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RFBerlin
ROCKWOOL Foundation Berlin –
Institute for the Economy
and the Future of Work

Gormannstrasse 22, 10119 Berlin
Tel: +49 (0) 151 143 444 67
E-mail: info@rfberlin.com
Web: www.rfberlin.com

Collective Bargaining and the Wage Structure*

Lorenzo Cappellari† Bernardo Fanfani‡

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Abstract

We study how updates in pay floors set by collective bargaining agreements (CBAs) shape the wage structure in Italy. We estimate stacked event-panel difference-in-differences models around changes of contractual minima and trace distributional impacts. Pay-floor hikes of 2.2% on average raise mean log FTE daily wages by 2.2%, but effects are near zero at the 10th percentile and stronger at the 90th, implying inequality-enhancing wage-rate responses. This asymmetry reflects both within-agreement heterogeneity, as lower-paid workers within CBAs respond less, and between-agreement heterogeneity, as low-wage CBAs exhibit weaker pass-through. Non-compliance with pay floors is higher in low-wage CBAs, thus it is a potential driver of asymmetries even if its level is not affected by wage updates. Pay-floor hikes reduce employment and days worked only among low-wage workers and only among full-time jobs, which may further contribute to the muted wage response in the lower tail through selection mechanisms.

Keywords: collective bargaining, contractual minimum wages, wage structure.

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†Università Cattolica del Sacro Cuore. Address: Dipartimento di Economia e Finanza, Largo Gemelli 1, I-20123 Milano, Italy. Email: lorenzo.cappellari@unicatt.it

‡Università di Torino and Collegio Carlo Alberto. Address: Dipartimento di Scienze Economico-Sociali e Matematico-Statistiche, corso Unione Sovietica 218b, I-10134 Torino, Italy. Email: bernardo.fanfani@unito.it

1 Introduction

The determinants of the secular rise in wage and earnings inequality observed in several OECD countries have been studied extensively. In this vast literature, an important open question concerns the role played by labor market institutions. A seminal contribution by Di Nardo et al. [1996] attributed between 40% and 50% of the growth in the U.S. 50/10 wage gap between 1979 and 1988 to the decline in the real minimum wage. Lee [1999] attributed an even stronger role to this mechanism, emphasizing spillover effects of the minimum wage extending above its statutory level. More recently, Autor et al. [2016] partly downplayed the importance of the minimum wage for U.S. pay inequality relative to earlier estimates, while Fortin et al. [2021], adapting the Di Nardo et al. [1996] approach to more recent years, appears to confirm a prominent role of the real minimum wage level in determining the evolution of lower-tail wage inequality. Evidence on the role of minimum wages in shaping the wage structure has also been produced for other countries. Recently, Bossler and Schank [2023] attribute about half of the decrease in monthly wage inequality in Germany since 2010 to the introduction of a statutory minimum wage in 2015.

Notably, this literature has mostly focused on national or state-wide minimum wages uniformly established by law. An alternative institution designed to protect pay levels is collective bargaining, which is quite widespread among OECD countries (Jäger et al. [2024]). Importantly, collective bargaining remains relevant despite an overall decline in union density in advanced economies, since many countries extend the coverage of collective contracts to workers and firms that are not members of signatory associations (OECD [2019]). In systems characterized by collective bargaining, minimum pay floors are typically set through centralized negotiations between unions and employers' associations, often industry-specific. Moreover, pay floors are usually set not only at the bottom of the distribution but also for mid- and high-paid occupations along a contractual wage ladder. For these reasons, the impact of updates in minimum pay floors set by collective bargaining agreements (CBAs) on the wage structure is likely to be broader –and potentially qualitatively different– than the impact of changes in the level of state-wide minima that primarily "bite" at the bottom.

In this paper, we provide the first evidence on the causal effect of pay floors updates set by CBAs on the wage distribution. The analysis focuses on Italy, an ideal case study that has attracted considerable attention in the recent literature (e.g. Dustmann et al. [2025]). Italy is characterized by centralized industry-specific wage setting, with virtually full coverage due to the automatic extension of collective agreements to non-signatory parties. Empirically, we combine comprehensive administrative earnings records with a re-constructed archive of contractual minimum wages set by Italian CBAs. We estimate the distributional effects of CBA pay-floor changes using stacked two-period difference-in-differences comparisons and RIF regressions. This approach allows us to trace how pay-floor hikes shape the wage structure. We further connect these reduced-form patterns to evidence on non-compliance and employment adjustments.

Indirect evidence on the importance of collective bargaining in shaping the size and evolution of wage inequality has been provided by several studies. Card et al. [2013] argue that decentralization in the German system of collective bargaining may have played an important role in the growth of West-German wage inequality between the 1980s and 1990s. Devicienti et al. [2019] and Boeri et al. [2021] argue that the absence of similar reforms in Italy may have prevented the growth of firm pay-premium inequality during the same period, also limiting geographic wage differentials. Moreover, cross-country studies seem to confirm a link between the level of pay inequality and the presence of centralized collective bargaining, which tends to mitigate its level (*e.g.* Blau and Kahn [1996]; Garnero [2021]).[1] While this evidence suggests that collective bargaining matters for inequality, it does not directly identify how changes in bargained pay floors translate into changes in the wage distribution.

A recent and growing literature has provided more specific evidence on the dynamics and direct effects of centrally bargained minimum wages. This development has been possible mainly because of the collection of detailed information on collective agreements and its linkage with high-quality information on pay levels derived from administrative sources. Studies on the relationship between contractual wage growth and several firm and worker outcomes—

[1]Besides collective bargaining coverage, declining unionization rates have been pointed out as a related institutional factor driving higher inequality, particularly in the U.S. context (*e.g.* Card [1996]; Fortin et al. [2021]).

including average pay levels—have emerged for several countries.[2] However, this literature has not yet provided direct causal evidence on how changes in bargained pay floors affect the *pay structure*, that is, how they reshape wage and earnings outcomes at different points of the distribution. Our analysis fills this gap by estimating the effects of CBA minimum-wage changes at several wage quantiles, allowing us to identify where along the pay distribution contractual pay-floor hikes have the strongest impact.

Our analysis covers the period between 2006 and 2017. We first document trends in Italian wage and earnings inequality during this period, showing that inequality in FTE daily wages was mostly flat and even slightly declining, and real wage levels grew slightly. By contrast, the monthly earnings inequality increased sharply, due to a flat real trend at the top and middle of the distribution and a strong decline at the bottom. In particular, the 10th percentile of monthly earnings decreased by approximately 15% over twelve years. We show that a fall in average days worked per month –especially among low-wage workers– was a central factor behind the divergence between wage and earnings inequality.

We then turn to the causal effect of changes in bargained pay floors on the distribution of daily FTE wages and monthly earnings. Building on distributional methods typically employed in the minimum wage literature (e.g. Di Nardo et al. [1996], Fortin et al. [2021], and Bossler and Schank [2023]) we implement an estimation approach tailored to our context of staggered contractual renewals. Our empirical strategy stacks adjacent-period comparisons around CBA pay-floor hikes, contrasting agreements that experience sizeable minimum-wage increases with agreements whose minima remain unchanged over the same time. This approach is closely related to recent difference-in-differences methods for staggered treatment timing and heterogeneous effects, including de Chaisemartin et al. [2025], and to the stacked event-study framework used by Cengiz et al. [2019].

Our approach is robust in the presence of treatment effect heterogeneity and avoids comparing treated and control groups that are many years apart, thereby avoiding the pitfalls of traditional TWFE methods emphasized in the recent minimum wage literature (Dube and

[2] European countries covered by this recent literature include, among others, France (Avouyi-Dovi et al. [2013]), Italy (Fanfani [2023]; Faia and Pezone [2024]), Norway (Bhuller et al. [2022]), Portugal (Card and Cardoso [2022]), Spain (Adamopoulou et al. [2025]). Several other middle-income economies, such as Argentina (Hermo [2023]), have also been studied using similar data sources on collective bargaining.

Lindner [2024]). This approach also allows us to implement placebo tests, comparing the outcome evolution in the treated and control groups prior to the pay floor update. Our estimates identify the effect of changes in bargained pay floors on the wage distribution. They should therefore not be interpreted as the effect of collective bargaining as an institution relative to alternative wage-setting regimes, such as firm-level bargaining or statutory minimum wages alone.

Our results show that updates in bargained pay floors influence the pay structure in ways that differ from minimum floors set only at the bottom of the distribution. Whereas higher statutory minima have often been found to compress lower-tail inequality, bargained minima increases tend to generate stronger wage-rate responses toward the top. Minimum-wage hikes of around 2.2% on average do not have a statistically significant effect at the 10th percentile, increase average daily wages by 2.2%, and raise the unconditional 90th percentile by approximately 4.5%. Thus, hikes in bargained pay floors tend to increase wage inequality, with an estimated positive effect of 4.1% on the 90-10 percentile gap . When considering monthly earnings, contractual pay-floor hikes have a more uniform impact, although point estimates remain larger at higher percentiles; for this outcome, implied changes in the 90-10 and 50-10 gaps are positive but not statistically significant.

We further show that the distributional asymmetry on daily wages reflects both within- and between-contract components. When focusing on the internal wage distribution of CBAs, we still find an asymmetric effect, although it is less pronounced than in the unconditional distribution. This implies that lower-paid workers within CBAs respond less to pay floor hikes. On the other hand, contract-specific average treatment effects are positively correlated with the median pay level of the agreement, suggesting that differences in the effectiveness of pay floor hikes between high- and low-wage CBAs also contribute to the overall pattern.

Finally, we explore two mechanisms that can rationalize the asymmetric effects of pay floor updates along the daily-wage distribution. First, we consider non-compliance with CBAs. In Italy, CBA provisions are not strictly enforced, and firms are subject mainly to random controls or to judicial rulings on pay adequacy initiated by workers or trade unions. Previous survey-based evidence indicates a non-negligible proportion of employees reporting earnings below the level implied by the prevailing collective contract, between 5 and 20 percent depending on the industry (Garnero [2018]). We develop an administrative lower-bound measure of non-compliance and find an average level around 5.8%, unevenly distributed across provinces and concentrated in lower-wage CBAs. Non-compliance does not respond significantly to renewals, suggesting that underpayment reflects relatively stable differences across firms, workers, contracts, and local labor markets rather than short-run adjustments to contractual pay-floor hikes. However, the higher prevalence of non-compliance in weaker segments of the labor market represents a potential driver of asymmetric wage responses to contract renewals.

