LABOUR

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Labor Market Imperfections and Reforms: Evidence From Italy

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ABSTRACT

This paper investigates the impact of two major labor market reforms enacted in Italy during the 2010s, namely the 2012 Fornero reform and the 2015 Jobs Act, on labor market imperfections. Building on recent methodological advances in the estimation of markups and markdowns, we estimate firm-level measures of the labor wedge, defined as the ratio between the marginal revenue product of labor and its unit cost. Exploiting the institutional setup of the reforms, which affected firms above or below a 15-employee threshold differently, we adopt a difference-in-differences framework to assess whether the reduction in employment protection legislation altered the degree of labor market imperfections. The results show that the reforms favored firms over their employees, in line with a simple theoretical framework predicting an increase in the distance between labor compensation and productivity following a reduction in employment protection legislation. The effect is found to be stronger vis-à-vis white-collar workers compared to blue-collar workers.

JEL Classification: D22, J31, J38, J42, L19

1 | Introduction

Building on recent advances in the analysis of markups and markdowns in factor markets (Syverson 2025), this paper analyzes the effects of Italy's 2010s labor market reforms on labor market imperfections, with particular attention to heterogeneity between white- and blue-collar workers.

During the last decade, the availability of detailed firm-level data, often including matched information on firms' workers, and rapid methodological advances in the estimation of price–cost margins (starting from De Loecker and Warzynski 2012), have led to increased interest in the presence and evolution of market power. The analysis has moved beyond the traditional industrial organization literature and scholars have widened the scope of their focus, moving from markups in the output market, that is, the ability to set a price above the marginal cost, to markdowns in factor markets, equivalent to the fact that firms manage to pay factors below their marginal product.[1]

Italy is an interesting case study both because of its well-known labor market imperfections (see, e.g., Ichino and Riphahn 2005; Boeri and Garibaldi 2007; Sestito and Viviano 2018) and due to the attempts to inject more flexibility into the system (see, e.g., Boeri and Garibaldi 2019; Gnocato et al. 2020). Between 2012 and 2015, two reforms were implemented with the aim of reducing hiring and firing costs by lowering employment protection legislation (EPL) in the case of large companies. The legislative changes only affected a specific group of firms, namely those with more than 15 employees; this makes it possible to use these reforms as quasi-natural experiments and exploit the size threshold to determine their impact on the ratio between the marginal product of labor and the wage. This labor wedge is our measure

of labor market imperfections. More specifically, the paper asks whether the reforms reduced imperfections, whether the resulting changes favored employers or employees, and whether the effect differed between white-collar and blue-collar workers.

A simple theoretical framework shows that the reforms can be thought of as a reduction in the cost of adjustment faced by firms, and this drives labor wedges up because the share of labor costs associated with firms' market power increases when adjustment costs go down. Empirical results indicate that labor market reforms indeed increased wage wedges, with a stronger effect on white- than on blue-collar workers.

By focusing on labor wedges rather than standard outcomes such as employment, contract types, and job flows, the paper complements existing evidence on the effects of Italian labor market reforms. It also speaks to the recent literature on monopsony and wage markdowns, which measures firms' ability to pay workers less than their marginal revenue product of labor (Yeh et al. 2022; Berger et al. 2024, 2026). Thus, our contribution is to study for the first time, to the best of our knowledge, how an institutional reduction in firing costs affects this wedge, which, in the present setting, should not be interpreted as a pure measure of monopsony power: it captures a broader set of labor market imperfections, including adjustment costs, bargaining frictions, and wage-setting power.

The remainder of the paper is organized as follows: Section 2 provides background information on the Italian labor market and describes the reforms. Section 3 outlines the theoretical framework, while Section 4 presents the data and the empirical methodology. Section 5 reports the main results, including robustness checks and extensions. Finally, Section 6 provides a broader discussion of the findings and presents some concluding remarks.

2 | Labor Market Reforms in Italy

2.1 | Institutional Setting

The Italian labor market has undergone significant reforms in the 21st century. We focus on the "Fornero" reform of 2012 (named after the cabinet minister Elsa Fornero who proposed the legislation) and the 2015 "Jobs Act." Among other things, the reforms introduced changes to the rules on dismissals, making it easier for companies to lay off workers in the event of economic difficulties. The new rules were intended to reduce uncertainty about the costs of dismissal by limiting the instances in which workers could be reinstated by a court order, providing monetary compensation instead (Cirillo et al. 2017; Boeri and Garibaldi 2019).

Before 2012, Article 18 of the 1970 Workers' Statute made a clear distinction between large companies, namely those with more than 15 employees, and smaller ones. The legislation mandated that workers could not be dismissed without a "justified objective reason" related to the employee's conduct or to economic, organizational, or production reasons. In case of unjustified dismissal, firms had to reinstate workers, although this provision was more stringent for large firms, whereas small companies had more leeway to fire workers.

The availability of reinstatement as the primary legal remedy, together with uncertainty over judicial rulings and the duration of legal proceedings, increased expected firing costs for firms above the statutory employment threshold. These higher costs, in turn, may have discouraged firms from expanding their workforce beyond the cutoff and contributed to distortions in the firm size distribution around the 15-employee threshold (Schivardi and Torrini 2008).

The Fornero reform and then the Jobs Act substantially reduced EPL for employees in large firms while leaving it unchanged for small companies. The Fornero reform of 2012 introduced the first modification to this regime, restricting reinstatement rights to cases of discriminatory firing and a subset of disciplinary dismissals deemed "manifestly unfounded" for firms with more than 15 employees. In all other cases, including most economic dismissals, reinstatement was replaced by monetary compensation. However, the classification of the dismissal (e.g., economic vs. disciplinary) and the determination of compensation remained partly subject to judicial discretion. Consequently, while the reform reduced the likelihood of reinstatement, it did not entirely eliminate the uncertainty surrounding the firing costs faced by firms above the statutory threshold.

