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Collective bargaining and monopsony: The regulation of noncompete agreements in France

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Abstract
Can collective bargaining mitigate monopsony power? This paper addresses this question by examining how the regulation of noncompete agreements for employees by collective agreements affects firm-level markdowns in the French manufacturing sector. Using a staggered difference-in-differences approach, we find that the regulation of noncompetes set by collective agreements leads to a 1.3%–2.2% reduction in markdowns on average. The effect grows over time and is more pronounced for smaller, less productive firms that pay lower wages. Studying a landmark decision of the French Supreme Court that introduced the obligation to have a compensation to consider a noncompete enforceable, we find a significant complementarity between the regulation of noncompetes at the national level (e.g., via case law) and sectoral collective bargaining. By enhancing compliance or imposing further restrictions, collective bargaining can therefore serve as an effective tool to regulate the use of noncompete agreements.

Keywords: monopsony, unions, noncompetes
JEL codes: J42; J51; J53; J58; J31

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1 Introduction

Growing evidence on the spread of noncompete agreements in the United States, as well as numerous regulatory initiatives at the state and federal level (Starr, 2021 among many others), have sparked a global debate on the optimal regulation of noncompete agreements. While the legitimacy of such restrictive covenants has traditionally been justified on the grounds that they protect legitimate business interests (e.g., trade secrets or specific investments in the workforce), there are now robust elements to argue that they also reduce job mobility, wages, and employees' bargaining power, ultimately stifling competition in labor and product markets. It is based on evidence of this kind that the U.S. Federal Trade Commission has proposed to ban noncompetes altogether (Starr 2023).

Evidence outside the United States remains scattered, but the significant use of such clauses in countries as diverse as Australia, Italy, Japan and the Nordic countries, suggest that their diffusion is not limited to flexible labor markets with high turnover and limited role of unions and collective bargaining (Andrews and Garnero 2025). Not only these labor markets are less fluid than the US one, but also unions and collective bargaining play a stronger role in wage setting and in promoting pro-labor legislation—in particular, in Italy and in the Nordic countries. By enforcing national regulation or expanding the scope of restrictive rules, sectoral collective bargaining might limit the use of noncompetes. Through industry-level collective bargaining, firms can internalize the costs of mobility restrictions in terms of misallocation of labor. Plant-level collective bargaining should strengthen enforcement on the ground—especially when decentralized bargaining is as widespread as in the Nordic countries. The increased bargaining power of workers can also ensure that employers do not unduly limit mobility, allowing wages to fall below the value of marginal productivity. At the same time, collective bargaining forces (or at least incentivizes) coordination between employers sitting at the same bargaining table, which may lead to mutual assistance in enforcing noncompete obligations while facilitating collusive behaviors, such as wage fixing and informal no-poaching agreements.

This paper is the first to examine the role of collective bargaining in the regulation of noncompete agreements and their impact on monopsony power. To conduct the analysis, we first analyze the content of about a thousand of collective agreements in France between 1950 and 2024 to build a novel sector-level dataset that contains information on whether and to what extent collective agreements regulate noncompetes, with detailed information on the specific content of such provisions. An increasing number of French collective agreements contain some provisions on noncompetes, some simply mentioning the issue, some providing guidance on their use, and others setting additional requirements beyond what is foreseen in the existing body of case law. Second, we link this information to firm-level balance sheet data for 1996-2023 from Orbis Historical, a comprehensive dataset of private and listed companies around the world, that is particularly well suited for firm-level analyses in France, and allows us to estimate firm-level markdowns (i.e. the gap between the wage and the marginal revenue product of labor).

We then analyze the relationship between the stringency of the regulation of noncompete agreements in collective agreements and markdowns in the French manufacturing sector by estimating a staggered event study model based on the date when the provision regulating noncompetes was added to the collective agreement applying to each firm in the industry. In these estimates we use some of the most recent approaches proposed in the literature (Gardner 2022 and Sun and Abraham 2021).

In the absence of an unambiguous identification strategy, there are a number of potential endogeneity threats to identification (e.g. industry dynamics correlated with the treatment, heterogeneity in union strength, firm influence on collective agreements, etc.) and issues arising from sample selection biases. We account for these confounding effects using a shift-share instrumental variable approach that leverages union density in other European countries to proxy for workers' bargaining power and inverse probability weighting, together with several other robustness tests.

While Boeri et al. (2024) have shown that collective bargaining does not play a role in the regulation of noncompetes in Italy, we show that it does in France. Our baseline results show that markdowns are 1.3%-2.2% lower in sectors covered by a collective agreement regulating the use of noncompetes beyond case law than in sectors where collective agreements do not cover noncompetes. The dynamic analysis shows that it takes several years for the regulation to have some effect. Moreover, the thrust of the baseline results remain largely unchanged when we adopt the shift-share and inverse probability weighting specifications.

When comparing the results across firms with different characteristics, we see that the reduction in the markdown is more pronounced for smaller, less productive firms that pay lower wages. These are the firms that are more likely to employ low-skilled workers who may have less knowledge of the legal restrictions to the use of noncompetes, and hence are more likely to be bound by “unfair” noncompete agreements (Boeri et al. 2024).

We also examine the impact of a landmark decision by the French Court of Cassation in 2002, which imposed an obligation to provide a financial compensation to employees subject to a noncompete agreement. We first analyze the impact of this decision by comparing the reduction in markdowns in firms before and after the decision, and contrast the differences in the behavior of firms in sectors where the collective agreement regulated noncompetes but did not provide for compensation prior to the 2002 court decision (the treated firms) with firms in sectors where compensation was already provided for (the control firms). We then compare this first result with what is obtained when the treatment sample is extended to all firms, i.e. including firms operating in sectors where the collective agreement did not regulate noncompetes. The results indicate that the court decision only led to a reduction in markdowns in sectors where noncompete agreements were already regulated by collective agreements to some extent. This complementarity suggests that regulation at the national level—set by case law, in the case of France—is more effective when it builds on an existing regulatory framework (and enforcement mechanism) established by collective agreements, which can ensure compliance with the law as well as further restrict the use of noncompetes.

This paper contributes to three strands of the literature. Prior research shows that the use of noncompete agreements is not limited to high-skilled workers (Starr et al. 2021; Johnson and Lipsitz 2022; Alves et al. 2024; Boeri et al. 2024) and that these agreements are also negatively associated with wages and job mobility (Garmaise 2011; Marx et al. 2009; Starr et al. 2021; and US Treasury 2016, 2022). Furthermore, firms often include noncompetes in individual contracts even when they are unenforceable, resulting in negative effects on worker mobility (Starr et al. 2020; Prescott and Starr 2023; Boeri et al. 2024) and that workers frequently do not negotiate noncompete agreements (Starr et al. 2021). We contribute to this growing body of evidence by introducing the collective bargaining dimension and investigating its effects at the firm level, for the first time.

Second, we contribute to the literature on the factors influencing firm-level monopsony power by highlighting the role of noncompete agreements. Díez et al. (2022) and Yeh et al. (2022) identify a robust association between markdowns and firm size. Consistent with the present analysis, Crescioli and Martelli

(2023) emphasize the importance of labor market institutions as mediators of the evolution of monopsony power. Alongside Bartelsman et al. (2024), our study is the first to examine the link between noncompetes and firm-level markdowns. While Bartelsman et al. (2024) focuses on how lifting noncompetes for temporary workers can enhance their wages and mobility, we study the direct effects of noncompetes on markdowns, while exploring the role of unions in these dynamics.

Third, we contribute to the so far rather scant literature (e.g., Ash et al. 2019) that looks into the blackbox of collective bargaining analyzing in detail the actual content and scope of collective agreements. The literature looking into the actual content of contractual arrangements has so far been mostly concentrated on franchising (Blair and Lafontaine 2005), mergers and acquisitions (Talley and O'Kane 2012), regulated industries (Moszoro et al. 2013) or contracts deposited at SEC (Sanga 2018). In this context, we also develop a new database on the regulation of noncompetes within collective agreements providing information on the specific content of such regulations, which may be useful for further research on the role of bargaining in the enforcement of noncompetes.

The rest of the paper is organized as follows. Section 2 illustrate the French institutional context. Section 3 describes the data sources while section 4 the empirical strategy. The results are discussed in section 5. Finally, section 6 concludes.

2 Institutional Context

Differently from other European countries but like in the United States, no legislation regulates the use of noncompete agreements in employment contracts in France. In the absence of a specific law, case law has played an important role in defining the legal requirements for the enforceability of such restrictive covenants.

At first, courts adopted a lenient approach in assessing the reasonableness of the restriction. Except when freedom of work was excessively limited (in terms of time, space, and the nature of the activity concerned), courts were inclined to recognize the legitimacy of noncompetes by default—for instance, the presence of a legitimate business interest was deemed mandatory only if specified by the applicable collective agreement (Auzero et al. 2023; e.g., French Court of Cassation, 13 October 1988).

In the early 1990s, courts started adopting a more stringent standard—e.g., French Court of Cassation, 14 May 1992, imposing the presence of a legitimate business interest—which culminated in a landmark decision delivered on July 10, 2002. The Supreme Court for the first time established the following cumulative conditions for the validity of a noncompete: *(i)* a noncompete must be essential to protect employer's legitimate interests; *(ii)* the restriction must be limited in time and space, and account for the nature of the employee's position; *(iii)* the employer is obliged to provide the employee with financial compensation. This last point was in contrast with earlier decisions which held that a financial compensation was required *only if* stipulated by the collective agreement (for example, French Court of Cassation, 9 October 1985: "given that the validity of a noncompete clause is not contingent on the granting of financial compensation to the employee if such compensation is not provided for by a collective agreement", and Court of Cassation, 11 October 1990: "unless otherwise provided by a collective agreement or accord, no financial compensation is required").

While we exploit this discontinuity in the regulation of noncompetes later in Section 5.2.5, we generally interpret the various references to collective agreements role by courts as indicative of their role in defining certain limitations to the use of noncompetes.

3 Data

3.1 French Collective Bargaining Data

We gather information on collective agreements from *Légifrance*. Légifrance is an official French government website that provides public access to various legal sources, including legislation, case law, and collective agreements. It serves as a comprehensive legal resource, offering up-to-date information on French law, European Union law, and relevant international legal agreements.

We collect data on noncompetes by scraping a comprehensive set of sectoral collective bargaining agreements, focusing specifically on those that include references to noncompetes. We first categorize the agreements into two groups: collective agreements that define specific criteria and requirements for the use of noncompetes, and those that merely mention such agreements, without offering any further guidance. Next, we further divide the collective agreements that define specific criteria and requirements for the use of noncompetes into two subcategories, based on whether or not they provide a regulation that goes beyond the minimum requirements established by courts—in other words, we code collective agreements providing an effective regulation as those that define at least one of the key criteria outlined in Section 2, meaning that they define a specific maximum duration for the restriction, and/or a geographical/sector scope, and/or a minimum amount of compensation that the employer must correspond to the employee. In contrast, the residual category includes collective agreements that define specific criteria and requirements for the use of noncompetes but do not introduce meaningful limitations to judge-made rules.

Table 1: Categorization of collective agreements depending on the regulation of noncompetes

Category	**Description**
1 - Only mention	Only mention noncompetes
2 - Equate the national regulation	Offer guidance without additional requirements beyond case law
3 - Add to the national regulation	Set additional requirements beyond case law

Our empirical strategy focuses on collective agreements that define specific criteria and requirements for the use of noncompetes, beyond what is established by case law (i.e., category 3). In other words, we discard those that merely mention noncompetes and those that do not go beyond the minimum requirements already set (i.e., categories 1 and 2).[1] For these agreements, we identify the year when the use of noncompetes was regulated for the first time—note that this year may differ from when the collective agreement was first stipulated, as the regulation of noncompetes may have been the result of a later revision. Additionally, we are able to determine the categories of workers to whom these changes apply. Since our firm-level data do not allow us to differentiate between types of employees, we exclude changes in the regulation of noncompetes that apply solely to executives—these collective agreements cover only a small fraction of the workforce, thus reducing the likelihood of aggregate effects.

For many of these agreements, Légifrance provides the industry code according to the French industry classification (NAF code), and we identified the industry codes not directly provided by Légifrance by manually reviewing the collective agreements. The NAF classification corresponds exactly to the NACE classification up to four digits, which represents the highest level of granularity in the NACE system. However, in some cases, the NAF codes can be more detailed than the NACE classification. Since our firm-level

[1]Agreements that merely mention noncompetes and those that do not go beyond the minimum requirements are used only for robustness checks.

data follows the NACE system, when NAF codes are more granular than NACE, we assign the most detailed corresponding NACE industry classification.

In summary, we construct a novel sector-level dataset that includes, for each sector, information on whether and to what extent collective agreements regulate noncompetes, with detailed information about the specific content of such regulations.

3.2 Firm-Level Data

We obtain data on French firms from Orbis Historical, a comprehensive dataset that includes balance sheet information for private and publicly listed companies across the globe. Orbis Historical allows to track firm-level financial statements over time, providing detailed information about the evolution of companies. The data in Orbis are sourced from national business registers, which operate under the legal and administrative filing requirements of the respective country. In countries like France, where firms are legally required to submit financial statements to national registries, Orbis uses the same data sources as national statistical offices, ensuring a high degree of accuracy and completeness (Kalemli-Ozcan et al., 2024).

The historical version of Orbis was developed following the methodology established by Kalemli-Ozcan et al. (2024), which aimed to improve the representativeness of earlier versions of the dataset. This methodology adjusts for common biases in the original data collection process, such as non-reporting, and enhances the temporal consistency of firms' records. Although Orbis does not cover the entire firm population, as census data would, the version processed with the Kalemli-Ozcan et al. (2024) approach provides extensive coverage for European countries, particularly for France.

