3 in Grembi et al. (2016)). This assumption is violated in our setting. Firms just above and just below the 15-employee threshold face different employment protection regimes, which directly shape their incentives to resort to blank resignations. As a result, the reform restricting blank resignations is expected to have a differential effect precisely because of the discontinuous change in firing costs at the threshold. Consequently, the change in the discontinuity after the reform would conflate the effect of the reform with its interaction with employment protection legislation, preventing a causal interpretation of a difference-in-discontinuities estimator.

15 employees would have evolved similarly to those in firms with less than 15 employees. The effect of the reform would then capture the change in the extent to which illegal resignations are used in firms above the 15-employee threshold, where workers' dismissal is more costly, compared to those below the 15-employee threshold, where the costs of dismissal for firms are more limited.

To validate this assumption, we employ an event-study model, a method that offers a transparent approach to testing the parallel trends assumption, while at the same time allowing us to study the dynamic impact of the introduction of the reform.

We augment the two-way fixed effect model (1) with a set of 12 monthly leads and 14 lags of the treatment effect:

$$\begin{aligned} Y_{i,t} = \alpha &+ \sum_{k=m}^{-1} \beta_k \cdot 1(Emp_{i,2007} > 15) \cdot (t = k) \\ &+ \sum_{k=0}^{M} \gamma_k \cdot 1(Emp_{i,2007} > 15) \cdot (t = k) + \beta X_{it} + \tau_t + f_i + \epsilon_{i,t} \end{aligned} \tag{2}$$

where $Y_{i,t}$ is the number of resignations in firm i in month t and $(Emp_{i,2007}) \cdot (t = k)$ is a set of event-time fixed effects interacted with the treatment dummy. τ_t is a set of calendar-month fixed effects, f_i a set of firm fixed effects and X_{it} includes firm specific linear trends. The vector γ_k includes the coefficients of interest, namely the differential number of resignations in treated firms compared to resignations in control firms following the reform. The vector of coefficients β_k allows us to evaluate whether the parallel-trends assumption is likely to hold in the pre-treatment period.

In the event-study, we extend the period of analysis to 14 months after the start of the reform, to better study the dynamic impact of the reform introduction and its persistence over time. To increase the precision of our estimates, we estimate treatment effects at a bimonthly frequency (e.g., event-time 0 refers to November and December 2007). All coefficients are measured with respect to the reference period, event time $t = -1$, corresponding to September and October 2007.[7] It is worth mentioning that our treatment is not staggered, ruling out potential biases arising from heterogeneous treatment effects (e.g., Goodman-Bacon 2021 and Sun and Abraham 2021).

[7] As discussed in Sun and Abraham (2021), we also exclude a distant period, event time $t = -6$, to avoid multi-collinearity issues.

5 Results

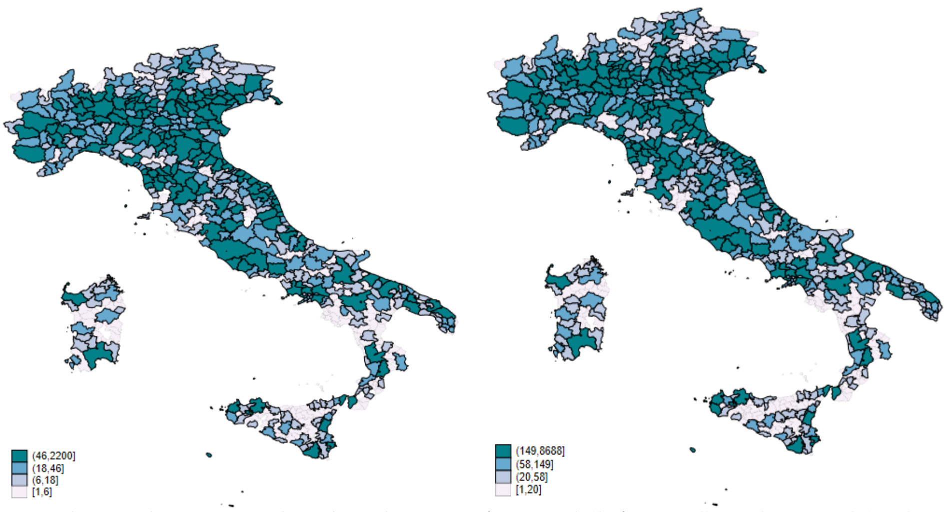
5.1 Descriptive evidence

Tabella 1 shows descriptive statistics for firms in the final dataset measured in the first semester of 2007, before the introduction of online resignations.

Overall, control and treated groups are largely balanced in terms of firm-level baseline characteristics. As expected, the number of separations, new hires and voluntary resignations is higher in larger firms. Regarding our main outcome variable, we observe on average 0.27 and 0.18 voluntary resignations per firm-month in the treated and control group, respectively. In terms of sectorial distribution, a greater proportion of firms in the control group operate within the manufacturing sector, while the opposite holds for the construction sector.

Figura 1 displays the geographic distribution of firms in our sample at the local labour market level for treated and control units. Overall, firms in both groups are more concentrated in more economically developed areas of the country, but there are no relevant differences in the geographic distribution of treated and control firms.

Figura 1: Geographic distribution of treated and control firms in our sample



Notes: The map shows the geographic distribution of treated (left panel) and control (right panel) firms in our sample in 2007. Break points are quartile intervals in the number of firms per local labour market.

To provide preliminary evidence on the effect of the introduction of online resignations, in Figura 2 we show the evolution over time of the monthly average of resignations in the treatment and control group before and after the reform. The figure presents three main takeaways. First, we observe that the resignation patterns during the

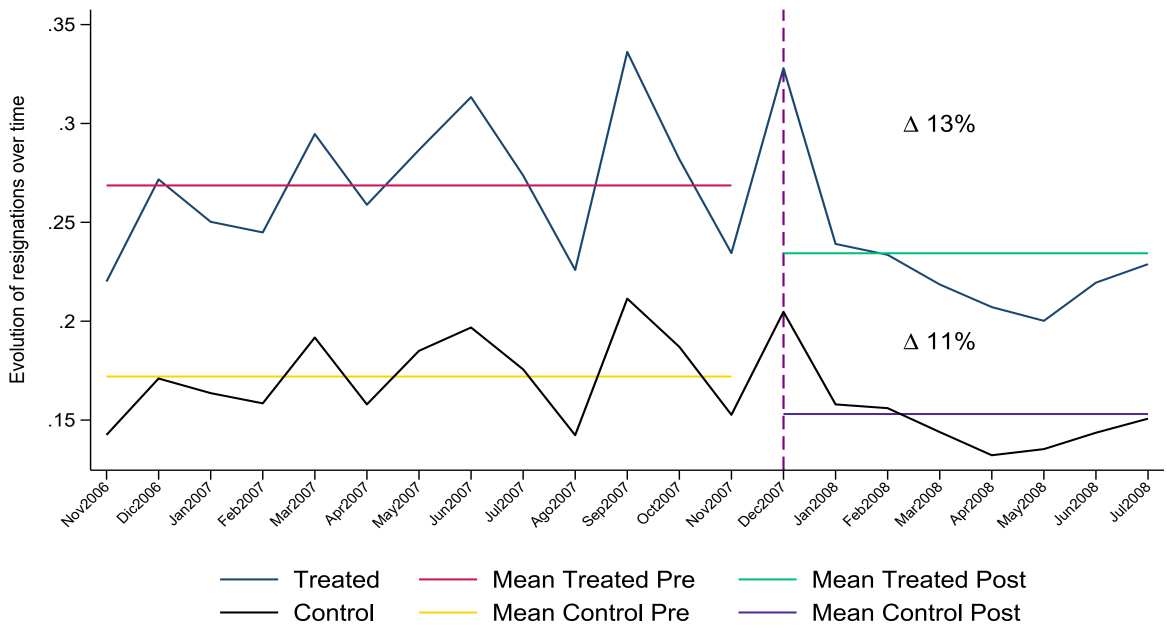
Tabella 1: Descriptive statistics for treated and control group pre-determined characteristics

	Treated **Firms above 15**	**Control** **Firms below 15**
Share females	0.347	0.355
	(0.32)	(0.32)
Share migrants	0.112	0.116
	(0.17)	(0.18)
Share young	0.591	0.606
	(0.21)	(0.23)
Age	37.923	37.624
	(4.5)	(4.8)
Daily wage	73.919	70.955
	(18.83)	(18.02)
Experience	17.443	17.039
	(5.18)	(5.51)
Share blue-collars	0.585	0.581
	(0.32)	(0.33)
Share clerks	0.307	0.313
	(0.31)	(0.32)
Share managers	0.006	0.004
	(0.03)	(0.03)
Share apprenticeship	0.071	0.074
	(0.11)	(0.12)
Share open-ended	0.864	0.886
	(0.18)	(0.18)
Share fixed term	0.104	0.086
	(0.16)	(0.15)
N. Separations	0.465	0.274
	(1.44)	(0.926)
N. New hires	0.612	0.358
	(1.58)	(1.01)
N. Voluntary resignations	0.264	0.168
	(0.82)	(0.59)

Notes: This table shows summary statistics for firms in our working sample in the first semester of 2007, prior to the implementation of the reform. Columns (1) and (2) show means and standard deviations (in parenthesis) of firms in the treatment and control group, respectively.

pre-reform period in both the treatment and control groups exhibit similar dynamics, supporting the validity of selecting firms with fewer than 15 employees as the control group. Second, both groups witness a significant decrease in resignations immediately following the reform. Third, while the decrease in resignations is common across both groups, the reduction among firms with more than 15 employees (13%) is more pronounced than in firms with less than 15 employees (11%). This finding supports our working hypothesis that firms exceeding this threshold, since they face higher firing costs, were more likely to resort to the illicit practice of blank resignations.