The 2015 Jobs Act further modified this framework by introducing a standardized severance scheme for newly hired workers under a new open-ended "increasing protection" contract (*contratto a tutele crescenti* in Italian). In the event of unjustified dismissal, compensation was explicitly linked to tenure through a predetermined formula, substantially reducing judicial discretion. However, reinstatement remained applicable in specific circumstances, such as discriminatory dismissals. The reform aimed to attenuate the discontinuity in expected firing costs at the 15-employee threshold by making dismissal costs more predictable.

Both reforms mandated a general abandonment of judicial reinstatement in favor of monetary transfers (predetermined by law and proportional to job tenure), thus extending to all firms the system previously limited to small firms and providing large firms with much greater flexibility to adjust their labor input. However, the two reforms differed in their scope of application. While the EPL reduction introduced by the Fornero reform applied to both preexisting and newly established open-ended contracts, the tenure-based dismissal rules introduced by the Jobs Act applied only to workers hired after its implementation. As a result, any effects of the Jobs Act's EPL reduction are likely to materialize gradually, as the share of workers covered by the new regime increases over time.

In addition, the reforms were accompanied by hiring incentives, although these differed in scope and generosity. These subsidies reduced the cost of permanent contracts and encouraged the conversion of temporary positions into open-ended ones. Under the Fornero reform, incentives were targeted to specific worker groups (young workers, women, and unemployed above the age of 50), while the Jobs Act provided for broader and more generous subsidies that applied across all firms and worker categories (Boeri and Garibaldi 2019; Cirillo

et al. 2017). This institutional setting provides the basis for our empirical strategy.

2.2 | Related Literature

Other papers have exploited the differential implementation of the Fornero reform and the Jobs Act across small and large firms to study their effects on various labor market outcomes (Cirillo et al. 2017; Sestito and Viviano 2018; Boeri and Garibaldi 2019; Pigini and Staffolani 2022; Ardito et al. 2023; Zeli and Nascia 2025) and other socio-economic dimensions, such as credit access (Mistrulli et al. 2023) or fertility (De Paola et al. 2021; Cerruti et al. 2023).

Cirillo et al. (2017) find that monetary incentives have played a key role in explaining the increase in open-ended contracts, and this increase is mainly driven by transformations rather than new hiring. Moreover, a relevant share of new open-ended positions is part-time, and the increase in employment is concentrated among older workers and in low-skilled and low-tech service sectors. Sestito and Viviano (2018) suggest that the EPL reform has had a separate and positive effect on hiring above and beyond the positive impact of employment subsidies, and the new rules made companies slightly less risk-averse, offering permanent positions to yet untested workers. These studies are in line with additional evidence showing that institutional rigidities can affect firms' allocative efficiency and workforce composition. Gianfreda and Vallanti (2017) show that judicial delays in labor trials act as a de facto increase in firing costs. These delays reduce job turnover and dampen labor productivity for Italian firms exceeding the 15-employee threshold and thus subject to judicial reinstatement. By reducing these rigidities, the Fornero reform led to an improvement in job match quality and an increase in productivity through enhanced labor reallocation (Berton et al. 2017). However, Francesconi and Sonedda (2024) highlight that the reform also increased the cost of job loss by reducing re-employment probabilities, especially for young workers and people residing in Southern regions. Pigini and Staffolani (2022) document an increase in both hiring and firing activity after the Jobs Act, but the same (or even lower) probability of employment termination for workers hired under the new rules over the medium term (3 years and a half). Their explanation for this result is that the reduction in firing costs allows firms to better screen workers, improves matching, and raises labor productivity. Furthermore, Bertoni et al. (2023) show that the 2015 Jobs Act further eroded protection for new hires in large firms, increasing perceived job insecurity and triggering a differentiated response. Workers in low-quality firms actively pursued job-to-job mobility toward higher-paying positions, whereas those in high-paying companies intensified their work effort to secure their current employment.

Boeri and Garibaldi (2019) confirm the differential effect of hiring subsidies, taken up more intensively by small firms, and show that lower firing costs led to more job terminations, concentrated among large companies. In contrast, Zeli and Nascia (2025) do not detect a significant impact of the measures either on employment levels or on changes among flexible workers. Ardito et al. (2023) analyze the combined and distributional effects of lower firing costs and hiring incentives. They show that small firms reacted immediately to the subsidies, whereas larger firms adjusted only once these were coupled with reduced employment protection. The reforms encouraged small firms to transform temporary contracts into permanent ones, while larger firms largely maintained fixed-term arrangements, possibly to test new hires. The benefits of the reforms were stronger for domestic and more educated workers, as well as for prime-age and older individuals, with no significant gender differences.

De Paola et al. (2021), Cerruti et al. (2023) and Mistrulli et al. (2023) analyze additional dimensions of the reforms looking at the potential adverse effect of increased job insecurity on access to credit, fertility, and household formation. They find that the reduction in job security has reduced the leverage ability of Italian workers and has lowered the fertility rate.

In parallel, recent work using the "production function approach" (De Loecker and Warzynski 2012) to estimate markups and markdowns documents the existence of a significant wedge between the cost of labor and its marginal product in Italy (Caselli et al. 2023). Moreover, Caselli et al. (2025) show that there are sizable differences between white- and blue-collar employees: the former are paid less than their marginal revenue product, while the latter obtain more than their contribution to firms' revenues. The recent empirical literature uses production-function methods to measure labor wedges, usually interpreted as wage markdowns. Yeh et al. (2022) estimate plant-level markdowns in U.S. manufacturing by comparing the marginal revenue product of labor with labor compensation, while using flexible inputs to separate labor-market power from product-market markups. Berger et al. (2026) place this evidence in a broader macro perspective, emphasizing that markdowns may have aggregate consequences when they are concentrated among large employers and when firms' hiring decisions affect the allocation of workers across firms.

