Competition in the Labor Market: Evidence from Swedish Pharmacies*

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Abstract

How important is labor market competition for workers? Despite growing regulatory interest in employer market power, empirical evidence on the wage effects of competition remains limited. We address this gap by studying the 2009 Swedish pharmacy deregulation, which dismantled a state monopoly and introduced exogenous variation in labor market structure. Difference-in-differences estimates show that wages increased by 3.2% in markets that experienced an increase in competition. To determine which models of firm behavior rationalize the data, we estimate a structural model of labor supply and demand and develop a conduct test that compares model predictions to the quasi-experimental evidence. We find that all standard models of market-based wage-setting overshoot the observed wage response by an order of magnitude. Competition moves wages in the expected direction, but far less than strategic models predict, consistent with post-reform wage-setting remaining largely governed by non-market conditions.

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1 Introduction

One of the key tenets of industrial organization is that firms have market power where they can set prices above marginal cost. Whether competition authorities should intervene depends on the source. Market power arising from well-differentiated products, through quality improvements or better advertising, may benefit consumers and lies outside the purview of antitrust, while market power stemming from unhealthy competition warrants scrutiny. A parallel logic applies to labor markets: when firms can set wages below workers' marginal product, workers earn less than the value they create. Again, whether competition authorities should act depends on the source of market power. If workers accept lower wages for jobs with desirable amenities, such as flexible hours, good colleagues, or meaningful work, the wage gap reflects compensating differentials rather than exploitation. Similarly, if wage markdowns arise from search frictions that make job-finding costly, then it lies outside the scope of antitrust. But if the gap arises from imperfect labor market competition, it falls squarely within the domain of antitrust enforcement.

This insight, that employers may possess *monopsony* power, has recently gained renewed attention among both economists and policymakers (Azar and Marinescu, 2024; Posner, 2021; Naidu et al., 2018). The U.S. Federal Trade Commission and Department of Justice issued new merger guidelines in 2023 that explicitly consider labor market effects (U.S. Department of Justice and Federal Trade Commission, 2023), and enforcement actions citing harm to workers have increased sharply. The European Commission has similarly signaled greater attention to labor market competition. Yet it remains unclear what the role of competition is in the labor market. More precisely, do firms actually set wages strategically in response to market structure, or do they follow simpler rules that make labor market competition irrelevant?

We study the 2009 Swedish pharmacy deregulation, which provides an ideal laboratory for analyzing labor market competition. Before the reform, all pharmacies were owned by a single state-owned company, Apoteket AB, creating a textbook monopsony. The reform privatized approximately two-thirds of pharmacies, selling them in geographic clusters to private buyers while retaining the remainder under state ownership. This generated exogenous variation in local labor market concentration: some municipalities saw their pharmacies split among multiple owners, while others remained in ownership of a single chain or the retained monopoly.

Several institutional features make this setting particularly well-suited to study labor market power. Wages in the pharmacy sector were set individually rather than through collective bargaining, ensuring that wage variation reflects employer decisions. Product

prices are regulated, so employers cannot pass labor cost savings to consumers or finance higher wages through product market markups. The cluster structure of the privatization means that the reform's geographic incidence was largely predetermined, limiting selection concerns. Additionally, pharmacist licensing requires a specific degree, allowing us to observe the complete pool of potential pharmacists in each labor market.

We proceed in three steps. First, we present reduced-form evidence using difference-in-differences, comparing wages in municipalities where pharmacies were sold versus retained. We find that wages increased by approximately 3.2% more in treated markets, while value added per pharmacist rose by approximately 6%. The imperfect passthrough of wages to productivity (0.5) is inconsistent with frictionless labor markets, where wages equal the marginal product and should track productivity one-for-one. This confirms the presence of labor market imperfections, consistent with the rent-sharing literature (Card et al., 2018). However, the reduced form alone cannot determine whether competition raised wages by reducing monopsony markdowns (firms paying a larger share of the marginal product) or simply by raising productivity, since the value added increase is confounded by workforce restructuring.

Second, we estimate a structural model of labor supply and demand. Workers choose among differentiated jobs via discrete choice, facing switching costs and commuting disutility. Firms strategically set wages and labor demand, internalizing their effect on labor supply. Third, we use the estimated model to predict what wages would have been under the post-reform ownership structure under alternative models of firm conduct, and compare these predictions to the quasi-experimental evidence. Every standard model of market-based wage-setting overshoots the observed wage response by an order of magnitude. Competition moves wages in the expected direction, but far less intensely than strategic oligopsony models predict.

Relation to the Literature. We contribute to two strands of literature. The first studies how competition affects labor market outcomes. Concentration measures such as market shares or the Herfindahl-Hirschman Index are often used as proxies for competition, and many studies have documented a negative correlation between labor market concentration and wages (Azar et al., 2022; Benmelech et al., 2022; Rinz, 2022; Thoresson, 2024; Autor et al., 2023).[1] We complement reduced-form estimates with structural analysis, allowing us to examine how market structure causally affects workers.

[1]The theoretical relationship between concentration and wages is ambiguous. As Berry et al. (2022) demonstrate, higher concentration can coexist with higher wages when productivity differences across markets are significant. This ambiguity mirrors the long-standing debate in industrial organization about the link between prices and concentration, which motivated the New Empirical Industrial Organization literature (Bresnahan, 1989).

The second strand focuses on modeling labor market power. The existing literature focuses on the different sources: differentiated jobs, search frictions, and market structure. The first builds on Rosen (1986), where workers trade off wages against amenities. The second, following Burdett and Mortensen (1998), models search and matching frictions that allow firms to set wages below marginal product because workers face costs when switching jobs. The third models monopsony as workers facing a finite number of employers, an idea dating back to Robinson (1933).

Recent work has begun incorporating multiple channels. Berger et al. (2022) model a representative household economy where labor market power arises from differentiated jobs and oligopsonistic market structure. Jarosch et al. (2024) use a random search model with large firms, emphasizing search frictions as the primary driver of monopsony power. We unify these three frameworks into a single structural model that incorporates job differentiation, search frictions, and strategic market structure.

A parallel question is how to *test* which model of firm behavior best fits the data. In product markets, the conduct testing literature uses exclusion restrictions to discriminate between models of competition (Bresnahan, 1989; Backus et al., 2021). Roussille and Scuderi (2025) adapt this approach to labor markets, using exclusion restrictions in the spirit of Backus et al. (2021) to test between monopsonistic and oligopsonistic conduct. Our approach differs in that we exploit a quasi-experimental shock in market structure to set-identify conduct, trading point identification for credibility of the identifying variation.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting of the Swedish pharmacy market. Section 3 presents our data sources and sample construction. Section 4 provides reduced-form evidence on the wage effects of deregulation. Section 5 develops our structural model of labor market competition. Section 6 describes the estimation procedure and presents parameter estimates. Section 7 tests models of firm conduct by comparing counterfactual predictions to the quasi-experimental evidence. Section 8 concludes.

2 Institutional Background

This section describes the Swedish pharmacy market, focusing on features relevant for studying labor market competition: the state monopoly that prevailed until 2009, the deregulation process that introduced variation in market structure, and the institutional features that limit confounding factors.

2.1 The Swedish Pharmacy Monopoly (1971–2009)

In 1971, the Swedish government nationalized all pharmacies under a single state-owned company, Apoteket AB, through Statute (1970:205).[2] The stated rationale was the universal and equal availability of pharmaceuticals, including in rural areas where private pharmacies might not be profitable (SOU, 1969). By 1984, the number of pharmacies had grown to 762, and the monopoly employed 12,000 people (SOU, 1984). By the time of the 2009 deregulation, Apoteket operated 924 pharmacies nationwide (Swedish Agency for Public Management, 2013).

At the beginning of 2008, approximately 9,000 trained pharmacy workers were employed in Sweden: roughly 3,000 pharmacists (*apotekare*, holding a five-year master's degree) and 6,000 pharmacy technicians (*receptarier*, holding a three-year bachelor's degree). About two-thirds worked for Apoteket AB; the remainder were employed in the pharmaceutical industry, hospital pharmacies, and other sectors (SOU, 2008). Practicing as a pharmacist requires a specific degree and an occupational license issued by the National Board of Health and Welfare (Socialstyrelsen), allowing us to observe the complete pool of licensed pharmacists regardless of current employment status.[3] Unemployment among trained pharmacy workers was negligible: 57 individuals in November 2007, a rate of 0.6% (SOU, 2008). This combination of a single employer, a clearly defined workforce, and near-full employment makes the pre-reform pharmacy sector an ideal setting to study labor market power.

Wage-Setting Practices. An important institutional feature is that wages in the Swedish pharmacy sector were set individually within a collective bargaining framework. Two unions organized the vast majority of pharmacy workers: Sveriges Farmaceuter (representing pharmacists, with approximately 90% membership in 1995; Lönngren et al., 1999) and Farmaciförbundet (representing pharmacy technicians, later merged into Unionen in 2014; Thoresson, 2024). Together, these unions covered over 95% of pharmacy employees (Thoresson, 2024).

The collective agreements negotiated between these unions and the employer organization Almega were *sifferlösa avtal* (figure-less agreements): they specified the process for wage-setting rather than wage levels.[4] The 2002 agreement states: "The salary formation

[2]Prior to nationalization, Sweden had 637 pharmacies: 377 independent, 238 subsidiary, and 22 hospital pharmacies. The sector employed approximately 8,900 people, including 900 pharmacists, 1,800 dispensers, and 6,200 pharmacy technicians and other staff (SOU, 1969). Steps toward collectivization had been underway since the 1930s, including redistributive measures for pharmacy returns (1931) and government pharmaceutical insurance (1955) (Stanfors and Öberg, 2007; Lönngren et al., 1999).