Figura 2: Evolution of the number of resignations over time in treated and control group



Notes: The figure show the evolution of the number of resignations in treated (blue) and control (black) units. The vertical dashed line indicates the reform date. For each group, we additionally report averages pre- and post-reform. The two Δs refer to the percentage drop in resignations after the reform in the treatment and control group, respectively.

5.2 Voluntary resignations

We show our main results in Tabella 2, where we report the coefficients of the regression of the number of voluntary resignations in each firm-month on the variable *Treat* $*$ *Post*, the interaction between treatment and a post-treatment time indicator. In Column (1), we only control for month and firm fixed effects. Since changes in the number of resignations are sensitive to changes in the size of the firm, one concern might be that firms of different size might grow at different rates, which could undermine the validity of the common trend assumption. To mitigate this concern, in Column (2) we control for the firm size (measured in (t-1) to reduce endogeneity concerns). In Column (3), we allow for differential linear trends in the outcome variable for firms above and below the 15-employees threshold. Finally, in Column (4) we allow for firm-specific linear trends.

The estimated coefficients indicate that the reform led to a negative, statistically significant reduction in voluntary resignations in treated firms with respect to control firms. The result is robust across different specifications. Quantitatively, our preferred specification presented in Column (4) indicates a reduction of 0.013 in the number of resignations, corresponding to 4.7%.

This result is in line with our expectation that the impact of the reform is stronger for firms above the 15-employee threshold, where the criteria for dismissal are more stringent. A potential concern is that the observed effect might be driven by a higher number of resignations in control group firms. This could occur if workers in the control group were more likely to resign and move to companies exceeding the 15-employee threshold, attracted by the enhanced protections, since larger firms are less likely to threaten the use of blank resignations. However, if increased resignations in the control group were the primary driver of the observed effect, we would expect to see a rise in resignations following the reform in Figura 2. Instead, our data show the opposite trend. This evidence mitigates concerns about spillover effects influencing our results.

If the reform was completely successful in eliminating the practice of "blank resignations", this would mean that at least 4.7% of all resignations in treated firms were the result of this illegal practice. Our result is to be interpreted as a lower bound on the total share of illegal resignations, as our estimate is relative to resignations in control firms.[8]

We now analyze the dynamics of the effect identified in the static DiD using an event-study framework. Estimated coefficients from Equazione 2 are reported in Figu-

[8] It is worth noting that the recession cannot account for our findings nor act as a confounding factor. Italy experienced a pronounced economic contraction related to the U.S. Great Recession only starting in 2009, a period that lies outside our sample window (Ministero dell'Economia e delle Finanze, 2010). Moreover, there is no reason to expect aggregate economic shocks to differentially affect firms just above and just below the 15-employee threshold. In particular, the threshold does not coincide with structural breaks in firm characteristics—such as workforce composition, the inclusion of family members, or the measurement of employee headcount—that would plausibly generate heterogeneous responses to macroeconomic conditions.

Tabella 2: DiD main estimates

	Outcome: Number of Voluntary Resignations			
	(1)	(2)	(3)	(4)
TreatxPost	-0.012***	-0.019***	-0.017***	-0.013***
	(0.002)	(0.002)	(0.004)	(0.005)
Month FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm size (t-1)	No	Yes	No	No
Group linear trends	No	No	Yes	No
Firm linear trends	No	No	Yes	Yes
Observations	2,575,381	2,575,378	2,575,381	2,575,381

Notes: The table reports OLS estimates obtained from our main specification (Equazione 1). The sample includes all months from January 2006 to July 2007, included. All regressions include firm and month fixed effects. In Column (2) we include firm size measured in (t-1) (Firm size (t-1)), in Column (3) we include group specific linear trends (Group linear trends) and in Column (4) we include firm specific linear trends (Firm linear trends). Standard errors are clustered at the firm-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

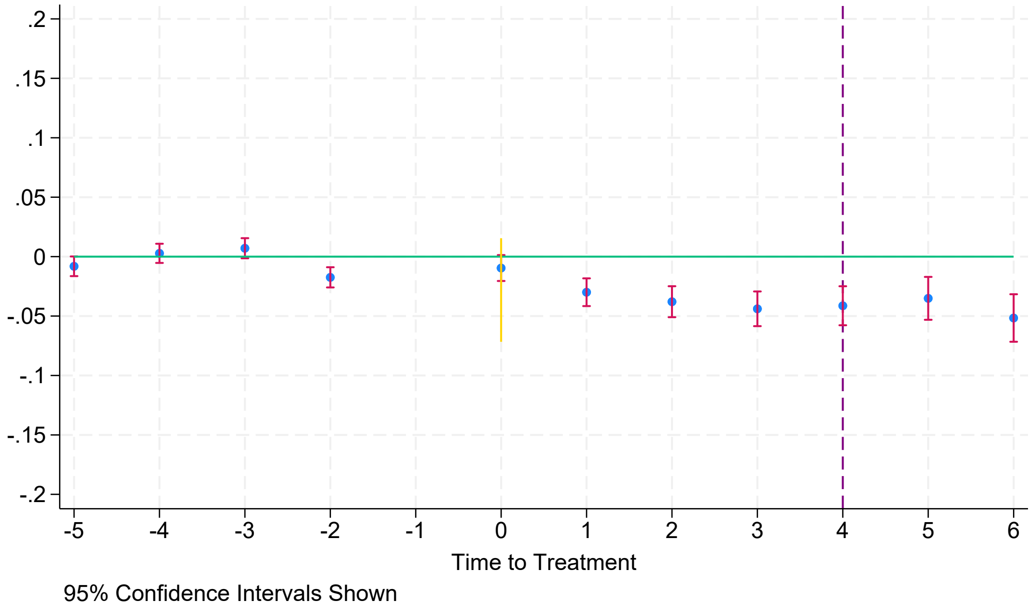
ra 3. Two main results emerge. First, the data suggest that the parallel trend assumption is not rejected, as examination of the figure shows no discernible pre-treatment effect. The only exception is the coefficient at time -2 that is marginally significant (p-value equal to 0.93). Second, the treatment effects' dynamics indicate a notable decline in resignations following the reform, stabilizing around time +2. Interestingly, this effect persists even after the reform is abolished (indicated by the dotted vertical bar), suggesting a non-transitory behavioural response from firms, without any apparent mean reversion, at least in the period of time considered.

5.2.1 Robustness

We perform a number of sensitivity checks to test the robustness of the baseline estimates discussed in Section 5.2.

Randomization inference As an alternative inference strategy we adopt a non-parametric permutation-based method (see Fisher et al. 1966, Rosenbaum and Rosenbaum 2002, and MacKinnon and Webb 2020). This consists in permuting the treatment assignment randomly in the original data multiple times, so that the dataset remains unchanged except for the reassigned treatment. In order for our main results to be valid, we should expect estimates using the placebo samples to be centered around zero and to be mostly concentrated to the right of our observed baseline estimate (Column 4 of Tabella 2). We show the distribution of the baseline DiD placebo t-statistics based

Figura 3: Event study estimates on the number of resignations at the firm-level



Notes: This figure plots estimates of the effect of 5 leads and 7 lags of the reform on the number of resignations. Estimates are obtained from Equazione 2. All effects are relative to the two-months period before the reform (i.e., time to treatment=-1). The dashed purple line indicates the end of the reform. The regression includes year fixed effects, firm fixed effects and firm specific linear trends. 95% confidence intervals are shown. Standard errors are clustered at the firm-level.

on 100 permutations in Figura 4. The distribution is concentrated around zero and the estimated t-statistics are always larger than the one obtained from our original data, represented by the vertical dotted line. This finding strongly suggests that our estimated effect is unlikely to be the result of a spurious correlation.

