Opting Out of Centralized Collective Bargaining: Evidence from Italy

Christian Dustmann Chiara Giannetto Lorenzo Incoronato Chiara Lacava
Vincenzo Pezone Raffaele Saggio Benjamin Schoefer

March 2025

Abstract

We estimate the effects of employers opting out of heavily centralized collective bargaining agreements on workers' wages and employment. Our analysis studies two emerging cracks in the rigid system in Italy, where all employers are mandated to pay occupation and industry-specific wage floors specified in national agreements. First, we study the secession of large retailers from their employer association in 2011. Second, we examine the emerging loophole allowing individual employers to enter into lower-wage "pirate agreements." By merging contract data with matched employer-employee data, we implement a harmonized matched event-study design for both events. Our results show that workers whose employer opts out experience a 2% wage loss—and a 3% higher employment probability.

Affiliations: Dustmann: University College London, Giannetto: University College London, Incoronato: University of Naples Federico II and CSEF, Lacava: University of Naples Federico II, Pezone: Tilburg University, Saggio: University of British Columbia, and Schoefer: University of California, Berkeley. We thank the Italian Social Security Institute (INPS) for providing access to the data through the VisitINPS Scholars program. We thank our discussant Salvatore Lattanzio, and audiences at the NBER Labor Studies meeting, the 6th Bank of Italy-CEPR Workshop on Labour Market Policies and Institutions, the 18th CSEF-IGIER Symposium on Economics and Institutions, CReAM-RFBerlin Workshops, VisitINPS seminars, VisitINPS VI Conference, UC Berkeley, University of Southern California, University of Siena, LMU Munich, University of Barcelona, Universitat Autonoma de Barcelona, VATT Helsinki, University of Konstanz, ifo Institute Fürth and IWH-Halle. We also thank Gabriele Rovigatti and coauthors for sharing their data on shopping hours liberalization. The views expressed in this article are those of the authors and do not necessarily correspond to those of INPS.

1 Introduction

A key policy concern in many European countries are the trade-offs arising from centralized, high-coverage collective bargaining regimes (Visser, 2013). While these institutions establish minimum labor standards for all workers and firms, many observers point to the distortions from their rigidity. For instance, the rigid standards may prevent firms from optimally adjusting in response to negative shocks, and may depress employment in unproductive firms or areas. To balance those trade-offs, the OECD has proposed "coordinated decentralization" as the most suitable institutional set-up (OECD, 2019). In such a regime, centralized (e.g., national sector-level) agreements define a broad negotiation framework, but individual firms can opt out to negotiate wages and other labor provisions more directly with their respective workforces. Such decentralization would raise competitiveness and employment stability, but may entail wage reductions—the "competitiveness channel." This channel is also a crucial ingredient in models of optimal decentralization of bargaining regimes, which posit a hump-shaped relationship between bargaining centralization and unemployment (Calmfors and Driffill, 1988; Jimeno and Thomas, 2013). However, there is little micro-empirical evidence on this channel, and on the benefits and costs of such decentralization to firms and workers more generally. The key challenges are twofold: first, to precisely identify in microdata the firms and workers subject to opt-outs, and, second, to obtain suitable quasi-experiments in opt-out events.

This paper examines the recent decentralization of industrial relations in Italy. Italy is an ideal laboratory, as its economy features rigid collective bargaining institutions that are heavily centralized and impose national wage floor schedules for each sector that frequently bind, particularly in low-productivity firms and regions (see, e.g., Boeri, Ichino, Moretti and Posch, 2021). Italy's persistent challenges following the Great Recession have increased pressure on labor market institutions (Boeri and Garibaldi, 2019). On the one hand, this pressure has motivated calls to reform the rigid system. On the other hand, employers have increasingly attempted to evade rigid regulations and gain flexibility within the existing system. Our paper focuses on these emerging attempts of firms to opt out of centralized collective bargaining.

To study how firm opt-outs affect worker outcomes, we combine information on collective bargaining agreements (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 Italian matched employer-employee data is that it permits us to observe the collective bargaining agreement applied to a particular job—and hence precisely identify opt-out events of firms from their national CBA.

First, we study a prominent secession of a group of 56 large employers operating in the mass-retail sector (such as Ikea, Zara, Carrefour). In 2011, these employers abandoned their original employer organization—which represented *all* employers in retail and whose provisions on various margins, such as opening hours of business, tended to favor small businesses. This opt-out, in the form of a secession, permitted them to bargain separately with unions. In our data, we can identify precisely the firms (and therefore workers) that are part of the retail opt-out.

Second, the setting and data allow us to use opt-out variation in the form of idiosyncratic firm-level events: individual employers evading their original CBAs into "pirate agreements" that offer flexible working conditions and lower wage floors. These opt-out events stem from a regulatory loophole that gained traction during the recession of the early 2010s, and by 2019 constituted around two-thirds of the total number of collective contracts covering half a million workers (3% of total private-sector employment). Our matched contract-firm-worker data lets us directly observe such transitions from a standard to a pirate CBA at the worker level.

To study the effects of these opt-out events, we employ matched difference-in-differences designs to compare workers (and firms) subject to the opt-out to a suitable control group. To mitigate the primary remaining identification concern, namely the endogeneity from dynamic selection, we assign each treated observation to a similar control peer employed in a control firm based on pre-opt-out characteristics, checking for parallel trends before the event. Moreover, our comparison of results from two separate sources of variation in opt-out (and distinct potential dynamic selection) fosters the causal interpretation. Finally, dynamic selection is likely less pronounced for our worker-level analysis (as in, e.g., Goldschmidt and Schmieder, 2017), where we track the cohort of workers employed at the firm right before the opt-out event. Intuitively, it is unlikely that workers are able to select employers based on the likelihood that they will engage, in the future, in an opt-out decision. To further reduce this possibility, we impose a three-year tenure restriction, so that all the workers included in our final sample are hired long before the opt-out.

We find evidence broadly consistent with the competitiveness channel of opt-outs, both for the retail secession and for the pirate agreements. Our main results are that opt-outs significantly lower workers' wages—implying that firms used the increased flexibility to lower wages. At the same time, we see an increase in employment stability—i.e., workers subject to an opt-out are more likely to stay employed at the original firm going forward. On net, the positive employment effect balances out the negative wage effects, such that earnings remain unchanged. Quantitatively, in the long run wages fall by 2-3% for either opt-out event, while the probability of staying employed (with any employer) increases by 3-4 percentage points, driven by higher retention with the original employer. The earnings effects are statistically insignificant, with point estimates ranging between about +195 to 0 euros per year, respectively.[1]

Last, we investigate how our results differ between the Center-North and the South of Italy, where national CBA wage floors are particularly restrictive due to lower average productivity (Boeri et al., 2021). Across both opt-out events, we observe that the positive employment effects are predominantly concentrated among incumbent workers employed by firms in the Center-North. In contrast, the overall employment probability for Southern workers does not appear to increase significantly, particularly over longer time horizons. We speculate that this may be due to Southern firms facing a relatively inelastic labor supply curve. For these firms, opting out allows to set wages

[1]In additional investigations, we also compute firm-level effects of opting out with the caveat that dynamic selection could be a concern even after matching. For the pirate agreements, we find an about 3% reduction in labor costs, but positive effects on firm survival, of about 4 percentage points in the 1-2 years immediately after the opt-out, which however declines towards zero afterwards.

at a level that could lead to lower employment compared to what observed when using the wage floors mandated by national CBAs.

Our paper adds to a nascent literature on the effects of opting out of centralized collective bargaining, which has used either much smaller datasets and survey data (rather than administrative population-level data) and has focused so far primarily on wages (whereas we study the trade-off between wage flexibility and employment gains simultaneously). Our paper is mostly closely related to Lucifora and Vigani (2021), Dahl, le Maire and Munch (2013) and Gürtzgen (2016). In the context of Italy, Lucifora and Vigani (2021) also examines the rise of pirate agreements in Italy, which is one of our two sources of variation, and finds substantial wage penalties. Our paper extends Lucifora and Vigani (2021) in several dimensions. First, we study the effects of pirate agreements on a richer set of outcomes, first of all, employment probability. Second, we leverage the universe of Italian private-sector workers (rather than a 1/90 sample) and hence study the universe of pirate agreements in Italy. Third, our paper draws on two sources of variation in opt-outs; besides pirate agreements, we study the retailer opt-out event, and employ a harmonized research design.

In the context of Denmark, Dahl, le Maire and Munch (2013) study gradual decentralization using longitudinal data and a fuzzy approach based on occupation and sector codes to identify job-level bargaining regime shifts. In the context of Germany, Gürtzgen (2016) studies the effects of manufacturing and mining firms leaving industry-level agreements, however drawing on survey data to infer the opt-out. This paper extends the analysis in Dahl, le Maire and Munch (2013) and Gürtzgen (2016) by identifying opting-out events from administrative data and by drawing on sharper variation. Moreover, our setting, Italy, features a significantly more rigid (at baseline) centralized bargaining system compared to Denmark or Germany (see, e.g., Dustmann, Fitzenberger, Schönberg and Spitz-Oener, 2014; Jäger, Noy and Schoefer, 2022). Finally, our analysis covers a period of remarkable difficulties for European labor markets—a period perhaps particularly relevant for the debate that motivates our study—whereas both Dahl, le Maire and Munch (2013) and Gürtzgen (2016) study a period of stability (1992-2001 and 1999-2007, respectively).

2 Institutional Background

Industrial relations in Italy are based on national sector-level collective bargaining agreements (*Contratti Collettivi Nazionali del Lavoro*, henceforth "CBAs"). CBAs serve as the primary source of regulation, through which trade unions representing workers and employers' associations jointly establish the rules governing the employment relationship.[2] Typically, CBAs are signed by associations that are considered representative of their respective sectors. Although there are no specific criteria outlined by law for representativeness, historically, CBAs have been signed by the three largest workers' unions, and the term "representative" generally refers to CBAs endorsed by these

[2]The key role of CBAs is established by the Italian Constitution (Article 39): *Registered trade unions (...) may, through a unified representation that is proportional to their membership, enter into collective labor agreements that have a mandatory effect for all persons belonging to the categories referred to in the agreement.*

unions (henceforth referred to as *traditional* unions).[3] Firms could adopt any CBA available in their sector, even if not among the signatory parties. Importantly, a sector's representative CBA is effectively extended *erga omnes*, i.e., it also applies to workers who are not members of the unions that signed the CBA as well as to employers not part of the signatory employer association. As a result, CBA coverage is universal, putting Italy at the very top of the OECD countries when it comes to collective bargaining coverage (ICTWSS database, OECD, 2024).

Besides establishing broad employment conditions (e.g., vacations and working hours), CBAs set a *schedule* of wage floors for different (typically eight) job titles (*livelli di inquadramento*), which roughly correspond to different occupations. CBAs typically last three years. Wage floors are periodically adjusted following a predetermined schedule to account for expected inflation. At expiration, CBAs should be immediately renewed. In the frequent case of delays, the expired CBA remains in force until replaced.

On top of CBAs' provisions, employers and firm-level union delegations can negotiate firm-level agreements (for a summary, see Boeri, 2014, and Dell'Aringa, 2017). However, this second tier of bargaining is subordinated to sector-level bargaining. Firm-level agreements can only regulate matters when explicitly allowed to do so by the CBA ("non-repeatability" clause) and can deviate only to set better conditions for the worker ("favorability" clause). For instance, firm-level agreements can establish "top-ups", i.e., premiums paid by the firm above the wage floor set by the CBA (Guiso et al., 2005).

The actual wage received by workers is therefore given by the CBA wage floor plus a potential top-up if there is a firm-level agreement in place. However, only around 20% of firms larger than 20 employees have negotiated directly with workers between 2010 and 2016 (D'Amuri and Nizzi, 2017). As a result, firm-level agreements tend to have very limited scope in Italy, especially because these agreements do not permit firms to set wages *below* the CBA wage floors.[4] These constraints have contributed to the opt-out events that are the focus of our paper and described in Section 3 below.

3 The Opt-Out Events

We now describe the two facets of opt-out events that have allowed Italian firms to abandon their CBA: a coordinated departure of large retailers from their employer association (Section 3.1), and individual employers exploiting an emerging loophole to switch into lower-wage "pirate agreements" (Section 3.2).

[3]These unions are Confederazione Generale Italiana del Lavoro (*CGIL*), Confederazione Italiana Sindacati Lavoratori (*CISL*), and Unione Italiana del Lavoro (*UIL*), which have long dominated the Italian industrial relations and have branches representing workers in each sector of the Italian economy.

[4]Consistent with this, prior studies find large pass-through of changes to CBA wage floors onto observed wages (Fanfani, 2023; Faia and Pezone, 2023).

3.1 The 2011 Secession of Mass Retailers

Background The first opt-out event is the 2011 secession of a group of large retailers—*Federdistribuzione* (FD)—from their employer association, *Confcommercio* (CC), to negotiate a separate CBA. CC comprised 90 employer sub-associations (e.g., of butchers, hotels, stationers,...).[5] FD was the sub-association for mass retailers, such as large hypermarkets (e.g., Carrefour), clothing (e.g., Coin), furniture (e.g., Ikea) and home improvement and gardening (e.g., Leroy Merlin). Several companies are the Italian branches of foreign multinational corporations. As of 2010, there were 56 firms in FD, representing around 6% of retail employment (authors' calculation based on the matched employer-employee data described below).

On December 23, 2011, FD announced the exit of its members from CC, in order to lobby independently and negotiate a separate FD-CBA. The stated reason was a strategic disagreement between the large FD employers and the small retailers, which dominated CC and opposed FD's push for liberalization of opening hours and large shopping malls.

Implications for Flexibility and Wage Setting While not the primary motivation, the opt-out had important consequences for wage floors in FD firms vis-à-vis CC firms. Figure 1 plots the evolution of wage floors between 2005 and 2020 for those groups, for three job titles: title "7," the lowest rank aside from apprentices, title "4," the modal job title, and for mid-managers ("*quadri*," i.e. the highest job title).

Before the opt-out in 2011, the wage lines align perfectly as FD was part of CC and hence subject to CC wage floors. FD would also be bound by the three-year CBA that had gone into force in January 2011, expected to last through 2013. However, in 2014, wages did not diverge because of delays in the bargaining process between CC and the unions. They finally agreed on a new CC-CBA on March 30, 2015, covering the period of April 2015 to March 2018.

Hence, wage floors in FD firms remained frozen at the 2013 levels and diverged substantially starting in 2015, with the gap's 2018 peak being around 4.8% (homogeneous across job titles).

Figure 1 also reports (green dashed lines) the wages FD paid. Differences between the two lines indicate the degree to which FD firms (i) were able to deviate from the CC-CBA wage floors and (ii) decided to take advantage of the former. Hence, they are equilibrium outcomes. A continuation of the 2013 wages would denote the full force of the freedom brought by the opt-out, as FD firms could have let wages be frozen at 2013 levels.

