

How Do Firms Exercise Labor Market Power?

Evidence from Swedish Pharmacies*

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Abstract

A growing body of evidence shows that firms have labor market power, but less is known about how much of it they exercise. We study wage-setting conduct using the 2009 break-up of the Swedish pharmacy monopoly, combining quasi-experimental variation in market structure with matched employer-employee data and a novel model that uses internalization parameters to allow for partial exercise of labor market power. We find that firms exercise a quarter of their potential labor market power. At this estimate, the break-up raised wages by 3.3 percent, against 11.4 percent under full exercise of labor market power. This result has two implications. First, models of full exercise can overpredict how strongly wages respond. Second, small wage responses to demand shocks may reflect partial exercise rather than an absence of labor market power.

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1 Introduction

Over the past two decades, there has been a meteoric rise in empirical work quantifying labor market power, with much of the focus on estimating the elasticity of labor supply (Caldwell et al., 2026). These advances have in turn shaped debates over minimum wages, collective bargaining, and more recently antitrust enforcement in labor markets (Card, 2022; Azar et al., 2024; Prager and Schmitt, 2021; Azar and Marinescu, 2024; Prager, 2026). Much less is known about how much firms exercise their potential labor market power. In standard models, wages depend on three components: the marginal revenue product of labor, labor supply, and firm conduct. While the marginal revenue product and labor supply determine the potential for wage-setting power, conduct dictates how firms exercise their power.

Most studies assume a model of conduct, typically monopsonistic competition (e.g., Kroft et al., 2025; Volpe, 2026) or, more rarely, oligopsony (e.g., Berger et al., 2022; Jarosch et al., 2024). This assumption sits uneasily with two strands of evidence. First, there is evidence that firms do not fully exercise their labor market power. Wage setting reflects imperfect optimization (Dube et al., 2025), fairness norms (Dube et al., 2019) and organizational constraints (Hazell et al., forthcoming), all of which standard models leave out. Second, when standard models of monopsony are taken to the data, they tend to overpredict how strongly wages and markdowns respond (Derenoncourt and Weil, 2025; Faia et al., 2026). Despite its importance for evaluating mergers and labor market policies, there is no evidence on how much of their potential labor market power firms exercise (Caldwell et al., 2026).

This paper provides the first evidence of how much firms exercise their *potential* labor market power. We develop a labor demand model in which internalization parameters map firms' potential market power into wages, nesting the familiar cases of monopsonistic competition and oligopsony. We estimate these parameters using the 2009 break-up of the Swedish pharmacy monopoly. We find that firms exercise only a quarter of their potential labor market power. This matters for wages. At our estimate, the break-up raised wages by 3.3 percent, against no change under monopsonistic competition, where a pharmacy sets the same wage whether its owner runs one pharmacy or several, and 11.4 percent under full exercise. This result has two implications. First, models of full exercise can overpredict how strongly wages respond. Second, small wage responses to demand shocks may reflect partial exercise rather than an absence of labor market power.

Three challenges have so far hindered the estimation of internalization parameters in labor markets, and the break-up allows us to address each of them. First, estimation requires a well-defined labor market in which employers compete for workers by setting wages. Pharmacists are high-skill workers whose wages are set by managers rather than collective agreements, and each pharmacy has an incentive to make profits. Pharmacists also form a narrow, licensed occupation, and those who work in a retail pharmacy rarely leave the sector. Second, estimation requires a measure of potential labor market power. Concentration indices and production functions measure equilibrium outcomes, so they reflect, at best, how market power is exercised. Using matched

employer-employee data, we instead observe the full pool of potential pharmacists, which pharmacy they chose, and where they live, which allows us to estimate a flexible labor supply model with both job differentiation and frictions. Third, potential market power is likely endogenous to productivity. Employers that offer attractive jobs have more market power, but they are often also more productive. The break-up shifted potential market power in some local labor markets but not in others. Since prescription drug prices are regulated and pharmacists are mainly hired to dispense them, the break-up plausibly held the marginal revenue product of labor fixed.

We begin by showing that the change in market structure indeed moved wages. The break-up was organized so that about half of the pharmacies were grouped into eight clusters. These clusters were then sold to four private chains causing about half of the municipalities to go from one to several owners, while the other half kept a single owner. Studying the wage effect across these municipalities, we find that wages increase by 2 percent, which cannot be explained by changes in productivity, measured as the prescription margin per pharmacist. Since this was a nationwide shock and labor markets are likely to extend across municipal borders, it is likely that spillovers are present making our results a lower bound. Indeed, we find that restricting the control group to municipalities further away increases the wage effect. Furthermore, the break-up was paired with an influx of new pharmacies. This quasi-experimental evidence alone therefore cannot tell us how much labor market power firms exercise. The wage effect mixes the change in ownership with spillovers and entry. Moreover, the same wage effect could come from a large change in potential labor market power that firms partly exercise or from a small change that they fully exercise.

To understand the mechanism behind this wage effect and to measure how much of their potential labor market power firms exercise, we develop a model of both labor supply and labor demand in the spirit of Berry et al. (1995) that we estimate using matched employer-employee data. Following an increase in competition, positive wage effects can only be realized with strategic firms that internalize upward-sloping labor supply curves. More precisely, pharmacies that internalize how their wages affect labor supply at co-owned pharmacies should raise wages after the break-up. A pharmacy no longer has to take into account that hiring more pharmacists bids up wages at its former sister pharmacies. We therefore estimate internalization parameters, to determine the extent to which chains internalize the competition between the pharmacies they run. The model also allows us to isolate the total effect of introducing competition in the labor market. That is, in the model we can replicate the break-up holding all else constant, and show how its wage effect varies with the degree of internalization.

We first develop a model of labor supply based on a discrete choice model with heterogeneous utility (Train, 2009). Each licensed pharmacist chooses to work at a pharmacy based on three components. First, pharmacists value the wage and the amenities of a job. Second, pharmacists take into account the distance to their home and to their parents. Third, as labor markets are frictional, pharmacists incur a disutility when they switch jobs. We furthermore allow each pharmacist to have heterogeneous utilities towards each of these three components based on age, number of children

and other demographics. We estimate the model in two steps. We first recover the preferences for distance and for switching pharmacies from the choices of individual pharmacists, and then recover wage preferences by instrumenting wages with shifters of labor demand (Berry et al., 1995). The estimates deliver finite elasticities of labor supply to each pharmacy with respect to its own wage and to the wages of all other pharmacies, in line with the literature on monopsony, as well as the wage equivalents of commuting and of losing the current job. We find a median own-wage elasticity of 1.8, a cost of commuting of 2.1 percent of the wage per kilometer, and that losing their current pharmacy costs pharmacists as much as a wage cut of 32 percent. Because we observe the full pool of pharmacists and where they live and work, we can estimate these preferences flexibly, and the estimates are plausible. The median elasticity is in line with, if somewhat below, the median of 2.1 across the 91 studies surveyed by Caldwell et al. (2026), as one would expect in a narrow professional labor market.

We now turn to our main contribution, a model of labor demand that maps this potential labor market power into wages. Pharmacies compete for pharmacists by choosing how many to employ, given their labor supply curves and their marginal revenue product. We allow each pharmacy to internalize how a wage change affects labor supply both to itself and to its co-owned pharmacies in the same region. We refer to the corresponding components as the own-markdown and the cross-markdown. The cross-markdown is the labor market counterpart of the cannibalization that a multiproduct firm weighs when it sets prices. A higher wage at one pharmacy draws workers from the chain’s other pharmacies, just as a lower price for one product draws sales from the firm’s other products. By allowing flexible weights on both markdowns, our equilibrium wage equation nests the canonical models of monopsonistic competition and oligopsony. Under monopsonistic competition, pharmacies disregard how their wages affect other pharmacies, including those of their own chain, so ownership plays no role for wages. Under oligopsony, pharmacies account for these effects but treat the other pharmacies of their chain as competitors. Under full internalization, each pharmacy also maximizes the joint profit of its chain, and pharmacies fully exercise their potential labor market power. We estimate these weights by adapting an approach from the industrial organization literature (Miller and Weinberg, 2017) to the labor market. Instrumenting the markdown components with the variation generated by the break-up, we find that pharmacies only partially exercise their labor market power. While pharmacies mostly ignore the strategic part of their own-markdown, they put a weight of about 0.3 on the cross-markdown. In sum, a pharmacy effectively exercises about 25 percent of its potential labor market power. Our estimate is robust to heterogeneity. The new chains and the former monopolist internalize alike, and the weight does not differ between dense and sparse labor markets or between pharmacies that did and did not change their manager. Partial internalization is thus not a legacy of state ownership.

Our estimated model then allows us to run counterfactual simulations. We simulate the market after the reform as if the incumbent pharmacies in each commuting zone still had a single owner, holding marginal revenue and the pool of pharmacists fixed. At our estimate of internalization,

the break-up raised wages at the average incumbent pharmacy by 3.3 percent, about one and a half times the quasi-experimental estimate. Unlike the quasi-experimental estimate, the simulation leaves out entry and includes the spillovers to control municipalities. With no internalization, that is, under monopsonistic competition, ownership does not enter the wage equation and the break-up would have left wages unchanged. With full internalization, wages would have risen by 11.4 percent, more than three times our estimate.

Our results have implications for labor economics well beyond the pharmacy sector. Much of the literature measures labor market power through the elasticity of labor supply, which captures its potential (Caldwell et al., 2026). Equating this potential with its exercise can mislead in two ways. Ex ante, quantifying the wage effects of mergers or other policies from labor supply alone risks overpredicting them. In our setting, full exercise implies a wage effect of the break-up more than three times our estimate. Ex post, reading quasi-experimental evidence as full exercise risks rejecting monopsony models. Two recent examples are Derenoncourt and Weil (2025), who find that firms respond little to wage increases at competing employers, and Faia et al. (2026), who find that markdowns respond much less to a binding minimum wage than the standard monopsony model predicts. Such evidence is also consistent with firms that have substantial potential labor market power but exercise only part of it. Partial exercise likewise reconciles conduct tests that favor monopsonistic competition (Roussille and Scuderi, 2025) with evidence that mergers lower wages (Arnold, 2019; Prager and Schmitt, 2021). While we study a single occupation, the distinction between potential and exercise applies wherever labor market power is inferred from labor supply. The share that firms exercise may differ across markets, but our approach carries over to any setting in which a change in ownership, such as a merger or a divestiture, shifts potential labor market power while leaving the marginal revenue product of labor unchanged. Understanding labor demand is therefore just as important as understanding labor supply.

Relation to the Literature. Our paper speaks to the literature on how wages are set. When labor supply elasticities are finite, the size of markdowns depends on how firms internalize their wage-setting power. The literature currently models this internalization in two broad ways. The large majority of papers that make an assumption on labor demand abstract from strategic interactions (e.g., Card et al., 2018; Lamadon et al., 2022; Kroft et al., 2025). The explicit assumption is that firms do not internalize that their wage-setting behavior changes the behavior of competitors. Firms then either set wages equal to the marginal revenue product of labor (perfect competition) or face a constant labor supply elasticity (monopsonistic competition) (Caldwell et al., 2026). In our general model of labor demand, monopsonistic competition corresponds to firms internalizing neither the strategic part of the own-markdown nor the cross-markdown. While this assumption keeps these models tractable, it implies that shifts in market structure or ownership do not affect equilibrium behavior. A second strand does allow for strategic interactions, albeit not always allowing for ownership. Berger et al. (2025), for instance, extend Berger et al. (2022) to allow for mergers. When firms internalize strategic interactions, they have a reason to adjust wages when

competition changes. We explicitly separate two channels of strategic interaction: firms taking into account their competitors, the strategic part of the own-markdown, and firms taking into account that they share an owner with some of those competitors, the cross-markdown.¹ Our paper is the first to distinguish between these as our model of labor demand nests all of them. The closest paper to ours is Roussille and Scuderi (2025), who conduct a pairwise test of strategically small firms against firms that internalize their own markdown. We differ from them in two ways. First, we directly estimate internalization parameters, rather than comparing two models, and find that firms internalize about 30 percent of their cross-markdowns. Second, we allow firms to internalize the cross-markdown, which arises when employers share an owner and which their test does not consider. Like them, we find that firms largely ignore the strategic part of their own-markdown. Partial internalization of the cross-markdown then explains why wages rose after the break-up, which monopsonistic competition cannot, and why they rose by much less than full internalization predicts.

A second body of work studies labor demand using quasi-experimental evidence. A large part of it relates wages to employer concentration, measured by the Herfindahl-Hirschman Index (e.g., Azar et al., 2022b; Benmelech et al., 2022; Rinz, 2022; Thoreson, 2024).² Such regressions have been criticized because concentration and wages are joint equilibrium outcomes, so that the relation between these objects does not reveal how firms set wages no matter the instrument (Bresnahan, 1989; Berry et al., 2019). Another literature studies wage setting through shocks such as mergers or minimum wages. Some of this evidence shows that firms respond little to wage increases at competing employers and to local labor market conditions (Derenoncourt and Weil, 2025; Hazell et al., forthcoming). Other evidence shows the opposite, with mergers that sharply raise local concentration lowering wages (Arnold, 2019; Prager and Schmitt, 2021). Markdowns also respond much less to a binding minimum wage than the standard monopsony model predicts (Faia et al., 2026). Partial internalization offers a way to reconcile this mixed evidence. When firms only partly internalize strategic interactions, not every shock delivers a full response.

More broadly, our study relates to the literature on monopsony, which dates back to Robinson (1933) but was rejuvenated by empirical evidence on minimum wages (Card and Krueger, 1994) and by two new theoretical frameworks. The first grounds monopsony in frictional labor markets (Burdett and Mortensen, 1998; Jarosch et al., 2024), the second in differentiated jobs (Card et al., 2018; Lamadon et al., 2022; Berger et al., 2022). We bridge the two by developing a choice model that combines differentiated jobs with frictions, captured by commuting distance and a state-dependence term. We contribute to this literature both by characterizing the sources of finite labor supply elasticities and by adopting models from industrial organization to understand wage

¹A further group of models lets employers collude (Delabastita and Rubens, 2025; Sharma, 2025). Finding significant internalization of cross-markdowns is in itself a rejection of full collusion. In ongoing work, we extend our model to allow for partial collusion between formerly jointly owned pharmacies.

²This approach has also been applied to the reform we study. Thoreson (2024) relates the wages of individual pharmacy workers to the Herfindahl-Hirschman Index of local employer concentration, instrumented with the concentration created by the break-up. We instead focus on firm behavior using job-level wage premia.

setting. In a companion paper, we estimate the Job Diversion Matrix, scaling up our estimation of labor supply to estimate the potential labor market power across the entire Swedish economy (Friederiszick et al., 2026).

Finally, our work is related to the industrial organization literature on the exercise of product market power. Berry and Haile (2014) show that conduct is testable when instruments shift markups (here, markdowns) but not marginal costs (here, the marginal revenue product of labor). This literature has developed two approaches. The first compares two models of conduct against each other using a Rivers–Vuong test, as developed for this purpose by Duarte et al. (2024) and Backus et al. (2021) and subsequently applied to the labor market by Roussille and Scuderi (2025) and Sharma (2025). The second assumes a class of models governed by an internalization parameter and estimates that parameter, as in Miller and Weinberg (2017) and Crawford et al. (2018). To our knowledge, this paper is the first to estimate such a parameter in the labor market.

Our paper proceeds as follows. Section 2 describes the Swedish pharmacy market and the break-up. Section 3 presents the data. Section 4 reports quasi-experimental evidence on the wage effects of the break-up. Section 5 develops and estimates the model of labor supply, and Section 6 develops the model of labor demand and estimates the degree of internalization. Section 7 uses the estimated model to quantify the effects of the break-up. 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 that would be present in another setting.

2.1 The Swedish Pharmacy Monopoly (1971–2009)

In 1971, the Swedish government nationalized all pharmacies under a single state-owned firm, Apoteket AB, through Statute (1970:205).³ 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). Apoteket AB was run as a limited company with a return requirement: its prices were to cover costs and yield a reasonable return on its capital (Lönngren et al., 1999). It paid dividends of SEK 750 and 330 million in 1994 and 1995 (Lönngren et al., 1999), and in 2000 the government as owner required a dividend of SEK 100 million (Apoteket AB, 2001, p. 44).

³Prior 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).

At the beginning of 2008, approximately 9,000 trained pharmacy workers were employed in Sweden: roughly 3,000 *apotekare*, holding a five-year master’s degree, and 6,000 *receptarier*, holding a three-year bachelor’s degree. Throughout the paper we refer to both groups as pharmacists.⁴ 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.⁵ Unemployment among trained pharmacy workers was negligible: 57 individuals in November 2007, a rate of 0.6 percent (SOU, 2008).

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, which represented pharmacists and had approximately 90 percent membership in 1995 (Lönngren et al., 1999), and Farmaciförbundet, which represented pharmacy technicians and later merged into Unionen in 2014 (Thoresson, 2024). Together, these unions covered over 95 percent 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.⁶ The 2002 agreement states: “The salary formation 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). The system of individual wage-setting within a collective framework was adopted by Farmaciförbundet, organizing pharmacy technicians, in 1988 following a pivotal 1984 agreement. Sveriges Farmaceutförbund, organizing pharmacists, had practiced individualized wage negotiation for longer, similar to other unions organizing academically trained professionals (Övling and Jacobsson, 2014).

It is the local manager who conducts annual salary discussions with each employee based on individual performance, competence, and results.⁷ This institutional arrangement means that different establishments of the same chain can and do pay different wages to observationally similar workers.

⁴In a pharmacy the two groups are legally interchangeable. The Medical Products Agency defines a *farmaceut* as a licensed *apotekare* or *receptarie* (LVFS, 2009, ch. 1, § 3). Under the 2009 Act on Trade in Pharmaceuticals, every pharmacy must have at least one *farmaceut* on site during opening hours (SFS, 2009, ch. 2, § 6), and only a *farmaceut* may prepare a prescription for dispensing (LVFS, 2009, ch. 5, § 15). The pharmacist responsible for a pharmacy (*läkemedelsansvarig*) must be an *apotekare* or another *farmaceut* with sufficient competence and experience (SFS, 2009, ch. 2, § 9). *Receptarier* also ran pharmacies under the monopoly: in 1983, about half of Apoteket’s 820 pharmacy managers were *receptarier* (SOU, 1984, p. 163).

⁵Pharmacist licensing was formalized in 1999, when both *apotekare* and *receptarie* became legally protected titles subject to revocable licenses.

⁶Some agreements included aggregate “wage pots” (typically 2.9 to 3.2 percent annual increases) to be distributed individually, while others were pure process agreements with no stipulated wage space (National Mediation Office, 2003).

⁷Post-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 agreement) with seven individual wage criteria and no centrally fixed wage levels.

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).

The wage-setting authority, the *lönesättande chef*, is in our setting either the pharmacy store manager (*apotekschef*) who manages a single pharmacy, or the pharmacy area manager (*apoteksområdeschef*) who would manage several pharmacies. In January 2008 there were 113 area managers, and 168 store managers (SOU, 2008, pp. 179–180), leaving one manager in charge of on average 3.2 pharmacies. One example from an interview study is an area pharmacy manager working at the monopoly in 2007, who supervised 48 workers at 6 different pharmacies, ranging from just over 2 to 20 employees at each location. The second manager from the same study is instead in charge of only 1 pharmacy, with 26 employees (Babic and Cirgic, 2007). In a separate example, a job advertisement of the monopoly dated 25 August 2008 describes Borrbý as a small pharmacy with two pharmacists, near a healthcare center. Applications were to be directed to a pharmacy area manager based at Apoteket Svanen in Ystad, 26 kilometers away and in a different municipality (Apoteket AB, 2008). Whether a pharmacy was allotted a separate manager or an area manager was not based on geography but rather on pharmacy size and location, contrasting city pharmacies (*centrumapotek*) with community pharmacies (*samhällsapotek*) (Babic and Cirgic, 2007).

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. The collective framework for wage setting did not change after the deregulation, and wage floors introduced by Farmaciförbundet in 2009 affected very few workers (Thoresson, 2024). Who set wages did change at many pharmacies: after the reform most pharmacies appointed a manager of their own (Appendix B), which we examine in Section 6.2.

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

Prescription drug prices in Sweden 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.⁸ Pharmacies have no discretion over prescription drug prices. These regulated pharmaceuticals account for the majority of pharmacy revenues: approximately 72.2 percent of total sales in 2008 (Apoteket AB, 2010). In an

⁸Before the creation of TLV’s predecessor, the Pharmaceutical Benefits Board (LFN), in 2002 (renamed TLV in 2008), this role was fulfilled by Riksförsäkringsverket (RFV). The structure of the formula remained unchanged through the reform, but its level rose. From November 2009, TLV added a fixed fee of SEK 10 per package of generic drugs and of original drugs facing generic competition, which raised the regulated margin by about SEK 460 million a year, from about 16 to 18 percent.

earlier 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 4 percent in 2008.⁹ 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 (Swedish Agency for Public Management, 2013).

2.2 The 2009 Deregulation

In 2009, the Swedish government dismantled the pharmacy monopoly and introduced private ownership of pharmacies. 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, 2009.¹⁰

Anticipating the passing of the law, 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 Omstrukturering AB, 2009). OAB divided Apoteket’s pharmacies into three groups: 466 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.¹¹ 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 clusters were national in scope, while the six smaller clusters were regional. 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. 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 SEK 5,867 million (EUR 560 million) (National Audit Office, 2012). Workers generally stayed on at their pharmacy after the ownership change. Only a small fraction of employees declined the offer to transfer their employment to the

⁹Sveriges Apoteksörening, the trade association representing pharmacy operators in Sweden, reports an operating margin of 4.0 percent for community pharmacies in 2008 (Sveriges Apoteksörening, 2017), and a lower margin of 1.7 to 2.2 percent during 2020–2023 (Sveriges Apoteksörening, 2025, p. 13).

¹⁰The 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).

