Opting Out of Centralized Collective Bargaining: Firm and Worker Consequences[1]

August 2023
Preliminary

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Abstract

This paper assesses the effects of opting out of centralized collective bargaining agreements on firms and workers. We study two opting-out events that occurred in Italy, a country with very rigid industrial relations. In the first event, firms left centralized collective bargaining agreements to reach agreements with smaller and often local unions. In the second event, a group of large employers decided to abandon their collective bargaining agreement in order to re-negotiate a new one with national unions. Drawing on a matched event-study design, we find evidence that opting out of centralized collective bargaining agreements lowers firms' labor costs while increasing their survival probabilities. Workers in those firms experience wage losses but higher employment stability and higher earnings. These effects are larger in firms facing stricter employment protection regulation, and in the less productive regions in the South of Italy.

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1. Introduction

Centralized collective bargaining regimes, common in many European countries, are often lauded for effectively redistributing productivity gains from firms to workers. But they are also blamed for their rigidity and inflexibility, disallowing adjustment to firm or region-specific needs and sluggishly responding to economic recession. Not surprisingly, therefore, there is intense debate about reform of collective bargaining frameworks to allow for sufficient flexibility to take account of firm heterogeneity and local conditions but continue to protect working conditions and ensure wage adjustments for workers. The OECD identifies "coordinated decentralization" as a possible institutional set-up (OECD, 2019), where centralized (e.g., national sector-level) agreements set a broad negotiation framework but where firms can opt out under certain conditions to negotiate wages and other labor provisions more directly with their employees. To date, there is little rigorous empirical evidence on how decentralization of this type benefits or harms firms and workers.

In this paper, we examine the effects of the recent decentralization of industrial relations in Italy. Italy is an ideal laboratory given its rigid, heavily centralized, collective bargaining institutions that impose national wage floors for each sector (see, e.g., Boeri et al., 2021). The Italian economy was also hit hard by the Great Recession, and, as a result, the debate about decentralization accelerated considerably in its aftermath (Financial Times, 2011).

We study two episodes that led to decentralization from centralized collective agreements in the period after the Great Recession. First, regulatory loopholes enabled firms to opt out from their national collective bargaining agreement (CBA) to adopt new agreements with smaller unions that allowed for more flexibility in negotiating working conditions and wages. These so-called "pirate agreements" gained traction after the Great Recession. By 2019 they constituted around two-thirds of the total number of collective contracts, covering half a million workers, or 3 percent of total private-sector employment. Second, there was a coordinated opt-out by a large group of firms, mostly in the retail sector (e.g. Ikea, Zara, Carrefour) in 2011, where firms left traditional employer organizations, membership of which committed them to accept national collective agreements, and started to negotiate separately with unions. This opt-out affected around 25% of all retail workers.

To study how these two events affect firm and worker outcomes, we combine information on CBAs with detailed matched employer-employee data provided by the Italian Social Security Institute (INPS) for the universe of private-sector workers and firms in Italy from 2005 to 2019. A key feature of the INPS data is that it provides information on the CBA for a worker in a particular job, which allows us to identify events where firms opt out from their national CBA. Moreover, we directly observe transitions from a national to a pirate CBA, and we can identify firms (and workers) that are part of the coordinated opt-out in the retail industry.

Our identification strategy uses a matched difference-in-differences design to compare firms and workers subject to the opt-out (or change from national to pirate CBA) to a suitable control group. To mitigate the problem of dynamic selection, we assign each affected firm an observationally equivalent control firm in the years before the opt-out. We then compare treated and control firms in an event-study design and explore the robustness of our results to alternative matching strategies.

Studying first the firm-level effects of firm transitions to pirate contracts, we find that treated firms face roughly 3 percent lower labor costs than control firms in the years following the opt-out. We also find a positive effect on firm survival of about 4 percentage points in the 2-3 years immediately after the transition. Transitions to pirate agreements also lead to faster growth and the hiring of more women, young workers, blue collar and temporary workers.

We then move to the level of the worker, focusing on wages and employment of workers who experience a transition from a standard to a pirate CBA within their job spell. Similar to our firm-level design, we assign each treated worker to an equivalent control worker before their firm transitions to a pirate agreement. We find that treated workers suffer a persistent wage loss of 2-3 percent following the CBA transition. However, they are also more likely to remain employed, by about 3 percentage points relative to control workers. We show that the employment effect overcompensates the wage effects, with worker earnings rising following the transition. Moreover, not only are transitions to pirate contract agreements more widespread in the South (consistent with firms responding more as national wage floors are particularly restrictive, see Boeri et al., 2021), but their effect on the firm survival probability and workers' wages and employment is larger for Southern firms. Effects on firm survival and worker retention are particularly salient for larger firms (above 15 employees), for which employment protection legislation under national CBA's was more restrictive.

While our matching procedures effectively accounts for potential differences between treated and control firms in observable characteristics, we cannot fully rule out the possibility of unobservable characteristics of opting out firms preventing a causal interpretation of this evidence. For example, opting out firms may be characterized by more collaborative employer-employees relationships, so that managers are able to credibly commit to higher employment stability in exchange for lower wages.

To address these concerns, we turn to a "case study," namely the coordinated opt-out of a group of large retailers ("Federdistribuzione") from their employers' association ("Confcommercio"), occurred in 2011. Confcommercio has the role of bargaining with national unions to sign collective agreements. As a result of this opt-out event, when the collective agreement for the retail sector was renewed in 2015, the new, higher minimum wages, did not apply to Federdistribuzione firms.

Importantly, as we discuss in Section 5, the opt-out decision was not motivated by labor cost-cutting consideration. Federdistribuzione firms regared forming a separate employers' association as a more effective way to lobby for their interests. Hence, the "freezing" of the collective agreement was simply a side effect of this decision, ameliorating endogeneity concerns.

In the coordinated opt-out in the retail sector, we find broadly consistent firm-level and worker-level patterns. At the worker level, our findings echo those from our analysis of the pirate agreements: workers employed by retail firms that abandon their initial CBA experience wage declines but benefit from higher employment probabilities. At the firm level, we find moderately positive effects on firm survival but no significant effects on firm-level wages. This suggests that the negative wage effects at the worker level are concentrated in larger firms.

Our work presents novel evidence of the effects of decentralizing collective bargaining on workers and firms. Despite a global decline over the past decades, collective bargaining remains highly relevant in many labor markets, especially in Europe, where CBAs cover between 60 and 100 percent of the workforce (OECD, 2019)—with potentially large labor market implications (Visser, 2013). For instance, many European countries, like Italy and Spain, impose rigid sector-specific wage schedules at the national level that apply to all workers, irrespective of their union status (Adamopoulou and Villanueva, 2022).

Dustmann et al. (2014) contrast the inflexibility of CBAs typically found in Southern European economies with the autonomous industrial relations in Germany, where opening clauses allowed re-negotiations of union contracts at the firm level in times of economic hardship. They argue that this flexibility permitted firms to gain competitiveness during a period of high unemployment and severe challenges through global competition, preventing firms from outsourcing production to Central and Eastern European countries and thus keeping jobs in the country at the price of lower wages. Card et al. (2013) show some suggestive evidence that firms opting out from sectoral bargaining agreements helps understand recent trends in wage inequality in Germany. Rigorous and *direct* micro-evidence on the effects of firms changing to more flexible wage agreements remains, however, scarce as it is often hard to precisely identify firms and workers that are affected by transitions, in particular in administrative data. This paper fills this gap by leveraging Italian social security data to identify opt-out events, thanks to information on the collective bargaining agreement applied to every job.

Our paper is most closely related to Lucifora and Vigani (2021), Dahl et al. (2013) and Gürtzgen (2016). Lucifora and Vigani (2021) also examine the rise of pirate agreements in Italy and find substantial wage penalties. Our paper extends their work in several dimensions. First, we leverage the universe of Italian private-sector workers from 2005-2019, whereas Lucifora and Vigani (2021) focus on a 1/90 random sample of workers from 2005-2014. Hence, our analysis identifies the universe of pirate agreements. Our coverage through 2019 also permits us to include the post-2014 boom in pirate agreements. Second, we also study the firm-level effects on variables such as labor costs, profits, or the wage of coworkers not directly subject to the pirate agreement. Third, our paper not only studies transitions from CBA to pirate agreements but also investigates the coordinated opt-out from CBA by firms operating in the mass-retail sector.

Dahl et al. (2013) study the effects of a period of gradual decentralization in Denmark using longitudinal data and a fuzzy approach based on occupation and sector codes to identify the bargaining regime associated with a given job. Gürtzgen (2016) studies the effects of manufacturing and mining firms leaving industry-level agreements in Germany, drawing on survey data to infer the opt-out. This paper extends the analysis in these papers by adding evidence for Italy, which features a significantly more rigid centralized bargaining system. Finally, our analysis covers a period of remarkable difficulty for Southern European labor markets—and is thus of particular relevance in assessing the importance of introducing more flexibility in industrial relations—whereas both Dahl et al. (2013) and Gürtzgen (2016) study a period of relative stability (1992-2001 in Denmark and 1999-2007 for Germany).

The paper is organized as follows. Section 2 illustrates the institutional background. Section 3 describes the main data. Section 4 outlines the empirical approach and results of studying the pirate contract design separately for firm and worker outcomes. Section 5 does so for our second opt-out event by large retailers. The last Section concludes.

2. Institutional Background

National Sectoral Collective Agreements The Italian system of collective bargaining is a two-tier system. The first tier consists of sector-level CBAs (*Contratti Collettivi Nazionali del Lavoro*) that have been historically negotiated by the most representative employer and employee associations.[2] The resulting "representative CBA" signed by the dominant associations extends *de-facto* to all other workers and firms belonging to that sector. Contracts typically last three years and establish wage floors and broader employment conditions (such as vacation, working hours, etc.) on a national scale. More precisely, CBAs establish a *schedule* of minimum wages, for different "job titles", which roughly correspond to different occupations (a typical contract distinguishes around eight job titles). The wage paid by the firm consists of the wage floor plus a firm-level component discussed below. Wage floors are periodically adjusted following a predetermined schedule, reflecting, e.g., expected inflation. At the expiration, CBAs are renewed. In case of delays of new negotiations, the expired CBA remains in force.

Importantly, minimum wages established by CBAs have the dual role of wage floors and fixed components of total pay (Fanfani, 2020). In practice, the wage effectively paid to a worker can be split in two components, the minimum wage and a firm-level "cushion." If the minimum wage increases, the resulting worker's compensation will be equal to the cushion plus the new minimum wage. An important implication of this contractual arrangement is that changes in minimum wages induced by opt-out events can potentially affect the entire wage distribution, and not only wages close to the floors.

Limited Scope for Firm-Level Agreements The second tier consists of firm-level agreements, where negotiations occur directly between employers and firm-level union delegations. Firm-level bargaining is almost entirely subordinated to the sector-level agreement, which limits the scope of this channel of decentralization, as firm contracts can only regulate matters when explicitly allowed to do so by the superior CBA ("non-repeatability" clause). Moreover, derogations from CBAs are only possible if they improve conditions for the worker ("favorability" clause). Hence, only around 20 percent of firms larger than 20 employees have negotiated directly with workers between 2010 and 2016 (D'Amuri and Nizzi, 2017).

Pressure towards Decentralization: Opt-Outs Following the Great Recession and the harsher competition on global markets, the standards set by centralized bargaining became hard to meet for some employers. Firms sought to deviate from representative CBAs and to adopt alternative agreements signed with smaller or new unions or to adopt CBAs from other sectors (Lucifora and Vigani, 2021). This was possible since the country's legal framework does not define hierarchical

[2] While the law does not define representativeness criteria, the term "representative" has been used to denote the employer association (Confindustria) and three unions (CGIL, CISL, UIL, also referred to as "union triad") that have long dominated the Italian industrial relations.

relationships between different CBAs in the private sector, nor does it provide criteria to assess the representativeness of unions.[3] We describe and exploit two major opt-out events in the following sections: the explosion of the pirate contracts, in Section 4, and the opt-out by large retailers in Section 5.

3. Data

We merge three datasets: matched employer-employee panel data, additional firm data providing financial information on firms, and data on details of CBAs.

Matched Employer-Employee Data Our analysis is based on longitudinal matched employer-employee data for the universe of worker-firm matches in the Italian private sector (roughly 15 million workers) between 2005 and 2019. The data are sourced from the INPS through the VisitINPS Scholars program and provide detailed information on workers' wages, contract status (part- versus full-time, temporary versus permanent), occupation, weeks worked as well as demographic information such as date of birth, gender, and municipality of residence and of work.

Additional Firm Data On the firm side, INPS records also include firm-related information such as size, age, location, and sector. For incorporated firms, balance sheets and income statements are available until 2018. We merge this data, sourced from the Cerved group using firm identifiers. Our full dataset spans the period from 2005 to 2019 for a sample size of around 200 million worker-year observations.[4]

Contract-level Data All CBAs (including their texts and hence content) are recorded in the archives of the CNEL (National Council for Economics and Labor), along with contract details including their signatory parties. This allows us to track the diffusion of pirate agreements over time by looking at the number of CBAs not signed by the three dominant unions.

Identifying Pirate Agreements Crucially to our purposes, social security records also report the CBA covering each worker, which can be used to classify CBAs into standard and pirate. Specifically, each CBA in the INPS data is identified by a code that can be merged with the CNEL archives with an INPS-CNEL crosswalk to obtain information on the signatory parties for each CBA. Importantly,

[3] These matters have not been legislated until only very recently, and the new representativeness criteria have not yet been implemented. So far, a union's representativeness has been assessed by labor courts on a case-by-case basis (D'Amuri and Giorgiantonio, 2015).

