Labor Market Consequences of Domestic Outsourcing: Evidence from Legalization in Brazil

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Abstract

We analyze the effect of domestic outsourcing on labor markets using Brazil's unexpected legalization of outsourcing in 1993, which sharply increased outsourcing among security guards. We show that outsourcing legalization led to a wave of occupational layoffs. Incumbent security guards affected by the layoffs moved to lower-wage firms and experienced persistent wage reductions. However, outsourcing legalization also had positive market-level effects, as revealed by a triple-difference specification that uses regional variation in the pre-legalization permissiveness of labor courts and compares security guards with less impacted occupations. Jobs were reallocated from older to younger workers. Total employment of security guards rose by 8-15% and their average wages increased by 2-4%. Interpreted through an economic framework, the results imply that outsourcing legalization benefitted entrant workers and generated lasting efficiency gains. If laid-off incumbent workers were fully compensated for their earnings losses, social breakeven would be achieved in one to five years.

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1 Introduction

There is growing concern among labor economists about the "fissured" workplace, as businesses increasingly rely on contract-firm workers to provide labor services once performed by direct employees, such as cleaning, security, logistics, HR, and IT. A number of studies warn that the rise of domestic outsourcing hurts low-wage workers by reducing wages and benefits (Dube and Kaplan 2010; Weil 2014). Many economists suggest a potential link between outsourcing and rising inequality (Appelbaum 2017; Goldschmidt and Schmieder 2017; Song et al. 2019; Autor et al. 2020; Stansbury and Summers 2020).

Despite its increasing prominence in the economy, empirical evidence about the consequences of domestic outsourcing is scarce (Bernhardt et al. 2016).[1] Recent work uses firm-level variation in contractual arrangements to document that outsourced workers earn lower wages due to exclusion from firm-specific wage premia (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2020). While informative, these estimates do not account for potential market-level equilibrium effects of outsourcing. In particular, outsourcing may generate efficiency gains by increasing workforce flexibility and specialization (Abraham and Taylor 1996; Plimmer 2013). Efficiency gains increase labor demand and raise equilibrium wages and employment. Assessing the full economic consequences of domestic outsourcing therefore requires knowledge about its market-level effects.

This paper leverages an unexpected court ruling in Brazil to estimate the effects of domestic outsourcing on labor markets. In 1993, Brazil's Superior Labor Court legalized outsourcing of all non-core activities by private-sector firms, generating an exogenous reduction in the legal cost of outsourcing.[2] We focus on Brazil's large market for security guards for two reasons. First, security guards are a licensed and mostly formal-sector occupation. Second, the Brazilian government

[1] Domestic outsourcing is related to but conceptually different from the extensively studied phenomenon of offshoring. Offshoring changes the location of productive tasks across countries (Grossman and Rossi-Hansberg 2008), whereas domestic outsourcing changes the firm boundary without a change in the geographical location of tasks (Coase 1937). The two may have some similar economic effects. For example, this paper suggests that, like offshoring, the rise of domestic outsourcing generates aggregate benefits but creates concentrated losses for some workers.

[2] As explained in Section 2, outsourcing was not uncommon even before its legalization because until then judges differed widely in their legal interpretation of relevant labor code. If brought to court and if the judge was not favorable to outsourcing, firms that outsourced security guards could be found liable for steep sums (e.g., unpaid salary differentiables, benefits, lawyer fees, penalties for infringing labor laws, etc.). Legalization sharply reduced litigation costs by clarifying that outsourcing of all non-core activities of the firm was legal.

heavily regulates and closely oversees security service firms. For these reasons, legal restrictions against outsourcing before legalization were particularly binding for security guards, who upon legalization experienced the largest rise in outsourcing among all major occupations.

By combining Brazil's administrative employer-employee data, quasi-experimental identification, and economic theory, this paper answers three fundamental questions: (i) how do firm outsourcing decisions affect incumbent workers? (ii) how does an exogenous increase in outsourcing affect market-level wages, employment, and employment composition? (iii) do the social benefits of outsourcing exceed its social costs?

In the first part of this paper, we investigate how firm outsourcing decisions affect incumbent workers. We show that outsourcing legalization led to a large wave of occupational layoffs, defined as sudden drops in security guard employment at the establishment level that did not coincide with a large drop in total employment. By contrast, on-site outsourcing events, defined as large flows of workers from a direct employer to a contract firm, were much rarer.

The effects of occupational layoff outsourcing on incumbent workers are both negative and long-lasting. To show this, we compare the employment and wage trajectories of workers impacted by occupational layoffs with observably similar workers at non-outsourcing establishments. After an occupational layoff, the likelihood that an impacted worker remained formally employed sharply fell. Though their employment fully recovered within five years, their wages did not. Conditional on formal employment, incumbent wages persistently fell by roughly 10 percent. In present-value terms, incumbent workers lost 1.2 years of pre-outsourcing earnings.

The decline in the wages of incumbent workers is explained to a large extent by a loss of firm wage premia. To measure firm wage premia, we implement a full decomposition of wages in Brazil into firm and worker fixed effects following Abowd et al. (1999). We find that incumbents move to firms with lower wage premia after experiencing occupational layoffs. Furthermore, incumbents initially employed by higher-wage firms experience larger wage reductions. The loss of firm-specific wage premia explains about 46 percent of the total wage losses five years after an occupational layoff.

In the second part of the paper, we estimate the market-level effects of outsourcing. For identification, we compare microregions in Brazil's South, where labor court judges tended to forbid outsourcing prior to legalization, with microregions in the rest of Brazil, where judges tended to

be more permissive. We reweight permissive microregions to be similar to restrictive microregions in mean pre-legalization characteristics, such as crime rates, unemployment rates, and local exposure to concurrent tariff reductions. We then use a triple-difference regression specification that compares both restrictive microregions with permissive microregions and security guards with occupation groups less affected by legalization. This allows us to flexibly control for confounding regional and occupational trends.

Though outsourcing hurt incumbent security guards, the market-level estimates reveal that outsourcing legalization also created winners in the occupation. Specifically, outsourcing legalization increased entry of younger workers into the occupation. Meanwhile, employment of older security guards fell. The average age of security guards persistently declined by about 2 years. In other words, outsourcing *reallocated* jobs from older workers to younger workers.

Even more strikingly, outsourcing legalization *increased* both market-level wages and employment of security guards. The composition-adjusted wages of security guards rose by 2-4 percent. Their total employment increased by 8-15 percent. These effects are unlikely to be due to pre-existing differences in the evolution of local occupational labor markets, since there are no differential pre-legalization trends. The estimates are also similar whether inverse propensity score weights, entropy-balancing weights, or regression adjustment is used to account for confounding labor market trends.

The third and final part of the paper uses a simple parametric framework to interpret our market-level findings and to compute the aggregate welfare effects of outsourcing legalization. We use a right-to-manage model that allows outsourcing legalization to affect both worker wage bargaining power and transactions costs. A reduction in the wage markup lowers the wage, while a reduction in transactions cost raises firm labor demand and thus increases the wage. With assumed elasticities of labor supply and demand, our most conservative reduced-form estimates imply that outsourcing legalization lowered the representative firm's transactions cost by the equivalent of 5 percent of the initial worker's wage, and resulted in at most a small reduction in the representative firm's wage markup.

The estimated welfare gains from outsourcing legalization are large relative to the total earnings losses for laid-off incumbent workers. Firm surplus increased by roughly 3 percent of the initial security guard wagebill, while worker surplus increased by an analogous 4 percent. If in-

cumbent workers affected by occupational layoffs were fully compensated for their present-value earnings losses and the welfare gains of outsourcing were immediately realized, then the economic benefit of outsourcing legalization would exceed its cost in one to five years, depending on whether transactions costs are assumed to be dissipated as pure waste or redistributed in the economy.

To our knowledge, this paper is the first to leverage an exogenous rise in outsourcing to quantify the market-level effects of domestic outsourcing. Much prior work uses firm-level variation in contractual arrangements to estimate outsourcing wage differentials (e.g., Abraham 1990; Berlinski 2008; Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2020).[3] While informative, these estimates do not account for potential market-level equilibrium effects. We make three main contributions. First, we use legalization to establish that firm outsourcing decisions primarily took the form of occupational layoffs and estimate their effects on incumbent workers. Second, we contribute the first estimates of the effects of outsourcing on market-level wages, employment, and employment composition. Third, we compare the aggregate economic costs and benefits of outsourcing by combining our reduced-form results with a parametric economic framework. Overall, our results reveal that domestic outsourcing not only redistributed firm wage premia, as emphasized by recent studies, but also had large reallocative and efficiency-enhancing effects.