¹¹Apoteket Farmaci AB was later renamed Apotek Produktion & Laboratorier AB (APL) and remained state-owned throughout the period.

new owners (Apoteket Omstrukturerings AB, 2009). Consolidation later reduced the number of major private pharmacy chains: Vårdapotek i Norden was acquired by Apotek Hjärtat in 2013, and Kronans Droghandel acquired Medstop in July 2013.¹² Even Apoteksgruppen, the network of entrepreneurial pharmacies, consolidated into one firm in 2018, and was later sold to Kronans Apotek. In 2026, this leaves two national private chains alongside the retained state-owned chain Apoteket AB, each retaining about 30 percent of the retail pharmacies.

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 percent increase, while only 5 of the original 924 pharmacies had closed (Swedish Agency for Public Management, 2013).¹³

An important aspect of the reform is the assignment of pharmacies to clusters. The assignment was carried out 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). Each cluster was designed to be geographically contiguous to make the regional clusters more attractive and easier to manage (Wikberg, 2010). The clusters created meaningful variation between the change in ownership and competition between different pharmacies. Among the 150 municipalities where ownership remained concentrated with a single firm after the reform, 102 experienced a change in ownership identity (from Apoteket AB to one of the private chains) without any change in local market structure, while 48 retained both the original state owner and monopoly structure.

3 Data

We combine multiple administrative registers from two different Swedish government agencies. Combining datasets from Statistics Sweden (SCB) allows us to construct a matched employer-employee dataset covering the universe of Swedish pharmacists and pharmacies from 1990 to 2021. We add pharmacy level prescription data to inform our analysis of the firm side.

Worker and Firm Data. Our analysis draws on six administrative registers. LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies) provides annual demographic and labor market information for all individuals aged 16 and older. It also provides each worker’s education level and subject. RAMS (Register-Based Labour Market Statistics) provides the matched employer-employee link identifying each worker’s establishment in November of each year. The establishment identifiers allow us to follow a workplace through ownership changes

¹²Appendix Table A3 counts the firms that ran retail pharmacies in each year.

¹³To ensure continued rural access, buyers of a pharmacy cluster 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 at least 20 kilometers from the nearest other pharmacy. The agreements expired in February and March 2013 and were not renewed (Swedish Agency for Public Management, 2013, p. 47). By 2015, four of the 110 designated pharmacies had closed (Dental and Pharmaceutical Benefits Agency, 2015, p. 41), and from 2013 to 2022 seven pharmacies at least 20 kilometers from the nearest other pharmacy closed (Dental and Pharmaceutical Benefits Agency, 2024, p. 11).

and identify establishments belonging to the same firm. Lönestrukturstatistiken (Wage Structure Statistics) is a survey-based register with detailed wage information covering approximately 50 percent of the workforce annually. JOBB (Employment Spells Register) provides all employment spells with monthly earnings. Företagens Ekonomi (FEK) provides establishment-level financial information including revenues, value added, and costs. Geocoded coordinates for workers’ residences and establishment locations allow us to calculate commuting distances.

Prescriptions. We complement the employer-employee dataset with pharmacy level sales data from the Swedish eHealth Agency (eHälsomyndigheten), which compiles the sales of every pharmacy in Sweden. The data are monthly and by pharmacy from 2009 to 2016.¹⁴ The dataset reports for each pharmacy the retail value and the purchase value of prescription drugs within the pharmaceutical benefit scheme, which covers about 93 percent of prescription drug sales in 2009. Sales are reported separately for drugs and consumables. Because retail prices and the pharmacy margin are regulated (see Section 2), the difference between retail and purchase value is the dispensing margin, that is the value added a pharmacy generates from prescriptions. We link pharmacies to the SCB’s establishment register using establishment identifier and location. Once matched we are able to follow the pharmacies over time and through ownership changes. The matched pharmacies account for 96.1 percent of national sales in 2009.

3.1 Sample Construction

Identifying Pharmacists. Practicing as a pharmacist in Sweden requires the requisite degree. Everyone holding this degree can easily apply to be licensed to work as a pharmacist.¹⁵ We identify licensed pharmacists using education codes from the SUN classification (Swedish Standard Classification of Education) of educations, including their subject, observing the required education in administrative records. Specifically, we classify individuals as *apotekare* if they have completed 5–6 years of education with code 727, and as *receptarier* if they have completed 3–4 years with a related code, and we refer to both as pharmacists. We retain all individuals who ever obtain a pharmacy-relevant education, identifying the complete universe of licensed pharmacists regardless of current employment status. We keep all employed pharmacists aged 16 to 74. We keep all non-employed pharmacists aged 20 to 59. Appendix Table A2 counts the licensed pharmacists and those working at retail pharmacies by year.

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. Among these pharmacies we keep retail

¹⁴In October 2026 we received data from before 2009, which we will integrate in the next version.

¹⁵There is a small fee (SEK 990, about EUR 94) and no additional checks other than having the degree. Given the small hurdle of applying for a license if you hold the appropriate degree we refer to workers with the degree as licensed pharmacists throughout the paper.

pharmacies, using the pharmacy type in the register of the Swedish eHealth Agency: hospital, dose-dispensing, distance, veterinary and administrative units are excluded. Appendix Table A1 counts the pharmacies by year.

Sample. In the last pre-reform year 2008, our sample comprises 944 retail pharmacies, of which 761 are in treated municipalities and 183 in control municipalities. The 912 of them that employ licensed pharmacists employ 5,064 pharmacists, not counting the workers of the 2008 hospital-supply spin-off.¹⁶ Another 2,583 licensed pharmacists were employed outside pharmacies and 2,148 were not employees.

4 Quasi-Experimental Evidence

We begin with quasi-experimental evidence that competition raises wages.

4.1 Definitions and Assumptions

Treatment Definition. We define treatment according to the sale of 67 percent of the pharmacies in 2009. Out of the 289 municipalities in our data, 150 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 139 municipalities, where the reform introduced multiple competing owners, constitute our treatment group. That is, for a pharmacy j in period t , we consider an outcome y_{jt} and estimate a difference-in-differences regression that compares municipalities (m) that, as a result of the sell-off in 2009, had more than one chain, against municipalities where the number of chains remained the same. In addition, we also allow for controls (Ξ_{jt}) such as year, municipality, and county-by-year fixed effects.

$$y_{jt} = \sum_{\tau} \beta_{\tau} \cdot \mathbf{1}[> 1 \text{ chains in } 2009_m] \times \mathbf{1}[t = \tau] + \Xi'_{jt}\vartheta + \varepsilon_{jt} \quad (1)$$

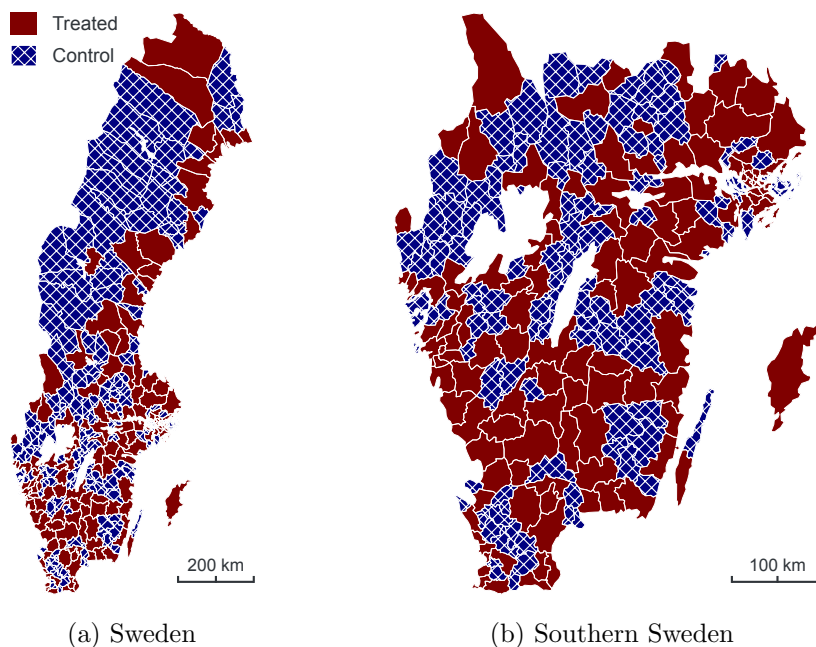
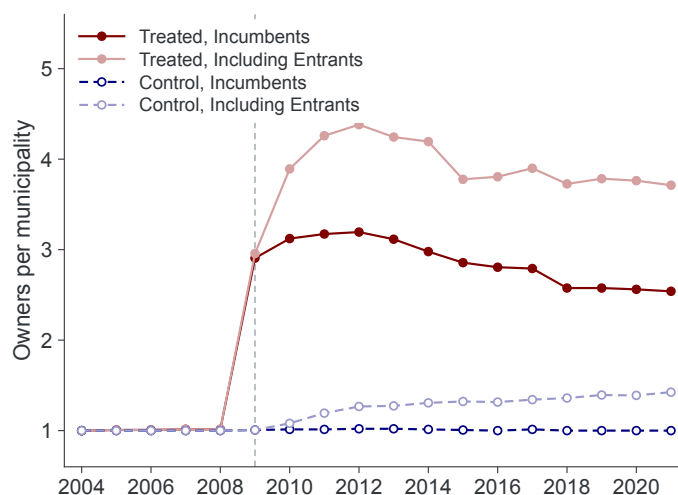
Wages. In what follows, we need to take a stance on our wage definition. Wages are observed at the worker level, but our interest is in pharmacy-level wage policy. We therefore construct a measure that averages full-time-equivalent monthly wages, correcting for observed worker composition.¹⁷ We construct this measure separately for each year by regressing individual monthly wages on gender, age groups, doctoral degree, foreign-born status, one-digit occupation code, an indicator for *receptarier* and pharmacy-year fixed effects. Controlling for occupation matters because, after the reform, most pharmacies appointed one of their pharmacists as manager with a pay rise, which

¹⁶We focus on licensed pharmacists and the prescriptions they dispense because together they define a tight market: only a licensed pharmacist may dispense a prescription, and one must be present whenever a pharmacy is open (Section 2). How far pharmacies can substitute other staff for pharmacists is a further mechanism through which labor market power can work (Rubens, 2023). Exploring this mechanism will be part of future work.

¹⁷This wage measure is similar to the one used by Felix (2022). An alternative here is to estimate a richer specification akin to Abowd et al. (1999) (AKM). We report results of an AKM specification in Figure 2d. This specification regresses wages on both worker fixed effects and establishment-year effects, which mix the wage premia of pharmacists and non-pharmacists. In ongoing work we are exploring an AKM specification which includes occupation-establishment-year effects.

changed the occupational mix most at small pharmacies (Appendix B).

Figure 1: Treatment and First Stage



(c) Owners per municipality

Notes: This figure shows the treatment assignment and its first stage. Panels (a) and (b) map municipalities by treatment status. A municipality is treated if more than one owner held its pharmacies after the 2009 sale. There are 139 treated and 150 control municipalities. In 48 of the control municipalities the former monopolist kept every pharmacy, and in 102 a single private buyer bought them all. Vingåker, whose only pharmacy was sold in the small-business package, is shown as a control and excluded from the estimation sample. Panel (c) plots the mean number of owners among a municipality's retail pharmacies, for incumbent pharmacies (present by 2008) and including entrants, in treated and in control municipalities. The dashed vertical line marks the sale, announced on 9 November 2009, which the 2009 registers capture because they record employment in November. The former monopolist's subsidiary is counted as the monopolist.

Parallel Trends. 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 plausibly 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 caretaker company composed the clusters after analyzing the profitability and location of each pharmacy (Apoteket Omstrukturerings AB, 2009), and the buyers had to keep 110 designated pharmacies in sparsely populated areas open at an unchanged service level for three years (Swedish Agency for Public Management, 2013, p. 47), so that treatment assignment followed the composition of the clusters rather than local labor market conditions.

4.2 Results

First Stage. Figure 1c shows what the sell-off did to ownership. Before 2009 every municipality had one employer. In treated municipalities the mean number of firms among incumbent pharmacies jumps to about three in 2009 and stays there until 2012, after which chains consolidate and the mean drifts down to about two and a half by 2021. In control municipalities it stays at one. Counting entrants raises the number of firms in both groups, to about four in treated municipalities in 2012 and to about 1.4 in control municipalities by 2021.

Wages. Before the reform, treated and control municipalities paid nearly identical monthly wages. We report results in Figure 2, Panels (a) and (b), and in Appendix Table A4. Pre-trends are flat and parallel in the two groups before the 2009 sale. The pooled pre-period estimate relative to 2008 is EUR 9 (SE 14) with year fixed effects and EUR 24 (SE 14) with county-by-year fixed effects, so that we cannot reject the parallel trends assumption at the 5 percent level in either specification (a joint test of the three pre-period coefficients has $p = 0.66$ and $p = 0.35$). In our basic specification with municipality and year fixed effects, the treatment effect over 2010 to 2013 relative to 2008 is EUR 72 (SE 22), or 2.1 percent.¹⁸ With county-by-year fixed effects, the specification of Section 6, the treatment effect is EUR 83 (SE 22), or 2.5 percent. The gap opens in 2010 and persists through the end of the sample. Panel (d) of Figure 2 shows that the effect does not depend on how pharmacies are weighted. Weighting pharmacies by their number of pharmacists gives EUR 78 (SE 23) and EUR 85 (SE 23), and weighting them by their number of new hires, which measures the premium at the pharmacies that were hiring, gives EUR 95 (SE 33). Appendix Figure A2 splits the effect into basic pay and unsocial-hours pay. On the pharmacy-years that report a basic salary, total pay rises by EUR 65 (SE 22), of which EUR 38 (SE 22) is basic pay and EUR 27 (SE 7) is pay for evening, night and weekend work. Pharmacies thus raised pay both through basic salaries and through more paid evening and weekend work, as treated pharmacies extended their opening

¹⁸All monetary values are in 2020 prices and converted from SEK to euros at 10.48 SEK per euro, the 2020 average of the ECB reference rate.

hours (Appendix D).

Distance to the Nearest Treated Municipality. Panel (c) of Figure 2 restricts the control group to the municipalities in a given distance band from the nearest treated municipality. With controls within 25 kilometers, the effect is EUR 39 (SE 31) with year fixed effects and EUR 66 (SE 32) with county-by-year fixed effects; with controls 25 to 50 kilometers away it is EUR 68 (SE 28) and EUR 87 (SE 31), and with controls 50 to 100 kilometers away EUR 191 (SE 73) and EUR 195 (SE 69). Beyond 100 kilometers the few remaining controls leave the estimate imprecise, EUR 94 (SE 89) and EUR 16 (SE 93). The effect is smallest with the nearest controls, in line with wages in nearby control municipalities responding to the break-up as well, so that the baseline understates the effect on treated pharmacies. Section 7.2 quantifies this spillover with the model.

Employment. A key prediction of the standard monopsony model is that both wages and employment are suppressed. Figure 3 reports the employment results. Panel (a) counts the pharmacists working at retail pharmacies in each municipality. Relative to control municipalities, treated municipalities gained 2.3 pharmacists (SE 1.0) (2.1 (SE 0.7) with county-by-year fixed effects). Panel (b) reports the share of pharmacists working at a pharmacy. Among the pharmacists living in a municipality, the share working at a retail pharmacy rose by 2.6 percentage points (1.1) more in treated municipalities (3.1 (1.5) with county-by-year fixed effects), weighting municipalities by their 2008 residents. Panel (c) turns to the number of pharmacists per incumbent pharmacy. Staffing fell in both groups, but not differentially between the two types of municipalities. Appendix D shows that contracted working time per pharmacist did not change, while weekly opening hours rose by 2.1 hours (SE 0.6) in treated relative to control pharmacies and paid unsocial hours per pharmacist by 1.8 per month (SE 0.4). Taken together, the evidence suggests that employment, and not only wages, responded to the shift in market structure. The effects are less clear-cut at the pharmacy level because more new pharmacies opened in treated municipalities. The supply of pharmacists is inelastic in the short run, since a license requires three to five years of university study, so every new pharmacy hires from a fixed pool.¹⁹ The employment effect is therefore visible at the municipality level, where the number of pharmacists rose, but not at the pharmacy level, where the entrants absorb it.

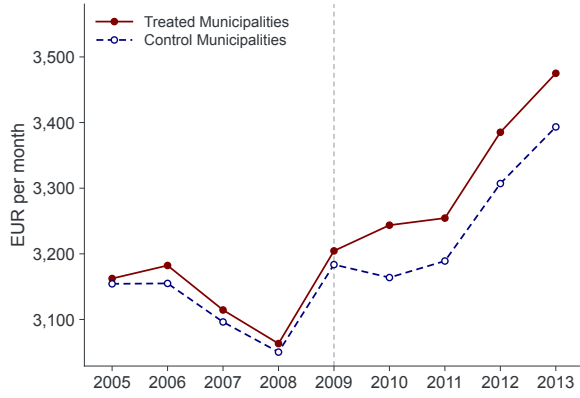
4.3 Robustness

We conduct a large number of robustness checks to verify the positive wage effect from competition that we find in our preferred specification (Figure 2d and Appendix Table A5). As throughout this section, we compare 2010 to 2013 with 2008, the base year of the conduct estimates in Section 6.

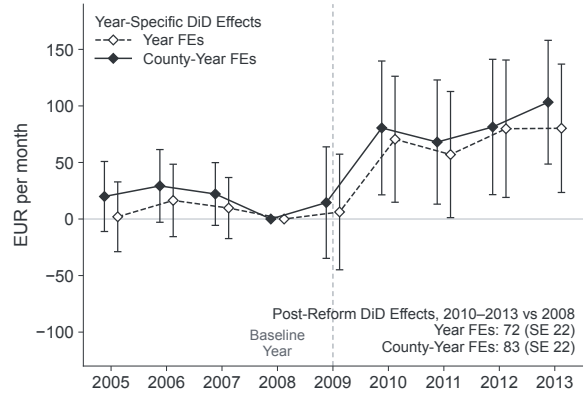
County-by-Year Effects. Since the conduct estimates of Section 6 include county-by-year fixed effects, which net out shocks common to the pharmacies of a county in a year, we report the

¹⁹The pool grows only slowly: from 2008 to 2013 the number of licensed pharmacists rose from 11,243 to 12,275 and the number working at retail pharmacies from 5,064 to 5,495 (Appendix Table A2). Where the staff of the new pharmacies came from is part of future work. Appendix Figure A1 shows the number of pharmacists per incumbent pharmacy in levels. It has a pre-trend: the largest treated pharmacies were already shrinking before 2008.

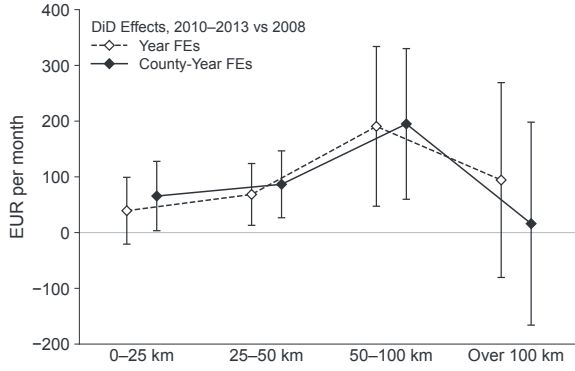
Figure 2: Effect of Deregulation on Wages



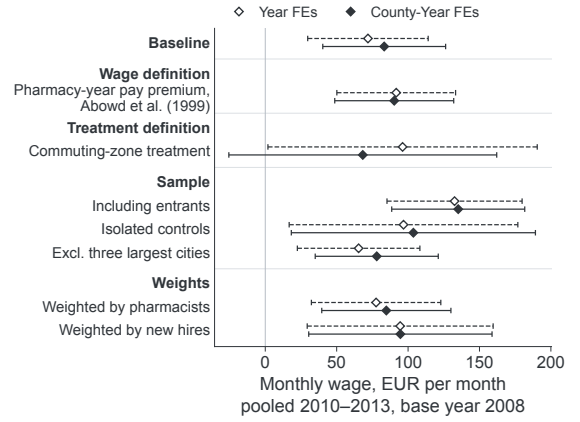
(a) Wage Premium: Levels



(b) Wage Premium: Difference-in-Differences



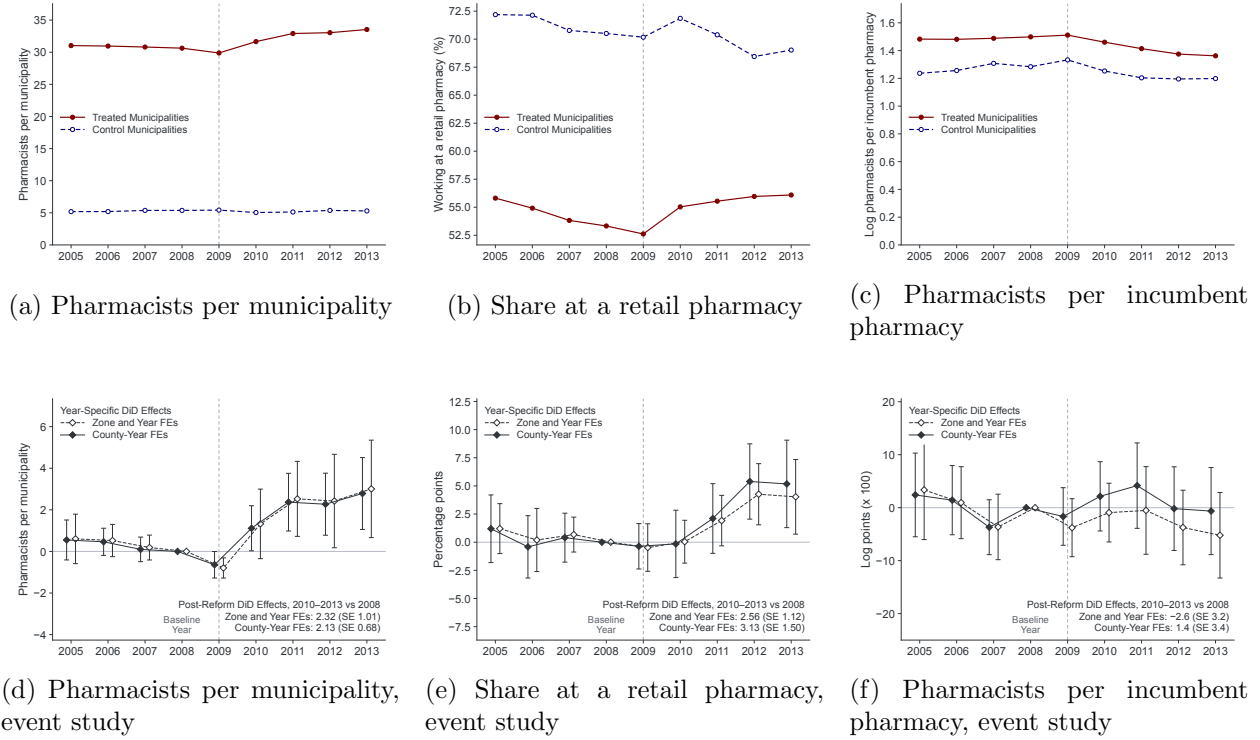
(c) Wage Premium: Difference-in-Differences by Distance of Controls



(d) Wage Premium: Robustness

Notes: This figure shows the effect of the break-up on the wage premium of incumbent pharmacies (present by 2008), adjusted for observed worker composition, in euros per month at 2020 prices. Panel (a) plots the mean premium in the 139 treated municipalities (more than one owner after the 2009 sale) and the 150 control municipalities (one owner) from 2005 to 2013. Panel (b) plots difference-in-differences coefficients by year relative to 2008. Panel (c) plots the post-reform effect when the controls are restricted to the municipalities in the indicated distance band from the nearest treated municipality, measured between the centroids of their 2005 to 2008 pharmacies. Panel (d) plots the post-reform effect for the baseline, the pharmacy-year pay premium of Abowd et al. (1999), treatment by commuting zone, the sample including entrants, controls in commuting zones without a treated municipality, the sample without Stockholm, Gothenburg and Malmö, and the baseline weighted by each pharmacy's number of pharmacists and by its number of new hires (pharmacists observed the year before at another pharmacy or outside pharmacies). Appendix Figure A2 splits the effect by pay component. The post-reform effect is the mean of the 2010 to 2013 coefficients relative to 2008, reported in the panels and in Appendix Table A4. Hollow markers use municipality and year fixed effects, filled markers municipality and county-by-year fixed effects, the specification of Section 6. The dashed vertical line in panels (a) and (b) marks the sale, announced on 9 November 2009, which the 2009 registers capture because they record employment in November. Bars are 95 percent confidence intervals based on standard errors clustered by municipality. The commuting-zone row is clustered by commuting zone.