[4] We keep workers aged 18 to 67. In case of multiple spells within a year, we keep the job with more weeks worked and, if these are the same, the job with the highest wage paid.

a large number of CBAs in the INPS database are not assigned a unique identifier and classified "Different Contract." By inspecting the CNEL records, we notice that virtually all these CBAs are signed by new, unrecognized unions, exactly coinciding with the description of pirate agreements.[5] We thus follow Lucifora and Vigani (2021) and define pirate CBAs as all those classified as “Different Contract”. To these, we add about 100 CBAs that, despite having a unique INPS identifier (not just "Different Contract"), are not signed by at least one member of the union triad and do not cover categories where the representative CBA has historically been signed by non-triad unions.[6] The remaining CBAs are classified as standard.

Identifying Participating Firms in the Mass Retail Opt-Out To identify large retail firms involved with the decision to leave the standard retail CBA, we obtained from the association of employers operating in mass-retail (*Federdistribuzione*) the national tax identifiers of the firms that opted out from their CBA in 2011. INPS then merged and anonymized these identifiers in order for us to conduct the analysis.

4. Evidence from the Adoption of Pirate Agreements

We now describe our first design to analyze the effect of firm opt-outs through the adoption of pirate contracts on firm and worker outcomes. To do so, we compare treated firms and workers to observationally similar units over time by means of a matched event study design, which exploits the longitudinal dimension of the data to account for time-invariant unobserved confounders. Section 4.1 describes the institutional background of pirate agreements and some descriptive statistics. Section 4.2 presents the firm-level analysis, and Section 4.3 presents the worker-level analysis. In Section 4.4, we conduct heterogeneity analysis to understand the mechanisms underlying the wage and employment effects.

4.1 Background: The Pirate Agreements

As described in Section 2, following the Great Recession and the consequences of tougher competition on global markets, the conditions set by centralized bargaining became hard to meet for some employers. Firms, therefore, sought to deviate from representative CBAs to adopt new ones signed with minor or new unions or to adopt CBAs from other sectors. Since the representativeness

[5] The CNEL data shows that there is a very low number of "Different Contracts" signed by the union triad (around 30). These cannot be identified in the INPS data.
[6] These cases are easily identified in the CNEL data since these CBAs had originally been signed long before the diffusion of pirate agreements. Most of these contracts cover managers.

of a CBA is not tightly defined, this loophole allowed firms to opt out of their original CBA and to shop among alternative ones. A new class of contracts—dubbed *pirate* CBAs—started to emerge. These contracts usually set lower wages and more flexible working conditions than the national, representative agreements.[7]

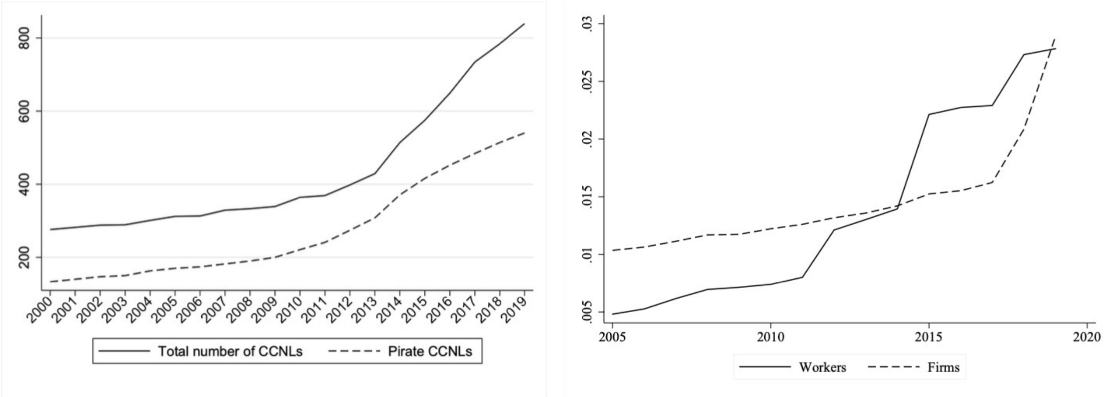
Figure 1 Panel (a) illustrates the explosion in pirate contracts following the Great Recession by the count of contracts; Panel (b) does so for the share of firms and workers. Despite constituting the majority of CBAs in Italy, pirate contracts covered only about 3 percent of private sector workers and firms in 2019 (roughly half a million workers and about 40,000 firms). Appendix Figure B1 illustrates that in 2019 pirate contracts were concentrated in the Center-South of Italy. This is consistent with the findings of Boeri et al. (2021), who note that wage floors are particularly binding in the less productive regions of Southern Italy.[8] Finally, Appendix Table B1 shows the industry distribution of firms using pirate agreements in comparison to those who do not, showing a concentration in services, with the share in manufacturing being low at about 3 percent (or 13 percent if employment weighted, suggesting that larger firms in manufacturing have made use of pirate contracts); Appendix Figure B3 shows the evolution over time by sector.

[7] In practice, firms did not renew their CBAs and signed new ones at expiry. There have been cases (subsequently condemned by labor courts) in which firms opted out of their original CBA before expiry. Firms that did not sign any CBA in the first place, but that were just applying the representative CBA for their sector, could begin adopting a new CBA immediately.

[8] The picture for firms changes slightly if weighting each firm by its size, as many large firms in the North of Italy also adopted pirate CBAs. See Appendix Figure B2.

Figure 1. The Evolution of Pirate CBAs

(a) Counts of contracts (b) Share of workers and firms with pirate CBA



Note: Panel (a) depicts the total number of collective bargaining agreements in Italy per year. Pirate CBAs are defined as those not signed by at least one union in the union triad. Panel (b) reports the share of firms (dashed line) and workers (solid line) with a pirate CBA. For firms, the share is computed as the number of firms adopting a pirate CBA for at least one employee as a share of the total number of private-sector firms in the INPS data each year. For workers, the share is computed as the total number of workers covered by a pirate CBA as a fraction of the total number of private-sector workers in the INPS data each year.

To illustrate the differences between pirate agreements and standard CBA's, we compare pirate CBAs to the representative CBA for the wholesale and retail sector, where – in terms of worker coverage - one of the largest pirate agreements was signed (see Appendix A). The pirate contract specifies lower wage floors, particularly at the low end of the distribution and allows for regional differentiation in pay.

4.2 Firm-Level Analysis

4.2.1 Firm-Level Design: Strategy

Difference-in-Differences Strategy Our difference-in-differences strategy compares treated firms—those that opt out—with a matched group of control firms that did not. We define the treatment year as the first year a firm uses a pirate CBA for at least one worker and focus on all firms for which the treatment year is included between 2008 and 2016, to ensure enough time periods before and after the event, as our data span 2005 to 2019. We then run the following event-study regression:

$$y_{j,t} = \alpha_j + \delta_t + \sum_{k \neq -1} \gamma_k \cdot 1\left[t = t_j^* + k\right] + \sum_{k \neq -1} \beta_k \cdot 1\left[t = t_j^* + k\right] \cdot T_j + v_{j,t} \quad (1)$$

Here $y_{j,t}$ is the outcome of interest for firm j in year t, α_j and δ_t are firm and year dummies and γ_k is a dummy denoting the number of periods relative to the event year, t_j^* (that is, the year of the op-out).[9] The treatment indicator T_j is equal to one if firm j adopts the pirate agreement and zero otherwise. The coefficients of interest (the β_k 's) capture the difference in $y_{j,t}$ between treated and control firms k years before/after the opt-out relative to the same difference in the year before the opt-out, which is normalized to zero. The main outcomes we focus on are the firm's average wage paid to its employees and its survival probability (a dummy taking value of one if the firm is observed in the data and zero otherwise).

Matching Strategy and Sample As noted above in Section 2, opting-out firms tend to differ from other firms in terms of size, sector, location and workforce composition. Table 1 shows descriptives for firms covered by pirate agreements in 2019. The table reveals that firms using pirate CBAs are larger, younger and tend to pay 4-5 percent less and to employ more part-time workers and more women. They also seem to be in more problematic financial conditions, as measured by short-term solvency (a dummy taking value of one if short-term assets are larger than short-term liabilities) and financial leverage.[10] All of these factors make treated firms not directly comparable to the average Italian firm. To obtain a more comparable set of control group firms, we implement a matching algorithm that assigns each treated firm to a control one with similar characteristics prior to the opt-out.[11] Potential control firms are all those that never applied a pirate contract.[12] We run a logit model relating the probability of opting out to the firm's average wage paid in the three years before the opt-out and dummies for firm sector, location and firm size deciles. We also include an indicator for whether the firm had negative short-term liquidity, measured as the difference between short-term assets and short-term liabilities (to proxy financial constraints), as well as an indicator for negative profits in the year before the opt-out. We then match on the estimated propensity score using a nearest-neighbor matching approach (NNM henceforth) (Abadie and Imbens, 2016).[13] The matching procedure delivers a balanced sample of 2,249 treated firms (roughly evenly split across treatment cohorts) and an equal number of control firms.

[9] For a control firm, this is the year of the adoption of the pirate agreement of the treated firm matched to this particular control firm. The next paragraph describes the 1:1 matching algorithm that we implement.
[10] The adoption of a pirate CBA is not typically a one-off event for firms. Out of firms adopting pirate CBAs between 2008 and 2012 and that survive for at least seven years since the first use, more than half of them still use a pirate CBA seven years after the adoption (Appendix Table B2).
[11] For a similar approach see Gathmann et al. (2020).
[12] We drop firm in small sectors such as agriculture, public administration, activities of households as employer and extraterritorial organizations.
[13] The matching algorithm is performed without replacement only on common support firms and within a 0.05 caliper.

Table 1. Pirate CBAs: Firm Descriptives

	Treated firms	Other firms
Firm size	18.06	9.45
	(370.56)	(195.99)
Firm age	10.35	14.11
	(11.41)	(13.36)
Log weekly wage	5.83	5.87
	(0.35)	(0.37)
Share of part-time	0.59	0.50
	(0.41)	(0.43)
Share of temporary	0.22	0.21
	(0.34)	(0.33)
Share of women	0.60	0.48
	(0.40)	(0.41)
Mean worker age	39.95	40.38
	(9.14)	(9.54)
Log Assets	6.15	6.45
	(1.58)	(1.63)
Short-term solvency	0.69	0.74
	(0.46)	(0.44)
Leverage	0.81	0.77
	(0.35)	(0.39)
Number of firms	45,102	1,515,270

Note: Treated firms defined as those applying a pirate CBA to at least one employee. "Short-term solvency" is an indicator taking value of one if short-term assets are larger than short-term liabilities. "Leverage" computed as $1 - equity/assets$. For treated firms, solvency and leverage are defined in the year before the first use of a pirate CBA. All statistics as of 2019, except for balance sheet variables for which the last available year is 2018. Standard deviations in parentheses.

Appendix Table B3 shows the balancing properties of the resulting sample. Importantly, treated and control firms are very similar in the year prior to the opt-out not only in the matching variables (average wage, size distribution, solvency) but also in many other characteristics that were not explicitly included in the matching algorithm such as workforce composition, age and gender as well as total assets.

Parallel Trends Assumption The causal interpretation of our results depends on whether the parallel trends assumption holds, according to which the outcomes would have evolved in the same way in treated and control firms in the absence of the treatment. The key limitation of our approach is that it only controls for selection on observables and is therefore undermined by the presence of unobserved firm characteristics that drive a firm's opt-out decision and are also correlated with trends in potential outcomes. To mitigate these concerns, we repeat this analysis at the worker level (to whom the employer's decision is likely exogenous), and present results from an additional strategy that builds on a mass opt-out event primarily motivated by disagreement about non-wage elements of the CBA.

4.2.2 Firm-Level Design: Results

We show firm-level event studies when Equation 1 is estimated on the matched firm sample. We focus on the impact of opt-outs on labor costs and survival probability relative to firms that do not opt out of standard bargaining agreements.

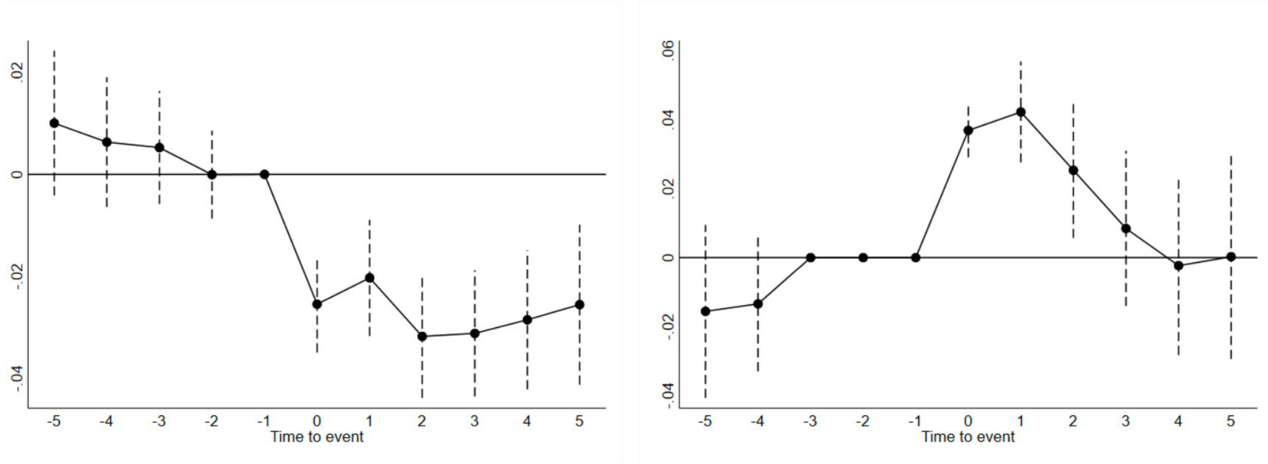
Wages Panel (a) of Figure 2 shows the event study plot for the average (log) weekly wage paid by the firm. Coefficients in the pre-event periods are indistinguishable from zero. We then observe a sudden decline in labor costs of about 3 percent in the opt-out year, which remains roughly stable in the ensuing years.