Second, we study employment effects using a stacked two-period difference-in-differences approach on aggregated data. Measuring employment along the extensive (number of workers) and intensive (days worked) margins, we find significant negative effects only in the lower tail of the wage distribution. This pattern suggests that negative selection among workers at the bottom of the wage distribution in contracts experiencing pay schedule increases may bias estimates of wage elasticities downward. Employment effects are also significant only among full-time workers, whereas part-time employment is largely unaffected. The absence of employment effects among part-time workers suggests that, where non-compliance is more prevalent and easier to implement –as is often the case for these contracts– adjustment along the employment margin is more limited, consistent with the pattern observed for wages.

2 Data and Descriptives

This paper combines two data sources. The first is the administrative archive of the Italian Social Security Institute (INPS), which covers the universe of employees in the private non-agricultural sector. This sector represents approximately two-thirds of the total employment in Italy and roughly 85% of all employees. It is also the segment of the labor market in which national industry-level collective bargaining agreements are the relevant wage-setting institution.

The INPS data provide monthly information on earnings, days worked, and a rich set of worker and job characteristics. Crucially for our purposes, employers are required to report the national collective agreement (*contratto collettivo nazionale*) that applies to each employment spell. We use this information to merge the INPS records with a hand-collected archive of contractual minimum wages covering about 160 national industry-level agreements, which is our second data source. Through this merge, we are able to assign a time-specific bargained minimum wage to approximately 75% of private-sector employees over the 2006–2017 period.

Starting from the monthly administrative records, we build an individual-level panel of actual wages and bargained minima at a semiannual frequency. Specifically, for each year between 2006 and 2017, we retain observations for May and November. This choice reflects both measurement and design considerations. From a measurement standpoint, these months tend to have a relatively low incidence of vacation-related interruptions, and observed pay is typically closer to base pay, i.e. the wage component directly regulated by collectively bargained minima. By contrast, one-off payments and contractual bonuses —most notably the 13th and 14th monthly wages— are generally paid in June, July, or December, depending on the agreement. From a design standpoint, focusing on two regular months per year delivers a parsimonious panel structure while preserving the relevant variation in the timing of minimum-wage updates and wage adjustments.

Given the size of the matched worker-level panel, we further restrict the estimation sample to a 25% random sample of workers, identified by month of birth (March, July, and November). This sampling rule substantially reduces the number of observations while preserving representativeness with respect to the underlying employee population.

The descriptive statistics for the resulting analysis sample are reported in Table 1. The final sample includes more than 52 million worker-semester observations and over 5 million individuals. Men account for approximately 60% of the sample, around a third of workers are employed in manufacturing, and the share of part-time employment is 24%, in line with national labor market statistics.

3 Background Evidence: Wage Inequality and Contractual Minimum Wages

Before turning to the causal analysis, we provide background evidence on the two objects at the core of the paper: the evolution of pay dispersion and the dynamics of bargained pay floors. We first compare trends in FTE daily wages and monthly earnings, and then document how contractual minimum wages vary across CBAs and over time.

3.1 Wage Inequality in Daily Wages and Monthly Earnings

We begin by documenting the evolution of wage and earnings inequality in the estimation sample over the period covered by our analysis.

Figure 1 reports the evolution of selected percentiles of the distribution of real full-time-equivalent (FTE) daily wages (left panel) and real monthly earnings (right panel), expressed as log changes relative to 2006. The left panel shows limited changes in the dispersion of daily wages over time. Real daily wages increase in all selected percentiles and the overall profile is broadly similar across the distribution, especially after the early part of the sample period. If anything, growth at the bottom of the distribution is only slightly stronger than at the median and at the top, implying at most a modest reduction in inequality in daily wage rates.

The picture is markedly different for monthly earnings. In the right panel, earnings at the median and at the top of the distribution display moderate cumulative growth, whereas earnings at the bottom decile decline relative to their 2006 level and remain persistently lower for most of the sample period. As a result, the dispersion in monthly earnings broadens substantially, particularly in the lower half of the distribution.

The contrast between the relatively stable distribution of daily wages and the pronounced divergence in monthly earnings suggests that changes in labor input, rather than pay rates alone, played an important role. Figure 2, which plots the average number of days worked in FTE per month in the first and fourth quartiles of the daily wage distribution, supports this interpretation. Workers in the highest wage quartile show a remarkably stable profile, with average FTE days worked remaining close to 24 days per month throughout the sample period. By contrast, workers in the bottom wage quartile experience a clear decline in labor input, from about 21.5 FTE days per month at the beginning of the period to around 19.5 by the end. This widening gap in days worked across the wage distribution helps explain why inequality in monthly earnings increases much more than inequality in daily wage rates.

3.2 Contractual Minimum Wages Across Collective Agreements

In the INPS data, each worker can be deterministically matched to one of about 160 national collective agreements. Each agreement typically sets multiple contractual pay floors, which differ by occupation, skill level, seniority, and job classification. The upper panel of Table 2 reports descriptive statistics on actual wages and contractual minimum wages in the estimation sample, including the minimum, median, and maximum pay floors defined within each collective agreement.

The actual wages are, on average, about 18% higher than the median contractual minimum wage. At the same time, the difference between the average maximum and average minimum contractual pay floors set by CBAs is close to 60%, a magnitude comparable to the cross-worker standard deviation of actual wages reported in the table. This pattern indicates that individual agreements regulate occupations located at very different points of the pay distribution and therefore contain substantial within-contract wage structure.

Within each job grade, the contractual minimum is a fixed component of the base pay. An increase in the grade-specific minimum should therefore raise the contractual component of pay for all workers classified in that grade, including those whose actual wage exceeds the applicable floor. Contractual updates are generally coordinated across the wage ladder: as shown in Table 2, changes in the minima of different job grades within the same CBA are highly correlated. At the same time, these increases need not be identical in nominal or proportional terms across grades.

Table 2 shows a positive gradient in nominal minimum-wage growth across the distribution of contractual pay floors. The annualized nominal growth is about 1.8% at the bottom of the pay-floor distribution and increases to approximately 2.0% at the median and 2.2% at the top. Despite these differences, nominal changes in pay floors are highly synchronized within the same agreement: the correlation in pay floor growth across job grades is around 0.85–0.90 when calculated over periods with positive changes in the median floor. This strong within-CBA comovement makes the median contractual minimum a good proxy for the overall direction and timing of contractual pay-floor changes of a given contract, without implying that every worker covered by the agreement receives exactly the same nominal or proportional increase.

During the sample period, the real contractual minimum wages increase on average. As reported in Table 2, the average semi-annual growth in the real median contractual minimum wage is approximately 0.3%, which corresponds to roughly 0.6% annually and about 7.2% cumulatively over the entire analysis period. Figure 3 illustrates these dynamics for the two largest collective agreements in the private sector, trade, and metal manufacturing (which together cover close to a fourth of private sector employees). Each line in the figure corresponds to an occupation-specific contractual pay floor (job title) and plots its cumulative real growth relative to January 2006.

Three features emerge from the figure. First, real pay floors increase over time in both agreements, but the timing and magnitude of the increases differ substantially between sectors. In particular, the trade agreement exhibits a temporary decline in real pay floors around 2010–2012 and loses ground relative to metal manufacturing, before only partially catching up later in the sample. Second, real minimum-wage growth differs between job titles within the same agreement. As Figure 3 shows, in the trade contract, the cumulative real growth ranges approximately from 4% to 13%, while in metal manufacturing it ranges from approximately 7% to 12%. Third, in both agreements, the distance between the higher and lower contractual pay floors increases over time, consistent with the stronger nominal growth observed at the top of the pay floor distribution in Table 2.

These descriptive patterns highlight two key features of the institutional environment. On the one hand, contractual minimum wages exhibit substantial and heterogeneous variation across agreements and over time, with staggered timing of renewals and non-uniform real growth. However, changes in contractual minima are strongly correlated across job titles within a given agreement. This combination of within-contract comovement and between-contract heterogeneity provides a useful source of variation to study how bargained wage floors affect the distribution of actual wages and earnings.

4 Estimation Approach

The object of interest in this paper is distributional: we ask whether CBA pay-floor hikes affect the bottom, the middle, and the top of the pay distribution differently. For this purpose, we use recentered influence function (RIF) regressions, following Firpo et al. [2009]. The RIF approach provides a convenient way to relate covariates or policy changes to distributional statistics of an outcome, such as quantiles, by estimating linear models for the recentered influence function associated with the statistic of interest. In our setting, this allows us to estimate the effect of contractual minimum-wage changes at different points of the wage and earnings distributions within the same regression framework used for mean outcomes. This approach has become standard in recent individual-level analyses of how minimum-wage policies shape the pay distribution, including Fortin et al. [2021] and Bossler and Schank [2023].

This method is based on an OLS regression where the outcome is a functional of a given distribution statistic of interest. Let y_{it} represent the natural logarithm of full-time-equivalent (FTE) daily wages, or monthly income. We focus on several statistics for y_{it}: the mean, the θth percentiles (with $\theta = 10, 50, 90$) and the 90-10, 90-50 and 50-10 percentile gaps. The RIF-regression, when the distributional statistic of interest is the outcome mean, corresponds to the standard OLS regression, that is, $RIF_{\text{mean}} = y_{it}$. Instead, when the distribution statistic of interest is the θth percentile, the RIF is defined as

$$\text{RIF}_\theta(y_{it}) = y_\theta - \frac{\theta - 1[y_{it} \leq y_\theta]}{f_Y(y_\theta)}$$

where y_θ is the θth quantile of the wage distribution, $f_Y(y_\theta)$ is the wage density at the point y_θ, and $1[y_{it} \leq y_\theta]$ is an indicator function for wage levels below or equal to the θth quantile.[3]

[3] RIF_θ can be computed by estimating the θth sample quantile and by estimating the density at y_θ using kernel methods.

4.1 A continuous-treatment starting point

The institutional setting naturally features a continuous treatment. Contractual minimum wages vary across CBAs and over time, and workers covered by a given agreement are exposed to changes in the level of the relevant contractual pay floor. To fix ideas, suppose one were to exploit this variation directly in two-way fixed-effects diff-in-diff specification. This would lead to regressions of the following form:

$$\mathrm{RIF}_J(y_{it}) = \beta^J cw_{it} + \gamma^J X_{it} + \delta_c^J + \alpha_i^J + \tau_t^J + e_{it}^J \tag{1}$$

In equation (1), $\mathrm{RIF}^J(y_{it})$ denotes the recentered influence function associated with statistic J of the outcome y_{it}, where y_{it} is either log FTE daily wages or log monthly earnings. Depending on J, the specification can therefore be read as a regression for the mean or for a selected quantile of the outcome distribution. The variable cw_{it} is the log contractual minimum wage applying to worker i in semester t. The vector X_{it} includes time-varying controls, namely an age cubic polynomial and indicators for part-time status, occupation, sector-by-local-labor-market cells, and open-ended contracts. The terms δ_c^J, α_i^J, and τ_t^J denote CBA, individual, and time fixed effects, respectively.