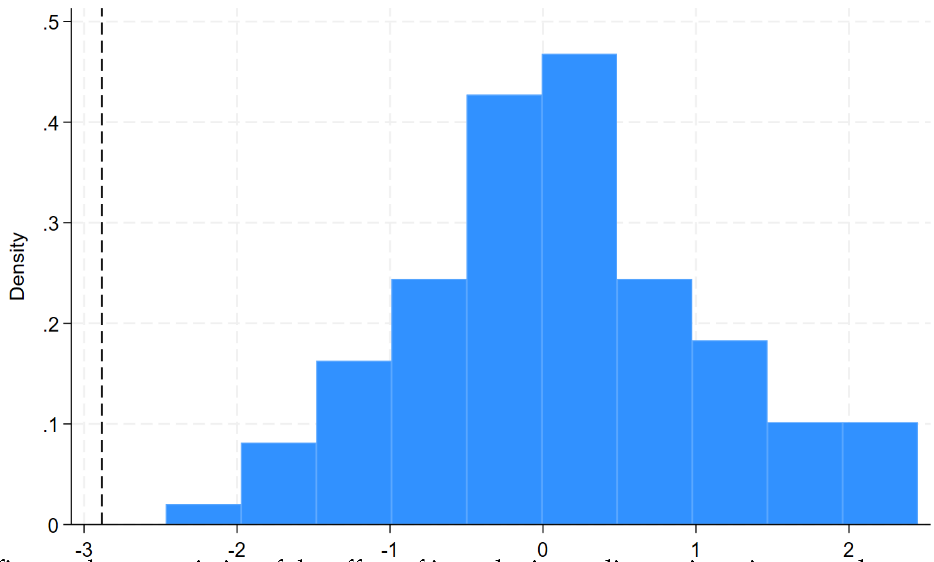
Firm survival In our sample, we only require firms to be active for at least one quarter each year. A potential concern arises regarding compositional shifts induced by the reform, particularly if firms above and below the 15-employee threshold have a different likelihood to shut down. Besides, firm survival is inherently interesting as an additional outcome: a liquidity constrained firm, unable to offset demand contractions by dismissing workers without costs through "blank resignations", may consequently exit the market.

We run our baseline model (Equazione 1) on a dummy that takes value one if the firm is operating in month t, and 0 otherwise. In Tabella 3, Column (1), our findings indicate that the implementation of the reform does not consistently predict a differential probability of survival between treated and control firms.

Excluding alternative channels: alternative end-of-contract reasons

Firms may have found alternative ways to dismiss workers once the reform was

Figura 4: Randomization inference



Notes: This figure plots t-statistics of the effect of introducing online resignation on voluntary quits from estimating Equazione 1, when the treatment status is randomly assigned to individuals in the sample (100 permutations). The black dashed vertical line indicates t-stats from our baseline model (Column 4 of Tabella 2). Regressions include year fixed effects, firm fixed effects and firm linear trends. Standard errors are clustered at the firm-level.

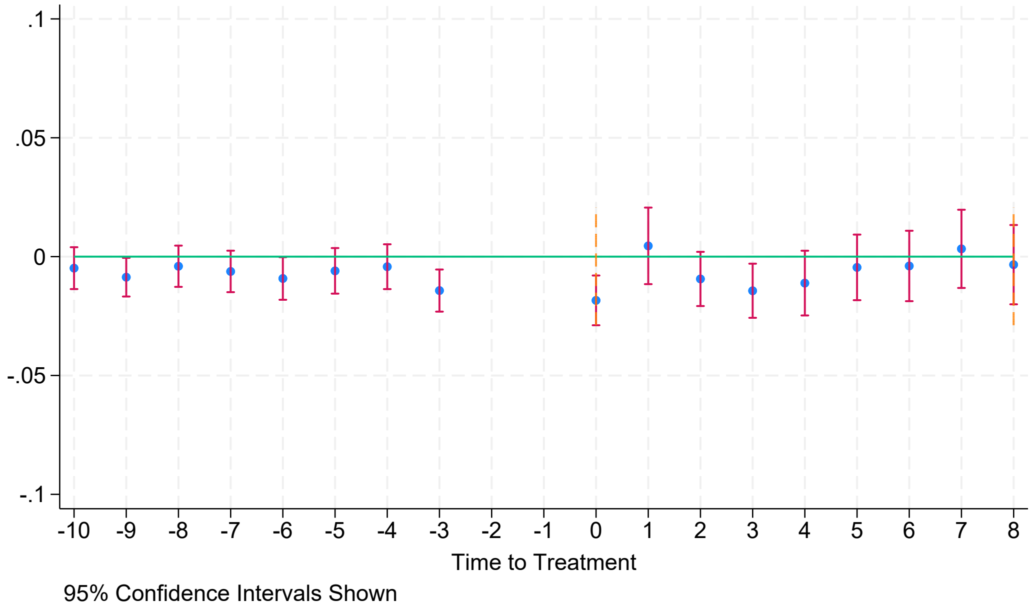
implemented, especially since blank resignations were no longer a viable option. As discussed in Section 2, Italy is characterised by strict labour market regulations that make workers' dismissal highly restrictive, especially for firms with more than 15 employees. This implies that employers cannot easily substitute layoffs through blank resignations with genuine dismissals. In turn, this implies that we should expect the reform to have none or little impact on dismissals.

In Figura 5, we present event study estimates for the number of resignations at the firm-level, using the same specification as described in Equazione 2. While the visual evidence again supports the absence of differential pre-treatment trends, unlike in Figura 3, we detect no effect on the number of dismissals, consistently with the institutional features in our setting. This suggests that the primary channel of the reform operated through a reduction in resignations.

Strategic anticipation of the reform

Another potential concern is that firms might anticipate the reform and strategically sort out of treatment by reducing their size to be just below the 15-employee threshold. In Figura 6 we show the share of firms that were above the threshold in $(t-1)$ and moved below the threshold in t, around the reform cutoff date. By visual inspection, the figure reveals no evidence of a spike in the share of firms that reduced their size

Figura 5: Event study estimates on the number of dismissal at the firm-level



Notes: This figure plots estimates of the effect of 10 leads and 9 lags of the reform on the number of dismissals. Estimates are obtained from Equazione 2. All effects are relative to the two-month period before the reform (i.e., time to treatment=-1). Dashed lines indicate the start and the end of the reform. The regression includes year fixed effects, firm fixed effects and firm specific linear trends. 95% confidence intervals are shown. Standard errors are clustered at the firm-level.

before November 2007.

We conduct a more formal examination to verify the absence of a discontinuity in the distribution of firms above and below the threshold by employing a McCrary test (McCrary, 2008) estimated using the method of Cattaneo et al. (2020). Our analysis results in not rejecting the null hypothesis, indicating continuity of the density function at the reform cutoff date (p-value=0.183).

Finally, we examine firm sorting as in (Cingano et al., 2016).[9] As a first step, we compute for each firm the average number of resignations before November 2007 (the reform date) and use this time-invariant firm characteristic as one of the determinants of the firm probability of shrinking.[10] This variable is supposed to capture unobserved firms' characteristics. We want to test that the reform did not induce compositional changes around the threshold in terms of unobserved characteristics that are correlated with the outcome of interest (Cingano et al., 2016).

[9]Cingano et al. (2016) study a 1990 reform that introduced unjust-firing costs in Italy for firms below 15 employees, leaving firing costs unchanged for larger firms.

[10]We calculate the time-invariant number of resignations based on the firm's average number of resignations in the first semester of 2007.

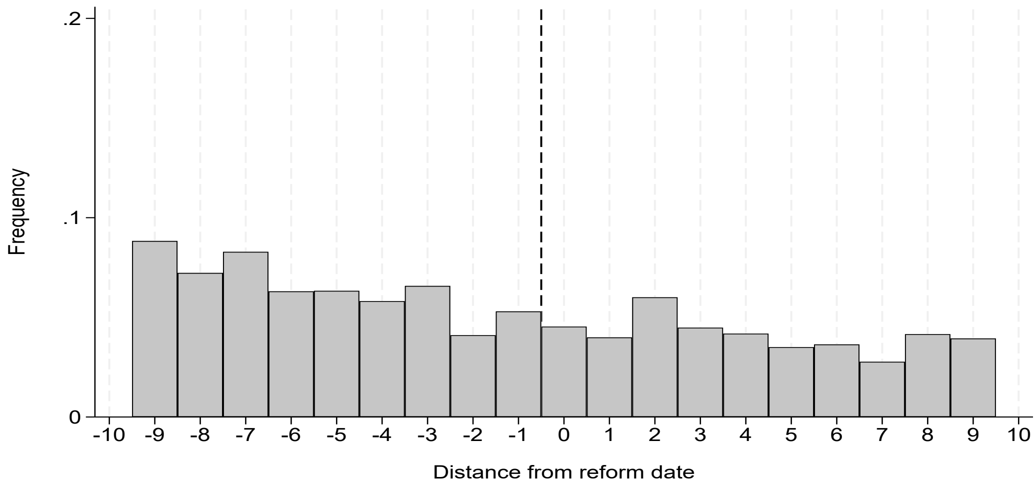
Specifically, we run the following regression:

$$\begin{aligned} d_{i,t} =& \delta_0 Post + \delta_1 SizeDummy_{i,t-1} + \delta_2 \overline{K}_{pre,i} + \alpha_0 (SizeDummy_{i,t-1} \cdot Post) + \\ & \alpha_1 (\overline{K}_{pre,i} * Post) + \alpha_2 (SizeDummy_{i,t-1} \cdot \overline{K}_{pre,i} \cdot Post) + \beta X_{it} + \tau_t + f_i + \epsilon_{i,t} \end{aligned} \tag{3}$$

Where $d_{i,t}$ is an indicator taking value one if firm i in year t has a smaller size than in $t-1$, $SizeDummy_{i,t-1}$ denotes a set of firm size dummies, $Post$ takes the value of one from November 2007, and $\overline{K}_{pre,i}$ denotes the estimated time-invariant average pre-reform number of resignations. τ_t are calendar-time fixed effects, f_i are firm fixed effects and X_{it} includes firm specific linear trends. α_2 is the vector of coefficients of interest.