Firm Survival This reduction in firm labor costs is mirrored by a larger survival probability for treated firms relative to control firms in the opt-out year of about 4 percentage points (Figure 2, Panel (b)). This probability remains positive the two years after the opt-out but declines towards zero afterwards. (Our outcome variable is the year-to-year survival indicator.)

DiD Effects and Other Outcomes Table 2 shows estimates from the difference-in-differences version of Equation (1), which pools post-event coefficients together, for a range of other outcomes. First, we compute the mean wage separately for pirate and non-pirate workers within treated firms to shed light on whether the overall drop in labor costs seen in Figure 2 is driven by pirate workers only. We confirm that this is the case and that, in fact, wages of workers on standard CBAs rise in treated firms relative to control firms in the years following the opt-out. We also find that treated firms grow around 8 percent more than control firms after the event and witness increases especially in the number of temporary and blue collars workers, women and young workers. Opting-out firms improve in terms of solvency and assets. The probability of being solvent is 2 percentage points higher than for control firms, while total assets rise by 4 percent. We do not find significant effect on profits.

Figure 2. Pirate CBAs Design: Event Study Coefficients - Firms

(a) Log weekly wages | (b) Survival probability



Note: Panels (a) and (b) estimate the event study design of Equation (1) using as outcome the average log weekly wages at firm j and a dummy equal to 1 if firm j is active in year t. 95% confidence intervals are obtained after clustering the standard errors at the firm level.

Table 2. Pirate CBAs: Diff-in-Diff Coefficients - Firms

	Mean wage pirate	Mean wage standard	(Log) Firm size	Temporary
Coefficient	-0.03	0.06	0.08	0.09
	(0.01)***	(0.01)***	(0.02)***	(0.02)***
Mean	5.95	5.95	2.70	0.24
S.D.	0.34	0.34	1.43	0.81
N	46,180	46,180	55,402	55,402
R-squared	0.78	0.80	0.89	0.38

	Full time	White collar	Blue collar	Women
Coefficient	0.16	0.11	0.14	0.13
	(0.05)***	(0.05)**	(0.03)***	(0.03)***
Mean	0.78	0.52	0.59	0.57
S.D.	1.87	1.26	1.83	0.96
N	55,402	55,402	55,402	55,402
R-squared	0.49	0.58	0.47	0.54

	Young (<35)	Solvency	Profit margins	(Log) Assets
Coefficient	0.10	0.02	-0.01	0.04
	(0.02)***	(0.01)**	(0.02)	(0.02)**
Mean	0.47	0.72	-0.01	7.02
S.D.	0.84	0.45	0.79	1.73
N	55,402	49,120	48,990	49,114
R-squared	0.48	0.54	0.15	0.95

Note: Difference-in-differences coefficients after the opt-out event, obtained from pooling post-event coefficients in Equation (1). "Mean wage pirate" and "Mean wage standard" denote average (log) wages among pirate CBA workers and standard CBA workers in treated firms (for control firms, they just denote the average wage). "Temporary", "Full time", "White collar", "Blue collar", "Women", "Young" are all computed as shares of employment at each firm in the year prior to the opt-out. "Profit margins" denote firm profits as a share of total revenues. Each regression controls for firm and year effects. Standard errors clustered by firm in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4.3 Worker-Level Analysis

We next conduct a similar analysis at the worker level and study the evolution of wages and employment when workers experience transitions from a standard to a pirate contract, due to changes of the firm's agreement.

4.3.1 Worker-Level Analysis: Strategy

Difference-in-Differences Strategy Again, we estimate a similar event study design as that of Equation 1, which is now run at the worker level:

$$y_{i,t} = \alpha_i + \delta_t + \sum_{k \neq -1} \gamma_k \cdot 1[t = t_i^* + k] + \sum_{k \neq -1} \beta_k \cdot 1[t = t_i^* + k] \cdot T_i + v_{i,t} \quad (2)$$

where i denotes workers, T_i is the treatment indicator and t_i^* is the year when worker i experiences a within-spell opt-out, i.e. the employing firm's contract has moved from the standard collective bargaining agreement to a pirate one. All other variables are the same as described for Equation 1.

Identification of Worker-level Events To identify worker-level events, we proceed as follows. To address worker selection into (or out of) pirate contracts we only focus on transitions occurring within job spells and not between different jobs. For each worker, we define a dummy variable taking value one in a given year if the employing firm changes her CBA from a standard to a pirate one during her job spell at the firm. To correctly identify opt-outs and pin down the precise year when they occur, we resort to the more granular monthly data available at INPS.[14] Because the monthly CBA variable can sometimes be quite jumpy, we impose the following restrictions when defining the opt-out. A worker experiences an opt-out in month m if the firm has been in a standard CBA in months $m-2$ and $m-1$ of the same spell, then moves to a pirate CBA in month m and is still covered by that CBA in month $m+1$ of the worker's spell.[15] We further drop the (very few) spells in which at least two transitions of the same nature (e.g., standard-to-pirate) occur within 12 months (which means that the worker had at least three CBA switches in 12 months). This procedure delivers about 265,000 opt-outs between 2005 and 2019. Almost half of these occurred in two years -2012 and 2015- when few very large firms opted out of their previous CBA, not considered in this analysis.[16] To have enough

[14] The CBA variable in the yearly INPS data is not best suited to this purpose as it is computed as the mode of monthly CBA observations, implying that some opt-outs may not be attributed to the correct year. For example, assume that a worker experiences a standard-to-pirate transition in August of a given year and that, following the opt-out, the worker changes job to one applying a standard CBA in December of the same year. The yearly CBA variable would not capture the original opt-out as it would not show that the worker was covered by a pirate agreement for a few months before leaving the firm.

[15] If the spell starts in month $m-1$ or $m-2$, or ends in month m, we still consider this to be an opt-out.

[16] As noted above, 2012 corresponds to the Fiat opt-out, when thousands of employees at the car manufacturer switched to a new firm-specific CBA. 2015 saw instead the mass opt-out from a few very large firms in the wholesale and retail

pre- and post-event periods, we again focus on the opt-outs that occurred between 2008 and 2016—-a total of around 165,000 CBA transitions. For those workers who are treated more than once, we keep the first opt-out. We also drop workers that, within the same calendar year, experience a transition back to a standard CBA after the opt-out.

Matching and Sample Table 3 documents that workers covered by pirate agreements tend to be slightly younger, more likely to be female, employed part-time or with a temporary contract, and lower earners. To perform the event study analysis, we construct a suitable control group, by matching on the worker level.

We first define a broad group of potential controls that include those workers who were never covered by a pirate CBA in their working history. Our worker sample excludes managers and apprentices and workers employed in small sectors (agriculture, public administration, activities of households as employer and extraterritorial organizations) in the year before the opt-out. We also impose a three-year employment and two-year tenure requirement for both treated and control workers at the time of the opt-out.

Similar to the firm-level analysis, each treated worker is matched with a suitable control worker with similar characteristics before the opt-out, using nearest-neighbor propensity score matching. Crucially, we impose that each treated worker be matched with a control worker covered by the same (standard) CBA in the year prior to the opt- out. We then estimate a logit model that relates a worker's probability of experiencing an opt-out to their age, gender, wage, contract status, firm size, and dummies for sector, location, and broad occupation.

trade sector, which we analyze more in detail in Section 5. (As we discuss later, the opt-out event occurred in 2011 but the difference in wage floors materialized starting from 2015, when the new CBA for the non-opting out firms was signed.) In this analysis we remove these two events to make sure that the results do not depend on their unique characteristics.

Table 3. Pirate CBAs: Worker Descriptives

	Total economy		Excl. manufacturing	
	Treated workers	Other workers	Treated workers	Other workers
Age	40.79	41.30	39.47	40.75
	(11.82)	(11.90)	(11.96)	(12.09)
Women	0.45	0.42	0.51	0.46
	(0.50)	(0.49)	(0.50)	(0.50)
Part-time	0.37	0.32	0.46	0.37
	(0.48)	(0.46)	(0.50)	(0.48)
Temporary	0.28	0.24	0.36	0.28
	(0.45)	(0.42)	(0.48)	(0.45)
Weekly wage	5.88	6.04	5.78	5.98
	(0.54)	(0.51)	(0.51)	(0.52)
Blue collar	0.59	0.56	0.57	0.54
	(0.49)	(0.49)	(0.49	(0.50)
White collar	0.33	0.35	0.37	0.37
	(0.47)	(0.48)	(0.48)	(0.48)
Apprentice	0.04	0.05	0.05	0.05
	(0.19)	(0.22)	(0.22)	(0.22)
Number of workers	421,210	14,710,559	326,447	11,063,546

Note: Treated workers defined as those covered by a pirate CBA. Data for 2019. Standard deviations in parentheses.

Our matching procedure delivers a balanced sample of 90,223 treated workers and an equal number of control workers. Appendix Table B4 (top panel) confirms the balancing properties of the matched sample. Treated and control workers are overall very similar in the year before the opt-out. Differences in their observable characteristics, though statistically significant due to the large sample size, are very small. Importantly, roughly 70,000 workers in the treatment group experienced their within-spell opt-out in very large firms in 2012 and 2015. In turn, our results would largely reflect these unique events if we ran event studies on the full matched sample. We, therefore, restrict the baseline estimation sample by dropping treated workers in very large firms in 2012 and 2015.[17] This step drastically reduces the sample size to 21,682 treated workers, along with their respective control, which is now roughly evenly split across treatment cohorts. The bottom panel in Appendix Table B4 shows the descriptive statistics for the baseline matched sample, separately for treated and control workers. As expected, the average firm size goes down dramatically from about 3,000 workers in the original matched sample to around 150 in the baseline sample. The share of blue collars also rises at

[17] In practice, we compute size percentiles in the universe of firms separately for 2012 and 2015, then exclude from the sample treated workers employed in firms in the 100th size percentile in 2012 and 2015.

the expense of white collars, and the average wage is smaller in the baseline sample relative to the original one.

Parallel Trends Again, valid identification relies on the parallel trends assumption. Moreover, for the worker-level analysis, the selection-related identification concerns we flagged above for firms are likely less severe because the opt-out decision is made by the firm, especially as in our sample construction, the CBA transition occurs within a job spell and is thus "imposed" on workers.

4.3.2 Worker-Level Analysis: Results

We now describe the worker-level results, i.e., estimates of Equation 2 on the matched sample of workers to study how within-spell opt-outs affect their wages and employment.

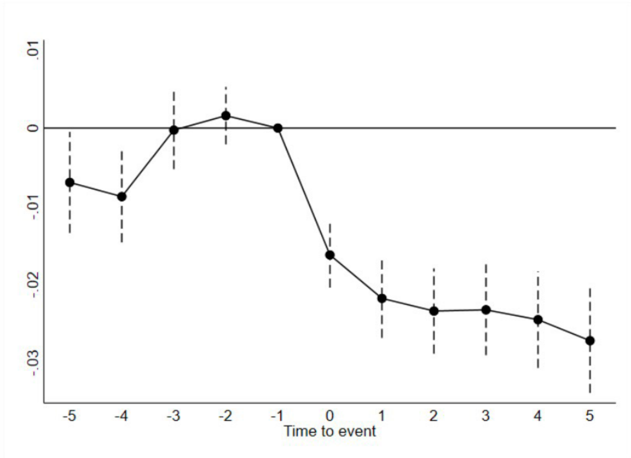
Event Studies Figure 3 plots the estimated coefficients for workers' wage, employment probability and probability of remaining at the opting-out firm. In line with the firm-level evidence, workers experiencing a within-spell opt-out face a sizable wage loss of about 3 percent following the CBA transition, which persists in the ensuing years. In contrast, the employment probability is larger for treated workers by about 2 percentage points relative to controls in the opt-out year. This difference increases to more than 3 percentage points five years after the event and is due, at least in part, to a higher probability of staying at the same firm as in the opt-out year.[18]

DiD Effects Table 4 shows difference-in-differences estimates for other worker outcomes. Notably, we estimate a positive impact on worker earnings, suggesting that the positive employment effect of the opt-out dominates the negative wage effect.

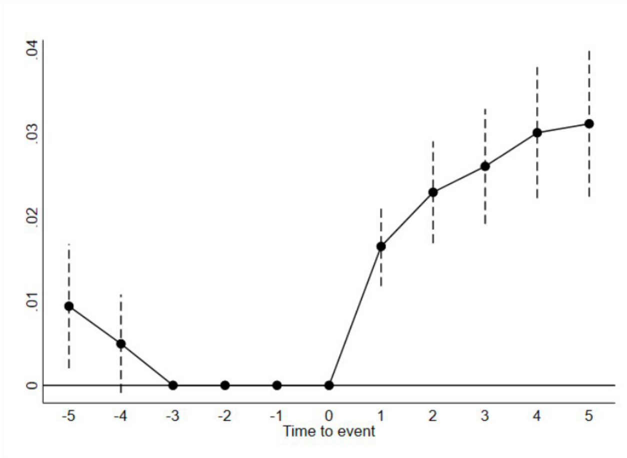
[18] As noted above, we show the baseline event studies for the matched sample excluding exceptionally large firms in 2012 and 2015. Results for the full sample are in Appendix Figure B4.