In practice, workers can be deterministically matched to a CBA, but not to a specific job title within the CBA wage grid. We therefore define cw_{it} as the minimum wage associated with the median job title in the agreement, and we use this as a proxy for the overall dynamics of contractual pay floors within that CBA. This approximation is supported by the descriptive evidence in Table 2, which shows that changes in pay floors across job titles within the same agreement are highly correlated. Hence, movements in the median contractual minimum wage provide a good summary of the renewal-induced wage-floor changes faced by workers covered by the agreement.

The formulation of equation (1) is useful because it clarifies the dose-response nature of the variation in bargained minima. The treatment variable of interest is continuous and units are always treated with a time-varying intensity. Indeed, minimum wages tend to be updated quite frequently. What is exploited for identification are changes in the level of contractual pay floors within CBAs over time, while absorbing permanent worker heterogeneity, persistent differences across agreements, and aggregate shocks. In this setting, the two-way fixed effect specification is quite natural, as it allows one to use the entire variation in the data. It also makes explicit how the RIF framework can be combined with an individual-level panel design to study distributional responses to contractual minimum-wage changes.

Although equation (1) is useful to conceptually frame the dose-response nature of our setting, in practice it is not the specification used for our estimates. Because contractual minima change frequently and at staggered times across CBAs, a conventional two-way fixed-effects specification with a continuous treatment is difficult to interpret in the presence of heterogeneous treatment effects and could suffer from the well known bias issues that may arise in such contexts (e.g. Goodman-Bacon [2021]). Moreover, it does not directly allow to test for the parallel trend hypothesis using a placebo specification. For these reasons, our main empirical strategy replaces equation (1) with a stacked diff-in-diff design described below.

4.2 Stacked Diff-in-Diff around Relevant Pay Floor Hikes

To overcome the biases of TWFE estimators in the presence of staggered treatments, we rely on the approach proposed by de Chaisemartin et al. [2025]. They study the case of a continuous treatment with always treated units and suggest comparing treatment-dose switchers to non-switchers by stacking panels of two consecutive time periods. Following this approach, we build separate rolling panels of two consecutive time periods that cover the entire window of observation (2006-2017). Within each panel, we define treated workers as those whose collective contracts median nominal minimum wage increased by at least 1% from period 1 to 2. This focus on relevant pay floor hikes has been adopted also in the minimum wage literature, e.g. in the stacked event-study specification of Cengiz et al. [2019]. We define control workers as those who did not experience any nominal growth of median minimum wages from period 1 to 2. We exclude remaining cases from estimation, that is workers who experienced some modest growth of nominal minimum wages, less than 1%.

The average nominal growth of the median contractual minimum wage between the first and second period of each panel is 2.19% in the treated group. When computing this intensity of treatment using the lowest minimum wage of the collective contract, instead of the median, the average growth is 1.92%. Instead, it is 2.33% when considering the highest minimum wage of the collective contract. This suggests that relatively low paid workers within a contract tend to have a less generous wage adjustment on average, but these differences are quantitatively small and the treatment tends to reflect a similar pay floor hike for all skill levels covered by the same collective agreement.

We stack all panels of two consecutive time periods described above and estimate the following regression model.

$$\mathrm{RIF}_J(y_{iT_p}) = \beta^J 1[\Delta cw_{T_p} > 1\%]1[T_p = 2] + \delta^J_{c_p} + \gamma^J X_{iT_p} + \alpha^J_{i_p} + \tau^J_{T_p} + e^J_{i_pT_p} \qquad T_p = 1,\ 2 \quad (2)$$

In the above equation, p denotes a panel dataset and T_p is a time by panel index. The parameter of interest is the coefficient β^J associated to the interaction between a treatment status indicator and a second period dummy variable. The other controls in the model are the same set of time-varying covariates X_{iT_p} as in equation (1), a collective contract by panel fixed effect ($\delta^J_{c_p}$), an individual by panel fixed effect ($\alpha^J_{i_p}$), and a time by panel fixed effect ($\tau^J_{T_p}$).[4] Because minimum wages are quite heterogeneous across collective contracts, we do not condition treatment-control comparisons on the baseline minimum wage, as this would imply a dramatically large loss of sample size.

The robust estimation approach of equation (2) allows testing the parallel trend assumption in a natural way. Following the approach proposed by de Chaisemartin et al. [2025], the absence of differential trends in the outcome between the treated and control groups can be tested whenever the overall time window covered by the data is longer than two periods. The placebo test compares future treated workers and control workers over the two periods preceding the actual treatment, restricting the sample to cases in which contractual minima are unchanged for both groups throughout this pre-treatment window. This fake treatment approach is based on the same specification of equation (2). It allows testing whether trends in the outcome that are not accounted for by other independent variables are systematically

[4] We cluster standard errors at the collective contract level.

different between the treated and control groups in a period where there were no differences in minimum wage trends. A significant coefficient would therefore signal differential pre-trends rather than an effect of the subsequent pay-floor increase.

4.3 Validity of the Research Design

The main identification assumption in the model of equation (2) is that, conditional on the independent variables, changes in contractual minimum wages are as good as random.

In Italy, contractual minimum wages are bargained by a few most representative trade unions and employers' associations by industry at the national level, usually every two years. These negotiations typically establish a series of gradual minimum pay increases that take place on certain dates that are planned in advance. Thus, minimum wage increases usually occur more frequently than every two years. The timing of minimum wage changes is quite uncoordinated across collective contracts. Indeed, frequent delays in negotiations often imply that renewal dates do not follow regular patterns. Moreover, during phases of negotiation delays, some contracts have automatic minimum wage adjustment clauses, which further contribute to the frequency and uncoordinated timing of minimum wage hikes.

Existing evidence on the determinants of minimum wage setting in Italy suggests that, despite the irregular timing and frequency of updates, the overall size and direction of pay floor growth tends to be quite harmonized between sectors, as it is highly sensitive to national inflation but rather rigid with respect to sectoral productivity (Fanfani [2025]). Macro evidence seems to support this notion. For example, Boeri et al. [2021] note that wage differences between the northern and southern Italian regions amount to only 4% on average, while productivity differences are as high as 30%. By contrast, in Germany, where the bargaining process has been decentralized since the 1990s, wages tend to reflect more closely local productivity.[5]

[5] Recent evidence has shown that since around 2011 a process of de-centralization of collective bargaining has started to take place in Italy as well, mainly through a gradual increase in the adoption of very small, non-representative collective agreements by a minority of firms, representing around 3% of private-sector employment according to the most recent estimates (Dustmann et al. [2025]). The current analysis does not consider non-representative agreements, given the absence of comprehensive information on their content. Thus, it is restricted to the far largest group of private-sector employment covered by a fairly centralized and representative collective contract.

The rigidity of real contractual wages suggests that differences in minimum-wage growth across CBAs are likely to reflect heterogeneity in the timing and frequency of contractual adjustments, rather than short-run sector-specific shocks that independently affect wages. Several institutional features support this interpretation. Minimum-wage adjustments are negotiated by unions and employer associations at the national sector level, a relatively centralized level of bargaining; increases are often implemented through pre-specified steps over the life of the agreement; and coordination across sectors is encouraged by the government's target inflation rate, which is meant to serve as a common reference in wage negotiations.

At the same time, the fact that some wage adjustments are anticipated could, in principle, raise concerns about sorting or anticipatory behavior. For example, workers or firms might respond to expected wage growth by moving across contracts, or contracts with scheduled increases might already be on different wage trajectories before the increase takes place. In our stacked two-period DiD framework, such concerns would be problematic if workers in future treated CBAs exhibited differential wage dynamics relative to workers in the control group before the minimum-wage hike. This is precisely what the placebo test is designed to assess. A non-significant placebo effect supports the identifying assumption that, conditional on fixed effects and controls, residual wage changes are not systematically different between workers subsequently exposed to a contractual minimum-wage hike and workers in the control group.

Our treatment is defined at the CBA level and therefore requires contractual pay-floor hikes to be relevant for a broad set of workers covered by the agreement, rather than only for those located immediately above a given floor. As discussed in Section 3.2, contractual minima are fixed components of base wages within job grades, which implies that workers who are paid more than the floor should also be affected by updates. Moreover, changes in grade-specific minima are highly correlated within the CBA wage ladder. A hike in the median contractual minimum therefore captures a broad CBA-level pay adjustment, although the precise nominal or proportional increase may differ across grades.

Consistent with these institutional features, Faia and Pezone [2024] show that top-up pay components above contractual minima play only a limited role in offsetting pay-floor increases in Italy. They find that the elasticity of wages to contractual minima is similar for workers paid close to the applicable floor and for those earning substantially above it.[6] This evidence supports our treatment definition, which captures exposure to a CBA-wide pay-floor adjustment. For the same reason, designs based on the bite of the minimum wage, which define an intensity of treatment dependent on whether a worker is close to the pay floor (e.g. Dustmann et al. [2021]) would not be well suited for the current application.

5 Results

This section presents the main evidence on how bargained increases in contractual minimum wages affect the level and distribution of pay. We begin by focusing on FTE daily wages, which isolate changes in wage rates and exploit the finest earnings measure available in the administrative data. Our benchmark results use the stacked 2 by 2 event-panel difference-in-differences design described in equation (2). The design compares collective agreements that experience a significant nominal increase in contractual minima of at least 1% at the median between two consecutive periods to agreements with unchanged minima over the same adjacent periods. We complement the benchmark specification with placebo tests implemented within the same stacked framework, providing a direct falsification check on differential pre-trends between treated and control groups in the semester leading up to the minimum wage hike.