As shown in Table D.1.1 of Kalemli-Ozcan et al. (2024), Orbis Historical captures an average of 78% of the gross output of the Eurostat Structural Business Statistics (SBS) for French firms between 1999 and 2012.

3.3 Union Membership Data

We use the European Social Survey (ESS) to obtain data on union membership at the NACE two-digit industry level, which represents the highest level of granularity available in the survey. The ESS is a biennial, cross-national survey designed to measure attitudes, beliefs, and behaviors across European countries. The survey employs a probability-based sampling design to ensure national representativeness. Surveys are conducted through face-to-face interviews with individuals aged 15 and older, residing in private households. To enhance comparability across countries, the ESS uses standardized questionnaires, translated and pretested to maintain linguistic and cultural validity. Data collection adheres to strict guidelines, including the use of trained interviewers and detailed fieldwork protocols.

4 Empirical Strategy

We conduct our estimation using a novel dataset that merges firm-level data with information from collective bargaining agreements at the NACE four-digit level, covering the period from 1996 to 2023. Our analysis focuses exclusively on the manufacturing sector, as this sector is particularly suited for estimating markdowns, following the methodology outlined in the next subsection which relies on the use of material expenditures (Tortarolo and Zarate, 2018; Morlacco, 2019; Yeh et al., 2022).

4.1 Monopsony Estimation

The markdown is defined as follows:

$$\nu = \frac{MRPL}{w}, \tag{1}$$

where $MRPL$ is the marginal revenue product of labor and w is the wage. When a firm holds monopsony power, this ratio exceeds one, indicating that the firm pays workers a wage below their marginal revenue product.

Unfortunately the marginal revenue product of labor is not observed. Yet we know from balance sheet data firm-level revenues, the structure of costs, and the wage bill. We can then apply the approach of Yeh et al. (2022). in estimating markdown in the US manufacturing sector. The methodology outlined in Yeh et al. (2022) builds upon De Loecker and Warzynski (2012), which itself is based on production function estimation methods from empirical industrial organization (Olley and Pakes 1996; Levinsohn and Petrin

2003).

Yeh et al. (2022) rely on the estimation of an industry-level gross output production function. We estimate this production function at the NACE 2-digit level, using data on revenues, number of employees, material expenses, and fixed assets. Nominal variables are deflated using the GDP deflator. While it would be preferable to use industry- and variable-specific deflators, such data is only available up to 2019, making it impossible to assess the effects of collective agreements changes from 2020 to 2022.

We follow Ackerberg et al. (2015) and we consider labor as a state variable and not as a variable input of production. In practical terms, this means that the firm chooses the labor input one period in advance. This assumption is legitimatized by the rather strict employment protection regulations present in France, which make labor a quasi-fixed input in the short-run.

We specialize the production function as a translog. Compared to the Cobb-Douglas specification, the translog allows for firm-level variations in elasticities and more flexible interactions between inputs. In estimating the translog production function, we follow the literature (De Loecker and Warzynski 2012; De Loecker et al. 2016; Morlacco 2019) and incorporate controls for market share, year, industry (NACE 3-digit), and location (NUTS 3-digit) fixed effects. Additionally, as suggested by Morlacco (2019), we account for input price bias and buyer power using buyer shares for materials.

The key result of Yeh et al. (2022) is that firm-level markdowns can be expressed as:

$$\nu = \frac{\theta^L}{\alpha^L}\mu^{-1}, \tag{2}$$

where θ^L, α^L, and μ^{-1} are the firm's output elasticity of labor, the employees cost share of revenues, and the markup. The markup is obtained by dividing the elasticity to output of materials by the share of material cost in revenues. While revenues cost shares can be easily obtained from our firm-level data, elasticities require the estimation of production functions.

4.2 Baseline Empirical Strategy

Our purpose is to identify the effects of collective bargaining on markdowns. To this end we exploit the staggered nature of collective contracts (see figure 1a). In particular, we estimate the effect of the regulation of noncompetes in collective bargaining agreements on markdowns using the following staggered event-study model:

$$\log(\nu_{jit}) = \sum_{k=-9,\ k\neq -1}^{9} \beta^v T_{it}^t \times R_{it} + X'jt\gamma + \theta \log(\bar{\nu}it) + \alpha_j + \tau_t + \epsilon_{jit}, \quad (3)$$

where j represents the firm, i the industry (NACE 4-digit), and t the year. The main independent variable is R_{it}, a dummy that takes the value of one after the introduction for the first time in a collective contract of a clause regulating noncompetes beyond what is established by case law. We interact this variable with dummies for pre- and post-treatment periods, spanning from -9 to 9 (9 years before and after the collective agreement), with period -1 serving as the reference period. Observations more than nine years before or after the treatment are grouped into the dummies for periods -9 and 9, respectively.

We include a set of firm-level controls to account for size (log of employees), productivity (log of total factor productivity), and product market power (log of markup). Additionally, we control for industry-wide trends in monopsony power using the log of the average industry markdown. Firm fixed effects and time fixed effects are included to control for time-invariant characteristics, such as location, and shocks that affect all firms equally.

We implement several data cleaning steps before estimating the model. First, following common practice in the literature, we trim the top and bottom three percent of the markdown and markup distributions to mitigate the influence of outliers (De Loecker et al. 2016, 2020; Morlacco 2019). Second, we consider only firms in the treatment group that are observed at least one period before and after receiving the treatment. Third, we exclude firms in industries where collective agreements do not introduce additional regulations beyond the minimum requirements established by courts (i.e., we exclude category 1 and 2 of table 1), and firms in industries treated in years outside our sample (i.e., before 1996 and after 2023). Consequently, our control group consists of firms in industries where collective bargaining agreements never regulate the use of noncompetes.

Staggered event study designs may produce biased coefficients in the presence of heterogeneous cohort and time effects (De Chaisemartin et al. 2020; Callaway and Sant'Anna 2021). For this reason, in addition to traditional two-way fixed-effects estimates, we employ two alternative approaches to account for these potential sources of bias. The first approach is Gardner's two-stage difference-in-differences (DID) method (2022). In the first stage of Gardner's method, we perform a regression of the outcome variables on the control variables, including cohort and year fixed effects, within the sample of non-treated units. In the second stage, we run a regression of the modified outcome on the treatment variable. The second alternative approach follows Sun and Abraham (2021), whose estimator is robust to biases caused by contamination effects across different pre- and post-treatment periods.

4.3 Shift-Share Strategy

A potential threat to our identification strategy and a source of endogeneity of the treatment is the presence of unobservable industry-specific factors that correlate with the regulation of noncompetes in collective bargaining agreements and monopsony power. Examples of such factors include the self-selection of firms into industries based on union strength, powerful firms capable of influencing regulations, and industry-specific demand fluctuations.

To mitigate these concerns, we use a shift-share instrument based on industry union density in other European countries. The logic behind our instrument is twofold. First, stronger unions are more likely to negotiate collective bargaining agreements that regulate noncompetes. Second, the direct use of French union density is not feasible, as it is subject to the same endogeneity problems previously described. We are aware that union density is an imperfect measure of the influence of unions in countries where the coverage of collective bargaining is significantly larger than union density, that is, there is a large *excess coverage* of collective bargaining. In future work we plan to use as instrument union density in Anglo-Saxon countries where there are no administrative extensions of collective bargaining, and hence union density provides a better measure of the influence of unions.

We compute union density at the country-NACE two-digit industry level using ESS data as the proportion of workers who reported being members of unions within a given industry. We then average union density across three different groups of countries, which are selected based on data availability. The country groups are: Western European countries (Austria, Belgium, Germany, Ireland, and the Netherlands), Northern European countries (Denmark and Sweden), and Southern European countries (Greece, Italy, Portugal, and Spain). Averaging across country groups, rather than using single-country instruments, enhances the reliability of the instruments given the relatively limited country samples in ESS. In fact, while the ESS provides a representative sample at the national level, its coverage at the industry level can be less precise. Additionally, we use different country groups rather than a single instrument averaging across all countries to allow for testing over-identified restrictions. Since collective bargaining agreement regulations vary at the NACE four-digit level, we weight the union density of each country group g ($ud_{gi_{2-digit}t}$) using France's pre-2002 average employment share of the four-digit industry within the two-digit industry (w_i).[2] Formally, our weighted union density instruments are:

$$wud_{git} = ud_{gi_{2-digit}t} \cdot w_i, \text{ where } w_i = \frac{\sum_{t=1996}^{2001} \frac{employment_{it}^{France}}{employment_{i_{2-digit}t}^{France}}}{6} \tag{4}$$

4.4 Inverse Probability Weighting

Given the non-random nature of the treatment, our specification may suffer from selection bias and imbalances between the treatment and control groups, which could potentially obscure our results. To address these concerns, we implement an inverse probability weighting approach. Inverse probability weighting operates in two steps. In the first step, we estimate the probability of receiving the treatment (i.e., the propensity score) by running a logit regression of the treatment variable on a set of controls that may influence treatment assignment. The propensity scores are then used to define the following weights:

$$w_j = \frac{T_j}{e(x_j)} + \frac{1 - T_j}{1 - e(x_j)},$$

where T_j represents the treatment status of unit j, and $e(x_j)$ is the propensity score. The purpose of these weights is to make the treatment and control groups more comparable by assigning greater weight to control units with a high propensity score and treatment units with a low propensity score. In the second step, we run a weighted regression of the outcome variable on the treatment.

Applying inverse probability weighting in our setting presents two challenges: the treatment is assigned

[2] We compute time-fixed employment weights using pre-2002 data because 2002 is the first year of the ESS survey. The aim is to ensure that variations in union density are attributable to changes in union membership rather than fluctuations in employment shares. Ideally, pre-sample weights would have been calculated for a period prior to 1996. However, the limitations of Orbis data coverage prevent this.

at the industry level, and it follows a staggered timing. Since treatment is assigned at the industry level, it is crucial to define a treatment model that adequately captures the factors influencing unions' decisions to regulate noncompetes. To address this, we estimate the following model at the four-digit NACE level:

$$R_i = \bar{F}_i^{0'} \beta^F + \bar{M}_i^{0'} \beta^M + \bar{W}_i^{0'} \beta^W + \epsilon_i.$$

The set F includes the log of the average total factor productivity, the average markup, and the total number of employees. These variables account for factors that may influence unions' decisions to intervene in a sector, such as productivity, industry size, and the extent of surplus available for distribution (i.e., the markup). Additionally, since these variables are aggregates of the firm-level controls used in the main analysis, their inclusion helps reduce imbalances between the treatment and control groups. The vector M includes aggregate monopsony indicators, such as the log of the average industry markdown and the average local labor market employment HHI, as unions may be more inclined to intervene in industries with greater monopsony power. Finally, W contains aggregates characterizing working conditions in the industry, including the log of the average net hourly wage and the average wage bill per worker. Trade unions could be more motivated to regulate noncompetes in sectors with poorer working conditions. The bar and superscript zero denote that these variables are pre-treatment averages.

Although necessary for the technique, the use of pre-treatment variables poses some challenges due to the staggered nature of the treatment. Specifically, it is unclear how to define the pre-treatment period for untreated observations. To address this issue, we assign pre- and post-treatment status for control units as follows. For each three-digit NACE industry, we identify the year in which the first sectoral collective bargaining agreement regulating noncompetes was introduced. Untreated four-digit NACE industries are considered to be in their pre-treatment period for any years before this specified date.

After estimating the treatment model in (4.4), we use the resulting propensity scores to define inverse probability weights for each four-digit NACE industry. We then run a weighted version of model (3), where firms within the same four-digit industry are assigned the same weight.

4.5 Summary Statistics

The summary statistics of the main variables in our final regression sample are presented in Table 2. There do not appear to be severe imbalances between the treatment and control groups in terms of the main control variables. However, in our analysis, we will apply techniques to address potential confounding effects due to differences in covariates between the control and treatment groups. Another important consideration is the presence of firm-level markdowns below unity. As shown by Dobbelaere and Mairesse (2013), markdowns below one are consistent with models of efficient bargaining, in which bargaining takes place not only over wages, but also employment. A significant number of firm-level markdowns below one are also found in the work of Dobbelaere and Mairesse (2013), Tortarolo and Zarate (2018), Morlacco (2019), and Bartelsman et al. (2024). Markdown values below unity could be more prevalent in European economies compared to those in the U.S., given the presence of a more established set of labor market institutions (employment protection legislation, collective bargaining, unemployment insurance, etc.).

Another potential source of markdowns below unity is measurement error, particularly due to the lack of direct price observations. Unobserved prices are a common concern, as firm-level data with broad coverage often come from financial statements, which typically lack price information. Nevertheless, De Ridder et al. (2024) demonstrate that the dispersion of market power indicators among firms and their trends over time can still be reliably assessed using the method developed by De Loecker and Warzynski (2012). Therefore, these measurement errors are unlikely to pose a significant issue, as the primary goal of this paper is to analyze the evolution of markdowns over time in response to the regulation of noncompetes in collective bargaining agreements.