Figure 3. Pirate CBAs Design: Event Study Coefficients - Workers

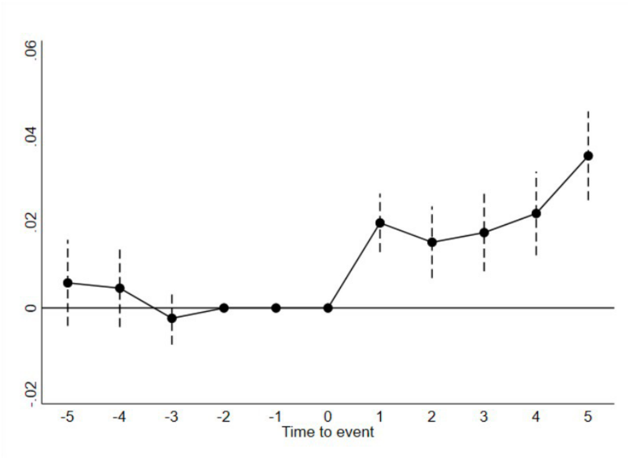
(a) Log weekly wages



(b) Employment probability



(c) Probability of remaining at the firm



Note: Panels (a), (b) and (c) estimate the event study design of Equation (2) using as outcome the log weekly wages, a dummy equal to one if worker i is employed in year t according to the INPS records and a dummy equal to 1 if worker i in period t is employed by a different employer than the one observed in year before the opt-out. 95% confidence intervals are obtained after clustering the standard errors at the worker level.

Table 4. Pirate CBAs: Diff-in-Diff Coefficients - Workers

	Weeks worked	Total earnings	Full time	Temporary
Coefficient	1.07	259.95	0.01	-0.01
	(0.16)***	(83.96)***	(0.00)***	(0.00)*
Mean	34.27	16443	0.67	0.09
S.D.	19.62	13019	0.47	0.28
N	650,460	650,460	556,491	556,491
R-squared	0.52	0.73	0.73	0.35

Note: Difference-in-differences coefficients after the opt-out event, obtained from pooling post-event coefficients in Equation (2). "Total earnings" are set to zero if a worker is not observed in the data. Each regression controls for worker and year effects. Standard errors clustered by worker in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4.4 Heterogeneous Effect of Pirate Agreements

Our results show that firm opt-outs have sizable effects on firm and worker outcomes, by lowering wages on the one hand and increasing firm survival and worker labor market attachment on the other. This evidence is consistent with the view that opt-outs serve as "safety valves" for firms during periods of economic distress. We now investigate this hypothesis by testing for more pronounced effects in firms in need of more flexibility, in the face of rigid and centralized standard CBAs.

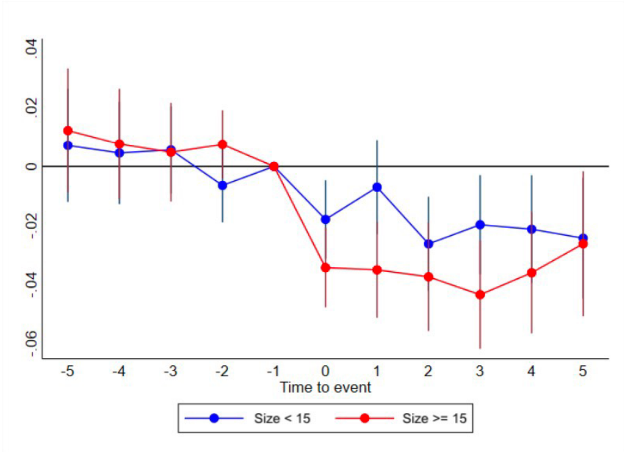
Firm size A first dimension of interest is firm size. Italian firms employing at least 15 workers face more stringent employment protection regulation, as they are obliged to reinstate workers on permanent contracts in case of dismissals due to economic reasons.[19] Figure 4 Panels (a) and (b) shows that the effects of the adoption of a pirate CBA on firm labor costs and survival probability are more marked in firms above the 15-employees threshold. We split treated firms in the matched sample into those employing up to 15 workers and those employing more than 15 workers in the year before the opt-out, and for each treated firm retain its matched control. Firms above 15 employees experience a larger reduction in labor costs of about 4 percent, versus 2 percent in smaller firms. The most dramatic difference is in firm survival probability (again, year-to-year), which is around 7 percentage points larger in treated firms above 15 workers relative to their controls immediately after the opt-out. This effect remains stable over time and still hovers around 5 percentage points five years after the event. In contrast, the increase in survival probability for smaller firms is more muted and only short-lived.

Panels (c) and (d) report effects for our worker-level design. We find that the wage loss suffered after the opt-out is limited to less than 2 percent for workers in small firms and more than doubles for workers in firms above the threshold. We also document that the positive impact found above on workers' employment probability is exclusively concentrated in larger firms, while there is no employment effect of opt-outs for workers in firms below 15 employees.

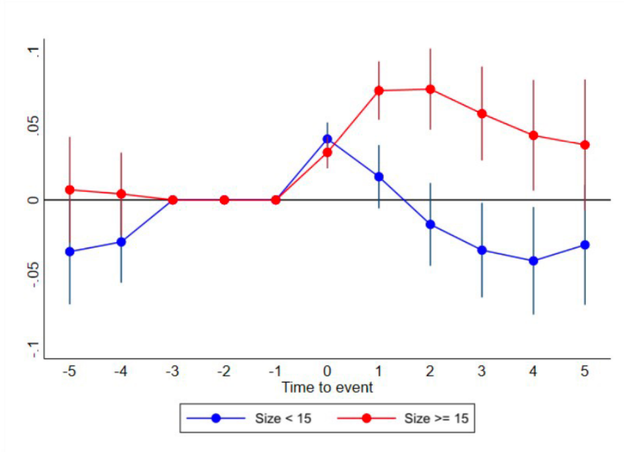
[19] This rule was relaxed in March 2015, when reinstatement was replaced by a severance payment, but this new law applied only to new hires (Boeri and Garibaldi, 2019). Moreover, firms with more than 15 employees face higher firing costs even after the reform because severance payments in case of dismissals are higher.

Figure 4. Pirate CBAs Design: Heterogeneity by Firm Size

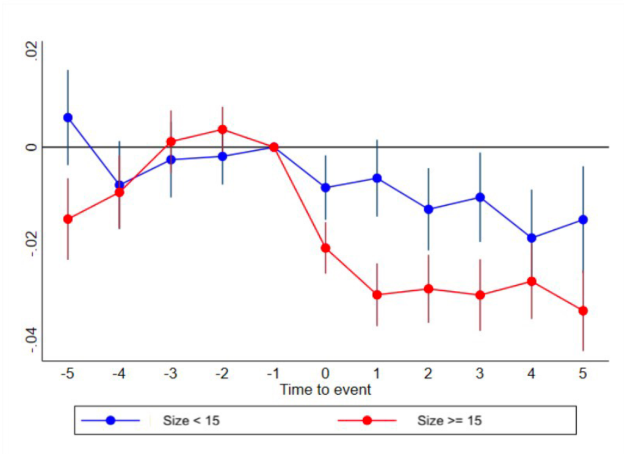
(a) Firms: Log weekly wages



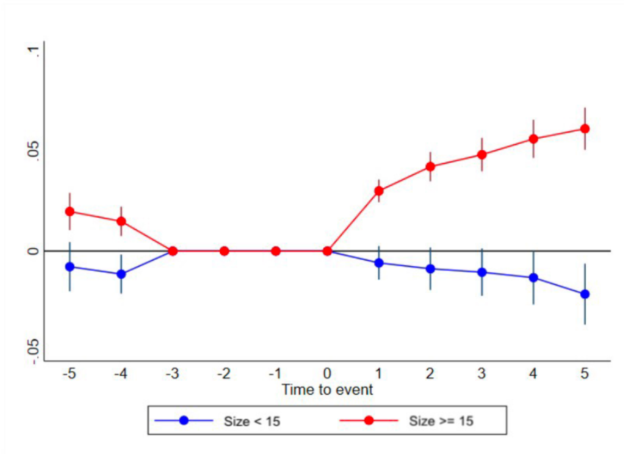
(b) Firms: Survival probability



(c) Workers: Log weekly wages



(d) Workers: Employment probability



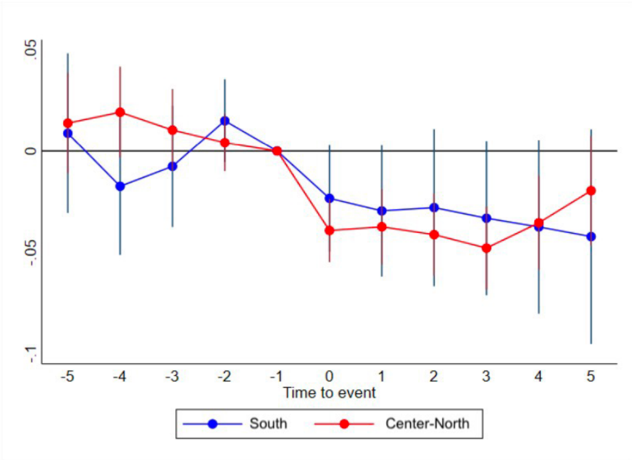
Note: The figure shows event-study coefficients by firm size. Panels (a) and (b) estimate the event study design of Equation (1) using as outcome the average log weekly wages at firm j and a dummy equal to 1 if firm j is active in year t. 95% confidence intervals are obtained after clustering the standard errors at the firm level. Panels (c) and (d) estimate the event study design of Equation (2) using as outcome the log weekly wages and a dummy equal to one if worker i is employed in year t according to the INPS records. 95% confidence intervals are obtained after clustering the standard errors at the worker level.

South vs. North In Figure 5, we report heterogeneity analyses by region (South/North). Incentives to apply pirate CBAs are larger in Southern regions (as documented in Section 4.1)—consistent with a view that low productivity leads national wage floors to bind, and in a more costly way. We therefore split the sample based on the location of treated firms, in the South versus other regions.[20]

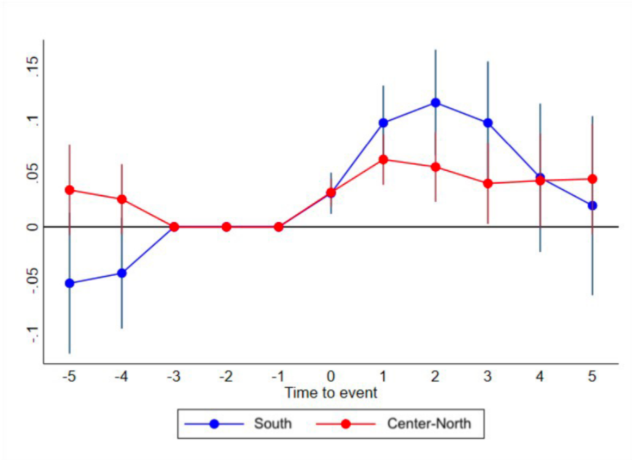
[20] We only focus on firms larger than 15 employees, to account for the fact that the average firm in the South is typically of smaller size.

Figure 5. Pirate CBAs Design: Heterogeneity by Geography (North/South)

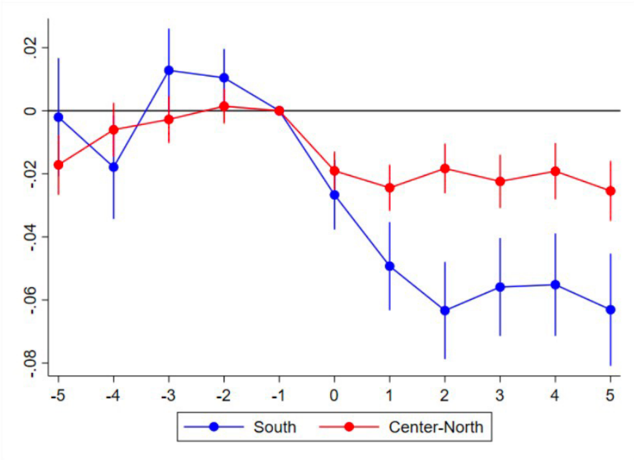
(a) Firms: Log weekly wages



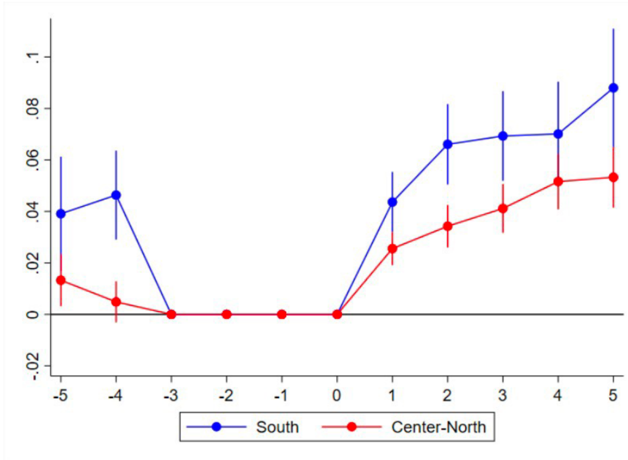
(b) Firms: Survival probability



(c) Workers: Log weekly wages



(d) Workers: Employment probability



Note: The figure shows event-study coefficients separately by region (South vs. Center North). Panels (a) and (b) estimate the event study design of Equation (1) using as outcome the average log weekly wages at firm j and a dummy equal to 1 if firm j is active in year t. 95% confidence intervals are obtained after clustering the standard errors at the firm level. Panels (c) and (d) estimate the event study design of Equation (2) using as outcome the log weekly wages and a dummy equal to one if worker i is employed in year t according to the INPS records. 95% confidence intervals are obtained after clustering the standard errors at the worker level. Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily.