5.1 Average and Distributional Effects of Pay Floor Updates on Daily Wages

Table 3 reports the estimated effect of contractual minimum-wage increases on mean log FTE daily wages. The estimate is 0.022 (s.e. 0.005), implying an increase in average FTE daily wages of about 2.2% in contracts where bargained minima rise by an average level of 2.19% at the median, relative to contracts with unchanged minima over the same adjacent periods. This estimate is obtained in a demanding specification: with individual by event panel fixed

[6] Related evidence on Italy documented by Adamopoulou et al. [2026] shows that wages tend to be more rigid to negative business cycle shocks the closer they are to the pay floor. This is consistent with the institutional setting, given that contractual minimum wages also act as a statutory minimum, leaving less adjustment margins available when workers are close to the floor.

effects, identification comes from within-worker changes across the two periods of each event panel; contract by event panel fixed effects absorb time-invariant differences across collective agreements within each panel; and time by event panel fixed effects net out aggregate shocks common to all contracts in a panel. The placebo estimate is very small (0.000, s.e. 0.006) and statistically indistinguishable from zero, supporting the absence of differential pre-trends between the treated and control groups before the event takes place.

Under the identification assumptions of the stacked design, the evidence is consistent with a causal reduced-form pass-through from bargained wage floors to average pay rates. The magnitude of the estimate is sizeable: the effect on average FTE daily wages is close to the average percentage increase in contractual minima among treated CBAs. Several considerations can help rationalize this result. First, the estimate is obtained from an individual-level design. Unlike specifications based on aggregated cells, which may attenuate the relationship between contractual minima and observed wages by averaging across heterogeneous workers and jobs, our approach exploits worker-level wage variation while absorbing individual-by-panel fixed effects. This allows the pass-through from contractual minima to pay rates to be measured more directly.

Second, this estimate captures relatively short-run adjustments occurring within at most one semester from the minimum wage hike. Alternative adjustment margins available to firms (such as a reduction in top-up components of the pay) may take more time to implement. Moreover, our estimates are based on wage rates paid in the months of May and November, when the salary tends to be close to its base level, given that most one-off pay premiums tend to be compensated in December, January, June or July. Thus, the large estimated treatment effect may also reflect the relative rigidity of the baseline pay level. Finally, the magnitude of our estimate may also derive from the characteristics of the Italian institutional setting, given that minimum wages are a fixed pay component, implying that a 1 Euro increase in the floor should be associated with a same amount mandated increase also for wages that are well above the minimum level.

Mean effects provide a useful summary, but they can conceal substantial heterogeneity between workers and across segments of the wage distribution. Since the central goal of this paper is to assess how collective bargaining shapes the wage structure, we now move beyond averages and examine distributional responses. Table 4 reports estimates obtained from RIF regressions evaluated at selected percentiles of the FTE daily-wage distribution. The estimates reveal a marked gradient: the effect at the 10th percentile is essentially zero (0.004, s.e. 0.004), while it is sizeable and precisely estimated at the median (0.024, s.e. 0.004), and larger at the 90th percentile (0.045, s.e. 0.022). Interpreted as approximate percentage changes, bargained minimum-wage increases raise the unconditional median by about 2.4% and the unconditional 90th percentile of the pay distribution by 4.5%, with no discernible change at the lower tail. This implies that the mean effect reported in Table 3, which is very close to the median effect, largely reflects wage growth around the center and top of the distribution rather than gains concentrated at the bottom. Placebo estimates are small and statistically insignificant for all percentiles, supporting the view that the distributional pattern is not driven by systematic differences in trends already in place before the treatment.

The combination of a negligible response at the 10th percentile and a substantially larger response at the 90th percentile implies that contractual increases provide a net inequality-enhancing effect within the FTE daily-wage distribution. The RIF regression approach allows for directly estimating the effect of the treatment on unconditional percentile gaps. The results of this model are reported in the lower part of Table 4, which shows that the growth of contractual minimum wages induces a change in the 90-10 gap of about 0.041 log points (or 4.3% of the unconditional 90-10 gap level). This treatment effect is significant, although imprecisely estimated. The effect on the 50-10 gap is about 0.021 log points (5.3%) and is also estimated more precisely. Instead, the effect on the 90-50 gap, while positive, is not statistically significant. These results reflect the fact that the marginal effect estimated for the 90th percentile, while providing a larger point estimate, is also associated with a larger confidence interval.

The distributional pattern reported in Table 4 cannot be completely ascribed to a differential intensity of treatment among relatively low- and high-wage workers within a given collective contract. As mentioned, increases in median minimum wages in treated contracts amounted to 2.19% on average, against an average growth of 1.92% when considering the lowest minimum wages and of 2.33% when considering the highest minimum wages of the treated contracts. These differences in treatment intensity alone, which amount to at most 0.4 percentage points on average between the highest and lowest paid occupations within contracts, cannot rationalize an increase in percentile gaps of 4-5%. This raises the question of whether the inequality-enhancing effect documented in Table 4 is mainly driven by heterogeneity across collective agreements, with higher-wage contracts exhibiting stronger responses, or whether it also arises within each agreement, reflecting differential responsiveness to minimum wage growth along the internal wage ladder.

5.2 Effects Heterogeneity Within and Between Collective Contracts

To investigate whether there is a relevant heterogeneity in the responsiveness to minimum wage growth within the internal wage ladder of collective contracts, Table 5 reports the results of a RIF regression model where wages are expressed as a difference from the contract and event-panel specific average. In this way, the RIF estimates can be interpreted as effects on unconditional quantiles of the within-contract wage distribution, abstracting from level differences across agreements. The results show that the asymmetry attenuates but does not disappear: the effect at the 10th percentile becomes positive (0.012, s.e. 0.004), while the median remains essentially unchanged (0.023, s.e. 0.004) and the 90th percentile is still larger (0.033, s.e. 0.013). All placebo effects are generally close to zero and never statistically significant.

Relative to Table 4, the implied increase in the 90–10 differential falls from about 0.041 to about 0.023 log points (a 2.7% increase), with an effect that is only marginally significant, while the implied increase in the 50-10 percentile falls from 0.020 to 0.013 (3.6%). The effect on the 90-50 gap is again positive but not statistically significant. Overall, given the reduction in the size of the effects on percentile gaps, this implies that a non-trivial share of the inequality-enhancing reduced-form effect of minimum wage growth reflects differences in responsiveness between relatively high- and low-wage contracts. However, an important component of this inequality-enhancing effect persists within the internal wage ladder of collective contracts as well.

Figure 4 investigates directly the heterogeneity in the effect of minimum wage growth between relatively high- and low-wage collective contracts. The figure plots contract-specific estimates of the pay floor hike effect on average wages, obtained by interacting the treatment by time variable in equation (2) with collective contract fixed effects and estimating the model through OLS. Each estimated treatment effect is plotted against the actual median FTE wage in the collective agreement. The diameter of the circles is proportional to the contract size and the 95% confidence intervals is reported by the vertical bars. The fitted relationship is upwards sloped, with a correlation coefficient of approximately 0.32. Therefore, Figure 4 documents a positive association between CBA-specific estimates of the average wage response and the agreement's median wage level.

As discussed in Section 3.2, CBAs establish grade-specific minima whose changes are highly correlated across the wage ladder but need not be identical in either nominal or proportional terms. Table 2 shows that contractual increases are, on average, somewhat larger for higher pay floors. This variation in treatment intensity may contribute to the observed distributional gradient, although its magnitude appears too limited to account for it fully.

The evidence instead points to a combination of within- and between-CBA heterogeneity. The within-CBA estimates show that the asymmetry persists, albeit in attenuated form, after removing level differences across agreements. Figure 4 additionally shows that CBA-specific estimates of the average response are positively associated with the agreement's median wage level. These patterns may reflect differences in the implementation and effective pass-through of contractual minima across workers and agreements.

The RIF estimates should also be interpreted as effects on quantiles of the unconditional wage distribution, rather than as average wage responses for workers initially located at a given rank. They may therefore incorporate re-ranking and changes in the composition of employment in addition to individual wage pass-through. We consequently view the upper-tail tilt as a reduced-form pattern generated by several margins rather than as the implication of a single mechanism.

5.3 *Effects on Monthly Earnings*

Having documented how contractual minima affect the distribution of FTE daily wages, we next turn to monthly earnings. This outcome is important in light of the descriptive evidence in Figure 1: while dispersion in daily wages has been relatively stable over time, inequality in monthly earnings widens substantially over time, driven in particular by the decline of earnings at the bottom decile relative to 2006. The contrast between daily wages and monthly earnings suggests that changes in labor input—rather than wage rates alone—play a central role in the long-run evolution of earnings inequality in Italy.

Table 6 reports RIF-regression estimates of the impact of contractual minimum-wage increases on the distribution of log monthly earnings. The shape of point estimates derived from this model is qualitatively similar to the one documented for daily wages. Treatment effects are positive across the distribution but stronger in the upper tail. At the 10th percentile, the estimated effect is 0.010 (s.e. 0.006); at the median it is 0.021 (s.e. 0.003); and at the 90th percentile it is 0.043 (s.e. 0.021). Placebo estimates are close to zero and not significant for all three percentiles, providing reassurance that the results are not driven by differential pre-trends around the event windows. The implied change in the 90–10 gap is of about 0.033 log points (or 2.2% with respect to the baseline level), but this effect is not statistically significant. Also the estimated effects on the 90-50 and 50-10 gap are positive (respectively 3.5% and 1.2%) but not significant. It follows that the growth in contractual minimum wages has a positive effect on earnings at all percentiles. Despite an asymmetric shape with an upward tilt of the point estimates, no statistically significant effects on percentile gaps are detected.

It is useful to clarify how these treatment effects relate to the patterns of monthly earnings described in Section 2. The descriptive evidence documents unconditional trends during 2006-2017 that reflect the combined influence of compositional effects and broad changes in wage rates and labor input between workers' groups. By contrast, Table 6 isolates the reduced-form response of monthly earnings to discrete contractual minimum-wage updates within adjacent-period event panels. This reduced-form estimate is obtained exploiting only within worker variation, thus it is conditioned on individuals being continuously employed over time, and cannot capture potential longer-run adjustments. The fact that earnings effects tend to be positive across the distribution implies that, at the horizon captured by the stacked design and among continuously employed workers, contractual wage-floor increases translate into higher monthly earnings. Instead, the secular rise in earnings inequality documented in descriptive statistics appears to be driven primarily by longer-term shifts in labor input and employment intensity.

Considered alongside the estimates for daily wages, the relatively stronger effect at the bottom of the monthly earnings distribution is suggestive of an increase in employment intensity among continuously employed low-wage workers following the pay floor update. However, the estimated effects at the 10th percentile are comparable in magnitude across the daily wage and monthly earnings specifications, and their confidence intervals overlap substantially. Consequently, there is little statistical evidence that the two effects differ.