[3]Pharmacist licensing was formalized in 1999, when both *apotekare* and *receptarie* became legally protected titles subject to revocable licenses.

[4]Some agreements included aggregate "wage pots" (typically 2.9–3.2% annual increases) to be distributed

process takes place at the company level, where the individual salary discussion is an important stage. Salary negotiations must take place annually based on known salary principles directly between the salary-setting manager and the employee" (National Mediation Office, 2003). This system of individual wage-setting within a collective framework was adopted by Farmaciförbundet in 1988 following a pivotal 1984 agreement; Sveriges Farmaceutförbund, as a Saco-affiliated union of academically trained professionals, had practiced individualized wage negotiation for longer (Övling and Jacobsson, 2014).

The wage-setting authority, the *lönesättande chef*, is the pharmacy store manager (*apotekschef*), who conducts annual salary discussions with each employee based on individual performance, competence, and results.[5] Chain headquarters and regional managers set budget envelopes, but the individual wage decision is made locally in dialogue between the store manager and the employee. This institutional arrangement means that different establishments of the same chain can and do pay different wages to observationally similar workers, a pattern confirmed by our establishment-year fixed effects, which show meaningful within-chain wage variation. The Swedish system of decentralized wage formation within collective frameworks is well documented in the broader labor economics literature (Granqvist and Regnér, 2008; Ulfsdotter Eriksson et al., 2021).

Individual wage-setting is important for our analysis because it implies that wage variation across establishments reflects employer decisions rather than collectively bargained wage scales. Under collective bargaining with uniform wages, monopsony power would manifest through employment rather than wages, complicating our identification strategy. Wage-setting practices did not change after the deregulation, and wage floors introduced by Farmaciförbundet in 2009 affected very few workers (Thoresson, 2024).

Product Price Regulation. A potential concern with studying labor market power in retail pharmacy is that employers might also exercise product market power, confounding the relationship between market structure and wages. In the Swedish context, this concern is mitigated by extensive product price regulation.

Prescription drug prices are set by the Dental and Pharmaceutical Benefits Agency (TLV) through a regulated trade margin (*handelsmarginal*): a tiered formula combining a percentage markup on the pharmacy purchase price with a fixed per-package fee.[6] Pharmacies have

individually, while others were pure process agreements with no stipulated wage space (National Mediation Office, 2003). See Övling and Jacobsson (2014) for a detailed account of the transition.

[5]Post-reform, the employer organization for retail pharmacy chains shifted from Almega to Svensk Handel, but the process-based agreement structure was maintained. The current agreement (2025–2027) remains a *sifferlöst avtal* (figure-less agreements) with seven individual wage criteria and no centrally fixed wage levels.

[6]Before the creation of TLV in 2002, this role was fulfilled by Riksförsäkringsverket (RFV). The pricing mechanism remained substantively unchanged through the reform (Apoteket AB , 2001).

no discretion over prescription drug prices. These regulated pharmaceuticals account for the majority of pharmacy revenues: approximately 75% of total sales in 2000 (Apoteket AB , 2001). In its annual report, the monopoly firm stated that it had "limited opportunities to affect revenue and cost of goods" given the regulated pricing system (Apoteket AB , 2001). While over-the-counter products and non-pharmaceutical goods carry some pricing flexibility, overall pharmacy operating margins average only around 2%,[7] leaving limited scope for passing through labor cost changes on any product category. From November 2009, some non-prescription drugs were also permitted for sale outside pharmacies (e.g., in supermarkets), further limiting the product market power of pharmacy chains.

This price regulation implies that pharmacies cannot pass through labor cost savings to consumers, nor can they finance higher wages through product market markups. The returns to labor market power accrue directly to employers as profits, making the wage-setting problem more transparent.

2.2 The 2009 Deregulation

In 2009, the Swedish government dismantled the pharmacy monopoly and introduced private ownership. The reform was preceded by a government investigation (SOU 2008:4; SOU, 2008), formalized in Proposition 2008/09:145 (Prop., 2009), and approved by parliament on April 29, 2009. The monopoly was officially abolished on July 1, 2009.[8]

A state-owned caretaker firm, Apoteket Omstrukturering AB (OAB), was created in July 2008 to manage the privatization process. OAB's mandate was to "ensure that competitive neutrality and the best possible conditions for divestment are maintained" (Apoteket Omstrukturerings AB, 2009). OAB divided Apoteket's 924 pharmacies into three groups: 446 pharmacies organized into eight clusters for sale to corporate buyers, 150 pharmacies to be sold individually to pharmacist-entrepreneurs (Apoteksgruppen), and 315 pharmacies retained in the state-owned Apoteket AB (Wikberg, 2010; National Audit Office, 2012).

Hospital pharmacies, organized under a separate subsidiary (Apoteket Farmaci AB), were excluded from the privatization and reorganized independently; the reform affected only community pharmacies.[9] The eight clusters were sold to four corporate buyers in late 2009, with the first pharmacy rebranded under new ownership in January 2010 and all transfers completed during the first half of that year (Apoteket AB , 2010; SOU, 2017). The two largest

[7] Sveriges Apoteksförening sector reports consistently report operating margins of 1.5–2.2%.

[8] The reform was partly motivated by a 2005 ruling of the Court of Justice of the European Union (Case C-438/02), which found that the Swedish pharmacy monopoly violated Article 31.1 EC regarding non-discrimination in state trading monopolies (Hettne, 2005). Sweden initially responded by modifying purchasing practices, but the ruling contributed to the political case for full deregulation (Wikberg, 2010).

[9] Apoteket Farmaci AB was later renamed Apotek Produktion & Laboratorier AB (APL) and remained state-owned throughout the period.

clusters were national in scope, while the six smaller clusters were regional (Thoresson, 2024). Restrictions on cluster combinations ensured that no single buyer could dominate the market: the largest clusters could be purchased with at most one smaller cluster, and any buyer could acquire at most three of the smaller clusters (Thoresson, 2024). The four buyers were Apotek Hjärtat (208 pharmacies in clusters 1 and 8), Kronans Droghandel (171 pharmacies in cluster 2), Medstop (62 pharmacies in clusters 3, 4, and 5), and Vårdapotek i Norden (24 pharmacies in clusters 6 and 7), for a total sale price of EUR 617 million (SEK 5,867 million) (National Audit Office, 2012). Only a small fraction of employees declined the offer to transfer their employment to the new owners (Apoteket Omstrukturerings AB, 2009). Subsequent consolidation reduced the number of major private chains: Medstop was acquired by Apotek Hjärtat in 2014, and Vårdapotek i Norden was absorbed into the same chain, leaving two national private chains alongside the regional Apoteksgruppen network and the retained state-owned Apoteket AB.

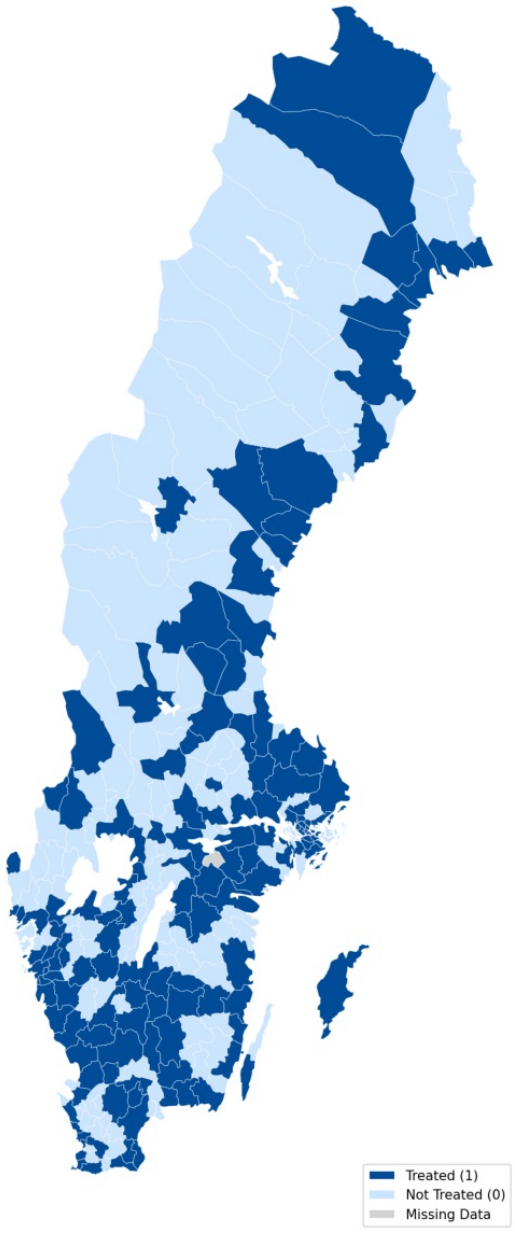
The reform also liberalized entry, allowing new pharmacies to open subject to licensing by the Medical Products Agency (Läkemedelsverket). This led to significant entry, particularly in urban areas: by May 2013, the number of pharmacies had increased to 1,280, a 39% increase, while only 5 of the original 924 had closed (Swedish Agency for Public Management, 2013). No new pharmacies were established in rural areas.[10]

The cluster structure of the privatization is central to our identification strategy. Figure 1 displays the geographic distribution of treated and control municipalities. Each cluster was designed to be geographically contiguous, meaning that the reform created local variation in ownership concentration.

Some municipalities saw their pharmacies split among multiple owners, while others remained dominated by a single chain or the retained state monopoly. Crucially, the assignment of pharmacies to clusters was determined by OAB based on geographic and operational considerations rather than labor market conditions, and the government's instructions required OAB to maintain competitive neutrality (Apoteket Omstrukturerings AB, 2009). Among the 127 municipalities where ownership remained concentrated with a single firm after the reform, 75 experienced a change in ownership identity (from Apoteket AB to a private chain) without any change in local market structure, while 52 retained both the original state owner and monopoly structure. This within-control variation provides a direct test for distinguishing competition effects from pure privatization effects.