While the effect of opt-outs on firm labor costs does not vary much with firm location, Southern firms seem to benefit particularly in terms of larger survival probability in the years immediately after the transition. As to workers, we estimate larger event-study coefficients for Southern workers both in their wage loss and for their employment probability.

5. Evidence from the 2011 Opt-Out Decision of Large Retailers

The previous section shows the consequences for firms and workers following a firm's decision to transit from a national CBA to a pirate agreement. Large employers could also unilaterally withdraw from the pre-existing CBA. If such an opt-out is coordinated and pursued by several firms in the same sector, then this group of firms can negotiate a new CBA with the main union rather than pirate unions.

This is what happened in 2011 when employers in the mass-retail sector (e.g., Ikea, Carrefour) decided to opt out from their employer organization—which represented *all* employers in retail and whose provisions were favoring small businesses—and formally established a new employer association with the purpose of separating their lobbying activities from those of smaller firms, whose objectives were often different. A similar event occurred in 2012, when FIAT opted out of existing agreements with the union. These events—especially Fiat's earlier opt-out—drew large attention in the media and are believed to have accelerated the transitions of many smaller firms into pirate agreements, which we discussed in the previous section.

Section 5.1 describes the institutional background surrounding this event. We then present the research design and results from this opting-out event on firms (Section 5.2) and on workers (Section 5.3).

5.1 Background: The 2011 Opt-Out of Large Retailers

The association representing employers in mass-retail is called *Federdistribuzione* (FD). Firms in FD include large hypermarket chains (e.g., Carrefour, as well as territorial supermarket chains), clothing (e.g., Coin), and furniture (e.g., Ikea, Leroy Merlin). In 2010, there were 56 firms in FD, representing around one-fourth of employment in the retail sector.

Until 2011, the CBA that FD firms were subject to was negotiated by the employer association *Confcommercio* (CC). The CC-CBA is the second largest CBA in Italy regarding employment coverage and is applied by all employers operating in the retail sector. The CC-CBA consists of unions and around 90 sub-associations of employers embodying the location- and sector- specific interests of different CC members (e.g., the sub-association of butchers, hotels, and stationers, ...), and is one of many specific sub-associations of employers in the retail sector (*Confcommercio,* CC).

The Opt-Out Event On December 23, 2011, firms in FD announced their exit from *Confcommercio*. The reason for the opt-out was the divergence of interests and objectives between the large employers belonging to FD and the small retailers that had a large influence on CC. Two were the points of contention. First, mass retailers were in favor of a full liberalization of shopping hours. Second, FD firms pushed for the opening of large shopping malls, whereas small retailers were actively lobbying local politicians to hinder them. As a result, FD firms realized that pooling their lobbying efforts with those of small retailers was not serving their interest and chose to leave CC. An indirect consequence of this decision was that, starting from the opt-out, FD firms would have to separately bargain with the unions to renew their CBAs.

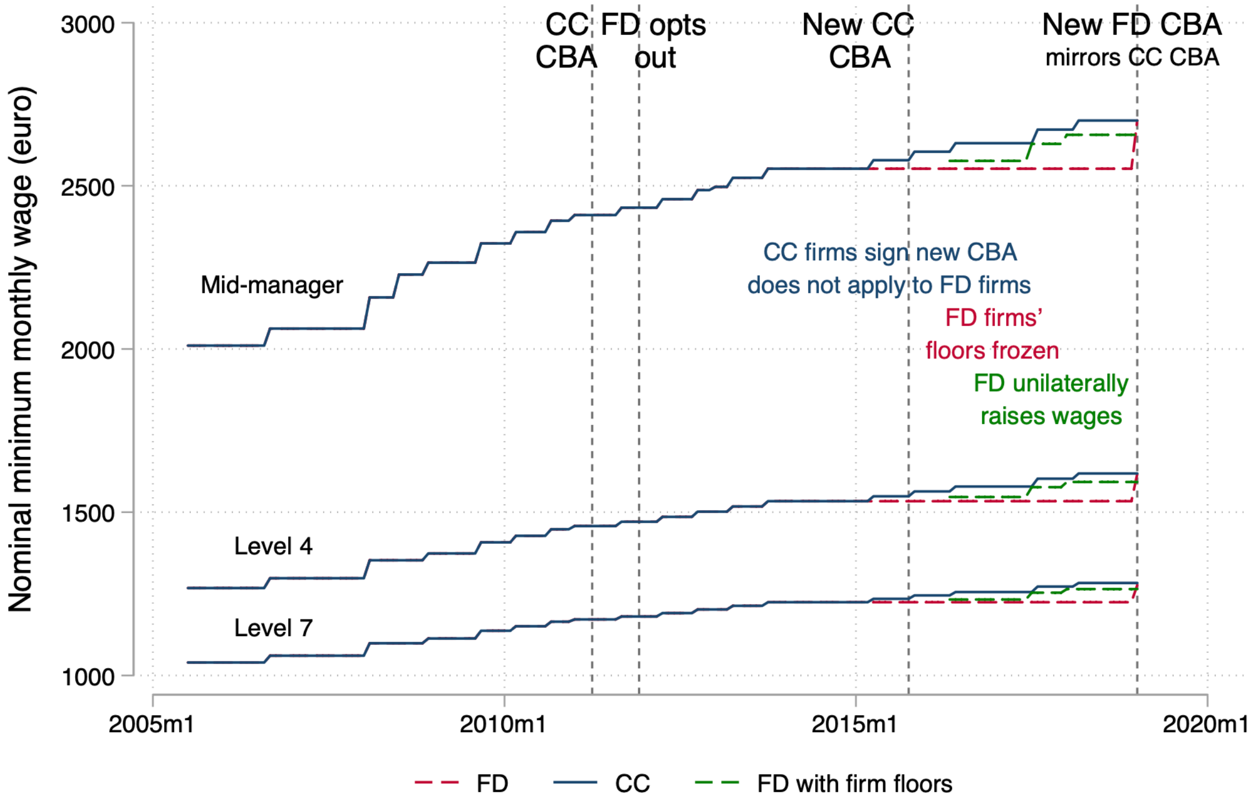
Impact on Wage Floors Lowering wage floors was, therefore, not a main reason for the opt-out, but the decision to leave CC had important consequences for wage floors. Figure 6 plots the evolution of wage floors between 2005 and 2020 separately for CC and FD firms for three job titles/job levels: level "7," the lowest ranked in the sector aside from apprentices, level 4, the typical occupation in the sector, and finally for middle-level managers, or "*quadri*."

Before 2011, FD was part of CC and hence was subject to the wage floors established in the CC-CBA. Wage floors also remained similar between 2012 and 2015, the reason being that a new 3-year collective agreement had come into force on January 1st, 2011. Because it was signed prior to the opt-out, it was still binding for FD firms, and stayed in force for all FD firms until its expiry at the end of 2013.

In 2014, wages did not immediately diverge, although the contract expired, as the implementation of a new contract was postponed due to delays in negotiations between CC and the unions. A new CC-CBA was finally agreed on March 30, 2015, covering the period of April 2015 to March 2018, and establishing significant raises in wage floors. This new contract applied to CC firms only, as FD firms did not sign the new CC-CBA and did not sign a separate CBA with unions. Hence, wage floors in FD firms started to diverge from CC wage floors in 2015. During the period 2015-18, the wage differences were quite substantial, with a peak in 2018 of up to 8.4%.

This period of divergence in wage floors lasted until January 2019, when a new FD contract went into force (signed on December 19, 2018). While formally a separate agreement, the new FD contract simply replicated the wage floors of the prevailing CC-CBA, and hence the two lines converge again in 2019.

Figure 6. Retailer Opt-Out Design: Timeline of Wage Floors for Three Job Titles



Note: The figure shows minimum wages for the workers of firms remaining in Confcommercio (CC, in solid blue lines) and for the Federdistribuzione (FD) firms that opt out of the collective agreement (in red dashed lines) between 2005 and 2020. Three out of eight occupational levels are displayed. The green dashed lines depict the minimum wages enforced by FD firms with unilateral raises.

Conflicts in Industrial Relations Various additional conflicts between FD and unions occurred during the opt-out period from 2011 to 2018. For instance, Figure 6 also reports (green dashed line) the wage policies FD voluntarily and *unilaterally* imposed from 2015 to 2018. During this period, FD and the unions failed to come to a new agreement. Unions rejected FD's proposal to merely replicate CC-CBA wage floor increases—which unions perceived as too low and to be designed for struggling, small and unproductive CC retailers, whereas they believed the large FD firms should provide larger real wage increases. As part of this conflict, the unions implemented three short strikes.[21] On top of the conflict about wages, FD firms also engaged in other hostile actions (e.g., cutting the generosity of supplementary health insurance and abolishing lower-level industrial relations councils, *Enti Bilaterali Territoriali*). This period of adversarial industrial relations ended in 2018, as FD employers and unions signed the first mutually agreed on FD-CBA. This new CBA precisely replicated the CC wage floors (that had been agreed on in 2015 for 2018) and most of the

[21] The strikes were: Nov 7, 2015 with 9.4% of workers participating, Dec 19, 2015 with 8.6%, and May 25, 2016 with 6.5%.

other provisions. While FD and CC continue to bargain separately, the 2022 CBAs for both associations remain close on all dimensions, sharing the same wage floors.

We view the actions taken by FD during the opt-out period—including the unilaterally imposed wage policies in green dashed lines depicted in Figure 6—as *outcomes* of the opt-out: FD firms voluntarily and indeed unilaterally formalized their wage policies as described above. Importantly, existing empirical evidence from Italy (Fanfani, 2020) and Portugal (Card and Cardoso, 2022) show that the pass through from wage floors to realized wages is only partial, suggesting that firm-level bargaining partially offsets the effect of sector-level agreements. The increase in wage floors granted by FD firms falls into the firm-level policies that firms adopt to attenuate the effect of collective bargaining. However, we preview those specific actions here in the institutional review to complement our empirical analysis of actually paid wages.

5.2 Firm-Level Analysis

5.2.1 Firm-Level Design: Strategy

Difference-in-Differences Strategy We use an event-study specification in difference-in-differences, comparing outcomes of firms that opt out in the treatment group (i.e., FD firms) with control firms (i.e., CC firms) that did not, before and after the opt-out event (2011):

$$y_{j,t} = \alpha_j + \delta_t + \sum_{t \neq 2010} \beta_t \cdot FD_j + v_{j,t} \quad (3)$$

Here $y_{j,\,t}$ is the outcome of interest for firm j in year t, α_j and δ_t are firm and year dummies and FD_j is a dummy equal to one for firms belonging to FD association in 2010. The coefficients of interest (the β_t's) capture the difference in $y_{j,\,t}$ between FD and CC firms in the years leading and following the decision of FD firms to abandon the major employer organization.

Sample and Matching Table 5 displays the average characteristics of firms that opted out and remaining firms in CC that have more than 15 employees, for 2010, the year before the opt-out. The table shows that FD firms are larger than CC firms. This reflects the context of the opt-out decision: FD firms are large firms that aimed to escape a CBA designed for small and medium sized firms and thus viewed as unsuitable for large conglomerates for a variety reasons. FD use more part-time contracts and employ a higher fraction of women. There are a few margins where both FD and CC firms appear similar. Wages, for instance, are not very different despite the enormous differences in firm size, plausibly reflecting their shared CBA during that period.

Table 5. Retailer Opt-Out: Descriptive Statistics of the Full and Matched Samples of Firms

	Full sample		Matched sample	
	Treated FD	Control	Treated FD	Control
Firm size	2,991	65.88	1,468.81	1,664.33
Firm age	18.69	19.27	17.45	24.02
Mean log weekly wage	6.29	6.25	6.32	6.31
Share of full-time	0.54	0.64	0.57	0.59
Share of temporary	0.04	0.04	0.03	0.04
Share of women	0.42	0.33	0.42	0.36
Mean worker age	37.47	38.61	38.10	37.66
Log assets	11.64	8.55	11.09	10.49
Short-term solvency	0.44	0.73	0.55	0.57
Leverage	0.74	0.82	0.74	0.78
N. firms	54	14,679	42	42

Note: This table reports averages of the characteristics by group: FD firms and CC firms in 2010. The sample includes firms that in the year of the FD opt-out were active for three years and in the year before the FD opt-out employed more than 15 employees and at least one worker in the CC CBA. Only firms with an obligation to report a balance sheet are included. Mean wages and financial variables are expressed in euros. The 2010 FD firms are: A&O, Assofranchising, Auchan, Bennet, Bricocenter, Bricoman, CA, Cadoro, Carrefour, Coin, Conbipel, Conforama, Decathlon, Despar - Eurospar - Spar, Di' Per Di', Douglas, Eldo, Elite, Emmezeta Moda, Esselunga, Etruria Retail, Euronics, Expert, Famila, Finiper, Fiordaliso, Gre, Grosmarket, Gruppo Vege', Gruppo Zambaiti, GS, Ikea, Il Gigante, In'S Mercato, Iperal, Italmark, Jysk, Kiko, La Gardenia, Ld Market, Leroy Merlin, Limoni, Max & Co., Max Mara, Maxi Zoo, Mediaworld - Saturn, Metro Italia, Gruppo Miroglio Fashion, Oasi - Gruppo Gabrielli - Tigre, OVS, Pac 2000A, Pam - Panorama, Pellicano, Penny Market, Percassi, Prix, Rinascente, Selex, Self, Sinergy, Sisa, SSC, Superconti, Unes, Unieuro, Universo Sport, Zara.