This paper also differs from recent literature by focusing on a licensed, medium-wage occupation. Many have studied low-wage occupations in high-income countries, where the loss of firm-specific wage premia due to outsourcing is large (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017). By contrast, security guards in Brazil are a relatively high-paying occupation. Furthermore, the average firm-specific wage premia of contract-firm guards in Brazil is very similar to that of direct-hire guards, and the outsourcing wage differential for guards in Brazil is small (Li and Wong 2021). This explains why we find that outsourcing legalization had a limited market-

[3] A number of papers document the rise of outsourcing, including Dey et al. (2010); Berlingieri (2014); Bloom et al. (2018); Katz and Krueger (2016, 2019). One strand of this literature documents the determinants of firms' decisions to contract out (Lee 1996; Abraham and Taylor 1996; Houseman 2001; Chaurey 2015). Another strand examines its effects on firm output (Bertrand et al. 2017; Bilal and Lhuillier 2021). Kalleberg (2000) surveys relevant research in sociology. Autor (2009) discuss a broader literature on labor market intermediation. Weil (2014) offers a detailed and largely qualitative analysis of the business practices of domestic outsourcing in the United States. Bernhardt et al. (2016) discusses data challenges for measuring outsourced work. Relatedly, Le Moigne (2021) study how increasing fragmentation of internal job ladders affected firm and workers in France. Battiston et al. (2021) study job rotation and talent poaching in a Columbian security service firm.

level effect on average worker wage bargaining power, which contrasts with recent findings from other settings. However, our finding that outsourcing generated sizeable efficiency gains likely generalizes to other occupations and settings, since the mechanisms underlying these efficiency gains are not specific to security guards in Brazil.[4]

Our study contributes to a broader literature on the effects of labor market regulation and reform, as well as an active legal debate regarding the proper regulation of outsourced work.[5] The literature has shown that labor market regulation is associated with lower output, employment, and productivity (Besley and Burgess 2004; Botero et al. 2004; Aghion et al. 2008), but that partial labor market reforms may have the perverse effect of increasing turnover without boosting employment (Bentolila and Saint-Paul 1992; Cahuc and Postel-Vinay 2002; Blanchard and Landier 2002; Daruich et al. 2020).[6] To this literature, we contribute evidence that partial labor market reform in the form of lifting a ban on outsourcing had both positive effects on employment and substantially negative consequences for incumbent workers. By identifying the winners and losers of a more "fundamental" labor market reform, our study complements theoretical work on the political economy of labor market reforms (Saint-Paul 2002).

Finally, we contribute to a growing literature on the role of firm-specific wage premia in determining the cost of job loss and domestic outsourcing (Goldschmidt and Schmieder 2017; Lachowska et al. 2020b; Schmieder et al. 2020). A key finding in our study is that the loss of firm-specific wage premia explains 46 percent of the total wage losses five years after an outsourcing event. This finding underscores the potential importance of firm-specific components in wage setting (Card et al. 2013; Barth et al. 2016; Song et al. 2019), as well as their role in explaining wage losses due to outsourcing (Goldschmidt and Schmieder 2017; Drenik et al. 2020).

The paper proceeds as follows. Section 2 provides institutional background. Section 3 estimates the effects of firm outsourcing decision on incumbent workers. Section 4 explores the relationship between outsourcing decisions and firm-specific wage premia. Section 5 introduces our market-level empirical approach. Section 6 reports our market-level results. Section 7 presents

[4]These mechanisms include improved worker training, reduced management costs, increased flexibility, and reduced litigation costs that arise from economies of scale at the contract firm.

[5]The regulation of outsourcing remains a controversial policy issue in Brazil, where the outsourcing of primary activities, in addition to non-core activities, became legal in 2017. A related and active legal debate in the US concerns the "joint employment doctrine," which governs the scope of legal responsibilities by clients to outsourced workers under the Fair Labor Standards Act (see, for example, Padin 2020).

[6]See Freeman (2009) for a review of the large literature on labor market regulation in developing countries.

our parametric framework and welfare analysis. Section 8 concludes.

2 Background

2.1 Legal History of Outsourcing in Brazil

Outsourcing emerged as a new business practice of uncertain legality in Brazil during the second half of 20th century. In 1967, the Brazilian dictatorship issued Law-Decree 200, stating that government bodies were allowed to outsource all non-governmental functions. However, lack of legislative action resulted in ambiguity in the legality of outsourcing by private-sector firms. As a consequence, local labor courts wielded substantial discretion to rule on the question: Who is the employer — the contract firm or the client firm? Since the answer determined collective bargaining agreement coverage as well as legal responsibilities for worker protection, the question had important implications for worker wages, benefits, and job security. The legal status of outsourcing may also affect frictions in the labor market, since the litigation costs of employment for firms are high in Brazil.[7]

In 1985, Brazil's Superior Labor Court embraced a very narrow vision of outsourcing by issuing Súmula 256.[8] Súmula 256 clarified that outsourcing was illegal except under two specific instances mentioned in prior legislation: (a) banks could hire specialized firms to provide security services under Law 7.102 of 1983; (b) in case of demonstrated need, firms were allowed to hire temporary work for a maximum of 3 months through government-authorized temporary work agencies under the Seasonal Employment Law (Law 6.019 of 1974). Firms that illegally outsourced faced the risk of costly labor litigation initiated by workers and worker unions, as well as legal penalties for unpaid wages, benefits, and taxes if found liable at court.[9]

However, Súmula 256 failed to put an end to the practice of outsourcing in the private sector.

[7] The Brazilian labor judicial system faced more than a million new cases per year between 1988 and 1994 (Cooney et al. 2015).

[8] See da Cruz (2009), available in Portuguese, for a comprehensive review of Brazilian labor laws concerning temporary work and outsourcing. Biavaschi and Droppa (2011) offer a detailed history of the events leading up to Súmula 331, which legalized outsourcing of non-core activities.

[9] If a firm were found to have illegally outsourced, it would have to compensate the worker for the delayed payment of any additional wages and benefits that the worker would have earned under the direct employment contract. In addition, the firm would have to pay all taxes relating to that working relation, as well as penalties for delayed tax payment.

Following a wave of market-oriented policies starting in 1990, the demand for labor flexibility rose and outsourcing became a major phenomenon in Brazilian labor relations (Cooney et al. 2015). In response, local labor judges offered inconsistent decisions concerning the legality of outsourcing. Court decisions and interview transcripts with prominent labor judges and lawyers show a wide range of opinions amongst regional judges about the legality of outsourcing of activities not explicitly permitted by legislation, as well as a wide range of legal reasoning on the issue, during the years between 1985 and 1993.[10] These labor law issues were particularly debated in the Southern regional courts, which tended to rule against the legality of outsourcing, a pattern we later document in Section 5.1 and exploit in our market-level empirical strategy.

An unanticipated series of events led the Superior Labor Court to revise its stance on the legality of outsourcing in 1993. In April 1993, the Labor Prosecution Office initiated an investigation of a reported illegal use of outsourced typists at Banco do Brasil, the largest bank in the country, following union complaints. This triggered a crisis at the government-owned bank, since Súmula 256 expressly disallowed banks from outsourcing activities other than security services. As remedy, the Labor Prosecution Office demanded that the bank conduct an open civil-service examination to hire replacements. However, the proposed remedy would effectively force unemployment upon 13,000 auxiliary staff and was strongly resisted by the bank, as well as two labor unions and a prominent trade association. In the face of mounting political pressure, the Labor Prosecution Office relented and instead petitioned the Superior Labor Court to revise Súmula 256.

On December 17, 1993, the Superior Labor Court responded by issuing Súmula 331, a surprising and sweeping precedent which declared outsourcing of non-core activities by any firm to be legal. Súmula 331 settled the disagreements about the legality of outsourcing among Brazil's regional courts. Henceforth, outsourced workers would be considered legal employees of the intermediary firm so long as the service provided by the worker was deemed a *non-core activity* of the client firm. The intermediary would therefore be responsible for compliance with Brazilian labor regulations regarding the outsourced worker's pay, benefits, and employment protection. Only in the event that the intermediary became bankrupt and defaulted on its obligations would the client firm be responsible to the worker.[11] As a consequence of Súmula 331, many forms of outsourcing

[10] Interview transcripts were generously provided by Magda Barros Biavaschi and Alisson Droppa.

[11] This requirement created incentives for client firms to check whether contract firms were able to pay their workers before entering into a contractual arrangement and helped to reduce wage theft.

suddenly became legal in jurisdictions that previously deemed them illegal, so the legal cost of outsourcing sharply fell.

2.2 Data

We use Brazil's employee-employer matched administrative data, *Relação Anual de Informações Sociais* (RAIS), covering 1985-2002, which track the near universe of Brazil's formal-sector workers. RAIS is widely seen as a high-quality database, since firms face fines for failure to report for all employees who are employed during a year and workers cannot receive payments from several government benefits programs unless accurate information is reported. The database contains annual information on the duration of employment, the average monthly wage over that period, a number of demographic variables (such as education, gender, and age), as well as detailed industry and occupation codes, for each matched worker-establishment pair. These data allow us to construct employment histories for individual workers and follow establishments over time.

Despite their richness, these data have three limitations. First, RAIS lacks information on workers who are not formally employed. Such workers account for roughly half of Brazil's workforce during the analysis period. Consequently, the data does not indicate whether a worker is out of the labor force, unemployed, informally employed, or self-employed if they are missing from RAIS.[12] Second, RAIS does not provide information on firms' use of contract-firm workers. The data identifies workers who are employed by a contract firm, but we do not observe which client a contract-firm worker serves. Therefore, to measure outsourcing decisions and prevalence, we construct indirect measures from employment histories. Third, RAIS does not capture non-wage benefits, such as food and transportation allowances, which are important forms of compensation in Brazil (Lagos 2019).