Figure 3: Effect of Deregulation on Employment



Notes: This figure shows the effect of the break-up on pharmacist employment. Panel (a) plots the number of pharmacists employed at retail pharmacies by municipality of work. Panel (b) plots the share of pharmacist-educated residents under 58 who work at a retail pharmacy, by municipality of residence, with event-study coefficients weighted by the 2008 number of residents (unweighted, the estimate is 0.56 (SE 1.74)). Panel (c) plots the log number of pharmacists at incumbent retail pharmacies (present by 2008), in log points times 100, from a regression at the pharmacy level. Panels (a) to (c) plot the means for treated and control municipalities, panels (d) to (f) event-study coefficients relative to 2008. Hollow markers use commuting-zone and year fixed effects with a treated indicator and standard errors clustered by commuting zone, filled markers municipality and county-by-year fixed effects with standard errors clustered by municipality, the specification of Section 6. The estimates reported in the panels are the mean of the 2010 to 2013 coefficients relative to 2008, with standard errors from their covariance. The dashed vertical line marks the sale, announced on 9 November 2009, which the 2009 registers capture because they record employment in November. Bars are 95 percent confidence intervals. Appendix Figure A1 reports panels (a) and (c) in the other functional form.

quasi-experimental estimates with the same effects throughout (Figure 2, Panel (b), and Appendix Tables A4 and A5). Relative to 2008, the post-reform effect is EUR 83 (SE 22) with county-by-year effects against EUR 72 (SE 22) with year effects. The pre-period estimate relative to 2008 is EUR 24 (SE 14) rather than EUR 9 (SE 14).

Including Entrants. In the baseline estimation we focus on pharmacies that exist before the reform, and focus on wage changes of their workers. This does not capture direct wage effects that could come from new entry into the municipality. Figure 1c shows that including entrant pharmacies raises the number of employers in both treated and control municipalities relative to the incumbent-only comparison. To investigate the full effect of the reform, privatization together with entry, we include workers at pharmacies established after the reform, while keeping the treatment municipalities definition that only includes the competition change from privatization. The effect

we measure now is going to be a mix of wage effects on existing pharmacies, together with a change in the mix of operating pharmacies in the municipality. Relative to 2008, including entrants raises the estimate to EUR 133 (SE 24), or 3.8 percent, with year fixed effects and to EUR 135 (SE 24), or 3.9 percent, with county-by-year fixed effects.

Commuting Zones. As a further robustness check, we estimate the effect using Statistics Sweden’s 2004 classification of commuting zones (lokala arbetsmarknader), which divides 290 municipalities into 85 areas constructed from residence-to-workplace commuting flows. The areas are defined from the commuting of all employed workers in the economy, not only pharmacists. A municipality forms the center of its own area if fewer than 20 percent of its employed residents work in another municipality and no single other municipality receives 7.5 percent or more of them. Every other municipality is, as a rule, assigned to the municipality that receives its largest commuting flow (Hedin and Tegsjö, 2006). The 2004 classification predates the reform, so the areas do not respond to changes in commuting that the reform induced. The break-up creates 58 treated local labor market areas and 27 controls. With treatment assigned and standard errors clustered by commuting zone, we find relative to 2008 a wage increase of EUR 96 per month (SE 48), or 3.0 percent, against EUR 72, or 2.1 percent, in the baseline (Figure 2, panel (d)). With county-by-year fixed effects the estimate is EUR 68 (SE 48), whose confidence interval includes zero: with 85 zones in 21 counties, the county-by-year effects absorb much of the variation in treatment across zones.

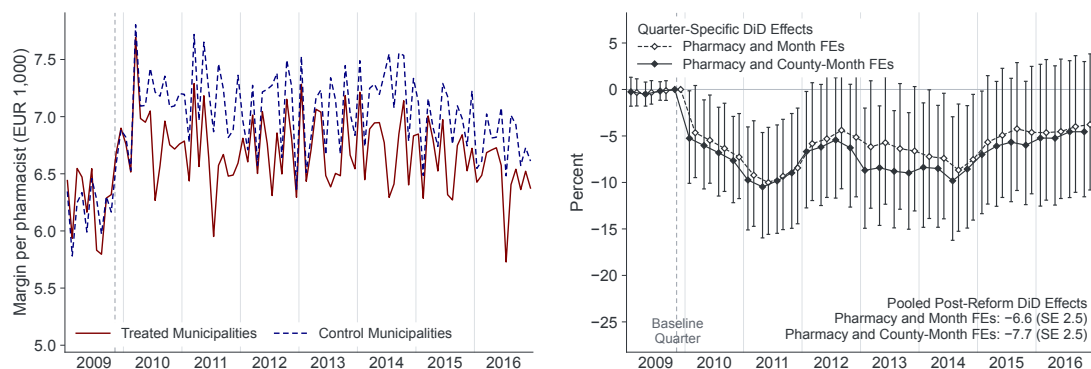
Marginal Revenue of Labor. On top of changing competition for workers one possibility is that the reform changed management, staffing or production methods at pharmacies, affecting worker productivity and through that the wages of workers. To investigate this channel we compare prescription sales values and dispensing margins between pharmacies in the control and treated municipalities. We find no evidence that pharmacies in treated municipalities became more productive than those in control municipalities.

Figure 4 plots the dispensing margin per pharmacist at incumbent pharmacies from 2009, when the sales data start. Productivity, if anything, fell in treated relative to control municipalities. From 2009 to 2010–2013 the margin per pharmacist grew by 6.6 percent less at treated pharmacies (SE 2.5, weighting pharmacies by their 2009 headcount) (7.7 percent, SE 2.5, with county-by-month fixed effects), with no differential trend over the quarters of 2009 ($p = 0.97$). The increase in the regulated fee from November 2009 (Section 2) applied nationally, and our treatment depends on the ownership of a municipality’s pharmacies rather than on their mix of drugs, so we do not expect exposure to the fee to differ systematically between treated and control pharmacies within a county. The wage increase is therefore unlikely to be the pass-through of higher productivity. Nor did the new owners raise productivity: after the sale, the margin per pharmacist did not differ significantly between sold and retained pharmacies (Figure 4b).²⁰ Sales per pharmacy fell by similar amounts in

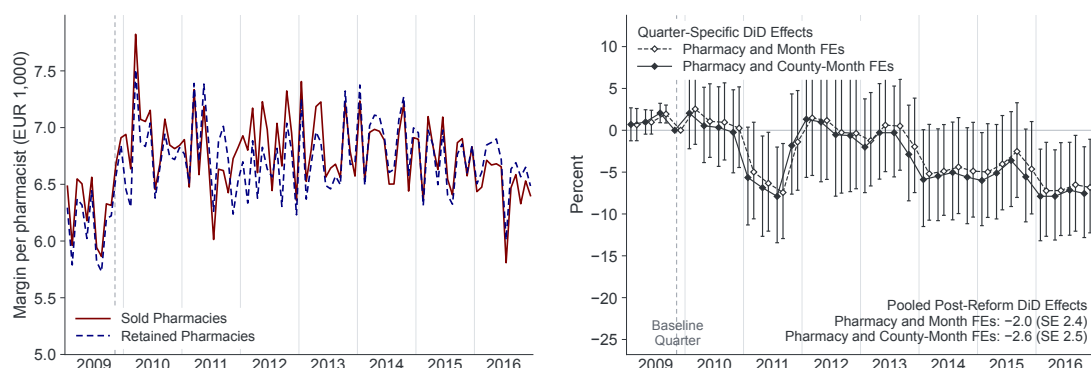
²⁰The three 2009 coefficients of panel (b) jointly differ from zero, but they are small, between 0.7 and 2.1 percent. With earlier prescription data that have recently become available to us, we will extend the pre-period in the next version.

both types of municipality between 2009 and 2013. These results are in line with our institutional understanding of the pharmacy industry. Pharmacists are hired to dispense prescription drugs and the demand for these drugs are, especially since prices are regulated, very stable over time.

Figure 4: Margin per Pharmacist at Incumbent Pharmacies



(a) All incumbent pharmacies



(b) Sold and retained pharmacies

Notes: This figure shows the prescription margin per pharmacist at incumbent retail pharmacies (present by 2008) linked to the eHealth sales data, which begin in 2009. The margin is the retail minus the purchase value of prescriptions within the pharmaceutical benefit scheme in the month, divided by the number of licensed pharmacists employed at the pharmacy in November of the same year according to the annual employment register, which assigns each pharmacist to one workplace, in thousand euros. Panel (a) compares treated and control pharmacies. Panel (b) compares the 565 pharmacies that were sold with the 291 that Apoteket retained, where a pharmacy is sold if its owner in 2010 is not Apoteket. The left panels plot the total margin over total pharmacists of each group by month. The right panels plot quarterly event-study coefficients for the log margin per pharmacist, in percent ($100(e^{\beta} - 1)$), with the fourth quarter of 2009 omitted and pharmacies weighted by their 2009 number of pharmacists, net of the mean difference between the groups by calendar quarter over 2010 to 2016. Hollow markers use pharmacy and calendar-month fixed effects and filled markers pharmacy and county-by-calendar-month fixed effects. The pooled estimates, reported in the panels, compare 2010 to 2013 with 2009. The joint test of the three 2009 coefficients has $p = 0.97$ and $p = 0.82$ in panel (a) and $p = 0.003$ and $p = 0.004$ in panel (b), where they lie between 0.7 and 2.1 percent. The dashed vertical line marks the sale, announced on 9 November 2009. Bars are 95 percent confidence intervals based on standard errors clustered by municipality. Sources: Swedish eHealth Agency and Statistics Sweden.

5 Labor Supply

We consider a discrete choice model of pharmacy choice in the spirit of Train (2009) and Berry et al. (1995).²¹

Consider the national market of pharmacists in year t . Each year consists of $|N_t|$ licensed pharmacists indexed by $i \in N_t$ that face a discrete choice between pharmacies indexed by $j \in \mathcal{J}_t$. In addition, the pharmacist can choose the outside option $j = 0$, which includes working at a non-retail pharmacy, working in another industry and not working. Each licensed pharmacist i that chooses j gains the utility U_{ijt} . We normalize the utility of the outside option to $U_{i0t} = \varepsilon_{i0t}$. Finally we summarize the choices of each pharmacist by $y_{it} = (y_{ijt})_{j \in \mathcal{J}_t \cup \{0\}}$, where $y_{ijt} = 1$ if pharmacist i chooses j in year t , and we denote by $j_{i,t-1}$ the alternative that pharmacist i chose in year $t - 1$.

Each utility U_{ijt} takes the following general form:

$$U_{ijt} = \delta_{jt} + \gamma \mathbb{1}[j_{i,t-1} \neq j] + \mu_{jt}(X_{it}) + \varepsilon_{ijt} \quad (2)$$

where δ_{jt} is the job specific component, common across workers, $\gamma \mathbb{1}[j_{i,t-1} \neq j]$ is the cost of starting at a pharmacy other than last year's, $\mu_{jt}(X_{it})$ is the worker job specific component, and ε_{ijt} is an idiosyncratic shock.

The first component δ_{jt} captures the mean utility of a job which takes the following form:

$$\delta_{jt} = \alpha \log w_{jt} + A'_{jt}\beta + \xi_{jt} \quad (3)$$

This component contains features of the job that are constant across workers, such as wages (w_{jt}) and non-wage amenities (A_{jt}) such as the pharmacy's location or flexible working hours. We also allow non-wage amenities that are unobserved to the econometrician (ξ_{jt}) such as reputation or work environment. The unobserved ξ_{jt} gives rise to endogeneity concerns which we will address in our estimation procedure in Section 5.1.

The fact that jobs are more than just wages is related to compensating job differentials. That is, all else equal, a worse amenity can be offset with a higher wage to generate the same utility.²² Finally, we assume that workers have logarithmic preferences over wages. Such an assumption is standard in the literature (Card et al., 2018; Berger et al., 2022), so that workers compare jobs in proportional terms.²³

The second component is the switching term $\gamma \mathbb{1}[j_{i,t-1} \neq j]$, which is one unless pharmacist i worked at pharmacy j last year. A pharmacist incurs the disutility γ when moving to another pharmacy and when taking up a pharmacy job from the outside option. Leaving pharmacy work carries no switching cost. We capture here another traditional source of upward-sloping labor

²¹The model is a special case of the choice model we further develop in our companion paper (Friederiszick et al., 2026).

²²Indeed, Humlum et al. (2025) find that higher-paying firms in Denmark offer slightly worse amenities overall, and Schneider et al. (2024) find that jobs that are perceived as immoral tend to be higher paid.

²³In ongoing work we also allow wages to enter in levels, and preliminary results are similar to the results we report.

supply curves, namely that switching jobs is costly because a worker needs to engage in a costly search and matching process. The switching term also includes reasons such as inertia and even loyalty and should hence be interpreted as an accumulation or reduced form solution to address that labor markets are frictional. Heterogeneity in the switching cost across workers is part of the third component.

The third component $\mu_{jt}(X_{it})$ collects everything else that depends on the match between pharmacist i and pharmacy j . It has two parts, a general distance disutility that is common to all workers and a set of heterogeneous preferences that depend on demographics X_{it} :

$$\mu_{jt}(X_{it}) = \underbrace{\psi(d_{ijt})}_{\text{distance measures}} + \underbrace{h_{jt}(X_{it})}_{\text{heterogeneous preferences}} \quad (4)$$

The first part, $\psi(\cdot)$, is a function of the distance measures d_{ijt} between pharmacist i and pharmacy j . These cover several margins, here the commuting distance and the distance to family, $d_{ijt} = (d_{ijt}^c, d_{ijt}^f)$. Distance is an important margin (Le Barbanchon et al., 2021, see e.g.). The second part, $h_{jt}(X_{it})$, collects the heterogeneous preferences. It interacts the distance measures and the switching indicator with demographics, shifts the switching cost after the reform, and lets demographics shift the value of working at a pharmacy at all, relative to the outside option. In principle it adds any other preference that varies with demographics, such as interactions between amenities and demographics.

The fourth component is an idiosyncratic utility component ε_{ijt} , which is also unobserved to the econometrician. We assume this term to be distributed as a Type 1 Extreme value.

The final component is the choice set $\mathcal{J}_t$ which contains every pharmacy within 50 km of home in year t and the option of not working at a pharmacy. The underlying assumption is that pharmacists do not consider pharmacies outside a 50 km radius, but still consider pharmacies outside of their municipality.²⁴ The 50 km radius reflects our focus on daily commuting from workers' existing homes. Jobs farther away can involve moving or staying near the workplace during the week, decisions that our model does not capture.²⁵ Fewer than 7 percent of pharmacists work more than 50 km from home.²⁶ We also assume that every pharmacy in a pharmacist's choice set is an available employment option. This assumption is plausible because unemployment among pharmacists was

²⁴The average distance from a municipality's border to its centroid is equal to 19 km (median 16 km). Preliminary results show that the estimates are stable when we lift the choice-set restriction.

²⁵Swedish tax law also uses a 50 km threshold for deductions associated with temporary work and maintaining a second residence. Among other conditions, eligibility requires overnight stays at the work location and a distance exceeding 50 km between the home and work locations (SFS, 1999, ch. 12, § 20).

²⁶We drop these pharmacists from both estimation steps rather than assigning them to the outside option, so that the employment of a pharmacy in equation (7) counts the pharmacists who commute to it daily from home. Extending this framework to other labor markets warrants some caution in defining the choice set as it could be another source of labor market power (Caldwell et al., 2026). In Friederiszick et al. (2026) we restrict choice sets to the jobs that are feasible given a worker's education. A more refined approach requires a model of consideration sets. Goeree (2008) and Agarwal and Somaini (2025) provide such methods, an avenue we are actively exploring in future work.

very low and employers reported shortages of pharmacists.²⁷

We then assume each licensed pharmacist solves the discrete choice problem so that a choice is given by:

$$y_{ijt} = \mathbb{1} \left\{ j = \arg \max_{k \in \mathcal{J}_t} U_{ikt} \right\}. \quad (5)$$

As we do not observe the idiosyncratic component ε_{ijt} , the probability that pharmacist i chooses pharmacy j is

$$s_{ijt} = \frac{\exp(\delta_{jt} + \gamma \mathbb{1}[j_{i,t-1} \neq j] + \mu_{jt}(X_{it}))}{1 + \sum_{k \in \mathcal{J}_t} \exp(\delta_{kt} + \gamma \mathbb{1}[j_{i,t-1} \neq k] + \mu_{kt}(X_{it}))} \quad (6)$$

and the labor supply of pharmacy j is the sum of these probabilities over the pool of pharmacists,

$$L_{jt} = \sum_{i \in N_t} s_{ijt} \quad (7)$$

5.1 Estimation

We estimate the model in two steps. We first parameterize the distance and heterogeneity terms and estimate them together with the job fixed effects by maximum likelihood. We then recover the wage coefficient from the job fixed effects by two stage least squares. We close with the wage measure and with the endogeneity of wages.

Parameterization. The distance disutility is piecewise linear in each distance measure. The heterogeneous preferences shift the same splines and the switching cost with demographics:

$$\begin{aligned} \psi(d_{ijt}) &= \psi'_c b(d_{ijt}^c) + \psi'_f b(d_{ijt}^f) \\ h_{jt}(X_{it}) &= X'_{it}(\Psi_c b(d_{ijt}^c) + \Psi_f b(d_{ijt}^f)) \\ &\quad + (\gamma^{X'} X_{it} + \gamma^p \mathbb{1}[t \geq 2010]) \mathbb{1}[j_{i,t-1} \neq j] + \iota' X_{it} \mathbb{1}[j \neq 0] \end{aligned} \quad (8)$$

Here $b(\cdot)$ is the linear spline basis with knots at 5, 15 and 30 kilometers, applied to the commuting distance d_{ijt}^c , which the choice set caps at 50 kilometers, and to the distance to family d_{ijt}^f , which is not capped. The vectors ψ_c and ψ_f and the scalar γ are common to all workers. The matrices Ψ_c and Ψ_f and the vector γ^X shift them with demographics, γ^p shifts the switching cost after the reform, and the vector ι shifts the value of working at a pharmacy at all, relative to the outside option, with demographics. The demographics X_{it} are the age band (under 30, 30 to 39, 40 to 49, 50 and over), a doctoral degree, the number of children, a child under four, foreign birth, and residence in a big city. The interactions with the distance to family use only the age band, the number of children and a child under four, so the other rows of Ψ_f are zero.

²⁷Before the reform, unemployment among pharmacists had been very low for a long time, and employers had reported a shortage of experienced *receptariet* for many years. For *apotekare*, the Swedish National Agency for Higher Education instead projected supply to exceed demand (SOU, 2008, pp. 190–191). After the reform, the owner of ICA’s Cura pharmacies reported that the rapid growth in the number of pharmacies had led to “a shortage of pharmacists and intense competition for staff in Sweden” (Hakon Invest, 2012, p. 26).

Maximum Likelihood Estimation. First, we estimate the job specific components δ_{jt} together with the parameters of equation (8) by maximum likelihood on the individual choices equation (6), treating δ_{jt} as pharmacy by year fixed effects so that predicted and observed shares coincide.

$$(\hat{\delta}, \hat{\gamma}, \hat{\psi}, \hat{\Psi}, \hat{\gamma}^X, \hat{\gamma}^p, \hat{\iota}) = \arg \max_{\delta, \gamma, \psi, \Psi, \gamma^X, \gamma^p, \iota} \sum_t \sum_{i \in N_t} \sum_{j \in \mathcal{J}_t \cup \{0\}} y_{ijt} \log s_{ijt} \quad (9)$$

IV Estimation. Next, we obtain α , the mean wage preference of pharmacists. As we are ultimately interested in wage setting, it is crucial to understand how workers react to shifts in wages. We face two challenges.