[10]To ensure continued rural access, buyers agreed to standstill clauses prohibiting closure of over 110 designated pharmacies in sparsely populated areas for three years after transfer (Swedish Agency for Public Management, 2013). The government subsequently introduced a rural pharmacy grant (*glesbygdsbidrag*) for pharmacies more than 20 kilometers from the nearest alternative.

Figure 1: Treatment Status by Municipality



Note: Dark blue municipalities experienced ownership fragmentation during the 2009 privatization (treated); light blue municipalities retained single-owner structure (control). Treatment is defined as whether the reform introduced multiple competing owners in the municipality.

3 Data

We combine multiple administrative registers from Statistics Sweden (SCB) to construct a matched employer-employee dataset covering the universe of Swedish pharmacists from 1990 to 2021.

Data Sources. Our analysis draws on six administrative registers: LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies), which provides annual demographic and labor market information for all individuals aged 16 and older; RAMS (Register-Based Labour Market Statistics), which provides the matched employer-employee link identifying each worker's establishment in November of each year; Lönestrukturstatistiken (Wage Structure Statistics), a survey-based register with detailed wage information covering approximately 50% of the workforce annually; JOBB (Employment Spells Register), which provides all employment spells with monthly earnings; Företagens Ekonomi (FEK), which provides establishment-level financial information including revenues, value added, and costs; and geocoded coordinates for workers' residences and establishment locations, allowing us to calculate commuting distances.

3.1 Sample Construction and Wage Measure

Identifying pharmacists. To practice as a pharmacist in Sweden requires the requisite degree; everyone holding this degree is licensed to work as a pharmacist. We identify licensed pharmacists using education codes from the SUN (Swedish Standard Classification of Education) register, observing this credential in administrative records. Specifically, we classify individuals as pharmacists (*apotekare*) if they have completed 5–6 years of education with code 727, and as pharmacy technicians (*receptarier*) if they have completed 3–4 years with a related code. We retain all individuals who ever obtain a pharmacy-relevant education, identifying the complete universe of licensed pharmacists regardless of current employment status.

Identifying pharmacies. We identify pharmacy establishments using the SNI (Standard Industrial Classification) code for retail pharmacy (47730 in SNI 2007, equivalent codes in earlier versions). Following Thoresson (2024), we apply a persistence rule: if an establishment was classified as a pharmacy in year $t-1$, we classify it as a pharmacy in year t even if the SNI code changes, to avoid spurious exits due to reclassification.

Measuring wages. Our analysis requires an establishment-level wage measure that isolates the employer's wage policy from variation in workforce composition. We construct this measure by regressing individual monthly wages on gender, age groups, doctoral degree,

foreign-born status, and one-digit occupation code, while absorbing establishment fixed effects. The resulting establishment fixed effect captures what a pharmacy pays relative to the market average after controlling for the characteristics of its workforce. We normalize this fixed effect so that the mean equals the average wage in the sample, making the measure interpretable in EUR. To reduce the influence of outliers, we winsorize at the 1st and 99th percentiles. The resulting variable represents the establishment wage premium within occupations rather than reflecting differences in worker composition across pharmacies. In the pre-reform reference year (2008), our sample comprises 864 pharmacy establishments, of which 709 are in treated municipalities and 155 in control municipalities.

4 Reduced-Form Evidence

We begin with reduced-form evidence that competition raises wages. This analysis serves two purposes: it provides transparent, model-free evidence of labor market power, and it establishes the empirical benchmark that disciplines the structural conduct test in Section 7.

Treatment Definition. We define treatment based on whether a municipality experienced a shock to its ownership structure. Out of 290 municipalities in Sweden, 127 form the control group: in these municipalities, ownership remained concentrated with a single firm both before and after the reform, whether the former state monopolist or a new private owner. The remaining 163 municipalities, where the reform introduced multiple competing owners, constitute our treatment group.[11]

Identifying Assumptions. The key identifying assumption is parallel trends: absent the reform, wages would have evolved similarly in treated and control municipalities. Several institutional features support this assumption. First, treatment assignment was exogenous: the selection of pharmacies for sale was determined by a government-appointed caretaker organization, not by the monopolist or potential buyers, and the caretaker's mandate was to ensure competitive neutrality and optimal conditions for divestment (Apoteket Omstrukturerings AB, 2009). Second, the geographic allocation was pre-determined: the monopolist was required to retain pharmacies in smaller or less densely populated regions to ensure minimum supply, meaning treatment assignment was largely determined by population density rather than labor market conditions. Third, our event study analysis shows common pre-trends, with no differential trends in wages between treated and control municipalities

[11] Our treatment definition differs from Thoresson (2024), who uses a continuous measure of concentration. We prefer a binary indicator because concentration measures can be misleading when productivity differs across markets, and because we aim to estimate the effect of the reform directly rather than mediating it through concentration, which is determined simultaneously with wages.

in the years preceding the reform.

Results. Before the reform, treated and control municipalities paid nearly identical occupation-adjusted wages (a difference of EUR 30, or 0.9%). After the reform, this gap widened to EUR 145, yielding a difference-in-differences estimate of EUR 113, or 3.2% of pre-reform wages.[12] Figure 2 confirms parallel pre-trends and a sharp divergence after 2009, with the gap persisting through the end of the sample.

Composition and Mechanisms. The positive wage effect coexists with an important composition channel. At the worker level, *raw* monthly wages in treated markets actually fell by approximately EUR 62, a statistically significant decline. This apparent contradiction is resolved by the dramatic shift in occupational composition: treated pharmacies hired substantially more SSYK 3 workers (technicians) and fewer SSYK 1 workers (managers), mechanically lowering the average wage. Once we condition on occupation, the sign flips: the establishment wage premium rises by approximately EUR 54, confirming that pharmacies in competitive markets pay more for comparable workers.

Two additional findings sharpen the interpretation. First, the wage effect is concentrated in markets with more competitive entry. Municipalities with above-median entry of new pharmacies experienced wage increases of approximately EUR 176, nearly twice the effect in low-entry markets (EUR 105). This dose-response pattern provides direct evidence that competitive pressure, rather than privatization per se, drives the wage gains (see Figure A1 in the Appendix).

Unconditional quantile regressions confirm that the wage effect is concentrated at the upper end of the distribution (Q75–Q90), with no significant effects at the median or below (Figure A2 in the Appendix).

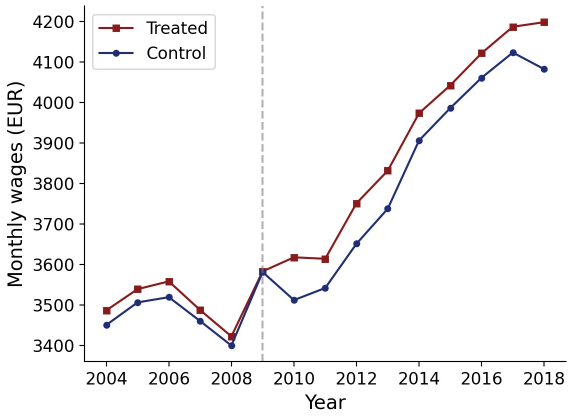
Second, employment per establishment fell by 23% (-1.72 pharmacists, se $= 0.24$) in treated municipalities, while value added per pharmacist rose by 6% and revenue productivity was unchanged, consistent with efficiency gains through workforce restructuring in a price-regulated market.[13]

Interpreting the Reduced Form. The 3.2% wage increase is likely a lower bound on the true competition effect. The control group also experienced increased competition through post-reform entry, and commuting across municipality boundaries generates spillovers that attenuate the measured treatment effect (Figure A3 in the Appendix shows the treatment

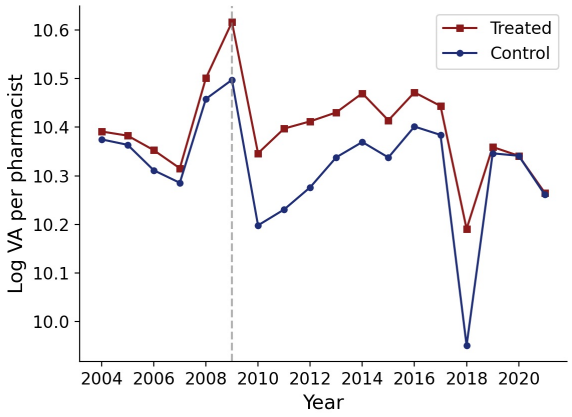
[12] All monetary values are converted from SEK to EUR at the average exchange rate of 9.5 SEK/EUR over the sample period.

[13] Production-function estimates of the monopsony markdown (Appendix B.3) confirm this pattern: the DiD estimate for the labor markdown ν is -0.177 (se $= 0.046$), with clean pre-trends and a gradual effect emerging after 2010.