Table 2: Summary Statistics

	No Collective Regulation of Noncompetes				Collective Regulation of Noncompetes			
	N	Mean	SD	Median	N	Mean	SD	Median
Markdown	64316	1.14	0.51	1.05	15713	0.90	0.46	0.75
TFP	64316	12.42	2.65	12.38	15713	10.62	1.47	9.97
Employees	64316	68.87	257.55	13.00	15713	44.89	243.33	6.00
Markup	64316	1.12	0.33	1.04	15713	1.16	0.20	1.18
Industry markdown	64316	1.14	0.33	1.09	15713	0.91	0.34	0.73
Hourly wage	61436	13.86	6.85	12.77	15338	11.92	5.24	10.86
Wage bill per worker	64316	41 545.17	20 670.92	38 116.75	15713	35 095.18	16 061.87	31 638.14
EBITDA margin	64220	0.05	0.10	0.05	15709	0.07	0.09	0.07

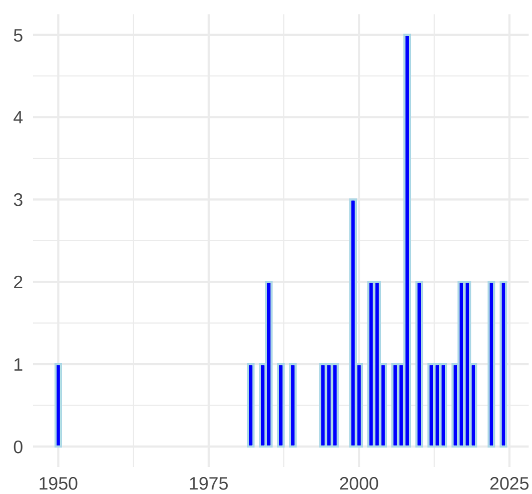
5 Results

5.1 Descriptive Evidence

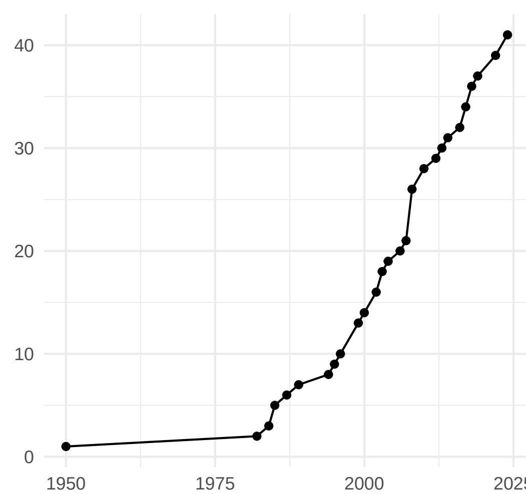
5.1.1 Descriptive Evidence on Collective Bargaining Agreements

This section provides an overview of the characteristics and evolution of collective bargaining agreements that regulate noncompetes (i.e., categories 2 and 3 of table 1 excluding category 1, those that merely mention such clauses). Although firm-level markdowns are estimated solely for the manufacturing sector, here we present statistics for all sectors.

Figure 1 shows the introduction of noncompete regulations in collective bargaining agreements by year (1a) and the cumulative number of agreements addressing them (1b).The first collective agreement regulating the use of noncompetes for technicians in the film industry was stipulated in 1950. No further regulations of noncompetes were found until the early 1980s, after which the number of collective agreements regulating noncompetes grew significantly.



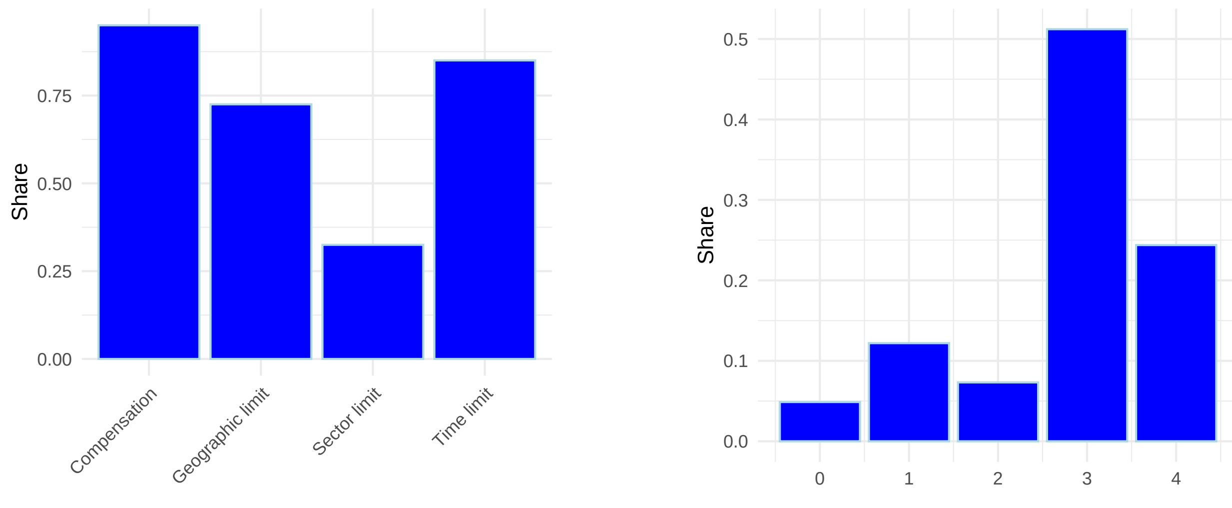
(a) New noncompetes regulations in collective bargain agreements



(b) Cumulative number of collective bargaining agreements regulating noncompetes

Figure 1: Regulation of noncompetes in collective bargain agreements overtime

Figure 2 shows the main characteristics of sectoral collective bargaining agreements provisions regulating noncompetes (i.e., categories 2 and 3 of table 1). Restricting the analysis to collective bargaining agreements that specify at least one requirement, 95% mention compensation, 85% require a time limitation, 73% define a geographic scope, and 33% specify a sector of application. The lower number of agreements defining a sectorial scope can be explained by the fact that the national regulation does not explicitly require it. The seond panel of Figure 2 shows the distribution of collective bargaining agreements based on the number of main requirements regulated. The majority (76%) regulates three or more of the main requirements.



(a) Share of collective bargaining agreements by main criteria

(b) Share of collective bargaining agreements by cumulative number of criteria

Figure 2: Characteristics of the collective bargaining agreements regulating noncompetes

Note: For subfigure 2a, categories are not mutually exclusive. Thus, shares add up to a figure above one.

5.1.2 Descriptive Evidence on Aggregate Trends in Monopsony Power

In this section, we present descriptive evidence on aggregate markdowns. Aggregating markdowns, however, is not a straightforward task, as there is no clear indication of the appropriate weights or the dimension of aggregation. We follow the technique proposed by Yeh et al. (2022, p. 2121), which builds on Edmond et al. (2023). This aggregate measure has two desirable characteristics. First, it is theoretically consistent with the aggregate wedge on markdowns without requiring specific assumptions about the structure of the labor or output markets. Second, it accounts for the features of the local labor market, aligning with studies that highlight the importance of proximity in monopsony power (Manning and Petrongolo 2017; Marinescu and

Rathelot 2018; Kambourov and Manovskii 2009).

We define the local labor market as the interaction between three-digit NACE sectors (s) and three-digit NUTS regions (r). For each local labor market, we compute the aggregate markdown using the formula from Yeh et al. (2022):

$$\mathcal{V}srt = \frac{\left(\sum j \in F_t(s,r) s_{jt} \frac{\theta^L_{jt}}{\theta^L_{srt}} (\nu_{jt}\mu_{jt})^{-1}\right)^{-1}}{\left(\sum_{j \in F_t(s,r)} s_{jt} \frac{\theta^M_{jt}}{\theta^M_{srt}} (\mu_{jt})^{-1}\right)^{-1}}, \tag{5}$$

where $F_t(s,r)$ denotes the set of firms in local labor market s, r, s_{jt} is the local labor market revenue share of firm j, μ_{jt} is the firm's markup, θ^L_{jt} and θ^M_{jt} are the firm-level output elasticities of labor and materials, and θ^L_{srt} and θ^M_{srt} are the local labor market aggregates.

Figure 3 displays the change in economy-wide aggregate markdowns from 2000 to 2022, using Yeh et al.'s (2022) aggregation method. The figure shows a significant increase in the aggregate markdown over time. The difference between the value in 2023 (1.36) and in 1996 (1.20) represents a 13% increase. The aggregate markdown appears to exhibit cyclical behavior, with substantial drops during the Great Recession and the COVID-19 crisis.

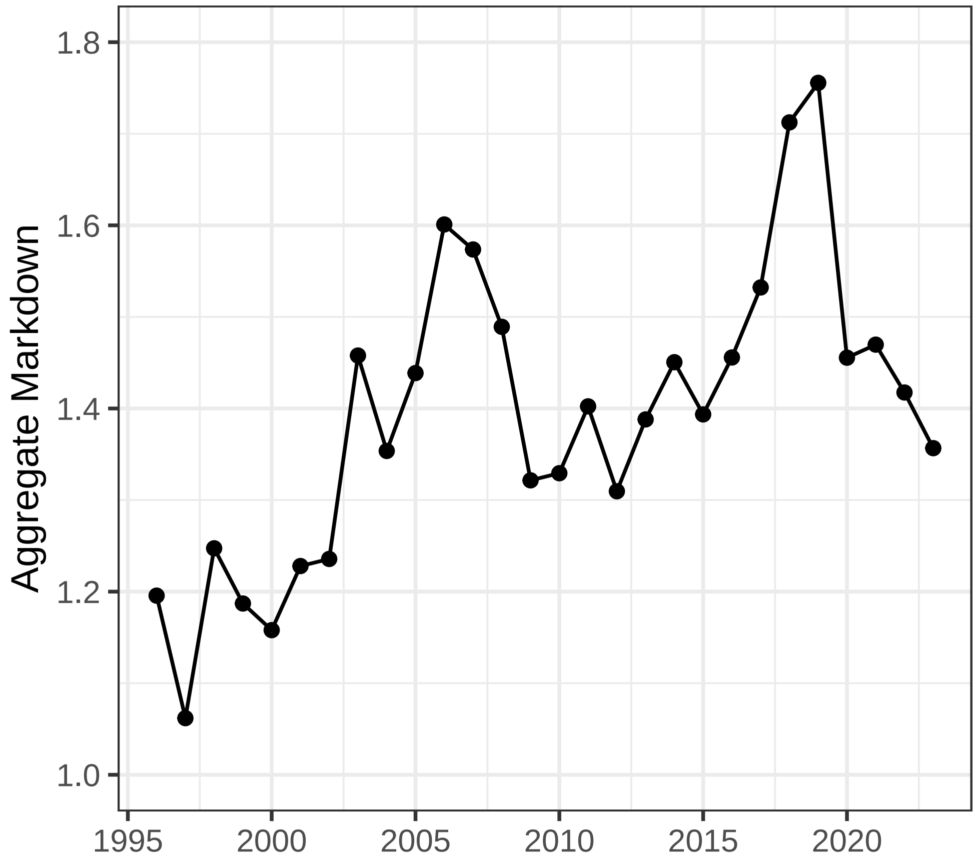


Figure 3: Economy-wide aggregate markdown, 1996-2023

Note: aggregate markdowns have been obtained using (5).

5.1.3 Descriptive Evidence of Monopsony Power and Firm-Level Characteristics

In this section, we present a series of results showing the correlations between the estimated markdown and firm-level characteristics. To assess these correlations, we run the following regression:

$$\log \nu_{jt} = \sum_{d=2}^{10} \beta^d X_{it}^d + \alpha_j + \tau_t + \epsilon_{jt}, \tag{6}$$

where X^d represents the deciles of the variable of interest, with the first decile serving as the reference category.

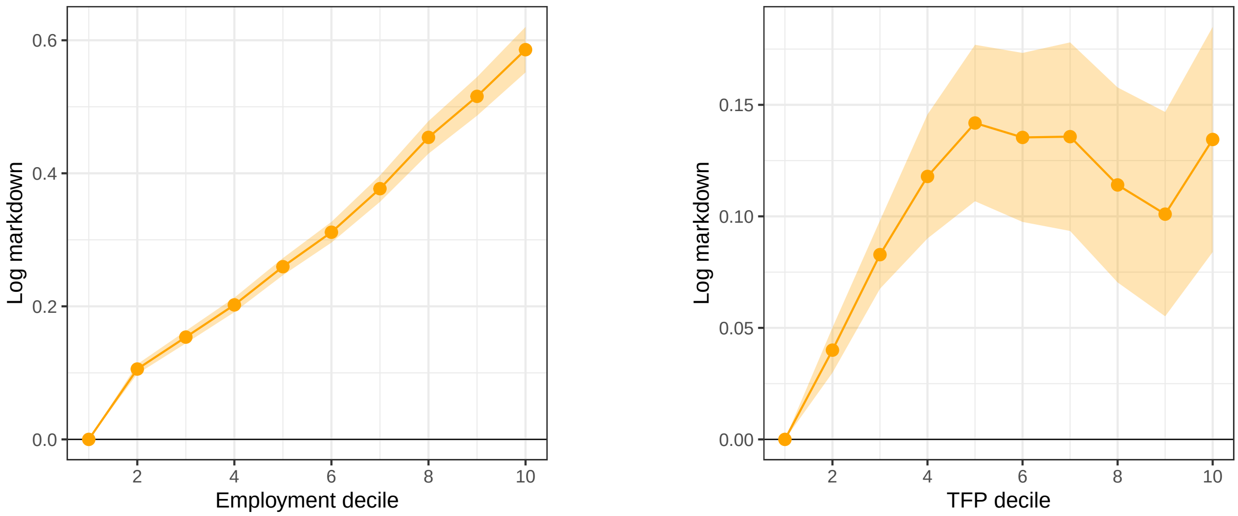
Subfigure 4a illustrates the relationship between firm size, measured by employment, and the (log) markdown. Similar to the findings of Yeh et al.(2022), we observe that larger firms tend to have higher markdowns, and this relationship is monotonic. Similarly to Yeh et al. (2022) again, we find an increasing but non-monotonic relationship between total factor productivity and firm-level markdowns, as shown in subfigure 4b.