The actual FD wages reflect the wage policies FD voluntarily and *unilaterally* self-imposed from 2015 to 2018. This is because during that period, unions rejected FD's proposal to merely replicate CC-CBA wage floor increases—whereas unions believed the large FD firms should provide larger real wage increases than those designed for small and struggling CC retailers. As part of this conflict, the unions implemented three short strikes between November 2015 and May 2016. FD firms

[5]The CC-CBA was first signed in 1967 by the dominant associations Confcommercio (employer) and CGIL, CISL and UIL (the three traditional unions). This contract had then been renewed periodically and became the "representative" CBA in the sector.

also engaged in hostile industrial relations actions, e.g., they cut the generosity of supplementary health insurance and abolishing lower-level work councils (*Enti Bilaterali Territoriali*). To partially accommodate unions' demands, FD firms implemented unilateral raises that took effect on May 2016 and June 2017. We view all of the aforementioned events as *outcomes* of the opt-out, previewed here to complement our econometric analysis on how this opt-out event impacted actual wages and employment outcomes.

This period of divergence in wage floors—and of adversarial industrial relations—lasted until January 2019. Then, FD and the three traditional unions signed a new CBA on December 19, 2018. While formally a separate agreement, this contract replicated the wage floors of the prevailing CC-CBA (as well as most other provisions), and hence the two lines in Figure 1 reconverge. While FD and CC continue to bargain separately, the two associations' 2022 CBAs remain close on all dimensions, including wage floors.

3.2 Adoptions of Pirate Agreements

Background The second facet of opting out is the emerging adoption of so-called "pirate" agreements.[6] Here, an individual firm opts out from its assigned CBA (typically subscribed by the three traditional unions) by signing a new CBA with minor and less representative unions. Pirate CBAs often entail lower wages and more flexible working conditions.

Pirate agreements exploit a loophole in Italian labor laws. In principle, an Italian firm must adopt the wage floors established by the representative CBA of their sector of activity. However, as noted in Section 2, the law does not clearly define this concept of a "representative CBA" for a given firm or sector—leaving legal room for employers to try and abandon the main CBA and adopt another CBA signed by "pirate" unions rather than the three main unions.[7] This loophole has been increasingly exploited, especially after the Great Recession, as incentives to cut costs and raise labor flexibility became more pressing for several firms.

Implications for Flexibility and Wage Setting The implications of pirate agreements for wage setting are complex in that they depend on the original wage floors the firm faced, the menu of applicable pirate agreements of a given firm, and the ultimate choice of the employer from that menu. In principle, the firm's workforce will be switched from the old to the new wage floors, making it possible to *lower* wages rather than merely freezing the old terms and avoiding scheduled increases as instead occurred in the FD-event described in Section 3.1.

[6]This term evokes the disruptive effects on the traditional system of industrial relations, as in the following quote: "Although the pirates we are dealing with today are not equipped with sabers or muskets, but, probably, with pens, smartphones and tablets, the metaphor seems to hit the mark: the proliferation of "pirates" in the industrial relations system - or in inter-union system, [...] is a symptom of the insufficient organization of this system and its (in)ability to repel 'incursions' and 'hostile acts.'" (Centamore, 2018, authors' translation).

[7]Such unions were either existing, minor unions that competed with the three main ones, or newborn unions created after 2010. In practice, firms did not renew their existing CBA after expiration and instead signed a new, pirate contract. There have been cases (subsequently condemned by labor courts) in which firms did so before expiration. Firms that were not among the signatory parties of a CBA in the first place, but that were just applying the representative CBA for their sector, could begin adopting a new CBA immediately.

Figure 2 presents a case study of a pirate agreement, covering wages and other job aspects—in fact for the wholesale and retail sector, where the dominant contract was the CC-CBA (see above) and where both contracts had comparable occupational groups.[8] Except for the highest wage group, the pirate agreement specified lower wage floors, in particular at the low end of the distribution. Moreover, the agreement allowed for regional differentiation in pay—with the signatory parties acknowledging the issue, posited in Boeri, Ichino, Moretti and Posch (2021), that nominal wages should be adjusted to better reflect productivity levels across the country. Hence, the figure presents two wage floors for each title, representing the wage floor in the region with the highest and lowest wage level (Lombardia, in the North, and Molise, in the South). This geographic adjustment amounts to roughly 5.3% of the (national) wage floor in Lombardia. In Molise, it is 0.4% for mid-managers, and up to 0.9% for the lowest occupational group. Moreover, non-wage aspects of jobs are also less generous under the pirate agreement, such as maternity leave.

Time Series and Geographic Distribution Drawing on data we describe below in Section 4, we discuss some basic descriptive statistics regarding the trends of pirate agreements and the workers and firms subject to them. Figure 3 illustrates the growth of pirate agreements following the Great Recession. Panel (a) shows the number of pirate agreements signed each year; Panel (b) displays the share of firms and workers covered by pirate agreements. Despite being the majority of CBAs in Italy, pirate contracts still cover only about 3% of private sector workers and firms in 2019, roughly half a million workers and about 40,000 firms. This gap between contract counts and coverage reflects the fact that standard CBAs by their nature cover most firms, and pirate agreements are usually tailored contracts and signed by small unions.

Appendix Figure A1 depicts the geographic heterogeneity: pirate contracts are more used in the Center-South—consistent with the findings of Boeri, Ichino, Moretti and Posch (2021), who note that wage floors are particularly binding in less productive regions. Out of all workers covered by pirate contracts, roughly 33% are in Southern regions compared to 24% for non-pirate workers. For firms, these shares are 15% and 8%, respectively (see summary statistics tables described below).

Descriptives: Workers and Firms Appendix Table A1 compares workers covered by pirate agreements to other workers. It reveals that workers subject to pirate agreements have a higher

[8] In 2012, smaller employer and employee associations (Confazienda, Fedimpresa, Unica and Cisal) signed a new CBA in the sector, which by 2018 covered 731 firms and 12,000 workers (the 0.2% and 0.5% of the total number of firms and workers covered by the CC-CBA, respectively). An excerpt of the contract sheds light on the stated motivations: "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."

share of women and part-time workers, and lower average wages. Appendix Table A2 provides information on the industry distribution, with an over-representation in services, and under-representation in manufacturing. Last, Appendix Table A3 reports descriptives for firms, and shows that firms using pirate CBAs tend to be larger and younger and employ more women and part-time workers.

4 Data

We now describe the three linked datasets on which we base our study: matched employer-employee data, firms' financial balance sheet, and CBA contract data. We then provide details on how we identify the opt-out events described in the previous section in these datasets.

Matched Employer-Employee Data We draw on data on the universe of worker-firm matches in the private sector between 2005 and 2019, on earnings, weeks worked, type of contract (part- versus full-time, temporary versus permanent), occupation (apprenticeship, blue-collar, white-collar, mid-management, manager), as well as demographic information such as date of birth and gender.[9] The data ("INPS data") are collected by the Italian Social Security Institute (INPS), which we access via the VisitINPS program.

Crucially, unlike in most other settings, this dataset in Italy contains, for each job spell, the CBA that the firm applies, with a unique code that we describe next. This peculiarity, that firms have to report applicable CBAs, is owed exactly to the coverage rules in Italy, which require a firm to pay the worker according to the (representative) CBA.

CBA Contract Data All CBAs are recorded in the web archive of CNEL (National Council for Labor and Economic Policies), along with details including their signatory parties.[10] We merge the CNEL information on contract data with the INPS administrative data by drawing on the INPS-CNEL crosswalk provided by CNEL (also used in Daruich, Di Addario and Saggio, 2023; Faia and Pezone, 2023).

Firm Financials We merge on a dataset that provides key accounting variables for all nonfinancial incorporated firms between 2005 and 2018. These data are collected by the Cerved Group and can be merged to the INPS data using a unique national tax firm identifier (*codice fiscale*).

Identifying FD Firms We obtained from FD the names of the firms that opted out from the CC-CBA in 2011.[11] Using these company names, we manually match this list with the firm-level

[9]We exclude all workers employed as managers because managers are subject to a different collective agreement legislation.

[10]The archive can be accessed at this link: `https://www.cnel.it/Archivio-Contratti`.

[11]The brands of the firms that were associated to FD in 2010 are: A&O, Assofranchising, Auchan, Bennet, Bricocenter, Bricoman, C&A, 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,

balance sheet data to retrieve their national tax identifiers. Out of the 56 firms in FD that opt-out from the CC-CBA, we were able to match 52 firms in the INPS data.

Identifying Pirate Agreements Following Lucifora and Vigani (2021), we classify as pirate agreements those CBAs in the INPS database that are labeled as "Different Contract." By inspecting the CNEL records, we confirm that virtually all these CBAs are signed by non-traditional unions.[12] We also define as pirate agreements those CBAs with a unique identifier in the INPS data (i.e., they are not recorded as "Different Contract") but not signed by at least one of the three main unions in Italy according to the CNEL archive.[13]

Main Outcome Variables The analysis will focus on four main worker-level outcomes: i) weekly wage, computed as the annual wage net of social security contributions of a worker at their (dominant) employer, scaled by the number of weeks worked at that employer in the year;[14] ii) employment, defined as an indicator variable taking value of one if a worker has at least one day of employment at any firm in a year and zero otherwise; iii) employment at the original (i.e., opting-out) firm, defined as an indicator variable taking value of one if a worker is employed at the original firm and zero otherwise; and iv) annual earnings, calculated as the sum of labor earnings obtained by the worker from all employers in a given year (the outcome is equal to zero for non-employed workers).

5 Econometric Framework

This section presents the harmonized matched difference-in-differences research design used to analyze the effects of the FD opt-out event (Section 5.1) and of the adoption of pirate agreements (Section 5.2). We then discuss the identification assumptions in Section 5.3.

5.1 The 2011 Secession of Mass Retailers

Specification We compare a worker employed in a FD firm in 2010—the year before the opt-out event of FD employers occurred— with a similar worker employed by a firm that applies the

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.

[12]The CNEL data show that there is a very low number of "Different Contracts" signed by the three main unions (around 30), i.e., a tiny fraction of contracts. These cases cannot be identified in the INPS data (and so we cannot drop them or gauge affected worker observation counts).

[13]In the latter case, we exclude those few sectors or occupations where the traditional unions have not historically signed a national CBAs because it was always bargained by specialized minor unions. To give an example, managers in Italy have their own CBA. These manager-CBAs, however, are never signed by the three traditional unions as they do not represent managers. Therefore, manager CBAs are not part of our definition of pirate agreements.

[14]This outcome is missing for non-employed workers. If a worker holds multiple jobs in a year, we pick their dominant employer as the one where the worker has the highest number of weeks worked in that year.

CC-CBA in 2010 and that has not opted-out from this CBA (henceforth a CC firm). We thus fit regression equations of the following type:

$$y_{it} = \alpha_i + \delta_t + \sum_{t \neq 2010} \beta_t \cdot FD_i + v_{it}, \tag{1}$$

where y_{it} represents an outcome of individual i in year t (e.g., total earnings); α_i and δ_t are worker and year fixed effects; FD_i is an indicator for whether an FD firm is worker i's dominant employer in 2010 (i.e., having employed i for the most weeks that year). β_t are therefore difference-in-differences (DiD) regression coefficients, normalized relative to 2010, i.e., the year before the opt-out decision of FD firms. We cluster standard errors at the level of the 2010 employer.

Sample and Matching The analysis sample are the set of workers who are employed by either an FD firm or a CC firm in 2010 and who are similar along various observables up to the opt-out event. To construct this sample, we fit a propensity score for the probability of being employed by an FD firm in 2010 (similar to what is done in Goldschmidt and Schmieder, 2017; Jäger and Heining, 2022). We restrict the sample to workers with at least four years of labor market experience and three years of tenure with their 2010 dominant employer, as typical in the job displacement literature (e.g., Bertheau, Acabbi, Barceló, Gulyas, Lombardi and Saggio, 2023; Schmieder, Von Wachter and Heining, 2023). We also restrict the analysis to firms with at least 15 employees in 2010.[15] The propensity score comprises as covariates region fixed effects and age, tenure, gender, contract type (temporary versus permanent, full- versus part-time), broad occupation dummies and the log weekly wage in 2010, 2009 and 2008. To each FD worker, we assign a single control worker, using the CC-CBA worker with the closest propensity score (without replacement).

Descriptives Table 1 presents summary statistics, before and after matching, by sample. Columns (1) and (2) present the average characteristics of workers in the analysis sample (see above) in FD firms (Column (1)) and CC firms (Column (2)), *before matching*. FD firms are much larger than CC firms. This reflects the context of the opt-out decision: FD firms are large retailers that want to escape the CBA designed for small and medium-sized firms.[16] FD firms are also more likely to use part-time contracts, which can, perhaps, also explain why we also see a higher fraction of women among FD firms. Interestingly, however, there are some margins where both FD and CC firms appear quite similar, such as wages, even before matching.

Columns (3) and (4) of Table 1 show the characteristics of the matched sample. Matched workers are about 90% of the original sample of FD firm workers and are very similar to the general population of FD employees.

[15]This restriction is imposed to reduce the imbalances in firm size that exists between FD and CC firms, see also Table 1. We chose 15 as it is the typical cutoff also in labor law (e.g., Kugler and Pica, 2008).

[16]Firm size is thus not included in the propensity score because doing so would lead to large violations of the overlap assumption.

5.2 Adoptions of Pirate Agreements

To estimate the impact of the adoption of pirate agreements, we implement a similar matched DiD research design, with two notable differences. First, since the event (i.e., the adoption of a pirate agreement) can occur in different years, we implement a staggered DiD specification.[17] Second, since firm size is more balanced, we can now include firm size in the matching procedure.

Specification The specification we use to analyze the effects of adopting a pirate CBA is a staggered difference-in-difference design that takes the following form:

$$y_{it} = \alpha_i + \delta_t + \sum_{k=a}^{b} \gamma_k \cdot \mathbb{1}[t = t_i^* + k] + \sum_{k=a}^{b} \beta_k \cdot \mathbb{1}[t = t_i^* + k] \cdot T_i + v_{it}, \tag{2}$$

where T_i is an indicator for a worker i experiencing a within-employer transition to a pirate CBA, and t_i^* denotes that event's year. For a control worker, this is the year of the adoption of the pirate agreement of the treated worker matched to this particular control worker (described below). For the few treated workers who experience a within-employer pirate CBA transition more than once over the 2005-2019 period, we refer to their first transition experienced. We let $t_i^* \in [2008, 2009, \ldots, 2016]$ in order to have sufficient periods before and after each event (i.e., $a = -5$ and $b = 5$ in Equation (2)) and normalize the coefficient β_{-1} to zero. Standard errors are clustered at the level of the $t_i^* - 1$ employer. Hence, intuitively, each event is treated as in the FD design, in a stacked regression.

Sample and Matching To obtain a comparable set of workers, we implement a matching algorithm analogously to the FD design. The pool of potential control workers is represented by workers who have the same CBA as treated workers prior to the transition to a pirate CBA (period $t_i^* - 1$) and were never employed at a firm that ever used a pirate CBA until 2019. We also exclude managers and apprentices as well as workers employed in agriculture and public administration (see Footnote 13). As above for the FD design, we focus on the sample of workers with at least four years of labor market experience and three years of tenure with their $t_i^* - 1$ dominant employer. We then implement the same propensity score model estimated in the analysis of the FD opt-out event but with the addition of firm size and sector dummies in the matching algorithm.[18] The first difference is explained above (and motivated by the large average size of FD firms), and the second difference is inconsequential (as all FD firms and CC control firms are in the same sector

[17] Specifically, the adoption of a pirate agreement event occurs in month m of a given year if a worker is employed with a representative CBA in months $m-2$ and $m-1$ of the same spell but is then moved to a pirate CBA in month m and is still covered by that pirate CBA in month $m+1$. We also drop the (rare) spells in which at least two transitions of the same nature (e.g., standard-to-pirate) occur within 12 months or those where the worker returns to be hired under their regular CBA within the same calendar year.