Our analysis is complementary to this literature. Building on the production function approach and in consideration of the recent debate on wage markups and markdowns (Syverson 2025), we investigate the impact of the Italian labor market reforms on labor wedges and labor market imperfections. Rather than estimating the overall level or trend of labor wedges, we exploit institutional variation generated by the labor market reforms to study whether a reduction in firing costs changes these wedges and whether these effects differ between white- and blue-collar workers.

3 | Theoretical Framework

We build on Hashemi et al. (2022) and assume a situation in which a firm maximizes its profits, allowing for both labor market power and labor adjustment costs. Without loss of generality, labor is assumed to be the only factor of production, and we distinguish between white- and blue-collar workers (W and B):

$$\max_{B,W} \left[R(B,W) - P^B(B)B - P^W(W)W - \Psi^B(B) - \Psi^W(W)\right] \tag{1}$$

where $R(B,W)$ represents revenues, $P^B(B)$ and $P^W(W)$ the wage paid to blue- and white-collar workers (which may vary with the

number of employees, giving firms some degree of monopsony power), and $\Psi(.)$ is a convex function of labor adjustment costs. The first-order conditions of the profit maximization problem are:

$$\frac{\partial R(B,W)}{\partial B} = P^B(B) + \frac{\partial P^B(B)}{\partial B}B + \frac{\partial \Psi^B(B)}{\partial B}; \tag{2}$$

$$\frac{\partial R(B,W)}{\partial W} = P^W(W) + \frac{\partial P^W(W)}{\partial W}W + \frac{\partial \Psi^W(W)}{\partial W}. \tag{3}$$

A key concept in our analysis is the "labor wedge" (μ), defined as the ratio between the marginal revenue product of labor and its unit cost. For a generic labor type $L \in \{B, W\}$, the wedge can be expressed as:

$$\mu^L = \frac{\text{MRP}^L}{C^L/L} \tag{4}$$

with $C^L = P^L(L)L + \Psi^L(L)$ being total costs for labor type L. The wedge captures all market imperfections that drive labor costs away from the frictionless benchmark $P^L = \text{MRP}^L$, such as hiring and firing costs, efficiency wages, monopsony power, minimum wage laws and the like. In other words, under perfect market conditions, the wedge equals 1.

In the present context, we focus on two sources of departures from the perfect market hypothesis, namely labor market power and adjustment costs. Values of $\mu^L > 1$ signal a wedge in favor of firms (e.g., due to a wage markdown), while $\mu^L < 1$ indicates that the unit cost of labor is higher than its marginal revenue product, either because firms share rents with their workers (or workers extract rents from the firms) or because adjustment costs are large. Note that in the absence of any adjustment costs, the labor wedge is simply the ratio of the marginal revenue product of labor to the wage, as discussed for instance in Caselli et al. (2025).

Starting from Equations (2) and (3), and following Hashemi et al. (2022), the labor wedge μ^L can be expressed as a cost-weighted average of the relevant structural elasticities ν^L and κ^L, with $L \in \{B, W\}$:

$$\mu^B = \frac{\text{MRP}^B}{C^B/B} = 1 + \frac{P^B B}{C^B}\nu^B + \frac{\Psi^B}{C^B}\kappa^B \tag{5}$$

$$\mu^W = \frac{\text{MRP}^W}{C^W/W} = 1 + \frac{P^W W}{C^W}\nu^W + \frac{\Psi^W}{C^W}\kappa^W. \tag{6}$$

In Equations (5) and (6), $\nu^L = \frac{\partial P^L}{\partial L} \cdot \frac{L}{P^L}$ is the inverse elasticity of labor supply and $\kappa^L = \frac{\partial \Psi^L}{\partial L} \cdot \frac{L}{\Psi^L} - 1$ captures the sensitivity of adjustment costs.

Modeling labor market reforms as an exogenous change in the adjustment cost $\Psi(.)$ for any level of employment, we can express the impact of the reforms on the labor wedges as:

$$\begin{aligned}\frac{\partial \mu^B}{\partial \Psi^B} &= -\frac{P^B B}{(C^B)^2}\nu^B - (\kappa^B + 1)\frac{C^B}{(C^B)^2} + \kappa^B\frac{C^B - \Psi^B}{(C^B)^2} \\ &= -\frac{P^B B}{(C^B)^2}\nu^B - (\kappa^B + 1)\frac{\Psi^B}{(C^B)^2} - \frac{P^B B}{(C^B)^2} \\ &= -(1+\nu^B)\frac{P^B B}{(C^B)^2} - (\kappa^B + 1)\frac{\Psi^B}{(C^B)^2} < 0\end{aligned} \tag{7}$$

$$\frac{\partial \mu^W}{\partial \Psi^W} = -(1+\nu^W)\frac{P^W W}{(C^W)^2} - (\kappa^W + 1)\frac{\Psi^W}{(C^W)^2} < 0. \tag{8}$$

The inequality stems from the fact that, for $L \in \{B, W\}$, $\kappa^L > 0$ and $\nu^L > 0$ since we allow for monopsony power by firms. Hence, according to Equations (7) and (8), the model predicts that a reduction in the cost of adjustment should drive the labor wedges up. This occurs because as adjustment costs go down, the share of labor costs associated with firms' market power increases.