5.4 Comparisons with the Existing Literature and Discussion

A direct comparison with the existing literature on collective bargaining is more informative when based on a continuous-treatment specification, since previous studies typically report elasticities of wages or earnings with respect to contractual pay growth using TWFE or first-difference panel regressions. We therefore also estimate the individual-level TWFE specification described in equation (1), treating the log contractual minimum wage as a continuous regressor. These estimates, available from the authors upon request, deliver an average daily-wage elasticity of 0.408 (s.e. 0.075). This magnitude is fully in line with existing evidence: Fanfani [2023] and Devicienti and Fanfani [2025] estimate average daily-wage elasticities of 0.43 and 0.26 for Italy using data aggregated at the local-sector or firm level; Faia and Pezone [2024] estimate an average monthly-earnings elasticity to contractual base wages of 0.7; and Card and Cardoso [2022] report wage elasticities for Portugal ranging between 0.2 and 0.77 depending on the demographic group considered. Importantly, the continuous-treatment estimates also preserve the distributional pattern documented above: the elasticity is close to zero and statistically insignificant at the 10th percentile, 0.043 (s.e. 0.297), sizeable and precisely estimated at the median, 0.426 (s.e. 0.120), and larger at the 90th percentile, 0.748

(s.e. 0.236). Thus, while the stacked design remains our preferred specification because it addresses staggered treatment timing and provides placebo tests, the continuous TWFE estimates show that our average pass-through is in the range of the existing literature and that the upper-tail tilt is not an artifact of the binary treatment definition.

To our knowledge, no study has documented distributional responses to collectively bargained pay floors. The literature on minimum wage effects on the wage distribution generally finds stronger positive effects on the lower tail, with potential but limited spillover effects for higher wage groups (e.g. Cengiz et al. [2019], Dustmann et al. [2021], Fortin et al. [2021]). Thus, such minimum wage effects are markedly different from the responses to CBA floors increases documented here. A comparison of our results with the literature on the effects of unionization is less straightforward. The positive wage union effect documented in systems with partial coverage (e.g. Beauregard et al. [2025]) can have distributional effects that depend on the incidence of unionization and have generally been shown to reduce inequality (e.g. Card [1996], Farber et al. [2021], Fortin et al. [2021]). Our study instead documents the reduced-form effects of contracted wage increases in a system with virtually full union coverage, which should not interpreted against a counterfactual scenario with firm- or individual-level wage bargaining.

Overall, the results of this section show that contractual minimum wage growth provides a positive reduced-form effect on FTE daily wages and monthly earnings. For daily wages, we document a statistically significant inequality-enhancing effect. This distributional effect is driven by both a larger responsiveness of higher pay levels to contractual floors within the internal wage ladder of the collective contract, and also a higher sensitivity of relatively high-wage collective contracts to minimum wage increases. In the next section, we examine possible mechanisms that generate the reduced-form patterns documented here.

6 Mechanisms: Non-Compliance and Employment Adjustments

The reduced-form results show that increases in contractual minimum wages raise pay in the middle and upper part of the distribution, while effects at the lower tail are muted, so that the overall response is inequality-enhancing. This section investigates some mechanisms that can generate this pattern, focusing on non-compliance with bargained minima and employment adjustments.

A key institutional feature of sectoral collective bargaining, in Italy and elsewhere, is that negotiated updates do not operate like a statutory minimum wage that primarily "bites" at the bottom. Collective agreements typically raise multiple pay floors, often generating institutional wage growth over a broad portion of the wage ladder. This distinction matters for interpretation: under a statutory minimum wage, it is natural to expect non-compliance or displacement concentrated at the bottom because the policy disproportionately raises costs for low-paid jobs. In contrast, with bargained minima, the cost shock is in principle more diffuse and there is no mechanical reason why adjustments should be confined to low-wage workers.

Although bargained minima raise labor costs broadly along the contractual wage ladder, mechanisms that operate primarily in the lower tail may still arise. A first margin is non-compliance. The implementation of CBA pay floors is not automatic: firms may have some scope to avoid full compliance with contractual provisions, especially where monitoring is weaker and workers have less bargaining power. Since these conditions are more common at the bottom of the pay distribution, imperfect compliance may dampen the effective pass-through of contractual minima among low-paid workers.

A second margin is labor-input and employment adjustment. Following a contractual minimum-wage hike, firms may adjust through quantities as well as prices, reducing employment or days worked in some segments of the labor market. This process may also induce selection in our reduced-form estimates. Since CBA updates shift the entire wage ladder, firms may re-optimize based not only on workers' current pay but also on their expected wage progression within the agreement. In that case, job displacement may disproportionately involve workers that have steep expected wage growth, so that a near-zero wage effect at the bottom of the observed distribution can reflect a composition effect, rather than (or in addition to) limited pass-through.

In what follows, we proceed by first documenting non-compliance with contractual minima and its relationship with contract wage levels. Then we turn to employment adjustments that may shape the observed distribution of wages.

6.1 Non-compliance

We begin by measuring non-compliance with collectively bargained wage floors directly in administrative data. We define non-compliance as a situation in which a worker's observed pay (expressed in FTE daily-wage levels) falls below 95% of the lowest contractual minimum wage stipulated by the collective agreement covering the job. Because we use the lowest contractual floor, this definition delivers a lower bound on true non-compliance, as we cannot detect underpayment relative to higher occupation- or grade-specific minima within the agreement.

Our administrative measure provides a useful complement to the existing evidence for Italy, which has relied largely on survey-based indicators of underpayment relative to the contractual wage implied by the applicable agreement. For example, Garnero [2018] reports non-negligible underpayment rates, ranging between about 5% and 20% depending on the industry, using estimates based on workers' survey responses.

Our estimates of non-compliance based on administrative data have several advantages. First, we have explicit information on the collective contract applied to each worker, as employers must report this information in social security records. Second, wages are accurately estimated and are not subject to rounding errors that could be more common in surveys. Moreover, we use wages measured in the months of May and November, which tend to be among the closest to the baseline pay level regulated by contractual minimum wages, and this further increases the precision of our estimates. Finally, although employers may be reluctant to report underpayment in administrative data, there is no system of automatic monitoring and enforcement of minimum pay standards based on the social security records data. Sanctions related to underpayment can be implemented only if workers or trade unions sue employers, or after random labor inspections by government agencies. Thus, there are no automatic sanctions for employers who report underpayment in social security records, while this entails the advantage of having to pay lower social security contributions. This suggests that underpayment may be relevant and not severely under-estimated even when using administrative data.

Figure 5 documents both the time profile and the spatial heterogeneity of non-compliance in our data. The left panel shows that non-compliance is persistent and fairly stable over time, hovering around roughly 5–6% throughout 2006–2017, with only modest year-to-year fluctuations. This stability suggests that underpayment relative to contractual minima is not an episodic phenomenon confined to particular years, but rather a structural feature of the labor market segment covered by collective agreements. The right panel highlights substantial geographic dispersion. Province-level non-compliance rates range from about 4% to around 10%, with a clear concentration of higher non-compliance in parts of the South and Islands relative to much of the North. This spatial pattern is consistent with local variation in enforcement capacity and other institutional features (which the literature has shown to differ systematically across Italian regions) shaping the extent to which bargained wage floors are implemented in practice.

Table 7 characterizes the workers in our sample by compliance status. Non-compliant workers earn substantially less (mean log FTE daily wage 2.82 vs 4.38), and they are, on average, younger (35.2 vs 40.0) and more likely to be foreign-born (16.2% vs 10.7%). They are also much more likely to hold part-time jobs (54.7% vs 22.2%) and less likely to be male (41.2% vs 60.4%) or on an open-ended contract (80.9% vs 88.8%). Non-compliance is relatively higher in primary (1.2% vs 0.1%), tourism (12.4% vs 4.4%), and other services (36.3% vs 26.3%), and lower in manufacturing (24.1% vs 34.2%) and trade (14.8% vs 20.0%). Overall, the table suggests that non-compliance is concentrated among groups and sectors in the lower tail of the wage distribution.

This pattern is further confirmed by Figure 6, which relates non-compliance to the overall pay level of the collective agreement. The figure plots the incidence of underpayment against the contract's median log FTE daily wage and shows a strong negative association: non-compliance is markedly higher among low-wage contracts and declines as the median wage level of the agreement increases (the correlation is about -0.47). This relationship is consistent with our earlier results from RIF regressions. If lower-wage CBAs are also those where minima are less tightly enforced, contractual updates may translate into weaker effective wage growth at the bottom.

We next examine whether non-compliance changes in response to contractual minimum-wage increases within the same stacked event-panel design used in the reduced-form analysis. The estimates in Table 8 indicate essentially no response: the DiD coefficient is 0.001 (s.e. 0.001)—about 0.1 percentage points relative to an average non-compliance rate of 5.8% (roughly 1.7% of the baseline). This evidence suggests that renegotiated contractual minima do not systematically shift non-compliance status. A natural implication is that the main wage effects documented in Section 5 are unlikely to be driven by changes in compliance or reporting behavior around renewals.

Although no evidence of underpayment responses to minimum wage hikes have been documented in contexts characterized by collective bargaining, some studies have documented a positive association between non-compliance and increases of the minimum wage (e.g. Clemens and Strain [2022]). In our context, the descriptive patterns show that non-compliance is a quantitatively relevant feature of the wage-setting environment. However, Table 8 indicates that this feature is largely stable in response to pay floor hikes once we condition on fixed effects and controls. Our interpretation is that non-compliance reflects relatively persistent firm- and worker-level practices, together with local enforcement conditions. These underlying factors vary substantially across contracts and locations but are largely orthogonal to the short-run renegotiation events exploited by the stacked design, i.e. small changes in contractual pay floors do not materially alter the underlying propensity to comply.

Even if it is not responsive to pay floor hikes, non-compliance still has the potential to rationalize the findings of Section 5. In particular, the negative association of underpayment with median wages of the collective contract documented in Figure 6, as well as its higher incidence in weaker segments of the labor market within agreements, can potentially induce an asymmetric effect of minimum wage growth across the pay distribution under the reasonable assumption that non-compliant employers are less likely to adjust wages in response to changes in the pay floors.

6.2 Employment

Contractual renewals raise bargained pay floors that are also relevant for high-paid workers, thus increasing firm wage costs. Firms may respond by re-optimizing labor demand, disproportionately reducing jobs concentrated in the lower or upper tail of the distribution. This process may induce non-random selection that could have an influence on the estimates of the wage effect of contractual minimum wage hikes.