We use 2-digit occupation codes and identify contract firms using 5-digit industry codes.[13] A worker is said to be a contract-firm worker if they are employed by a contract firm, and a direct-hire worker if they are employed by any other private-sector firm. To consistently classify the industry

[12] These data are assembled yearly by the Brazilian Ministry of Labor (now Secretary of Labor, Ministry of Economy) and all registered establishments are required by law to submit detailed information on all employment contract changes in the prior calendar year. Consequently, they do not include domestic workers, which can be formally employed but are not in RAIS, as well as any workers without signed work cards.

[13] Specifically, we use the occupation codes that are consistent for the period 1985-2002, prior to a major revision in occupation codes in 2003.

of establishments over time, we use crosswalks along with our best judgement. Throughout the paper, we focus on private-sector workers.

2.3 Security Guards: Relevant Facts

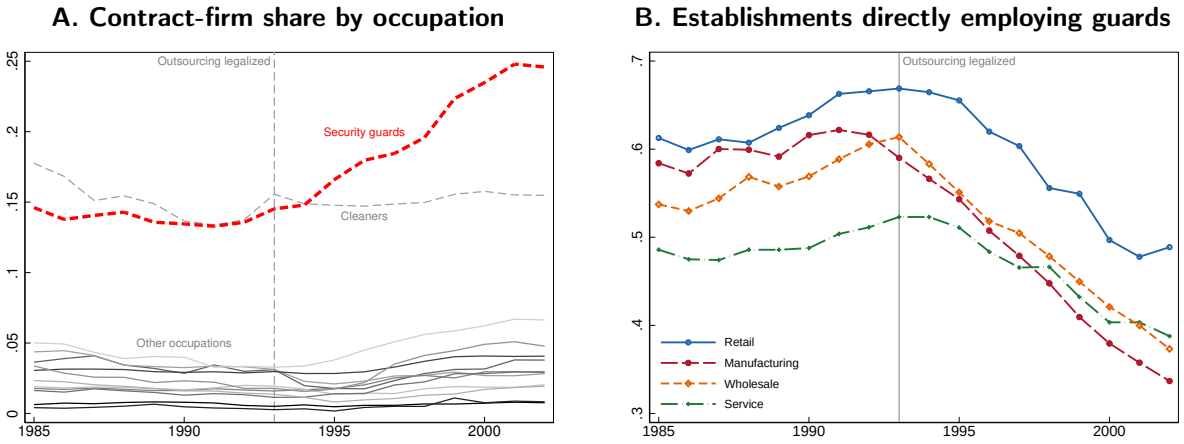
We focus on Brazil's large market of security guards for two reasons. First, security guards are a licensed and mostly formal-sector occupation in Brazil. The Brazilian government administers mandatory training and imposes regulatory requirements for gun carry licenses.[14] During the study period, roughly 80 percent of security services workers were formally employed, according to data from the Pesquisa Nacional por Amostra de Domicílios (PNAD) household survey, and hence covered in the RAIS data. By contrast, only roughly 50 percent of all workers were formally employed. Because of their high rate of formality, the ban on outsourcing was likely to be particularly binding for security guards.

Second, security service firms are heavily regulated by the Brazilian government and were the only contract firms to see new legislation reflecting an expanded view of outsourcing in the immediate wake of Súmula 331. The 1983 law that governs security service firms at first only explicitly allowed banks to outsource security services (Law 7.102/1983). Many judges, therefore, ruled according to Súmula 256 that outsourcing of security guards by non-banks was illegal. Four months after Súmula 331, the National Congress revised this law to explicitly allow security service outsourcing by non-banks. By contrast, other contract firms were neither regulated by special legislation nor experienced any change in legislation due to Súmula 331. As such, outsourcing legalization was likely to have a particularly powerful effect on security guards.

Figure 1, Panel A shows that security guards were the only major occupation to experience a large rise in outsourcing after legalization. Each line in the figure shows the trend in the share of private-sector workers employed in contract firms (i.e., the "contract-firm share") for an occupation, averaged across microregions. Before 1993, there are two occupations – security guards and cleaners – whose contract-firm share far exceeds other occupations. In the years immediately

[14] Security guards must have successfully completed mandatory security services training administered by Brazil's *Polícia Federal* (equivalent to the Federal Bureau of Investigations in the United States); and have no criminal records. Other requirements include being Brazilian and at least 21 years of age, have studied at least up to 4th grade, and present proof of no pending obligations with either the electoral court (as voting is mandatory in Brazil) or with the military (as men are required to report to the military at age 18 for enlistment, but most are dismissed).

Figure 1: Trends in outsourcing across occupations and sectors



Note: Each line in Panel A shows the share of private-sector workers employed in contract firms, averaged across microregions, for an occupation. We include only major occupations and microregions that are in our estimation sample, which is described in Section 5 and tabulated in Table 5. Panel B plots the share of establishments with at 50 employees in the respective sectors that employ at least one security guard.

following legalization, the contract-firm share indeed rose dramatically for security guards, by almost 10 p.p., from an average of 15 p.p. By contrast, the contract-firm share for cleaners hardly changed.[15] The contract-firm shares for the remaining occupations are also low throughout the sample period. Though outsourcing is a broader phenomenon that has affected many occupations (see, e.g., Goldschmidt and Schmieder 2017), the data confirm that Brazil's outsourcing legalization had a much larger effect on security guards than other occupations.

Security guards are a large and economically important occupation in Brazil. In 1993, the number of private-sector security guards in our data is 481,446, which accounts for 3.1 percent of total private-sector formal employment. The vast majority of both contract-firm and direct-hire security guards in our data have indefinite-duration full-time contracts. Appendix table A.1 provides summary statistics.

Many sectors of the economy were affected by the rise of security service outsourcing. Figure

[15] Section 3 documents a large and long-lasting increase in the frequency of occupational layoffs for security guards after legalization, but the analogous increase for cleaners was short-lived. This suggests that legalization had a detectable, but much smaller, impact on cleaners than on security guards, and as such did not significantly change the contract-firm share of cleaners. The smaller effect on cleaners could be because, unlike security guards, who are primarily in the formal sector and subject to occupational licensing, a substantial portion of cleaners are informally employed, making outsourcing laws in the formal sector less binding for this sector.

1, Panel B plots the trend in the share of establishments with at 50 employees that employ at least one security guard, separately for the manufacturing, services, wholesale, and retail sectors. Across all four sectors, the share was generally steady before 1993, but began to fall sharply beginning around 1993.[16] In 1992, about 60 percent of wholesale establishment had at least one security guard on staff. By 2002, less than 40 percent did. Since there is a concurrent increase in contract-firm employment (see Figure A.2), many of these firms must have contracted out their needed security services, and thus no longer directly employed security guards.

3 Effects of Outsourcing on Incumbent Workers

In this section, we first show that outsourcing legalization led to a wave of occupational layoffs. We then estimate the effects of occupational layoffs on the employment and wage trajectories of incumbent security guards, and compute the present discounted value of the resulting earnings losses.

3.1 Measuring Outsourcing Decisions

The literature provides two methods to identify outsourcing decisions from administrative employee-employer data. Goldschmidt and Schmieder (2017) focus on *on-site outsourcing events*, defined as the occurrence of a large flow of workers from a direct employer to a contract firm, and use these events to estimate contract-firm wage differentials. Another method, also considered by Goldschmidt and Schmieder (2017), is to identify *occupational layoff*s, wherein an establishment drastically reduces their number of direct employees in a given occupation.

We first explore on-site outsourcing in our data. We define an on-site outsourcing event as a situation where a group of 3 security guards were all employed in a non-business service establishment with at least 10 employees in one year and then in the following year were all employed in a business service establishment. To avoid misclassifying mass layoffs, we exclude establishments who non-guard employment fell by more than 10 percent. These events are called *on-site* outsourcing events, because the workers were presumably transferred to a contract firm but continued

[16] A minor exception is manufacturing, whose contract-firm share begins to decline just before 1993. This is likely related to concurrent trade liberalization, which disproportionately affected manufacturing establishments.

to perform the same job.[17]

On-site outsourcing events are rare in Brazil. Between 1990 and 2000, we identify a total of 107 on-site outsourcing events in the security guard occupation. These events affected 2,842 security guards, about 0.7 percent of the nearly half a million security guards in the nation. By comparison, the number of direct-hire guards declined by 49,186 during the same period, and the number of contract-firms guards increased by 112,225. On-site outsourcing therefore accounts for less than a tenth of the changes in direct-hire and contract-firm employment before and after outsourcing legalization. A potential reason for the rarity of on-site outsourcing events is that Brazil prohibits nominal wage reductions for continuing workers, which is generally understood to include the firing and rehiring workers through an intermediary to perform the same job but at a lower wage.[18]

Given the rarity of on-site outsourcing in Brazil, it is likely that when an establishment decided to outsource, the vast majority of workers were either fired or reassigned to other jobs within the establishment. As such, we instead focus on occupational layoffs, wherein an establishment suddenly reduces their number of direct employees in the occupation. Specifically, we define an occupational layoff as the occurrence of an establishment with at least 10 employees and 3 security guards reducing its security guard employment by at least two thirds in the following year. To avoid misclassifying normal fluctuations in headcount, we require that the number of security guards must fall to zero for establishments initially with 5 or fewer guards. To avoid misclassifying mass layoffs, we exclude establishments who non-guard employment fell by more than 10 percent.[19]

Occupational layoffs are much more common than on-site outsourcing events. Between 1990

[17] Goldschmidt and Schmieder (2017) use a more stringent definition, where an on-site outsourcing event is when a group of 10 security guards were all employed in the predecessor in one year and then in the following year were all employed in a business service establishment. In their definition, the predecessor establishment must have at least 50 employees in the year prior to the event, continue to exist in the following year, and not shrink by more than 50%. The flow must also represent less than 30% of employment in the predecessor in the previous year, such that outsourced workers represent only a small part of the predecessor's business. Using their definition, we identify only 27 on-site outsourcing events in the security service industry between 1990 and 2000. These events affected 1061 security guards, less than 0.25 percent of the security guards in the nation.