Subfigure 4c displays the correlations between markdowns and labor market concentration. This subfigure shows a strong positive relationship between local labor market employment shares and markdowns. Consistent with the findings of Azar et al. (2020, 2022), we observe that concentration in local labor markets correlates with firm-level markdowns. In other words, we find an association between markdowns and more intuitive, static, and less computationally demanding measures of monopsony power.

Lastly, we explore the relationship between markdowns and a proxy for the average hourly wage. Since Orbis Historical does not provide worker-level data, we estimate the net hourly wage as follows: first, we divide the annual wage bill per worker by the labor tax wedge, and then divide this by the industry-level hours per worker obtained from KLEMS data. In contrast to the previous regressions, the deciles of firm-level markdown now represent the main independent variable. In addition to firm and year fixed effects, we control for a proxy of the marginal revenue product of labor, calculated as value-added per worker.

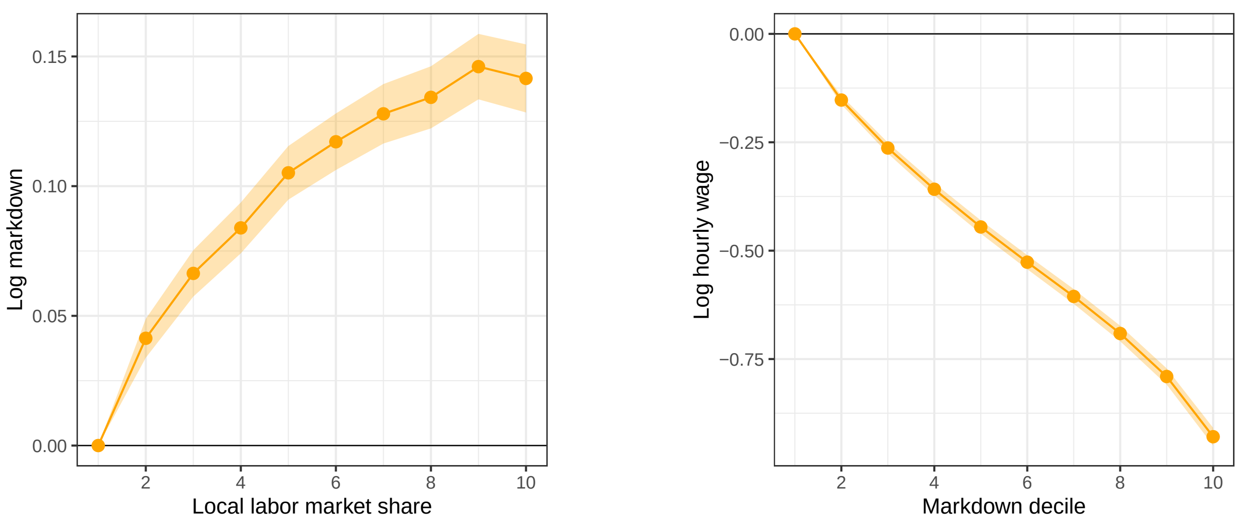
Subfigure 4d shows a negative monotonic relationship between markdowns and the average hourly wage, holding labor productivity constant. This monotonic relationship strengthens the credibility of our markdown estimates: among firms with equal marginal revenue product of labor, wages should be mechanically lower for those with higher markdowns. Despite the presence of several confounding factors, subfigure 4d demonstrates a particularly strong negative relationship, with firms in the top markdown decile paying approximately 60% less in hourly wages compared to those in the first decile.

The correlations presented in figure 4 lend credibility to our estimated firm-level markdowns. Similar to the findings of Yeh et al. (2022), we observe a positive relationship between markdowns and both employment and total factor productivity, with the former being monotonic and the latter non-monotonic. Furthermore, we identify a strong positive correlation between a more straightforward indicator of monopsony power, such as the employment share in the local labor market, and markdowns. Lastly, we find a strong negative monotonic relationship between markdowns and the hourly wage, holding labor productivity constant, which aligns with the theoretical definition of markdown.



(a) Employment

(b) Total factor productivity



(c) Local labor market employment shares

(d) Log average hourly wage

Figure 4: Markdowns and firms' characteristics

Note: For the first three panels, the figure shows point estimates of firm-level markdowns on decile indicators, obtained by running (6). The decile indicators are computed using: employment, total factor productivity, and local labor market shares. The local labor market is defined as NUTS three-digit region and NACE three-digit industry pair. The last panel shows point estimates of firm-level log average hourly wage on markdown decile indicators, obtained by running (6). The average hourly wage has been obtained by applying the labor tax wedge to the firm's wage bill per worker and by dividing the resulting figure by the industry hours per worker. The coefficients reflect differences with respect to the first decile, which is the omitted category. Standard errors are clustered at the firm level. Shaded areas denote 95% confidence intervals.

5.2 Noncompetes, Collective Bargaining, and Monopsony

5.2.1 Baseline Results

In this section, we present the baseline results of the effects of sectoral collective bargaining agreements regulating noncompetes beyond what is established by case law (i.e., category 3 of table 1) on firm-level markdowns.

We begin by displaying the results from a static version of model (3), which excludes the leads and lags of R. Table 3 indicates that the regulation of noncompetes in collective bargaining agreements reduces firm-level markdowns by 1.3% to 2.2%. The coefficients show minimal variation between the traditional two-way fixed effects specifications and the models that account for potential biases due to the staggered nature of the treatment. These findings provide initial evidence of the effectiveness of collective agreements in reducing monopsony power and improving workers' conditions.

Table 3: Static effect of regulating noncompetes in collective bargaining agreements on markdown

	TWFE	Two-Stage DID	Sun & Abraham (2021)
R	-0.018^{***}	-0.022^{***}	-0.013^{**}
	(0.006)	(0.008)	(0.005)
Firm effects	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
Observations	79 864	79 864	79 864

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. The table shows the estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running the static version of (3). The first, second, and third columns report the estimates obtained obtained via two-way fixed effects, Gardner (2022)'s two-stage DID, and Sun and Abraham (2021) estimator. For the two-stage DID control coefficients are not reported as they are used in the first stage. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

While the static specification offers a useful initial idea of the effect size, it does not account for the temporal dynamics that may influence the analysis. Specifically, firm-level markdowns may not have been on parallel trends during the pre-treatment period. To address this concern, in figure 5 we present the results from the event-study specification obtained by running model (3). The first finding is that the pre-treatment coefficients oscillate around zero and remain very close to it. Although there is no definitive procedure to check for parallel trends, these results provide some evidence that firm-level markdowns were not already declining in treated industries before the regulation. The second key result is that the effects of collective agreements regulating noncompetes grow over time. By the post-treatment period of +9, which bins all treatment periods beyond nine years, the treatment effect is approximately 10% larger than in the reference period. Intuitively, such regulations may take time to produce measurable effects on markdowns.

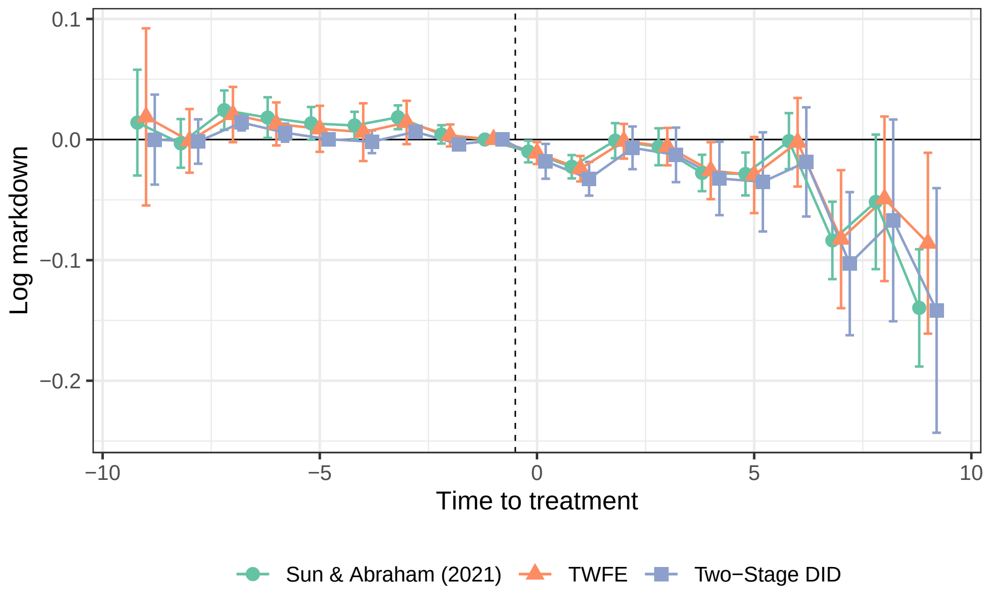


Figure 5: Dynamic effect of regulating noncompetes in collective bargaining agreements on markdown

Note: The figure shows the point estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running (3). Pre-treatment period -1 is taken Estimates are obtained obtained via two-way fixed effects, Gardner (2022)'s two-stage DID, and Sun and Abraham (2021) estimator. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level. Vertical bars denote 95% confidence intervals.

5.2.2 Identification Issues & Robustness Checks

Heterogeneity in Union's Strength & Endogeneity. A concern is that the estimated effects may reflect union strength rather than the regulation of noncompetes itself. More precisely, although stronger unions likely have more power to regulate noncompetes through collective bargaining, issues arise when the observed reduction in monopsony power is due to variations in union strength rather than the regulation itself, leading to inflated coefficient estimates. As mentioned in Section 4.3, there may be unobserved factors that correlate with the treatment and markdowns, posing significant threats to our identification strategy.

We address these concerns in three ways. First, if this were the case, we would expect to see negative and significant pre-treatment coefficients for R, indicating that markdowns were already lower in these industries due to stronger unions. However, the estimated pre-treatment coefficients are both very low and positive, suggesting that, if anything, markdowns were slightly higher in treated industries before the introduction of the regulation. Second, we re-estimate the main results by explicitly controlling for industry union density. Third, we employ the instrumental variable approach outlined in section 4.3.

Related to the second point, we re-run the main analysis by adding wud^{FR}, computed using the methodology described in Section 4.3, as control and report the results in Table 4. As we can see, controlling for union density does not change the size and significance of the coefficients of the baseline specification.

Table 4: Static effect of regulating noncompetes in collective bargaining agreements on markdown controlling for union density

	TWFE	Two-Stage DID	Sun & Abraham (2021)
R	-0.018^{***}	-0.022^{***}	-0.013^{**}
	(0.006)	(0.008)	(0.005)
Firm effects	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
Observations	74 560	74 560	74 560

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table shows the estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargain agreements, obtained by running the static version of (3). The first, second, and third columns report the estimates obtained obtained via two-way fixed effects, Gardner (2022)'s two-stage DID, and Sun and Abraham (2021) estimator. For the two-stage DID control coefficients are not reported as they are used in the first stage. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, weighted industry union density, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

Table 5 presents the results of our instrumental variable approach. The coefficient of the instrumented variable remains negative, significant, and comparable in magnitude to the baseline estimates. The large value of the first-stage F-statistic suggests that the instruments are strongly correlated with the treatment, alleviating concerns about weak instruments. Furthermore, the Sargan-Hansen P-value of 0.118 indicates that we fail to reject the null hypothesis that the instruments are uncorrelated with the error terms. Overall, the sign and magnitude of the instrumental variable estimates address some of the endogeneity concerns associated with the baseline estimates.[3]

[3] For clarification, we have not used either weighted industry union density as a control or the instrumental variable strategy in the main analysis because ESS data begins in 2002, which would result in the loss of all observations from years prior to that date.

Table 5: Static effect of regulating noncompetes in collective bargaining agreements on markdown, instrumental variable approach

	IV
R	-0.041^{**}
	(0.016)
Firm effects	Yes
Year effects	Yes
Observations	69 560
First stage F-statistc	1142
Sargan-Hansen P-value	0.118

* p < 0.1, ** p < 0.05, *** p < 0.01

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. The table shows the estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running the static version of (3). The treatment variable is instrumented using three different instruments consisting of the average weighted industry union density across the following country groups: Western European countries, Northern European Countries, and Southern European countries. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

Declining Industry Markdowns. One potential threat to our identification strategy is the presence of industry dynamics that correlate with the treatment and may influence firm-level markdowns. For instance, collective bargaining agreements regulating noncompetes could have been implemented in industries where markdowns were already declining due to reduced economic activity. We address this concern in three ways. First, pre-treatment coefficients that hover around zero help mitigate these concerns, as they provide evidence that firms in treated and untreated industries were on parallel trends. Second, the inclusion of time-fixed effects and the industry average markdown further controls for these potential endogeneity issues. Third, the identification threats arising from French-specific industries should be mitigated by the shift-share estimation strategy.

Firms' Influence on Collective Bargaining Agreements. A significant threat to the interpretation of our results is the ability of more powerful firms to influence the content of collective bargaining agreements. This concern is mitigated in two ways. First, our treatment stems from collective agreements negotiated at the sectoral level, where the influence of individual firms is likely to be more limited by the need to set standards that fit the entire sector. Second, the inclusion of controls for total factor productivity, firm size, and product market power helps address concerns about differing markdown dynamics among more powerful firms. Third, the previously implemented instrumental variable approach, which relies on union density in other European countries, helps reduce endogeneity concerns related to the bargaining power of French firms.

Sample Selection Bias & Treatment-Control Group Imbalances. To partially address sample selection bias and imbalances between the treatment and control groups, we present the results of the inverse probability weighting approach in figure 6. Similar to the baseline results, all pre-treatment coefficients are centered around zero, and in this case, all of them are also statistically non-significant. Regarding the post-treatment period, the coefficients exhibit a dynamic very similar to the baseline analysis. Overall, the results from the inverse probability weighting suggest that any potential imbalances between the treatment and control units do not pose significant threats to our identification strategy.