The findings presented in Tabella 3, Columns (2) and (3), confirm that firms experiencing higher rates of resignations are not significantly more inclined to downsize as a consequence of the reform. Overall, the evidence presented provides reassurance that our estimated effect is not the result of compositional changes.

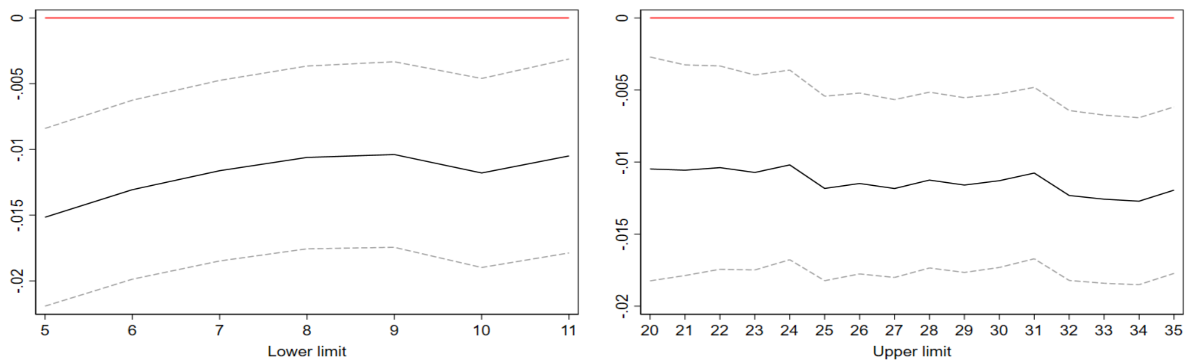
Figura 6: Distribution of firms moving below the 15-employee threshold



Notes: The figure shows the distribution of the number of firms that, in the months around the reform (dashed line at 0), where above 15 employees in t-1 and go below 15 employees in t. The p-value of the McCrary test is equal to 0.183. The McCrary test p-value is estimated with the method of Cattaneo et al. (2020).

Alternative firm size bounds Our results are estimated on a sample of firms with size ranging from 9 to 22 employees. Figura 7 shows estimates of our baseline model when employing different thresholds for both lower (Panel A) and upper (Panel B) bounds of firm size. Specifically, each graph reports the coefficient estimate along with 90% confidence intervals obtained by running our model on a sample of firms ranging

Figura 7: Alternative firms' size bandwidths



Notes: The figure reports the estimate of the coefficient γ in Equazione 1 and 90% confidence intervals obtained by running our model on the sample of firms with i) panel A: firm size from 5 to 11 and ii) panel B : firm size from 23 up to 35. Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses.

from 5 to 11 employees (Panel A) and from 23 to 35 employees (Panel B). The depicted results distinctly indicate that our observed effect remains robust across varying bandwidth selections.

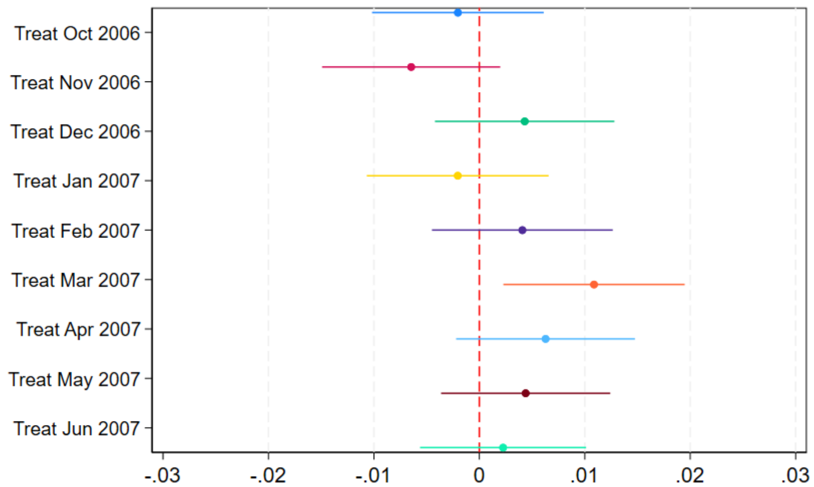
Donut hole We further show the robustness of our results to sorting at the 15-employee threshold by removing all observations close to the cut-off and using the remaining data to fit the DiD model, in the spirit of the "donut hole" method used in regression discontinuity designs (Barreca et al., 2011). In Tabella 3, Column (4), we show that our main estimate is not sensitive to the exclusion of firms with 15 or 16 employees.

Alternative definition of the outcome variable In our main analysis we define our outcome variable as the monthly number of resignations at the firm-level. Given the small size of firms in our sample, the monthly number of resignations is predominantly either zero or one. In order to check that our results are not driven by a very small number of observations with a large number of resignations, we employ an alternative definition of the outcome variable. Specifically, we define the outcome as a dummy equal to zero if the number of resignations is zero, and equal to one if the number of resignations is strictly positive. Consistently with our main finding, results in Column (5) of Tabella 3 show a significant decrease in resignations.

As a further check, we repeat our analysis by removing firms with a number of resignations within the top one percent of the distribution of resignations larger than zero. As shown in Column (6) of Tabella 3, our results are robust to the exclusion of these firms.

Placebo reforms To strengthen the confidence in attributing the change in outcomes to the intervention rather than other concurrent factors, we conduct placebo regressions by employing "fake" treatment dates proximate to the actual intervention period. The anticipation is for the effect to diminish or vanish. Figura 8 presents coefficient estimates from various specifications, using hypothetical reform dates ranging from October 2006 to June 2007. Consistently, no significant effects are observed across these specifications, reinforcing the validity of the reform's causal impact. The sole exception is the coefficient for the treatment start date, March 2007, which is positive and significant. However, this result contrasts with the direction of the effect found in our primary analysis, suggesting a spurious relationship.

Figura 8: Placebo reforms



Notes: The figure reports regression coefficients of placebo reforms that would have been implemented in October, November, or December 2006 or January, February, March, April, May, or June 2007 instead of in November 2007. The regressions include firm fixed effects, year fixed effects, and firm linear trends. Standard errors are clustered at the firm-level.

Alternative clustering choice We finally test the sensitivity of our estimate precision to alternative clustering strategies. As illustrated in Figura 9, varying clustering methods (no clustering, clustering at the firm, municipality, or local labour market level) has minimal impact on the statistical significance of our estimates, lending support to the robustness of our results.

5.3 Heterogeneity analysis

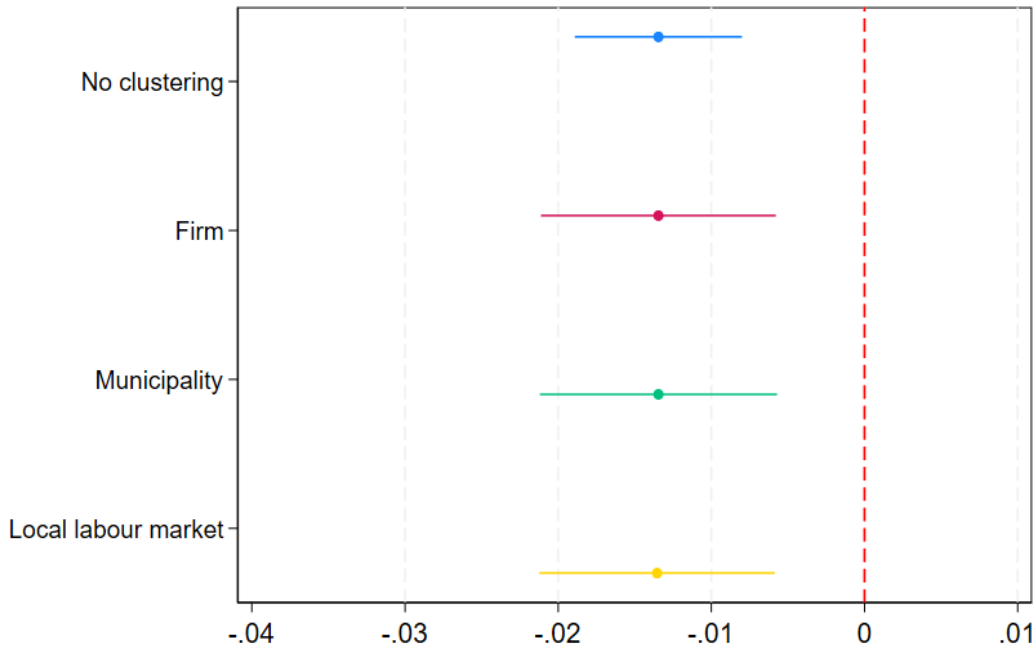
We next conduct heterogeneity analyses based on the firms' characteristics, the pre-existing composition of their workforce, and labour market characteristics. This exer-

Tabella 3: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm survival	Firm sorting (down)	Firm sorting (up)	Donut hole	Binary outcome	Resignations no outliers
TreatxPost	-0.000			-0.014***	-0.005**	-0.010***
	(0.000)			(0.005)	(0.002)	(0.003)
Size13xK_{pre}xPost		-0.009				
		(0.007)				
Size14xK_{pre}xPost		-0.003				
		(0.007)				
Size15xK_{pre}xPost		0.001				
		(0.008)				
Size16xK_{pre}xPost			-0.002			
			(0.009)			
Size17xK_{pre}xPost			0.007			
			(0.009)			
Size18xK_{pre}xPost			0.014			
			(0.010)			
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,641,001	2,575,381	2,575,381	2,473,108	2,575,381	2,540,295