[18] See Articles 453 and 468 of *Consolidação das Leis do Trabalho*. See also *Portaria* MTB 384/1992 and Law 6.019 Article 5^0-D.

[19] Specifically, we say that establishment j had an "occupational layoff" in year t if: (1) establishment j had at least 10 total employees; (2) establishment j employed at least three guards in years $t-1$ and $t-2$; (3) the number of guards at establishment j in year t fell by at least two-thirds compared to year $t-1$; (4) the number of guards fell to zero in year t if j employed fewer than six guards in year $t-1$; (5) establishment j's non-guard employment shrinks by less than 10 percent between $t-1$ and t; and (6) establishment j was not a contract firm nor government entity in year $t-1$.

and 2000, the number of occupational layoffs averaged 471 per year and affected a total of 35,544 security guards, about 8.4 percent of the security guards in the nation. [20] Occupational layoffs thus can account for a vast majority of the decline in direct-hire employment during this period.

3.2 The Effect of Legalization on the Frequency of Outsourcing Decisions

Outsourcing legalization led to a large wave of occupational layoffs for security guards.

To show this, we use a linear probability model to estimate the likelihood of an occupational layoff in each year. Our sample includes all establishment-years where the establishment had at least 10 employees and 3 security guards and its non-guard employment fell by less than 10 percent in the subsequent year. We exclude manufacturing establishments, because they are heavily affected by trade liberalization in the early 1990s. We then regress a dummy indicating the occurrence of an occupational layoff on year fixed effects, relative to the omitted year of 1992. We include microregion fixed effects to allow for potential baseline differences in the probability of outsourcing decisions across microregions. We cluster standard errors at the establishment level.

Figure 2, Panel A plots the estimated the likelihood of occupational layoffs, which is stable prior to 1993, the year of outsourcing legalization, but rises sharply in 1994, the year after legalization. The increased likelihood of occupational layoffs persists for several years before slowly falling back towards baseline.

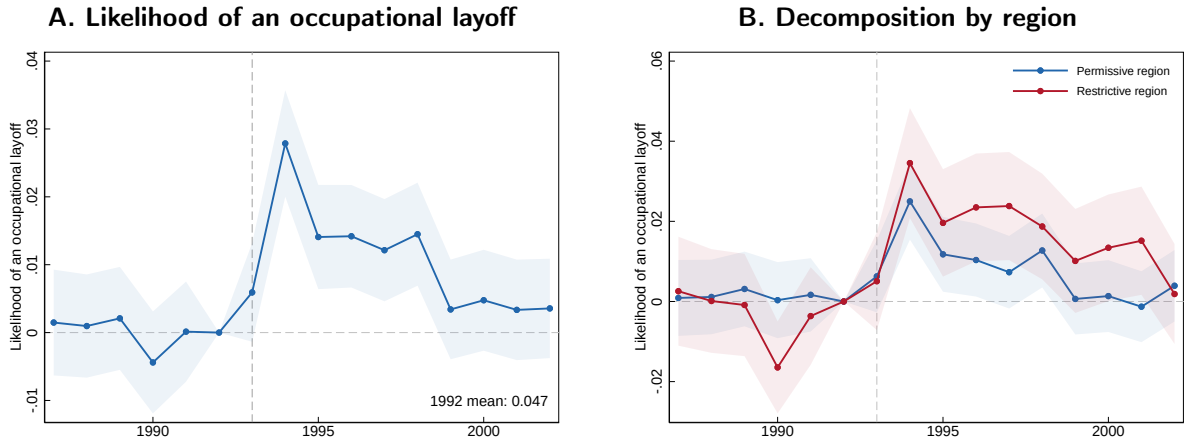
Figure 2, Panel B shows that there was a larger increase in the incidence of occupational layoffs in Brazil's South, where labor courts tended to be more restrictive towards domestic outsourcing, than in the rest of Brazil, where courts tended to be more permissive.[21] The estimated likelihood of occupational layoffs rises in both restrictive and permissive regions relative to 1992. However, the increase in outsourcing decisions is both greater and more prolonged in restrictive regions than in permissive regions. In restrictive regions, we estimate that the likelihood of outsourcing decisions rose by about 4 p.p., almost doubling the overall likelihood in restrictive regions in 1993. In permissive regions, the likelihood rose by about 2.5 p.p., which is almost 50 percent higher than the overall likelihood in permissive regions in 1993.

There was no corresponding increase in occupational layoffs for two major occupations with

[20] See Figure B.2 for the counts of occupational layoff over time.

[21] See Section 5.1 for the definition of permissive and restrictive regions.

Figure 2: Trend in occupational layoffs



Notes: Panel A plots coefficients from a linear probability model where we regress a dummy indicating the occurrence of an occupational layoff on year fixed effects, relative to the omitted year of 1992, with controls for microregion fixed effects. Our sample includes all establishment-years where the establishment had at least 10 employees and 3 security guards and its non-guard employment fell by less than 10 percent in the subsequent year. We exclude manufacturing establishments, because they are heavily affected by trade liberalization in the early 1990s. Panel B plots coefficients from a linear probability model with separate year fixed effects for restrictive and permissive regions. We cluster standard errors at the establishment level.

low contract-firm shares: salesmen and construction workers. As shown in Appendix Figure B.3, the likelihood of occupational layoffs for both of these occupations fluctuated from year to year, with little discernible difference immediately before or after legalization. This finding confirms that the sharp rise in occupational layoffs was caused by outsourcing legalization rather than other events in the economy.[22]

3.3 Effects of Outsourcing Decisions on Incumbent Workers

Having established that outsourcing legalization increased occupational layoffs, we now estimate how occupational layoff outsourcing affected incumbent workers.

Method. We measure the effects of outsourcing on incumbent workers by comparing long-tenured workers who were directly affected by an occupational layoff to similar workers who

[22]There was also a sharp increase in occupational layoffs for cleaners in 1994, the year immediately after legalization. Appendix Figure B.3 shows that the increase for cleaners was short-lived, since by 1996, the likelihood of occupational layoffs had already returned to the baseline level. This is consistent with our earlier finding that outsourcing legalization had a much larger impact on the incidence of outsourcing among security guards than in any other major occupation.

were unaffected by such events. We define the treatment group to be the sample of security guards whose employer had an occupational layoff between 1990 and 2000 and were not employed as a security guard at the same establishment following the occupational layoff. This definition captures all workers who did not continue their jobs as direct-hire security guards when their employer eliminated a large portion of such jobs. Therefore, treated workers could either separate from the establishment or be reassigned another occupation within it.[23]

We construct a control group using a matching algorithm. We define non-outsourcing establishments as those with no occupational layoffs between 1990 and 2000. For each treated worker, we take the set of workers employed by non-outsourcing establishments in the same 2-digit industry, 5-digit occupation, and regional court jurisdiction to be our potential control group. For both treated and control groups, we restrict to workers who were employed at the same establishment as a security guard for three consecutive years prior to outsourcing. We then estimate a probit regression of whether a worker is experienced an occupational layoff, controlling for wages two and three years prior, as well as tenure and AKM firm effect in the year prior to outsourcing.[24] For each treated worker, we then choose the non-outsourced worker with the closest propensity score to the comparison worker.

The following dynamic difference-in-differences specification is used to estimate the effects of occupational layoffs on incumbent workers:

$$y_{it} = \sum_{k=-4, k\neq -1}^{8} \delta_k \left(D_i \times I_{t=t^*+k}\right) + \alpha_i + \tau_t + X_{it}\beta + \varepsilon_{it} \tag{1}$$

where i indexes individual, t indexes year, y_{imt} is i's observed outcome in year t (e.g. employment, wage), D_i is an indicator for a person being outsourced in year t^*, X_{it} are demographic variables — namely, (age, age squared, and age cubed) $\times$ education categories, and ε_{imt} is an error term. We cluster standard errors by the outsourcing establishment.

Each coefficient δ_k measures the change in the outcome y_{imt} for impacted workers relative to the

[23] While this definition is natural for our study of occupational layoffs, note that it is different from the typical definition in the mass layoff literature, wherein the treated workers necessarily separate from the employer (e.g. Jacobson et al. 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Lachowska et al. 2020b; Schmieder et al. 2020). Furthermore, we do not condition on treated workers being transferred to a contract firm, so our estimates are also conceptually different from those in Goldschmidt and Schmieder (2017).

[24] See Section 4 for a precise definition of AKM firm effects.

control group in the k-th year relative to the year when the firm decided to outsource. These effects are best understood as the partial equilibrium effects of an occupational layoff on an incumbent direct-hire worker relative to the counterfactual that their particular employer did not decide to outsource at the time. As such, these estimates do not capture the market-level effects of increasing prevalence of outsourcing that we investigate in Sections 5 and 6.