Bringing the prediction to the data faces the difficulty that the labor wedge is unobservable, as we cannot directly measure the marginal revenue product of labor. To provide an empirical content to the labor wedge, we follow the production function approach to markup estimation (De Loecker and Warzynski 2012) and write μ^L as the ratio between the revenue elasticity of labor ($\gamma^L = \text{MRP}^L\frac{L}{R}$) and the share of labor costs on revenues ($\alpha^L = C^L/R$). Starting from Equation (4), we write:

$$\mu^L = \frac{\text{MRP}^L}{C^L/L}\frac{LR}{LR} = \frac{\gamma^L}{\alpha^L}. \tag{9}$$

Since cost shares are easily computed using balance-sheet data, and revenue elasticities can be estimated using standard econometric techniques, Equation (9) allows us to derive a firm-specific value of the labor wedge.

4 | Data and Methods

4.1 | Revenue Function Estimation

In the previous section, we showed that labor wedges can be computed as the ratio of revenue elasticities (γ^B and γ^W) to revenue shares (α^B and α^W) for each worker type. While revenue shares can be computed directly from the data, revenue elasticities must be estimated. An advantage of this approach is that labor wedges (unlike price–cost margins on the output market) can be computed from nominal values and do not require knowledge about firm-level prices or quantities (Bond et al. 2021; Hashemi et al. 2022).

In this paper we adopt a second-order trans-log specification for the revenue function, with four inputs: white- (W) and blue-collar labor (B), intermediate goods (M), and capital (K).[2] This approach yields firm-level and time-varying revenue elasticities. Labor is measured in full-time equivalents, while materials and capital inputs are deflated using sector-specific indexes. The value of capital is obtained through the perpetual inventory method and that of intermediate goods is expressed as the sum of goods and services purchased by the firm in a given year.

As in Caselli et al. (2023) and Caselli et al. (2025), we assume that materials and both types of labor are flexible inputs and instrument them, as well as their interactions, with the first and second lags of the two types of labor and the second lags of capital and materials. This methodology, which builds on the control function approach to production function estimation (Olley and Pakes 1996; Levinsohn and Petrin 2003), addresses the main endogeneity issues commonly associated with such empirical endeavor

(Wooldridge 2009; Ackerberg et al. 2015). A separate estimation is run for each 2-digit Nace-rev. 2 sector of economic activity in order to balance sample size and sector-specific technologies.

4.2 | Data

The analysis exploits information on Italian limited liability companies from 2002 to 2019 (approximately 100,000 firms per year). Balance sheet data from Cerved Group are combined with information on the number of white- and blue-collar employees collected by the Italian Social Security Institute (INPS). Additional information on the data can be found in Supporting Information Appendix A.

Supporting Information Appendix B summarizes the estimates of the factor elasticities stemming from the revenue function at the two-digit sector level. Results are analogous to those presented in more detail by Caselli et al. (2025).

To distinguish labor market distortions across worker types, we estimate the revenue function parameters on firms with at least five employees, including at least one white-collar and two blue-collar workers.

Given our focus on the impact of labor market reforms on labor wedges, and the 15-employee threshold used to distinguish treated from untreated firms, we restrict the analysis to firms with 10 to 30 employees to enhance comparability.[3] They represent roughly 40% of the available observations, 30% of employment, and 25% of real value added across the sample period.

Table 1 presents some summary statistics on this subsample, distinguishing between firms that are below or above the 15-employee threshold. Larger companies tend to pay slightly higher salaries, with the difference larger for white-collar workers. On average, labor wedges are larger than 1 and similar between firms of different sizes.[4]

TABLE 1 | Descriptive statistics.

	All	Small	Large
	(1)	(2)	(3)
Observations	590,901	328,536	262,365
		(55.6%)	(44.4%)
Number of employees	16.42	11.63	22.41
μ^W	1.28	1.30	1.27
μ^B	1.03	1.04	1.02
P^W	39.53	37.74	41.78
P^B	32.23	31.84	32.72
MRP^W	45.48	43.45	48.03
MRP^B	31.83	31.63	32.08

Note: Simple averages on firms with a number of employees ranging between 10 and 30. P^W and P^B are the cost of white- and blue-collar workers (including security contributions). Column (2) includes information for companies below the 15-employee threshold, while column (3) reports mean values for firms above the threshold.

However, while the data show that labor costs and productivity are broadly aligned for blue-collar workers, white-collar workers display a productivity that is about 30% higher than their costs to the firm, giving rise to a sizable wedge in favor of employers.[5]

The level of the white-collar labor wedge is in line with recent production function estimates of markdowns. A value of μ^W equal to 1.28 implies that white-collar workers receive roughly 78% of their marginal revenue product. This magnitude is close to the 70%–80% range summarized by Berger et al. (2026) and to recent estimates by Berger et al. (2024), who find an average markdown of 21% using Norwegian data, despite not distinguishing between labor types. The difference between white- and blue-collar workers is in line with Bachmann et al. (2022), who document larger markdowns for jobs involving nonroutine tasks in Germany.

4.3 | Empirical Strategy

We exploit the institutional feature of the reforms, in particular the differential impact on firms below/above the 15-employee threshold, to implement a Difference-in-Differences (DiD) approach and study, first, whether the reforms have had a significant impact on labor wedges and, secondly, whether the effect has been different across white- and blue-collar workers. More specifically, we implement the following regression:

$$\mu_{it}^L = \delta_0 + \delta_1 1\left(n_{it-1} > 15\right) + \delta_2 \left[\text{Fornero}_t \times 1\left(n_{it-1} > 15\right)\right] + \delta_3 \left[\text{JobsAct}_t \times 1\left(n_{it-1} > 15\right)\right] + \lambda_i + \lambda_t + \varepsilon_{it} \tag{10}$$

where the dependent variable, μ_{it}^L, is a measure of labor wedges for worker-type $L \in \{W, B\}$ by firm i at time t, $1\left(n_{it-1} > 15\right)$ is a binary variable equal to 1 if firm i had more than 15 employees in year $t-1$, Fornero_t is an indicator variable equal to 1 for years between 2012 and 2014, JobsAct_t is an indicator variable equal to 1 for years greater than 2014, λ_i and λ_t are firm and time fixed effects. We restrict the sample to the period 2007–2019 in order to have periods of similar length before and after the reforms.