First, we observe wages for individual pharmacists, but the model needs one wage per pharmacy. We therefore use the wage premium from Section 4. As in Abowd et al. (1999), we assume that the worker and pharmacy components of pay are separable. Qualifications and experience can make wages differ between pharmacists, but the premium a pharmacy pays is common to all of them. This restriction is plausible within a narrowly defined occupation whose members perform similar core tasks. The wage premium therefore captures a pharmacy’s wage policy rather than the observed composition of its workforce.²⁸ We assume that pharmacists know these wage policies when they form wage expectations for the pharmacies in their choice set.

Second, wages are endogenous to other unobserved job characteristics ξ_{jt} . This gives pharmacies an incentive to adjust wages accordingly. To remedy this we use an instrument akin to a BLP instrument (Berry et al., 1995). Here a valid instrument shifts the wage a pharmacy sets while leaving the preferences of pharmacists unchanged (Azar et al., 2022a). The first-best instrument would be a shift in the marginal revenue of labor, which we observe in our prescription data. Prescription-based demand shifters generate little wage variation over our estimation period. We therefore use the density of neighboring pharmacies around each pharmacy which proxies local competition. That is, the number of neighboring pharmacies within one kilometer, weighted by their inverse distance. Rivals nearby matter for the wage a pharmacy sets, since they compete with it for pharmacists, but they do not enter the utility a pharmacist derives from working at the pharmacy itself.²⁹ Before 2009, when Apoteket owned every pharmacy, the instrument measures the density of the network around a pharmacy rather than competition between owners. We freeze the instrument at its 2004 value to avoid endogeneity through new pharmacies opening where wages and amenities were attractive. The 2004 value postdates the outcomes of 2001 to 2003, so for these years the freeze does not rule out this channel. We then estimate equation (10) by two stage least squares on the estimated $\hat{\delta}_{jt}$, controlling for the amenities A_{jt} , the log population density in 2004,

²⁸Unlike Abowd et al. (1999), the premium does not net out worker fixed effects, so it can contain the mean unobserved ability of a pharmacy’s pharmacists, and the instrument must also be uncorrelated with this component. An AKM specification gives a similar reduced-form effect, EUR 92 (SE 21) against EUR 72 (SE 22) in the baseline (Appendix Table A5). Its establishment-year effects, however, mix the pay of pharmacists and other staff, so we keep the premium estimated on pharmacists alone. We are exploring an AKM specification for the structural model in ongoing work.

²⁹One way to interpret this instrument is as a BLP instrument for commuting time.

and municipality and year fixed effects, and pooling the years 2001 to 2013 except 2009.

Exclusion from utility is necessary but not sufficient. Our identifying assumption is that the 2004 density of neighboring pharmacies is uncorrelated with ξ_{jt} once we condition on the amenities A_{jt} , local population density, and municipality and year fixed effects. Because the instrument does not vary over time, the estimate compares pharmacies in the same municipality that have more or fewer neighbors. The main threat is that pharmacies with many neighbors sit in central locations with different amenities. We measure population density in the 250-meter grid cell of each pharmacy in 2004, so that the comparison is between pharmacies in equally populated surroundings.

$$\hat{\delta}_{jt} = \alpha \log w_{jt} + A'_{jt}\beta + \xi_{jt} \quad (10)$$

5.2 Results

Panel A of Table 1 reports the choice-model estimates. Pharmacists prefer jobs closer to home. The disutility of commuting is steepest within the first five kilometers and flattens beyond, and the distance to family plays little role. Attachment to the current employer is substantial.³⁰ The switching cost rises with age and is higher for pharmacists with a child under four, for those with a doctoral degree and for those living in the three largest cities. It is lower for foreign-born pharmacists, falls slightly with each additional child, and is lower after the reform. Younger pharmacists are less likely to work at a pharmacy at all. A doctoral degree lowers that propensity too, but imprecisely.

Panel B of Table 1 reports the second step. The two stage least squares estimate of α is 5.71 (SE 2.54), with a first-stage F statistic of 11.2 on 7,259 pharmacy-years, against 0.10 (SE 0.27) by ordinary least squares.

Elasticities. The own-wage elasticity of labor supply follows from equations (6) and (7).

$$\epsilon_{jjt} \equiv \frac{\partial \log L_{jt}}{\partial \log w_{jt}} = \frac{\alpha}{L_{jt}} \sum_{i \in N_t} s_{ijt} (1 - s_{ijt}) \quad (11)$$

Figure 5 plots the distribution of own-wage elasticities across pharmacies. The median elasticity is 1.8, which is lower than in most settings: the quasi-experimental studies surveyed by Caldwell et al. (2026) have a median elasticity of 3.5, and all 91 studies in their survey a median of 2.1. Such a number indicates high potential labor market power, in line with a narrow labor market. We revisit this in Section 6.

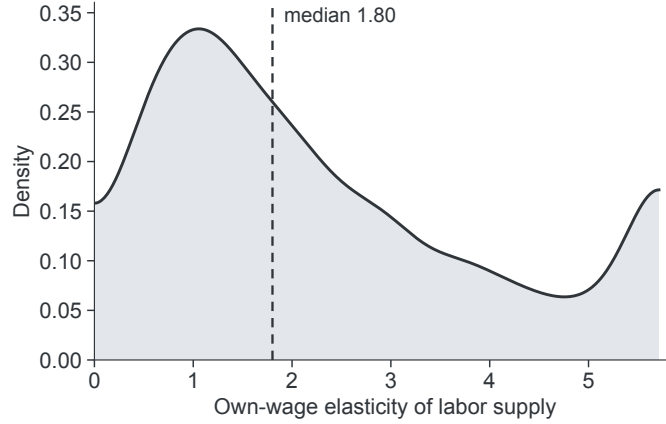
³⁰A large switching term can reflect a cost of moving, which Heckman (1981) calls structural state dependence, or persistent preferences for the current employer, which he calls spurious state dependence. Dubé et al. (2010) separate the two in consumer brand choice with a flexible distribution of persistent preferences and find that state dependence remains. We let the switching term vary with age, a doctoral degree, children, foreign birth, big-city residence and the period after the reform, so that it absorbs much of this heterogeneity, and we remain agnostic about its source. For the markdowns, what matters is how strongly pharmacists are attached to their employer rather than why, so persistent preferences for the current employer give a pharmacy labor market power just as a cost of moving does.

Table 1: Labor Supply

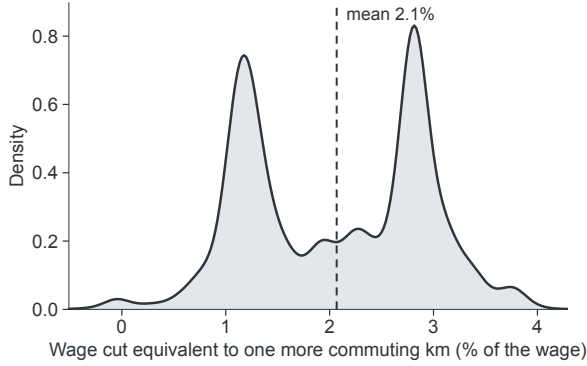
	Estimate	SE		Estimate	SE
A. First step: choice model					
<i>Commuting distance, per km</i>			<i>Switching term</i>		
0–5 km	–0.163	(0.053)	Switching (50 and over)	–7.575	(0.116)
5–15 km	–0.065	(0.022)	× under 30	2.229	(0.143)
15–30 km	–0.071	(0.014)	× 30–39	1.283	(0.149)
30–50 km	–0.077	(0.014)	× 40–49	0.635	(0.150)
<i>Distance to family, per km</i>			× doctoral degree	–3.068	(0.811)
0–5 km	–0.027	(0.056)	× number of children	0.175	(0.064)
5–15 km	0.024	(0.023)	× child under four	–0.868	(0.161)
15–30 km	–0.022	(0.012)	× foreign born	0.775	(0.123)
over 30 km	–0.000	(0.000)	× big city	–0.789	(0.120)
<i>Working at a pharmacy (inside option)</i>			× After the reform	0.735	(0.109)
× under 30	–0.801	(0.277)			
× 30–39	–0.490	(0.285)	Pharmacists, 2008 / 2011	8,056 / 8,350	
× 40–49	–0.018	(0.280)	Pharmacies, 2008 / 2011	905 / 1,178	
× doctoral degree	–1.054	(1.181)	Choice rows	1,370,224	
× number of children	–0.104	(0.131)	Log likelihood	–15,799.8	
× child under four	0.473	(0.364)	McFadden R^2	0.760	
× foreign born	–0.008	(0.189)			
× big city	–0.026	(0.178)			
B. Second step: wage coefficient					
α , log wage	5.71	(2.54)			
by ordinary least squares	0.10	(0.27)			
First-stage F	11.2				
Pharmacy-years	7,259				
Municipalities	285				

Notes: This table reports the estimates of the labor supply model of Section 5. Panel A reports maximum likelihood estimates of the choice model, equation (6), on the stacked choices of 2008 and 2011 with pharmacy-by-year fixed effects. The sample keeps the licensed pharmacists who are employees or not employed and whose home location we observe, with the pharmacists of hospital and other non-retail pharmacies in the outside option. It excludes the self-employed, the workers of the 2008 hospital-supply spin-off, non-employed pharmacists younger than 20 or older than 59, and pharmacists who work more than 50 km from home, so it is smaller than the counts of Section 3. Distance coefficients are per kilometer within each segment of the spline. The switching term is one for every pharmacy other than the one the pharmacist worked at in the previous year, and zero for the outside option. The base group is pharmacists aged 50 and over, and the last switching row is the shift after the reform. Standard errors clustered by pharmacist are in parentheses. Panel B reports two stage least squares estimates of equation (10) on the pharmacy-by-year effects of the choice model estimated year by year, 2001 to 2013 excluding 2009. The log wage premium is instrumented with the inverse-distance count of rival pharmacies within 1 km in 2004. The controls are the 2004 log population density and the part-time and overtime shares, and the regression includes municipality and year fixed effects. Standard errors clustered by municipality are in parentheses, and the first-stage F statistic is the Kleibergen–Paap statistic. The row by ordinary least squares reports the same regression without the instrument, on the same pharmacy-years.

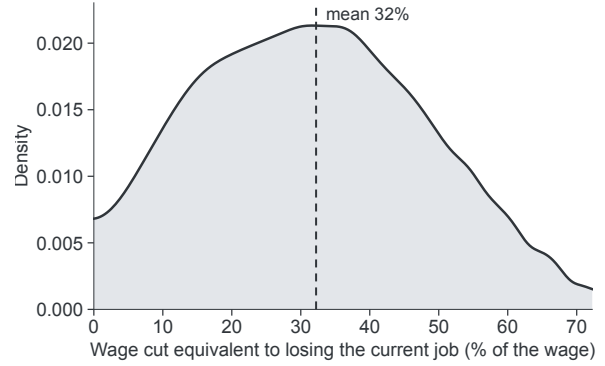
Figure 5: Labor Supply: Own-Wage Elasticities, Commuting and the Value of the Current Job



(a) Own-wage elasticity of labor supply



(b) One more kilometer of commuting, wage equivalent



(c) Losing the current job, wage equivalent

Notes: Panel (a) plots the density of own-wage elasticities of labor supply across pharmacies, equation (11), from the first-step estimates of Table 1, pooling the 905 pharmacies of 2008 and the 1,178 of 2011. The density is a Gaussian kernel estimate with Silverman's bandwidth on the elasticity, reflected at zero and at the upper bound α . The dashed line marks the median. Panel (b) plots the density of the wage equivalent of one additional kilometer of commuting implied by the first-step estimates of Table 1, pooling 2008 and 2011: the wage cut $100(1 - \exp(u/\alpha))$ that costs a pharmacist as much as the utility cost u of that kilometer, evaluated at each pharmacist's own commuting distance, in percent of the wage, across pharmacists employed at a pharmacy, with $\alpha = 5.71$ from Panel B of Table 1. It includes the demographic shifts of equation (8), and the dashed line marks the wage cut at the mean utility cost. The density is a Gaussian kernel estimate with Silverman's bandwidth on the utility scale, transformed to the wage-cut scale. Panel (c) plots the density of the value of the current job across pharmacists whose pharmacy of the previous year is in their choice set, pooling 2008 and 2011: the wage cut $100(1 - (1 - s_i)^{1/\alpha})$ that costs pharmacist i as much as losing the option to stay, where s_i is the predicted probability that i stays, in percent of the wage. The density is a Gaussian kernel estimate with Silverman's bandwidth in log points of the wage, reflected at zero and transformed to the wage-cut scale. The dashed line marks the mean wage cut.

Compensating Differentials. With log wages in equation (3), a utility cost $c > 0$ has a wage equivalent, the proportional wage cut that leaves the worker indifferent, and a money value that scales with the wage because the marginal utility of money is α/w_{jt} :

$$\frac{\Delta w}{w} = 1 - \exp(-c/\alpha) \approx \frac{c}{\alpha}, \quad \Delta w = w_{jt}(1 - \exp(-c/\alpha)) \quad (12)$$

Figure 5b plots the wage cut that costs a pharmacist as much as one more kilometer of commuting. At the mean utility cost, one more kilometer of commuting is worth 2.1 percent of the wage. The commuting cost is of the same order as in Le Barbanchon et al. (2021), who estimate from the reservation strategies of French job seekers that the value of commuting time is 80 to 98 percent of the hourly wage, about 0.6 to 0.7 percent of the wage per kilometer at their mean commute. Pharmacists value distance more, in line with a narrow professional market in which jobs are spread thin. Losing access to their current pharmacy costs the average attached pharmacist as much as a wage cut of 32 percent (Figure 5c).³¹ Survey evidence points to a similar attachment: in choice experiments, 39 percent of German workers would stay with their employer rather than take a 20 percent raise at a firm they named (Caldwell et al., 2025).

6 Labor Demand

We consider a pharmacy chain z operating multiple pharmacies j in period t . When setting wages, each pharmacy may take into account how attracting workers affects profits at other pharmacies belonging to the same chain. We restrict this internalization to co-owned pharmacies within the same commuting zone, using commuting patterns to approximate the geographical scope of competition for pharmacists.³² We denote the commuting zone of pharmacy j by $cz(j)$, the set of pharmacies belonging to chain z in zone cz at time t by $J_{z,cz,t}$, and the set of pharmacy j 's co-owned pharmacies in its commuting zone by $\mathcal{C}_{jt} \equiv J_{z(j),cz(j),t}$. Each pharmacy yields a revenue and a wage bill, so that the profit at pharmacy j is given by:

$$\pi_{jt} = R_j(L_{jt}) - w_{jt}L_{jt} \quad (13)$$

Each pharmacy j maximizes the joint profit of the pharmacies of its chain $z(j)$ in the same commuting zone $cz(j)$:

$$\Pi_{jt} = \sum_{r \in \mathcal{C}_{jt}} \pi_{rt}. \quad (14)$$

Every period, firms announce the number of workers they want to hire, and wages adjust so that

³¹In a logit model the expected loss from losing an option chosen with probability s is $-\ln(1-s)$ in utility (Train, 2009), or $-\ln(1-s)/\alpha$ in log points of the wage. We evaluate it at each attached pharmacist's predicted probability of staying, which averages to the observed stay rate (0.85 in 2008 and 0.75 in 2011). The average wage cut is 36 percent in 2008 and 28 percent in 2011. The measure accounts for pharmacists' idiosyncratic preferences across pharmacies.

³²Section 6.2 examines the sensitivity of our estimates to alternative geographical definitions of the internalization set.

markets clear.³³ That is, firms play a Cournot game in labor demand for differentiated jobs.³⁴ This is equivalent to a two-stage game where firms first set their labor capacity and then, in the second stage, wages, so that in equilibrium firms hire at their capacity and pay the Cournot wages.³⁵ A pharmacy thus first decides how many pharmacists it needs and then pays what it takes to fill the positions.

The firm then faces the following problem:

$$\max_{L_{jt}} \Pi_{jt}. \quad (15)$$

Taking first-order conditions, we obtain the following equilibrium wages:³⁶

$$w_{jt} = \underbrace{\rho_{jt}}_{\text{marginal revenue}} - \underbrace{L_{jt} \left(\frac{\partial L_{jt}}{\partial w_{jt}} \right)^{-1}}_{\mathcal{M}_{jt}^{\text{own}}} - \underbrace{\sum_{r \in \mathcal{C}_{jt} \setminus \{j\}} L_{rt} \left(\frac{\partial L_{rt}}{\partial w_{jt}} \right)^{-1}}_{\mathcal{M}_{jt}^{\text{cross}}}. \quad (16)$$

Equation (16) sets the wage equal to the marginal revenue of labor, ρ_{jt} , minus a markdown with an own and a cross component. The own component, $\mathcal{M}_{jt}^{\text{own}}$, is the usual monopsony markdown: to attract another pharmacist, a pharmacy must raise pay not only for the additional worker but also for its existing staff.

The cross component, $\mathcal{M}_{jt}^{\text{cross}}$, works through the co-owned pharmacies in the same commuting zone. These pharmacies compete for the same pool of pharmacists. When pharmacy j expands employment, its co-owned pharmacies must therefore pay more to keep their staff. Accounting for these costs reduces the incentive to expand employment and lowers the wage at pharmacy j .

Both components are nonnegative when labor supply slopes upward, so they push wages below the marginal revenue of labor.³⁷

Monopsonistic Competition. Under monopsonistic competition the pharmacy solves the same problem, equation (15), but treats its own job as a negligible part of each pharmacist's choice set.

³³Because switching is costly, a pharmacy has both an incentive to pay its attached pharmacists less today and an incentive to pay more to attach new pharmacists for the future. Our static wage-setting problem treats the two as offsetting, and we leave a dynamic model to future work.

³⁴Under Bertrand competition in wages each pharmacy would take the wages rather than the employment of its competitors as given, so that competition would be fiercer. In a differentiated-products setting with unobserved costs, Magnolfi et al. (2022) show that the Cournot model attributes a larger share of prices to markups than the Bertrand–Nash model. By the same logic we expect Bertrand competition to imply higher wages and lower markdowns at every value of the internalization parameters. We leave this comparison to future work.

³⁵This is a result in theoretical industrial organization that holds for both homogeneous goods (Kreps and Scheinkman, 1983) and differentiated goods (Hendel, 1994), here jobs.

³⁶The derivatives are those of the inverse labor supply system. Pharmacies choose employment, and the wages that deliver a given vector of employment follow from inverting the labor supply system of equation (7). We write $\left(\frac{\partial L_{rt}}{\partial w_{jt}} \right)^{-1} \equiv \frac{\partial w_{rt}}{\partial L_{jt}}$ for the elements of the inverse of the Jacobian matrix $\partial L_t / \partial w_t$, so that the own term is the wage increase at pharmacy j required to attract one more pharmacist and the cross term is the increase required at each co-owned pharmacy r to keep its staff. The inverse is that of the matrix, not the reciprocal of each partial derivative.

³⁷In our labor supply model this is equivalent to finding a strictly positive preference for wages. That is, $\alpha > 0$ in equation (3).

When it differentiates the choice probability of equation (6) it differentiates only the numerator, so the term that comes from the pharmacy's own job in the denominator is set to zero, and so are the derivatives of the other pharmacies' labor supply:

$$\frac{\partial s_{ijt}}{\partial w_{jt}} = \frac{\alpha}{w_{jt}} s_{ijt} - \underbrace{\frac{\alpha}{w_{jt}} s_{ijt}^2}_{\text{denominator} = 0}, \quad \underbrace{\frac{\partial L_{rt}}{\partial w_{jt}}}_{\text{cross} = 0} \quad \text{for } r \neq j, \quad (17)$$

so that $\partial L_{jt}/\partial w_{jt} = \alpha L_{jt}/w_{jt}$, every pharmacy faces the same elasticity α , and the first-order condition of equation (16) reduces to a markdown that is a fixed fraction of the wage:

$$w_{jt} = \rho_{jt} - \underbrace{\frac{w_{jt}}{\alpha}}_{\mathcal{M}_{jt}^{\text{monopsonistic}}}. \quad (18)$$

We denote this markdown by $\mathcal{M}_{jt}^{\text{monopsonistic}}$. This is the conduct assumed by Card et al. (2018) and Lamadon et al. (2022), and the one Roussille and Scuderi (2025) select in a test between models of conduct. The own markdown of equation (16), $\mathcal{M}_{jt}^{\text{own}}$, exceeds $\mathcal{M}_{jt}^{\text{monopsonistic}}$ by two terms that monopsonistic competition sets to zero: the term from the pharmacy's own job in the denominator, which grows with the pharmacy's share in its workers' choice sets, and the substitution with other pharmacies, which enters through the inverse of the Jacobian. The cross markdown is the sum of the cross derivatives over the co-owned pharmacies. The estimation below asks how much weight pharmacies put on these two sets of derivatives.

6.1 Estimation Procedure

We follow Miller and Weinberg (2017) and estimate the model from the wage equation evaluated at the observed outcomes before and after the reform, with the break-up as the instrument.

We introduce two weights on the derivatives that monopsonistic competition sets to zero: $\lambda \in [0, 1]$ on the part these derivatives add to the own markdown and $\kappa \in [0, 1]$ on the cross derivatives. Appendix A shows that, when a pharmacy accounts fully for its own markdown ($\lambda = 1$), κ is the weight it places on the profits of its co-owned pharmacies.³⁸ Second, we assume that our marginal revenue ρ_{jt} is a function of observed (c_{jt}) and unobserved shifters (ς_{jt}).³⁹ This yields the following estimating equation:

$$w_{jt} = \underbrace{c'_{jt}\varphi + \varsigma_{jt}}_{\rho_{jt}} - (1 - \lambda) \mathcal{M}_{jt}^{\text{monopsonistic}} - \lambda \mathcal{M}_{jt}^{\text{own}} - \kappa \mathcal{M}_{jt}^{\text{cross}} \quad (19)$$

³⁸We allow for λ to capture partial internalization of the own markdown irrespective of co-ownership, which has footing in the literature (Dube et al., 2019, 2025; Faia et al., 2026). In future work, we plan to integrate the parameter λ into the firm's profit function, as Appendix A does for κ .