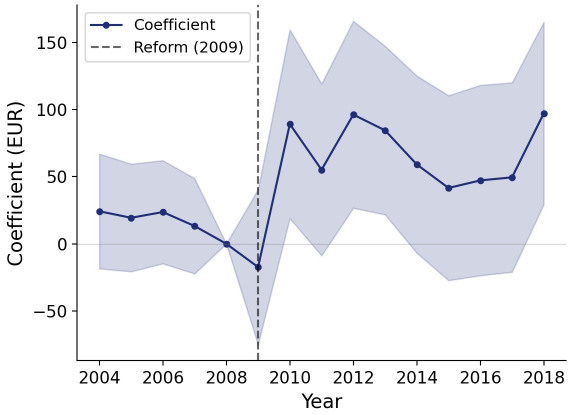
Figure 2: Effect of Deregulation on Wages and Productivity



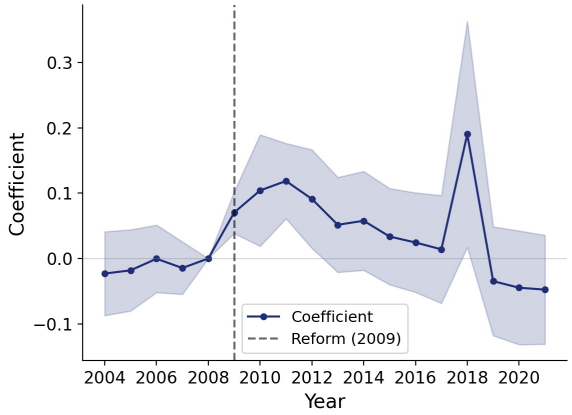
(a) Wages: treated and control means



(b) Log VA per pharmacist: treated and control means



(c) Wages: DiD coefficients



(d) Log VA per pharmacist: DiD coefficients

Note: Panels (a) and (b) plot treated and control municipality means for occupation-adjusted (SSYK) normalized monthly wages (EUR) and log value added per pharmacist, respectively. Panels (c) and (d) plot year-by-year DiD coefficients with 95% confidence intervals; the omitted category is 2008. Treated municipalities (163) are those where the reform introduced multiple competing owners; control municipalities (127) retained single-owner structure. Pre-reform mean wage: EUR 3,468 (control) vs. EUR 3,499 (treated). DiD estimate: EUR 113 (3.2% of pre-reform wages).

effect increasing with control municipality distance).

The joint response of wages and productivity helps clarify what the reduced form can and cannot identify. The occupation-adjusted wage increase of 3.2% alongside a 6% increase in value added per pharmacist implies a passthrough rate of approximately 0.5, within the range documented in the rent-sharing literature (Card et al., 2018). Under frictionless labor markets, wages equal the marginal revenue product of labor and should track productivity one-for-one in percentage terms. The imperfect passthrough of 0.5 is therefore inconsistent

with perfectly competitive labor markets and confirms the presence of labor market frictions. However, the low passthrough is consistent with multiple models: monopsony with a changing markdown, bargaining frictions, efficiency wages, or wage rigidity. Moreover, the value added increase is itself confounded by workforce restructuring (the shift toward SSYK 3 workers mechanically raises value added per pharmacist without necessarily raising the marginal product of any individual worker), making it unreliable as a proxy for changes in the marginal revenue product of labor.

The reduced form therefore establishes that competition raises wages but cannot identify the mechanism. Does the wage increase reflect reduced monopsony markdowns (firms paying a larger share of MRPL), productivity passthrough, or some combination? Answering this requires a structural model that separately identifies labor supply elasticities, productivity, and the ownership structure, combined with a conduct test that asks which model of firm behavior is consistent with the causal evidence.

5 Model

This section develops a structural model of labor market competition in the pharmacy sector. The model has two components: a discrete choice model of worker labor supply, and a model of firm wage-setting under oligopsony. The key feature is that firms internalize their market power when setting wages, leading to equilibrium wages below the marginal revenue product of labor.

5.1 Labor Supply

The foundation of our theoretical framework is modeling the labor supply decision as a discrete choice problem. In each period t, worker i selects among a finite number of jobs j, generating the following indirect utility:

$$u_{ijt} = \alpha_i w_{jt} + X_{jt}\beta_i' + \xi_{jt} + \psi_i z_{ijt} + \gamma_i \mathbb{1}[y_{it-1} \neq j] + \varepsilon_{ijt} \tag{1}$$

Jobs differ in wages (w_{jt}), non-wage amenities (X_{jt}) and distance to work and mother (z_{ijt}). Workers have idiosyncratic preferences over both wages (α_i) and their amenities (β_i, ψ_i). We allow jobs and workers to have an unobserved component in the utility function denoted by ξ_{jt} and ε_{ijt}. Workers choose every period whether to switch jobs conditional on their previous job (y_{it-1}). When workers switch ($y_{it-1} \neq j$), they incur a (dis)utility captured by γ_i. Finally, every worker has the option not to choose any of the available jobs and to leave the labor market. This option is denoted by $j = 0$ with respective utility $u_{i0} = \varepsilon_{i0}$ without loss of generality.

Assuming that the unobserved idiosyncratic preference ε_{ijt} follows a Gumbel distribution, we obtain the following formulation for labor supply:

$$L_{jt} = N_t \times \int_i \frac{\exp\left(\alpha_i w_{jt} + X_{jt}\beta_i' + \xi_{jt} + \psi_i z_{ijt} + \gamma_i \mathbb{1}[y_{it-1} \neq j]\right)}{1 + \sum_k \exp\left(\alpha_i w_{kt} + X_{kt}\beta_i' + \xi_{kt} + \psi_i z_{ikt} + \gamma_i \mathbb{1}[y_{it-1} \neq k]\right)} dF(i) \tag{2}$$

Here, N_t denotes the potential labor force in a given period. Hence, our model can be applied to either a specific occupation or the whole economy.

5.2 Labor Demand

We model labor demand as a Cournot game with multi-job firms. Every firm f sets labor demand across all jobs j they offer $(L_{jt})_{j \in J_f}$ yielding the following profit maximization problem:

$$\max_{(L_{jt})_{j \in J_{ft}}} R_f((L_{jt})_{j \in J_{ft}}) - \sum_{k \in J_{ft}} w_{kt}(\mathbf{L_t}) L_{kt} \tag{3}$$

where R_f denotes the revenue for firm f, which depends on the number of workers a firm hires across all jobs. Wages depend on labor choices across all jobs ($\mathbf{L_t}$), introducing strategic interaction between firms. In a Nash equilibrium, each firm equates its marginal revenue to its marginal costs:

$$\underbrace{\frac{\partial R_f}{\partial L_{jt}}}_{\rho_{jt}} = w_{jt} + L_{jt}\frac{\partial w_{jt}}{\partial L_{jt}} + \sum_{k \in J_{ft}/\{j\}} L_{kt}\frac{\partial w_{kt}}{\partial L_{jt}} \tag{4}$$

Here, firms internalize not only how the wage at job j affects labor supply at j, but also the cross-effects on other jobs within the firm.

6 Estimation

We estimate the choice model in Equation (2) following Train (2009), decomposing utility into a job-specific and a worker-specific component:

$$u_{ijt} = \delta_{jt} + \boldsymbol{\psi}_i' \mathbf{d}_{ijt} + \boldsymbol{\mu}_i' \mathbf{m}_{ijt} + \gamma_{ijt}\mathbb{1}[y_{it-1} \neq j] + \varepsilon_{ijt} \tag{5}$$

The first stage estimates the job intercepts δ_{jt}, distance preferences $\boldsymbol{\psi}_i$ and $\boldsymbol{\mu}_i$, and switching costs γ_{ijt} by maximum likelihood, constraining estimated market shares to match observed shares. We parameterize distance using a linear spline with knots at 5, 15, and 30 km, yielding four segments $\mathbf{d}_{ijt} = (d^1_{ijt}, \ldots, d^4_{ijt})'$. Distance from the worker to her mother

uses the same spline, $\mathbf{m}_{ijt} = (m^1_{ijt}, \ldots, m^4_{ijt})'$.

We model worker heterogeneity by interacting worker demographics with distance segments and switching costs:

$$\begin{aligned}
\psi_{ik} &= \bar{\psi}_k + \sum_l \psi_{kl} D_{il}, \quad k = 1, \ldots, 4 \\
\mu_{ik} &= \bar{\mu}_k + \sum_l \mu_{kl} D_{il}, \quad k = 1, \ldots, 4 \\
\gamma_{ijt} &= \bar{\gamma} + \sum_l \gamma_l D_{il} + \boldsymbol{\gamma}'_d \mathbf{d}_{ijt} + \boldsymbol{\gamma}'_m \mathbf{m}_{ijt}
\end{aligned} \tag{6}$$

Here, $\bar{\psi}_k$ refers to the average (dis)utility in the population for increasing distance within spline segment k. Demographic interactions (D_{il}) on all four segments allow distance sensitivity to differ by age, education, family structure, immigrant status, and city size. Mother-distance interactions follow the same structure with a subset of demographics (age, children, baby). The switching cost parameter γ_{ijt} captures the baseline disutility from job switching, its heterogeneity across worker demographics, and its interaction with distance to the new job and distance from the worker's mother.

We use distance splines rather than log distance because the log-linear form imposes a constant elasticity that overstates decay at short distances and understates it far away; the spline reveals steep decay within 15 km and near-zero marginal disutility beyond 30 km. The switching cost interacts with both distance measures, so that the cost of changing jobs depends on the geographic implications of the move.

The full specification contains 78 parameters. Since firms with attractive non-wage amenities can offer lower wages, the wage coefficient α in the mean utility δ_{jt} is biased downward. We address this endogeneity in the second stage:

$$\delta_{jt} = X_{jt}\beta' + \alpha w_{jt} + \xi_{jt} + \eta_t + \theta_m \tag{7}$$

where X_{jt} includes job characteristics, w_{jt} is the wage, ξ_{jt} captures unobserved amenities, and η_t and θ_m are year and municipality fixed effects. Pharmacies with high ξ_{jt} can offer lower wages, biasing α downward. We instrument wages using production-side variation that shifts labor demand without directly affecting worker preferences.

6.1 Results

Table 1 presents the first-stage maximum likelihood estimates from Equation (5). Each row corresponds to a base variable (distance spline segment, mother-distance spline segment, or state dependence), and each column reports the interaction with a worker demographic.