Given the pre-existing differences between firms in FD and CC, we implement a similar strategy as in Section 4.2 to ensure that *trends* in key economic outcomes are similar between FD and CC firms. That is, we define our treatment year as 2011, where firms in FD are our treatment group. The pool of potential controls is represented by firms in CC with more than 15 employees in 2010. We then fit a propensity score model identical to the one used when studying pirate agreements but excluding firm size in the propensity score matching. This is because *essentially all* large firms departed the CC sector, and thus matching on firm size would lead to a failure of the overlap condition. Column 3 and 4 shows that after performing NNM we obtain a sample where FD and CC firms are now similar along several observable characteristics.

Limitations The research design suffers from two important limitations. First, firm size is unbalanced between treatment and control groups. If trends in key economic outcomes are moving in systematically different ways between large and small firms in the years leading up to the opt-out event, then this affects the interpretation of the coefficients β_t. Another limitation concerns spillover effects on workers in firms covered by the CBA (Bassier, 2022). Unlike in the pirate agreement context in Section 4 (where a single firm's opt-out is unlikely to entail large general equilibrium effects), the FD firms coordinated their opt-out and were large and made up a significant share of the original CC sector's employment and sales. Our research design will not capture these effects. In an attempt to account for these limitations, we also explored a triple difference-in-difference design and found qualitatively similar effects.[22]

5.2.2 Firm-Level Design: Results

Wages Figure 7 shows the effects of the opt-out decision made by FD firms on the average weekly wages and the probability to remain active. Weekly wages tend to exhibit common trends between FD and CC firms in the years leading up to the opt-out decision. From 2011 to 2015, years where FD and CC firms still share the same CBA as detailed in Section 5.1, there are no significant differences in average wages. The same holds also post-2015, years in which the CBAs applied by FD and CC firms start to be different.

Interestingly, however, when looking at the worker-level impact in the next section, we see that the opt-out decision led to a decrease in wages of workers employed by FD firms which were particularly pronounced for workers that remained in FD firms post-opt-out decision ("stayers"). A possible reason for this divergence in results is the weighting as the worker-level effects weigh more observations of workers employed in large firms. Future versions will contain a firm-size weighted version of the firm-level analysis that could be more easily compared with the worker-level effects.

Firm Survival Despite the insignificant effects on (raw) wages, we see that the opt-out decision leads to positive effects on the probability that FD firms remain active (i.e., do not close), particularly post-2015. We caveat, however, that these effects appear somewhat imprecisely estimated.

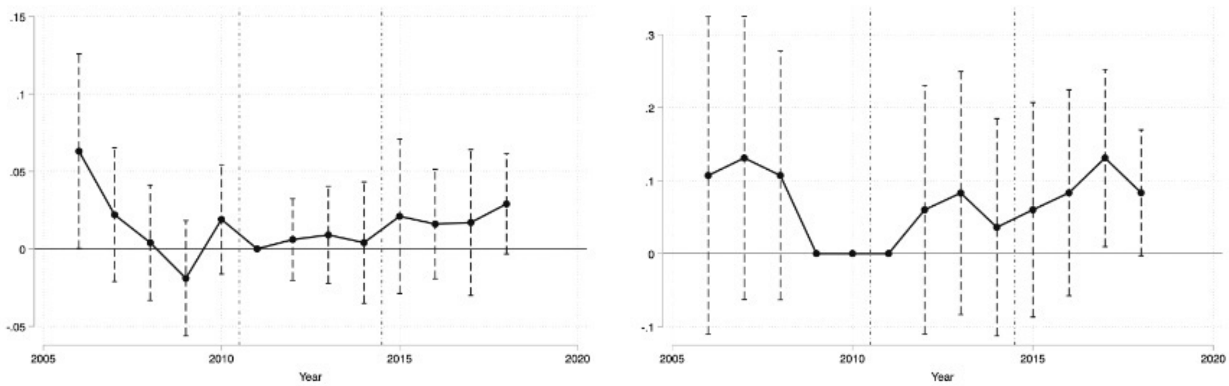
[22] Specifically, we matched firms in either FD or CC in 2010 to similar firms not implementing the CC-CBA in 2010 and thus were unlikely to be affected by the opt-out decision. Essentially, we compare the simple double difference (between workers employed by FD vs. CC firms) with a placebo double difference among the matched control firms. The latter difference permits us to control for differential trends between large and small enterprises that could impact worker-level outcomes even in the absence of the opting-out event.

DiD Effects and Other Outcomes Furthermore, Table 6 shows the impact of the opt-out on other firm-level outcomes using a difference-in-differences specification based on Equation (3). We find that the opt-out decision made by large retailers does not appear to lead to significant changes on a variety of firm-level outcomes.

Figure 7. Retailer Opt-Out Design: Event Study Coefficients - Firms

(a) Average log weekly wages at firm

(b) Firm survival probability



Note: Panels (a) and (b) estimate the event study design of Equation (3) using as outcome the average log weekly wages at firm j and a dummy equal to 1 if firm j is active in year t. 95% confidence intervals are obtained after clustering the standard errors at the firm level.

Table 6. Retailer Opt-Out: Diff-in-Diff Coefficients - Firms

	Mean log wage	Mean log wage stayers	Log firm size	Temporary†
Coefficient	0.00	-0.00	-0.03	0.06
	(0.02)	(0.02)	(0.17)	(0.07)
Mean	6.30	6.32	5.48	0.08
S.D.	0.26	0.26	2.17	0.51
N	1,095	1,008	1,095	1,095
R-squared	0.92	0.92	0.93	0.39
	Full time	**White collar**	**Blue collar**	**Women**
Coefficient	0.04	0.01	0.02	0.00
	(0.02)*	(0.03)	(0.02)	(0.02)
Mean	0.53	0.49	0.13	0.36
S.D.	0.22	0.23	0.20	0.16
N	1,095	1,095	1,095	1,095
R-squared	0.84	0.82	0.90	0.82
	Young (<35)	**Temporary**	**(Log) Productivity**	**(Log) Labor Cost**
Coefficient	-0.00	-0.00	0.11	0.16
	(0.02)	(0.01)	(0.09)	(0.14)
Mean	0.22	0.04	3.70	8.99
S.D.	0.14	0.07	0.89	1.99
N	1,095	1,095	985	1,053
R-squared	0.84	0.41	0.77	0.95

Note: Difference-in-differences coefficients after the retailer opt-out event, obtained from pooling post-event coefficients in Equation (3). "Mean wage stayers" denote average (log) weekly wages of workers employed by the same firm in the previous year. "Temporary", "Full time", "White collar", "Blue collar", "Women", "Young" represent the firm-level share of a given group for firm j in year t. "Profit margins" denote firm profits as a share of total revenues. Temporary† represents the number of temporary workers of firm j in year t divided by firm size in the year 2010. Each regression controls for firm and year effects. Standard errors, clustered at the firm level, are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

5.3 Worker-Level Design

We now move to the implications of the opt-out decision on workers.

5.3.1 Worker-Level Design: Strategy

Difference-in-Differences Strategy We again estimate a regression specification mirroring Equation (3), now at the worker level, with treated workers being those employed in an FD firm in 2010 and control workers drawn from CC firms that do not opt out:

$$y_{i,t} = \alpha_i + \delta_t + \sum_{t \neq 2010} \beta_t \cdot FD_{j(i,2010)} + v_{i,t} \quad (4)$$

where *j(i,2010)* is a function that returns the identity of the employer of worker i in 2010 and FD_j is again a dummy equal to one if employer j belongs to FD association. The treatment year is again 2011.

Sample and Matching Our treatment group are workers employed by a FD firm in 2010. Employees of CC firms in 2010 represent the set of potential control workers. Akin to the strategy described in Section 4.3.1, we only consider workers employed at least two years with their 2010 employer and were also working in 2007. The propensity score matching used to match FD workers to suitable control workers is the same—except for our omitting again firm size when estimating propensity score due to the inherent imbalances in this variable (see Section 5.2.1).

Table 7 shows the characteristics of workers. After matching, we obtain a sample of roughly comparable workers between treatment and control groups. Moreover, the subset matched FD workers are also similar to the overall sample of employees in FD firms.

Table 7. Retailer Opt-Out: Descriptive Statistics of the Full and Matched Samples of Workers

	Full sample		Matched sample	
	Treated FD	Control	Treated FD	Control
Woman	0.60	0.47	0.59	0.58
Age	38.17	39.32	38.85	38.67
Full time	0.59	0.82	0.64	0.64
Temporary contract	0.01	0.01	0.01	0.01
Log weekly wage at t	6.21	6.27	6.23	6.22
Blue collar	0.07	0.35	0.07	0.08
White collar	0.91	0.61	0.90	0.89
Mid manager	0.03	0.04	0.03	0.03
Firm size	3,292.47	62.10	3,292.47	69.42
N. workers	102,959	422,705	92,507	92,507
N. firms	49	16,222	49	10,706

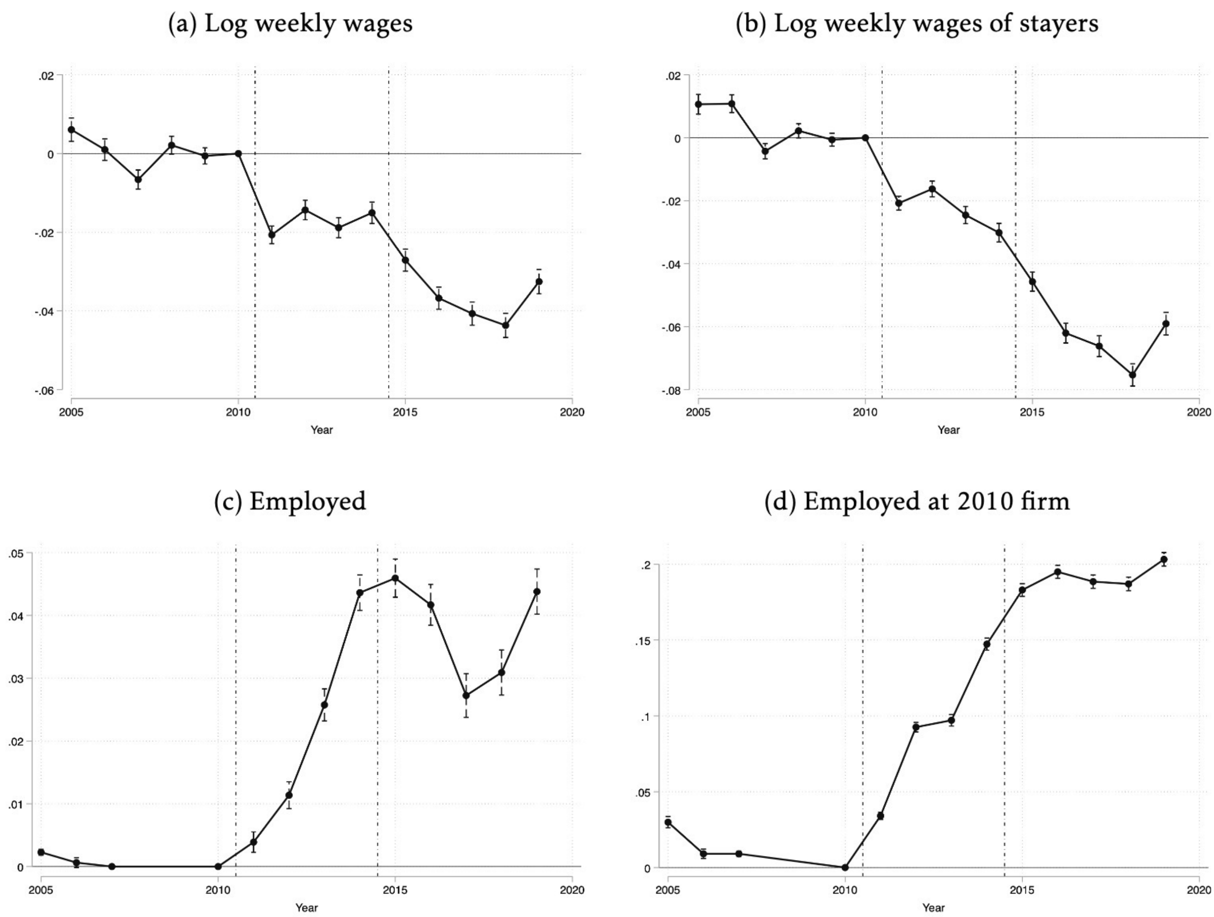
Note: This table reports averages of the characteristics by group: workers in FD firms in 2010, and those in CC firms (but not FD) in 2010. Firm size is firm-weighted.

5.3.2 Worker-Level Design: Results

Event Studies The event-study results based on (4) are shown in Figure 8. While the wages of workers in FD and CC firms display a similar trend in the year preceding the opt-out decision, following the opt-out decision we see a drop in the log wages of around 2% immediately following the decision to abandon the CC employer organization made by FD firms. We find these negative effects to accentuate post-2015, perhaps due to CC firms increasing wage floors while FD firms left them unchanged. These negative wage effects on workers are, however, to be contrasted with the positive employment effects displayed in Figure 8 Panel (b).

It appears, therefore, that the opt-out decision of large retailers leads to effects on workers akin to what we observe in the case of opting out via the adoption of pirate agreements: lower wages but higher employment probabilities. To confirm that this positive employment effect is driven by the fact that the opt-out decision gave FD firms more flexibility and thus increased retention probabilities, Panel (c) of Figure 8 shows that the event-study coefficient on an indicator equal to 1 if worker *i* in period *t* is employed with their 2010 employer. The opt-out event leads to a systematic increase in the probability of observing the retention of employees in FD firms.