³⁹Appendix C reports preliminary work that measures marginal revenue from a production function and compares the markdowns it implies with those of the model.

Interpretation. Our parameters of interest are (λ, κ) , the weights on the strategic part of the own markdown and on the cross markdown. Equation (19) nests the models of conduct in the literature. At $\lambda = \kappa = 0$ pharmacies disregard strategic interactions: each sets its wage as if its decision had no effect on the decisions at competing pharmacies, including those of its own chain, and pays the wage of monopsonistic competition, equation (18), with the markdown w_{jt}/α . This is the conduct that most of the literature assumes, as in Volpe (2026) and Kroft et al. (2025), and under it ownership plays no role for wages. At $\lambda = 1$ and $\kappa = 0$ pharmacies are oligopsonists: each accounts for how its wage decision affects the decisions at competing pharmacies, and therefore for its own weight in its workers’ choice sets, but it treats the other pharmacies of its chain as competitors. This is the conduct of Berger et al. (2022) and Jarosch et al. (2024). At $\lambda = \kappa = 1$ each pharmacy also internalizes its common ownership and maximizes the joint profit of its chain within the commuting zone, equation (16), the standard assumption in industrial organization, applied to the labor market by Setzler (2025). Between these corners, λ measures how far pharmacies go in accounting for strategic interactions and κ how far they internalize their common ownership.

Endogeneity. The unobserved component ς_{jt} gives rise to endogeneity concerns for both the own markdown and the cross markdown. Both a larger own and cross markdown can be correlated with a larger return per pharmacist. For example, a pharmacy located in a denser commuting zone also has a larger demand for prescription drugs. For this purpose we use the variation that the break-up generated. For the own markdown we use a labor supply shock purged of endogeneity in the spirit of Berry et al. (1999).⁴⁰ Ownership does not enter the own markdown, so the break-up does not move this instrument. Its variation comes from changes between 2008 and each post-reform year in the set of pharmacies, in where pharmacists live and in their previous employers. The instrument is valid if these changes are uncorrelated with the change in unobserved marginal revenue $\Delta\varsigma_{jt}$, conditional on the controls. Entry is the main threat, since new pharmacies may open where marginal revenue grows, and our control for the prescription margin per pharmacy absorbs part of it. Our estimate of λ rests on this assumption. For the cross markdown we use the variation generated in the reform, matching our quasi-experimental evidence in Section 4. The exclusion restriction is that, conditional on the controls, the break-up changed wages only through the cross markdown and not through the marginal revenue of labor. The demand for prescription drugs depends on the health of the local population, which does not respond to who owns its pharmacies. Prices and the dispensing margin are regulated nationally and are therefore the same in treated and control municipalities, and sales per pharmacy fell by similar amounts in both (Section 4). With a fixed volume of prescriptions, the marginal revenue of an incumbent pharmacy changes mainly when entrants take a share of its sales, which our control for the change in the prescription margin

⁴⁰We calculate the predicted own markdown from our labor supply model in each year, with that year’s pharmacies, including entrants, and that year’s pharmacists, with their home locations and previous employers. We then shut down two channels that lead to endogeneity. First, wages enter both supply and demand, so we evaluate the markdowns at the county average wage of the year. Second, we set the unobserved amenities ξ_{jt} to zero, keep the observed amenities at their 2004 values and adjust a constant for each county so that it matches its observed employment.

per pharmacy captures. We then difference equation (19) within pharmacy between 2008 and each year from 2011 to 2013, where we leave out 2009 and 2010 as they were transition years. We then instrument the change in own markdown with a purged predictor of change in own markdown and the cross markdown with the same treatment indicator D_j as in the reduced form (Section 4), which is one for pharmacies in municipalities that shifted from one to multiple chains operating. That is:

$$\Delta w_{jt} = -(1-\lambda) \Delta \mathcal{M}_{jt}^{\text{monopsonistic}} - \lambda \Delta \mathcal{M}_{jt}^{\text{own}} - \kappa \Delta \mathcal{M}_{jt}^{\text{cross}} + \Delta c'_{jt} \varphi + \Delta \varsigma_{jt}, \quad t = 2011, \dots, 2013 \text{ w.r.t. } 2008 \quad (20)$$

The monopsonistic markdown is w_{jt}/α and contains the wage. We therefore take it to the left-hand side and estimate

$$\left(1 + \frac{1}{\alpha}\right) \Delta w_{jt} = -\lambda \Delta \left(\mathcal{M}_{jt}^{\text{own}} - \mathcal{M}_{jt}^{\text{monopsonistic}}\right) - \kappa \Delta \mathcal{M}_{jt}^{\text{cross}} + \Delta c'_{jt} \varphi + \Delta \varsigma_{jt}, \quad (21)$$

with α from the labor supply estimates of Section 5. Equation (21) is equation (20) rearranged, so λ and κ keep their meaning. The own regressor is the strategic part of the own markdown, the part that monopsonistic competition sets to zero, and its instrument $\Delta \widetilde{\mathcal{M}}_{jt}^{\text{own}}$ is the same object evaluated at the county wage, as in the first stage (22). At $\lambda = \kappa = 1$ the equation reduces to the difference of equation (16).

$$\begin{aligned} \Delta \left(\mathcal{M}_{jt}^{\text{own}} - \mathcal{M}_{jt}^{\text{monopsonistic}}\right) &= \chi_{11} \Delta \widetilde{\mathcal{M}}_{jt}^{\text{own}} + \chi_{12} D_j + \Delta c'_{jt} \varphi^{\text{own}} + \nu_{jt}^{\text{own}} \\ \Delta \mathcal{M}_{jt}^{\text{cross}} &= \chi_{21} \Delta \widetilde{\mathcal{M}}_{jt}^{\text{own}} + \chi_{22} D_j + \Delta c'_{jt} \varphi^{\text{cross}} + \nu_{jt}^{\text{cross}} \end{aligned} \quad (22)$$

6.2 Results

Panel A of Table 2 reports the baseline estimates. Pharmacies put a weight of $\lambda = 0.009$ (0.016) on the own markdown and a weight of $\kappa = 0.309$ (0.122) on the cross markdown.⁴¹

The estimates are stable when we control for the change in the margin per pharmacy and privatization.⁴² That is, κ does not pick up changes in the profitability of the pharmacy. Nor does it pick up a wage change common to the pharmacies that changed hands.

Post-Reform Years. Appendix Figure A4 estimates the cross-markdown weight separately for each post-reform year, with the treatment indicator interacted with the year as instruments. On

⁴¹These standard errors treat α and the first-step estimates of Table 1 as known, so they understate the uncertainty in λ and κ . We will assess the sensitivity of the conduct estimates to this uncertainty in future versions.

⁴²Our wage is total pay. Part of its rise is pay for more evening and weekend work, which may itself respond to stronger competition for pharmacists. Schedules that make a job more or less attractive enter the pharmacy effects δ_{jt} of the labor supply model and therefore the markdowns. Our estimates assume that longer opening hours change when prescriptions are dispensed rather than how many, so that they do not move marginal revenue beyond our controls. The margin control measures the change since 2009, when the prescription data start, rather than since 2008. In 2009 no incumbent pharmacy had yet changed hands, since the sold pharmacies passed to their new owners only from January 2010 (Section 2). The demand for prescription drugs also changes little from one year to the next, and the margin per pharmacist shows no differential trend over the quarters of 2009 (Section 4). We will extend the control to 2008 with earlier prescription data that have recently become available to us.

Table 2: Internalization: Baseline and by Owner, Geography and Population Density

A. Baseline					C. By the location of the co-owned pharmacies			
	(1)	(2)	(3)	(4)		(1)	(2)	(3)
λ , own markdown	0.009 (0.016)	0.010 (0.016)	0.009 (0.016)	0.010 (0.016)	Internalization set	Zone	Municipality	Sweden
κ , cross markdown	0.309 (0.122)	0.298 (0.119)	0.307 (0.123)	0.296 (0.121)	λ , own markdown	0.009 (0.015)	0.004 (0.013)	0.064 (0.071)
Margin change	No	Yes	No	Yes	κ , links in the set	0.302 (0.139)	0.203 (0.075)	0.916 (0.914)
Sold indicator	No	No	Yes	Yes	κ , links elsewhere	-0.003 (0.064)		
First-stage F , λ	62.3	61.1	62.1	61.6	Pharmacy-years	1,498	1,498	1,498
First-stage F , κ	15.1	15.5	14.4	14.4				
Pharmacy-years	1,498	1,498	1,498	1,498				
B. By the owner setting the wage					D. By population density (2004)			
	(1) κ varies	(2) Both vary				(1) κ varies	(2) Both vary	
λ , own markdown	0.011 (0.015)				λ , own markdown	0.022 (0.021)		
λ , Apoteket		0.015 (0.016)			λ , below median		0.026 (0.024)	
λ , new owners		0.005 (0.020)			λ , above median		0.017 (0.039)	
κ , Apoteket	0.290 (0.124)	0.305 (0.141)			κ , below median	0.162 (0.126)	0.159 (0.131)	
κ , new owners	0.350 (0.299)	0.494 (0.486)			κ , above median	0.874 (1.388)	0.888 (1.352)	
Pharmacy-years	1,498	1,498			Pharmacy-years	1,213	1,213	

Notes: This table reports two stage least squares estimates of equation (21) on pharmacy-years from 2011 to 2013, each differenced from 2008, with county-by-year fixed effects. The instruments are the treatment indicator D_j and the predicted change in the own markdown. The controls are the change in the log prescription margin per pharmacy since 2009 and an indicator of the pharmacies sold to a new owner. Panel A adds them as marked. Panels C and D include both. Panel B includes the margin change only, because the sold indicator is the owner split's own instrument. In Panel D the groups are the municipalities below and above the median 2004 population density. In Panels B and D the weights are instrumented by D_j interacted with group indicators; column (1) lets κ vary by group and column (2) both weights. In Panel C, column (1) separates co-owned pharmacies in the same commuting zone from those elsewhere in Sweden, and columns (2) and (3) set the internalization set to the municipality and to Sweden. First-stage F statistics are Sanderson–Windmeijer statistics. Standard errors clustered by municipality are in parentheses.

the pharmacies present in every post-reform year, the weight is 0.29 (0.15) in 2011, 0.39 (0.16) in 2012 and 0.44 (0.17) in 2013. On all pharmacy-years it is 0.20 (0.12), 0.29 (0.12) and 0.43 (0.20). Each lies within the confidence interval of the pooled estimate. Appendix Table A6 reports the estimates year by year and for the pooled window.

Heterogeneity. We then explore the heterogeneity of the internalization parameter in three directions. In Panels B to D of Table 2 the weights are instrumented by the treatment indicator interacted with group indicators, so that the baseline weight is a weighted average of the group weights.

One concern is that the new chains have a different profit function from the (former) monopolist Apoteket. Panel B of Table 2 lets the weight depend on the owner that sets the wage. Apoteket’s weight applies to every pharmacy’s 2008 cross markdown and to the post-reform markdown of the pharmacies it retained. The new owners’ weight applies to the post-reform markdown of the pharmacies they bought. The two weights are 0.290 (0.124) and 0.350 (0.299) and do not differ ($p = 0.76$). That is, the new chains and the former monopolist internalize alike.⁴³

We also ask whether pharmacies that were co-owned under the monopoly and now belong to different owners keep coordinating. If they did, the break-up would not change the markdown they internalize, and the wages of the pharmacies Apoteket retained would not respond to the treatment indicator. Their weight in Panel B, 0.290 (0.124), is significantly above zero, which speaks against full continued coordination, given that Apoteket’s weight is the same before and after the reform. The break-up in itself cannot identify partial coordination among former co-owners, since it only moves the markdown over the severed links (notes to Table 2).

Panel C reports how sensitive the results are to our local labor market definition. It splits the cross markdown into internalization of co-owned pharmacies within the same commuting zone and elsewhere in Sweden. Within-zone co-ownership is instrumented by the treatment indicator, and the internalization outside the zone is instrumented by the indicator interacted with the predicted 2008 markdown over those links, similar to the instrument for the own markdown. We find that including internalization beyond the commuting zone leaves the weight within the zone stable, $\kappa = 0.302$ (0.139), while the weight beyond it is small and statistically insignificant, -0.003 (0.064) with $p = 0.96$. Narrowing the block to the municipality gives 0.203 (0.075). Widening it to the whole country gives 0.916 (0.914), with more than seven times the baseline’s standard error (columns (2) and (3) of Panel C).

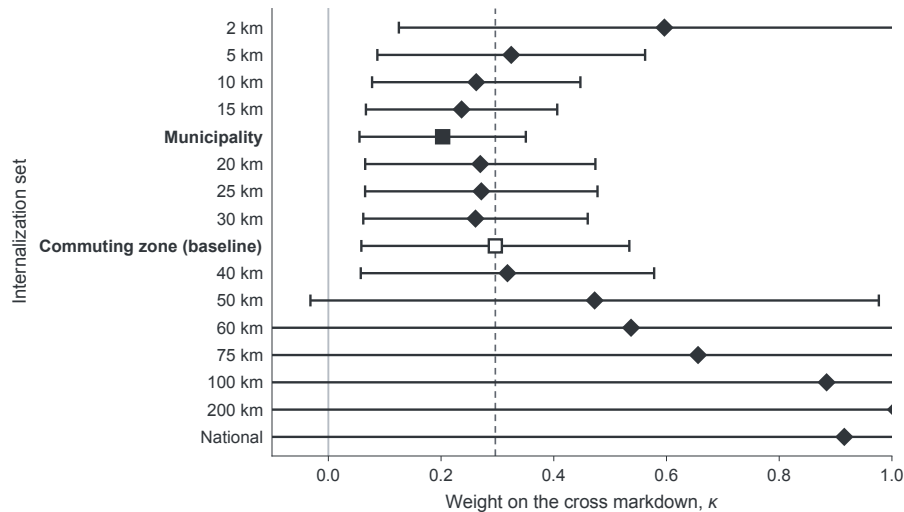
In Figure 6 we further vary the labor market we assume a chain to internalize. Around each pharmacy we draw a radius and only internalize the other pharmacies owned by the same chain within that radius. Our results are robust across different specifications in line with Panel C of Table 2. The parameter κ increases with the radius, and the increases are paired with larger

⁴³We impose the same weight before and after the reform. If the weight changed, our estimate combines the two, and it equals the post-reform weight when the 2008 cross markdown does not differ between treated and control pharmacies. The split also uses the sale as an instrument and therefore assumes that the sale changed wages only through the markdowns. Panel A supports this: adding the sale as a control moves the weight from 0.309 to 0.307.

standard errors. This pattern is in line with our instrument being defined at the municipality level: the larger the labor market, the smaller the share of the cross markdown that the break-up moves.

Panel D reports how the internalization parameter varies across different types of labor markets. As in the previous exercises, we are unable to detect heterogeneity between denser labor markets, where competition might be more salient, and sparsely populated municipalities.

Figure 6: The Cross-Markdown Weight by the Radius of the Internalization Set



Notes: This figure plots two stage least squares estimates of κ on the sample, controls and years of Panel A of Table 2, with the internalization set redefined as the chain's co-owned pharmacies within r kilometers of the pharmacy, in any commuting zone, with both controls of column (4). The hollow square is the baseline estimate with the commuting zone as the set, 0.296, placed between the 30 and 40 kilometer rings, since the mean distance from a commuting zone's centroid to its border is 37 kilometers. The dashed line marks its value. The filled square is the estimate with the municipality as the set, 0.203, placed between the 15 and 20 kilometer rings, since the mean distance from a municipality's centroid to its border is 19 kilometers. The national row is the estimate with all of the chain's pharmacies in Sweden as the set. Bars are 95 percent confidence intervals based on standard errors clustered by municipality.

Finally, we study the role of managers in wage setting. Section 2 describes several types of managers who set wages, and the allocation of this authority could lead a firm to internalize its strategic interactions imperfectly. Wage setting and accountability for results sat largely with the manager of a pharmacy or area. Already in 1984 the manager of each pharmacy was responsible for its economic returns (SOU, 1984, pp. 78–81), and after the reform Apoteket AB paid staff and first-line managers a bonus of SEK 5,000 if three targets were met, one of them an improvement in the result of their own unit (Apoteket AB, 2011, p. 53). A manager who sets wages and is judged on the result of a single pharmacy has little reason to account for the effect of those wages on its sister pharmacies. Appendix Table A7 explores heterogeneity in internalization along this dimension from two angles. First, we ask whether pharmacies whose manager changed after 2008 internalize differently from those that kept their manager. The two weights, 0.291 (0.159) and 0.297 (0.155), do not differ ($p = 0.98$). Second, we split pharmacies by the background of the manager.⁴⁴ In

⁴⁴In line with Acemoglu et al. (2022), who find that managers with a business background set wages differently.

neither case do we find evidence that the manager drives heterogeneity in internalization (Appendix Table A8).

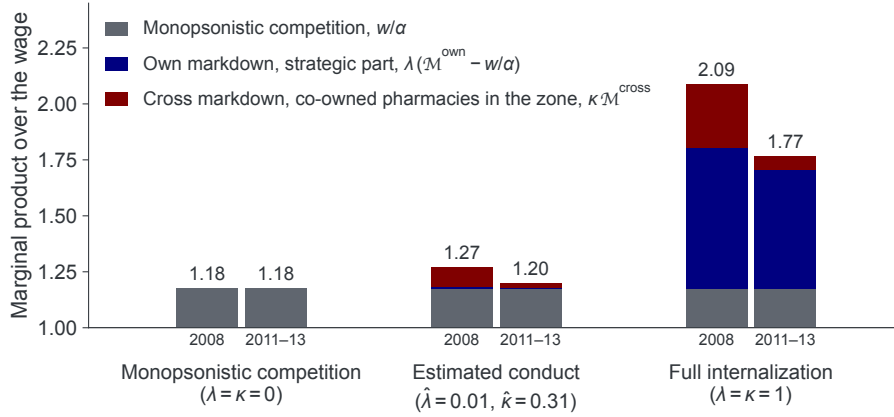
In sum, our estimate of internalization is unlikely to be driven by a particular subgroup of pharmacies.⁴⁵

Markdowns. Figure 7 reports the implied markdowns. Under monopsonistic competition the markdown is a fixed fraction of the wage, w/α , so that marginal product is 1.18 times the wage before and after the reform alike. At the estimated conduct it is 1.27 times the wage before the reform and 1.20 after it, close to monopsonistic competition. Two things stand out. First, after the break-up the markdown falls under both the estimated conduct and full internalization, and the largest part of the fall comes, unsurprisingly, from the cross markdown: pharmacies have less labor market power through owning several pharmacies. Second, there is a large gap between the estimated conduct and full internalization. Pharmacies leave about three quarters of their potential labor market power on the table (panel (b)). The next section shows that, while monopsonistic competition and partial internalization imply similar markdowns, they imply different wage effects of the break-up.

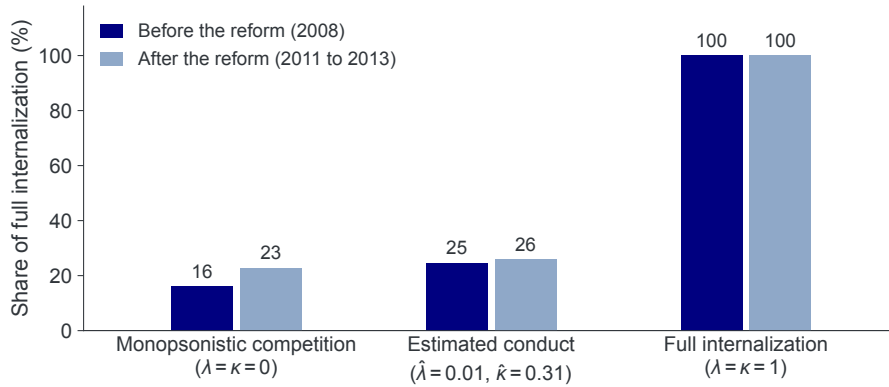
⁴⁵Understanding heterogeneity in internalization is therefore better left to settings with more variation across firms, which we aim to explore in future work.

Figure 7: Markdowns by Conduct, Before and After the Reform

(a) Markdowns, marginal product over the wage



(b) Exercised labor market power



Notes: This figure shows the markdown of a pharmacist's wage, marginal product over the wage, ρ/w , that the estimating equation (19) implies at the observed wage of the median pharmacy in the baseline sample of Table 2, under three conducts. Before the reform uses each pharmacy's 2008 markdown once and after the reform the pharmacy-years of 2011 to 2013. Under each conduct the marginal product is backed out from the observed wage. Panel (a) stacks the three parts of $\rho/w - 1$ in the legend's notation: w/α , the markdown of monopsonistic competition, which every conduct carries; λ times the strategic part of the own markdown; and κ times the cross markdown over the co-owned pharmacies of the commuting zone, each in percent of the wage. Each part is the median across pharmacies, so the bars are sums of medians. Panel (b) divides each conduct's markdown in excess of one, $\rho/w - 1$, by that under full internalization of the same year. The estimated conduct is column (1) of Table 2. The wage coefficient is $\alpha = 5.71$.

7 Counterfactual Results

Simulation. Following Miller and Weinberg (2017), we start from the market after the reform.⁴⁶ In each post-reform year from 2011 to 2013 we take that year’s pharmacies, pharmacists, preferences and wages. For each conduct (λ, κ) we back out the marginal revenue of each pharmacy so that, under that year’s owners, the observed wages solve the wage equation (19) exactly. We then change the ownership of the pharmacies and solve for the new equilibrium, holding the pharmacists and marginal revenue fixed.⁴⁷ We hold at their observed wages the pharmacies whose markdown at the observed wages would exceed five times the wage under a single owner and full internalization, 10 of 663 pharmacies in 2011, 16 of 773 in 2012 and 38 of 812 in 2013, entrants included.⁴⁸ Without internalization the wage is a fixed fraction of marginal revenue, $w_{jt} = \rho_{jt} \alpha / (1 + \alpha)$, so the observed wages are the solution under every ownership structure. The effect of a change in ownership on a pharmacy is the difference between its wages in the two equilibria. We pool the three years as in the quasi-experimental evidence.⁴⁹

Entrants. We treat the pharmacies that opened after the reform in two ways. To isolate the effect of ownership from that of entry, Section 7.1 removes the entrants from every ownership structure. Their workers then choose among the incumbent pharmacies and the outside option. Section 7.2 asks where the effect of the break-up lands, again with the entrants removed. Appendix Figure A3 repeats the conduct comparison with the entrants present under their actual owners.⁵⁰

7.1 Conduct and the Effects of the Break-up

We now ask how conduct changes the predicted effects of the break-up. Figure 8 disperses the ownership of the incumbent pharmacies in two steps, from a single owner per commuting zone to the actual post-reform owners and on to one owner per pharmacy, under three conducts. Every conduct fits the same observed market, since marginal revenue is backed out under each. The conducts differ in what they imply for ownership.