[18] Our set of transitions excludes the ones that occurred in the automotive sector in 2012 as they capture the opt-out of the large car manufacturer Fiat, which, being a single firm, VisitINPS privacy rules do not permit us to study. We additionally exclude those transitions to a CBA flagged as "Different Contract" occurred in the retail sector in 2015, as this code was also applied to the jobs applying the old "frozen" CC contract after the renewal of the CC-CBA.

by design). Differently from the study of the FD opt-out, we now also include firms with fewer than 15 employees, as there is large variation in the size of the treated firms.

Descriptives Similar to Table 1 for the FD design, Table 2 compares the baseline characteristics of treated and control workers, both before and after matching.

Columns (1) and (2) show descriptives before matching, drawing on years 2008-2016. Column (1) reports treated workers' characteristics as of period $t_i^* - 1$. Column (2) does so for other workers—those under a standard CBA that will not undergo treatment. Treated workers feature a larger share of women, part-time and white collars, are more likely to be based in the South and to earn less than other workers. Average firm size for treated workers is larger than for control ones—which also include workers employed in very small Italian firms.

Columns (3) and (4) report on the matched sample used to fit Equation (2). Out of the roughly 38,000 within-spell pirate agreement opt-outs between 2008 and 2016, we match around 25,000. As expected, matched treated and control workers are very similar on all dimensions, including average firm size.[19]

5.3 Discussion

The research designs focus on *incumbent* workers, i.e. workers who have a job and whose firm decided to conduct an opt-out event. In the context of the FD event, incumbent workers are represented by employees of FD firms in 2010. In the context of the pirate CBA opt-out event, incumbent workers are those whose employer decided to move them from a traditional CBA to a pirate CBA. This design follows a line of inquiry in labor economics that analyzes how a firm-level decision (e.g., a mass layoff as in Jacobson et al., 1993 or an outsourcing event as in Goldschmidt and Schmieder, 2017) impacts workers. We now provide details on the identifying assumptions needed for causal interpretation of our results and discuss advantages and limitations of this approach.

Identification A causal interpretation of the DiD coefficients $\{\beta_t\}$ and $\{\beta_k\}$ in Equations (1) and (2) relies on a parallel trend assumption. In the context of the FD event, this amounts to assuming that differences in outcomes between workers employed by a FD firm in 2010 and those employed under a CC-CBA would have remained constant in the counterfactual scenario where FD firms decided to remain with the CC-CBA. A similar assumption is needed to evaluate the effects of the adoption of pirate CBAs. To assess the plausibility of this assumption, we are going to inspect the evolution of outcomes in the years preceding the opt-out decision, with a particular focus on time periods not directly targeted by the propensity score. Moreover, while in the FD event study, we have provided a narrative contextualization and make concrete the associated endogeneity concerns, the large number of pirate agreements precludes such a detailed discussion.

[19]We match 2,497 treated workers in 2008, 879 in 2009, 1,567 in 2010, 2,115 in 2011, 4,638 in 2012, 4,882 in 2013, 2,603 in 2014, 3,454 in 2015 and 2,317 in 2016.

Workers vs. Firm Perspective Our empirical analysis shows how the decision of an employer to opt-out from its national CBA impacts the career trajectories of its employees. An advantage of focusing on workers is that we can track their outcomes even if these employees eventually leave their employer. However, focusing solely on incumbent workers might provide only a partial perspective, especially if opting out also induces large effects on the hiring margin of opting-out employers. Therefore, we also present firm-level effects of the adoption of pirate agreements based on a matched difference-in-differences research design that mimics Equation (2) but conducted at the firm level (see Appendix B for details).[20] This design requires stronger assumptions for its validity as we are now analyzing how an endogenous decision made by firms impacts their outcomes. Nevertheless, we view this firm-level analysis as still informative in painting a more comprehensive picture of the overall effects of opting out and its correlates.

6 Results

We now present the results, focusing on the effects of the opt-out events on worker wages, employment, employment with the opting-out employer, and earnings. Section 6.1 does so for the FD secession. We then turn to the staggered DiD design to study the pirate agreement adoptions in Section 6.2.

6.1 The 2011 Secession of Mass Retailers

Figure 4 reports the DiD effects (β_t) of the FD opt-out on the cohort of workers employed at the FD firms in 2010 relative to the cohort of workers employed instead by CC firms. Complementing the year-specific DiD coefficient plot, Table 3 reports full regression results, for three horizons: on-impact results for the 2011 coefficient, the medium run effects for the year 2013, and the longer run effects for the year 2016, along with additional information including a formal pre-period test and control means.

Wages We start by analyzing the effect of the opt-out event on wages—the primary channel through which opt-outs affect labor market outcomes. Panel (a) of Figure 4 (and Table 3 Column (1)) reports on those effects, first studying "unconditional" log weekly wages, i.e., without restricting to workers who continue to be employed with their 2010 employer. Overall, we find negative wage effects. We organize our discussion of the dynamics of the wage effects across the three time windows in which the FD opt-out occurred—see Figure 1 and the account of the event in Section 3.1.

Prior to the 2011 opt-out, Panel (a) of Figure 4 (blue hollow circles, "unconditional") shows no evidence for a differential wage evolution between FD firms and CC firms. This result implies

[20]The small number of treated firms present in the FD mass-retailers opt-out makes firm-level analysis for such event underpowered and we thus only focus on pirate agreements for this type of analysis.

parallel pre-event trends, supporting our identification strategy (the informative window is 2005-2007, since we match on 2008-2010 variables).

In the first post-period time window, right after the 2011 opt-out event, wages start to decrease. Effect sizes in this time window are moderate (around 1%, SE 0.0058) and mostly imprecisely estimated.

The bulk of the wage decrease is found post-2015, i.e., after the control firms signed the new CC-CBA with higher wage floors. The effect on wages reaches a peak of around 4% (SE 0.0095) in 2018, which is only slightly lower in magnitude than the maximum divergence in CBA wage *floors* between FD and CC firms (4.8%) documented in Figure 1. Therefore, it appears that the reduction in wages experienced by FD workers is primarily driven by higher wage floors signed in the CC-CBA.

Figure 4, Panel (a) further confirms this result by showing that the bulk of the wage decrease is found among stayers (in red triangles), i.e., individuals that remain with their 2010 employer and are thus mechanically exposed to the differences in wage floors following the opting-out decision of FD employers.[21] Hence, the wage effects reflect wage policy shifts induced by the opting-out event rather than compositional changes.

Finally, we find not clear evidence for attenuation of the wage effect in the least year of our sample, 2019, which is the year in which FD firms effectively re-adopted the CC-CBA (see the discussion in Section 3.1). That is, even after the realignment in wage floors, workers subject to the FD opt-out still experience wage losses of about 3-4% unconditionally, and 4-5% for stayers.

To summarize, firms that opt out of centralized collective bargaining take advantage of the resulting downward flexibility in wage adjustment. As a result, incumbent workers exposed to the opt-out event face significant wage declines that they are not able to evade, e.g., by rebargaining wage premia/top-ups or by switching employers, even in 2019 following the realignment between the FD and CC CBA described in Section 3.1.

Robustness Check: 2011 Reform of Shopping Hours Deregulation Appendix Figure A2 shows that the results are not driven by a potential confounder, a contemporaneous nationwide deregulation of retail shop hours introduced by decree 201/2011 (so-called *Salva Italia*) (Rizzica, Roma and Rovigatti, 2023), which could have affected FD firms differentially. We show this by exploiting heterogeneity in municipalities that were not exposed (as they had already liberalized shopping hours before 2011) and showing similar wage results in either group of workers.[22]

[21] For this specific outcome, to minimize selection issues arising from imposing a restriction based on an outcome affected by the treatment of the interest (remaining employed with the 2010 employer), we fit Equation (1) just among pairs of matched treatment and control workers where both the treated worker and their matched control worker are still employed by their 2010 employer. All other outcomes do not impose this restriction.

[22] In principle, such a liberalization could differentially affect mass-retail firms, i.e., precisely the FD firms that decided to opt-out from the CC-CBA. Importantly, a staggered liberalization of shop hours for municipalities most reliant on tourism had already started in 1998, with specific application criteria set by regional governments. To show that this reform does not explain our results, we repeat our analysis considering separately the workers employed in municipalities that had already liberalized shop hours in the year before the FD opt-out, and those that removed the shop hours restrictions only in 2011 as a result of the decree. We thank Rizzica, Roma and Rovigatti (2023) for sharing

Employment Figure 4 Panel (b) (and Table 3 Column (2)) reports the results for employment, defined as the probability that the worker employed in the treated or control firms in 2010 is employed in a given calendar year (i.e., has positive labor earnings). Pre-trends are flat (also in the early years we do not match on). We estimate a positive effect of around 3 percentage points (SE 0.0043) that opens up already in the "interim period" between 2011 and 2014 (after the opt-out but before the new CC-CBA was signed).[23] After 2015, the significant positive employment effect persists and increases to about 4 percentage points (SE 0.0138).

Hence, the evidence points towards employment and wages moving along the labor demand curve of the firm: by opting-out FD firms are able to pay lower wages compared to CC firms who are instead implementing the new CBA with higher wage floors. This causes a relative increase in demand among FD firms which improves the employment prospects of FD incumbent workers, as demonstrated further below, where we analyze what happens to retention probabilities.

Probability of Staying with 2010 Employer The positive employment effect appears to be driven by retention, as we find a significant increase in the probability of remaining employed by the 2010 employer as shown in Figure 4 Panel (c) (and Table 3 Column (3)). Post opt-out, FD employees see an increase in their probability to remain employed with an FD firm of around 20 percentage points (SE 0.0439) in the long run, despite not seeing any significant differences in these outcomes in the pre-opt out years (even prior to 2008, when no tenure or employment restrictions were imposed). Quantitatively, the large difference in effect sizes between employment anywhere and employment with the 2010 employer reflects the fact that around 3/4 of workers in the control group that separate from their CC firm are able to find employment at some other firm.

Thus far, the evidence suggests that providing firms with the possibility to deviate downwards from wage floors reduces incumbent workers' wages but raises their retention and ultimately their employment prospects. We next turn into understanding how this opt-out event ultimately impacts incumbent workers' earnings.

Earnings We close our analysis with the most comprehensive outcome: total earnings of affected workers.The results are displayed in Figure 4 Panel (d) (and Table 3 Column (4)). We find no clear positive or negative effect on earnings. Estimates are overall insignificant for the post-opt-out years, albeit with a slight positive upward trend in the pre-event years. As a result, FD incumbent workers do not seem to earn systematically more on average after the FD opting-out decision.

6.2 Adoptions of Pirate Agreements

We now study the effects of the pirate agreements adoptions, drawing on a harmonized (now staggered) DiD design—providing a much larger sample of firm-level events. We find effects that

these data with us.

[23] Positive employment effects before the divergence of the wage floors are consistent with firms' retention decisions being based on expected present values of wages.

are consistent with the FD event study of one specific opt-out event. Figure 5 shows the effects of the adoption of pirate CBAs, as estimated in Equation (2). Table 4 shows the associated regression results.

Wages As for the FD event, we find that workers experience a decrease in log weekly wages when their employer opts out and moves them into a pirate agreement, compared to the evolution of matched control workers' wages. Figure 5 Panel (a) (and Table 4 Column (1)) presents those estimates.

The wage loss is about 2% (SE 0.0041) and opens up nearly immediately following the opt-out event. The negative wage effect remains stable at around 2.5% in the years following the opt-out.[24] As for the FD event, stayers experience a wage reduction of roughly the same magnitude as what we find for the overall sample of workers, pointing towards wage policy shifts rather than compositional changes.

Employment Similarly to the FD opt-out event, we document a positive employment effect, of about 2 percentage points (SE 0.0052) two years after the event, which increases over time up to 3 percentage points (SE 0.0087, see Figure 5 Panel (b) and Table 4 Column (2)). This effect appears in part to be driven by a higher—but not precisely estimated—probability of staying with the opting-out firm (Panel (c) and Column (3)).[25]

Earnings As in the FD opt-out event, earnings seem largely unaffected by the adoption of pirate CBAs, consistent with the idea of the positive employment effect offsetting the negative effect on wages. We show these results in Panel (d) of Figure 5 (and Table 4 Column (4)). We note that pre-trends point estimates provide some caveats, and again confidence intervals are wide for this (dispersed) outcome of effects on euro levels of earnings.

Interim Summary The pirate agreement adoptions are associated with similar effects on workers as in the FD opt-out event: lower wages, higher employment probability, and, on net, statistically insignificant earnings effects that are centered around zero. Thus both FD and pirate-opt out events point to the same economic channel: firms cut wages when they can by opting out, and the wage reductions are accompanied by positive, rather than negative, employment effects.

Complementary Perspective: Firm-Level Design We complement the worker-level evidence with a (pirate agreement) firm-level analog, which we detail in Appendix B (which we could not credibly attempt in the FD design as the FD firms are few and due to the large baseline differences in firm characteristics). This firm-level analysis provides an assessment of the effects on the full

[24] Estimating the actual pass-through from wage floors to realized wages is more complex for the pirate CBA design, as we cannot observe wage floors for most pirate agreements and, as a result, we cannot properly quantify the drop in negotiated wage floors.

[25] Firm size and hence baseline turnover is more aligned in this design, and hence the point estimates are now comparable between the unconditional and conditional employment effects.

workforce, rather than focusing on incumbent workers. We observe a decline in labor costs of about 3% immediately following the opt-out, which remains stable in the ensuing years. 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 3 percentage points. This probability remains positive for two years after the opt-out but declines towards zero afterward. We find a positive effect on firm size (log employment), but with significant pre-trends that cloud interpretation.[26] This result—with the caveats on the firm-level design mentioned above in Section 5.3—suggests that the positive employment effects are primarily driven by incumbent workers.

7 Does Opting-Out Lead to Higher Employment in the South of Italy?

The longstanding inability of Italian firms to deviate from national CBAs has recently been identified as a potential factor contributing to the persistently high unemployment rates in Southern Italy (Boeri et al., 2021). Firms in the South typically exhibit lower productivity compared to their Northern counterparts. However, due to the wage floors mandated by national CBAs, these less productive firms cannot reduce wages to reflect their economic conditions, possibly resulting in higher unemployment in the region.[27] The opting-out events analyzed in this paper offer a unique opportunity to assess empirically how allowing firms to deviate from national CBAs would affect employment levels in Southern Italy.

Main Results on Wages, Employment, Earnings Figure 6 displays the results for log wages, retention and employment, separately for the North and South of Italy, across the two opting-out events: the secession of mass retailers (Panels (a) and (b)) and the adoption of pirate agreements (Panels (c) and (d)).[28] A consistent pattern emerges across both events: the positive employment effects are predominantly concentrated among incumbent workers employed by firms in the Center-North. In contrast, the overall employment probability for Southern workers does not appear to increase significantly, particularly over longer time horizons.[29] As with the baseline estimates, employment dynamics are primarily driven by the retention margin: the probability of remaining employed with the same firm following the opting-out event is significantly higher for workers in the North, whereas the effect is statistically insignificant in the South when looking at the FD event or even *negative* when considering pirate agreements.