To assess the employment margin, we move from individual-level to cell-level outcomes. We aggregate the data into cells defined by collective agreements, two-digit sectors, and around 600 local labor markets observed over time. For each cell-month, we consider two complementary outcomes: the extensive margin (the number of employed workers in the cell) and the intensive margin (total labor input measured by the sum of FTE days worked within cells). Aggregation in labor market cells allows us to estimate a comprehensive employment effect that may be driven by both changes in potential hires and changes in separation rates. Both channels could be relevant in determining selection effects on the estimates of wage elasticities. For example, lower hiring rates in the treated group may induce a selection in the type of workers that are retained across periods, while an effect on separation rates would directly affect the composition of workers contributing to the estimation of the wage elasticity.

Using cell-level data, we construct a similar stacked difference in difference design to that described in Section 4. In particular, we assign to the treated group all contracts for which the median pay floor increases by at least 1% over adjacent periods, and to the control group all contracts with flat minimum wage growth. We replicate the placebo analysis by testing for differences in trends between treated and control groups in the semester leading up to the event. The specification includes cell by panel and time by panel fixed effects.

Table 9 reports the effects of contractual minimum-wage increases on employment at the cell level, distinguishing the extensive margin (number of workers) from the intensive margin (FTE days). The first outcome corresponds to headcount employment, while the second is influenced by both the number of employed workers and the intensity of their schedule. The estimates point to modest but statistically meaningful contractions. Following a floor update, treated cells lose 0.065 workers (s.e. 0.037) relative to a mean of 11.5, and 1.387 FTE days (s.e. 0.712) relative to a mean of 259.8. Both estimated effects are on the order of a 0.5% reduction in the cell size relative to its average level in response to an average increase of wage floors of around 2% in the treated group. The placebo estimates are small and statistically insignificant (0.005, s.e. 0.019 for workers; 0.046, s.e. 0.419 for FTE days), supporting the view that the negative effects are not driven by differential pre-trends around the event windows.

Although Table 9 reports the average employment response, our interpretation of the reduced-form wage results hinges on whether the adjustments are concentrated at the bottom of the pay distribution. Figure 7 addresses this issue by estimating heterogeneous effects across the wage distribution. This estimates are obtained by aggregating the data into cells defined by collective contracts, two-digit sectors, local labor markets, and a time-constant indicator of workers' most common quartile in the de-trended FTE daily wage distribution. This approach allows us to test directly whether collective-bargaining renewals shift labor demand away from low-wage segments of the market.

The figure clearly shows a concentration of the employment effects at the bottom of the wage distribution. These effects are most negative in the first quartile, both for days worked and for the number of workers, and they become progressively smaller and statistically indistinguishable from zero moving toward higher-wage quartiles. In other words, the average contraction documented in Table 9 is largely accounted for by cells in the lower tail of the wage distribution.

The evidence in Figure 7 provides an indirect support for the hypothesis that selection effects may be a contributing factor to the muted wage elasticities to contractual pay floors at the bottom of the FTE daily wage distribution documented in Table 4. Notice that our wage elasticity estimator is individual-based, meaning that it is identified using only within-worker pay variation over time. In this setting, what matters for the direction of the selection bias is the potential workers' wage growth over time. A bias toward zero could be driven by a disproportionate negative employment effect on workers with higher potential wage growth at the bottom of the wage distribution. Explicitly testing for this mechanism would not be feasible, given that only realized wage growth for continuously employed workers can be observed in the data.

Using a similar estimation strategy, we also investigate heterogeneity in employment effects by part-time and full-time status. This is informative because it allows us to further characterize which segment of the labor market is affected by displacement while also providing indirect evidence on employers' compliance behavior. Indeed, it is generally more difficult to detect underpayment among part-time workers, given that work hours can be more easily manipulated by employers in such contracts, and official pay floors are expressed as a monthly wage for full-time workers.

Figure 8 reports estimates of employment effects by part-time status obtained from data aggregated using collective contracts, two-digit sectors, local labor markets, and full- or part-time status. Employment effects are clearly more negative for full-time jobs, both in terms of the number of workers and days worked, while effects for part-time jobs are close to zero and not significant.

Figure 8 does not show an increase in part-time employment that mechanically mirrors the decline in full-time jobs. Such result could be indicative of employers forcing workers to accept a shift from full-time to "fake" part-time arrangements in order to soften the effective bite of higher minima. Nevertheless, the fact that employment reduces only among full-time positions is consistent with the hypothesis that these jobs may be more sensitive to minimum wage increases due to a lower incidence of non-compliance within this group, which clearly also emerges from the descriptive evidence of Table 7. Instead, part-time employment could be more protected due to a lower compliance, which is more likely associated with muted wage responses as well.

The pattern of employment effects in Figure 8 can also be related to the secular decline in labor intensity, which was documented by the descriptive evidence in Figure 2. In this regard, the relative decline in full-time jobs after contract renewals suggests that this compositional change may affect earnings through the labor-intensity channel. Given that our reduced-form evidence on earnings effects was based on within worker variation over time, these estimates cannot fully capture this compositional effect.

7 Conclusions

This paper studies how the pay floors set by collective bargaining agreements shape the wage structure in Italy. Using administrative records for the universe of private-sector employees and a re-constructed archive of contractual minima for about 160 national agreements, we match workers to time-varying bargained pay floors over 2006–2017 and exploit the staggered timing of contractual updates in a stacked 2 by 2 event-panel DiD design combined with RIF regressions. Because CBAs set floors along a contractual wage ladder, their distributional implications do not need to mirror those of statutory minimums that bite primarily in the lower tail.

Our main finding is that bargained pay floors increase wage rates on average but do so in a markedly asymmetric way across the wage distribution. Contractual minimum-wage hikes raise mean log FTE daily wages by about 2.2%, yet effects are near zero at the 10th percentile and much larger at the 90th percentile, implying inequality-enhancing wage-rate responses. When we re-center wages at the agreement level to focus on within-contract dispersion, the asymmetry attenuates but persists, indicating that both between- and within-agreement components contribute to the overall distributional tilt. Consistent with this interpretation, contract-specific treatment effects are systematically stronger in higher-wage agreements.

We then provide evidence on two mechanisms that help interpret why the lower tail responds weakly to pay floor updates. First, we develop an administrative measure of non-compliance with bargained minima (a lower bound by construction) and document that underpayment is non-negligible and concentrated in low-wage agreements, yet it is largely stable around renewals. This suggests that while imperfect enforcement may contribute to a muted pass-through at the bottom due to a larger incidence of underpayment in weaker labor market segments, non-compliance is also a relatively persistent phenomenon whose size is not much affected by short-run changes in pay floors.

Second, we examine labor-input adjustments, finding modest but statistically significant contractions in both headcount employment and total days worked after renewals. Importantly, these negative effects are significant only in the lower tail of the wage distribution, a pattern consistent with composition forces that can potentially mute observed wage effects at the bottom depending on the selectivity of workers along their wage growth potential. Negative effects are also primarily driven by full-time employment, suggesting that in labor market segments characterized by higher underpayment, both wage and employment responses tend to be muted.

Overall, our results imply that pay floors set by collective bargaining can shape the wage structure in ways that differ from the canonical statutory-minimum-wage setting. In our context, contractual wage updates do not primarily compress the lower tail; instead, they raise wages more strongly toward the top of the distribution and are associated with employment reductions concentrated among low-wage (and full-time) jobs. These findings suggest that in collective bargaining systems policies aimed at protecting low-paid workers may need to consider the asymmetric incidence of non-compliance and labor-demand responses explicitly, especially when pay-floor adjustments propagate broadly along the contractual wage ladders.

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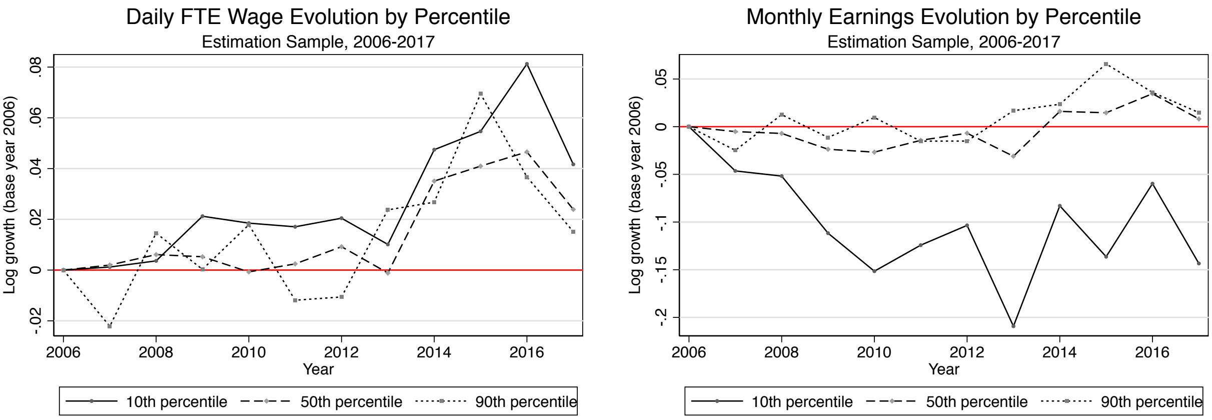
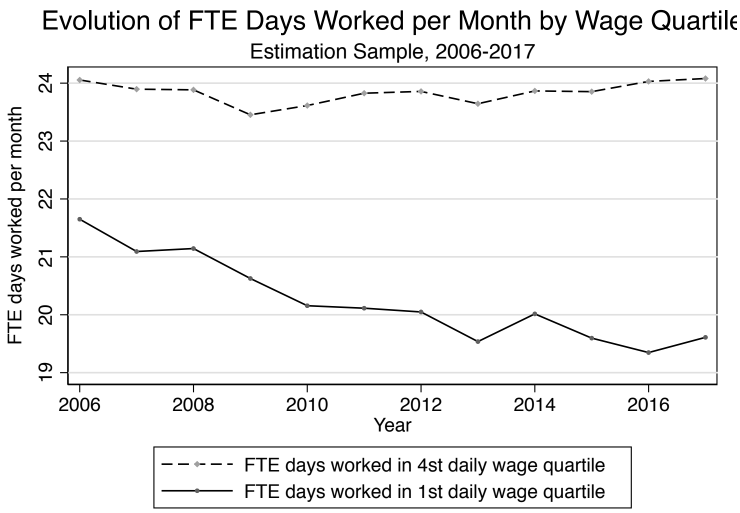
FIGURES