The first step is the break-up. Without internalization ($\lambda = \kappa = 0$), ownership does not enter

⁴⁶We follow the best practices for demand estimation and simulation laid out in Conlon and Gortmaker (2020).

⁴⁷Holding marginal revenue fixed abstracts from diminishing returns as employment changes. We expect this to matter little in our setting, since the work of a pharmacist consists mainly of dispensing prescriptions at regulated margins. Appendix C explores the curvature of production to some extent, and we will examine it further in future versions. Each step moves wages part of the way toward the right-hand side of equation (19) evaluated at the labor supply the current wages imply, $w \leftarrow (1 - \zeta) w + \zeta f(w)$. The step size ζ is adjusted between 0.03 and 0.5 according to the path of the residual $f(w) - w$, and the iteration stops when the largest residual falls below 10^{-3} in the wage units of the model.

⁴⁸The same pharmacies are held fixed under every conduct and ownership structure. Their wage equations do not hold in the counterfactual, so the solutions are equilibria for the other pharmacies given these wages. We will test the robustness of the results to this threshold in future versions.

⁴⁹We report the counterfactuals at the point estimates and do not propagate the sampling uncertainty of the labor supply and conduct estimates, which we will address in future versions.

⁵⁰The difference-in-differences of Section 4 differs from the counterfactual for two reasons. It subtracts the wage gains that spill over to control municipalities, and it contains the effect of the entrants on the wages of incumbents, although it excludes the entrants from the outcome. The counterfactual removes both and isolates the effect of the change in ownership. In future work we plan to expand considerably on the role of entrants.

the wage equation and the break-up leaves wages unchanged. At the estimated conduct, it raises the mean wage of the incumbent pharmacies by 3.3 percent. Under joint profit maximization, the same break-up would raise wages by 11.4 percent. A merger simulation under the usual assumption of joint profit maximization would thus overstate the wage effect of the break-up by a factor of more than three, and a model of monopsonistic competition would predict none. The diamond places the data next to the model: the difference-in-differences of Section 4 for the same pharmacies is 1.8 percent of the single-owner wage.⁵¹

Next, we take it to the first extreme by removing the chain structure, so that each pharmacy competes on its own. At the estimated conduct, wages would rise by a further 2.1 percent, to 5.4 percent above their level under a single owner. Under joint profit maximization they would rise to 20.6 percent. By this measure, the break-up delivered about three fifths of the wage gain that dispersing ownership fully would bring.

Panel (b) of Figure 8 shows that at the estimated conduct the break-up is mostly a transfer. Workers gain 1.4 percent of the single-owner wage per pharmacist in the market, pharmacies lose 1.1 percent, and total welfare rises by 0.3 percent, the gain from the employment that the lower markdown allows. Dispersing ownership fully would raise the workers' gain to 2.2 percent and the loss of profits to 1.8 percent, with a total gain of 0.4 percent.

7.2 Evaluating the Swedish Reform

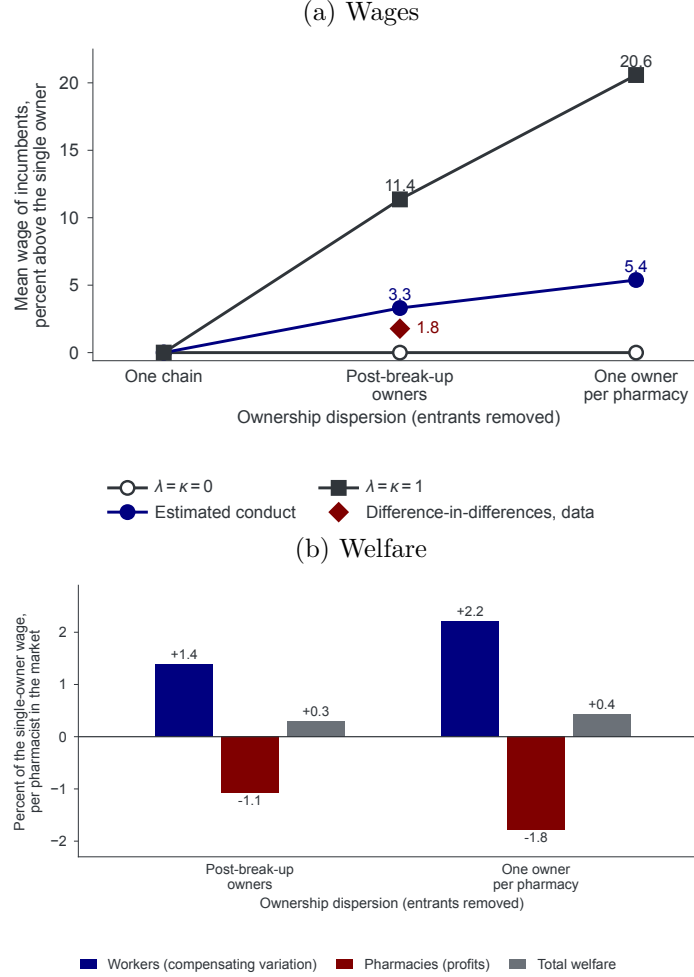
We now ask where the wage effect of the break-up lands, at the estimated conduct and with the entrants removed as before: across treated and control municipalities, across municipalities of different size, and across pharmacies.

Spillovers. The break-up raises wages in control municipalities too. Even without entry, the break-up raises incumbent wages in control municipalities by 2.0 percent, a spillover from the change in ownership elsewhere. Municipal borders contain neither the job options of pharmacists nor the networks of chains: pharmacists consider pharmacies in neighboring municipalities, and owners internalize the competition among their own pharmacies within a commuting zone.

Market Size. Figure 9 shows where the wage effect of the break-up without entry lands. The effect is hump-shaped in municipality size. It is largest, 4.4 percent, in municipalities of 50,000 to 100,000 inhabitants, and smallest at the two ends: 1.9 percent in Stockholm, 1.7 percent in Gothenburg and 2.1 percent in municipalities below 10,000 inhabitants. The two ends have different reasons. In small markets the break-up changed little: only 9 percent of the incumbent pharmacies in municipalities below 10,000 inhabitants were in treated municipalities, against 43 percent at 10,000 to 15,000 and all of them above 50,000. In large markets labor supply is more elastic, so the same change in ownership moves employment more and wages less. Across pharmacies, wages rise by 3.7 percent on average in treated municipalities and by 2.1 percent in control municipalities

⁵¹The small discrepancy with section xxx is the sample selection. CC:expand this sentence a bit but stay in my tone

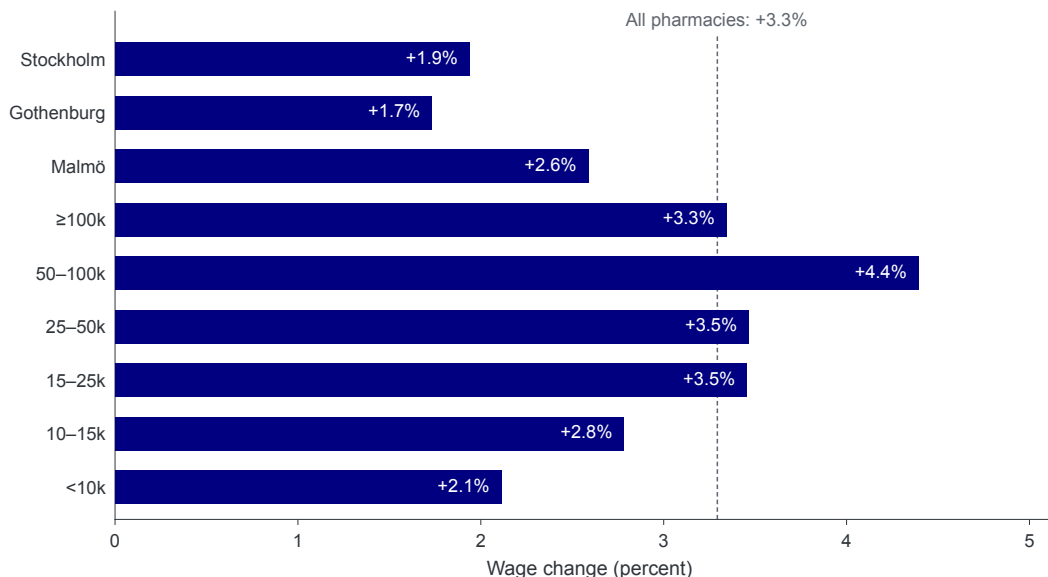
Figure 8: Conduct, Ownership Dispersion and Welfare: the Wage and Welfare Effects of the Break-up



Notes: Panel (a) shows the mean wage of the incumbent pharmacies (present by 2008) as the ownership of the pharmacies of each commuting zone is dispersed, pooled over 2011 to 2013, in percent of the mean wage under a single owner. Each line moves from the single owner (left) to the actual post-reform owners (middle) and to one owner per pharmacy (right). The diamond at the post-break-up owners is the difference-in-differences estimate in the data for the same pharmacies, treated minus control, 2011 to 2013 relative to 2008, in percent of the mean wage under the single owner. The entrants are removed from every ownership structure and their workers choose among the incumbents and the outside option. Three conducts are shown: no internalization ($\lambda = \kappa = 0$), the estimated conduct ($\lambda = 0.009$, $\kappa = 0.309$) and joint profit maximization ($\lambda = \kappa = 1$). Under each conduct, marginal revenue is backed out so that the observed wages of each year are an equilibrium under that year's owners and is then held fixed while the pharmacies are solved under each ownership structure. Pharmacies are weighted equally and the years are pooled by pharmacy counts. Panel (b) shows the change in welfare when the ownership of the incumbent pharmacies (present by 2008) is dispersed from a single owner per commuting zone to the actual post-reform owners and to one owner per pharmacy, with the entrants removed, at the estimated conduct ($\lambda = 0.009$, $\kappa = 0.309$), pooled over 2011 to 2013. Welfare is per pharmacist in the market, including pharmacists who work outside pharmacies, in percent of the mean incumbent wage under the single owner. Workers' welfare is each pharmacist's compensating variation: the proportional raise of the wages at all pharmacies, with the outside option unchanged, that makes the pharmacist as well off under the single owner as under the dispersed owners, valued at the pharmacist's expected pharmacy wage under the single owner. With log wages it has the closed form $t_i = (E_i^1/E_i^0)^{1/\alpha}$, where E_i is the pharmacist's odds of working at a pharmacy rather than outside under the dispersed (1) and the single owner (0). Profits are marginal revenue minus the wage times employment, with marginal revenue backed out so that the observed wages are an equilibrium under the actual owners and held fixed. Total welfare is workers' welfare plus profits.

(Appendix Figure A5). Control pharmacies cluster near zero, and 55 of their 310 pharmacy-years do not change at all.

Figure 9: Wage Effects of the Break-up by Municipality Size



Notes: This figure shows the wage effect of the break-up without entry at the estimated conduct, the first step of Figure 8, by municipality size, pooled over 2011 to 2013. Each bar is the mean wage change of the incumbent pharmacies of a size class divided by the mean incumbent wage under the single owner, the denominator of Figure 8, so that all pharmacies give 3.3 percent (dashed line). Municipalities are grouped by their 2010 population, with Stockholm, Gothenburg and Malmö apart. Population is from Statistics Sweden. Means weight pharmacies equally.

8 Conclusion

This paper studies how firms set wages. In particular we study a firm's response to increased competition in the labor market using the break-up of the Swedish pharmacy monopoly. The break-up raised wages in treated municipalities by about 2 percent relative to control municipalities, a lower bound as labor market competition can extend beyond municipal borders. A model of labor supply and labor demand shows that pharmacy chains internalize about 30 percent of their cross mark-downs, well below the full internalization that models assume when they translate concentration into wage effects, but above the zero internalization that most labor demand models assume. The total effect of the break-up on wages, 3.3 percent at the average incumbent pharmacy, is about one and a half times the difference-in-differences in the data.

Our paper has three direct implications. First, competition in the labor market matters. Assuming no internalization rules out wage effects from changes in ownership through this channel, while assuming full internalization can overstate their magnitude. By allowing for partial internalization, we reconcile descriptive evidence of wage responses to changes in market structure with evidence rejecting fully strategic wage setting.

Second, our findings provide empirical support for the growing attention to labor market com-

petition in antitrust enforcement.⁵² Our results show that shifts in ownership have substantial effects in the labor market through the relevant channel: firm behavior. Whether partial internalization warrants weaker enforcement depends, in part, on why firms do not fully internalize their strategic interactions. Partial internalization may reflect fairness norms or institutional constraints, such as minimum wages, but it may also arise from organizational frictions or misaligned managerial incentives. In the latter case, firms may move closer to joint profit maximization following changes in ownership or management. This possibility resonates with the findings of Acemoglu et al. (2022), who show that appointing business-trained CEOs reduces wages without corresponding gains in output growth, suggesting that managerial practices can shape wage-setting behavior. Our estimates cannot distinguish between these explanations, which we leave for future research. They highlight, however, that current conduct need not fully capture the consequences of consolidation.

Third, our results can inform not only which mergers to prevent, but also where and how interventions to strengthen labor market competition would be most effective. A central idea of the monopsony literature is that countervailing forces, such as minimum wages and unions, can offset the power of employers (Manning, 2003; Caldwell et al., 2026). Beyond antitrust, this perspective can for instance inform the targeting and design of regional aid. The relevant question is not simply where employment is scarce or employer concentration is high, but where additional jobs would provide meaningful alternatives to workers. Aid that attracts an independent employer may have different wage effects from aid that expands an incumbent, even when both create the same number of jobs. Unlike divestitures, entry creates new employment opportunities rather than merely changing their ownership, and can therefore strengthen competition through a different channel. It is therefore important to understand not only where additional jobs are created, but also how they affect workers’ outside options and firms’ incentives to compete for them.

Taken together, our results highlight the importance of understanding both labor supply and firm conduct. Workers’ substitution patterns determine the potential for labor market power, while firms’ wage-setting behavior determines how much of that power they exercise. In our companion paper Friederiszick et al. (2026), we develop a method to estimate job-level labor supply curves throughout the Swedish labor market, providing a broader picture of where this potential for labor market power arises. Understanding both dimensions is essential for predicting how changes in ownership and competition affect workers’ wages.

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⁵²See the 2023 U.S. Merger Guidelines, Guideline 10, and the European Commission’s 2026 draft Merger Guidelines, paragraphs 160–162.

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Appendix

A Internalization as a Weight on Co-Owned Profits

Each pharmacy j can internalize profits made in other pharmacies within this chain $z(j)$ in the same commuting zone $cz(j)$:

$$\Pi_{jt} = \pi_{jt} + \kappa \sum_{r \in \mathcal{C}_{jt} \setminus \{j\}} \pi_{rt}, \quad \kappa \in [0, 1]. \quad (23)$$

Here the parameter $\kappa \in [0, 1]$ governs the degree of internalization across pharmacies within the same chain.⁵³

When $\kappa = 0$, each pharmacy maximizes its own profit and competes for workers with co-owned pharmacies just as it does with other employers. When $\kappa = 1$, it fully accounts for profits at its co-owned pharmacies within the commuting zone. Intermediate values capture partial internalization, which could reflect imperfect alignment between local managers and the objectives of the chain.

The first-order condition of this objective is the wage equation of the model with the cross markdown weighted by κ ,

$$w_{jt} = \rho_{jt} - \mathcal{M}_{jt}^{\text{own}} - \kappa \mathcal{M}_{jt}^{\text{cross}}, \quad (24)$$

which is the estimating equation (19) at $\lambda = 1$.

⁵³This parameter resembles the internalization parameters used in industrial organization to study product markets. These usually capture internalization either across vertical divisions within a firm (Crawford et al., 2018) or across horizontal competitors, through common ownership (Backus et al., 2021) or collusion (Miller and Weinberg, 2017). A notable exception is Bhattacharya et al. (2026), who allow merging parties, horizontal competitors that now belong to the same firm, to internalize each other's profits imperfectly and study the resulting price effects.

Table A1: Pharmacies, 2004–2013

Year	All	Retail	Other
2004	897	879	18
2005	923	902	21
2006	938	917	21
2007	939	917	22
2008	1,007	944	63
2009	1,015	951	64
2010	1,183	1,118	65
2011	1,294	1,231	63
2012	1,315	1,255	60
2013	1,291	1,251	40

Notes: This table counts pharmacies in November of each year. A pharmacy is an establishment with the industry code for retail pharmacy (SNI 47730 or its predecessor codes), and establishment codes that continue the same workplace are linked into one pharmacy. Retail pharmacies are the pharmacies registered as retail pharmacies (*öppenvårdsapotek*) by the Swedish eHealth Agency and the pharmacies that cannot be matched to the register. They form the sample of the analysis. Other pharmacies are all remaining units, among them hospital, dose-dispensing, distance, veterinary and administrative units.

Table A2: Licensed Pharmacists and Pharmacists at Retail Pharmacies, 2004–2013

Year	Licensed pharmacists	At retail pharmacies
2004	10,327	5,220
2005	10,498	5,111
2006	10,767	5,152
2007	11,061	5,169
2008	11,243	5,064
2009	11,426	4,981
2010	11,639	5,186
2011	11,994	5,385
2012	12,183	5,437
2013	12,275	5,495

Notes: This table counts pharmacists in November of each year. A licensed pharmacist is a person with the education of an *apotekare* or a *receptarie* (Section 3) who is in the registers in that year, of any age. At retail pharmacies counts the licensed pharmacists employed at a retail pharmacy in a treated or control municipality, the headcount of Figure 3, not counting the workers of the 2008 hospital-supply spin-off in any year.

Table A3: Ownership of Retail Pharmacies, 2004–2013

Year	Firms	By retail pharmacies per firm			Share in firms with 10 or more (%)
		10 or more	2 to 9	1	
2004–2008	1	1	0	0	100.0
2009	14	10	0	4	99.5
2010	103	13	11	79	90.3
2011	152	13	14	125	86.9
2012	166	11	16	139	85.7
2013	172	8	19	145	84.1

Notes: This table counts the firms that run retail pharmacies in November of each year. A firm is the legal employer (organization number) of a pharmacy’s staff. Until July 2009, the state-owned Apoteket AB held the legal monopoly on retail pharmacies, and its companies count as one firm in every year. Firms are grouped by the number of retail pharmacies they run in the year. The last column reports the share of retail pharmacies run by firms with 10 or more.

Table A4: Effects on the Wage Premium

	Wage Premium (EUR per month) (1)	Log Wage Premium ($\times 100$) (2)
<i>DiD: Year FEs</i>		
Treatment (2010–2013)	72 (22)	2.14 (0.68)
Pre-Period (2005–2007)	9 (14)	0.26 (0.44)
<i>DiD: County-Year FEs</i>		
Treatment (2010–2013)	83 (22)	2.54 (0.70)
Pre-Period (2005–2007)	24 (14)	0.73 (0.44)
2008 Average (Control):	3,050	10.371
2008 Average (Treated):	3,063	10.376
N, Pharmacy-Years (Control):	1,048	1,048
N, Pharmacy-Years (Treated):	4,383	4,383

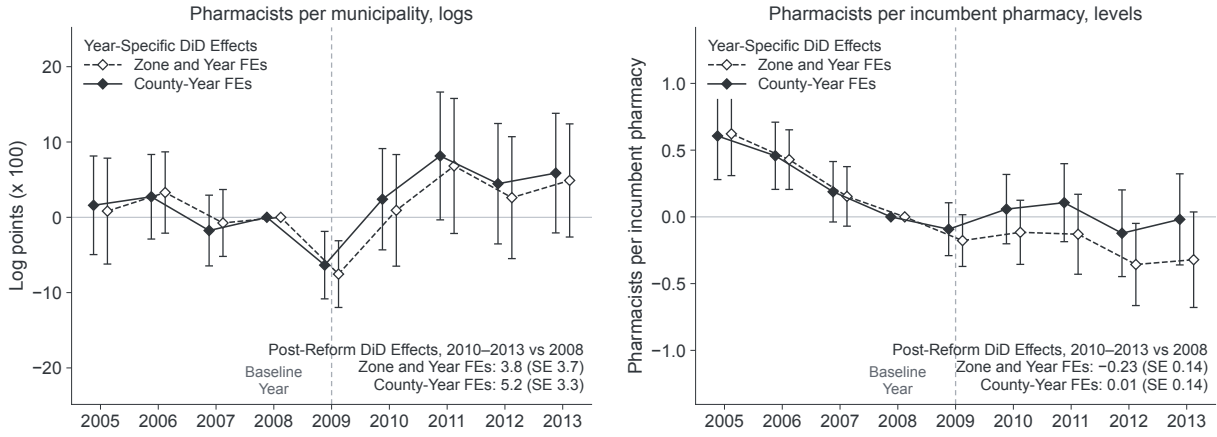
Notes: This table reports the difference-in-differences estimates of Figure 2, Panel (b), for the wage premium of incumbent pharmacies (present by 2008), adjusted for observed worker composition, in 139 treated and 150 control municipalities, equally weighted. Treatment is the mean of the 2010 to 2013 event-study coefficients relative to 2008, with its standard error from their covariance matrix. Pre-Period is the mean of the 2005 to 2007 coefficients relative to 2008, a test of parallel trends before the reform. The event study excludes 2009, the year of the sale. The first panel includes municipality and year fixed effects, and the second replaces the year effects with county-by-year fixed effects, the specification of Section 6. Amounts are in euros per month at 2020 prices, and the 2008 averages of the log premium are in log SEK. Standard errors clustered by municipality are in parentheses.