Notes: This table shows estimates of different specifications. In Column (1) we report the results of the estimation of Equazione 1 using as an outcome variable a dummy taking value one if the firm is operating in month t, and 0 otherwise. Columns (2) and (3) report estimates of Equazione 3. The outcome is an indicator variable taking value one if firm i in year t has a smaller size than in $t-1$, the coefficients $SizeDummy_{i,t-1}xK_{pre,i}xPost$ are those of a triple-interaction of a firm size dummy ($SizeDummy_{i,t-1}$), an estimated time-invariant average of pre-reform firm resignations ($K_{pre,i}$), and a dummy taking value equal to one from November 2007 (Post). Column (4) shows estimates of Equazione 1 on the number of resignations, excluding from the sample firms with size equal to 15 or 16. Column (5) reports estimates of Equazione 1 on the number of resignations, but we define the outcome as a dummy equal to zero if the number of resignations is zero, and equal to one if the number of resignations is strictly positive. Column (6) shows estimates of Equazione 1 on the number of resignations once firms with a number of resignations within the top one percent of the distribution of resignations larger than zero are removed. All specifications include month fixed effects, firm fixed effects and firm specific linear trends. Standard errors are clustered at the firm-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Figura 9: Alternative clustering choice



Notes: The figure depicts the coefficient estimates based on Equazione 1. The estimates differ only in terms of the clustering level used. We report estimates with (i) no clustering (ii) at the firm-level (*baseline*) (iii) municipal level and (iv) local labour market level. Regressions include year fixed effects, firm fixed effects and firm linear trends.

cise allows us to characterise compliers along multiple dimensions, to lay the groundwork for a discussion of the possible mechanisms behind the observed effects.

Heterogeneity by demographic characteristics of the workforce In Tabella 4 we focus on demographic characteristics of the workforce: (i) gender, (ii) migration background, and (iii) age. We categorize firms as having a high (low) share of women, migrants and young workers if they are above (below) the median of the distribution of the average share (measured pre-reform, in the first semester of 2007).

The findings presented in Tabella 4 indicate that the observed outcomes are primarily influenced by firms with a higher proportion of female, migrant, and younger employees. This aligns with the hypothesis that "blank resignations" were predominantly utilized for vulnerable workers. On the one hand, individuals in these demographic groups may have fewer alternative employment prospects, thus possessing limited bargaining power. On the other hand, they might be perceived as more costly options due to factors such as their relatively shorter experience, which could entail higher screening costs and women's potential take up of maternity leave.

Heterogeneity by contractual characteristics of the workforce In Tabella 5 we ana-

Tabella 4: Heterogeneity by share of female, migrants, young workers

	Outcome: Number of voluntary resignations					
	(1)	(2)	(3)	(4)	(5)	(6)
	Female		Migrant		Young	
	High	Low	High	Low	High	Low
TreatxPost	-0.019***	-0.008	-0.017**	-0.008	-0.031***	0.001
	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.006)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,288,716	1,286,665	1,287,415	1,287,966	1,287,586	1,287,795

Notes: This table reports DiD coefficients of our main specification estimated separately by high (low) pre-determined share of female, migrant and young workers (age<40). Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

lyze the heterogeneity driven by workforce composition in terms of contract characteristics, such as type of employment arrangement (share of full-time contracts), contract's horizon (share of fixed term contracts) and experience (share of highly experienced workers). We classify firms as having a high (low) share of full-time, fixed-term or highly experienced workers if the pre-determined share - as measured in the first 6 months of 2007 - is higher (lower) than the median among all firms.

The findings presented in Tabella 5 substantiate our initial hypothesis that ”blank resignations” were predominantly used by firms offering comparatively inferior contractual terms and employing less experienced workers. Specifically, our analysis reveals larger treatment effects in firms characterised by a lower proportion of full-time employees (Column 1), a higher prevalence of fixed-term contracts (Column 2), and a larger share of employees with limited experience (Column 3).

Heterogeneity by industry In Figura 10 we analyze heterogeneity by industry. We estimate our baseline model separately on each sector defined at the two-digit level.[11] The findings unveil notable disparities across sectors, with particularly pronounced and statistically significant effects observed in the manufacturing, retail, hospitality, renting services and education services sectors. This may be attributed to the inherently variable nature of demand, seasonal fluctuations, and the need for flexible staffing solutions, which are typical of these industries.

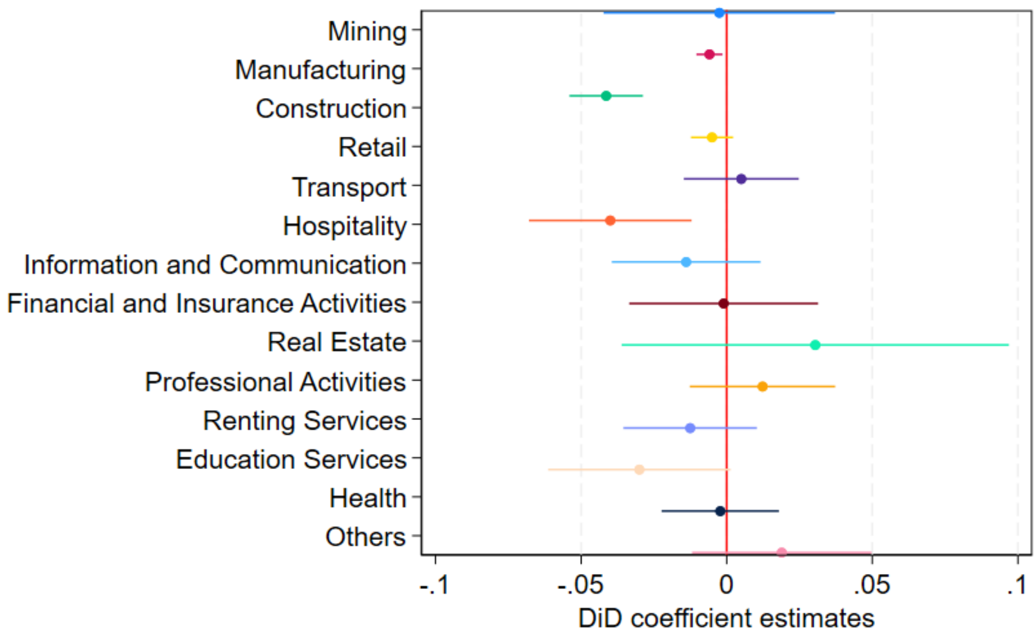
[11] We aggregate the Agriculture, Water and Electricity Supply, Entertainment, Public Administration and Extra Territorial Organization sectors in the *Other* category, given their relatively smaller scale.

Tabella 5: Heterogeneity by share of full-time, fixed-term, experience of employees

	Outcome: Number of voluntary resignations					
	(1)	(2)	(3)	(4)	(5)	(6)
	Full-time		Fixed Term		High Experience	
	High	Low	High	Low	High	Low
TreatxPost	-0.007	-0.019***	-0.019***	0.003	-0.001	-0.025***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.005)	(0.008)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,287,671	1,287,710	1,289,069	1,286,312	1,285,250	1,286,089

Notes: This table reports DiD coefficients of our main specification estimated separately by high (low) pre-determined share of full-time contracts, fixed-term contracts and experience. Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Figura 10: Heterogeneity by industry sector



Notes: The figure reports regression coefficients from the difference-in-differences specification in Equazione 1 together with 90% confidence intervals. Each coefficient comes from a different estimation. The regressions include firm fixed effects, year fixed effects, and firm linear trends. Standard errors are clustered at the firm-level.

Heterogeneity by characteristics of local labour market A crucial question is why workers do accept to work under the "blank resignations" threat. Dahl and Knepper (2021) show evidence that the under-reporting of sexual harassment, and its consequent tacit acceptance, stems from the implicit threat of retaliatory termination by employers. This threat is particularly acute when workers have limited alternative employment opportunities.

Building upon this premise, we turn to characterise the labour markets where firms are active to investigate whether the impact of the policy reform is more pronounced in municipalities exhibiting higher pre-reform unemployment rates,[12] in local labour markets with a lower prevalence of large-scale firms, and with high levels of informal labour (see Section 3 for additional details).

The findings presented in Tabella 6 show that the average effect masks substantial heterogeneity across local labour markets. Consistently with the notion that outside options have a crucial role in shaping workers' decisions, we find larger treatment effects for firms operating in local labour markets characterised by a low share of large firms, where there is a high presence of black economy, and where the occupation rate is low.