Employment effects. Figure 3 visualizes the effects of occupational layoffs on workers employed at the establishment as a security guard for three consecutive years. Panel A compares the likelihood of formal employment of impacted and control workers. Year 0 is the year of the occupational layoff. The two groups show very similar trends in wages prior to the outsourcing year. The likelihood of formal employment is exactly one in the three years prior by construction, since we restrict to workers who were employed at the same establishment as a security guard for three consecutive years prior. Immediately following the occupational layoff, however, the employment trajectories starkly diverge. The likelihood of formal employment falls by 18 p.p. for control workers, likely due to regular employment turnover. The likelihood of formal employment fell by 68 p.p. for impacted workers. The likelihood of formal employment for impacted workers converges with the control workers over the course of the following years, however. Five years after the occupational layoff, there is no detectable difference in the likelihood of formal employment.

The differences in raw means strongly suggest that occupational layoffs reduced employment, but time-varying variables on the individual level (such as age) may confound the interpretation of these differences. To control for for individual fixed effects as well as year-level shocks, we turn to our difference-in-differences regression specification. Figure 3, Panel B plots estimated δ_k coefficients from equation (1), with an indicator for formal employment as the outcome variable. The regression estimates confirm that outsourcing decisions significantly displaced workers from formal employment in the first year. The likelihood that an affected workers is employed is 49 p.p. lower in the year after. The spell of reduced formal employment is transitory, however. The negative employment effect fully dissipates after about five years.

Table 1 shows the point estimates for 0, 1, and 5 years after occupational layoffs for various outcome variables. Strikingly, workers affected by occupational layoffs not only see reduced employment, but also differentially transition to different occupations. Immediately after an oc-

cupational layoff, impacted workers are 76 p.p. less likely to be formally employed in the same occupation. Even five years later, impacted workers are less likely to formally employed in the same occupation (by 12 p.p)., though they are no longer less likely to be formally employed.

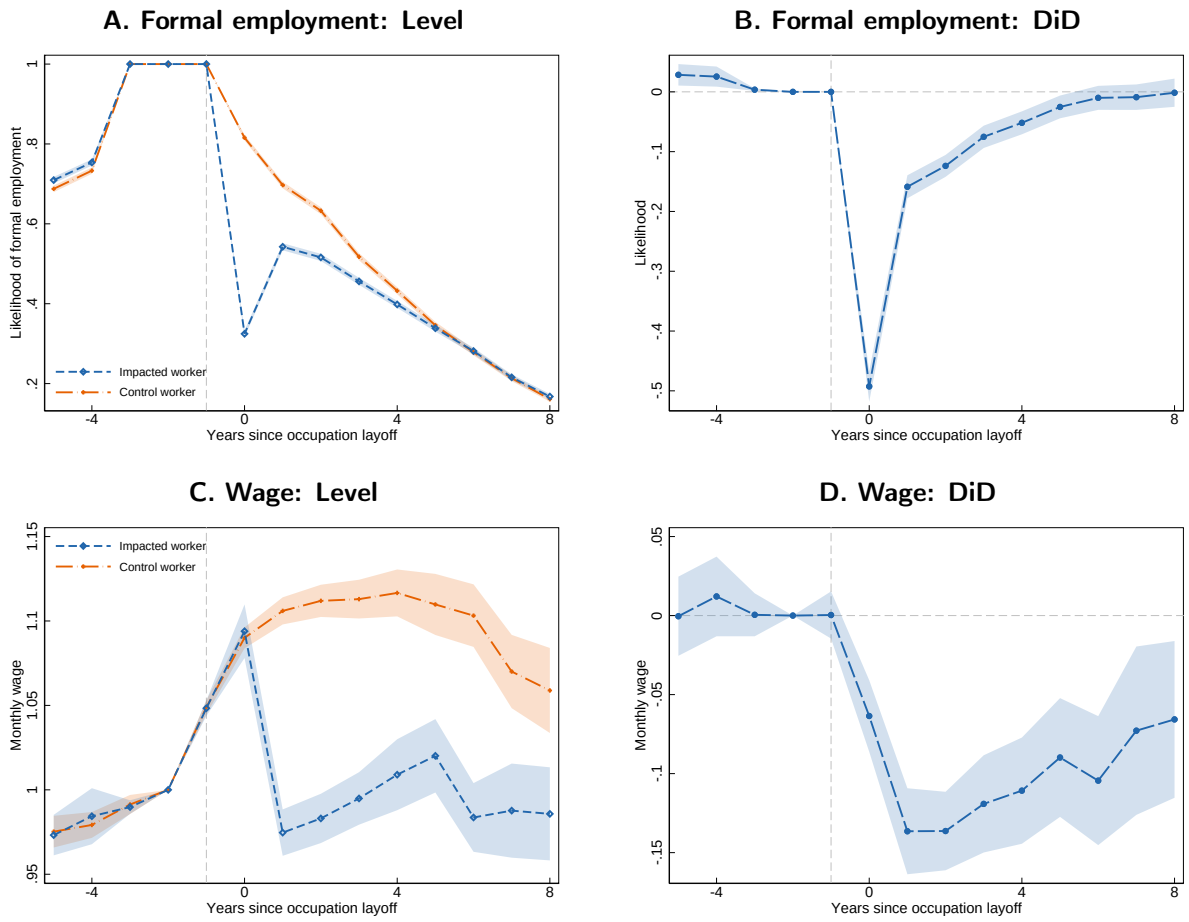
Only a small fraction of impacted workers end up in contract-firm employment. Impacted workers are only 0.2 p.p more likely to be formally employed by a contract firm immediately after an occupational layoff. The effect of occupational layoffs on contract-firm employment rises to 7.7 p.p in the following year, and is 11.7 p.p five years after.

Wage effects. The wage trajectories of treated and control workers in the years before and after an occupational layoff are shown in Figure 3, Panel C. Real wages are normalized within each worker such that their real wage in year $t-2$ is equal to one. We do not restrict this figure to be a balanced panel, but every worker in this sample must be observed for years $t=-3$ to $t=-1$, due to the tenure restriction of two years. As shown above, treated workers are less likely to be formally employed following occupational layoffs, so their wages are also less likely to be observed.

Selection makes it harder to compare the evolution of wages in the treated and control groups, but the figure clearly reveals that at the time of occupational layoff the observed wages of the two groups diverge. Within a year after the occupational layoff, the wages of treated workers (conditional on observation) is about 14 percent lower than the control group. Furthermore, these differences are large and persistent even after five years when the formal employment rates of the treated and control groups no longer exhibit any detectable differences, suggesting that worker selection is unlikely to explain the drop in wages.

Figure 3, Panel D displays regression estimates for equation (1) with normalized real wages as the outcome variable. Once again, observations are included only if the worker is formally employed and hence observed in our data. The estimates show that wages were roughly 14 percent lower one year after the occupational layoff. The wage decline was only somewhat attenuated after eight years. As shown in Table 1, point estimates using log wages as the outcome variable are somewhat larger, with wages falling by about 18 log points conditional on employment in the year after the occupational layoff.

Figure 3: Impact of occupational layoffs on incumbent workers



Note: Panels A and C show the raw outcomes of security guards affected by occupational layoffs relative to matched controls. Panels B and D plots coefficients γ_τ from a difference-in-differences regression measuring the impact of an occupational layoff on incumbent direct-hire security guards, where the control group are similar workers in establishments that did not have an occupational layoff. Panel A and B shows the likelihood of formal employment. Panels C and D show monthly wage, as measured as a fraction of wage two years prior to the outsourcing event, and observations are included only if the worker is formally employed. Our sample includes all occupational layoffs, as identified by sudden drops in an establishment security guard count, between 1990 and 2000. We include controls for individual and year fixed effects, and time-varying demographics. Shaded bands indicate 95% confidence intervals, with standard errors clustered at the establishment level.

Robustness. The findings of a short-run decline in employment and a persistent reduction in wage are highly robust to alternative matching strategies, as well as modifications to our definition of an occupational layoff. Appendix Figures B.7 and B.8 show that estimates are highly similar when instead matching within the 3-digit industry and local regional court jurisdiction, within the 2-digit industry and microregion, or within the 3-digit industry and microregion. Estimates are also highly similar when we include only establishments with at least 50 employees or at least

5 security guards, as well as when we only consider occupational layoffs where the number of security guards dropped to zero.

The effects of occupational layoffs were also similar across regions and time, as shown in the Appendix Figures B.9 and B.10. In all years, there was a sharp decline in employment after occupational layoffs, followed by a recovery within five years. The wage effects are somewhat more variable across years. For all years, wage declines were persistent, but they were larger in some years than in others. The disemployment effects of occupational layoffs were similar across regions, but there was a somewhat larger wage decline in the Brazil's South, where local courts were more restrictive towards outsourcing prior to legalization.