Figure 1: Evolution of Daily FTE Wage and Monthly Earnings by Percentile in the Estimation Sample



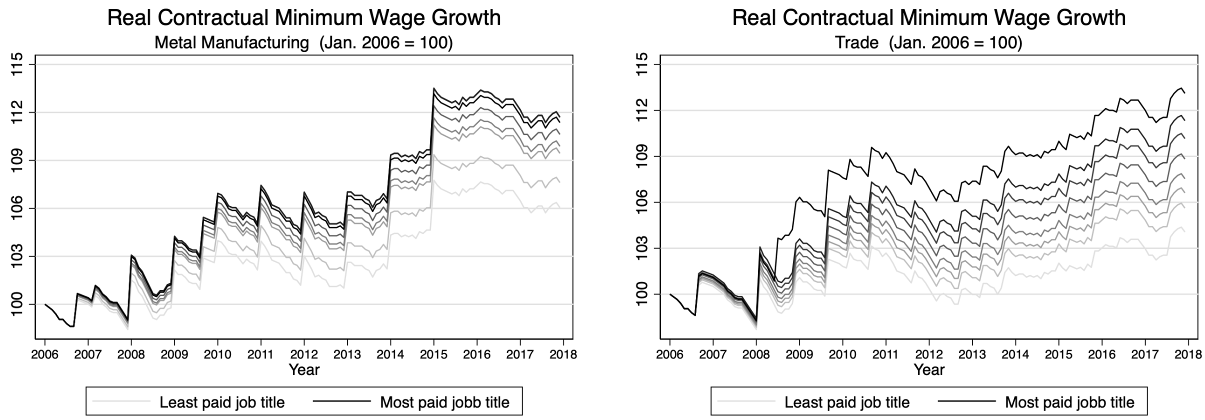
Evolution of real daily FTE equivalent wages (left panel) and of monthly earnings (right panel) by percentile in the estimation sample.

Figure 2: Evolution of Days Worked per Month by Quartiles of FTE Daily Wages



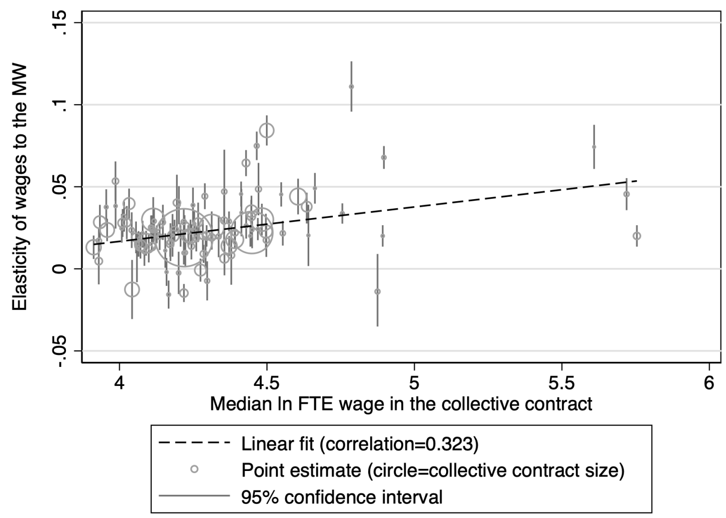
Evolution of Days Worked per Month by Quartiles of FTE Daily Wages.

Figure 3: Evolution of Real Contractual Minimum Wages in the Trade and Metal-Manufacturing Collective Contracts



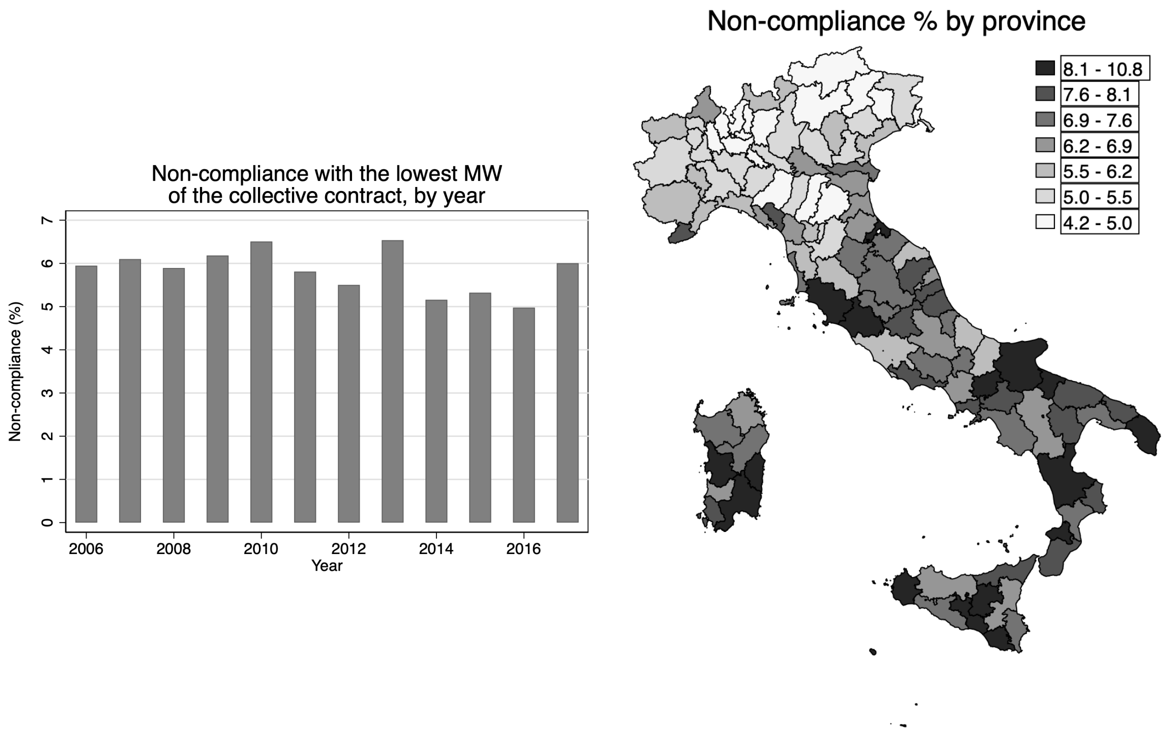
Each line provides the cumulated real growth of an occupation-specific contractual minimum wage (job title), with respect to the reference period (Jan. 2006). Nominal minimum wage levels are adjusted using the monthly CPI index provided by the Italian Statistical Institute.

Figure 4: Contractual Minimum Wage Effect on Average FTE Daily Wages by Collective Agreement and Median Contract FTE Wage



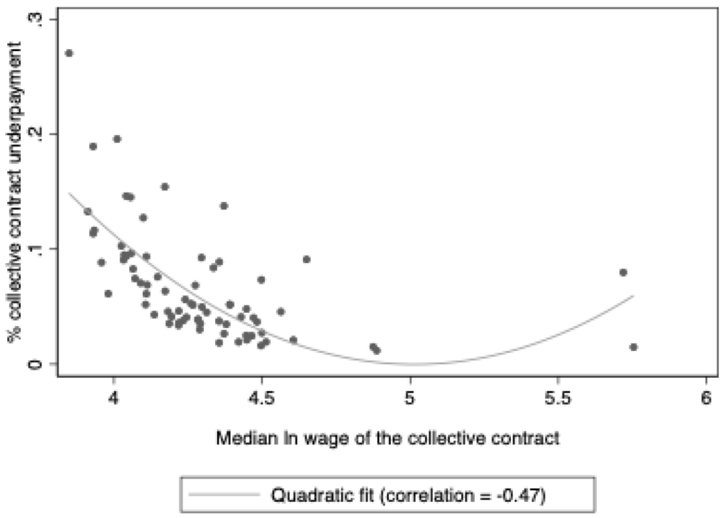
Estimates obtained using OLS in a specification where $1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$ *is interacted with collective contract dummies in equation (2). Only the 75% largest collective contracts in the sample are reported in the figure. The 1% lowest and highest 95% confidence interval estimates are excluded as well.*

Figure 5: Evolution and Geographic Distribution of Non-Compliance



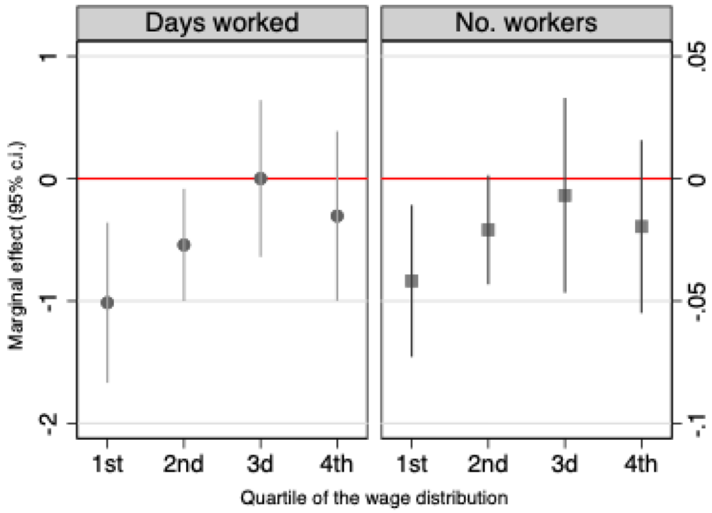
Non-compliance is defined as being paid less than 95% of the lowest contractual minimum wage of a worker's collective agreement.

Figure 6: Non-Compliance Incidence by Median FTE Daily Wage of the Collective Contract



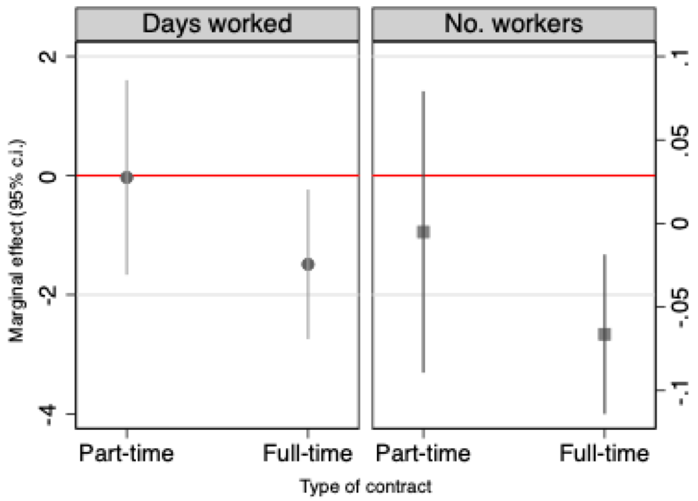
Non-compliance is defined as being paid less than 95% of the lowest contractual minimum wage of a worker's collective agreement.