Figure 8. Retailer Opt-Out Design: Event Study Coefficients - Workers



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Note: Panels (a), (b), (c) and (d) estimate the event study design of Equation (4) using as outcome the log weekly wages of all workers, the log weekly wages of workers employed by the 2010 employer, a dummy equal to one if worker *i* is employed in year *t* according to the INPS records and a dummy equal to 1 if worker *i* in period *t* is employed by a different employer than the one observed in year 2010. 95% confidence intervals are obtained after clustering the standard errors at the worker level.

DiD Effects Table 8 shows the implied difference-in-differences coefficient on the outcomes described in Figure 8 along with some additional outcomes. Importantly, when looking at earnings (and setting earnings to zero for workers non-employed in a given year), we find an overall positive earnings effect consistent with the pirate agreement analysis. Interestingly, it also appears that opt-outs increased labor supply (along the intensive margin) as seen by the results on weeks worked and full-time, while also decreasing the probability to be employed temporarily, a result also in line with the analysis on pirate agreements.

Table 8. Retailer Opt-Out: Diff-in-Diff Coefficients - Workers

	Log weekly wages	Employment probability	Probability of remaining at the firm	Earnings
Coefficient	-0.03	0.03	0.13	482.05
	(0.00)***	(0.01)***	(0.04)***	(42.99)***
Mean	6.20	0.92	0.76	21,617
S.D.	0.34	0.27	0.43	13,263
N	2,396,046	2,014,856	2,014,856	2,569,898
R-squared	0.666	0.484	0.532	0.739
	Weeks worked	**Full time**	**Temporary**	
Coefficient	2.29	0.06	-0.03	
	(0.04)***	(0.00)***	(0.00)***	
Mean	40.90	0.65	0.04	
S.D.	14.33	0.48	0.20	
N	2,396,054	2,396,046	2,396,046	
R-squared	0.494	0.805	0.258	

Note: Difference-in-differences coefficients after the retailer opt-out event, obtained from pooling post-event coefficients in Equation (4). Each regression controls for worker and year effects. Standard errors clustered at the worker level. * p<0.10, ** p<0.05, *** p<0.01.

6. Summary and Conclusions

Centralized collective bargaining regimes are common in many European countries. While often praised for redistributing productivity gains from firms to workers, they are also blamed for their rigidity and inflexibility. Unsurprisingly, intense debate exists about reforming collective bargaining frameworks by introducing additional flexibilities considering firm heterogeneity and local conditions. To date, however, very little is known about the effects of such reform on firms and workers, which is what we study in this paper.

Focusing on Italy, a country characterized by particularly rigid industrial relations that came under intense scrutiny following the Great Recession, we analyze two events, one where firms left centralized collective bargaining agreements to reach arrangements with smaller and often local unions and another where a group of large employers renegotiated with national unions. We find evidence that opting out of national collective bargaining agreements lowers firms' labor costs while increasing their survival probabilities. Moreover, workers in those firms experience wage losses but higher employment stability and earnings. These effects are larger in firms facing stricter employment protection regulations and located in the less productive regions in the South of Italy.

Our analysis suggests a trade-off between employment stability and firm survival on the one hand, and lower wages of workers on the other. Our finding that earnings of workers who worked in firms that opted out of national collective bargaining agreements increased over a prolonged period suggests that – from the worker perspective, and for the case of Italy – flexibilization of the sort we investigate in this paper had overall beneficial effects.

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A. Appendix A

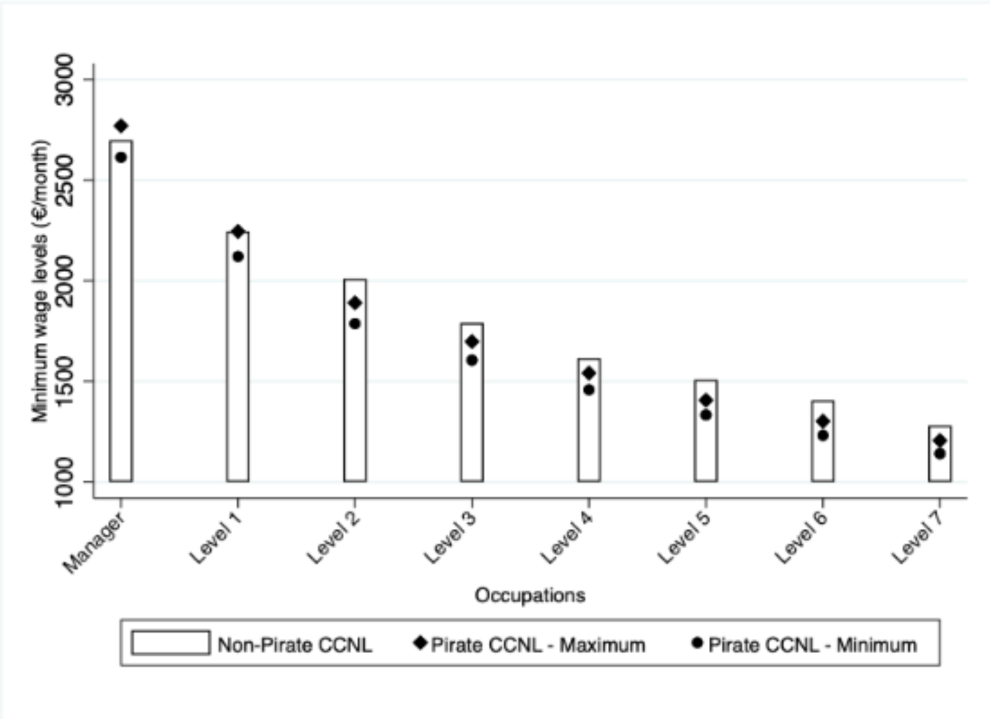
Here we present a case study comparing a pirate contract to the standard CBA in the trade sector. We choose this sector as one of the most prominent pirate CBAs was signed there, covering roughly the same occupations as the corresponding standard CBA, which makes comparisons more meaningful. The standard CBA in the trade sector (CNEL code H011) was first signed in 1967 by the dominant associations *Confcommercio* (employer) and CGIL, CISL and UIL (the three main unions). This contract had then been renewed periodically and became the "representative" CBA in the sector, covering almost 400,000 employers and 2.4 million workers in 2018. In 2012, newborn employer and employee associations (Confazienda, Fedimpresa, Unica and Cisal) signed a new CBA in the sector (CNEL code H024), which by 2018 covered 731 firms and 12,000 workers. Below is an excerpt of the text of the pirate contract:

The old contracts prefer the death of companies and jobs rather than giving in, albeit marginally, to previous economic and regulatory achievements [...]. The system thus prefers to talk about "Pirate Contracts" whenever there is a search for a contractual solution compatible with the existing difficulties [...]. Any CBA that is not a bad copy of the corresponding text written down by the so-called "comparatively more representative trade unions at national level" is qualified as a "pirate" [...]. The knowledge of the market situation by all the parties involved (Companies, Workers, Trade Associations and Trade Unions) [...] is the only contractually possible way to effectively combat the crisis [...]. The Parties now find anachronistic the claim to define all the various contractual institutions and salaries in a homogeneous way for the entire national territory, which has many and significant heterogeneities [...]. The choice of this CBA is: (a) to lay down essential wages and standards which meet the primary needs of all workers; (b) to give priority to second-level bargaining; c) to recognize a Regional Equalization Element, proportionate to the Regional Cost of Living Indices, to reduce differences in purchasing power at the same nominal wage.

Notably, the signatory parties acknowledge the issue, posited in Boeri et al. (2021), that nominal wages should be adjusted to better reflect productivity levels across the country. To this purpose, a *Regional Equalization Element* is introduced on top of the national wage floor and larger in regions with higher cost of living. Figure A1 provides a comparison of the wage floors envisaged by the representative CBA to the floors introduced by the pirate CBA. Each CBA defines so-called *livelli di inquadramento* (granular occupations) and sets a wage floor for each level. While there is not necessarily a one-to-one correspondence between occupations across different CBAs (even if regulating the same sector), we inspect the contract texts to make sure that the occupation levels defined by these two contracts are broadly comparable. For the pirate CBA, two wage floors are depicted for each level representing the wage floor in the region with the highest (Lombardia, in the North) and lowest (Molise, in the South) Regional Equalization Element, respectively.[23]

[23] The Equalization Element amounts to roughly 5.3 percent of the (national) wage floor in Lombardia. In Molise, this percentage is 0.4 percent for managers up to 0.9 percent for the lowest occupation.

Appendix Figure A1. Wage Floors in the Wholesale and Retail Sector, 2018



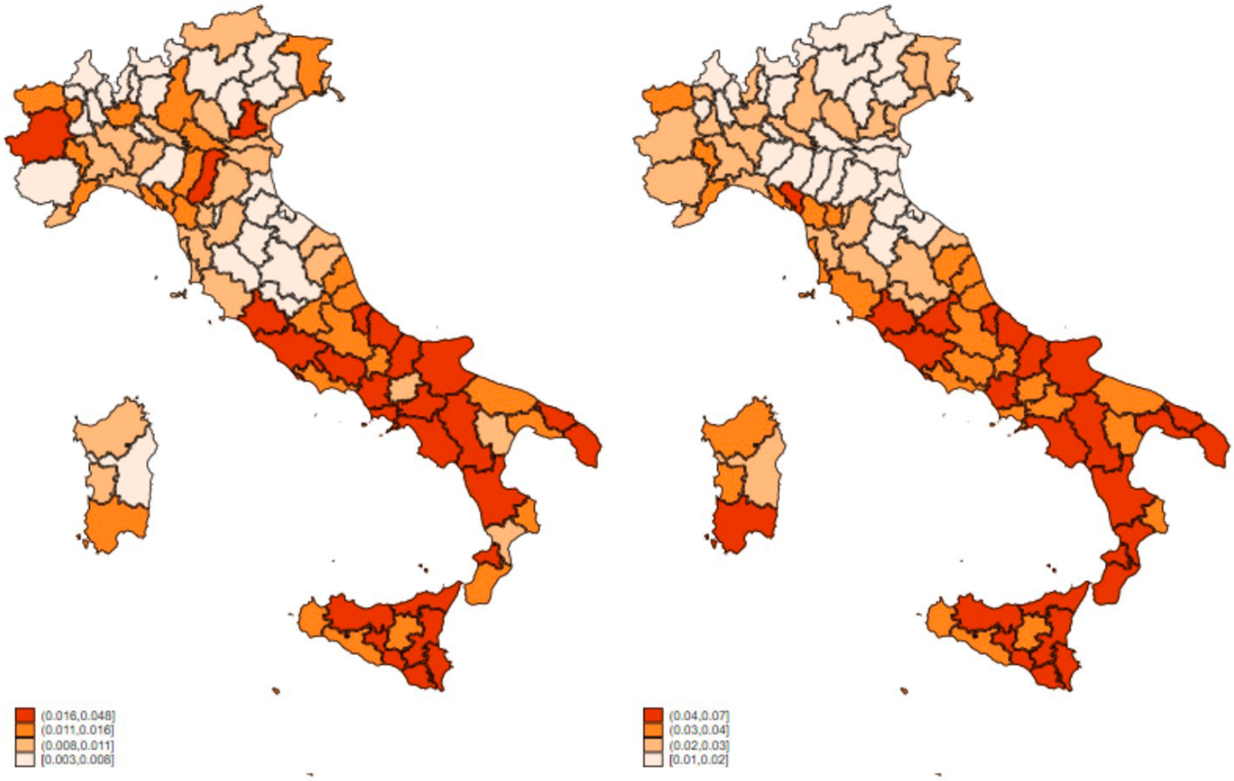
Note: Wage floors across job titles (*livelli di inquadramento*) for the standard (white bars) and pirate CBA (black markers) in the wholesale and retail sector in 2018. For the pirate CBA, the top marker is the wage floor for Lombardy (the region with the largest *Equalization element*) and the bottom marker is the wage floor for Molise (the region with the lowest *Equalization element*). See text for details.

As expected, the wage floors set in the pirate CBA are lower than those in the representative CBA, especially for occupations at the lower end of the wage distribution. Importantly, the advantages for firms when applying the pirate CBA extend to other aspects of the employment relationship, such as maternity leave. Law Decree n. 151/2001 imposes minimum maternity leave of five months (two before childbirth, three after) remunerated at 80 percent of pay. While the representative CBA allows for longer maternity leave (up to five months after childbirth, at the mother's discretion) and envisages 100 percent remuneration, the pirate CBA does not extend the provisions set by the Law, "at least during the crisis".