3.4 Present-Value of Earnings Losses due to Outsourcing Decisions

Figure 3 shows large employment and wage reductions associated with occupational layoffs, suggesting that workers suffered substantial losses in earnings. To estimate the present discounted value (PDV) of earnings losses, we follow the methodology of Davis and von Wachter (2011). We use a real interest rate of 5 percent, and sum the discounted losses over a 20-year period starting with the year of the occupational layoff. Because we do not observe the full 20 years of earnings after an occupational layoff, we impose a common rate of decay past the 8th year. The estimated mean PDV earnings losses for occupational layoffs is

$$PDV_{Loss} = \sum_{k=0}^{8} \hat{\delta}_k \frac{1}{(1+r)^k} + \sum_{k=8}^{19} \hat{\delta}_8 \frac{\left(1+\hat{\lambda}\right)^{k-8}}{(1+r)^k} \tag{2}$$

where $\hat{\delta}_k$ is the average estimated earnings loss in year k after occupational layoff, estimated using equation (1), and $\hat{\delta}_8 \left(1+\hat{\lambda}\right)^{k-8}$ is an extrapolated earnings loss using the common decay rate $\hat{\lambda}$. We calculate the decay rate as the average of annualized log differences in earnings losses from years 5 to 6 to years 7 to 8 after displacement.

A complication in our setting is that we do not observe earnings for workers who are employed in the informal sector. To impute earnings for missing observations, we use a range of methods that make different assumptions about what an unobserved worker would have earned. The first method simply assumes that workers earn nothing if they unobserved in our data. The second method

assumes that unobserved workers earn half the minimum wage. The third method assumes that unobserved workers earn exactly the minimum wage. The final method assumes that unobserved workers had the same earnings as observed workers, so we simply use the monthly wage estimates from the non-missing data that we reported in the previous section as the estimates for earnings losses. While the first method yields a strict upper bound on total earnings losses, the final method is likely to understate them, since earnings in the informal sector are lower on average (Bargain and Kwenda 2014).

Table 2 reports the alternative measures of the PDV earnings loss after an occupational layoff, for security guards with at least 3 years of positive earnings at an employer with at least 10 workers. We express the PDV earnings losses as the number of earnings years lost at the pre-occupational-layoff level of earnings. Our maximal estimate, which assumes that unobserved workers have zero earnings, show PDV earnings losses equivalent to 1.40 years of average pre-occupational-layoff earnings. When assuming that unobserved workers make either half of or exactly the minimum wage, losses are equivalent to 1.32 or 1.24 years of average pre-occupational-layoff earnings. When we use the monthly wage estimates from the non-missing data, losses are equivalent to 1.06 years of average pre-occupational-layoff earnings. Regardless of the imputation method, the PDV of earning losses appear to be substantial, suggesting that the persistent wage reductions account for much of the total earnings losses. The magnitude of earnings losses is similar to that of job displacement in the U.S. provided by Davis and von Wachter (2011), who report earnings losses equivalent to 1.4 years of pre-displacement earnings in non-recession years.

4 Outsourcing and Firm Wage Premia

Why do the wages of incumbent workers persistently fall after outsourcing decisions? A prominent idea in recent economic literature is that outsourcing reduces the wage premia that firms share with workers (Dube and Kaplan 2010; Weil 2014; Goldschmidt and Schmieder 2017).[25] For example, firms that outsource may avoid paying higher wages required by collective bargaining agreements, which are typically negotiated by sector and region in the Brazilian economy, by outsourcing.

[25]Larry Katz articulates this view as follows: "When janitors work at Goldman Sachs as Goldman Sachs employees, they tend to share in the firm's huge productivity benefits and huge rents. But if they work for Joe's Janitorial Services, they no longer share in those rents" (Clement 2017).

High-wage firms may also avoid pressure to pay lower-skilled workers a wage premium in the interest of fairness or equity by moving them outside the boundary of the firm.

In this section, we empirically test the idea that firm outsourcing decisions reduce worker access to firm-specific wage premia. We do so in three steps. First, we examine whether workers experiencing outsourcing decisions transition to lower-wage firms. Second, we test whether workers initially at high-wage firms experience larger wage declines when their employer decides to outsource. Third, we test whether high-wage firms are more likely to outsource. We find that the loss of firm-specific wage premia substantially explains why the wages of incumbent workers fell after occupational layoffs. However, we do not evidence that outsourcing legalization reduced the average wage premia in the occupation.

4.1 Measuring Firm Wage Premia using AKM Decomposition

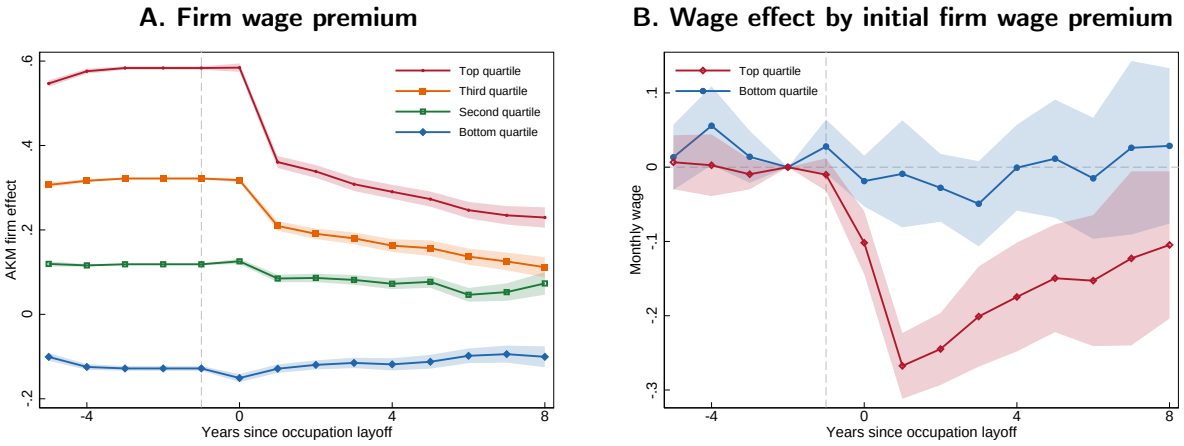
To measure firm-specific wage premia, we use the decomposition method of Abowd et al. (1999) (henceforth, "AKM firm effects"). Using data on all formal workers in RAIS spanning 1985-2002, we estimate:

$$\log w_{it} = \psi_{J(i,t)} + \alpha_i + \theta_t + X_{it}\beta + \varepsilon_{ijt}$$

where w_{it} represents real monthly wage, α_i is a individual fixed effect (capturing the general productive characteristics of workers), $\psi_{J(i,t)}$ is a firm fixed effect (capturing the wage premia for all workers at the firm), θ_t is a year fixed effect, $X_{it}\beta$ are the effects of time-varying observable worker characteristics (such as education and age), and ε_{ijt} is a composite error that may include idiosyncratic worker-firm match effects. The estimated firm fixed effect ($\hat{\psi}_j$) can be thought of as representing time-invariant policies of a given firm with respect to compensation.

To ensure that firm and worker fixed effects are identified, we restrict our analysis to the largest connected set of firms that are linked by workers moving between them. Identification of the AKM model also requires that workers do not move across firm in a manner that is systematically correlated with unmeasured productivity (Gibbons and Katz 1992). Alvarez et al. (2018) provide evidence that this assumption is justified in Brazilian RAIS data. A further concern when estimating the AKM model is limited mobility bias, which may generate misleading variance decompositions, as discussed by Andrews et al. (2008). However, limited mobility bias is likely to

Figure 4: Incumbent wage reduction is related to loss of firm wage premia



Note: Panel A plots the average AKM firm effect of an incumbent direct-hire security guards in the years before and after firm outsourcing decisions, conditional on being observed. Panel B shows difference-in-differences regression estimates for the effect of outsourcing decisions on incumbent monthly wage (as a fraction of wage two years prior to the outsourcing event) conditional on formal employment, separately for high-wage and low-wage outsourcing establishments. In the red series of Panel B, we include only workers initially employed at a firm in the top quartile of the AKM firm effect distribution among impacted workers; in the blue, we include only those employed in the bottom quartile.

be small in our setting since we use a long panel (Lachowska et al. 2020a; Bonhomme et al. 2020).

4.2 Effects of Outsourcing on Worker Access to Firm Wage Premia

Having obtained a measure of firm wage premia from the AKM model, we next investigate how outsourcing decisions affected firm wage premia of incumbent workers.

Figure 4, Panel A visualizes how the incumbent worker's access to firm wage premia evolves before and after an occupational layoff. Each line plots the mean AKM firm effect (conditional on observation) of worker initially employed (in years -3 to -1) by firms in different quartiles of the AKM firm effect distribution. The figure shows that the mean AKM firm effects of the incumbent workers is generally stable prior to occupational layoffs, but precipitously changes upon occupational layoffs. Incumbent workers initially at the top quartile to transition to firms with much lower firm wage premia. Their mean firm effect falls by almost 20 log points. By contrast, workers initially at lower-wage firms did not experience a significant change in the AKM firm effect.

If the loss of firm wage premia is a primary reason that incumbents experiencing occupational layoffs see substantial wage losses, one would expect that occupational layoffs at high-wage firms

would lead to larger drops in wages. This is indeed the case. Figure 4, Panel B plots coefficients from separate regressions for equation (1) that only include workers employed in year $t-1$ at the top and bottom quartile of the AKM firm effects distribution, respectively. We detect no statistically significant wages effect for workers initially at establishments in the bottom quartile of the AKM firm effects distribution. By contrast, the wages effect for workers initially employed by the top quartile firms experience a large level decline in wages. We infer from this that the negative wage effect shown in Panel B is related to the loss of firm-specific wage premia.