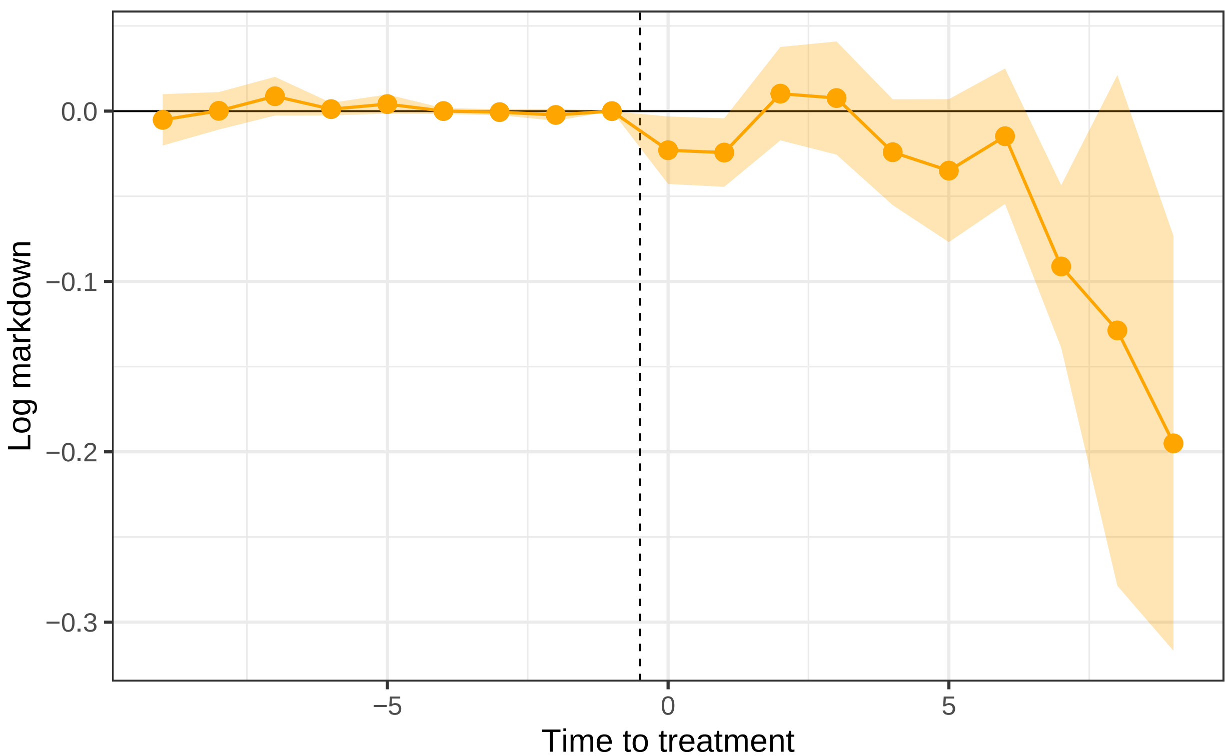


Figure 6: Dynamic effect of regulating noncompetes in collective bargaining agreements on markdown with inverse probability weights

Note: The figure shows the point estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running (3) using the two-stage DID specification with inverse probability weights. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level. The shaded area denotes 95% confidence intervals.

Placebo Test. When conducting a difference-in-differences design, there is no definitive procedure to test for parallel trends. While observing pre-treatment leads close to zero is a common practice, this alone is not sufficient to confirm the parallel trends assumption. Another frequently used approach is the placebo test, which involves applying "fake" treatments where no significant effect on outcomes is expected. To perform this, we replaced the original treatment group with units from industries where collective bargaining agreements either merely mention noncompetes or do not introduce any regulations beyond what is already established by case law (i.e., categories 1 and 2 of table 1). Finding negative effects for these units would pose a serious threat to our identification strategy, potentially suggesting that our findings are driven by other unobserved, coincidental events rather than the collective bargaining agreements themselves.

The results presented in Table 6 indicate that our identification strategy has passed the placebo test. As shown, the coefficients associated with the fake treatment are non-significant.

Table 6: Placebo test

	TWFE
$fake\ R$	0.041
	(0.032)
Firm effects	Yes
Year effects	Yes
Observations	64 151

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. The table shows the estimates of firm-level markdown on sectoral collective bargaining agreements that simply mention noncompetes or do not add anything compared to case law, obtained by running the static version of (3). Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

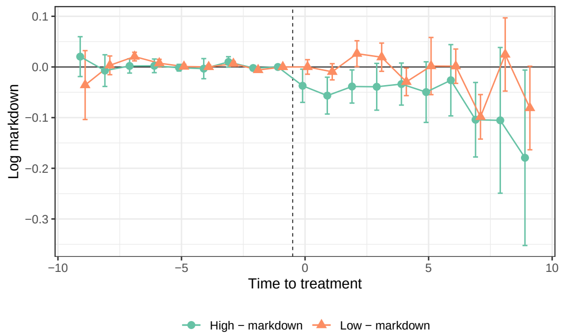
5.2.3 Firm Heterogeneity

Provisions regulating noncompetes in collective bargaining agreements often reinforce the requirements set by courts and further introduce stricter requirements regarding the duration, geographical and sectoral scope, and minimum compensation. As a result, these collective bargaining agreements are likely more effective in reducing firms' abuse of noncompetes. For instance, firms that do not set a time limit or fail to provide an adequate compensation may still exert downward pressure on workers' wages by limiting their outside options, even though these noncompetes may be unenforceable. When workers believe these clauses are valid, they may accept lower wages or decide not to quit due to the *in terrorem* effects of unenforceable noncompetes (Starr et al., 2020). For example, Boeri et al. (2024) find that a large fraction of noncompetes are unenforceable in Italy, and yet many workers are unaware of this, with negative consequences in terms of their willingness to quit—unenforceable noncompetes are more generally associated with lower wages and are more prevalent among less educated and low-skill workers (Starr et al., 2020; Boeri et al. (2024).

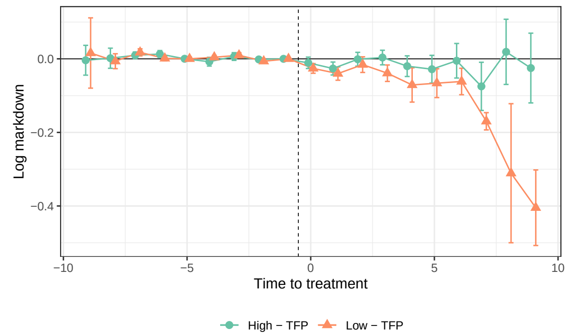
Therefore, we might expect the regulation of noncompetes in collective bargaining agreements to be more effective for low-skill workers, who are more likely to be exposed to the abuse of noncompetes. Ideally, we would have access to data on workers' wages and education levels. However, Orbis Historical does not provide information on this type of within-firm heterogeneity, as it only includes firm-level balance sheet data. To address this limitation, we analyze how the effects of regulating noncompetes vary according to firm characteristics that are correlated with workers' skill levels. The literature suggests that highly productive and large firms that pay higher wages tend to attract a more educated workforce (Kline 2024). Therefore, we expect workers in these firms to be less likely exposed to unfair noncompetes and, as a result, the impact of the regulation should be smaller.

To assess this hypothesis, we computed the pre-treatment averages of firm-level markdowns, TFP, size, and hourly wage for treated firms. For each of these variables, we created an indicator based on the median value, categorizing the treated firms into "high" and "low" groups. We then run model (3) on subsamples, comparing treated units in the high and low groups with untreated ones.

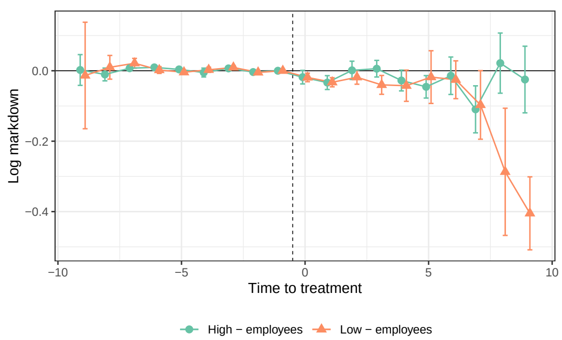
Figure 7 shows that the reduction in markdowns is more pronounced for treated firms with high pre-treatment markdowns and low pre-treatment TFP, size, and average hourly wages. These trends are particularly evident for size and TFP, suggesting that collective bargaining agreements regulating noncompetes have a larger impact on firms that are less productive and smaller in size. Overall, these results provide some support for the hypothesis that collective agreements regulating noncompetes are more effective for less productive, smaller firms that pay lower wages—firms more likely to attract a less educated workforce, which the literature has identified as being more exposed to an unfair use of noncompetes often violating legal limitations to the use of noncompetes.



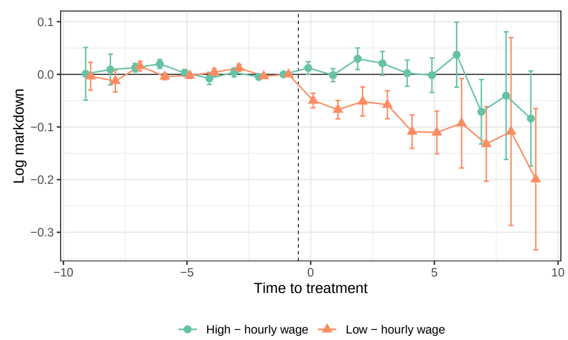
(a) Markdown



(b) TFP



(c) Number of employees



(d) Hourly wage

Figure 7: Heterogeneous effects of regulating noncompetes in collective bargaining agreements on markdown according to pre-treatment characteristics

Note: The figure shows the point estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running (3). Results are obtained by running the model into two different sample one for treated units in the "high" and one for the "low" category, where high and low are defined on pre-treatment firm-level characteristics. In both samples, the control group consists of never-treated units. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level. Vertical bars denote 95% confidence intervals.

5.2.4 Other Outcomes

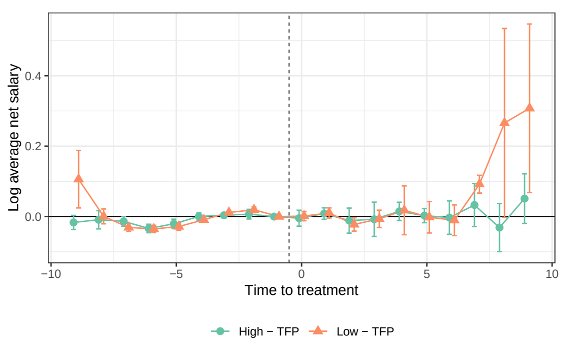
In the previous sections, we have shown that collective agreements regulating noncompete agreements reduce firm-level markdowns, with a stronger effect for smaller firms, those with lower total factor productivity, and those that pay their employees less. In this section, we extend our analysis to examine the impact of these regulations on two additional outcomes: the average annual salary and the EBITDA margin.[4] The goal of using these two additional outcomes is twofold. First, we aim to assess whether these regulations have led to an increase in workers' compensation. Second, we seek to determine whether there has been a reduction in

[4]The annual salary is derived by applying the French labor tax wedge to the annual average wage bill per worker. The EBITDA margin is defined as EBITDA over revenues.

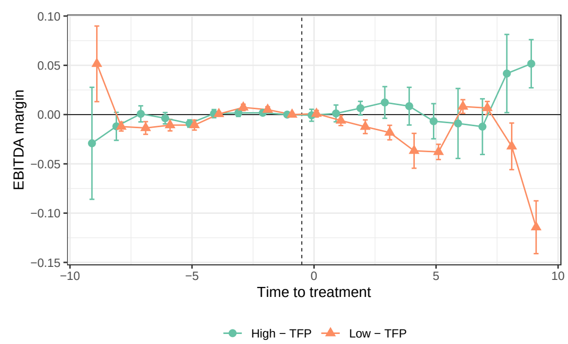
firm profitability. In doing so, we explore how these effects vary for treated firms based on their pre-treatment TFP, given the strong heterogeneity highlighted in the previous section.

The left panel of Figure 8 shows that the average annual salary has increased over time for low-productivity firms, while the effects are non-significant for high-productivity firms. These results are consistent with those presented in subsection 5.2.3, where we find a substantial reduction in markdowns for low-productivity firms, but no significant effects for high-productivity firms. The right panel reinforces the consistency of our analysis by showing that profitability, as measured by the EBITDA margin, has decreased for low-productivity firms, while it has increased for high-TFP firms.

Taken together, these findings suggest that the introduction of provisions regulating the use of noncompetes in collective agreements may have led to a within-firm reallocation of economic surplus in favor of workers, particularly in less productive firms.



(a) Average annual salary



(b) EBITDA margin

Figure 8: Heterogeneous effects of collective agreements regulating noncompetes on the average annual salary and EBITDA margin according to pre-treatment total factor productivity

Note: The figure shows the point estimates of firm-level log average annual salary and EBITDA margin on the regulation of noncompetes by sectoral collective bargain agreements, obtained by running (3). Results are obtained by running the model into two different sample one for treated units in the "high" and one for the "low" category of TFP, where high and low are defined on pre-treatment firm-level characteristics. In both samples, the control group consists of never-treated units. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level. Vertical bars denote 95% confidence intervals.

5.2.5 Sectoral Collective Bargaining & National Regulation

In section 2, we discussed the 2002 landmark decision delivered by the French Court of Cassation that introduced a significant regulatory shift by mandating employers to provide financial compensation to employees in exchange for a noncompete—prior to this judgment, an economic consideration was required only if stipulated by the collective bargaining agreement.