The coefficients of interest (δ_2 and δ_3) compare labor wedges before and after the reforms and across different firms, some of which are affected by the change in legislation (i.e., they are treated because they are above the threshold of 15 employees), while others are not (i.e., those below the threshold of 15 employees). It is worth stressing that the DID specification in Equation (10) takes the pre-Fornero setting as the reference period for both policy interventions. The coefficient δ_3 therefore represents the cumulative effect of the two reforms.

To test for parallel trends in the pretreatment period, we adopt the following event-study specification:

$$\mu_{it}^L = \delta_0 + \sum_{t=2007}^{2019} \delta_t \left[1\left(n_{it-1} > 15\right) \times \lambda_t\right] + \lambda_i + \lambda_t + \varepsilon_{it} \tag{11}$$

where variables and indexes are defined as in Equation (10). Figure 1 presents the results of the event-study analysis described in Equation (11).

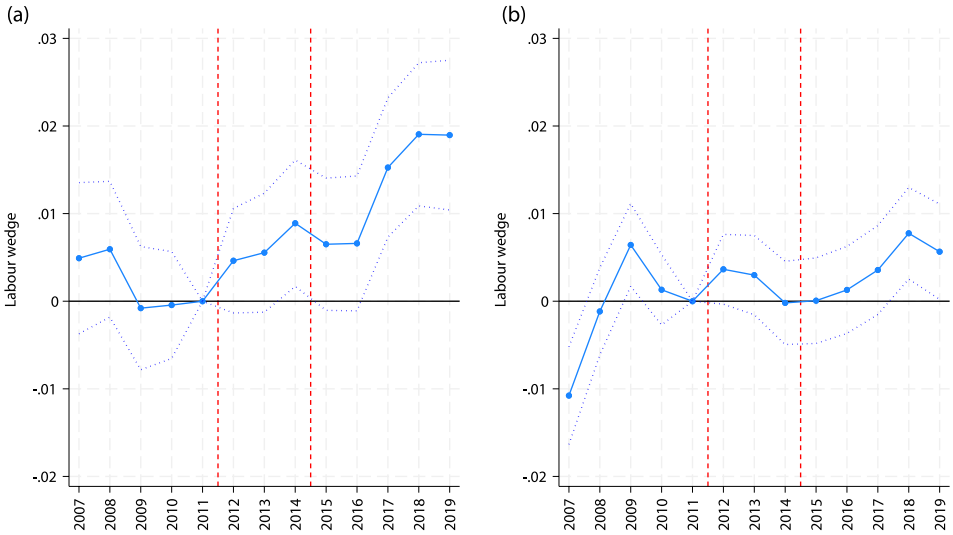

FIGURE 1 | Event study analysis. (a) White-collar employees. (b) Blue-collar employees.

5 | Results

5.1 | Baseline

Table 2 reports estimates of the parameters δ from Equation (10). The results suggest that the labor market reforms had an impact on the labor wedge, moving it in favor of companies. In fact, positive values of $\widehat{\delta}_2$ and $\widehat{\delta}_3$ mean that treated firms exhibit higher values of the labor wedge relative to untreated firms after the change in EPL corresponding to the two reforms. Estimates point to a higher effect of the Fornero reform on white-collar compared to blue-collar workers, and to a further moderate increase in labor wedges for both worker types after the Jobs Act. The size of the coefficient indicates a 0.5% increase in the labor wedge vis-à-vis white-collar workers after the Fornero reform. The coefficient doubles to 1.1% after the Jobs Act. In the case of blue-collar workers the overall effect reaches 0.4% in total. Since the labor wedge declined by approximately 8% for both types of workers over the period 2007–2019 (Caselli et al. 2025), a 1% movement in the opposite direction may appear small but it is economically meaningful.

We also distinguish between results for all sectors (columns 1–2) and for manufacturing firms only (columns 3–4), as production and revenue functions are typically more clearly defined and measurable in the manufacturing sector. Among manufacturing firms, we confirm a positive effect of the Jobs Act on white-collar employees, whereas the impact on blue-collar workers is limited to the Fornero reform and only borderline significant.

Figure 1 confirms that both reforms had a muted impact on blue-collar workers: estimated coefficients for the interaction terms are close to zero and standard errors quite large. In contrast, the left panel suggests a significant increase in labor wedges for white-collar workers, especially since 2017, as larger firms managed to widen the difference between wages and labor productivity. It is worth noting that, since both reforms were implemented during the year, in July 2012 for the Fornero reform and March 2015 for the Jobs Act, the first treatment year is only partially exposed to the new regulatory regime. Consistent with this, the estimated coefficients for 2012 and 2015 are less significant than those pertaining to the following years. More in general, Figure 1 indicates that statistically significant results emerge only toward the end of the sample period. The empirical strategy cannot properly distinguish between a lagged effect of the first reform (that only materializes after a few years) and the impact of the Jobs Act. However, taking into consideration the different scope of the two policy interventions, with the first applying to all existing contracts and the second only to new hires, results in Figure 1 are consistent with a genuine effect from the Jobs Act, which takes time to materialize as the share of employees covered by the EPL reduction increases, rather than a delayed effect of the Fornero reform.[6]

5.2 | Donut Estimator

To address the possible concern of strategic behavior by companies, which may decide to remain immediately below or above the 15-employee threshold, we repeat the exercise excluding firms with between 14 and 16 employees. The intuition behind this approach (often called the "donut" design in the context of regression discontinuity, see for instance Barreca et al. 2011) is that if sorting or heaping near the treatment threshold does not create substantial distortion, then excluding data in a neighborhood of the treatment should not change the estimation results.[7]

Table 3 confirms our previous results, both from a qualitative and a quantitative point of view. The labor market reforms of the 2010s increased the labor wedge, tilting the balance slightly in favor of employers, with a stronger effect on white-collar workers and not much difference between the manufacturing sector and the rest of the economy.