The evidence in Figure 6 thus suggests that Southern workers do not experience higher employment probabilities following the opt-out decision of their employer. As a result, Appendix

[26] In the supplemental specification, we find that after netting out this linear pre-trend (see Dustmann, Lindner, Schönberg, Umkehrer and Vom Berge, 2022, for a similar approach), the opt-out decision had overall a small positive impact on employment in the opt-out year, which does not persist in time and eventually turns negative.

[27] A similar argument is presented in Dustmann et al. (2014), which highlights how the ability of firms in Eastern Germany to deviate from national CBAs played a crucial role in the country's remarkable employment performance during the Great Recession and the Eurozone crisis of 2012.

[28] Specifically, we re-estimate Equations (1) and (2) depending on the location (Center-North vs. South) of the job observed in the year preceding the event.

[29] Note that for the pirate CBA we find larger, in absolute value, negative effects on log wages in the South compared to the North whereas for the FD design the effects on wages are pretty uniform between the North and the South.

Figure A3 shows that, for both events, the overall earnings impact for Southern workers is negative, as these individuals face large wage reductions that are not accompanied by higher employment prospects.

Exploring Potential Channels: Proxies for Voluntary and Involuntary Separations What can explain these divergent responses found between the North and the South? We attempt to answer this question by leveraging information on the reason behind a job separation, which is available in the INPS data since 2005.[30] This information allows to distinguish between involuntary separations, i.e., those initiated by the employer (these capture layoffs due to financial difficulties of the firm, outsourcing/mergers, end of temporary contracts, and firing for just cause) and voluntary separations (the worker has voluntarily resigned from the job).

Figure 7 displays the results on the probability to separate voluntarily or involuntarily from a job when looking at workers that before the opt-out event were employed either in the North or in the South. We begin by focusing on pirate agreements, where a stark pattern emerges: the drop in retention probabilities for Southern workers (Figure 6, Panel d) appears to be explained by an increase in the probability to voluntarily resign from a job where a pirate CBA has been applied, as shown in Figure 7 Panel (d). Southern workers are therefore resigning from these jobs covered now by a pirate agreement and appear to be able to find a new job at a different employer at a rate high enough to make the overall employment effect of the opt-out event close to zero, as shown in Figure 6, Panel (d). Note that Figure 7 Panel (d) also points to null or if anything positive effects on the probability to experience an involuntary separation. It therefore appears that the possibility to opt-out from national CBA does not significantly decrease the probability that Southern employers conduct a mass-layoff event or do not renew temporary contracts—the two most common factors that lead to involuntary separations in the INPS data.

These patterns differ for Northern workers whose CBA was converted to a pirate agreement, as shown in Figure 7 Panel (c). For them, the probability of involuntary separations drops significantly after adoption, while resignation rates remain basically unaffected. The possibility to deviate downwards from a national CBA by adopting a pirate CBA thus leads to higher employment prospects just for workers in the North as their employers are significantly less likely to initiate job separations. In contrast, pirate CBAs do not significantly reduce the likelihood of involuntary separations for Southern workers, who are also more likely to resign once covered by such agreements, possibly as a result of the larger negative wage effects experienced by these incumbent workers. These dynamics help explain why employment rises for Northern workers but not for Southern workers in the medium to long term.

We now turn to the FD event, reporting results in Figure 7 Panels (a) and (b). Here we find

[30]Other reasons for separations (e.g., retirements) are coded as "Other Reasons" and are not part of our definition of either voluntary or involuntary separations. The reason for separation is provided by the employer and is used to assess pension contributions and severance payments. This information is available for roughly 75% of all separations observed in the data. Missing values tend to occur for job separations where the employer shuts down or for separations that occurred in the last day of December. See Di Addario et al. (2023) and Daruich et al. (2024) for recent papers that have explored this information.

a broadly similar pattern in involuntary separations as for the pirate agreements. Workers in the South whose employer opts out of the CC-CBA do not appear to experience a reduction in the probability of an involuntary separation. Conversely, this probability decreases significantly for workers in the North and thus explain the divergent path in terms of employment between regions shown in Figure 6, Panel (a) and (b).[31]

Summary and Interpretation In conclusion, our findings show that opting out of national CBAs increases employment probabilities exclusively for workers in the North. This arises because firms in the North appear better positioned to utilize the flexibility from the opting-out event to avoid the need for, say, implementing a mass layoff. Conversely, opting-out does not raise significantly the probability of employment among southern workers as their employers do not appear less likely to engage in involuntary separation practices following the opting-out event.[32]

A potential explanation for these findings can be grounded in a monopsony framework with heterogeneous productivity levels and labor supply elasticities across firms. The opting-out event allows firms to transition from a wage equilibrium determined by the national CBAs to one where wages are set independently, based on each firm's specific production and labor supply conditions. Figure A4 illustrates this mechanism by comparing two firms that initially paid identical nationally mandated wage floors. These firms differ in their primitives: one is a highly productive firm with a relatively elastic labor supply (e.g., a firm located in the North), while the other has lower productivity and a more inelastic labor supply (e.g., a firm in the South). After opting out, the Northern firm sets wages below the CBA floor but achieves higher employment levels. The Southern firm—given its lower productivity and more inelastic labor supply curve—reduces wages even further but also experiences lower employment compared to the CBA-determined equilibrium. Thus, opting out results in lower wages for both firms (with a more negative effect for the Southern firm) but produces divergent employment outcomes: increased employment for the Northern firm and reduced employment for the Southern one. This pattern aligns with our empirical results and the spirit of standard monopsony models which predict that imposing a wage floor (such as a minimum wage) above the market equilibrium can increase employment

[31] Interestingly, the FD opt-out event differs from pirate agreements in its impact on the probability of resigning from the current job. For the FD event we find that (i) both workers in the South and the North are less likely to resign from their job and (ii) effects from the South are larger (in absolute value) compared to what found in the North. These patterns on resignation (combined with the effects on involuntary separations) explain why the event-study coefficients on the probability of retention for the FD-event for Southern workers show basically a null and insignificant effect (whereas this effect is negative in the pirate agreements event-studies). The fact that regardless of the region resignation patterns are substantially decreased when analyzing the FD-event suggests that the secession of mass retailers might have increased the amenities present in FD-jobs compared to what instead supplied in CC-jobs following the opting-out event. Based on accounts of why FD-firms decided to recess from the CC-CBA, it is possible that one such amenity is hours flexibility (Ganong et al., 2024) although we should caveat that we lack the data to test this particular margin.

[32] Our analysis focuses only on incumbent workers. It is possible that employment responses following the opting-out event are larger in the South relative to the North when looking at the hiring margin. Figure B2 in the Appendix shows the impact of opting-out on firm size in the North vs. South for the adoption of a pirate agreement. Under the caveats described in Section 5 when analyzing outcomes at the firm level, we do not find that opting-out leads to an increase in employment in Southern firms. There is, however, some evidence of substitution as Southern firms appear to be replacing workers who are now voluntarily leaving the firm following the adoption of a pirate CBA with new workers, although we should caveat the estimates on firm size appear noisy and characterized by some existing pre-trends.

when firms have the power to set wages significantly below workers' marginal product (Card and Krueger, 2016; Azar et al., 2023).

8 Conclusion

This paper estimates the effects of employers' opting out of heavily centralized collective bargaining agreements in Italy on the wage and employment of their incumbent workers. Analyzing two separate opt-out events, we find that opt-outs entail wage cuts but raise both tenure as well as employment. Inspired by the recent literature that notes the potential distortions caused by centralized CBAs on the allocation of labor across Italian regions (Dustmann et al., 2014; Boeri et al., 2021), we then examine how these effects differ between the North and South of Italy. The positive employment effects of opting out are concentrated among workers in the North, where employers are significantly less likely to engage in involuntary separations (e.g., mass layoffs, termination of temporary contracts) after opting out. In contrast, workers in the South face larger wage reductions, but their employers do not show a significant decrease in involuntary separations post-opt-out.

There are two key takeaways from this analysis. First, the possibility to set wages below what is determined by national CBAs appears, on average, to increase the retention and thus the employment of workers directly affected by the opting-out decision of firms. However, this ultimately leads to null effects of earnings as incumbents' wages drop significantly post opt-out.

Second, opting out does not appear to significantly increase employment of incumbent workers in the South. We speculate that this may be due to Southern firms facing a relatively inelastic labor supply curve. For these firms, opting out allows them to set wages at a level that could lead to lower employment compared to what observed when using the wage floors mandated by national CBAs. This finding underscores the importance of national CBA wage floors as a mechanism to address inefficiencies that stem from imperfect labor markets, similar to what has been noted when analyzing minimum wages (Acemoglu, 2001; Dustmann et al., 2022).

We close by acknowledging the limitations of our study and provide directions for future research. Our research design focuses on the consequences of opting-out on workers directly impacted by this institutional arrangement (e.g., workers whose CBA switches from a nationally representative one to a pirate one). This permits to examine the effect on retention probabilities of these workers and track their employment even if they are no longer employed by the employer that opted out. However, this limits our ability to study impacts of opting-out on the hiring margin and, more broadly, whether opting-out leads to increases in aggregate output. Simple event-studies at the firm-level for the adoption of pirate agreements suggest that these impacts appear to be quite limited but more research on this aspect—based on better suited research designs to capture impacts at the firm level—could potentially complement our worker-level findings. Also, our analysis is silent about the potential spillovers that opting-out decisions might have on other firms. Evidence from the union literature (e.g. Green et al., 2024) as well as wage settings where firms are interconnected via sectoral collective bargaining agreements (Hermo, 2024) suggests that

these spillovers can play an important role. We therefore view our estimates of opting-out based on micro-level data—where we can accurately observe employers abandoning national CBAs—as a complement to studies based on comparisons of equilibrium outcomes across different economies with varying degrees of unionization and centralization of collective bargaining agreements (e.g., Boeri, Ichino, Moretti and Posch, 2021; Jäger, Naidu and Schoefer, 2024).

References

Acemoglu, Daron, "Good jobs versus bad jobs," *Journal of labor Economics*, 2001, *19* (1), 1–21.

Addario, Sabrina Di, Patrick Kline, Raffaele Saggio, and Mikkel Sølvsten, "It ain't where you're from, it's where you're at: hiring origins, firm heterogeneity, and wages," *Journal of Econometrics*, 2023, *233* (2), 340–374.

Azar, José, Emiliano Huet-Vaughn, Ioana Marinescu, Bledi Taska, and Till von Wachter, "Minimum Wage Employment Effects and Labour Market Concentration," *The Review of Economic Studies*, 09 2023, p. rdad091.

Bertheau, Antoine, Edoardo Maria Acabbi, Cristina Barceló, Andreas Gulyas, Stefano Lombardi, and Raffaele Saggio, "The Unequal Consequences of Job Loss Across Countries," *American Economic Review: Insights*, 2023, *5* (3), 393–408.

Boeri, Tito, "Two-Tier Bargaining," IZA Discussion Papers 8358, Institute of Labor Economics (IZA) 2014.

_ **and Pietro Garibaldi**, "A Tale of Comprehensive Labor Market Reforms: Evidence from the Italian Jobs Act," *Labour Economics*, 2019, *59* (C), 33–48.

_ **, Andrea Ichino, Enrico Moretti, and Johanna Posch**, "Wage Equalization and Regional Misallocation: Evidence from Italian and German Provinces," *Journal of the European Economic Association*, 2021, *19* (6), 3249–3292.

Calmfors, Lars and John Driffill, "Bargaining Structure, Corporatism and Macroeconomic Performance," *Economic Policy*, 1988, *3* (6), 14–61.

Card, David and Alan B Krueger, *Myth and measurement: The new economics of the minimum wage*, Princeton University Press, 2016.

Centamore, Giulio, "Contratti Collettivi o Diritto del Lavoro «Pirata»?," *Variazioni su Temi di Diritto del Lavoro*, 2018, *2*, 471.

Dahl, Christian, Daniel le Maire, and Jakob Munch, "Wage Dispersion and Decentralization of Wage Bargaining," *Journal of Labor Economics*, 2013, *31* (3), 501 – 533.

D'Amuri, Francesco and Raffaella Nizzi, "Recent Developments of Italy's Industrial Relations System," Questioni di Economia e Finanza (Occasional Papers) 416, Bank of Italy, Economic Research and International Relations Area 2017.

Daruich, Diego, Martino Kuntze, Pascuel Plotkin, and Raffaele Saggio, "The Consequences of Domestic Outsourcing on Workers: New Evidence from Italian Administrative Data," 2024.

_ , **Sabrina Di Addario, and Raffaele Saggio**, "The Effects of Partial Employment Protection Reforms: Evidence from Italy," *Review of Economic Studies*, 2023, *90* (6), 2880–2942.

Dell'Aringa, Carlo, "Dai Minimi Tabellari ai Salari di Garanzia," in Carlo Dell'Aringa, Claudio Lucifora, and Tiziano Treu, eds., *Produttività, Diseguaglianze*, Il Mulino, 2017.

Dustmann, Christian, Attila Lindner, Uta Schönberg, Matthias Umkehrer, and Philipp Vom Berge, "Reallocation effects of the minimum wage," *The Quarterly Journal of Economics*, 2022, *137* (1), 267–328.

_ , **Bernd Fitzenberger, Uta Schönberg, and Alexandra Spitz-Oener**, "From Sick Man of Europe to Economic Superstar: Germany's Resurgent Economy," *Journal of Economic Perspectives*, 2014, *28* (1), 167–88.

Faia, Ester and Vincenzo Pezone, "The Cost of Wage Rigidity," *Review of Economic Studies*, 2023, p. rdad020.

Fanfani, Bernardo, "The Employment Effects of Collective Wage Bargaining," *Journal of Public Economics*, 2023, *227*, 105006.

Ganong, Peter, Pascal Noel, Christina Patterson, Joseph Vavra, and Alexander Weinberg, "Earnings Instability," 2024.

Goldschmidt, Deborah and Johannes F Schmieder, "The Rise of Domestic Outsourcing and the Evolution of the German Wage Structure," *The Quarterly Journal of Economics*, 2017, *132* (3), 1165–1217.

Green, David A, Benjamin Sand, and Iain G Snoddy, "The impact of unions on nonunion wage setting: Threats and bargaining," Technical Report, Working Paper Series 2024.

Guiso, Luigi, Luigi Pistaferri, and Fabiano Schivardi, "Insurance within the Firm," *Journal of Political Economy*, 2005, *113* (5), 1054–1087.

Gürtzgen, Nicole, "Estimating the Wage Premium of Collective Wage Contracts: Evidence from Longitudinal Linked Employer–Employee Data," *Industrial Relations: A Journal of Economy and Society*, 2016, *55* (2), 294–322.

Hermo, Santiago, "Collective Bargaining Networks, Rent-sharing, and the Propagation of Shocks," 2024.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan, "Earnings Losses of Displaced Workers," *The American economic review*, 1993, pp. 685–709.