Figure 9 provides an initial glimpse of the impact of this ruling. The vertical bars represent the proportion of collective bargaining agreements establishing the right to a compensation, both before and after 2002, over the total number of agreements regulating noncompetes before and after that date.[5] The share of provisions that entitle employees to receive a compensation in exchange for the noncompete sharply increased from 70% to 98% following the 2002 judgment. Although the ruling mandated a compensation, it did not specify a minimum threshold, leaving the requirement to be assessed by courts on a case-by-case basis. Importantly, the share of collective bargaining agreements that explicitly define a minimum compensation threshold also rose significantly after 2002, increasing from 55% to 97%.

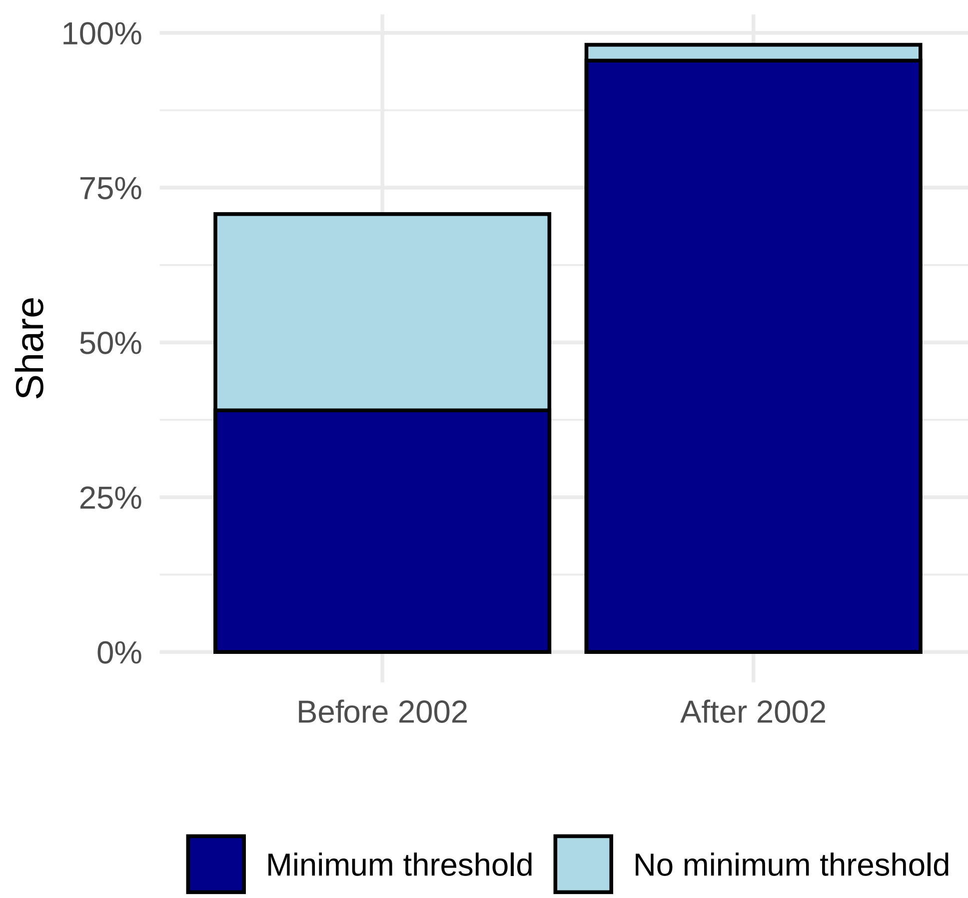


Figure 9: Share of collective bargaining agreements establishing the entitlement of a compensation before and after 2002

[5]Before and after 2002 is defined based on the year of the provision that regulates noncompetes.

We now exploit the 2002 discontinuity to compare the effects of judge-made national-level regulation with sectoral bargaining agreements. To do so, we run two regression models. In the first model, we restrict our sample to firms in industries where the collective bargaining agreement regulated noncompetes (i.e., categories 2 and 3 of table 1), and where the relevant provision was introduced before 2002. The treatment group consists of firms where the collective agreement did not establish the entitlement to compensation, while the control group includes firms subject to collective agreements where such compensation was already required. The treatment period begins with the year of the decision (i.e., 2002).[6] Since the decision established the entitlement to compensation regardless of what the collective bargaining agreement established, we expect a stronger effect in industries where compensation was not previously required by the collective agreement—in other words, we do not expect significant changes in markdowns in industries where the collective bargaining agreement already imposed compensation requirements for noncompetes.

In the second model, the control group remains the same (i.e., firms subject to collective agreements where a compensation was already required), but the treatment group is expanded to include firms in industries where the collective bargaining agreement did not regulate noncompetes at all as the 2002 judgment applies to all firms, regardless of the content of the collective bargaining agreements. Comparing these two models will allow us to evaluate the impact of a regulation at the national level (in this case, introduced by case law) when a collective regulatory framework already exists (i.e., noncompete regulations without compensation), and in cases where no sectoral negotiation has occurred.

The first column of Table 7 shows that markdowns decreased following the 2002 decision in industries where collective agreements regulated noncompetes but did not establish a mandatory compensation, compared to industries where compensation was already required. As shown in the second column, when we include firms subject to collective agreements that do not regulate noncompetes, we find no significant effect. These results suggest that national regulation (in this case, introduced by case law) is more effective in reducing markdowns when it builds upon an existing regulatory framework established by collective bargaining agreements.

[6] In contrast to the main analysis, this difference-in-differences design is not staggered.

Table 7: Effect of the 2002 judgment on firm-level markdown

	Collective Bargaining Regulations	Whole Sample
$judgement_{2002}$	−0.074**	0.035
	(0.026)	(0.028)
Firm effects	Yes	Yes
Year effects	Yes	Yes
Observations	8716	72 867

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. The table shows the estimates of firm-level markdown on the 2002 Cassation Court Judgment of noncompetes in sectoral collective bargain agreements. In the first column, the treatment group is composed by firms for which the collective bargaining agreement was regulating noncompetes before 2002 without establishing the entitlement of a compensation. In the second column, the treatment group also includes firms for which the collective bargaining agreement does not regulate noncompetes. In both cases, the control group is composed by firms for which the collective bargaining agreement regulated noncompetes before 2002 and established the entitlement of a compensation. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

6 Conclusions

In this paper, we have examined the role of unions in mitigating monopsony power through the regulation of noncompete agreements within collective bargaining agreements. First, we have constructed a unique sectoral dataset on the regulation of noncompetes by analyzing the universe of French collective bargaining agreements. Second, we have linked this sectoral-level data to firm-level markdowns and other firm's characteristics to study the relationship between the stringency of the regulation of noncompete agreements in collective agreements and markdowns in the French manufacturing sector by estimating a staggered event study model. Our baseline results indicate that the regulation of noncompete clauses reduces firm-level markdowns by 1.3%–2.2%, with this effect intensifying over time. The robustness of these findings is confirmed by the results of the shift-share instruments combined with inverse probability weighting, addressing concerns related to endogeneity and sample selection issues.

A key finding of our analysis is that the regulation of noncompetes by sectoral collective agreements has a more pronounced effect on markdowns in smaller, less productive firms that offer lower wages. This aligns with the hypothesis that such firms tend to attract low-skilled workers, who, as noted by Boeri et al. (2024), are more likely bound by overly restrictive noncompetes that fail to comply with legal requirements. Furthermore, our analysis of the French Court of Cassation's 2002 decision suggests that regulations at the national level could be more effective when they build upon an existing regulatory framework established through collective agreements. Our findings ultimately highlight a significant complementarity between the regulation of noncompetes at the national level and collective bargaining.

Our paper is the first contribution that links monopsony power, unions, and noncompetes. As such, it can be expanded in several directions. First, while evidence exists for countries such as Italy and the United States regarding the widespread use of unenforceable noncompetes, similar information is lacking for France. Gathering such evidence could further support our findings on the greater effects of regulating noncompetes in smaller, less productive companies that, on average, pay lower hourly wages. Second, the Orbis Historical data lack information on individual workers within firms, which prevents us from analyzing the impact of regulation across different groups of workers. Collection of data on the wage distribution at the firm level would also allow for assessing the role played by efficient bargaining (over wages and employment) on noncompetes and markdowns. Future research could thus build on the novel results of this paper by further exploring these two dimensions.

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Appendix

Contents

A Details on Markdown Estimation

In this section, we provide additional details on the procedure adopted to estimate markdowns and the underlying theoretical framework. In doing so, we do not offer any new theoretical or empirical contributions in the following two subsections, but we entirely rely on existing works (Olley and Pakes 1996; Levinsohn and Petrin 2003; De Loecker and Warzynski 2012; Ackerberg et al. 2015; Morlacco 2019; Yeh et al. 2022).

A.1 Theory

Consider a firm i that seeks to minimize costs according to the following production function:

$$Q_{it} = Q_{it}(X_{it}^1, \ldots, X_{it}^V, L_{it}, K_{it}, \omega_{it}),$$

where X_{it}^v (for $v = 1, \ldots, V$) is a vector of variable inputs, L_{it} denotes labor, K_{it} is capital, and ω_{it} indicates total factor productivity. Following Ackerberg et al. (2015), labor and capital are treated as state variables. The Lagrangian formulation for solving this minimization problem is given by:

$$L_{it}(X_{it}^1, \ldots, X_{it}^V, L_{it}, K_{it}, \omega_{it}) = \sum_{v=1}^{V} P_{it}^v X_{it}^v + w_{it}(L_{it})L_{it} + r_{it}K_{it} + \lambda_{it}(Q_{it} - Q_{it}(\cdot)),$$

where P_{it}^v, w_{it}, and r_{it} are the prices of variable inputs, wages, and capital, respectively. The first-order condition for minimizing costs with respect to labor is expressed as:

$$w_{it}\left(\frac{\epsilon_{it}^L w + 1}{\epsilon_{it}^L w}\right) = \lambda_{it}\frac{\partial Q_{it}(\cdot)}{\partial L_{it}},$$

where ϵ_{it}^{Lw} represents the firm's labor supply elasticity.[7] The Lagrange multiplier λ_{it} measures how the minimum cost changes with a marginal change in output, effectively representing the marginal cost. Using the relationship $\lambda_{it} = \frac{P}{\mu}$, where P is the output price, the ratio $\frac{\epsilon_{it}^L w+1}{\epsilon_{it}^L w}$ corresponds to the inverse of the markdown.

To further explore this, we use the dual framework and analyze the firm's profit-maximization problem,

[7] $\epsilon_{it}^{Lw} = \frac{\partial L}{\partial w}\frac{w}{L}$. Since $w(L)$ is the inverse function of L, $\frac{\partial L}{\partial w} = \frac{1}{w'(L)}$. Thus, $\epsilon_{it}^L w = \frac{1}{w'(L)}\frac{w}{L}$.

as proposed by Yeh et al. (2022). The profit-maximization problem can be written as:

$$\max R_{it}(L_{it}) - w_{it}(L_{it})L_{it},$$

where $R_{it}(L_{it})$ represents the revenue when all inputs other than labor are optimized. The first-order condition for labor under profit maximization becomes:

$$\frac{R'_{it}(L_{it})}{w_{it}(L_{it)}} = \frac{\epsilon^L_{it}w + 1}{\epsilon^L_{it}w}.$$

Using the definition of markdown as the ratio of marginal revenue product of labor (MRPL, denoted as $R'_{it}(L_{it})$) to the wage, we have:

$$\nu_{it} = \frac{w_{it}}{R'_{it}(L_{it})} = \frac{\epsilon_{it}}{\epsilon_{it} + 1}.$$

By substituting the expressions for the markdown and the Lagrange multiplier into the first-order condition of the cost-minimization problem and rearranging the terms, the following expression for firm-level markdowns is obtained:

$$\nu = \frac{\theta^L}{\alpha^L}\mu^{-1},$$

where θ^L_{it} represents the labor elasticity of output and α^L_{it} denotes the share of revenue allocated to labor costs. Regarding markups, Yeh et al. (2022: 2105) demonstrate the following relationship:

$$\mu_{it} = \frac{\theta^V_{it}}{\alpha^V_{it}},$$

where μ_{it} is calculated for a generic variable input X^V_{it} other than labor.

A.2 Estimation Procedure

To recover the market power index and the markup, we need the output elasticities and revenues share of labor and a variable input. We follow Yeh et al. (2022) and choose materials to recover markups. However, while the revenue shares are directly observable in Orbis data, elasticities require the estimation of a production function.

Consider the following (gross) output translog production function:

$$y_{it} = \beta_{it}^{l} l_{it} + \beta_{it}^{k} k_{it} + \beta_{it}^{m} m_{it} + \beta_{it}^{ll} l_{it}^{2} + \beta_{it}^{kk} k_{it}^{2} + \beta_{it}^{mm} m_{it}^{2} + \beta_{it}^{lk} l_{it} k_{it} + \beta_{it}^{lm} l_{it} m_{it} + \beta_{it}^{km} k_{it} m_{it} + \omega_{it} + \epsilon_{it},$$

where l_{it}, k_{it}, m_{it} are labor, capital, and materials expressed in logs, while ω_{it} is the firm's total factor productivity. This term is unobserved to the researcher but known by the firm. To obtain y_{it}, k_{it}, m_{it}, we have deflated operating revenues, total fixed assets, and material costs from ORBIS using the OECD GDP deflator, while for l_{it}, we have used the number of employees.