5.3 | Placebo

As a further robustness check, we repeat the event study analysis but arbitrarily change the treatment threshold. In particular, we split the sample into two separate groups: firms with a

TABLE 2 | Results—baseline specification.

	All sectors		Manufacturing	
	Log μ^W	Log μ^B	Log μ^W	Log μ^B
	(1)	(2)	(3)	(4)
$1(n_{it-1} > 15)$	0.023***	0.006***	0.029***	0.008***
	(0.002)	(0.002)	(0.004)	(0.002)
$1(n_{it-1} > 15) \times$ Fornero	0.005**	0.003	0.002	0.004*
	(0.003)	(0.002)	(0.004)	(0.002)
$1(n_{it-1} > 15) \times$ JobsAct	0.011***	0.004**	0.011**	0.004
	(0.003)	(0.002)	(0.005)	(0.003)
Observations	557,776	557,776	249,618	249,618
Firms	97,464	97,464	39,005	39,005
R^2	0.802	0.824	0.781	0.802

Note: The table reports estimates of the parameters δ from Equation (10); clustered standard errors in parentheses. $1(n_{it-1} > 15)$ is a binary variable that identifies firms treated by the Fornero and Jobs Act labor market reforms. Columns (1) and (2) report results for all firms with 10 to 30 employees, while columns (3) and (4) restrict the sample to firms in the manufacturing sector. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

TABLE 3 | Donut estimator.

	All sectors		Manufacturing	
	Log μ^W	Log μ^B	Log μ^W	Log μ^B
	(1)	(2)	(3)	(4)
$1(n_{it-1} > 15)$	0.032***	0.006***	0.036***	0.006
	(0.004)	(0.002)	(0.006)	(0.003)
$1(n_{it-1} > 15) \times$ Fornero	0.005*	0.001	0.003	0.004
	(0.003)	(0.002)	(0.004)	(0.003)
$1(n_{it-1} > 15) \times$ JobsAct	0.012***	0.001	0.014**	0.000
	(0.004)	(0.002)	(0.006)	(0.003)
Observations	430,934	430,934	193,954	193,954
Firms	87,555	87,555	35,626	35,626
R^2	0.815	0.833	0.797	0.812

Note: The table reports estimates of the parameters δ from Equation (10); clustered standard errors in parentheses. $1(n_{it-1} > 15)$ is a binary variable that identifies firms treated by the Fornero and Jobs Act labor market reforms. Sample restricted to companies with 10–30 employees, excluding those with 14 to 16 employees. Results in columns (3–4) only consider manufacturing firms. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

number of employees above 10 but below 14; firms whose size ranges between 16 and 30. Within each of the sub-samples, we set a "placebo" treatment threshold at the median number of employees, namely 11 and 21. This setup allows us to compare companies that are similar but for which the labor market reforms under investigation should have no differential effect because both groups are either treated (those above 15 employees) or not treated (those below 15).

Figure 2 summarizes the results and shows no significant effect of the two reforms when using these arbitrary thresholds. As expected, the labor wedge shows no clear-cut evolution and no marked change after the introduction of the new legislation.

5.4 | Decomposition

Overall, the estimation results are broadly consistent with the implications of the model presented in Section 3. To dig deeper into the mechanisms driving the effect of the labor market reforms on the labor wedge, Table 4 presents a decomposition of the overall impact (for each category of workers) into its two components: the marginal revenue product and labor costs.

Specifically, we implement the following regression:

$$Y_{it}^L = \delta_0 + \delta_1 1(n_{it-1} > 15) + \delta_2 [\text{Fornero}_t \times 1(n_{it-1} > 15)] + \delta_3 [\text{JobsAct}_t \times 1(n_{it-1} > 15)] + \lambda_i + \lambda_t + \varepsilon_{it} \quad (12)$$