Panel A shows distance preferences and Panel B shows switching costs.

Workers strongly prefer nearby pharmacies, with the marginal disutility of distance concentrated in the 5–30 km range. The steepest decline occurs in the 5–15 km segment (-0.080 per km), consistent with the fact that nearly half of all chosen jobs lie within 5 km of the worker's home. Beyond 30 km, the coefficient is effectively zero: pharmacies at 50 km and 200 km are equally unattractive. This sharp local decay is precisely the feature that generates employer market power in a geographic labor market.

The demographic interactions reveal that distance sensitivity varies substantially across workers. In the 5–15 km segment, where most job switching occurs, older workers are significantly less sensitive to distance (the positive coefficients for ages 30–50 partially offset the negative baseline), while workers with more children are more distance-averse. Big-city workers exhibit steeper distance decay, consistent with shorter commuting norms in urban areas. Proximity to the worker's mother matters primarily for workers aged 30–40, who show a large and significant disutility from being far from their mothers (-0.254 per km within 5 km), consistent with caregiving demands during middle age.

Switching costs are large. The baseline state dependence coefficient of -6.12 implies that a pharmacist in the reference group requires a monthly wage premium of approximately EUR 4,419 to be willing to change employers.[14] This magnitude is plausible for a licensed profession: the switching cost amounts to roughly 1.3 monthly wages. The demographic pattern is intuitive: switching costs increase monotonically with age for workers over 30, reflecting declining mobility as careers mature. Workers with doctoral degrees face substantially higher switching costs (-2.13 additional), consistent with specialized human capital that limits outside options. Parents of young children (-0.92) and big-city workers (-0.81) are also less mobile.

The switching cost interacted with distance captures a complementarity between geographic and job mobility: changing to a pharmacy that requires a longer commute is costlier than switching to an equally distant one. This interaction is significant in the 0–5 km and 15–30+ km segments, reinforcing the finding that distance creates local market power.

Table 3 presents IV estimates of α using four instruments. Log value added per pharmacist captures productivity-driven wage variation; Competition measures the number of nearby pharmacies; Productivity uses TFP residuals; and Doctor Distance captures local

[14] Willingness to pay is computed as the ratio of the preference coefficient to the wage coefficient α: WTP $= |\gamma|/\alpha \times 10{,}000$. With $\alpha = 1.46$ per 10,000 SEK from Table 3, the switching cost WTP is $6.12/1.46 \times 10{,}000/9.5 \approx 4{,}419$ EUR. For distance, the marginal WTP in the 5–15 km segment is $0.080/1.46 \times 10{,}000/9.5 \approx 58$ EUR per km per month, implying a total commuting cost of about EUR 577 for a 10 km commute (17% of the average monthly wage). These magnitudes are within the range reported in the commuting cost literature (Manning, 2003; Van Ommeren et al., 2000).

Table 1: First-Stage MLE Estimates: Distance Preferences

	0–5 km	5–15 km	15–30 km	30+ km
Panel A: Distance to Work				
Base	0.046	−0.080***	−0.065***	−0.002
	(0.054)	(0.025)	(0.015)	(0.001)
Age 30–40	−0.096	0.107***	−0.032	−0.001
	(0.071)	(0.034)	(0.022)	(0.002)
Age 40–50	−0.105	0.059*	−0.008	−0.001
	(0.069)	(0.032)	(0.021)	(0.002)
Age > 50	−0.082	0.079**	−0.009	−0.001
	(0.078)	(0.036)	(0.022)	(0.002)
Doctoral	−0.160	−0.042	−0.031	0.012***
	(0.101)	(0.098)	(0.062)	(0.004)
No. Children	0.000	−0.039***	0.018*	−0.001
	(0.038)	(0.015)	(0.010)	(0.001)
Baby	−0.006	0.036	−0.013	0.000
	(0.090)	(0.036)	(0.024)	(0.003)
Foreign Born	0.054*	−0.004	0.024	−0.002
	(0.031)	(0.023)	(0.018)	(0.002)
Big City	−0.043	−0.042*	0.021	0.002
	(0.028)	(0.022)	(0.017)	(0.002)
Panel B: Distance to Mother				
Base	−0.042	0.059**	−0.027**	0.000
	(0.058)	(0.027)	(0.014)	(0.000)
Age 30–40	−0.254***	0.002	0.007	0.001
	(0.080)	(0.043)	(0.022)	(0.001)
Age 40–50	−0.098	−0.042	0.008	0.000
	(0.076)	(0.038)	(0.020)	(0.001)
Age > 50	−0.046	−0.031	−0.009	0.000
	(0.084)	(0.041)	(0.021)	(0.001)
No. Children	0.030	−0.005	−0.002	−0.000
	(0.041)	(0.017)	(0.009)	(0.000)
Baby	0.119	−0.042	0.020	−0.001
	(0.098)	(0.045)	(0.025)	(0.001)
Number of Pharmacists	8,632			
Number of Pharmacies	864			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: MLE estimates of Equation (5). Each row reports the coefficient on the indicated demographic interaction; "Base" is the reference group (workers under 30, native-born, no children, non-doctoral, outside Stockholm/Gothenburg/Malmö). Distance splines have knots at 5, 15, and 30 km; coefficients scaled by 1,000 (per km). Switching cost estimates in Table 2.

Table 2: First-Stage MLE Estimates: Switching Costs

	State Dependence
Panel A: Demographic Interactions	
Base	-6.123^{***}
	(0.195)
Age 30–40	1.747^{***}
	(0.138)
Age 40–50	1.208^{***}
	(0.136)
Age > 50	0.566^{***}
	(0.140)
Doctoral	-2.130^{***}
	(0.398)
No. Children	0.150^{**}
	(0.060)
Baby	-0.915^{***}
	(0.157)
Foreign Born	0.042
	(0.126)
Big City	-0.814^{***}
	(0.128)
Panel B: Distance Interactions	
State Dep. × Distance to Work	
0–5 km	-0.111^{**}
	(0.051)
5–15 km	-0.012
	(0.024)
15–30 km	-0.082^{***}
	(0.018)
30+ km	-0.020^{***}
	(0.003)
State Dep. × Distance to Mother	
0–5 km	-0.008
	(0.062)
5–15 km	-0.016
	(0.027)
15–30 km	-0.001
	(0.014)
30+ km	-0.001^{**}
	(0.000)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Panel A reports the base switching cost and its demographic interactions. Panel B reports interactions between switching costs and distance to the new workplace or the worker's mother. Distance interaction coefficients are scaled by 1,000 (per km).

physician intensity. Each instrument shifts labor demand and hence wages, but should not directly enter worker utility. All specifications include year and kommun fixed effects with controls for population density and amenities.

Table 3: Second-Stage IV Estimates: Wage Coefficient (α)

Instrument	α	Std. Error	F-stat (KP)	N
Log VA (Pharmacist)	1.916***	(0.483)	42.7	4,213
Competition	1.830***	(0.507)	21.1	4,237
Productivity (TFP)	1.459***	(0.368)	52.9	2,112
Doctor Distance	3.630***	(1.269)	8.3	4,237

Note: IV estimates of Equation (7) with mean utility δ_{jt} as the outcome. Wage variable is mean monthly earnings by SSYK occupation code (10,000 SEK $\approx$ 1,053 EUR). δ_{jt} recovered from first-stage MLE (Table 1). Year and kommun FE. Controls: log population density, part-time, overtime, extra work. F-stat is Kleibergen-Paap rk Wald statistic. Pooled sample 2000–2008 (pre-reform).

The estimates are stable across instruments with strong first stages: α ranges from 1.46 (TFP) to 1.92 (Log VA), with F-statistics well above 10. The Doctor Distance instrument yields a larger estimate (3.63) but a weak first stage (F = 8.3), so we treat it as a sensitivity check. We take Productivity (TFP) as our preferred instrument ($\alpha = 1.46$, F = 52.9), which has the strongest first stage among the four specifications.

The implied markdowns are substantial. The median own-wage elasticity of labor supply is 1.7, implying a monopsony markdown of approximately 37%: pharmacists' limited outside options, due to licensing requirements and geographic frictions, allow employers to set wages at roughly 63% of the marginal revenue product.

The next section uses these estimates to test which models of firm wage-setting are consistent with the quasi-experimental evidence.

7 Testing Models of Firm Conduct

We test which models of firm wage-setting are consistent with the quasi-experimental evidence. The approach is to predict counterfactual wages under alternative conduct assumptions using the estimated labor supply model, and compare these predictions to the causal effect identified by the reduced-form DiD.

7.1 Counterfactual Design

Our counterfactual exercise asks: what would wages have been in 2010 if the ownership structure had changed but worker preferences and productivity remained at their 2008 levels? This isolates the pure competition effect from other changes that occurred around the reform.

Specifically, we fix worker preference parameters and the marginal revenue product of labor (ρ) at each establishment at their 2008 levels, change the ownership structure to the post-reform (2010) configuration, and solve for the new equilibrium wages. For each model of conduct, we then compute the model-implied DiD and compare it to the empirical DiD of EUR 113 (+3.2%). If the model-implied DiD matches the empirical DiD, the model is consistent with the data; if it differs significantly, the model is rejected.

Our approach sidesteps the pairwise model testing used by Backus et al. (2021) and Roussille and Scuderi (2025), who estimate markdowns under two competing conduct models and use exclusion restrictions to discriminate between them. Instead, we compare a single model's predictions against the actual data, using the quasi-experimental shock in market structure as the benchmark. This requires no exclusion restriction; the identifying variation comes from the deregulation itself.