The production function has been estimated at the NACE 2-digit industry level. A crucial assumption is that the generic variable input demand is a function of the state variables, productivity, and other market factors z_{it}. We follow the literature (De Loecker and Warzynski 2012; De Loecker et al. 2016; Morlacco 2019) and include in z industry revenue shares, year, industry (NACE 3-digit), and location (NUTS 3-digit) fixed effects. Additionally, as suggested by Morlacco (2019), we account for input price bias and buyer power using buyer shares for materials. As in Yeh et al. (2022), we have used materials as a variable input:

$$m_{it} = m(\omega_{it}, l_{it}, k_{it}, z_{it}).$$

If the function m is invertible, then we can express the unobserved firm productivity as:

$$\omega_{it} = h(m_{it}, l_{it}, k_{it}, z_{it}).$$

This technique is called the "control function" approach and allows us to obtain a proxy of ω_{it} to include in our estimation. Otherwise, ignoring productivity will lead to biased estimates since it creates a correlation between the regressors and the error term. The procedure is divided into two stages.

A.2.1 First Stage

We define the function ϕ:

$$\phi(l_{it},k_{it},m_{it},z_{it}) = \beta^l_{it}l_{it}+\beta^k_{it}k_{it}+\beta^m_{it}m_{it}+\beta^{ll}_{it}l^2_{it}+\beta^{kk}_{it}k^2_{it}+\beta^{mm}_{it}m^2_{it}+\beta^{lk}_{it}l_{it}k_{it}+\beta^{lm}_{it}l_{it}m_{it}+\beta^{km}_{it}k_{it}m_{it}+h(m_{it},l_{it},k_{it},z_{it}).$$

Which substituted in the production function gives:

$$y_{it} = \phi(l_{it},k_{it},m_{it},z_{it}) + \epsilon_{it}.$$

Then we regress y_{it} on a third-order polynomial expansion of $\phi(l_{it},k_{it},m_{it},z_{it})$ in all its terms and store $\hat{\epsilon}_{it}$ and $\hat{\phi}_{it}$.

A.2.2 Second Stage

Productivity is assumed to follow a Gauss-Markov process of order 1:

$$\omega_{it} = g(\omega_{it-1}) + \xi_{it}.$$

Following De Loecker and Warzynski (2012) we approximate $g(.)$ by a third order polynomial in its argument and use the error term ξ_{it} to define the following system of moment conditions:

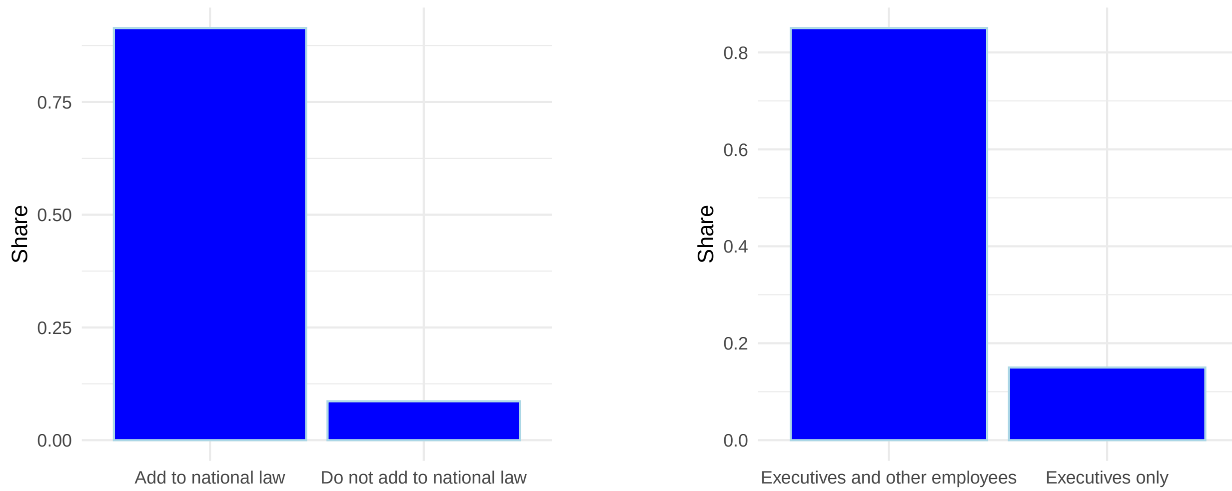
$$\mathrm{E}(\xi_{it}(\boldsymbol{\beta})\mathbf{z_{it}}) = 0.$$

The vector $\mathbf{z_{it}}$ contains lagged values of the input with the exception of capital and labor that are assumed to be chosen one period ahead. We can now recover the parameters of interest using a generalized method of moments estimation. We follow De Loecker and Warzynski (2012) and allow for measurement errors in output and unobserved shocks to the production function, which are combined in ϵ_{it}. Therefore, we divide revenues by $\hat{\epsilon}_{it}$ to get corrected expenditure shares for labor and materials. Since the coefficient of the log Cobb-Douglas corresponds to elasticities, we now have all the ingredients to compute market power and markups, plus markdowns as a ratio between the two indicators.

B Additional Descriptives

B.1 Collective Bargaining Agreements

In figure B.1 we present additional characteristics of collective bargaining agreements regulating noncompetes. The vast majority of them (91%) introduce additional regulations beyond what is established by courts. Only 15% of these provisions apply exclusively to executives, while 85% apply to both executives and employees.

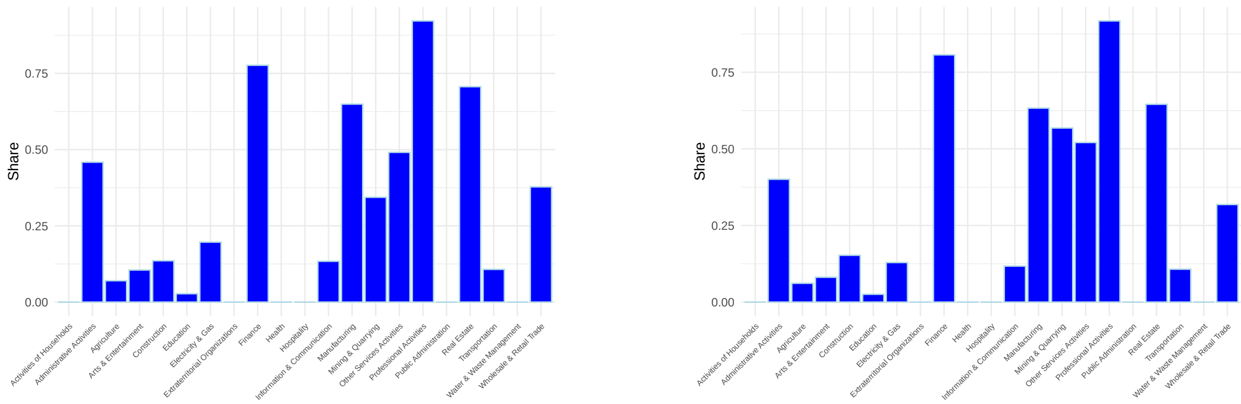


(a) Share of collective bargaining agreements by relationship with national law

(b) Share of collective bargaining agreements by type of employee covered

Figure B.1: Additional characteristics of the collective bargaining agreements regulating noncompetes

Figure B.2 illustrates to what extent the four-digit industry sectors, where collective bargaining agreements regulating noncompete clauses are applicable, contribute to the value-added and employment within the broader one-digit sector. The four one-digit sectors with the largest coverage, according to both variables, are professional activities, finance, real estate, and manufacturing. Notably, in the manufacturing sector, which is the focus of our analysis on monopsony, collective bargaining agreements regulating noncompetes cover 65% of total value-added and 63% of total employment.



(a) Value-added coverage

(b) Employment coverage

Figure B.2: One-digit sector coverage of collective bargain agreements

Note: Coverage is defined by summing the value-added or employment of the four-digit NACE sectors covered by the collective bargaining agreement over the total value for the one-digit sector.

B.2 Markdowns

In Figure B.3, we zoom in on the sectoral dimensions by plotting the change in the industry-level aggregate markdown. This plot reveals that the economy-wide aggregate trends mask significant heterogeneity. Although the aggregate markdown has increased in most industries, a considerable number of industries show a decline in this indicator. Therefore, Figure B.3 suggests that distinct monopsony power dynamics may be occurring at the sectoral level.

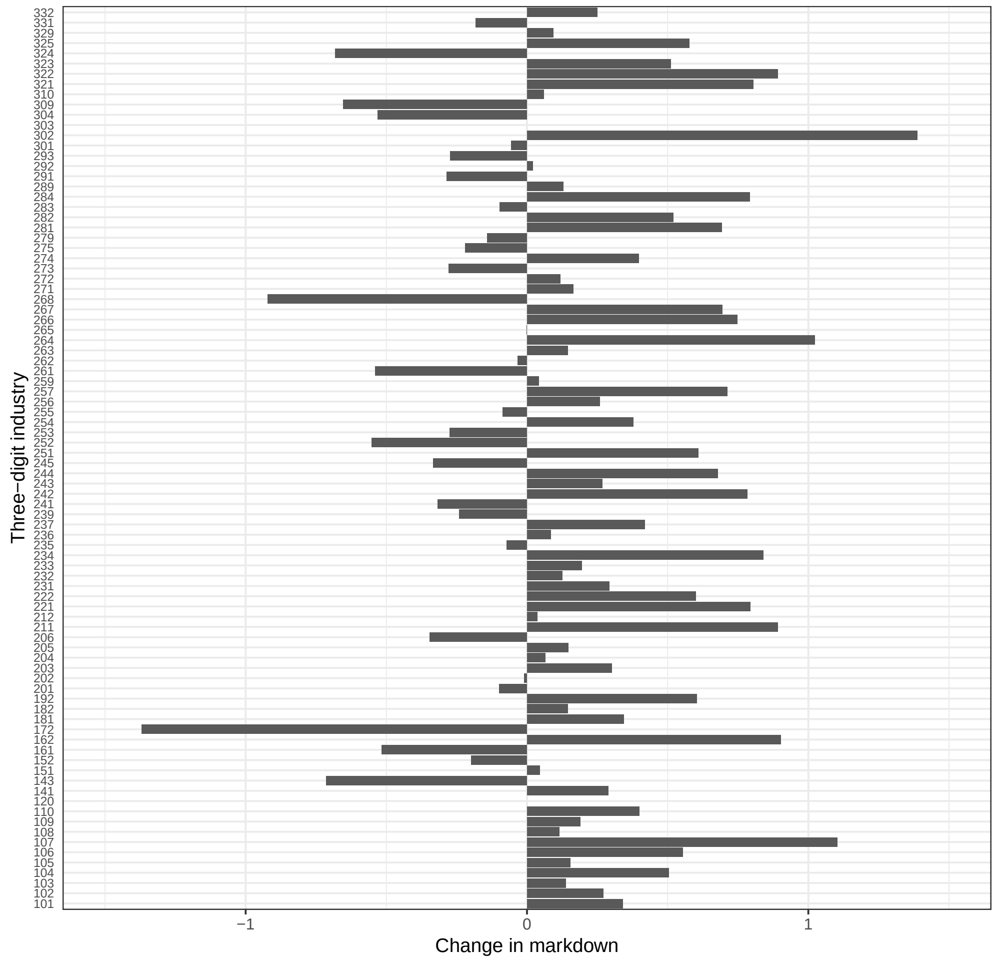


Figure B.3: Change in aggregate markdown by three-digit industry

Note: aggregate markdowns have been obtained using (5). The change is computed between the last and first year available.

Figure B.6 presents the geographical distribution of the aggregate markdown computed using (5) in 2022 at the NUTS-2 level (left-hand panel) and the absolute change between 2022 and 2002 (right-hand panel). The five regions with the largest aggregate markdowns in 2022 are, in order: Nord-Pas-De-Calais, Poitou-Charentes, Haute-Normandie, Bretagne, and Aquitaine. The regions recording the largest increases are: Haute-Normandie, Nord-Pas-De-Calais, Basse-Normandie, Alsace, and Bretagne. Overall, from the two figures, we observe an increase in markdowns since 2000, concentrated in northern regions. In fact, Nord-Pas-De-Calais, Haute-Normandie, and Bretagne are not only among the top five regions for aggregate markdowns in 2022 but also among those recording the largest increases since 2000.

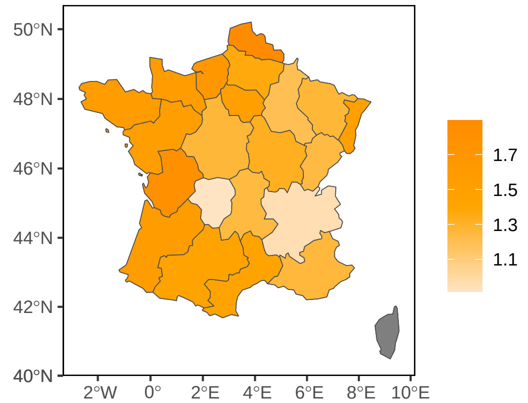


Figure B.4: Aggregate markdown by NUTS-2 region 2022

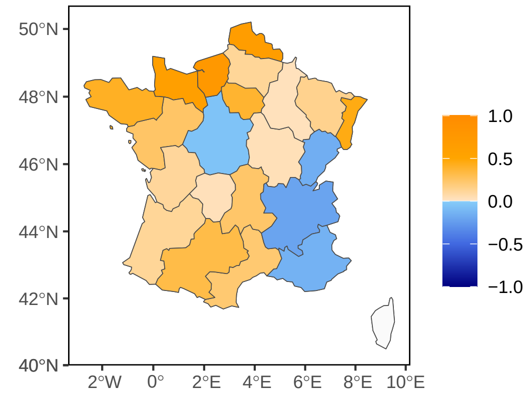


Figure B.5: Change in aggregate markdown by NUTS-2 region 2022-2000

Figure B.6: Geographical distribution and variations in aggregate markdown by NUTS-2 region

Note: Aggregate markdown at the NUTS-2 level are obtained using (5). The change in markdown are obtained by subtracting to the value in 2022 the value in 2000.