B. Appendix B – Appendix Figures & Tables

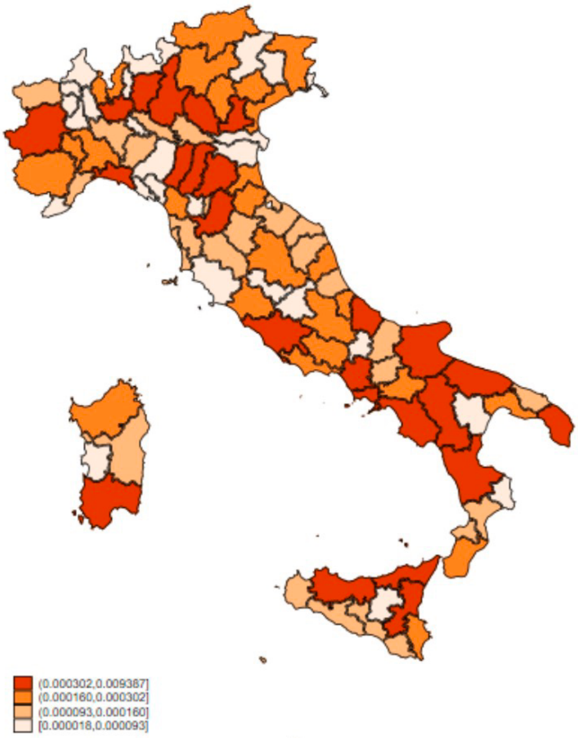
Appendix Figure B1. Geographical Distribution of Pirate CBAs in 2019

(a) Workers (b) Firms



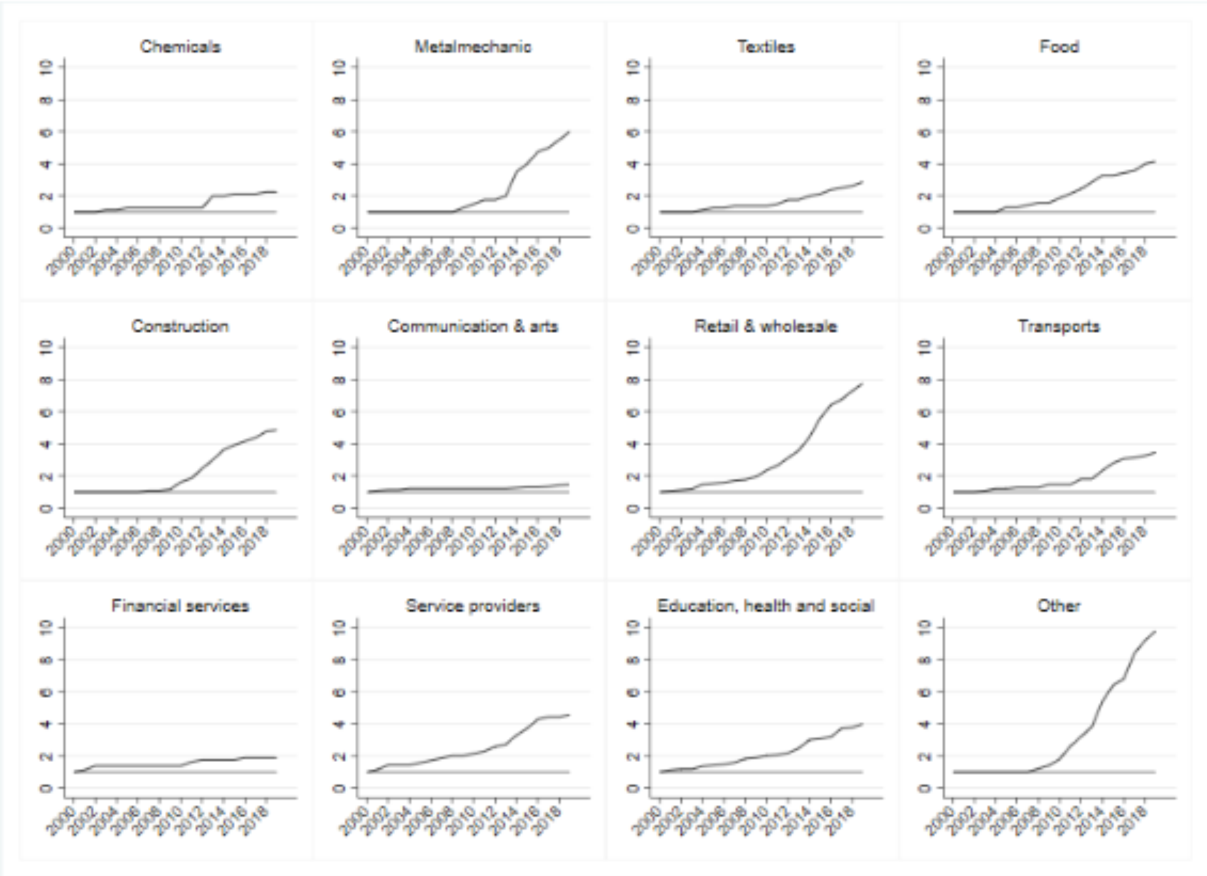
Note: For workers, the share of pirate CBAs is computed as the total number of workers covered by a pirate CBA as a fraction of the total number of workers in the INPS data in each province in 2019. For firms, the share is computed as the number of firms applying a pirate CBA to at least one employee as a share of the total number of firms in the INPS data in each province in 2019.

Appendix Figure B2. Geographical Distribution of Pirate CBAs Across Firms in 2019, Employment Weights



Note: The share is computed as the number of firms applying a pirate CBA to at least one employee as a share of the total number of firms in the INPS data in each province in 2019. Each firm is weighted by total number of workers in 2019.

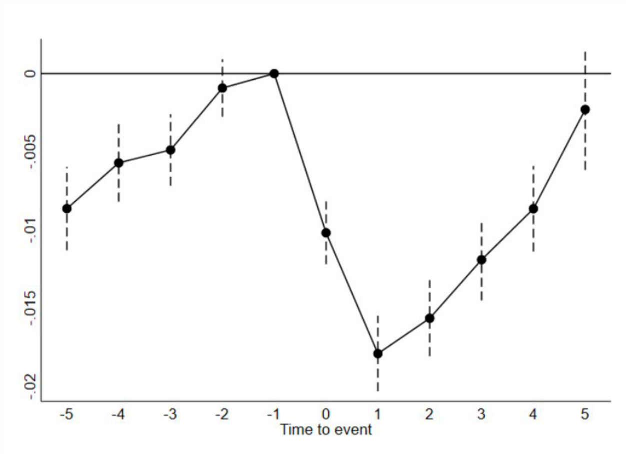
Appendix Figure B3. Growth in pirate CCNLs by sector



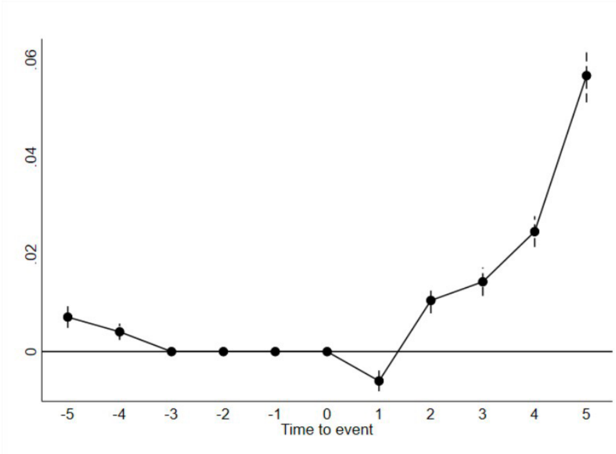
Pirate contracts by sector, 2000=1. Pirate agreements defined as those not signed by at least one union in the union triad.

Appendix Figure B4. Pirate CBAs Design: Event Study Coefficients - Workers - Full Worker Matched Sample

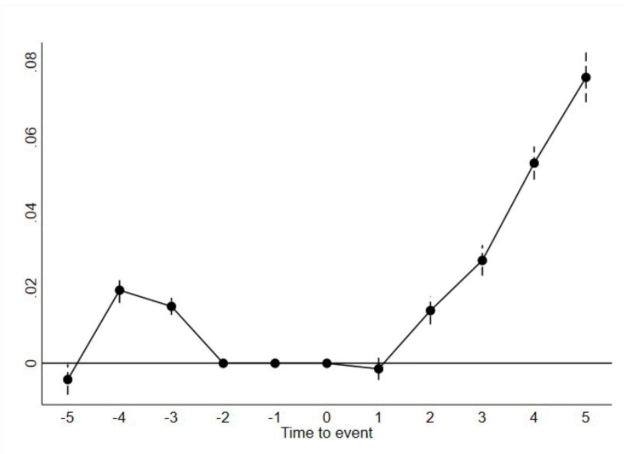
(a) Log weekly wages



(b) Employment probability



(c) Probability of remaining at the firm



Note: Event-study coefficients resulting from the estimation of Equation (2) on the full matched sample of workers, including exceptionally large firms in 2012 and 2015.

Appendix Table B1. Pirate CBAs: Distribution Across Sectors

	Treated firms	Other firms
Agriculture, Forestry and Fishing	4.62	0.90
Mining and Quarrying	0.02	0.14
Manufacturing	3.32	17.63
Electricity, Gas, Steam and Air Conditioning Supply	0.11	0.08
Water Supply Sewerage, Waste Management	0.23	0.32
Construction	1.83	13.02
Wholesale and Retail Trade	32.11	22.33
Transportation and Storage	2.47	3.35
Accommodation and Food	4.21	12.74
Information and Communication	16.41	1.77
Finance and Insurance	2.49	1.42
Real Estate Activities	3.61	1.15
Professional, Scientific and Technical Activities	6.58	6.57
Administrative and Support Service Activities	9.51	4.43
Public Administration and Defence	0.12	0.05
Education	1.54	1.06
Human Health and Social Work	2.93	4.26
Arts, Entertainment and Recreation	1.18	0.85
Other Service Activities	6.33	6.00
Activities of Households as Employers	0.17	1.91
Activities of Extraterritorial Organisations	0.19	0.02

Note: Treated firms defined as firms applying a pirate CBA to at least one employee. Shares computed within treatment group, years 2008-2019.

Appendix Table B2. Pirate CBAs: Retention of Pirate CBAs

Years since first use	Share of firms still using pirate CBA
0	1
1	0.83
2	0.74
3	0.67
4	0.63
5	0.59
6	0.57
7	0.55

Note: Sample includes firms using pirate CBA for the first time between 2008 and 2012 and surviving for at least the following seven years.

Appendix Table B3. Pirate CBAs: Balancing Table for Firms, Year Before the Opt-Out

	Control firms	Treated firms	p-value
Log weekly wage	5.93	5.92	0.33
	(0.31)	(0.32)	
Firm size 1st dec.	0.06	0.06	0.41
	(0.24)	(0.23)	
Firm size 2nd dec.	0.07	0.07	0.56
	(0.25)	(0.26)	
Firm size 3rd dec.	0.08	0.07	0.24
	(0.27)	(0.26)	
Firm size 4th dec.	0.06	0.06	0.85
	(0.23)	(0.23)	
Firm size 5th dec.	0.04	0.05	0.25
	(0.20)	(0.21)	
Firm size 6th dec.	0.06	0.04	0.03
	(0.24)	(0.20)	
Firm size 7th dec.	0.11	0.12	0.22
	(0.31)	(0.32)	
Firm size 8th dec.	0.07	0.07	0.64
	(0.26)	(0.25)	
Firm size 9th dec.	0.15	0.14	0.58
	(0.36)	(0.35)	
Firm size 10th dec.	0.30	0.32	0.22
	(0.46)	(0.47)	
Short-term solvency	0.70	0.70	0.79
	(0.46)	(0.46)	
Mean worker age	38.44	38.63	0.29
	(5.92)	(5.64)	
Share of full-time	0.66	0.66	0.65
	(0.35)	(0.34)	
Share of temporary	0.19	0.18	0.18
	(0.26)	(0.24)	
Share of women	0.49	0.49	0.69
	(0.33)	(0.33)	
Share of blue collars	0.49	0.49	0.75
	(0.39)	(0.40)	
Share of white collars	0.44	0.44	0.85
	(0.38)	(0.39)	
Log Assets	6.88	6.96	0.10
	(1.60)	(1.78)	
Number of firms	2249	2249	

Note: Balancing table for treated and control firms in the matched sample. All variables refer to the year prior to the opt-out event. The third column shows the p-value of the corresponding t-test for the equality of means. Standard deviations in parentheses.

Appendix Table B4. Pirate CBAs: Balancing Table for Workers, Year Before the Opt-Out

	Control workers	Treated workers	p-value
Full matched sample			
Log weekly wage	6.16	6.17	0.00
	(0.39)	(0.38)	
Gender	0.53	0.52	0.00
	(0.50)	(0.50)	
Age	41.17	41.45	0.00
	(8.88)	(8.80)	
Full time	0.69	0.68	0.03
	(0.46)	(0.46)	
Temporary	0.02	0.02	0.57
	(0.15)	(0.15)	
Tenure	6.73	6.80	0.00
	(2.75)	(2.74)	
Firm size	3232	3092	0.00
	(4295)	(3298)	
Blue collar	0.29	0.32	0.00
	(0.45)	(0.47)	
White collar	0.66	0.63	0.00
	(0.47)	(0.48)	
Number of workers	90,223	90,223	
Excl. large firms in 2012 and 2015			
Log weekly wage	5.98	5.99	0.48
	(0.41)	(0.40)	
Gender	0.53	0.54	0.09
	(0.50)	(0.50)	
Age	41.43	41.52	0.36
	(9.58)	(9.42)	
Full time	0.66	0.66	0.40
	(0.47)	(0.47)	
Temporary	0.06	0.06	0.17
	(0.24)	(0.23)	
Tenure	5.13	5.13	0.95
	(2.51)	(2.49)	
Firm size	178.1	147.9	0.00
	(1182)	(566.4)	
Blue collar	0.50	0.49	0.00
	(0.50)	(0.50)	
White collar	0.48	0.50	0.00
	(0.50)	(0.50)	
Number of workers	21,682	21,682	

Note: Balancing table for treated and control workers in the matched sample. The bottom panel excludes treated workers employed in firms in the 100^{th} percentile of firm size in 2012 and 2015, along with their controls. All variables refer to the year prior to the opt-out event. The third column shows the p-value of the corresponding t-test for the equality of means. Standard deviations in parentheses.