The counterfactual requires a measure of the marginal revenue product of labor (ρ_j) at each establishment. In principle, one could back out MRPL from the firm's first-order condition, but this requires taking a stance on pre-reform conduct. Since the pre-reform market was a state monopoly with potentially idiosyncratic wage-setting behavior, we opt for a conduct-free approach. Following Yeh et al. (2022), we estimate plant-level labor markdowns ν_j from a pooled translog production function on balance-sheet data (sales and materials) and compute $\rho_j = \nu_j \times w_j$. This measure depends only on the revenue side and is independent of the ownership matrix, the labor supply model, or any assumption about how firms set wages. The median pre-reform markdown is $\nu = 1.71$, implying that the monopolist paid pharmacist wages at approximately 58% of their marginal revenue product. We trim ν to the [p10, p90] range of the 2008 distribution ($[0.647, 2.147]$) and apply a uniform level shift of approximately EUR 1,389 to ensure $\rho_j > w_j$ for all establishments, as required for interior equilibrium.[15]

A key feature of the pre-reform data is that wages are nearly flat across pharmacies (IQR $\approx$ EUR 95) and uncorrelated with establishment-level MRPL (Spearman rank correlation $= -0.03$): MRPL ranges from near zero to EUR 7,895/month, while wages cluster in a narrow EUR 2,947–3,789 band (Figure A4 in the Appendix). This suggests that the state monopolist set wages according to non-market conditions such as seniority, demographics, and institutional norms rather than pharmacy-level productivity.

[15] Appendix B provides a detailed comparison of production-function specifications and discusses why the pooled translog is preferred.

7.2 Wage Equation and Conduct Parameters

We parameterize the degree to which post-reform wage-setting responds to market conditions. The generalized wage equation blends the profit-maximizing wage with the pre-reform wage:

$$w_j = \phi \cdot w_j^* + (1 - \phi) \cdot \bar{w}_j \tag{8}$$

where $\bar{w}_j$ is the pre-reform (2008) wage, w_j^* is the profit-maximizing wage under the post-reform ownership structure, and $\phi \in [0, 1]$ measures the degree of market-based wage-setting. At $\phi = 0$, wages remain at pre-reform levels; at $\phi = 1$, firms fully optimize.

The profit-maximizing wage is derived from the firm's first-order condition. For establishment j owned by firm f, the wage equation is:[16]

$$w_j^* = \rho_j - \underbrace{\kappa_0 \cdot L_j \left(\frac{\partial L_j}{\partial w_j} \right)^{-1}}_{\text{own markdown}} - \underbrace{\kappa_1 \cdot \sum_{k \in \mathcal{J}_f \setminus \{j\}} L_k \left(\frac{\partial L_k}{\partial w_j} \right)^{-1}}_{\text{cross markdown}} \tag{9}$$

where L_j is employment at establishment j and $\mathcal{J}_f$ is the set of establishments owned by firm f. The labor supply derivatives follow from the logit model in Section 5. The parameter $\kappa_0 \in [0, 1]$ governs the own-establishment markdown, while $\kappa_1 \in [0, 1]$ governs cross-establishment internalization. At $\kappa_0 = \kappa_1 = 1$, the firm fully optimizes; at $\kappa_0 = \kappa_1 = 0$, wages equal MRPL (competitive).

We also allow for diminishing returns through a parameter $\eta \geq 0$ that adjusts MRPL based on how the counterfactual employment share s_j deviates from the pre-reform share s_j^{pre}:

$$\rho_j(\eta) = \rho_j \cdot \left(\frac{s_j^{\text{pre}}}{s_j} \right)^{\eta} \tag{10}$$

When $\eta = 0$, MRPL is constant regardless of employment. When $\eta > 0$, a pharmacy that loses workers under the counterfactual ($s_j < s_j^{\text{pre}}$) sees its marginal product rise, reflecting the scarcity of the remaining workforce, which dampens the predicted wage increase. This captures a form of adjustment cost on the production side: pharmacies cannot instantly reorganize production around a different workforce size. While ϕ governs rigidity on the wage margin, η governs rigidity on the employment margin; both dampen the model's predicted wage response to the shock in market structure. We also allow the reform to affect

[16] The counterfactual simulation uses the wage coefficient $\alpha = 1.46$ per 10,000 SEK ($\approx$ EUR 1,053) from the Productivity (TFP) instrument (Table 3), which has the strongest first stage ($F = 52.9$). The qualitative conclusions are robust to the choice of α within the range of our IV estimates.

productivity through an efficiency parameter $\lambda > 0$:

$$\rho_j(\lambda) = \lambda \cdot \rho_j \tag{11}$$

At $\lambda = 1$, productivity is unchanged; $\lambda > 1$ allows the reform to raise MRPL (e.g., through better management practices).

We solve for equilibrium wages numerically. The Cournot FOC in Equation (9) defines a fixed-point problem, which we solve using a two-phase approach: first, we obtain a Bertrand (wage-competition) solution using the ζ-decomposition of Morrow and Skerlos (2011), guaranteed to converge under logit demand; second, we use this solution to warm-start the Cournot iteration with adaptive damping, converging to a tolerance of EUR 0.1.

7.3 Results

Full market optimization ($\phi = 1$). Table 4 presents counterfactual results across a range of conduct parameterizations at $\phi = 1$. The central finding is that every specification overshoots the empirical DiD by an order of magnitude. Full strategic oligopsony ($\kappa_0 = 1$, $\kappa_1 = 1$) produces a model-implied DiD of EUR 1,161, roughly ten times the target. Reducing cross-establishment internalization to zero ($\kappa_1 = 0$) increases the overshoot to EUR 1,336. Even the most favorable specification ($\kappa_0 = 0.5$, $\kappa_1 = 1$, $\eta = 1.0$) produces a DiD of EUR 563, still five times too large. The rejection is not about the specific form of strategic interaction; it is about the degree of market-based wage optimization itself.

Since the rejection concerns the degree of market optimization rather than the specific form of strategic interaction, Panel B relaxes the assumption of full optimization by varying ϕ. Matching the empirical DiD requires $\phi \in [0.02, 0.05]$: the magnitude of wage adjustment is far smaller than any standard model of market-based wage-setting predicts. This range is robust across MRPL specifications and IV estimates of α. Combining low ϕ with alternative conduct parameters (Panel C) does not change the conclusion: no specification with $\phi \geq 0.2$ achieves the target, regardless of κ_0, κ_1, or η.

7.4 Interpretation

Across all specifications, competition following the reform moved wages in the direction predicted by strategic models, but the magnitude of adjustment was far smaller than any standard parameterization predicts. An alternative interpretation is that the overshoot reflects misspecification of the labor supply model rather than incorrect conduct. However, the insensitivity of the result to conduct parameters (κ_0, κ_1, η, λ) argues against this: supply-side misspecification would typically interact with the conduct parameterization, whereas we

Table 4: Counterfactual Results

ϕ	κ_0	κ_1	η	λ	Model-implied DiD	Mean wage
Empirical benchmark						
—	—	—	—	—	**113**	3,415
Panel A: Full market optimization ($\phi = 1$)						
1.0	1.0	1.0	0.0	1.0	1,161	4,543
1.0	1.0	0.5	0.0	1.0	1,228	4,667
1.0	1.0	0.0	0.0	1.0	1,336	4,868
1.0	0.5	1.0	0.0	1.0	1,012	4,932
1.0	1.0	1.0	0.3	1.0	971	3,924
1.0	1.0	1.0	1.0	1.0	667	3,544
1.0	0.5	1.0	1.0	1.0	563	3,930
1.0	1.0	1.0	0.0	0.75	1,030	3,088
1.0	1.0	1.0	0.0	1.25	1,156	5,895
Panel B: Partial market optimization						
0.50	1.0	1.0	0.0	1.0	869	3,794
0.20	1.0	1.0	0.0	1.0	463	3,495
0.10	1.0	1.0	0.0	1.0	255	3,441
0.05	1.0	1.0	0.0	1.0	142	3,423
0.02	1.0	1.0	0.0	1.0	62	3,417
Panel C: ϕ interactions with conduct parameters						
0.20	1.0	1.0	0.3	1.0	421	3,466
0.20	1.0	1.0	1.0	1.0	362	3,430
0.20	0.5	1.0	0.0	1.0	342	3,671
0.30	1.0	1.0	1.0	1.0	443	3,446
0.30	0.5	1.0	0.0	1.0	498	3,805
0.30	0.5	1.0	1.0	1.0	323	3,650

Note: Model-implied DiD and mean counterfactual wage in EUR (1 EUR $\approx$ 9.5 SEK). The empirical DiD is EUR 113 (+3.2%). Parameters: ϕ (market optimization, Eq. 8), κ_0 (own markdown, Eq. 9), κ_1 (cross-establishment internalization, Eq. 9), η (diminishing returns, Eq. 10), λ (efficiency, Eq. 11). Panel A: at $\phi = 1$, every parameterization overshoots the target by 5–12×. Panel B: matching the data requires $\phi \in [0.02, 0.05]$. Panel C: no interaction of $\phi \geq 0.2$ achieves the target.

find that no combination of parameters brings the model within range at $\phi = 1$. Pre-reform, the state monopolist set wages according to seniority, demographics, and institutional norms rather than pharmacy-level productivity. Post-privatization, new owners primarily restructured employment (headcount fell 23%, value added per pharmacist rose 6%) rather than adjusting wages. The +3.2% wage effect documented in Section 4 is the small residual of a reform whose main impact was on the employment margin.

The conduct models tested here all assume unilateral wage-setting by the firm. In future work, we plan to extend the framework to alternative wage-setting mechanisms, including Nash bargaining models where the wage splits the bilateral surplus between firm and worker (Lachowska et al., 2022). The quasi-experimental design extends naturally to these models: any wage-setting mechanism that generates counterfactual wages can be tested against the empirical DiD.