In subfigure 4c in the main text, we plot the relationship between markdowns and local labor market shares. The following figure, instead, plots the relationship between markdowns and industry concentration measured using revenues. Figure B.7 shows a hump-shaped relationship between markdowns and the revenue share of the firm's industry. For firms above the sixth decile, this relationship is positive, suggesting that firms with a more dominant position in the product market may also impose larger markdowns.

Figure B.7: Markdown and industry revenues shares

Note: The figure shows point estimates of firm-level markdowns on industry revenues share decile indicators, obtained by running (6). The coefficients reflects difference with respect to the first decile, which is the omitted category. Standard errors are clustered at the firm-level. Shaded areas denote 95% confidence intervals.

B.3 Union Density

Figure B.8 plots the weighted industry union density by treatment group. As shown, this variable is significantly higher in the treated group than in the never-treated group. This difference aligns with the notion that more powerful unions may have played a key role in achieving the regulation of noncompetes within the industry, providing support for our shift-share instrument strategy.

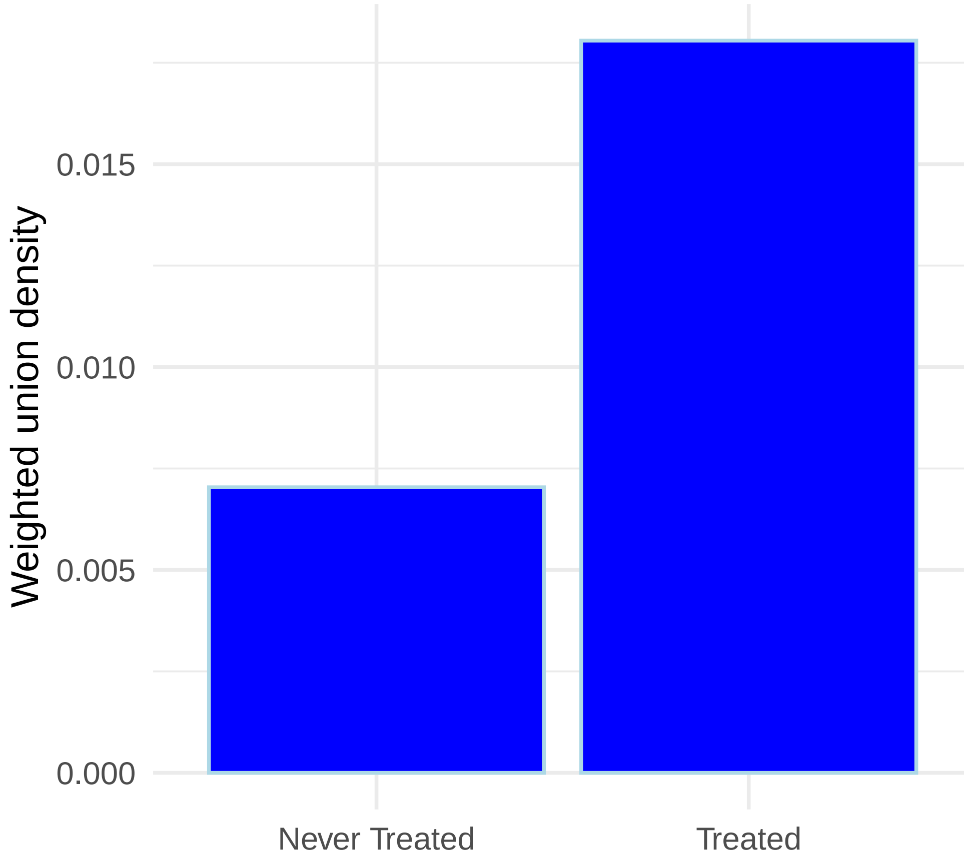


Figure B.8: Weighted industry union density by treatment group

To validate our union density statistics, we examine their correlation with measures from the OECD. However, to the best of our knowledge, no datasets are available for European countries that compute union density at the industry level over a reasonable time frame.[8] Therefore, the best approach to validate the ESS data is to compute nationwide, time-varying union densities and assess their correlation with OECD statistics. By doing so, we obtain a very strong correlation, measured by a Pearson coefficient of 0.97. While not perfect, this test provides some reassurance regarding the reliability of our union density measures.

[8]Exception is made for Agnolin et al. (2024), which we discuss in section C.2.

C Additional Robustness Checks & Evidence

C.1 Alternative Production Function Estimations

In the main analysis, we obtained firm-level markdowns by estimating NACE two-digit translog production functions over the period 1996–2023, where nominal variables were deflated using the GDP deflator. In Table C.1, we present results from three alternative production function specifications: a Cobb-Douglas, a translog with variable-specific and input-specific deflators, and a translog estimated over two-year windows allowing for time-varying betas. As shown, the coefficients from all the different specifications are negative and significant while remaining comparable in size to the baseline results. Overall, this robustness exercise shows that our findings are robust to the alternative production function estimation techniques.

Table C.1: Baseline estimates with alternative production functions

	Cobb-Douglas			Translog (Industry Deflators)			Translog (2-Year Windows)		
	TWFE	Two-Stage DID	Sun & Abraham	TWFE	Two-Stage DID	Sun & Abraham	TWFE	Two-Stage DID	Sun & Abraham
R	−0.026***	−0.028***	−0.019***	−0.035***	−0.038***	−0.020***	−0.018**	−0.022***	−0.019**
	(0.008)	(0.010)	(0.005)	(0.007)	(0.008)	(0.043)	(0.008)	(0.008)	(0.008)
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80 500	80 500	80 500	77 337	77 337	77 337	82 018	82 018	82 018

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table shows the estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running the static version of (3). Three groups of estimates are reported depending on the production function used to estimate markdowns: Cobb-Douglas (columns 1-3), translog with variable-specific and industry-specific deflators (columns 4-6), translog estimated over two-year windows (columns 7-9). For each production function estimations, the first, second, and third columns report the estimates obtained obtained via two-way fixed effects, Gardner (2022)'s two-stage DID, and Sun and Abraham (2021) estimator. For the two-stage DID control coefficients are not reported as they are used in the first stage. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

C.2 Agnolin et al. (2024) France Union Density

The only study attempting to compute union density at the industry level for European countries is Agnolin et al. (2024). These authors estimate union density using multilevel regression with poststratification (MRP), combining survey data from the European Social Survey with census data from IPUMS. From various potential model specifications, the dataset includes estimates derived from the model that performed best in France, as identified through cross-validation using the RMSE metric. In an nutshell, this metric measures the average difference between the model's predicted probabilities of union membership and the actual unionization rates observed in the survey, providing a gauge of predictive accuracy.

For this study, we only have access to their estimated union density data for France. Therefore, as a robustness check, we re-run the results of Table 4 using the union density estimates from Agnolin et al. (2024), but not the instrumental variable approach.

In Figure C.1, we present the results obtained when controlling for Agnolin et al.'s (2024) union density estimates, alongside a comparison with the results reported in the main text. As shown, these new estimates are only slightly reduced in magnitude compared to those previously obtained. We chose not to use Agnolin et al.'s (2024) union density estimates in the main text because their measure ends in 2018, making it impossible to capture the effects of regulations implemented thereafter.

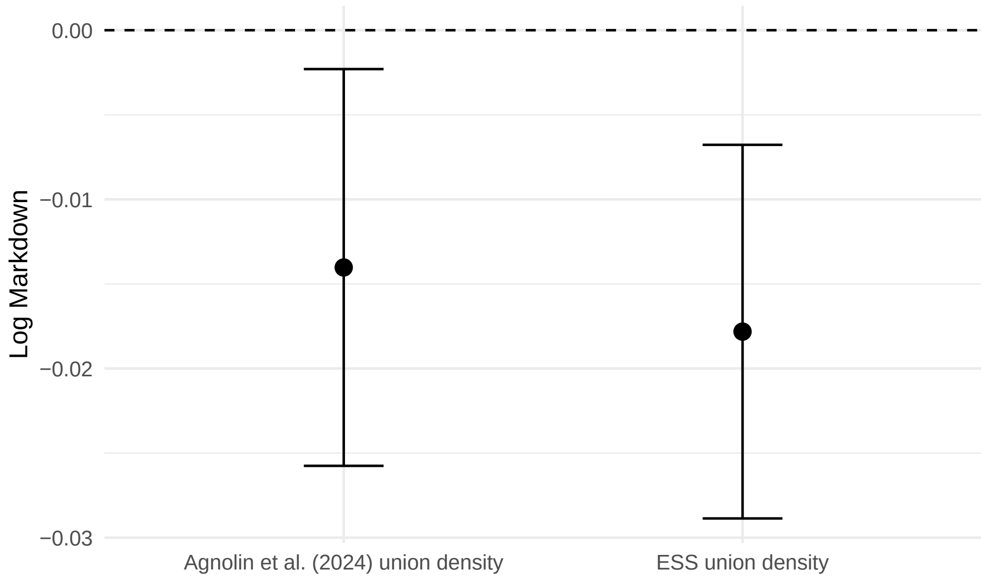


Figure C.1: Comparison of between controlling for Agnolin et al. (2024) and ESS union density

The figure shows the estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargain agreements, obtained by running the TWFE static version of (3). The model on the left control for Agnolin et al. (2024) weighted union density while the one of the right for the ESS union density used in the main text. Other controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level.

C.3 The Role of Intangibles

Bartelsman et al. (2024) develop and test a model where lifting noncompetes increases the wages of workers in high-intangible firms. The reasoning is that workers acquire knowledge about the use of intangible assets, which reduces the firm's marginal cost of production. If these workers resign and join a competitor, it negatively impacts the firm's profits. Thus, for workers possessing knowledge of intangible assets, the separation cost for the firm is higher. Firms can mitigate the risk of workers joining competitors by offering higher wages. However, the use of noncompetes can substitute for higher wages as a means to prevent worker departures. In this context, Bartelsman et al. (2024) find that the removal of noncompetes in the Netherlands for temporary workers has increased the wages of these workers in high-intangible firms because firms are now forced to offer higher wages to prevent departures.

In the context of the present paper, we do not observe the outright removal of noncompetes but rather their regulation in collective bargaining agreements. Specifically, our treatment involves regulations that go beyond national law by, among other provisions, defining a minimum compensation threshold. These regulations not only provide workers with clearer information about the effective rules governing noncompetes but also establish a minimum floor for compensation. In high-intangible-intensity firms, therefore, employers have stronger incentives to offer higher compensation for noncompetes. This higher compensation, in turn, is reflected in overall wages. For this reason, we expect to observe a greater reduction in markdowns following the regulation of noncompetes in high-intangible-intensity firms.

To test this prediction, we compute the pre-treatment averages of intangible intensity (i.e., intangible assets per employee) and re-run the analysis using the same approach as in Figure 7. Consistent with the above prediction and the findings of Bartelsman et al. (2024), the effect of regulating noncompetes on markdowns is stronger in high-intangible-intensity firms.

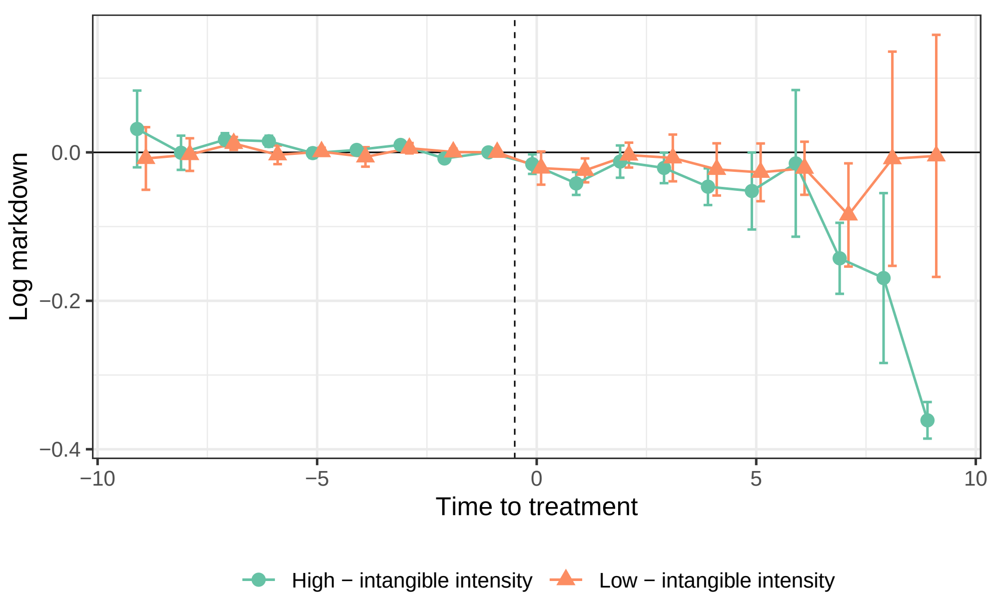


Figure C.2: Heterogeneous effects of regulating noncompetes in collective bargaining agreements on markdown according to pre-treatment intangible intensity

Note: The figure shows the point estimates of firm-level markdown on the regulation of noncompetes in sectoral collective bargaining agreements, obtained by running (3). Results are obtained by running the model into two different sample one for treated units in the "high" and one for the "low" category, where high and low are defined on pre-treatment firm-level intangible intensity. In both samples, the control group consists of never-treated units. Controls include: log total factor productivity, log employees, log markup, log industry (NACE four-digit) markdown, firm, and year effects. Standard errors are clustered at the NACE four-digit industry-level. Vertical bars denote 95% confidence intervals.

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