Tabella 6: Heterogeneity by local labour market characteristics

	Outcome: Number of voluntary resignations					
	(1)	(2)	(3)	(4)	(5)	(6)
	Large Firms		Black economy		Occupation rate	
	High Share	Low Share	High	Low	High	Low
TreatxPost	-0.007	-0.020***	-0.021***	-0.008	-0.010	-0.017**
	(0.007)	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,415,040	1,759,980	1,234,961	1,323,153	1,400,007	1,053,668

Notes: This table reports DiD coefficients of our main specification estimated separately by (i) high (low) pre-determined share of relatively big firms (larger than 50 workers) in the local labour market (Columns 1 and 2). We define local labour markets to have high (low) share of big firm if the share of firm with size greater than 50 is above (below) the median. (ii) high (low) incidence of black economy by region and sector. We define a sector-area to have high (low) incidence of black economy if the informality rate of the employed at the regional sectoral level is higher (lower) than the median. (iii) high (low) local labour market occupation rate. We define municipalities to have high (low) occupation rate if the occupation rate is higher (lower) than the median. Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

[12] We compute unemployment using 2001 Census data at the municipal level.

Heterogeneity by firms' characteristics Finally, in Tabella 7 we investigate whether firms more exposed to the reform - those that saw a larger decrease in voluntary resignations - differ in their productivity at baseline. We define productivity in two ways. First, in Columns (1) and (2) of Tabella 7, we link yearly firm-level balance sheet data from CERVED with our social security working sample. We classify firms as high- or low productivity based on their 2006 value added per worker, considering those above the median as high-productivity and those below as low productivity.

Second, we employ a productivity proxy derived from the average wages within each firm. Specifically, we compute the pre-reform average wage for each firm and categorize them based on whether their average wage is above (high productivity) or falls below (low productivity) the median.[13]

Findings in Tabella 7 show that, using either measure of productivity, the treatment effect is larger in low productivity firms. This is consistent with the notion that low productivity firms often wield disproportionate power over their employees due to limited job opportunities in the area or industry. Moreover, low productivity firms may rely on exploitative labour practices to cut costs and maintain profitability, prioritizing short-term cost savings over investments in human capital development or employee welfare.

Tabella 7: Heterogeneity by firms' characteristics

	Outcome: Number of voluntary resignations			
	(1)	(2)	(3)	(4)
	Value Added		Productivity	
	High	Low	High	Low
TreatxPost	-0.003	-0.017*	-0.007	-0.028***
	(0.006)	(0.010)	(0.005)	(0.009)
Month FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes	Yes
Observations	825,110	778,324	1,519,352	1,056,008

Notes: This table reports DiD estimates of our main specification by (i) high (low) pre-determined firm value added (Columns 1 and 2) and (ii) high (low) productivity. Using CERVED data, we define firms having high (low) value added if the firm value added is above (below) the median. The sample is smaller because small firms are under-represented in CERVED data. We define firms to have high (low) productivity if average pre-reform wage is higher (lower) than the median. Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

[13] Average wages are calculated over the first semester of 2007.

Summary Our findings suggest that the reform successfully reduced employer-induced resignations, thereby mitigating employers' abuse of power. These results are robust to several sensitivity checks, including alternative sample definitions and competing explanations that fail to account for our findings, as discussed in Section 5.2.1.

We have next examined whether power abuse was more prevalent among specific groups of workers and in particular types of labour markets. This analysis provides valuable insights into why some workers accept contracts with blank resignations attached. Examining workers' demographic and contractual characteristics, we find that blank resignations are predominantly used in firms with a higher share of women, migrants, and young workers. These firms also tend to rely more on fixed-term contracts, while employing fewer full-time and highly experienced workers. Regarding labour market characteristics, our results indicate that blank resignations are most prevalent in small, low productivity firms operating in stagnant economies with high levels of informality.

Overall, these findings indicate that the workers most vulnerable to employer exploitation are also among the most disadvantaged in the labour market. Although at the time of the introduction of the reform, the main justification was to guarantee protection to pregnant women, our results indicate that they were not the only target of this form of abuse of power. With limited job opportunities and lower credentials, immigrant and young workers often have little choice but to accept informal employment. In this context, jobs that carry the risk of forced resignations may still be seen as an improvement over informal work, helping to explain why some workers are willing to accept such contracts.

Similar to labour markets characterised by monopsony power, firms operating in these markets may not only influence wage setting but also provide poorer non-wage amenities, as discussed by Manning (2013) and Bassanini et al. (2024), among others. From this perspective, blank resignations could be interpreted as a reduction of work-related amenities. In the Italian context, where wages are largely determined through collective bargaining, employer's abuse within firms and local labour markets may thus play a crucial role in shaping job quality.

6 How do firms and workers adjust to the reform?

To better understand how workers and firms adjust to the reform-induced increase in formality, the next section examines additional outcomes, shedding light on the firms' and workers' reactions to the reform.

6.1 Firms' response: Hiring patterns

Given the evidence presented in Section 5.3, blank resignations were predominantly used by firms characterised by low productivity, employing workers with a weak attachment to the firm (e.g., low experience, part-time contracts) and operating in labour markets with a high prevalence of informal employment and low employment rates. Since these firms can no longer rely on blank resignations, they may seek to adjust their labour force through alternative margins.

First, we explore the spillover effects of the reform on new hires. We define new hires based on employees' hiring dates. To avoid confounding new hires and incumbent employees receiving new contracts within the same firm, we require that a new hire must either have been previously employed by a different firm or have had no prior employment before joining the current firm. Our outcome variable measures the share of new hires in each firm and month over total employment.[14]

Tabella 8, Column (1) presents the results from estimating Equazione 1 using the share of new hires as dependent variable. We find no significant effect on new hires in the months the reform was in place. Figura A4 provides event study estimates, confirming both the absence of short-term effects and the validity of parallel trends. The time frame immediately following the reform, however, is likely too short to observe firm responses in hiring. Consequently, leveraging the fact that parallel trends hold in the months immediately before and after the reform—indicating no short-term impact—we examine the medium-term effects, 18 months post-reform. In the medium term, we observe a negative and statistically significant effect on new hires (Column 2).

In Columns (3) and (4), we report the coefficients of treatment effects distinguishing

[14]This choice is driven by the pronounced seasonality in hiring patterns, with peaks in months such as September and January. A key requirement of our difference-in-differences strategy is that treated and control firms follow parallel trends in the outcome variable. Given that hiring peaks may differ in magnitude between larger (treated) and smaller (control) firms, parallel trends are more likely to hold in shares rather than in levels.

firms based on the prevalence of different contract types, namely fixed-term and open-ended contracts. We classify firms as having a high (low) share of workers with fixed-term or open-ended contracts if their pre-determined share — measured in the first six months of 2007 — is above (below) the median among all firms. Comparing the relative magnitudes and the significance of these effects, we find that the decline in new hires is mostly driven by a decline in the share of new workers with open-ended contracts.

Overall, our findings indicate that the reform systematically reshaped workforce composition in the medium term. Treated firms reduced hiring, likely in response to higher implicit firing costs introduced by online resignations. Notably, the decline was concentrated among workers with open-ended contracts, who are typically more costly to employ given that, especially in the Italian context, they cannot be easily dismissed. This pattern suggests that firms became more cautious in hiring in response to the inability to use blank resignations. Finally, we examine treatment effects across different types of new hires based on contractual (e.g., share of new hires with full-time vs. part-time contracts) and demographic characteristics (e.g., share of female new hires). We find no significant changes along these dimensions.

This analysis provides new insights into whether firms engage in the blank resignations illegal practice primarily as a cost-saving strategy or whether discrimination plays a central role. In the previous section, we showed that firms disproportionately target vulnerable workers — such as women, migrants, and young workers — when abusing their power. Do firms adopt this practice against these groups of workers mainly to reduce costs, or for discriminatory motives? Findings in Tabella 8 suggest that cost-saving considerations are the primary driver. Firms reduce overall hiring, particularly of workers on open-ended contracts, who — as said — are more costly to dismiss. However, the composition of these hiring cuts appears neutral with respect to gender and migrant status, suggesting that while firms seek to minimize costs, they do not systematically discriminate against specific worker groups when reducing hires (see Tabella A1). This finding is consistent with the results being driven by unproductive and unprofitable firms operating in stagnant labour markets, as discussed for the results in Tabella 7. This suggests that the practice of blank resignations might have allowed some employers to circumvent the normal competitive process.

Tabella 8: DiD estimates on the composition of the newly hired workforce

	Outcome: Share of new hires			
	(1)	(2)	(3)	(4)
	All		Fixed-Term	Open Ended
	Short-term effect	Mid-term effect		
TreatxPost	-0.033	-0.061*	-0.019	-0.041*
	(0.046)	(0.032)	(0.021)	(0.023)
Month FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes	Yes
Observations	2,575,375	3,701,031	3,701,031	3,701,031

Notes: This table reports DiD estimates of our main specification (see Equazione 1) on the share of new hires (i) in the 8 months where the reform was in place (Column 1), (ii) in the 18 months after the reform approval (Column 2), (iii) with a fixed-term contract in the 18 months after the reform approval (Column 3) and (iv) with an open ended contract in the 18 months after the reform approval (Column 4). Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

6.2 Worker-level outcomes: number of injuries

We now turn to explore the worker-level outcomes. Specifically, we investigate aggregate adjustments in workplace injuries after the reform.[15] As in previous sections, we estimate our main event study specification (see Equazione 2), using the number of injuries at the firm-level as the outcome variable. The INPS data only record serious injuries lasting more than seven days, a threshold dictated by the actuarial pension calculation method used by the Social Security, which operates on a weekly basis. As a result, the recorded injuries primarily reflect absences of at least a full week.