8 Conclusion

This paper studies how labor market competition affects wages, using the 2009 Swedish pharmacy deregulation as a natural experiment. Our reduced-form analysis shows that wages increased by approximately 3.2% in markets that experienced a bigger shock in labor market competition, while employment fell 23% and value added per pharmacist rose 6%.

Our structural estimates reveal the mechanisms underlying this effect. The discrete choice model uncovers substantial switching costs and commuting disutility, which generate inelastic labor supply and enable employer market power. Workers require compensation of approximately EUR 4,419 per month to switch jobs and around EUR 58 per km per month in commuting disutility, with the marginal cost of distance concentrated in the 5–30 km range and near zero beyond 30 km.

The conduct test produces a clear finding. Every standard model of market-based wage-setting, across a range of conduct parameterizations, overshoots the quasi-experimental evidence by an order of magnitude. Competition moves wages in the expected direction, but the magnitude of adjustment is far smaller than strategic oligopsony models predict: matching the data requires $\phi \in [0.02, 0.05]$, meaning only 2–5% of wage-setting shifted toward market optimization. Pre-reform wages bore no relationship to pharmacy-level productivity (Spearman rank correlation $= -0.03$), consistent with wage-setting driven by seniority, demographics, and institutional norms. The reform's primary effect was on the employment margin, not the wage margin.

Our set identification approach involves a trade-off relative to existing conduct tests that use exclusion restrictions to point-identify conduct (Backus et al., 2021; Roussille and Scuderi, 2025). By grounding identification in a credible quasi-experiment rather than functional

form assumptions, we recover a set rather than a point. For antitrust enforcement, this identified set provides a range of defensible conduct assumptions for merger simulation.

These findings speak directly to the growing policy interest in labor market competition. As the U.S. and European competition authorities incorporate labor market effects into merger review (U.S. Department of Justice and Federal Trade Commission, 2023), practitioners need tools to assess whether firms exercise monopsony power and how consolidation would affect wages. Our framework provides such a tool: by combining a structural model with quasi-experimental validation, it disciplines the counterfactual predictions that underpin merger simulation in labor markets.

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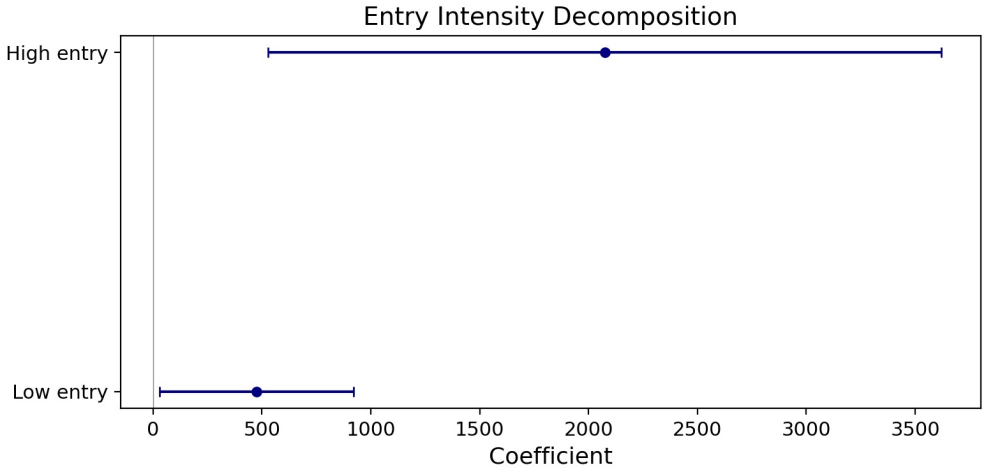
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Appendix

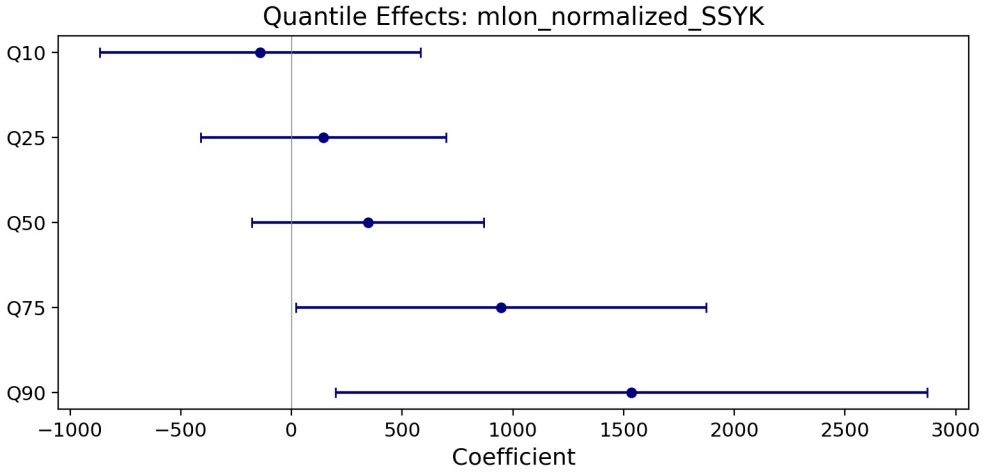
A Additional Reduced-Form Evidence

Figure A1: DiD Estimates by Entry Intensity



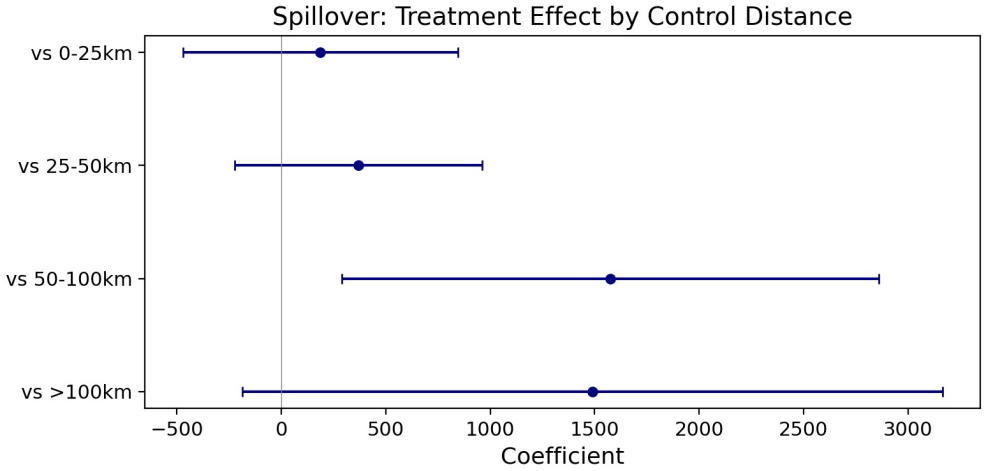
Note: DiD estimates of the effect of deregulation on occupation-adjusted wages, split by entry intensity. High-entry municipalities are those with above-median entry of new pharmacies after the reform. Bars show 95% confidence intervals.

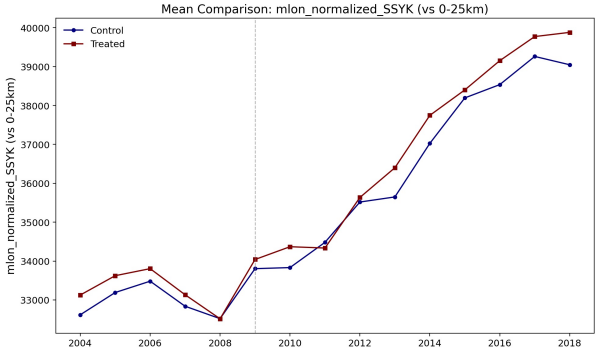
Figure A2: Quantile DiD Effects on Establishment Wage Premium



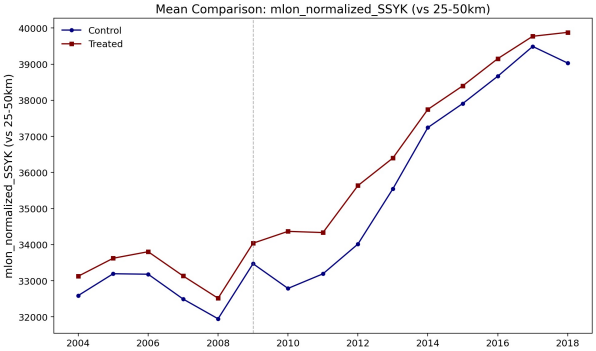
Note: Quantile difference-in-differences estimates of the effect of deregulation on occupation-adjusted wages. Each point represents the DiD estimate at the corresponding quantile, with 95% confidence intervals. Effects are concentrated at Q75–Q90, with no significant effects at the median or below.

Figure A3: Treatment Effect by Control Municipality Distance

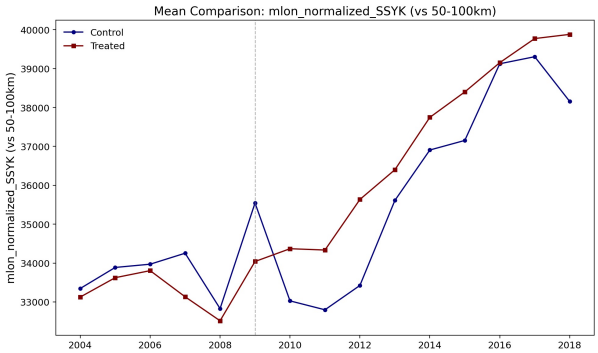




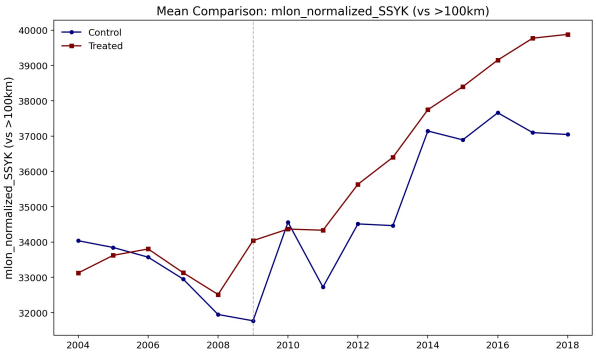
(a) Control: 0–25 km



(b) Control: 25–50 km



(c) Control: 50–100 km



(d) Control: >100 km

Note: The top panel plots DiD coefficients for occupation-adjusted wages using control municipalities stratified by Euclidean distance to the nearest treated municipality centroid. Panels (a)–(d) show treated vs. control means over time for each distance ring. The treatment effect is larger when compared to more distant controls, consistent with competitive spillovers attenuating the measured effect for nearby control municipalities.