Jäger, Simon and Jörg Heining, "How Substitutable are Workers? Evidence from Worker Deaths," Technical Report, National Bureau of Economic Research 2022.

_ , **Shakked Noy, and Benjamin Schoefer**, "The German Model of Industrial Relations: Balancing Flexibility and Collective Action," *Journal of Economic Perspectives*, 2022, *36* (4), 53–80.

Jäger, Simon, Suresh Naidu, and Benjamin Schoefer, "Unions, Collective Bargaining, and the Wage Structure: An International Perspective," *Working Paper*, 2024.

Jimeno, Juan and Carlos Thomas, "Collective Bargaining, Firm Heterogeneity and Unemployment," *European Economic Review*, 2013, *59* (C), 63–79.

Kugler, Adriana and Giovanni Pica, "Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform," *Labour Economics*, 2008, *15* (1), 78–95.

Lucifora, Claudio and Daria Vigani, "Losing Control? Unions' Representativeness, Pirate Collective Agreements, and Wages," *Industrial Relations: A Journal of Economy and Society*, 2021, *60* (2), 188–218.

OECD, "Negotiating Our Way Up," 2019, p. 270.

_ , "Trade Unions: Collective bargaining coverage (Edition 2023)," 2024.

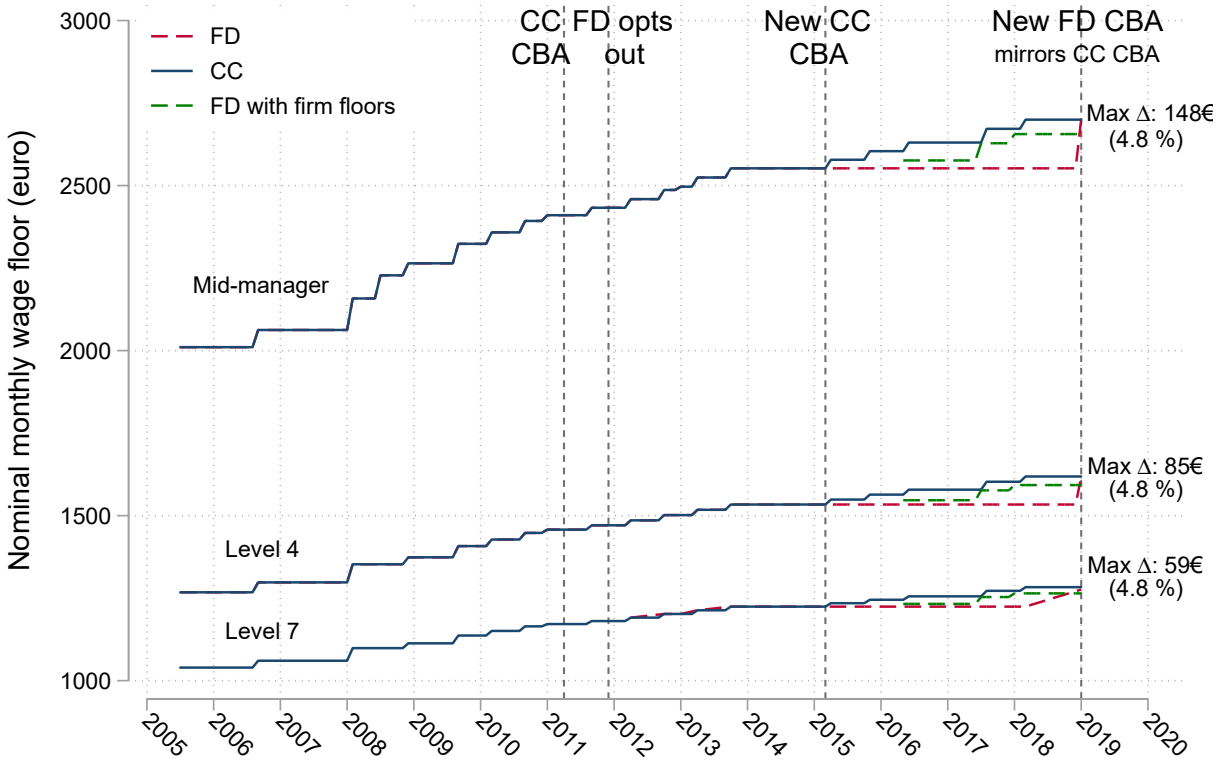
Rizzica, Lucia, Giacomo Roma, and Gabriele Rovigatti, "The Effects of Deregulating Retail Operating Hours: Empirical Evidence from Italy," *The Journal of Law and Economics*, 2023, *66* (1), 21–52.

Schmieder, Johannes F, Till Von Wachter, and Jörg Heining, "The Costs of Job Displacement over the Business Cycle and its Sources: Evidence from Germany," *American Economic Review*, 2023, *113* (5), 1208–1254.

Visser, Jelle, "Wage Bargaining Institutions – from Crisis to Crisis," European Economy - Economic Papers 2008 - 2015 488, Directorate General Economic and Financial Affairs (DG ECFIN), European Commission 2013.

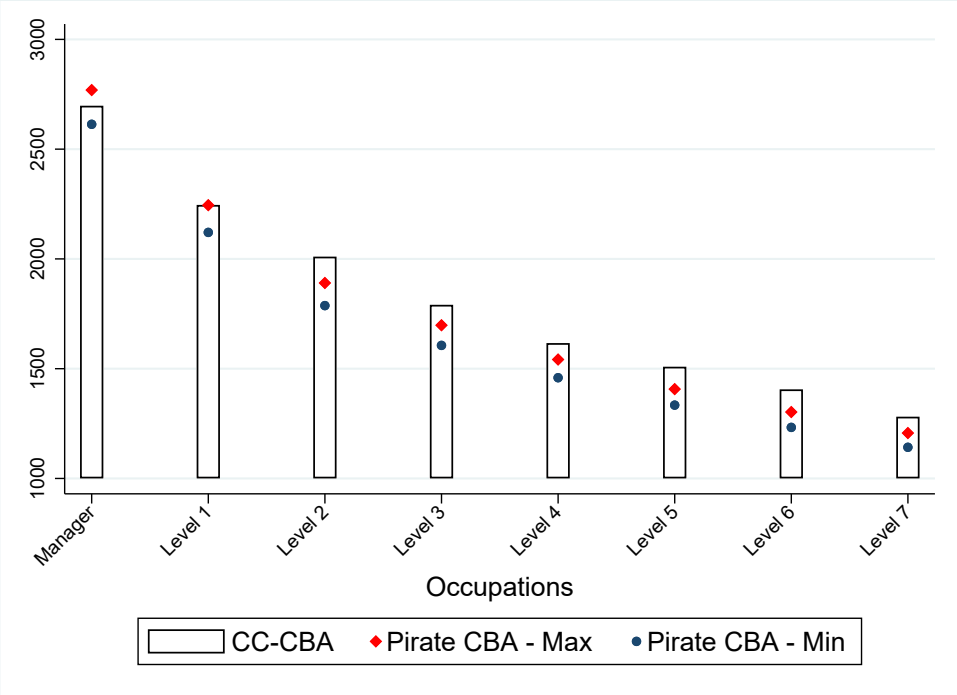
Figures

Figure 1. The 2011 Secession of Mass Retailers from their CBA: Timeline of Wage Floors for Three Job Titles



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

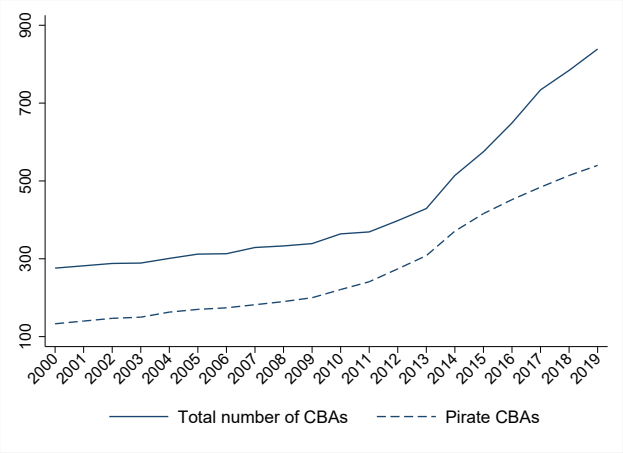
Figure 2. Case Study of Pirate Agreement vs. Conventional CBA: Wage Floors in the Wholesale and Retail Sector, 2018



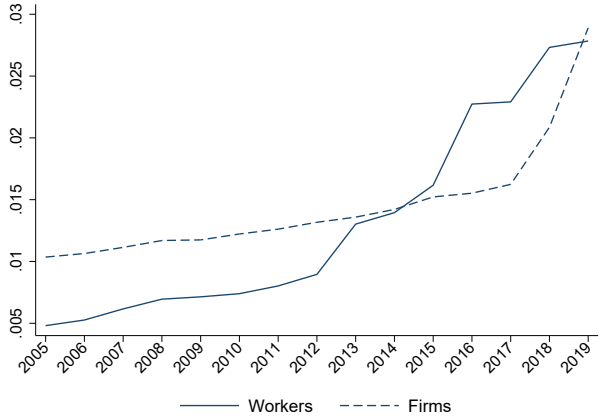
Notes: Wage floors across job titles for the CC-CBA (CNEL CBA code H011, white bars) and pirate CBA (CNEL CBA code H024, red and blue markers) in the retail sector in 2018. For the pirate CBA, the top (red) marker is the wage floor for Lombardy (the region with the largest equalization element) and the bottom (blue) marker is the wage floor for Molise (the region with the lowest equalization element).