Table A5: Robustness of the Wage Effect

	EUR per month		Log points $\times$ 100	
	DiD	N	DiD	N
Baseline	72 (22)	5,431	2.14 (0.68)	5,431
Headcount weights	78 (23)	5,431	2.27 (0.72)	5,431
Pharmacy fixed effects	63 (22)	5,402	1.87 (0.69)	5,402
Post period 2011–2013	72 (25)	5,431	2.12 (0.80)	5,431
Base year 2007	63 (22)	5,431	1.85 (0.69)	5,431
Including entrants	133 (24)	6,055	3.78 (0.74)	6,055
Fixed 2008 weights	74 (23)	5,401	2.16 (0.72)	5,401
Balanced pharmacies	70 (22)	4,973	2.12 (0.70)	4,973
Excluding the three largest cities	65 (22)	4,656	1.95 (0.69)	4,656
Raw monthly wage	2 (28)	26,262	–	–
AKM pharmacy premium	92 (21)	26,262	–	–
Inverse probability weights	69 (25)	21,794	–	–
One-to-one matching	86 (27)	7,173	–	–
Commuting-zone treatment	96 (48)	5,431	2.99 (1.57)	5,431
Isolated controls	97 (41)	4,641	2.99 (1.33)	4,641
Treatment from the 2009 sale packages	69 (21)	5,431	2.06 (0.67)	5,431
County-by-year fixed effects	83 (22)	5,431	2.54 (0.70)	5,431
County-by-year fixed effects, post period 2011–2013	84 (25)	5,431	2.52 (0.79)	5,431

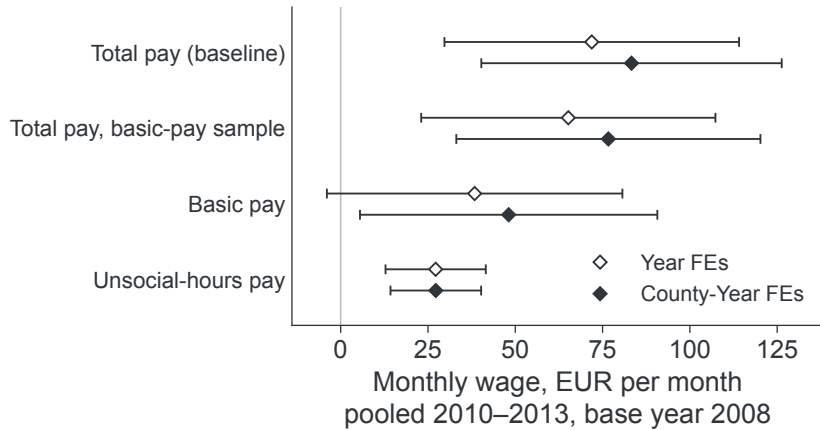
Notes: This table reports difference-in-differences estimates of the effect of the break-up on the wage premium of retail pharmacies present by 2008, adjusted for observed worker composition, in 139 treated and 150 control municipalities: the mean of the 2010 to 2013 event-study coefficients relative to 2008 (2011 to 2013 in the post-period rows, relative to 2007 in the base-year row), with its standard error from their covariance matrix. Each row changes one element of the baseline, which includes municipality and year fixed effects. Worker-level rows (raw wage, AKM premium, weighting and matching) use pharmacist-year observations. The commuting-zone row assigns treatment by zone and clusters by zone. Amounts are in euros per month at 2020 prices. Standard errors clustered by municipality are in parentheses.

Figure A1: Employment Event Studies in the Alternative Functional Form



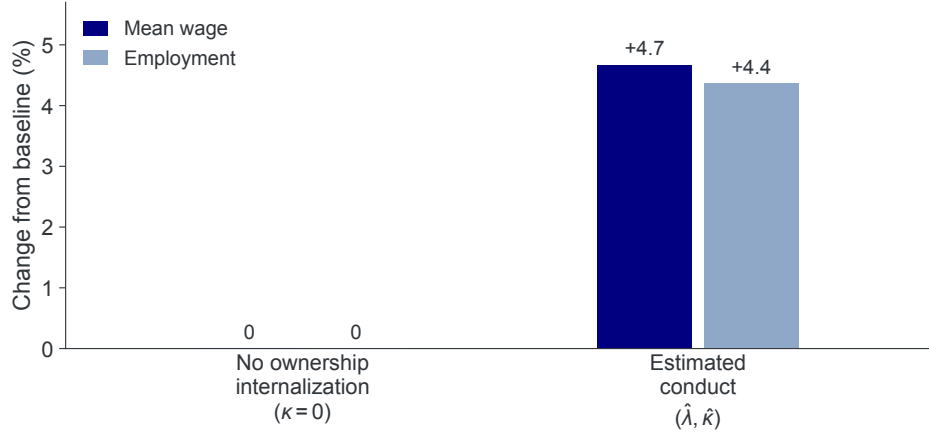
Notes: This figure repeats the event studies of Figures 3, panel (a), and 3c with the outcome in the other functional form. The left panel is the log number of pharmacists employed at retail pharmacies by municipality of work, in log points times 100; municipality-years without a pharmacist drop out. The right panel is the number of pharmacists at incumbent retail pharmacies (present by 2008), in levels, at the pharmacy level. Both are unweighted. The regressions include commuting-zone and year fixed effects and a treated indicator, and the estimates reported in the panels are the mean of the 2010 to 2013 coefficients relative to 2008, with standard errors from their covariance. The dashed vertical line marks the sale, announced on 9 November 2009. Bars are 95 percent confidence intervals based on standard errors clustered by commuting zone. Hollow markers use commuting-zone and year fixed effects with a treated indicator and zone clusters; filled markers municipality and county-by-year fixed effects with municipality clusters.

Figure A2: Wage Effect by Pay Component



Notes: This figure splits the wage effect of Figure 2, panel (d), by pay component. The first row is the baseline effect on total pay. The other rows use the pharmacy-years with a basic salary in the wage register: total pay on that sample, the basic salary, and unsocial-hours pay (evening, night, weekend and shift supplements). Each estimate is the mean of the 2010 to 2013 difference-in-differences coefficients relative to 2008, in euros per month at 2020 prices, with municipality and year fixed effects (hollow markers) or municipality and county-by-year fixed effects (filled markers). Bars are 95 percent confidence intervals based on standard errors clustered by municipality.

Figure A3: Conduct and the Effects of the Break-up with the Entrants Present



Notes: This figure repeats Figure 8 for the whole post-reform network, incumbents and entrants, without ownership internalization ($\kappa = 0$, with λ at its estimate) and at the estimated conduct ($\lambda = 0.009$, $\kappa = 0.309$). Ownership moves from a single owner of every pharmacy in each commuting zone, the baseline, to the actual post-reform owners, holding the set of pharmacies fixed. Under each conduct, marginal revenue is backed out so that the observed wages of each year are an equilibrium under that year's owners, and it is held fixed when ownership changes. The wage effect is in percent of the mean wage under the single owner, with pharmacies weighted equally. The employment effect is the change in the expected number of pharmacists employed at all pharmacies, in percent of its value under the single owner. The years are pooled by pharmacy counts for wages and by expected employment for employment.

Table A6: Internalization by Post-Reform Year

	2011	2012	2013	2011–2013
λ , own markdown	0.008 (0.027)	0.007 (0.015)	0.010 (0.021)	0.010 (0.016)
κ , cross markdown	0.200 (0.124)	0.292 (0.122)	0.427 (0.204)	0.296 (0.121)
κ , without the two controls	0.212 (0.129)	0.305 (0.120)	0.431 (0.202)	0.309 (0.122)
First-stage F , own markdown	15.2	39.5	33.1	61.6
First-stage F , cross markdown	13.0	18.6	6.7	14.4
Pharmacy-years	466	523	509	1,498

Notes: This table reports the specification of column (4) of Panel A of Table 2, with both controls, estimated on one post-reform year at a time and on the pooled window, each year differenced from 2008. The third row omits the two controls, as in column (1) of Table 2. First-stage F statistics are Sanderson–Windmeijer statistics. Standard errors clustered by municipality are in parentheses.

Table A7: Internalization by the Manager

A. Manager changed since 2008			C. The manager's education		
	(1) κ varies	(2) Both vary		(1) κ varies	(2) Both vary
λ , own markdown	0.010 (0.015)		λ , own markdown	-0.003 (0.165)	
λ , same manager		0.002 (0.016)	λ , other education		0.124 (0.648)
λ , new manager		0.014 (0.021)	λ , pharmacist		0.052 (0.093)
κ , same manager	0.291 (0.159)	0.323 (0.144)	κ , other education	-0.579 (4.414)	1.277 (4.966)
κ , new manager	0.297 (0.155)	0.301 (0.160)	κ , pharmacist	0.297 (0.158)	0.283 (0.199)
Equal κ , p -value	0.98	0.91	Equal κ , p -value	0.84	0.84
Equal λ , p -value		0.62	Equal λ , p -value		0.90
All $\kappa = 0$, p -value	0.035	0.030	All $\kappa = 0$, p -value	0.17	0.22
All $\lambda = 0$, p -value	0.52	0.79	All $\lambda = 0$, p -value	0.98	0.75
Pharmacy-years, same / new		359 / 1,139	Pharmacy-years, other / pharmacist		40 / 370
B. The 2008 manager left					
	(1) κ varies	(2) Both vary			
λ , own markdown	0.009 (0.016)				
λ , manager stayed		0.002 (0.020)			
λ , manager left		0.016 (0.022)			
κ , manager stayed	0.278 (0.141)	0.291 (0.137)			
κ , manager left	0.327 (0.175)	0.324 (0.174)			
Equal κ , p -value	0.81	0.86			
Equal λ , p -value		0.60			
All $\kappa = 0$, p -value	0.045	0.042			
All $\lambda = 0$, p -value	0.55	0.75			
Pharmacy-years, stayed / left		667 / 831			

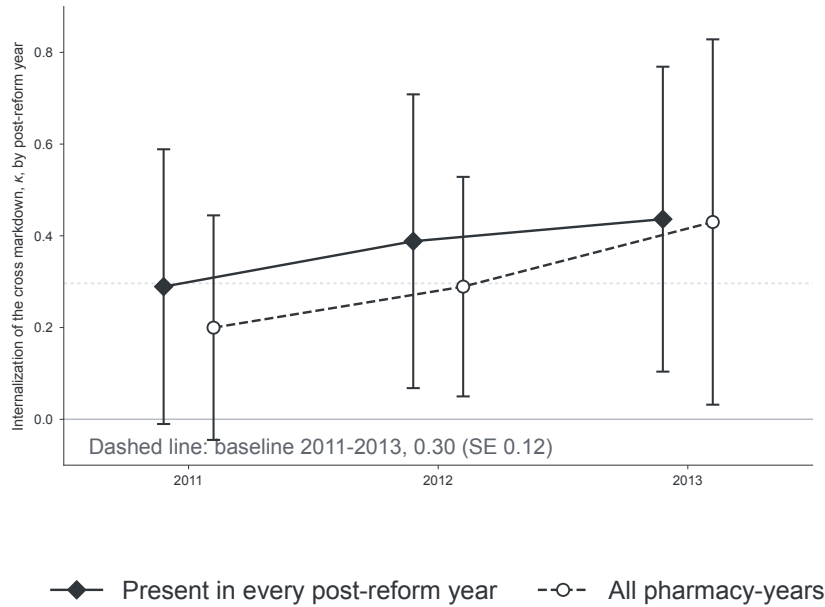
Notes: This table reports two stage least squares estimates of equation (21) on the sample, controls and years of Panel A of Table 2 (baseline λ 0.010 (0.016), κ 0.296 (0.121)). The weights are instrumented by D_j interacted with the group indicators, which are among the controls; column (1) lets κ vary by group and column (2) both weights. Each attribute is measured in the post-reform year. In Panels A and B the manager is the pharmacy's highest-paid employee (register earnings). Panel C uses the employee in the manager occupation (SSYK major group 1), observed for 1,352 of the 1,574 pharmacy-years, and splits by whether that manager is pharmacist-educated; the group without a pharmacist education has a first-stage F of 5. The p -values are from Wald tests that the weights are equal across groups or jointly zero. Pharmacy-years by group, 2011 to 2013. Panel C is estimated on 2011 alone, because in 2012 and 2013 the group without a pharmacist education has too few treated or control pharmacies. Appendix Table A8 describes the attributes. Standard errors clustered by municipality are in parentheses.

Table A8: Variation in the Manager Attributes

	2011	2012	2013
A. The head and the manager of the pharmacy			
Roster pharmacies with a record	791	782	773
Head changed since 2008	539 (68%)	589 (75%)	621 (80%)
Head with a business education	18 (2%)	18 (2%)	12 (2%)
Employee in the manager occupation	627	614	576
of which business-educated	17 (3%)	13 (2%)	13 (2%)
of which pharmacist-educated	558 (89%)	543 (88%)	504 (88%)
Manager occupation recorded in 2008	140	138	138

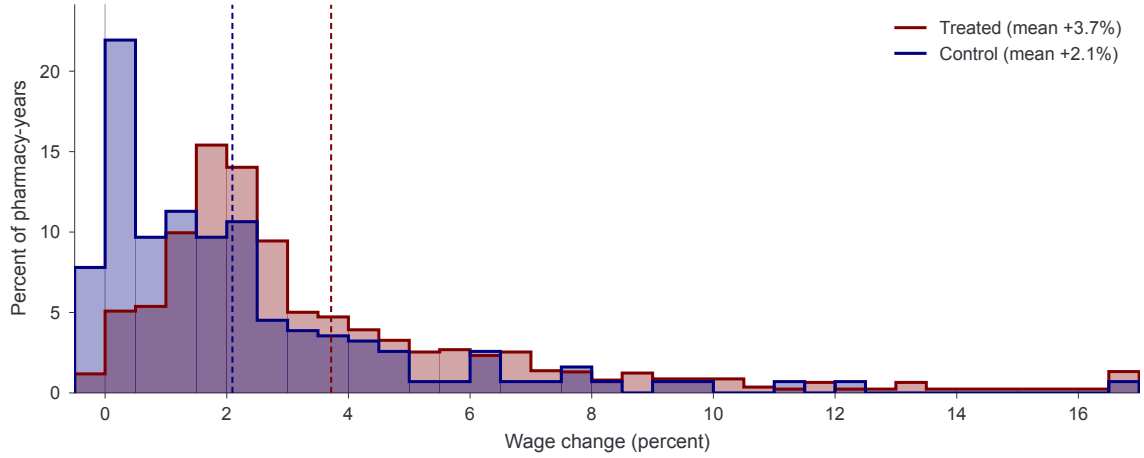
Notes: This table describes the pharmacies of the 2008 roster in each post-reform year; percentages are of the pharmacies with the attribute coded. The head of a pharmacy is its highest-paid employee (register earnings per month), the attribute of Panels A and B of Appendix Table A7; the manager occupation is SSK major group 1, recorded for only 18 percent of pharmacies in 2008, so the occupation-based splits on a change of manager, the manager's departure and a business education cannot form two groups and are not reported; the split on a pharmacist education can from 2011 (Panel C of Appendix Table A7).

Figure A4: The Cross-Markdown Weight by Post-Reform Year



Notes: This figure plots κ estimated separately for each post-reform year, with a single λ , the treatment indicator interacted with the year as instruments, and county-by-year fixed effects. The yearly weights come from the pooled 2011 to 2013 specification with both controls, as in Appendix Table A6. Filled markers use the pharmacies present in every post-reform year and hollow markers all pharmacy-years. The dashed horizontal line is the baseline estimate of Table 2. Bars are 95 percent confidence intervals based on standard errors clustered by municipality.

Figure A5: Wage Effects of the Break-up across Pharmacies



Notes: This figure shows the distribution of the wage effect of the break-up without entry, the first comparison of Figure 8, across incumbent pharmacies at the estimated conduct, pooled over 2011 to 2013. Each observation is a pharmacy in a year, and its wage change is in percent of its own wage under the single owner. The 1,376 pharmacy-years in treated municipalities and the 310 in control municipalities are each shown in percent of their group. Dashed lines mark the means, 3.7 percent for treated and 2.1 percent for control pharmacies. Over all pharmacies the mean is 3.4 percent, against 3.3 percent in Figure 8, which divides the mean change by the mean wage. Bins are 0.5 percentage points wide. The first bin includes changes below -0.5 percent and the last changes of 16.5 percent or more. Pharmacies whose wage does not change (27 treated and 55 control pharmacy-years) are in the bin from 0 to 0.5 percent. Bins with one to four pharmacy-years are suppressed for confidentiality and drawn at the average suppressed count of their group.

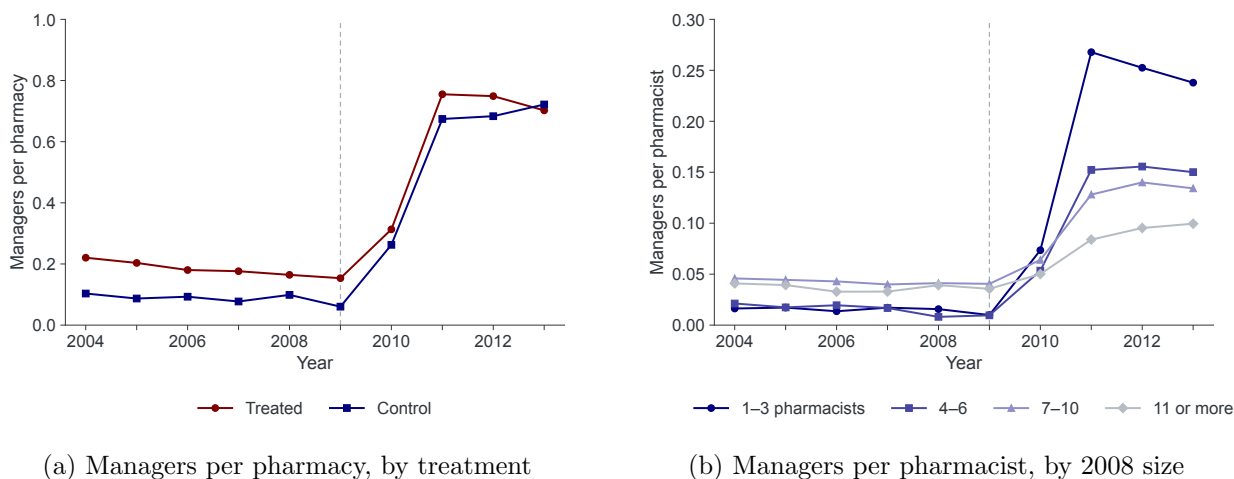
B Managers and the Occupation Adjustment

After the reform, most retail pharmacies appointed one of their pharmacists as manager. Figure A6, Panel (a), counts managers per pharmacy, defined as licensed pharmacists with an occupation code in major group 1 of the Swedish Standard Classification of Occupations. Before 2009, coded managers worked mostly at large pharmacies, and the average pharmacy had 0.10 managers in control and 0.16 in treated municipalities. By 2011, the average had risen to 0.67 and 0.76.

The promotion came with a pay rise. Table A9 compares the pay growth from 2010 to 2011 of pharmacists who became managers at their own pharmacy with that of pharmacists who were not promoted. Total pay grew 1.7 log points more than among pharmacists who remained non-managers and 2.8 log points more than among their non-promoted coworkers. The rise is concentrated in basic pay.

Each pharmacy added about one manager regardless of its size, so the occupational mix changed most at small pharmacies (Figure A6, Panel (b)). In 2011, managers were 27 percent of pharmacists at pharmacies with one to three pharmacists and 8 percent at pharmacies with eleven or more. Control pharmacies are smaller than treated pharmacies, with 3.9 against 5.1 pharmacists in 2011, and their share of managers rose from 2 to 17 percent against 3 to 15 percent. Without an adjustment for occupation, the higher pay of the new managers would raise the average pay of control pharmacies more than that of treated pharmacies. The wage premium therefore controls for one-digit occupation (Section 3).

Figure A6: Managers at Retail Pharmacies, 2004–2013



Notes: This figure shows licensed pharmacists coded as managers at retail pharmacies present by 2008. A manager is a pharmacist with an occupation code in major group 1 of the Swedish Standard Classification of Occupations. Panel (a) plots managers per pharmacy in treated and control municipalities. Panel (b) plots managers per pharmacist by the number of pharmacists a pharmacy employed in 2008. The dashed vertical line marks the 2009 sale. The series end in 2013 because the occupation classification changes in 2014.

Table A9: Pay Growth at Promotion to Manager, 2011

	Relative to non-managers (1)	Relative to coworkers (2)
Total pay	1.67 (0.58)	2.81 (0.69)
Basic pay	2.53 (0.58)	3.55 (0.67)
Promoted pharmacists	253	202

Notes: This table reports the change in log monthly pay from 2010 to 2011 of licensed pharmacists who became managers at their own retail pharmacy in 2011, relative to two comparison groups. Column (1) compares them with pharmacists who stayed at their pharmacy as non-managers in both years. Column (2) compares each promoted pharmacist with the non-promoted pharmacists of the same pharmacy. A manager is a pharmacist with an occupation code in major group 1 of the Swedish Standard Classification of Occupations. Total pay is the full-time equivalent monthly pay of the wage survey, and basic pay is its fixed component. The last row counts promoted pharmacists with total pay in both years. In column (2), basic pay is observed for 200 of them. Values are in log points ($\times 100$). Standard errors are in parentheses.

C Markdowns from a Production Function

The wage equation (19) does not measure marginal revenue. It backs ρ_{jt} out of observed wages and the estimated conduct. In pharmacies, marginal revenue can also be measured from the production side. The price of a dispensed package is regulated, and every dispensed package is bought at a regulated purchase price (Section 2). We use these two features to compute markdowns from a production function, following Yeh et al. (2022), and compare them with the markdowns the model implies, as De Loecker and Scott (2026) compare markups from production and demand data.