Figure 7: Contractual Minimum Wage Effect on Employment by Quartile of the Wage Distribution



Results from the specification of equation (2) estimated using OLS on data aggregated by time-constant quantiles of the FTE daily wage distribution, collective contract, two-digit sector and local labor market. The dependent variables are headcount employment in the labor market cell, and the total number of FTE days worked within the cell. The range bars report the 95% confidence intervals.

Figure 8: Contractual Minimum Wage Effect on Employment by Part-Time Status



Results from the specification of equation (2) estimated using OLS on data aggregated by part-time status, collective contract, two-digit sector and local labor market. The dependent variables are headcount employment in the labor market cell, and the total number of FTE days worked within the cell. The range bars report the 95% confidence intervals.

TABLES

Table 1: Demographic and job characteristics in the main sample of analysis.

Variable	**Mean**	**St. Dev.**
Age	39.64	10.41

Variable	**Proportion**
Males	59.21%
Foreigners	11.02%
Part-time	24.10%
Open-ended	88.35%
Apprentices	5.05%
Blue collars	53.59%
Clerks	38.06%
Mid-managers	2.86%
Managers	0.44%
Manufacturing	33.61%
Constructions	9.54%
Trade	19.75%
Transports	3.63%
Tourism	4.86%
Other services	26.99%
Other sectors	1.62%
North-West	33.49%
North-East	24.29%
Centre	20.44%
South	21.78%
Observations	52.922 Millions
N. of workers	5.082 Millions
N. of collective contracts	159

Descriptive statistics for the sample of analysis covering the period 2006-2017.

Table 2: Descriptive statistics on actual and contractual minimum wages in the main sample of analysis.

Variable	Mean	St. Dev.
Ln daily FTE wage	4.285	0.588
Ln monthly earnings	7.338	0.750
Ln median contr. minimum wage	4.109	0.135
Ln max contr. minimum wage	4.469	0.185
Ln min contr. minimum wage	3.882	0.124
Real median contr. minimum wage growth	0.003	0.012
Nominal median contr. minimum wage growth	0.010	0.014
Nominal max contr. minimum wage growth	0.011	0.015
Nominal min contr. minimum wage growth	0.009	0.012
Correlation median-min nominal min. wage growth if positive	0.904	
Correlation min-max nominal min. wage growth if positive	0.840	
Correlation median-max nominal min. wage growth if positive	0.903	
Observations	52.922 Millions	
N. of workers	5.082 Millions	
N. of collective contracts	159	

Descriptive statistics on actual wages and contractual minimum wages in the sample of analysis covering the period 2006-2017. Median, max and min contractual wages refer to the median, lowest and highest occupation-specific floor set within each collective agreement. Wage growth is computed as a log difference over a six months period. Correlations are computed only in periods when the median floor of the agreement has a positive growth.

Table 3: Contractual Minimum Wage Effect at the Mean

Dependent Var.	ln FTE daily wage	
Model	DiD	Placebo
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.022*****	**-0.000**
St. err.	0.005	0.006
F.e. occupation/contract	✓	✓
Age polynomial	✓	✓
F.e. individual * event panel	✓	✓
F.e. contract * event panel	✓	✓
F.e. time * event panel	✓	✓
A-R2	0.610	0.666
OBSERVATIONS	74.494 millions	26.668 millions

Results from the specification of equation (2) estimated using OLS. The dependent variable is the ln FTE daily wage. The placebo specification compares the treated and control groups in the semester leading up to the event. ***: 1% significance level; **: 5% significance level; *: 10% significance level.

Table 4: Contractual Minimum Wage Effect on FTE Daily Wages at Different Percentiles

Dependent Var.	**ln FTE daily wage**					
Model	**Rif perc. 10**		**Rif perc. 50**		**Rif perc. 90**	
	DiD	**Placebo**	**DiD**	**Placebo**	**DiD**	**Placebo**
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.004**	**0.001**	**0.024*** **	**0.005**	**0.045** **	**0.001**
St. err.	0.004	0.010	0.004	0.006	0.022	0.018
F.e./Controls	✓	✓	✓	✓	✓	✓
A-R2	0.504	0.576	0.692	0.720	0.642	0.663
Obs (millions)	74.494	26.668	74.494	26.668	74.494	26.668

Dependent Var.	*Effect on Percentile Gaps* **ln (FTE daily wage)**		
Model	**Rif 90/10 gap**	**Rif 50/10 gap**	**Rif 90/50 gap**
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.041***	**0.020*** **	**0.021**
St. err.	0,023	0.007	0.024
F.e./Controls	✓	✓	✓
A-R2	0.611	0.506	0.599
Perc. gap in the sample	0.945	0.372	0.573
Obs (millions)	74.494	74.494	74.494

Results from the specification of equation (2) estimated using unconditional quantile regression. The dependent variable is the ln FTE daily wage. The placebo specification compares the treated and control groups in the semester leading up to the event. ***: 1% significance level; **: 5% significance level; *: 10% significance level.

Table 5: Contractual Minimum Wage Effect across the Within-Contract Wage Distribution

Dependent Var.	**ln (FTE daily wage - avg. contract FTE daily wage)**					
Model	**Rif perc. 10**		**Rif perc. 50**		**Rif perc. 90**	
	DiD	**Placebo**	**DiD**	**Placebo**	**DiD**	**Placebo**
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.012*** **	**0.002**	**0.023*** **	**0.005**	**0.033** **	**-0.008**
St. err.	0.004	0.003	0.004	0.005	0.013	0.019
F.e./Controls	✓	✓	✓	✓	✓	✓
A-R2	0.485	0.534	0.648	0.670	0.608	0.601
Obs (millions)	74.494	26.668	74.494	26.668	74.494	26.668

Dependent Var.	*Effect on Percentile Gaps* **ln (FTE daily wage - avg. contract FTE daily wage)**		
Model	**Rif 90/10 gap**	**Rif 50/10 gap**	**Rif 90/50 gap**
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.023***	**0.013** **	**0.010**
St. err.	0.012	0.005	0.012
F.e./Controls	✓	✓	✓
A-R2	0.539	0.435	0.555
Perc. gap in the sample	0.833	0.354	0.478
Obs (millions)	74.494	74.494	74.494

Results from the specification of equation (2) estimated using unconditional quantile regression. The dependent variable is the difference between the ln FTE daily wage and its contract- and panel-specific average. The placebo specification compares the treated and control groups in the semester leading up to the event. ***: 1% significance level; **: 5% significance level; *: 10% significance level.

Table 6: Contractual Minimum Wage Effect on Monthly Earnings at Different Percentiles

Dependent Var.	ln (monthly earnings)					
Model	Rif perc. 10		Rif perc. 50		Rif perc. 90	
	DiD	Placebo	DiD	Placebo	DiD	Placebo
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0,010***	**-0,009**	**0,021*******	**0,004**	**0,043****	**-0,002**
St. err.	0,006	0,007	0,003	0,006	0,021	0,015
F.e./Controls	✓	✓	✓	✓	✓	✓
A-R2	0,548	0,607	0,716	0,747	0,704	0,723
Obs (millions)	74,494	26,668	74,494	26,668	74,494	26,668

Dependent Var.	*Effect on Percentile Gaps* ln (monthly earnings)		
Model	Rif 90/10 gap	Rif 50/10 gap	Rif 90/50 gap
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.033**	**0.011**	**0.022**
St. err.	0.022	0.007	0.021
F.e./Controls	✓	✓	✓
A-R2	0.556	0.488	0.629
Perc. gap in the sample	1.503	0.900	0.603
Obs (millions)	74.494	74.494	74.494

Results from the specification of equation (2) estimated using unconditional quantile regression. The dependent variable is the ln monthly earnings. The placebo specification compares the treated and control groups in the semester leading up to the event. ***: 1% significance level; **: 5% significance level; *: 10% significance level.

Table 7: Descriptive Statistics by Non-compliance Status

Variable	Above MW	Below MW
Ln(FTE daily wage)	4.38 (0.405)	2.82 (0.992)
Age	39.96 (10.23)	35.16 (12.05)
% foreign	10.7%	16.2%
% part-time	22.2%	54.7%
% male	60.4%	41.2%
% open-ended	88.8%	80.9%
% primary	0.1%	1.2%
% manufacturing	34.2%	24.1%
% trade	20.0%	14.8%
% tourism	4.4%	12.4%
% other services	26.3%	36.3%

Descriptive statistics by non-compliance status computed in the sample of analysis. Workers belong to the underpaid group if their wage is below 95% of the lowest floor of their collective agreement.

Table 8: **Contractual Minimum Wage Effect on Non-compliance Status**

Dependent var.	**Non-compliance status**
Model	**DiD**
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**0.001**
St. err.	0.001
F.e. /Controls	✓
A-R2	0.441
Avg outcome in sample	0.058
Observations	74.494 millions

Results from the specification of equation (2) estimated using OLS. The dependent variable is an indicator for being paid less than 95% of the lowest pay floor of the collective contract. ***: 1% significance level; **: 5% significance level; *: 10% significance level.

Table 9: **Contractual Minimum Wage Effect on Employment**

Dependent var.	**N. workers**	**N. workers**	**N. FTE days**	**N. FTE days**
Model	**DiD**	**Placebo**	**DiD**	**Placebo**
$1[\Delta cw_{T_p} > 1\%]1[T_p = 2]$	**-0.065***	**0.005**	**-1.387****	**0.046**
St. err.	0.037	0.019	0.712	0.419
F.e. /Controls	✓	✓	✓	✓
A-R2	0.997	0.998	0.997	0.998
Avg outcome in sample	11.5	9.7	259.8	217.7
Observations	6.554 millions	4.320 millions	6.554 millions	4.320 millions

Results from the specification of equation (2) estimated using OLS on data aggregated by collective contract, two-digit sector and local labor market. The dependent variables are headcount employment in the labor market cell, and the total number of FTE days worked within the cell. The placebo specification compares the treated and control groups in the semester leading up to the event. ***: 1% significance level; **: 5% significance level; *: 10% significance level.