The last line of Table 1 shows that the loss of firm-specific wage premia explains 42 percent of the total wage losses in the year of the occupational layoff, 43 percent one year after the occupational layoff, and 46 percent five years after. These numbers suggest that changes in firm wage premia explain a substantial fraction of incumbent wage losses.[26]

4.3 Firm Wage Premia and The Decision to Outsource

If wage reductions resulting from outsourcing decisions are substantially explained by the loss of firm wage premia, then rent exclusion could be a reason for firms to outsource.

To test for this possibility, we estimate a linear probability model to test whether high-wage firms are more likely to outsource during the years after outsourcing legalization. Our sample is establishments with at least 50 employees and at least 3 security guards in 1993. The dependent variable takes on a value of one if the number of security guards in the establishment fell by two thirds by 1998 (and dropped to zero if it initially had five or fewer guards) and a zero otherwise. We exclude establishments whose total non-security guard headcount declines by more than 10 percent by 1998, and include microregion fixed effects in all specifications.

Consistent with Goldschmidt and Schmieder (2017), we find evidence that high-wage establishments were more likely to outsource. As shown in Table 3, outsourcing decisions are positive correlated with three metrics of firm wage premia—mean wage in 1993, higher mean security-guard wage in 1993, and higher AKM firm effects. However, outsourcing decisions are not significantly correlated with establishment size.

[26] By comparison, Lachowska et al. (2020b) estimate that firm effects explain 17 percent of wage losses from job displacement in the U.S., while Schmieder et al. (2020) estimate that firm effects account for 75 percent of wage losses from job displacement in Germany. The striking differences in the importance of firm wage premia across countries remains an unresolved puzzle in the literature.

Even though high-wage firms were more likely to outsource and the average AKM firm effect of laid-off workers fell, outsourcing legalization did not cause the average AKM firm effects of all security guards to fall. In Appendix Figure B.14, we plot the mean AKM firm effect of security guards, averaged across microregions in our estimation sample, separately for restrictive and permissive microregions. The average firm effect among security guards was falling both before and after outsourcing legalization, in both restrictive and permissive regions. However, we do not detect any trend break in 1993, when outsourcing was declared legal and the contract-firm share began to quickly rise. Furthermore, the fall in mean firm wage premia appears larger in permissive regions, where there was a milder increase in outsourcing, than in restrictive regions.

To briefly summarize, we find that (i) high-wage firms were more likely to outsource, (ii) the average AKM firm effect of incumbents experiencing occupational layoffs fell, and (iii) the average firm wage premia among security guards was falling throughout the study period, but that (iv) outsourcing legalization did not cause the occupation-wide average firm-specific wage premia to fall. This final finding is consistent with our finding in Section 7 below that the effect of outsourcing legalization on the market-level wage markup was small, and suggests that outsourcing legalization did not significantly alter the wage premia of security guards who did not experience occupational layoffs.

5 Market-level Effects of Outsourcing: Empirical Approach

This section explains how we identify the market-level effects of outsourcing legalization. First, we describe regional variation in local labor court permissiveness towards outsourcing. We then present our triple-difference regression specification.

5.1 Regional Differences in Court Restrictiveness

As mentioned above, there were significant regional differences in court permissiveness towards domestic outsourcing during the pre-legalization period. We discovered these differences through field and archival investigations. Three primary source were particularly important for our discovery: (i) retrospective interviews with labor lawyers, judges, and contract firms in Brazil, (ii) an additional set of 28 interview transcripts generously provided by Magda Biavaschi, a Brazilian

jurist and legal historian who interviewed a large number of prominent judges and lawyers regarding the history surrounding Súmula 331, and (iii) a set of publicly available legal cases cited by Súmula 331.[27]

These primary sources reveal that outsourcing was both heavily litigated and frequently disallowed in the South, but not so in the rest of Brazil.[28] The available legal cases and interview transcripts show that Southern regional courts repeatedly disallowed outsourcing during the early 1990s, even in cases where the Superior Labor Court would have considered it legal. For instance, a union leader in the Southern state of Paraná reported he "litigated complaints... arguing that those who performed doormen services were bankers [and] these lawsuits were successful." By contrast, courts in the rest of Brazil appeared to have more frequently allowed outsourcing even before Súmula 331. For example, a labor lawyer in metropolitan São Paulo reported that "successive losses [on the issue of outsourcing] at court discouraged the unions."[29] Based on similar primary sources, we classify the four Southern-most regional courts as "restrictive" and the remainder as "permissive." The classifications are shown in Table 4. We describe the rationales for the exact classification of individual courts and report robustness checks in Appendix C.1.

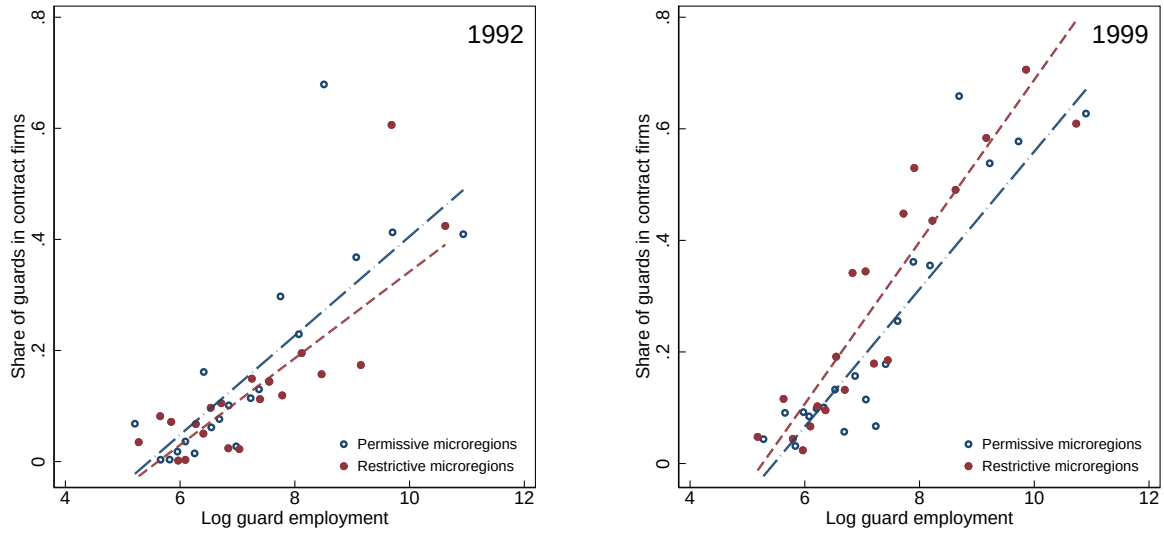
The data confirm our findings from the archives. First, restrictive microregions had systematically less outsourcing in the pre-legalization period. Figure 5, Panel A visualizes the relationship between contract-firm share and market size in 1992, separately for restrictive and permissive microregions. Consistent with the idea that scale economies are necessary for contract firms to be profitable, the contract-firm share is increasing the market size in both regions. Conditional on market size, however, restrictive microregions had systematically lower contract-firm shares in 1992. We statistically test for differences in contract-firm share of security guard employment between the restrictive and permissive microregions in 1992, with controls for log market size, in Table C.2. We find that restrictive microregions before Súmula 331 had contract-firm lower by a marginally

[27] Sadly, case records from the pre-legalization period no longer exist for most local labor courts. In cases where these records still exist, they are neither digitized nor searchable by keyword. We report our attempts to obtain court records in Appendix C.1, where we also discuss in more detail how we transformed the evidence from interview transcripts into court classifications.

[28] Labor courts in Brazil's South are also known throughout the legal profession to be more pro-labor. Brazil's South was also the intellectual home of the Alternative Law Movement, which advocated for activist jurisprudence to address social injustice (Barreto and de Lyra 2016).

[29] See Appendix Table C.1 for additional quotes regarding the proclivity of regional courts, extracted from our review of all interview transcripts, as well as from our own interviews of judges Magda Biavaschi (Regional Court 4, in the South) and Sérgio Torres (Regional Court 6, in the North).

Figure 5: Relationship between outsourcing and market size



Note: Figure presents a binned scatter of contract-firm share against log market size in 1992 and 1999, separately for permissive and restrictive microregions. Market size is measured by the total employment of private-sector security guards in RAIS.

significant 3 percentage points.

Second, restrictive regions experienced a much larger increase in outsourcing after legalization. By 1999, six years after legalization, the contract-firm share of security guard employment was a statistically significant 4 percentage points *higher* in restrictive microregions in 1999, after controlling for market size. This suggests that restrictive regions experience a much larger rise in outsourcing between 1992 and 1999. As shown in Figure 5, Panel B, the increase in contract-firm share occurred in larger markets, while smaller markets appear to see smaller increases in contract-firm share. As a result, the relationship between contract-firm share and market size strengthened between 1992 and 1999.