Production Function. Output is the gross margin on prescription medicines, $G_{jt} = AUP_{jt} - AIP_{jt}$: the sales of pharmacy j in year t at the regulated pharmacy retail price (AUP) less the same packages at the regulated pharmacy purchase price (AIP). A pharmacy cannot substitute staff for medicines, because each package it dispenses is a package it buys. Medicines therefore enter in fixed proportions, and dispensing requires staff:

$$G_{jt} = \min \left\{ AIP_{jt}/a_{jt}, L_{jt}^{\theta_L} O_{jt}^{\theta_O} e^{\omega_{jt}} \right\},$$

where L_{jt} and O_{jt} are the annual average numbers of pharmacists and of other staff at the pharmacy.⁵⁴ The coefficient a_{jt} is the purchase value of medicines per krona of margin: at the median pharmacy, the margin is about 20 percent of retail sales, so the pharmacy buys about four kronor

⁵⁴Besides pharmacy technicians, other staff include cashiers and pharmacy assistants. Pharmacists make up 59 percent of the staff of retail pharmacies in 2008. Thoresson (2024) reports, based on public records, that pharmacists make up over 60 percent of the pharmacy workforce and pharmacy technicians around one quarter.

of medicines for each krona of margin it earns. Pharmacies buy medicines to match dispensing, so the two terms are equal, and

$$\log G_{jt} = \theta_L \log L_{jt} + \theta_O \log O_{jt} + \omega_{jt}, \quad (25)$$

where ω_{jt} collects everything that raises the margin at given staffing: productivity in dispensing and the value of what is dispensed. Medicines have no output elasticity and enter only through their cost. This is the Leontief structure under which a production function in value added is well defined (Akerberg et al., 2015).

Pharmacies have no discretion over prescription prices (Section 2), so they cannot add a product markup to them. We therefore assume that the regulated price equals the marginal cost of a dispensing and attribute the whole gap between the marginal revenue product and pay to the labor markdown.⁵⁵ The marginal revenue product of a pharmacist is then $MRPL_{jt} = \theta_L G_{jt}/L_{jt}$, and the markdown is

$$\nu_{jt} = \frac{MRPL_{jt}}{W_{jt}/L_{jt}} = \frac{\theta_L G_{jt}}{W_{jt}}, \quad (26)$$

where W_{jt} is the gross annual payroll of the pharmacy's pharmacists, as in Yeh et al. (2022). The number of pharmacists cancels, so only θ_L depends on how labor is measured.

Estimation. We proxy productivity with observables, $\omega_{jt} = x'_{jt}\beta^P + \eta_{c(j)t} + u_{jt}$, where x_{jt} contains log population density and the number of competing pharmacies within one kilometer, weighted by inverse distance, and $\eta_{c(j)t}$ are county-by-year fixed effects. We estimate equation (25) by OLS. The identifying assumption is that u_{jt} is uncorrelated with $\log L_{jt}$ and $\log O_{jt}$ given x_{jt} and $\eta_{c(j)t}$: after these controls, the remaining productivity does not drive staffing. The elasticities are therefore identified from differences across pharmacies. This is a conditional production relationship, not a causally identified staffing experiment: unobserved demand and productivity can still affect staffing.⁵⁶

Data. Output comes from the prescription data of the Swedish eHealth Agency (Section 3). We estimate on the years 2009 to 2013 and keep pharmacy-years with sales in all twelve months, a positive margin and staff in every month. We count pharmacists and other staff as annual averages of the monthly number of employees at the pharmacy, from the employment spells in JOBB, so that staffing is measured over the same year as output and payroll. Pharmacists are the licensed

⁵⁵With a product markup, the production approach separates it from the markdown (Yeh et al., 2022). If the regulated price exceeded the marginal cost of a dispensing by a factor μ , a dispensing would be worth $AUP/\mu - AIP$, and the marginal product would fall by the share $(1 - 1/\mu)/m$, where $m \approx 0.2$ is the margin rate. At a product markup of 2.5 percent, the wage share in Panel A of Table A11 would rise to 0.86 before the reform and 0.89 after it.

⁵⁶We do not use the control-function approach, which recovers productivity from its law of motion (Akerberg et al., 2015; De Loecker and Scott, 2026). Its moments rely on staffing that responds to productivity within a pharmacy. Within a pharmacy, however, the margin moves little with staffing. With pharmacy fixed effects the elasticity is 0.13, and 0.14 in 2010–2013 with the annual-average number of pharmacists. It is a short-run elasticity: changes in staffing within a pharmacy are small and often temporary, such as a vacancy that is filled again within the year. Implemented on our data, the control-function estimator does not pin down a unique elasticity.

pharmacists of Section 3, and other staff are all other employees of the pharmacy. The payroll W_{jt} is the annual earnings that these pharmacists receive from the pharmacy in JOBB. It therefore includes supplements and varies with hours worked. The data start in 2009, so we backcast the margin of 2008. For each pharmacy, we deflate its 2009 sales by its county’s growth in dispensed prescriptions from 2008 to 2009 and by the national change in prices, and multiply them by its own margin rate in January to June 2009, before the sale and before the regulated margin rose in November 2009. The backcast enters only the comparison before the reform, not the estimation.

Estimates. Table A10 reports the elasticities. With pharmacists and other staff, column (2), the output elasticity of pharmacists is 0.68 (0.03) and that of other staff 0.18 (0.01), and returns to scale are 0.86. With pharmacists only, column (1), it is 0.87 (0.02). The two columns mark the ends of a range. Column (1) credits pharmacists with the work of all staff beside them, while column (2) treats every other employee as an input to dispensing. Most other staff are pharmacy technicians (*apotekstekniker*), who assist pharmacists in dispensing. No educational or occupational code identifies technicians over our period (Thoresson, 2024), so all other employees proxy for them. Because most other staff are technicians, we expect the pharmacist elasticity to lie closer to column (2), and we use it as the baseline.⁵⁷

Markdowns. Table A11 reports the wage as a share of the marginal product, w/ρ , at the median pharmacy, as Figure 7 does for the model. Panel A takes the marginal product from the production function and the wage as employer cost. With a proportional payroll tax, equation (16) holds with $\rho_{jt} = MRPL_{jt}/g_t$, where g_t is one plus the statutory employer social security contribution rate.⁵⁸ Each ratio uses its own wage: payroll per pharmacist on the production side and the wage premium in the model. Before the reform, pharmacists received 0.75 of their marginal product, and 0.79 after it. Panel B reports the same share from the wage equation under the three conducts of Figure 7, on the same pharmacies: 0.85 under monopsonistic competition, 0.79 and 0.83 at the estimated conduct, and 0.45 and 0.54 under full internalization. The production function places the wage about 0.04 further below the marginal product than the estimated conduct, and far above full internalization, and both shares rise after the reform by about the same amount. With pharmacists only in the production function, the share is 0.58 and 0.61, between the estimated conduct and full internalization.

Limitations. The production markdown inherits the level of θ_L , which is identified from differences across pharmacies only. Output covers only prescription medicines in the pharmaceutical benefit scheme. Before the reform, 44 of the 562 pharmacies of the baseline sample, all in treated municipalities, have no production data. Finally, the comparison speaks to average levels, not

⁵⁷With the numbers of pharmacists and other staff in November instead of their annual averages, on the same pharmacy-years, the pharmacist elasticity is 0.61 (0.02).

⁵⁸The pharmacy pays g_t kronor for each krona of wages, so the first-order condition behind equation (16) equates $MRPL_{jt}$ with g_t times the wage and the markdown terms. We use the statutory rate, 32.42 percent in 2008 and 31.42 percent from 2009, so g_t is 1.3242 and 1.3142. It leaves out other labor costs, such as occupational pension contributions.

to which pharmacies have high markdowns, and it does not identify κ , because the level of the production markdown scales with θ_L .

Table A10: Production Function Estimates

	(1) Pharmacists only	(2) Pharmacists and other staff
Log pharmacists, θ_L	0.874 (0.021)	0.680 (0.026)
Log other staff, θ_O		0.180 (0.013)
Returns to scale		0.859 (0.021)
Pharmacy-years	4,152	4,152
Pharmacies	1,031	1,031
R^2	0.577	0.613

Notes: This table reports OLS estimates on the same pharmacy-years. The dependent variable is the log gross margin on prescription medicines. Pharmacists and other staff are log annual-average head counts from monthly employment spells. The sample is the pharmacy-years from 2009 to 2013 with staff in every month. Both columns include county-by-year fixed effects, $\log(1 + \text{population density})$ and the number of competing pharmacies within one kilometer, weighted by inverse distance. Other staff enter in logs, with an indicator for pharmacies without other staff. Standard errors clustered by pharmacy are in parentheses.

Table A11: Markdowns from the Production Function and by Conduct, Before and After the Reform

	Before the reform (2008)	After the reform (2011–2013)
A. Production function		
Pharmacists and other staff	0.75	0.79
Pharmacists only	0.58	0.61
B. Model		
Monopsonistic competition ($\lambda = \kappa = 0$)	0.85	0.85
Estimated conduct ($\hat{\lambda} = 0.01$, $\hat{\kappa} = 0.31$)	0.79	0.83
Full internalization ($\lambda = \kappa = 1$)	0.45	0.54
Pharmacies / pharmacy-years	518	1,434

Notes: This table reports the wage as a share of the marginal product, w/ρ , at the median pharmacy of the baseline sample of Table 2, on the pharmacy-years with production data. Before the reform uses each pharmacy's 2008 value once and after the reform the pharmacy-years of 2011 to 2013. In Panel A, the marginal product is that of the production function, $\theta_L G_{jt}/L_{jt}$, with θ_L from Table A10 (column (2), and column (1) for pharmacists only), and the wage is employer cost per pharmacist, $g_t W_{jt}/L_{jt}$, where g_t is one plus the statutory employer social security contribution rate (1.3242 in 2008 and 1.3142 from 2009). Output in 2008 is the backcast margin. In Panel B, under each conduct the marginal product is backed out from the observed wage, as in Figure 7, on the pharmacies of Panel A, so the medians differ slightly from that figure, which uses every pharmacy of the baseline sample. The wage coefficient is $\alpha = 5.71$.

D Opening Hours and Unsocial-Hours Pay

Part of the wage increase in Figure 2, Panel (d), is unsocial-hours pay, the supplements for evening, night and weekend work. The collective agreement sets each supplement as a fixed percentage of the hourly wage, by time band. At fixed rates, more unsocial-hours pay means more unsocial hours. This appendix asks where these hours came from.

Two sources cover opening hours and working time, in different years. Opening hours come from three cross-sections. For November 2007 and November 2008, the Swedish Agency for Public Management recorded each pharmacy’s total weekly opening hours, but not when the pharmacy was open (Swedish Agency for Public Management, 2013). For week 46 of 2012, the Swedish Agency for Health and Care Services Analysis recorded each pharmacy’s full weekly schedule, which shows its opening hours in each time band of the pay agreement and whether it opened on Saturday afternoon and Sunday (Bergman et al., 2014). We therefore observe total weekly opening hours in 2007, 2008 and 2012, but their timing only in 2012. We link both sources to our incumbent retail pharmacies and drop hospital pharmacies; 619 treated and 173 control pharmacies have opening hours in 2008.

Pay and working time come from the Wage Structure Statistics (Lönestrukturstatistiken). Every September, they record each pharmacist’s full-time basic salary, the unsocial-hours supplements paid in cash and contracted working time as a percentage of full time.⁵⁹ We measure paid unsocial hours as unsocial-hours pay divided by the hourly wage (the full-time basic salary over 172 hours), so that an hour paid at a 50 percent supplement counts as half an hour. Pharmacist headcounts come from the employment register in November. As for wages, we report each change relative to 2008 with municipality and year fixed effects, followed in parentheses by the estimate with municipality and county-by-year fixed effects.

Treated pharmacies extended their opening hours after the break-up (Table A12, Panel A). Relative to control pharmacies, their weekly opening hours rose by 2.1 hours from 2008 to 2012 (SE 0.6; county-by-year: 1.9, SE 0.5). In 2012, conditional on county, treated pharmacies were open 2.7 more hours per week in unsocial time bands than control pharmacies (SE 0.5; Table A13) and were 22 and 18 percentage points more likely to open on Saturday afternoons and Sundays. Because we observe schedules only for 2012, this comparison cannot tell us whether these hours are new.

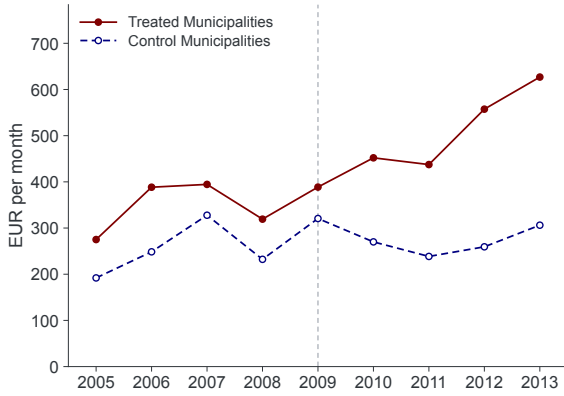
The wage survey can, for a smaller sample of pharmacists, because it records unsocial-hours pay in every year. In 2008, pharmacists in treated and control pharmacies were paid for the same unsocial hours, 2.5 per month, and the difference did not trend from 2005 to 2009. From 2010, paid unsocial hours rose in treated pharmacies but not in control pharmacies, reaching 5.2 against 2.8

⁵⁹The survey does not record when pharmacists work. It covers 83 to 92 percent of the licensed pharmacists at incumbent pharmacies in 2005 to 2008, when all of them worked for Apoteket, which the survey includes in full. Coverage falls to about 30 percent in 2009, the year of the restructuring and sale, and is 54 to 69 percent in 2010 to 2013, when the survey includes the large chains in full but samples smaller owners (Section 3).

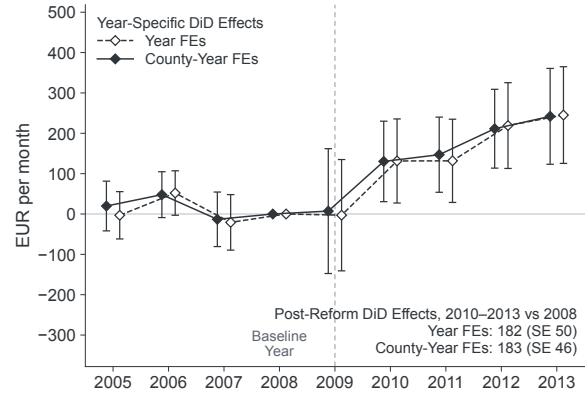
per month in 2012. Relative to control pharmacies, paid unsocial hours per pharmacist rose by 1.8 per month (SE 0.4; county-by-year: 1.7, SE 0.4) and unsocial-hours pay per pharmacist by EUR 30 (SE 6; county-by-year: 28, SE 6) (Figure A7, Panels (c) and (d)). Per pharmacy, paid unsocial hours rose by 10.5 per month (SE 3.0; county-by-year: 11.1, SE 2.9) (Figure A7, Panels (a) and (b)). Together, the two sources indicate that treated pharmacies began to open in the evenings and at weekends after the break-up.

We next ask whether pharmacists worked more hours to keep pharmacies open in the evenings and at weekends, or whether fewer pharmacists were present per opening hour. Contracted working time did not change relative to control pharmacies (0.57 percentage points of full time, SE 0.65; county-by-year: 0.41, SE 0.65), and incumbent pharmacies did not employ more pharmacists (Figure 3c). This implies the latter: treated pharmacies covered longer opening hours with unchanged pharmacist hours.

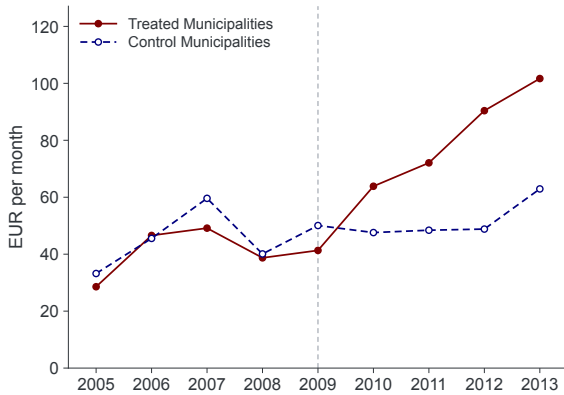
Figure A7: Unsocial-Hours Pay at Incumbent Pharmacies



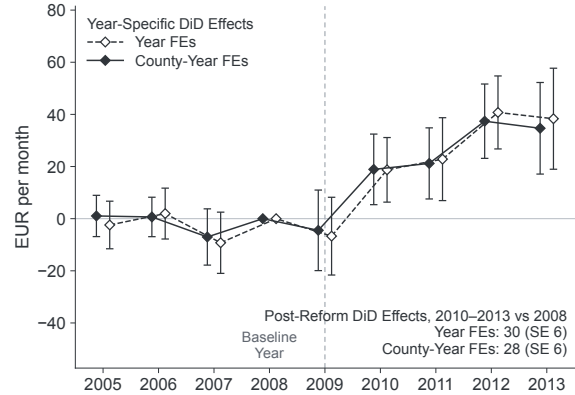
(a) Per pharmacy: levels



(b) Per pharmacy: DiD



(c) Per pharmacist: levels



(d) Per pharmacist: DiD

Notes: This figure shows the unsocial-hours pay of licensed pharmacists at incumbent retail pharmacies (present by 2008), in euros per month at 2020 prices. Panels (a) and (b) sum the pay per pharmacy, scaled to all its licensed pharmacists, for the 608 treated and 169 control pharmacies with licensed pharmacists in every year from 2005 to 2013; panels (c) and (d) report the pay per pharmacist at all 730 treated and 182 control incumbent pharmacies. Hollow markers use municipality and year fixed effects, filled markers municipality and county-by-year fixed effects; the panels report the mean of the 2010 to 2013 coefficients relative to 2008. The dashed vertical line marks the sale, announced on 9 November 2009. Bars are 95 percent confidence intervals based on standard errors clustered by municipality.

Table A12: Opening Hours and Unsocial Hours

	Control		Treated		DiD: Year FEs		DiD: County-Year FEs	
	2008	2012	2008	2012	Treatment (2010–2013)	Pre-Period (2005–2007)	Treatment (2010–2013)	Pre-Period (2005–2007)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Per pharmacy</i>								
Weekly opening hours	42.9	44.9	45.9	49.8	2.1 (0.6)	−0.6 (0.3)	1.9 (0.5)	−0.4 (0.3)
Paid unsocial hours per month	14.7	14.8	20.3	32.3	10.5 (3.0)	0.9 (1.7)	11.1 (2.9)	1.5 (1.7)
pharmacies with 80% coverage	11.7	13.7	18.1	30.8	7.0 (2.2)	−1.8 (1.5)	6.2 (1.9)	−0.8 (1.4)
Unsocial-hours pay (EUR)	232	259	319	557	182 (50)	9 (26)	183 (46)	18 (26)
Licensed pharmacists	4.6	4.0	6.3	5.0				
<i>Panel B: Per pharmacist</i>								
Paid unsocial hours per month	2.5	2.8	2.5	5.2	1.78 (0.38)	−0.13 (0.28)	1.73 (0.37)	−0.03 (0.24)
Unsocial-hours pay (EUR)	40	49	39	90	30 (6)	−3 (4)	28 (6)	−2 (4)
Contracted working time					0.57 (0.65)	0.14 (0.46)	0.41 (0.65)	0.29 (0.48)
Weekly opening hours per pharmacist	13.6	15.2	11.9	14.4	0.8 (0.7)	0.6 (0.5)	0.2 (0.7)	0.7 (0.5)

Notes: This table compares incumbent retail pharmacies (present by 2008) in treated and control municipalities. Columns (1) to (4) report averages. Treatment is the mean of the 2010 to 2013 event-study coefficients relative to 2008, and Pre-Period the mean of the 2005 to 2007 coefficients. Opening hours are observed for November 2007 and 2008 and for week 46 of 2012, for 619 treated and 173 control pharmacies; for them, Treatment and Pre-Period are the 2012 and 2007 coefficients. Paid unsocial hours are unsocial-hours pay divided by the full-time basic salary over 172 hours. Panel A covers the 608 treated and 169 control pharmacies with licensed pharmacists in every year from 2005 to 2013, with the wage-survey sums scaled to all licensed pharmacists, or unscaled where the survey covers at least 80 percent of them; on all incumbent pharmacies, column (5) is 12.5 (2.8) hours and EUR 211 (46). Panel B covers all 730 treated and 182 control incumbent pharmacies; contracted working time is in percent of full time. Amounts are in euros per month at 2020 prices. Standard errors clustered by municipality are in parentheses.

Table A13: Opening Schedules in 2012

	Control (1)	Treated (2)	Difference (3)
Unsocial opening hours per week	3.4	7.0	2.7 (0.5)
weekday evenings (50%)	0.3	1.7	1.1 (0.2)
Friday evenings (75%)	0.07	0.41	0.27 (0.04)
Saturday daytime (50%)	2.6	2.6	−0.3 (0.2)
Saturday afternoons (100%)	0.1	0.8	0.6 (0.1)
Sundays (100%)	0.3	1.5	1.1 (0.2)
Open on Saturday afternoons (%)	9.1	35.3	21.8 (3.1)
Open on Sundays (%)	6.1	26.8	18.2 (2.8)
Pharmacists on duty in unsocial opening hours	1.94	1.84	0.14 (0.25)

Notes: This table compares the opening schedules of 570 treated and 165 control incumbent retail pharmacies (present by 2008) in week 46 of 2012. Column (3) reports the difference with county fixed effects. The unsocial time bands follow the pay agreement, with the supplement in parentheses: weekday evenings are after 18:00 from Monday to Thursday and before 07:00 from Tuesday to Friday, Friday evenings run from 18:00 on Friday to 07:00 on Saturday, Saturday afternoons from 14:00 on Saturday to 07:00 on Sunday, and Sundays from 07:00 on Sunday to 07:00 on Monday. Pharmacists on duty divide paid unsocial hours by unsocial opening hours, each weighted by its supplement, for 386 pharmacies. Standard errors clustered by municipality are in parentheses.