B Production-Function MRPL Estimation

We estimate plant-level labor markdowns following Yeh et al. (2022), using a two-input production function (pharmacists L, materials M) on balance-sheet data from the LVE register merged with pharmacist counts and wages. Output elasticities θ_L and θ_M yield the markup $\mu = \theta_M/\alpha_M$ and markdown $\nu = (\theta_L/\alpha_L)/\mu$, where α_L and α_M are revenue shares.

We estimate three specifications: year-by-year Cobb-Douglas, pooled Cobb-Douglas, and pooled translog. Table A1 compares the results.

Table A1: Production-Function Estimation: Method Comparison

Method	Period	n	θ_L	θ_M	$\sum\theta$	ν median	$\% > 5$
Cobb-Douglas	pre	4,837	0.268	0.664	0.93	5.95	56.6%
Pooled CD	pre	4,837	0.330	0.591	0.92	7.08	81.1%
Pooled translog	**pre**	**4,837**	**0.121**	**0.876**	**1.00**	**1.71**	**0%**
Pooled translog	**post**	**8,120**	**0.106**	**0.906**	**1.01**	**1.06**	**0%**

Note: Pre-reform = 2003–2008, post-reform = 2009–2018. θ_L, θ_M are median output elasticities. ν is the labor markdown (MRPL/wage). Cobb-Douglas methods produce extreme markdown levels ($\nu > 5$ for majority of observations). The pooled translog yields constant returns to scale ($\sum\theta \approx 1$), plausible markdown levels, and no extreme outliers.

The pooled translog is preferred for three reasons. First, it yields constant returns to scale ($\sum\theta \approx 1.0$), consistent with pharmacy production. Second, pre-reform median $\nu = 1.71$ is within the range of Yeh et al. (2022)'s U.S. manufacturing estimates (median $\nu = 1.36$, mean $= 1.53$). Third, it produces no extreme outliers, whereas Cobb-Douglas methods generate $\nu > 5$ for 57–81% of observations.

A key feature of the data is the structural break in the MRPL-wage correlation around the reform. Table A2 reports the correlation between MRPL and wages by year for the pooled translog specification.

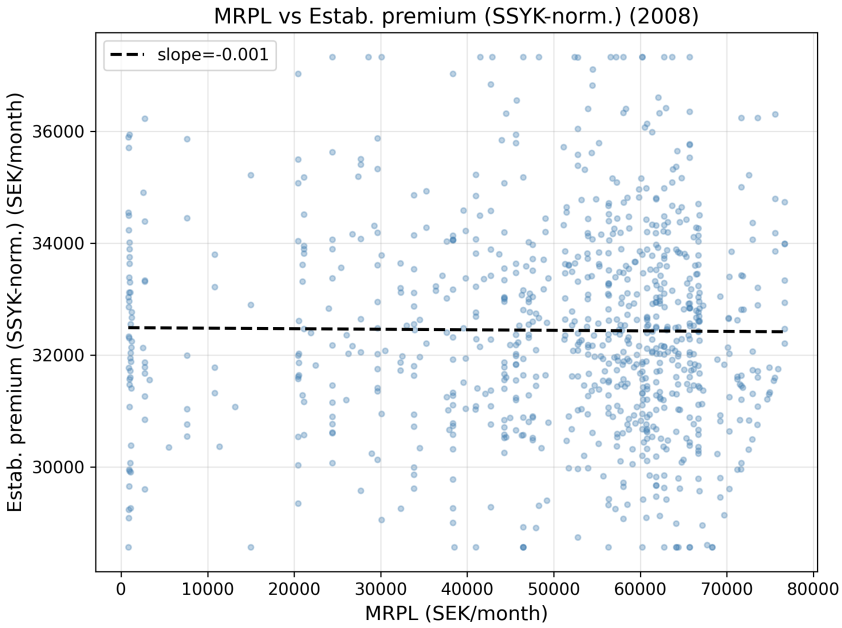
Pre-reform, the correlation is effectively zero: the state monopolist set wages independently of pharmacy-level productivity. After the 2009 reform, the correlation gradually strengthens to +0.26 by 2011, consistent with competitive pressure slowly shifting wage-setting toward market-based conditions. This pattern appears only in the translog methods; Cobb-Douglas methods confound MRPL with establishment size, producing negative correlations throughout.

Table A2: Correlation between MRPL and Wages over Time

Year	corr(MRPL, wage)
2003	−0.00
2008	−0.01
2009	+0.03
2011	+0.26
2014	+0.24
2018	+0.22

Note: Pearson correlation between pooled translog MRPL ($\nu \times w$) and observed wages, by year. Pre-reform, wages are uncorrelated with productivity; post-reform, the correlation gradually strengthens, consistent with competition shifting wage-setting toward market conditions.

Figure A4: Pre-Reform MRPL and Wages (2008)



Note: Each dot represents one pharmacy in 2008. MRPL is computed from the pooled translog production function ($\nu \times w$). Under competitive wage-setting, the slope would be 1; the observed slope near zero indicates the monopolist set wages independently of pharmacy-level productivity.

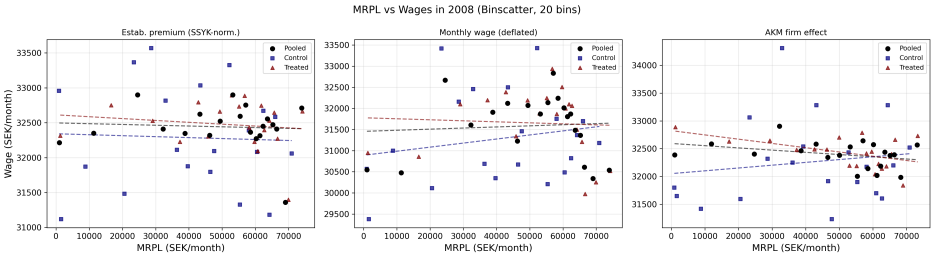
B.1 MRPL-Wage Scatter

B.2 MRPL-Wage Binscatter

B.3 Monopsony Markdown Trajectories

Figure A6 plots the mean monopsony markdown ν for treated and control municipalities over 2004–2018. Pre-reform, treated municipalities had higher markdowns ($\nu \approx 1.6$, implying

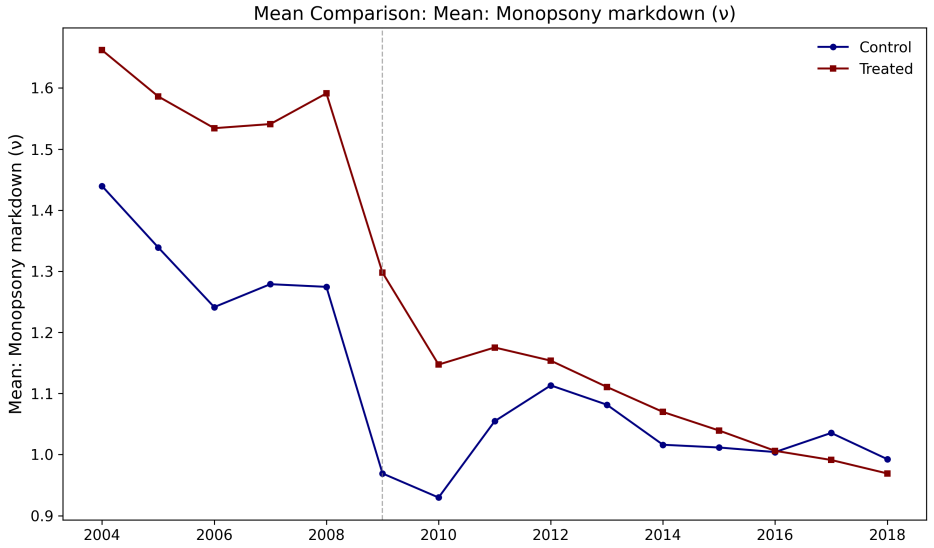
Figure A5: MRPL-Wage Binscatter (2008)



Note: 20-bin binscatter of production-function MRPL ($\nu \times w$) against three wage measures (establishment premium, AKM firm effect, raw wages) in 2008. Pooled (black) and by treatment status. All three wage measures show flat or slightly negative slopes, consistent with the monopolist setting wages independently of establishment-level productivity.

wages at 63% of MRPL) than control ($\nu \approx 1.3$). After the reform, the gap closes rapidly: by 2014, both groups converge to $\nu \approx 1.0$ (competitive wage-setting). The convergence is driven by treated municipalities, where competition eroded the monopsony wedge.

Figure A6: Mean Monopsony Markdown by Treatment Status



Note: Mean monopsony markdown $\nu = \text{MRPL/wage}$ from the pooled translog production function, by treatment status. $\nu > 1$ implies wages below marginal product; $\nu = 1$ is competitive.