Figure 3. The Evolution of Pirate Agreements

(a) Counts of Contracts

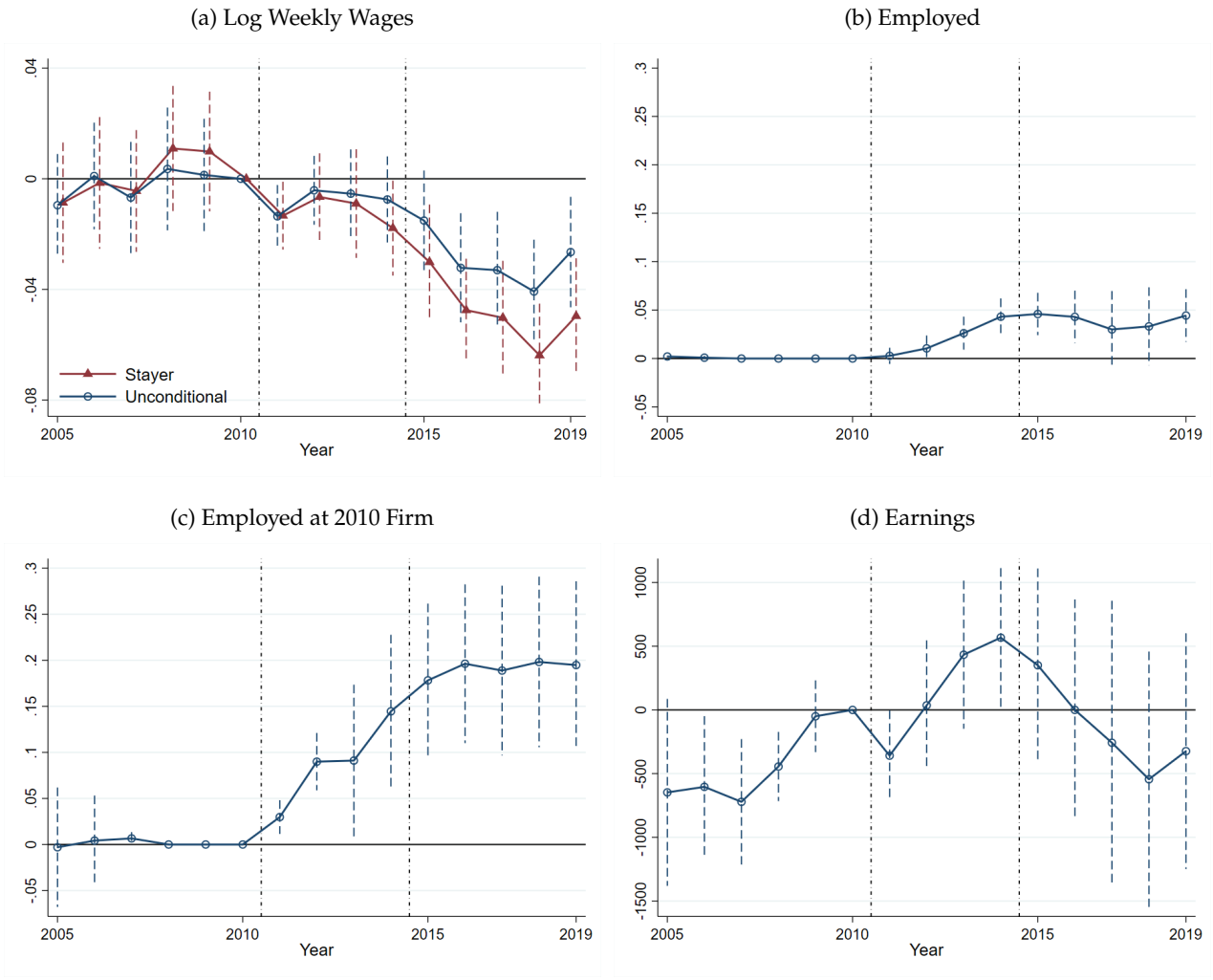


(b) Share of Workers and Firms With Pirate Contract



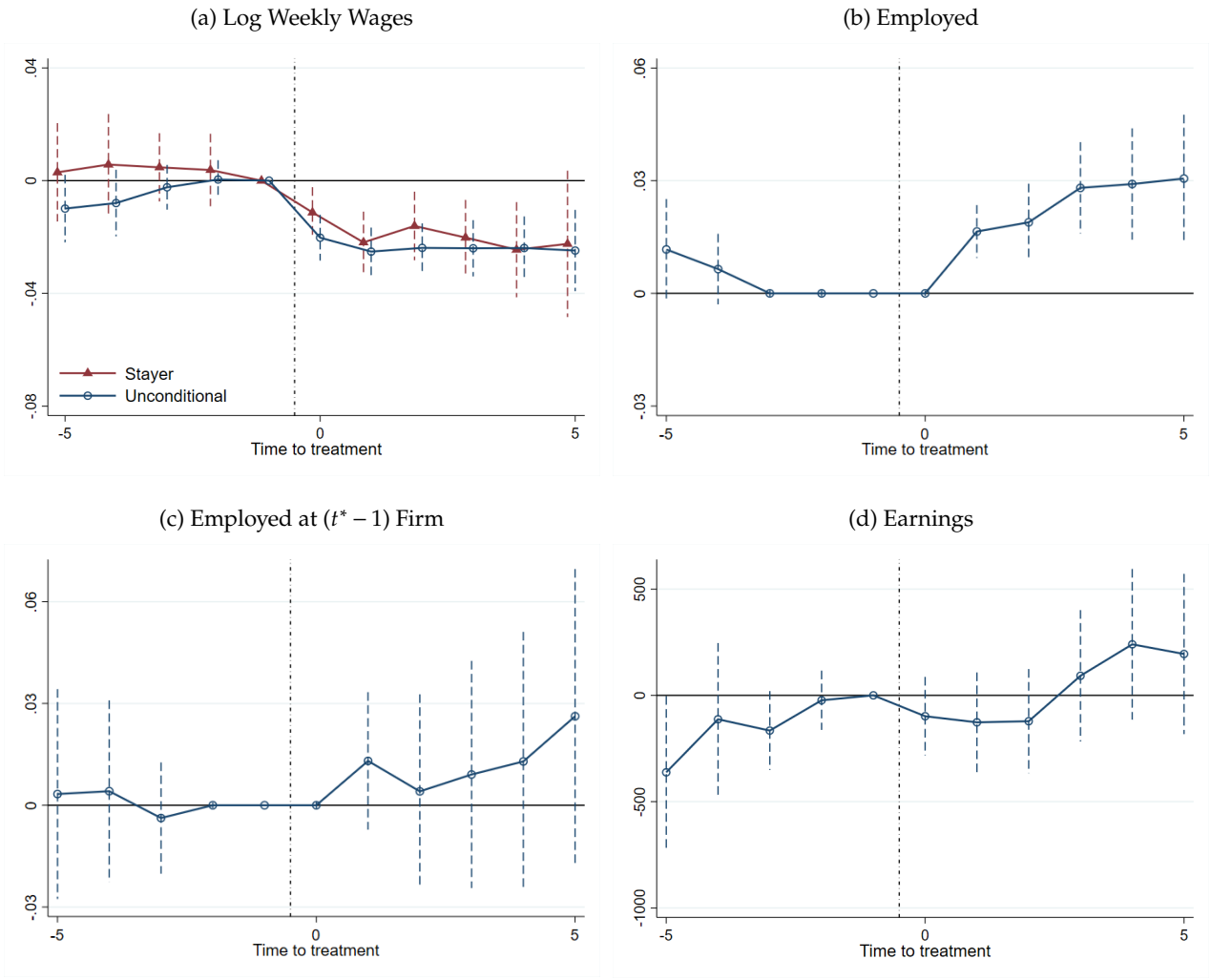
Notes: Panel (a) depicts the total number of CBAs in Italy per year, separately for pirate agreements (dashed line) and the total number including them (solid line). Pirate agreements are defined as those not signed by at least one of the three traditional unions (CGIL,CISL,UIL). Panel (b) shows the fraction of firms (dashed line) and workers (solid line) covered by pirate CBAs. For firms, the share is computed as the number of firms adopting a pirate contract 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 contract as a fraction of the total number of private-sector workers in the INPS data each year.

Figure 4. The Effects of the 2011 Secession of Mass Retail Employers on Workers



Notes: This figure displays the event-study coefficients from Equation (1) estimated on the matched sample defined in Section 5.1. Panel (a): Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. We report these effects unconditionally, and additionally conditioning on the worker staying with their 2010 employer. To construct the latter, we fit Equation (1) just among pairs of matched treated and control workers where they are both still employed with their 2010 employer. All other outcomes in this figure are for the unconditional sample. Panel (b): Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Panel (c): the outcome is an indicator equal to 1 if a given worker in year t is employed by their 2010 employer. Panel (d): Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Table 3 reports these event-study coefficients along with additional summary statistics. The two vertical lines correspond to 2011—the year when FD abandoned the CC employer organization—and 2015, the year when a new CC-CBA was signed. Standard errors are clustered at the level of the 2010 employer.

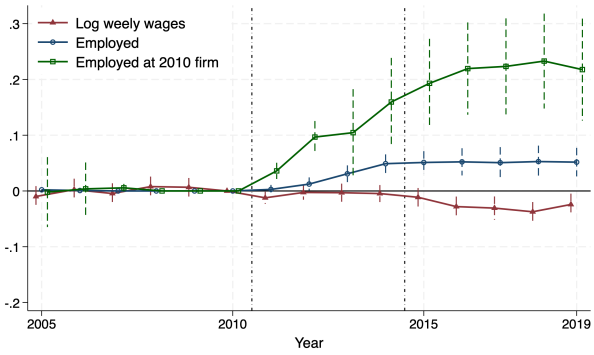
Figure 5. The Effects of Pirate Agreement Adoptions on Workers



Notes: This figure displays the event-study coefficients from Equation (2) estimated on the matched sample defined in Section5.2. Panel (a): Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. We report these effects unconditionally, and additionally conditioning on the worker staying with their $t^* - 1$ employer. To construct the latter, we fit Equation (2) just among pairs of matched treated and control workers where they are both still employed with their of the $t^* - 1$ employer. All other outcomes in this figure are for the unconditional sample. Panel (b): Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Panel (c): the outcome is an indicator equal to 1 if a given worker in year t is employed by their $t^* - 1$ employer, where t^* denotes the year of transition to a pirate agreement. Panel (d): Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Table 4 reports these event-study coefficients along with additional summary statistics. Standard errors are clustered at the level of the $t^* - 1$ employer.

Figure 6. The Effects of Opt-Outs on Workers: Heterogeneity by Geography

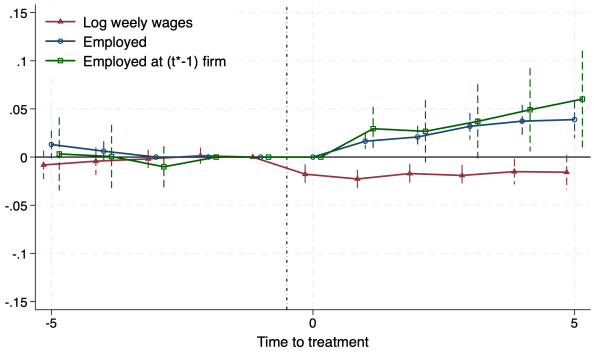
(a) FD design: Center-North



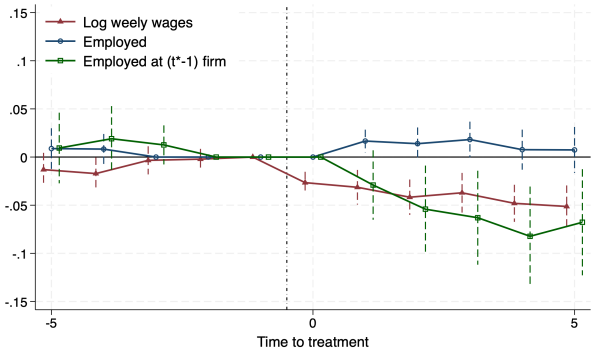
(b) FD design: South



(c) Pirate CBAs design: Center-North



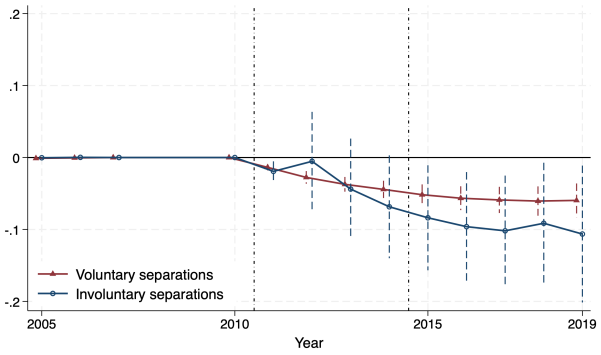
(d) Pirate CBAs design: South



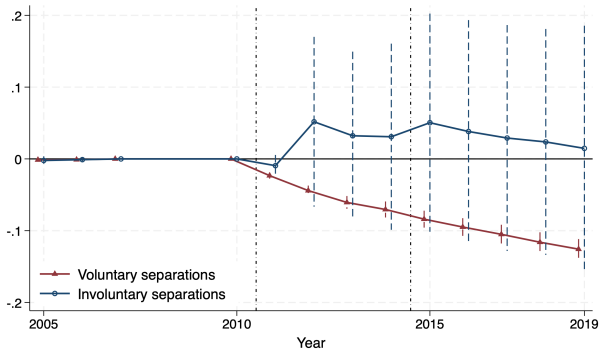
Notes: This figure displays the event-study coefficients from Equation (1) estimated on the matched sample defined in Section 5.1 (Panels a-b) and from Equation (2) estimated on the matched sample defined in Section 5.2 (Panels c-d). The regressions are run separately for workers in the Center-North and South of Italy in 2010 (Panel a for Center-North, Panel b for South) or at time $t^* - 1$ (Panel c for Center North, Panel d for South). Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily. Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Employed at 2010 firm or $(t^* - 1)$ firm is an indicator equal to 1 if a given worker in year t is employed by their 2010 employer or their $t^* - 1$ employer (t^* denotes the year of transition to a pirate agreement). Standard errors are clustered at the level of the 2010 employer (Panels a-b) or the $t^* - 1$ employer (Panels c-d).

Figure 7. The Effects of Opt-Outs on Worker Separations: Heterogeneity by Geography

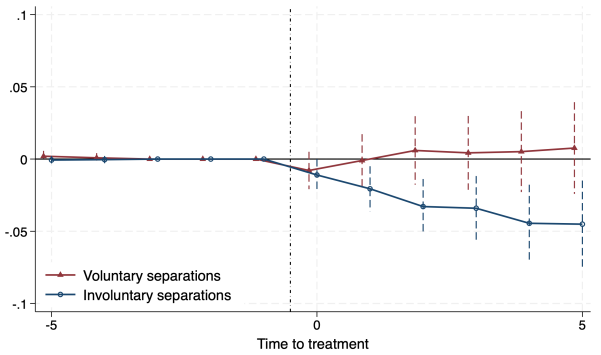
(a) FD design: Center-North



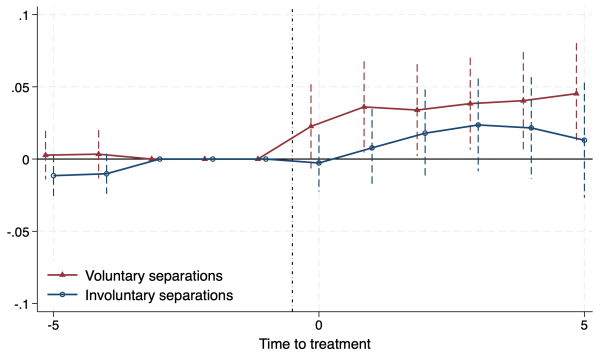
(b) FD design: South



(c) Pirate CBAs design: Center-North



(d) Pirate CBAs design: South



Notes: This figure displays the event-study coefficients from Equation (1) estimated on the matched sample defined in Section 5.1 (Panels a-b) and from Equation (2) estimated on the matched sample defined in Section 5.2 (Panels c-d). The regressions are run separately for workers in the Center-North and South of Italy in 2010 (Panel a for Center-North, Panel b for South) or at time $t^* - 1$ (Panel c for Center-North, Panel d for South). Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily. Voluntary separations is a binary indicator taking value of one if the worker has resigned from their 2010 job (Panels a-b) or their $t^* - 1$ job (Panels c-d). Once the worker resigns, the outcome stays equal to one for the following years. Panel (b): The outcome is a binary indicator taking value of one if the worker has been fired from their 2010 job (Panels a-b) or their $t^* - 1$ job (Panels c-d). Once the worker has been fired, the outcome stays equal to one for the following years. Standard errors are clustered at the level of the 2010 employer (Panels a-b) or the $t^* - 1$ employer (Panels c-d).

Tables

Table 1. The 2011 Secession of Mass Retailers from their CBA: Descriptive Statistics of Workers

	Full sample		Matched sample	
	(1) Treated	(2) Controls	(3) Treated	(4) Controls
Woman	0.60	0.47	0.59	0.58
	(0.49)	(0.50)	(0.49)	(0.49)
Age	38.18	39.35	38.94	38.81
	(8.48)	(9.09)	(8.33)	(8.39)
Full time	0.58	0.82	0.63	0.63
	(0.49)	(0.38)	(0.48)	(0.48)
Temporary contract	0.01	0.02	0.01	0.01
	(0.09)	(0.12)	(0.09)	(0.09)
Log weekly wage	6.19	6.25	6.21	6.21
	(0.31)	(0.38)	(0.31)	(0.35)
Blue collar	0.07	0.35	0.08	0.08
	(0.26)	(0.48)	(0.26)	(0.27)
White collar	0.90	0.61	0.90	0.89
	(0.29)	(0.49)	(0.3)	(0.31)
Mid manager	0.16	0.20	0.16	0.16
	(0.16)	(0.20)	(0.16)	(0.16)
Firm size	3,292	62	3,292	69
	(4,516)	(204)	(4,516)	(227)
South	0.15	0.18	0.15	0.14
	(0.36)	(0.38)	(0.36)	(0.35)
N. observations	102,911	419,448	91,753	91,753

Notes: 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. Standard deviations are reported in parentheses. Statistics are reported separately before (Columns (1)-(2)) and after (Columns (3)-(4)) matching. Firm size is firm-weighted. Log weekly wages are expressed in log euros.

Table 2. Pirate Agreement Adoptions: Descriptive Statistics of Workers

	Full sample		Matched sample	
	(1) Treated	(2) Controls	(3) Treated	(4) Controls
Woman	0.51	0.40	0.53	0.53
	(0.50)	(0.49)	(0.50)	(0.50)
Age	41.38	41.93	41.51	41.48
	(9.47)	(9.62)	(9.33)	(9.58)
Full-time	0.68	0.79	0.68	0.67
	(0.47)	(0.40)	(0.47)	(0.47)
Temporary contract	0.07	0.07	0.05	0.06
	(0.26)	(0.26)	(0.23)	(0.23)
Log weekly wage	6.00	6.13	6.01	6.00
	(0.43)	(0.43)	(0.42)	(0.45)
Blue collar	0.50	0.57	0.48	0.48
	(0.50)	(0.49)	(0.50)	(0.50)
White collar	0.47	0.39	0.49	0.48
	(0.50)	(0.49)	(0.50)	(0.50)
Mid manager	0.03	0.04	0.03	0.04
	(0.16)	(0.20)	(0.18)	(0.19)
Firm size	299	10	179	176
	(3,294)	(163)	(547)	(1,046)
South	0.30	0.22	0.28	0.27
	(0.46)	(0.42)	(0.45)	(0.45)
N. observations	38,248	67,285,888	24,952	24,952

Notes: This table reports descriptive statistics averaged between 2008 and 2016 by group. Columns (1) and (2) refer to the sample before the matching. Column (1) reports the $t^* - 1$ descriptives for workers on a standard CBA that will experience a within-job transition to a pirate CBA at t^*; Column (2) reports descriptives for potential control workers, that is, workers never covered by a pirate CBA. Columns (3) and (4) show descriptives for the matched sample, obtained as described in Section 5.2. Standard deviations are reported in parentheses. Firm size is firm-weighted. Log weekly wages are expressed in log euros.