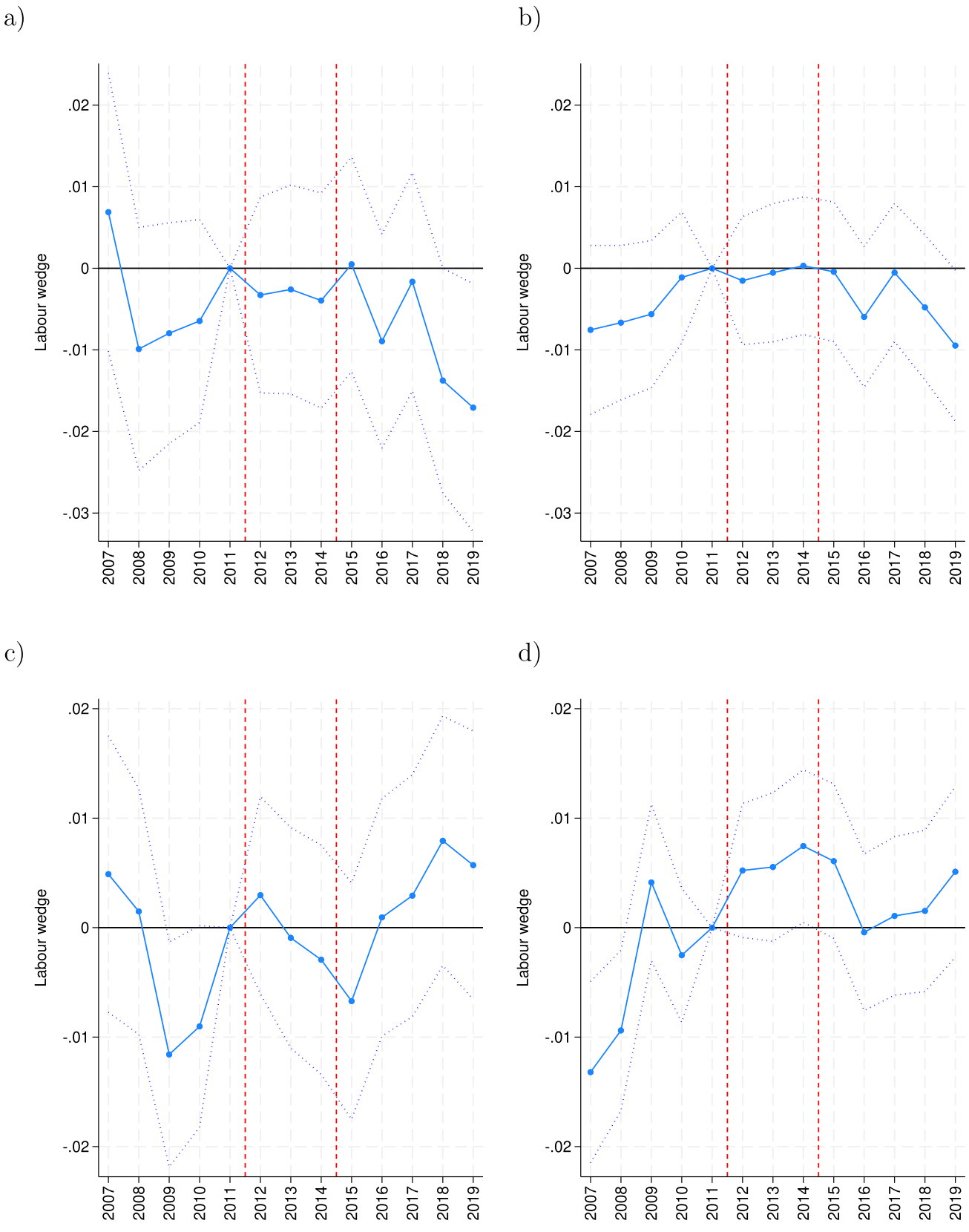

FIGURE 2 | Placebo event study. Top panel: Companies with 10–14 employees (treatment threshold set at 11). Bottom panel: Companies with 16–30 employees (treatment threshold set at 21). (a) White-collar employees. (b) Blue-collar employees. (c) White-collar employees. (d) Blue-collar employees.

TABLE 4 | Decomposition: Wages and productivity.

	White-collar workers			Blue-collar workers		
	Log μ^W	Log MRP^W	Log P^W	Log μ^B	Log MRP^B	Log P^B
	(1)	(2)	(3)	(4)	(5)	(6)
$1(n_{it-1} > 15)$	0.023***	0.042***	0.019***	0.006***	−0.007***	−0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
$1(n_{it-1} > 15) \times$ Fornero	0.005**	0.000	−0.005***	0.003	−0.001	−0.003***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
$1(n_{it-1} > 15) \times$ JobsAct	0.011***	0.008***	−0.003	0.004**	0.004**	0.000
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	557,776	557,776	557,776	557,776	557,776	557,776
Firms	97,464	97,464	97,464	97,464	97,464	97,464
R^2	0.802	0.871	0.826	0.824	0.888	0.890

Note: The table reports estimates of the parameters δ from Equation (12); clustered standard errors in parentheses. $1(n_{it-1} > 15)$ is a binary variable that identifies firms treated by the Fornero and Jobs Act labor market reforms. Sample restricted to companies with 10–30 employees. Columns (1–3) reports estimates for white-collar workers; columns (4–6) reports estimates for blue-collar workers. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

where the dependent variable Y_{it}^L is in turn: log μ^L, log MRP^L, or log P^L, for $L \in \{W, B\}$, and the remaining variables are defined as in Equation (10).

Although in the aftermath of the first reform, labor costs went down, the driving force behind the larger impact of the Jobs Act appears to be an increase in the marginal revenue product of labor that is not matched by a similar increase in labor compensation. The impact is larger for white-collar workers, but significant (although only half in magnitude) also in the case of blue-collar workers. The increase in the marginal revenue product of labor is consistent with the literature showing that a reduction in firing costs leads workers to increase mobility and firms to improve job match quality, thereby increasing workers' productivity (Berton et al. 2017; Bertoni et al. 2023).[8]

6 | Conclusions and Discussion

Our results suggest that the 2012 Fornero reform and the 2015 Jobs Act widened labor wedges between the marginal revenue product of labor and its unit cost, during a period in which overall market dynamics were moving in the opposite direction (Caselli et al. 2025). Since white-collar employees are, on average, paid less than their marginal product of labor ($\mu^W > 1$), the reduction in EPL appears to have widened distortions in this segment of the labor market, increasing the size of the wedge and slowing convergence toward the competitive benchmark.

The simple theoretical framework in Section 3, which incorporates both monopsony power and adjustment costs, suggests that reforms reducing firing costs can push the labor wedges upward by increasing the relative importance of the component of labor costs associated with firms' wage-setting power. The interpretation of EPL as a source of labor adjustment costs is supported by evidence by Gianfreda and Vallanti (2017), who show that (before the reforms) longer court delays over labor disputes have a detrimental effect on labor productivity for firms above the 15-employee threshold. Indeed, our analysis indicates that higher labor wedges are driven by improvements in the marginal productivity of labor that are not fully reflected in labor costs.