The frequency of workplace injuries serves as an important indicator of overall workplace quality, as well as of workforce training and upskilling. Following the reform, workers may have become more inclined to avoid dangerous situations that pose a heightened risk of serious injury, because they perceive a greater stability in the job relation and are less subject to the employer threat of being fired. These risks include working while ill, taking on overly strenuous shifts, or performing tasks without adequate preparation or proper safety equipment. At the same time, firms may no longer

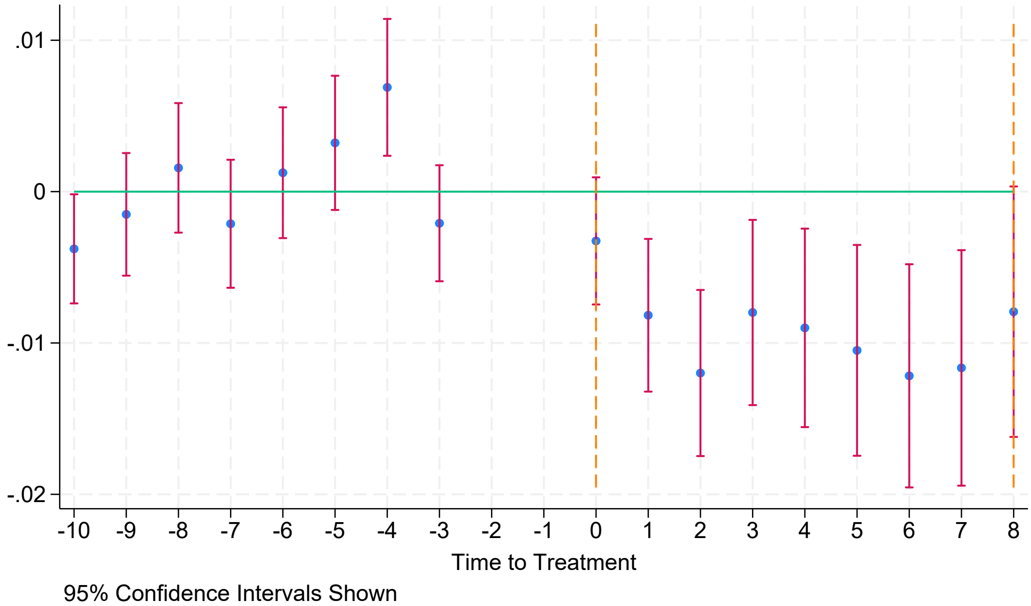
[15] It is worth highlighting that workplace fatalities and accidents constitute a national emergency in Italy. According to data from the Italian National Institute for Insurance against Accidents at Work (INAIL), 1,260 work-related deaths were recorded in 2007—the year used in our empirical analysis—while total workplace accidents approached one million (913,500). Despite the passage of time, workplace safety remains a pressing concern. In 2023, INAIL reported approximately 585,000 workplace accidents, including numerous fatalities.

bypass costs related to worker's safety - such as training programs and safety courses- and invest more in it. Consistent with these hypotheses, Figura 11 presents event study estimates showing a significant decline in firm-level injuries following the reform. This finding indicates a meaningful improvement in workplace safety and supports the view that blank resignations were primarily used for workers at higher risk of injury—such as inexperienced employees and migrants who may also have faced language barriers that hindered their ability to understand and follow safety protocols.

Overall, our results provide evidence that the policy effectively enhanced workers' bargaining power and access to rights, contributing to safer working conditions. By strengthening workers' ability to protect themselves from exploitative or unsafe environments, the reform led to a tangible reduction in workplace injuries, reinforcing its broader role in improving labour market conditions and increasing incumbent workers' bargaining power. This interpretation also aligns with the evidence on new hires (see Tabella 8). Incumbent workers, benefiting from increased bargaining power, can refuse to undertake risky tasks, knowing that firms have become unable to quickly and almost costlessly replace them. A potential concern is that firms might respond by employing fully informal workers. However, due to data limitations, we cannot assess whether the reform has led to an expansion of the informal sector.

In our analysis, we also explored the potential effects of the reform on maternity and parental leave take-up but found no significant impact in the short or medium term. This is not surprising, given the limited duration of the reform. Another relevant margin of adjustment is wages. The reform may have increased workers' bargaining power in the labor market, potentially leading to higher wages. Conversely, firms might have "made workers pay" for the inability to use blank resignations by lowering wages relative to the control group. Unfortunately, we cannot test this labor market outcome because in the monthly data, we observe spikes in June and December due to the payment of the fourteenth and thirteenth salaries. These are extremely difficult to control for empirically and are not captured by the difference between treated and control groups.

Figura 11: Event study estimates on the number of injuries at the firm-level



Notes: This figure plots estimates of the effect of 10 leads and 9 lags of the reform on the number of injuries. Estimates are obtained from Equazione 2. All effects are relative to the two-months period before the reform (i.e., time to treatment=-1). Dashed lines indicate the start and the end of the reform. The regression includes year fixed effects, firm fixed effects and firm specific linear trends. 95% confidence intervals are shown. Standard errors are clustered at the firm-level.

7 Conclusion

This paper sheds light on power abuse by employers in the workplace and its consequences for both workers and firms. We exploit a unique policy reform in Italy to examine the impact of regulations aimed at curtailing abusive practices and promoting formality in employer-employee relationships.

Specifically, we leverage a difference-in-differences estimation design with detailed employer-employee data to provide robust evidence of the effectiveness in reducing illegal resignations of a 2007 reform in Italy. We begin by documenting the prevalence of power abuse by employers, particularly focusing on the illegal practice of coercing employees into signing undated resignation letters. Through this practice, employers circumvent legal restrictions on dismissals, leading to adverse consequences such as reduced job security and exploitation of vulnerable workers, including women, migrants, and younger employees. Moreover, our analysis reveals heterogeneous effects across firms, highlighting that those with low productivity, operating in markets with high informality, and employing more vulnerable workers experience the most significant reduction in resignations following the reform.

Our study also shows how firms adjust their behaviour in response to the reform. We find evidence of a reduction in hires, especially on open-ended contracts, suggesting that some employers may have used illegal resignations as a cost-saving measure, allowing them to escape the standard competitive process and likely hindering the entry and growth of healthier and more productive firms. At the worker-level, we observe a significant reduction in severe injuries, indicating an improvement in workers' bargaining power and overall workplace safety as a result of the reform.

Our findings contribute to the understanding of power imbalances in the labour market, offering causal evidence on the consequences of regulatory interventions aimed at addressing such asymmetries. We show that regulation promoting formality and protecting workers' rights can help reduce the extent of power abuse and mitigate its adverse effects in the workplace, highlighting the potential benefits of regulatory measures in fostering fair and equitable labour practices.

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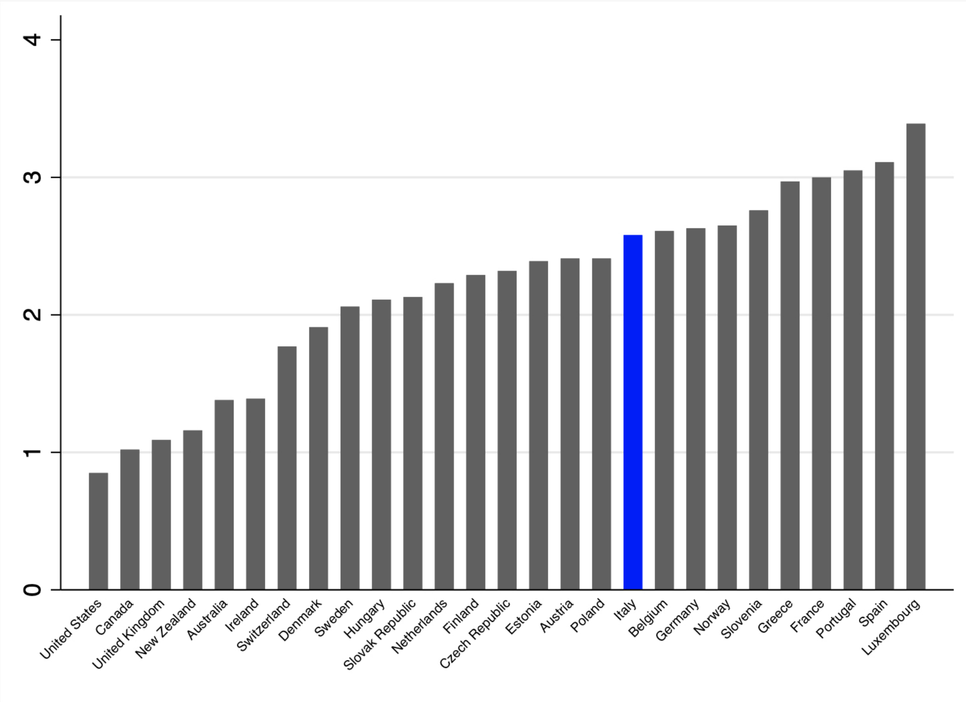
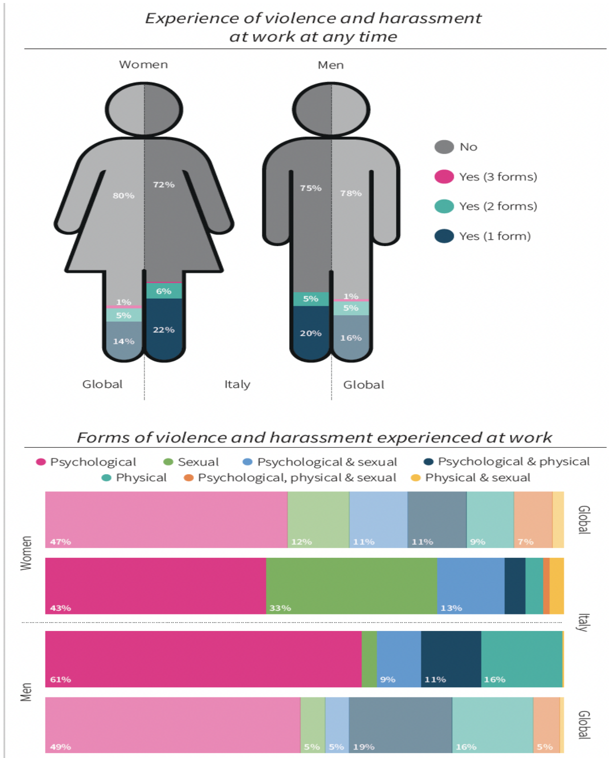
Appendices