Table 3. The Effects of the 2011 Secession of Mass Retail Employers on Workers

	(1) Log Weekly Wages	(2) Employed	(3) Employed at 2010 Firm	(4) Earnings
On Impact (t^*)	-0.0136	0.0027	0.0298	-358.97
	(0.0058)**	(0.0043)	(0.0093)***	(180.90)**
Medium Run (t^*+2)	-0.0054	0.0262	0.0911	433.42
	(0.0081)	(0.0087)***	(0.0420)**	(296.56)
Long Run (t^*+5)	-0.0322	0.0430	0.1962	0.03
	(0.0101)***	(0.0138)***	(0.0439)***	(442.35)
N. observations	2,523,222	2,732,810	2,732,810	2,732,810
Mean Outcome	6.1363	0.9988	0.9321	22,283
Average of Pre-Event Coeffs	-0.0021	0.0010	0.0026	-493.36
	(0.0082)	(0.0004)***	(0.0188)	(207.92)**
p-value Pre-Event Coeffs = 0	0.8001	0.0087	0.8890	0.02

Notes: This table reports three event-study coefficients from Equation (1) estimated on the matched sample defined in Section 5.1. 'On Impact' denotes the coefficient estimated for the year 2011, 'Medium Run' corresponds to the coefficient for the year 2013, and 'Long Run' refers to the coefficient for the year 2016. 'Mean Outcome' represents the mean of the outcome of interest over the period 2005-2010, computed considering the control group only. The last rows show the mean event-study coefficient between 2005 and 2010 ('Average of Pre-Event Coeffs'), the standard error in parentheses, and the p-value of the test that the mean pre-event coefficient is equal to zero, all computed using the *lincom* command in stata. Log weekly wages are calculated for the dominant job, i.e., the job with higher weeks worked in a given year. Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Employed at 2010 firm is an indicator equal to 1 if a given worker in year t is employed by their 2010 employer. Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010-euros. Standard errors, clustered at the firm level of the 2010 employer, are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 4. The Effects of Pirate Agreement Adoptions on Workers

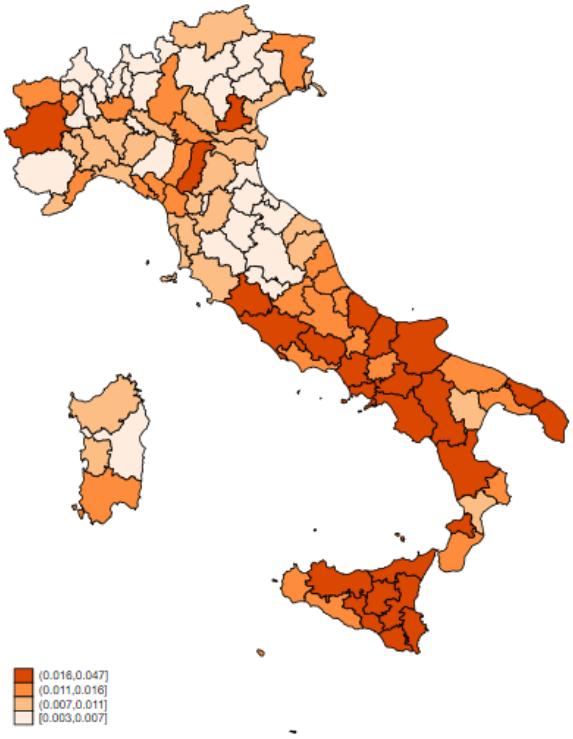
	(1) Log Weekly Wages	(2) Employed	(3) Employed at (t^*-1) Firm	(4) Earnings
On Impact (t^*)	-0.0202	0.0000	0.0000	-98.19
	(0.0041)***	(0.0003)	(0.0005)	(94.40)
Medium Run (t^*+2)	-0.0238	0.0189	0.0041	-121.73
	(0.0044)***	(0.0052)***	(0.0146)	(125.26)
Long Run (t^*+5)	-0.0248	0.0306	0.0262	194.57
	(0.0074)***	(0.0087)***	(0.0221)	(192.38)
N. observations	477,920	521,022	521,022	521,022
Mean Outcome	6.0011	0.9445	0.8062	18,561
Average of Pre-Event Coeffs	-0.0050	0.0045	0.0009	-165.89
	(0.0040)	(0.0028)	(0.0087)	(111.18)
p-value Pre-Event Coeffs = 0	0.2170	0.1061	0.9155	0.14

Notes: This table reports three event-study coefficients from Equation (2) estimated on the matched sample defined in Section 5.2. 'On Impact' denotes the coefficient estimated in the opt-out year t^*, 'Medium Run' corresponds to the coefficient two years after the opt-out, and 'Long Run' refers to the coefficient five years after the opt-out. 'Mean Outcome' represents the mean of the outcome of interest in the five years before the opt-out, computed considering the control group only. The last rows show the mean event-study coefficient in the five years before the opt-out ('Average of Pre-Event Coeffs'), the standard error in parentheses, and the p-value of the test that the mean pre-event coefficient is equal to zero, all computed using the *lincom* command in stata. Log weekly wages are calculated for the dominant job, i.e., the job with higher weeks worked in a given year. Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Employed at (t^*-1) firm is an indicator equal to 1 if a given worker in year t is employed by their t^*-1 employer. Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010-euros. Standard errors, clustered at the firm level of the t^*-1 employer, are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

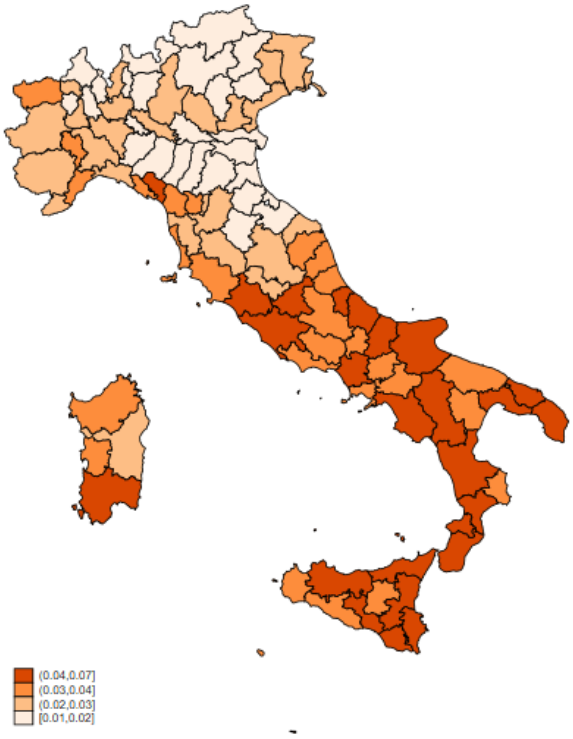
Appendix Figures

Appendix Figure A1. Geographical Distribution of Pirate Contracts, 2019

(a) Workers



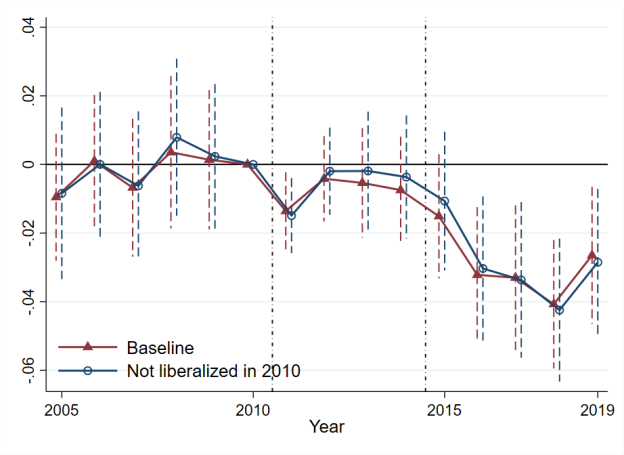
(b) Firms



Notes: For workers, the share of pirate contracts is computed as the total number of workers covered by a pirate contract 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 contract 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 A2. Robustness on liberalization of hours

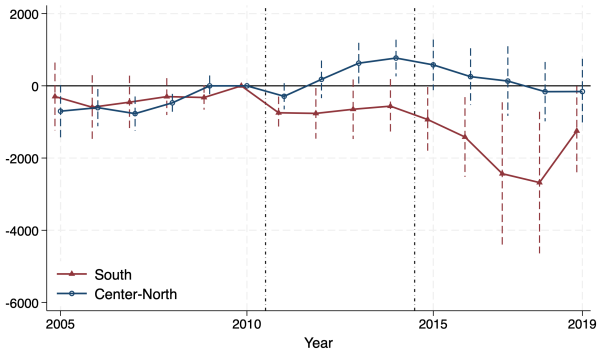
(a)



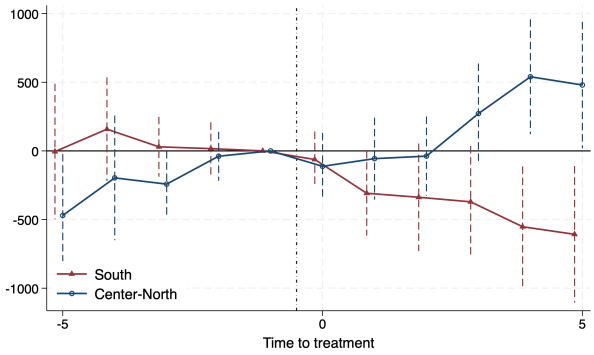
Notes: This figure displays the event-study coefficients from Equation (1) estimated on the matched sample defined in Section 5.1, using log weekly wages as outcome. In all charts, the red coefficients denote the baseline results. The blue event-study coefficients are obtained when excluding workers in municipalities where shop hours were not liberalized in 2010 (see text for details).

Appendix Figure A3. The Effects of Opt-Outs on Worker Earnings: Heterogeneity by Geography

(a) FD design

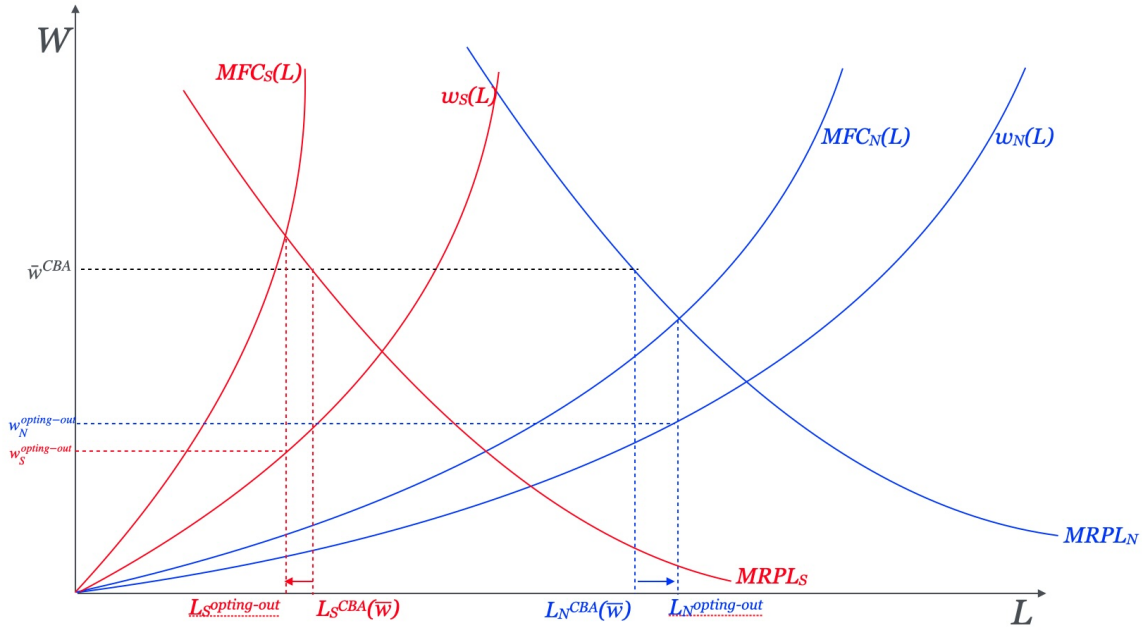


(b) Pirate CBAs design



Notes: This figure displays the event-study coefficients from Equation (1) estimated on the matched sample defined in Section 5.1 (Panel a) and from Equation (2) estimated on the matched sample defined in Section 5.2 (Panel b). The regressions are run separately for workers in the Center-North and South of Italy in 2010 (Panel a) or at time $t^* - 1$ (Panel b). Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily. Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Mean earnings for the control group in the pre-event years for FD design: €22,581 in the South, €22,230 in the Center-North; for Pirate CBAs design: €15,944 in the South, €19,520 in the Center-North. Standard errors are clustered at the level of the 2010 employer (Panel a) or the $t^* - 1$ employer (Panel b).

Appendix Figure A4. Opt-Outs with Heterogeneous Productivity and Labor Supply Elasticities



Notes: The figure highlights the predicted effects of an opt-out from a CBA in a simple monopsony model where there are two firms: (N, S). Firm N has higher productivity than firm S as captured by the rightward parallel shift in its marginal revenue product of labor (MRPL) curve while firm S has a less elastic labor supply curve (and thus more elastic inverse labor supply curve $w_S(L)$). In the status quo, both firms are mandated to pay a wage set by the national CBA, $\bar{w}^{CBA}$. The level of employment in the status quo is thus found at the intersection of $\bar{w}^{CBA}$ with labor demand (the MRPL curve). If both firms opt-out, then each employer becomes a wage setters. The level of employment chosen by the firm after the opting out event—$L_j^{opting-out}$— is found at the intersection of the MRPL curve with the marginal cost curve of the firm MFC_j and the wage is found on the inverse labor supply curve at the point $L_j^{opting-out}$. The more productive firm with a higher labor supply elasticities pays a lower wage but has higher employment level following the opting-out event. The less productive firm (but with a very inelastic labor supply curve) sets the wage even lower than the more productive firm and has lower employment compared to the status quo.

Appendix Tables

Appendix Table A1. Worker Characteristics by Adoption of Pirate Agreements

	(1) Pirate workers	(2) Other workers
Woman	0.47	0.41
	(0.50)	(0.49)
Age	39.67	39.46
	(10.70)	(10.89)
Full-time	0.67	0.74
	(0.47)	(0.44)
Temporary contract	0.18	0.19
	(0.38)	(0.39)
Log weekly wage	5.95	6.03
	(0.51)	(0.51)
Blue collar	0.50	0.56
	(0.50)	(0.50)
White collar	0.42	0.35
	(0.49)	(0.47)
Mid manager	0.03	0.03
	(0.18)	(0.17)
South	0.33	0.24
	(0.47)	(0.43)
N. Observations	1,402,379	120,028,877

Notes: This table reports descriptive statistics for all workers in the INPS data between 2008 and 2016. Column (1) refers to the sample of workers covered by a pirate CBA at any point in time in 2008-2016, column (2) to other workers (those not covered by a pirate CBA). All statistics are computed as averages between 2008 and 2016. Standard deviations are reported in parentheses. Log weekly wages are expressed in log euros. For further sample restrictions, see Section 5.2.

Appendix Table A2. Percent of Workers across Sectors by Adoption of Pirate Agreements

	(1) Pirate workers	(2) Other workers
Agriculture, Forestry and Fishing	2.11	0.63
Mining and Quarrying	0.01	0.35
Manufacturing	19.92	27.24
Electricity, Gas, Steam and Air Conditioning Supply	0.07	0.53
Water Supply, Sewerage, Waste Management	0.24	0.89
Construction	0.75	8.33
Wholesale and Retail Trade	22.29	15.16
Transportation and Storage	7.98	6.74
Accommodation and Food	1.89	9.59
Information and Communication	8.69	2.74
Finance and Insurance	0.54	3.94
Real Estate Activities	0.73	0.30
Professional, Scientific and Technical Activities	3.74	3.02
Administrative and Support Service Activities	12.11	9.95
Public Administration and Defence	0.67	0.57
Education	1.32	1.38
Human Health and Social Work	7.22	4.39
Arts, Entertainment and Recreation	1.64	0.58
Other Service Activities	7.63	3.34
Activities of Households as Employers	0.05	0.31
Activities of Extraterritorial Organisations	0.43	0.03

Notes: This table reports descriptive statistics for all workers in the INPS data between 2008 and 2016. Column (1) refers to the sample of workers covered by a pirate CBA at any point in time in those years, column (2) to other workers (those not covered by a pirate CBA). All statistics are computed as averages between 2008 and 2016. For further sample restrictions, see Section 5.2.