Hence, the reforms appear to have increased productivity without a corresponding rise in labor costs, resulting in wider labor wedges. This suggests that labor market reforms causing a reduction in firing costs can have efficiency-enhancing and distributional effects at the same time: they may allow firms to become more productive while also reducing the share of the gains that are passed on to workers.

While the empirical analysis is consistent with the implications of the model, additional explanations for the increase in labor wedges may be linked to an endogenous increase in the inverse elasticities of labor supply (ν^W and ν^B). Among the possible reasons are (i) reduced workers' bargaining power; (ii) better matching due to enhanced ability to hire and fire workers; (iii) lower outside options. First, lower firing costs make it cheaper and less risky for firms to terminate employment, thus lowering the bargaining position of employees. Facing lower job security, workers may be less able to demand higher wages, becoming less responsive to changes in working conditions. As a result, companies may be able to provide employees with smaller wage adjustments for productivity increases. Second, lower firing costs give firms more ability to experiment with hiring, as termination of employment in the case of a bad match is easier (Sestito and Viviano 2018). Because firms can more easily fire unproductive workers, they are better able to pay wages that reflect the marginal productivity of labor, which in turn leads to a greater ability to depress wages for workers with low outside options. In fact, Berton et al. (2017) document both an increase in the probability of a good match and a small increase in productivity following the Fornero reform. Third, previous works on the impact of the

Jobs Act (Cirillo et al. 2017; Boeri and Garibaldi 2019) indicate that the monetary incentives provided by the government to favor open-ended contracts over temporary ones –introduced in parallel with the labor market reforms under scrutiny– led to several "transformations" (rather than new hirings). This suggests that some may have offered, and some workers accepted, a trade-off between enhanced job security and less favorable salary conditions.

An additional question that arises from our results is why we observe a larger and more significant increase in the wedge for white-collar workers (μ^W) compared to blue-collar ones (μ^B). Looking at Equations (7) and (8), one candidate explanation is that white-collar labor supply is less elastic than that of blue-collar workers ($\nu^W > \nu^B$). In fact, the empirical evidence presented in Caselli et al. (2025) shows that white-collar workers face larger markdowns, which is consistent with the higher inverse elasticity of labor supply. As a result, the reduction in labor adjustment costs introduced by the reforms may have a more muted effect on μ^B and be more pronounced for μ^W.

Caselli et al. (2025) discuss a number of possible reasons why firms may have more monopsony power vis-à-vis white-collar workers relative to production workers. These channels are also relevant here because both the level of the labor wedges and the impact of the labor market reform–as shown in Equations (7) and (8)–depend on the value of the inverse elasticities of labor supply (ν^B and ν^W). Possible explanations include: the fact that blue-collar employment has already undergone substantial outsourcing, meaning that workers who have retained their jobs have a relatively strong bargaining position (Landesmann and Leitner 2023); the greater importance of job-specific human capital for nonroutine cognitive tasks, which makes white-collar workers less likely to change jobs (Bachmann et al. 2022); and the role of job differentiation, which makes different jobs imperfect substitutes and drives down the value of equilibrium wages (Azar and Marinescu 2024).

Building on these findings, future research could investigate some of the endogenous mechanisms that can explain both the evolution of the labor wedges and the differential impact of labor market shocks across worker types.

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Conflicts of Interest

The authors declare no conflicts of interest.

Endnotes

[1] Of course, it is possible that factors of production are paid *above* their marginal product, in case factor owners exert market power or there is bargaining over the rent generated in output markets (Mertens 2020; Caselli et al. 2021).

[2] This section draws from Caselli et al. (2025).

[3] We have experimented with various intervals around the 15-employees threshold. Overall, results are robust to the choice of the interval.

[4] Yeh et al. (2022) find a positive but gradual size-gradient in markdowns, with substantial overlap in confidence intervals across adjacent size classes. As such, the 15-employee cutoff used in Table 1 is likely too coarse to detect meaningful size-related differences in labor wedges.

[5] See Caselli et al. (2025) for a broader discussion of the estimates of the labor wedges for the two types of workers and their evolution over time.

[6] Our empirical analysis does not aim to provide a direct comparison between the effects of the Fornero reform and those of the Jobs Act. When we run a specification in which the Fornero indicator equals one for all years after 2012, so that the coefficient on the Jobs Act dummy captures the additional post-2015 effect, the results (available upon request) show that the coefficient on the Jobs Act dummy is simply equal to the difference of the coefficients on the Fornero reform and the Jobs Act dummies and it is statistically significant at the 5% level for white-collar workers. However, this estimate cannot be interpreted as the causal effect of the Jobs Act alone, as it may also reflect delayed effects of the Fornero reform.

[7] Although recent work by Noack and Rothe (2023) warns about the potential costs of this approach, which tends to reduce efficiency and may introduce biases, the large sample size at our disposal makes this robustness check worth considering.

[8] The positive effect on marginal revenue productivity might suggest differential productivity trends between firms above and below the threshold. However, a robustness check that includes TFP as a control yields results nearly identical to those in Table 2. We exclude TFP from our main specification because it is an endogenous outcome of firm choices (and so likely a 'bad control') which could potentially bias the coefficients of interest. Furthermore, event study analyzes (analogous to Figure 1) for both MRP^W and MRP^B show no evidence of pretrends, alleviates concerns about differential productivity dynamics across treated and control firms and thus supporting the validity of the DiD design. These additional results are available upon request.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section. **Supporting Information: Appendix A.** Data sources. **Supporting Information: Appendix B** Labor Wedges & Production Function Estimation.