Figura A1: Stringency in Employment Protection Legislation, OECD Index 2008



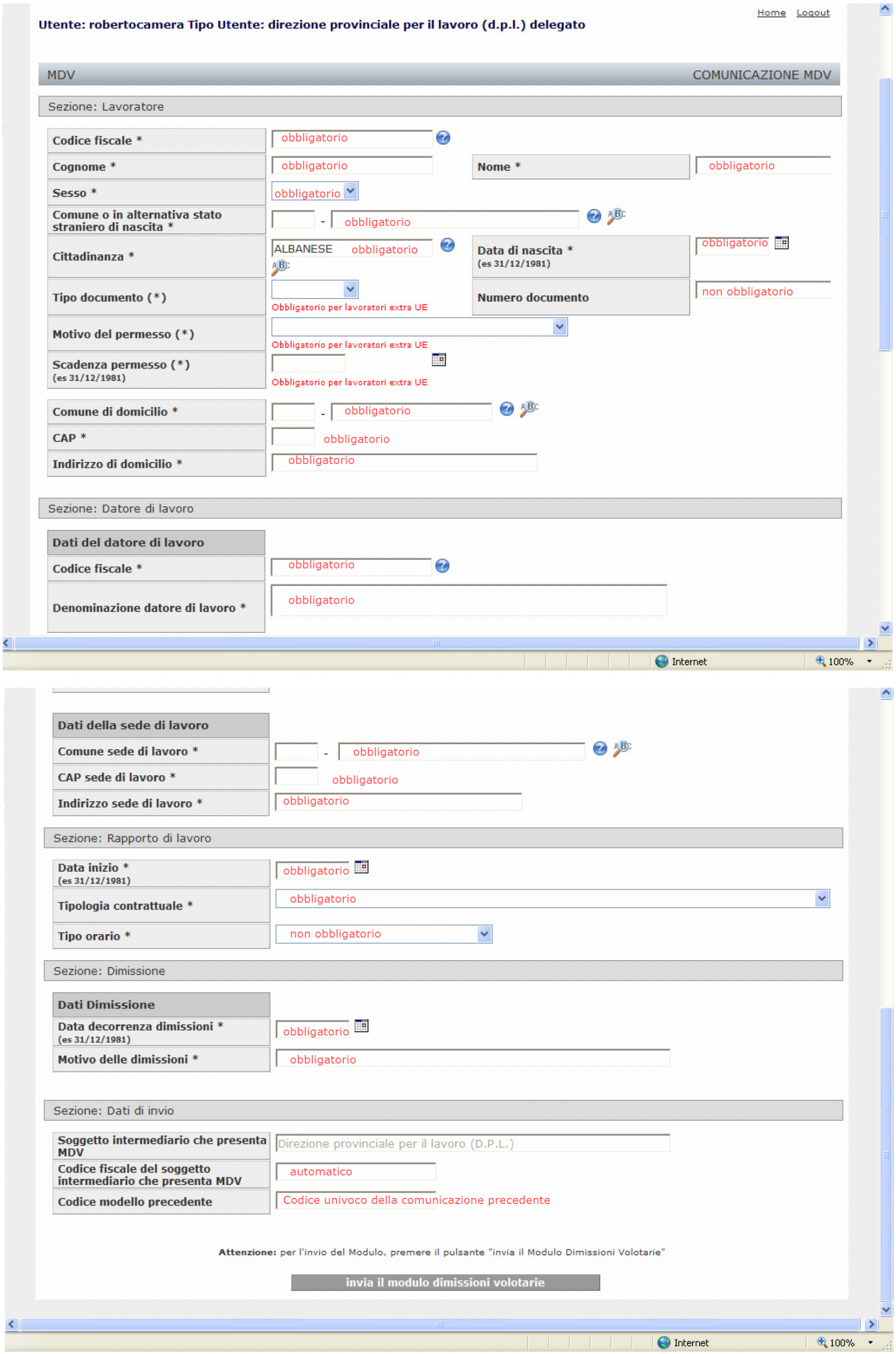
Notes: This figure compares stringency of employment protection legislation across OECD countries using the OECD index 2008 available at OECD index.

Figura A2: World risk poll 2021: Experience of violence and harassment at work at any time.



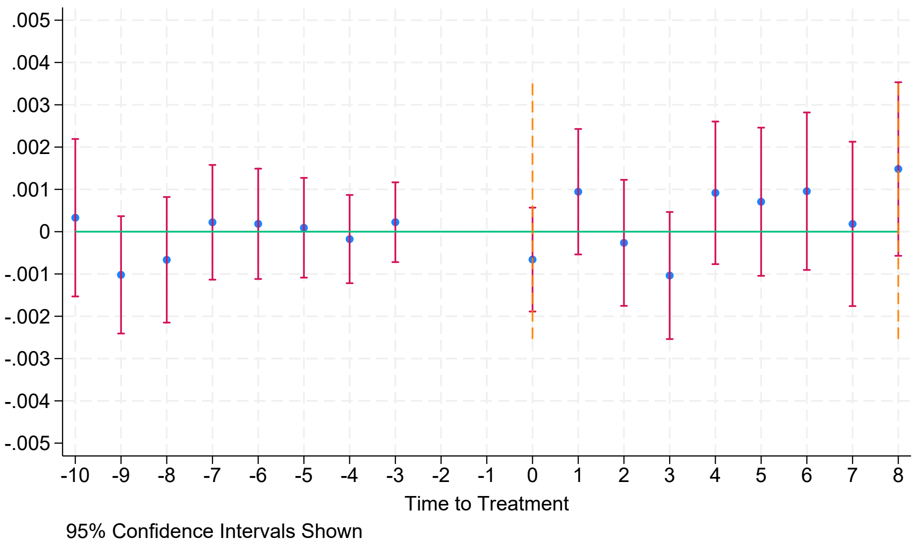
Notes: The figure depicts statistics for the frequency and types of of workplace violence experienced by 1,000 Italian respondents. In 2021, the survey was administered in 121 countries by the Lloyd's Register Foundation and the International Labour Organization (ILO). It is the first survey to to assess individuals' experiences of violence and harassment in the workplace. More than 125,000 people were interviewed using probability-based random sampling to ensure nationally representative data and results. As shown in the bottom part of the figure, respondents were specifically asked about three key manifestations of violence and harassment in the workplace: experience of physical violence and harassment, psychological violence and harassment, and sexual violence and harassment. For additional detail see Safe at Work? Global experiences of violence and harassment.

Figura A3: Example of the online resignation procedure implemented by the Law No. 188/2007).



Notes: The Figure reports an example of the online resignation procedure that workers had to fill in with the approval of the Law No. 188/2007.

Figura A4: Event study estimates on the share of new hires at the firm-level



Notes: This figure plots estimates of the effect of 10 leads and 9 lags of the reform on the number of resignations. Estimates are obtained from Equazione 2. All effects are relative to the two-months period before the reform (i.e., time to treatment=-1 and -2). The regression includes year fixed effects, firm fixed effects and firm specific linear trends. 95% confidence intervals are shown. Standard errors are clustered at the firm-level.

Tabella A1: DiD estimates on the composition of the newly hired workforce

	Outcome: Share of new hires		
	(1)	(2)	(3)
	Female	Migrant	Low tenure
TreatxPost	-0.023	0.018	-0.024
	(0.018)	(0.012)	(0.023)
Month FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm linear trends	Yes	Yes	Yes
Observations	3,701,031	3,701,031	3,701,031

Notes: This table reports DiD estimates of our main specification (see Equazione 1) on the share of new hires in the 18 months after the reform approval that are (i) females (Column 1) (ii) migrants (Column 2) and have (iii) low experience (Column 3). We classify workers as having low experience if they have worked for less than 14 years. Specifications include year fixed effects, firm fixed effects and firm linear trends. Standard errors clustered at the firm-level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.