Appendix Table A3. Firm Characteristics by Adoption of Pirate Agreements

	(1) Pirate firms	(2) Other firms
Firm size	14.14	8.39
	(374.39)	(157.69)
South	0.15	0.08
	(0.36)	(0.27)
Firm age	10.53	13.06
	(10.9)	(12.2)
Share of full-time	0.50	0.60
	(0.43)	(0.43)
Share of temporary	0.15	0.15
	(0.30)	(0.29)
Share of women	0.59	0.47
	(0.41)	(0.42)
Share of blue collar	0.43	0.58
	(0.46)	(0.43)
Share of white collar	0.47	0.31
	(0.45)	(0.41)
Share of apprentices	0.08	0.10
	(.23)	(.24)
Share of mid-managers	0.004	0.005
	(0.05)	(0.05)
Share of managers	0.006	0.002
	(0.06)	(0.03)
Mean worker age	37.1	37.8
	(8.7)	(9.0)
Log weekly wage	5.85	5.85
	(0.39)	(0.38)
Log assets	5.89	6.50
	(1.66)	(1.63)
Log productivity	3.33	3.51
	(0.92)	(0.87)
Leverage	0.81	0.81
	(0.33)	(0.32)
N. Observations	190,873	14,151,780

Notes: This table reports descriptive statistics for all firms in the INPS data between 2008 and 2016. Column (1) refers to the sample of firms using a pirate agreement for at least one worker at any point in time between 2008 and 2016, column (2) to firms not using pirate agreements. All statistics are computed as averages between 2008 and 2016. Standard deviations are reported in parentheses. Log weekly wages are expressed in log euros. Productivity is computed as value added per worker. Leverage is computed as 1-equity/assets (for pirate firms, it is computed in the year before the adoption of the pirate CBA). For further sample restrictions, see Appendix B.

Appendix B

This appendix describes our firm-level analysis of the pirate CBAs.

Econometric analysis We employ a matched difference-in-differences strategy that compares treated firms—those that opt out—with a matched group of control firms that do not. We define the treatment year as the first year a firm uses a pirate CBA for at least one worker and, as in the worker-level design, focus on all firms for which the treatment year is included between 2008 and 2016, to have a sufficiently long number of years before and after the event, as our data span 2005 to 2019. We then run the following event-study regression:

$$y_{jt} = \alpha_j + \delta_t + \sum_{k=a}^{b} \gamma_k \cdot \mathbb{1}[t = t_j^* + k] + \sum_{k=a}^{b} \beta_k \cdot \mathbb{1}[t = t_j^* + k] \cdot T_j + v_{jt}, \tag{B1}$$

where y_{jt} is the outcome of interest for firm j in year t, α_j and δ_t are firm and year dummies and γ_k captures the number of periods relative to the event year, t_j^* (that is, the year of the opt-out).[33] T_j is a treatment indicator 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, its survival probability (a dummy taking value of one if the firm is observed in the data and zero otherwise) and the (log) number of employees.

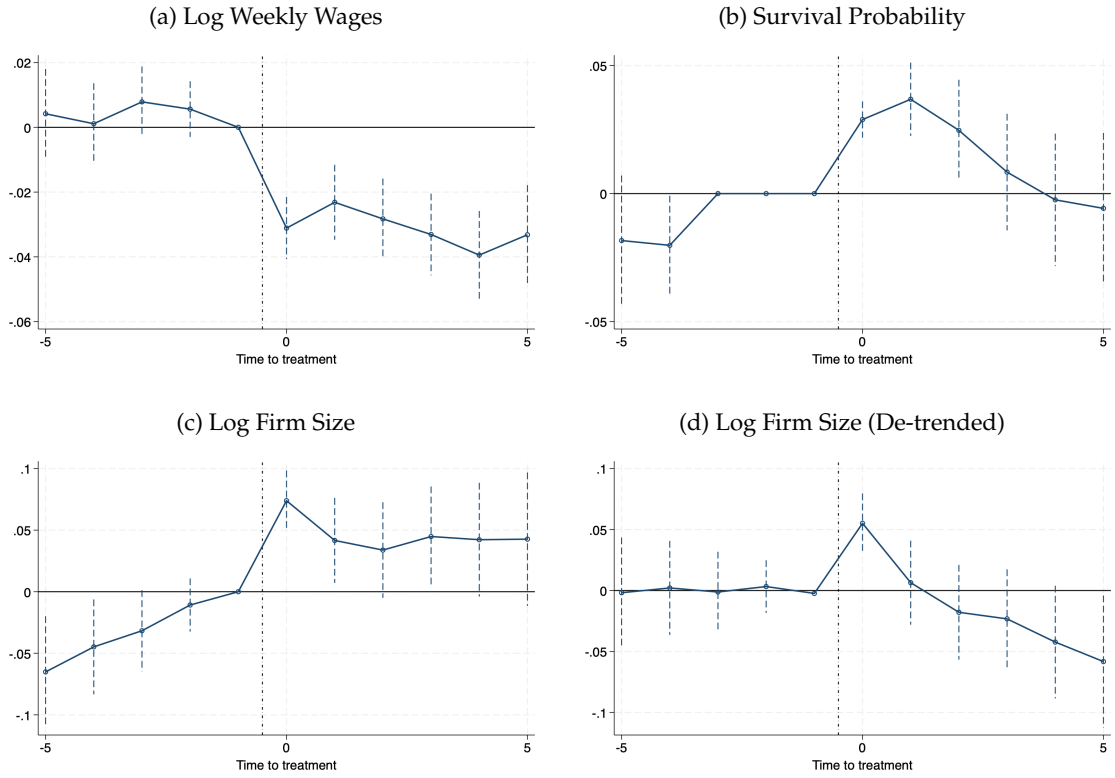
Matching Strategy and Sample We implement a matching algorithm that assigns each treated firm to a control one with similar characteristics prior to the opt-out. Potential control firms are all those that never applied a pirate contract.[34] The propensity score model controls for the firm's average wage paid in the three years before the opt-out, firm size, sector, productivity, sales growth, profits-to-assets ratio and financial leverage. The matching procedure delivers a sample of 2,144 treated firms and an equal number of control firms.[35]

[33] 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 matching algorithm that we implement.

[34] We drop firm in small sectors such as agriculture, public administration, activities of households as employer and extraterritorial organizations.

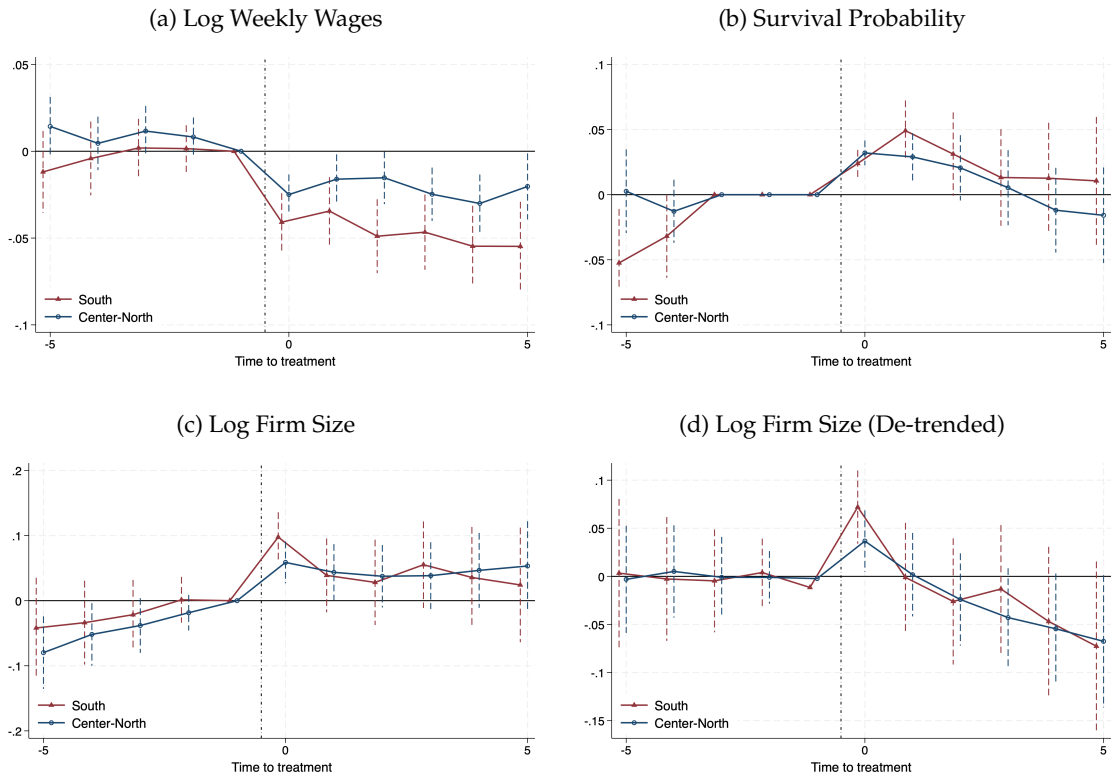
[35] Table B1 reports descriptive statistics.

Appendix Figure B1. The Effects of Pirate Agreement Adoptions on Firms



Notes: This figure displays the event-study coefficients from Equation (B1) estimated on the matched sample defined in Appendix B. Panel (a): Log weekly wages is the mean log weekly wage paid by firm j to its workers, expressed in euros; Panel (b): the outcome is a year-to-year binary indicator taking value of 1 if firm j is observed in the data; Panel (c): the outcome is the logarithm of the total number of the firm's employees; Panel (d) reproduces the coefficients of Panel (c) after subtracting their linear time trend estimated in the pre-event periods and extrapolated for the later years as in Dustmann, Lindner, Schönberg, Umkehrer and Vom Berge (2022). Standard errors are clustered at the firm level.

Appendix Figure B2. The Effects of Pirate Agreement Adoptions on Firms: Heterogeneity by Geography



Notes: This figure displays the event-study coefficients from Equation (B1) estimated on the matched sample defined in Appendix B. The regressions are run separately for workers in the Center-North and South of Italy at time $t^* - 1$. Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily. Panel (a): Log weekly wages is the mean log weekly wage paid by firm j to its workers, expressed in euros; Panel (b): the outcome is a year-to-year binary indicator taking value of 1 if firm j is observed in the data; Panel (c): the outcome is the logarithm of the total number of the firm's employees; Panel (d) reproduces the coefficients of Panel (c) after subtracting their linear time trend estimated in the pre-event periods and extrapolated for the later years as in Dustmann, Lindner, Schönberg, Umkehrer and Vom Berge (2022). Standard errors are clustered at the firm level.

Appendix Table B1. Pirate Agreement Adoptions: Descriptive Statistics of Firms

	Full sample		Matched sample	
	(1) Treated	(2) Controls	(3) Treated	(4) Controls
Firm size	28.18	8.13	118.61	29.92
	(370.35)	(143.45)	(865.14)	(156.48)
Firm age	10.39	13.07	14.19	14.60
	(11.6)	(12.2)	(11.54)	(11.32)
Share of full-time	0.53	0.60	0.66	0.68
	(0.42)	(0.43)	(0.34)	(0.34)
Share of temporary	0.19	0.15	0.18	0.16
	(0.32)	(0.29)	(0.24)	(0.24)
Share of women	0.55	0.47	0.49	0.48
	(0.40)	(0.42)	(0.33)	(0.36)
Share of blue collar	0.47	0.58	0.49	0.49
	(0.45)	(0.43)	(0.40)	(0.39)
Share of white collar	0.44	0.31	0.44	0.42
	(0.45)	(0.41)	(0.38)	(0.38)
Share of apprentice	0.07	0.10	0.06	0.05
	(0.22)	(0.25)	(0.13)	(0.14)
Share of mid-managers	0.006	0.005	0.01	0.01
	(0.05)	(0.05)	(0.05)	(0.03)
Share of managers	.006	.002	.005	.004
	(0.06)	(0.03)	(0.03)	(0.03)
Mean worker age	37.4	37.8	38.6	38.5
	(8.3)	(9.0)	(5.6)	(5.8)
Log weekly wage	5.84	5.85	5.92	5.92
	(0.40)	(0.38)	(0.32)	(0.32)
Log assets	6.21	6.50	6.94	6.64
	(1.85)	(1.62)	(1.78)	(1.52)
Log productivity	3.32	3.52	3.37	3.36
	(1.01)	(.87)	(.82)	(.82)
Leverage	0.83	0.81	0.80	0.81
	(0.30)	(0.33)	(0.27)	(0.28)
N. Observations	14,128	13,930,620	2,144	2,144

Notes: This table reports descriptive statistics averaged between 2008 and 2016 by group. Columns (1) and (2) refer to the firm sample before the matching. In particular, Column (1) reports $t^* - 1$ descriptives for firms that will transition to a pirate CBA at t^*; Column (2) reports descriptives for potential control firms, that is, firms that have never used a pirate CBA. Columns (3) and (4) show descriptives for the matched sample, obtained as described in Appendix B. Standard deviations are reported in parentheses. Log weekly wages are expressed in log euros. Productivity is computed as value added per worker. Leverage is computed as 1-equity/assets (for pirate firms, it is computed in the year before the adoption of the pirate CBA).

Appendix Table B2. The Effects of Pirate Agreement Adoptions on Firms

	(1) Log Weekly Wages	(2) Survival Probability
On Impact	-0.031	0.029
	(0.005)***	(0.004)***
Medium Run	-0.028	0.025
	(0.006)***	(0.010)**
Long Run	-0.033	-0.006
	(0.009)***	(0.015)
N	39,938	44,216
Mean outcome	5.92	0.94
Average of Pre-Event Coeffs	0.005	-0.010
	(0.005)	(0.005)*
p-value Pre-Event Coeffs = 0	0.326	0.067

Notes: This table reports three event-study coefficients from Equation (B1) estimated on the matched sample defined in Appendix B. 'On Impact' denotes the coefficient estimated in the opt-out year t^*, 'Medium Run' corresponds to the coefficient two years after the opt-out, and 'Long Run' refers to the coefficient five years after the opt-out. 'Mean Outcome' represents the mean of the outcome of interest in the five years before the opt-out, computed considering the control group only. The last rows show the mean event-study coefficient in the five years before the opt-out ('Average of Pre-Event Coeffs'), the standard error in parentheses, and the p-value of the test that the mean pre-event coefficient is equal to zero. Log weekly wages is the mean log weekly wage paid by firm j to its workers, expressed in euros. Survival probability is a year-to-year binary indicator taking value of 1 if firm j is observed in the data. Standard errors are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.