Third, the contract-firm share shows sharp trend breaks for each of restrictive jurisdictions in 1993, the legalization year. To test for a trend break, we use a Chow test to measure whether the linear trend in the contract-firm share of each local court jurisdiction during 1987-1993 is statistically distinguishable from that during 1994-2000. Table 4 reports that trend breaks are detected with a p-value less than 0.01 in all four regional labor courts classified as restrictive. Furthermore, the contract-firm share rose by at least 10 percentage points between 1992 and 1999 in all of the restrictive jurisdictions. By contrast, only one of the 20 permissive regional labor

courts have both a sharp trend break and a substantial increase in the contract-firm share.[30]

5.2 Regression Specification

To isolate the effects of outsourcing legalization from concurrent national-level events, we compare Brazil's South, where local judges had been more restrictive towards outsourcing, to the rest of the country. To mitigate the influence of confounding regional trends, we use both cross-region and cross-occupation variation in a triple-difference design, as well as reweighting methods to reduce observable regional dissimilarities.

We construct a balanced estimation sample of occupations and microregions as follows. We select all microregions with employment greater than 25 for both security guards and cleaners in every year, which yields 266 microregions. We then select all occupations with at least two workers in every year for all selected microregions. We exclude manager, public sector, and agricultural occupations, which are likely to follow different trends than a private-sector, urban, production occupation like security guards. Finally, we exclude cleaners, because they had a high contract-firm share prior to legalization, but did not experienced a significant increase in outsourcing following legalization (as previously shown in Figure 1, Panel A).

Table 5 shows the occupations and their characteristics in the final estimation sample, which includes security guards as the treated occupation and 11 comparison occupations. The sample includes 98.6 percent of all formal-sector security guards in the country, while the comparison occupations account for roughly 50 percent of total national formal employment.

We use two regression specifications for estimation. First, we use an event-study approach and estimate regressions using the following triple-difference design:

$$y_{ort} = \sum_{\tau=1985;\tau\neq 1992}^{2006} \beta_\tau \left(T_{or} \times 1_{t=\tau}\right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \varepsilon_{ort}, \tag{3}$$

where y_{ort} denotes the outcomes of interest (e.g. mean log wage in occupation o in microregion r in year t), T_{or} is indicator variable equal to one if and only if occupation o is treated and the regional

[30] Figures C.1 and C.2 provide visualizations of the regional trends in contract-firm share. Figure C.3 examines the relationship between contract-firm share and state-level union density, as measured by Brazil's household survey PNAD. States with stronger unions (which are predominantly in Brazil's South) had less outsourcing prior to legalization, but higher rates of outsourcing six years after.

labor court overseeing microregion r was restrictive to outsourcing prior to legalization, δ_{or} are microregion-year fixed effects, δ_{ot} are occupation-year fixed effects, and δ_{rt} are microregion-year fixed effects. The coefficients β_τ measure how the evolution of the outcome variable y_{mt} differed between the treated and comparison occupations in restrictive microregions, relative to the analogous difference in permissive microregions, with the pre-legalization year of 1992 normalized to zero. Equation (3) yields estimates of the causal effect of outsourcing legality under the assumption that differences in outcomes for security guards and other occupations in permissive and restrictive regions would have developed with parallel trends (i.e., at the same rate) if outsourcing legality were the same everywhere.

Second, we report estimates from a pooled regression that includes controls for potential microregion-occupation-specific linear time trends:

$$y_{mt} = \sum_{\tau=1994}^{1997} \gamma_{SR} \left(T_{or} \times 1_{t=\tau}\right) + \sum_{\tau=1998}^{2002} \gamma_{LR} \left(T_{or} \times 1_{t=\tau}\right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \left(\delta_{or} \times t\right) + \varepsilon_{ort} \qquad (4)$$

where γ_{SR} and γ_{LR} measure how the outcome variable y_{mt} evolved in restrictive microregions, compared to permissive microregions, during the short-run (1-4 years after legalization) and long-run (5-9 years after). This approach assumes that any pre-legalization trends in outcomes between permissive and restrictive regions are linear and would have continued at the same rate in the absence of the outsourcing legalization.

The main challenge to the assumption of parallel trends in our setting is that differences in labor demand across occupations may develop with non-parallel trends across microregions for reasons unrelated to outsourcing legalization. Since our specification includes flexible controls for both occupation-specific and regional-specific trends, the primary confounding factors are labor demand shocks that are both local to the South and specific to the security guard occupation. One such confounding factor is Brazil's concurrent trade liberalization, which differentially affected the manufacturing-heavy microregions in South and is known to have reduced local employment and increased local crime (Dix-Carneiro et al. 2018). Reduced economic activity in these regions is likely to reduce firms' demand for security guards. However, an increase in crime could also plausibly lead to increased demand for security guards.

To account for the influence of potentially confounding trends in labor demand, we use re-

gression specifications that adjust for occupation-specific trends that vary with six pre-legalization local labor market characteristics. These characteristics includes local log employment, unemployment rate, homicide rate (provided by Dix-Carneiro et al. (2018)), as well as three covariates that capture local exposure to international trade shocks. The three trade-related covariates are the share of employment in tradable industries, log formal employment of importers, local import tariff competition exposure (defined as the employment-weighted average of $\log(1+1994\text{ tariff}) - \log(1+1990\text{ tariff})$ across industries in the microregion and provided by Felix (2021)).

Our main specification uses entropy-balancing weights, as proposed by Hainmueller (2012), to ensure that restrictive and permissive microregions are balanced according to the above pre-legalization labor market characteristics.[31] For robustness, we report estimates using inverse propensity score weights that were computed using the same covariates, as well as estimates with regression adjustment using the same covariates. We also report estimates using a combination of reweighting and regression adjustment, which are known to be "doubly robust" to misspecification (Glynn and Quinn 2010). Figure 6 visualizes the entropy-balancing weights on a map.[32] Reweighting using entropy-balancing reduces differences between restrictive and permissive microregions even for characteristics that are not directly targeted (see Appendix Table C.3).

We report standard errors clustered at the regional court level. We also report randomization inference p-values, computed by permuting treatment assignment across the 24 regional courts, when tabulating pooled regression estimates.

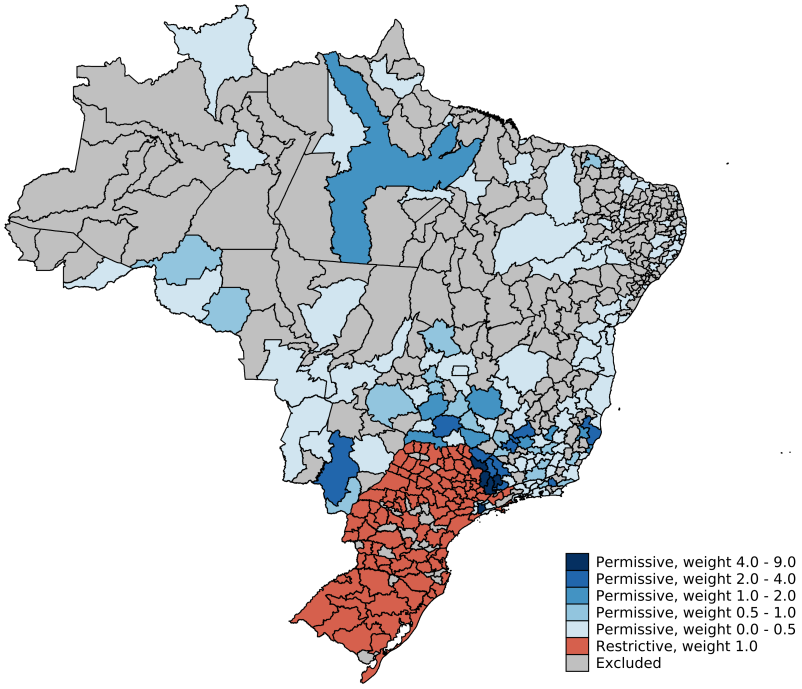
6 Market-level Effects of Outsourcing: Results

The triple-difference strategy described above reveals that outsourcing legalization (i) increased outsourcing, (ii) reallocated jobs from the old to the young, (iii) caused a substantial increase in employment, as well as (iv) a small increase in composition-adjusted wage. All of these effects appear to be persistent, rather than transitory.

[31] This method computes weights for the permissive microregions such that a set of desired characteristics exactly match those of restrictive microregions. We choose the set of weights that minimally deviates from uniform weights.

[32] Figure C.4 visualizes the inverse propensity score weights on a map.

Figure 6: Map of microregions with entropy-balancing weights



Note: Map shows microregions used in our main specifications, where observations are weighted using entropy balancing weights. Entropy balancing weights are computed using log employment, homicide rate, unemployment rate, share of employment in tradable industries, log formal employment of importers, and local import tariff competition exposure in 1992.

6.1 Effect on Outsourcing Prevalence

Our main specification confirms that legalization generated a large differential increase in outsourcing for security guards in the South. We use two indirect measures of outsourcing: (a) the share of occupational employment in contract firms, and (b) the occupational Herfindahl-Hirschman Index (HHI), a commonly used measure of labor market concentration, defined as the probability that two randomly selected security guards within a microregion are employed by the same firm.

For each outcome, we show two visualizations. First, we plot the difference in outcomes between the treated and comparison occupations over time, separately for restrictive and permissive microregions. Specifically, we plot the coefficients α_τ from the following occupation-level difference-in-difference regression specification:

$$y_{ort} = \sum_{\tau=1985;\tau\neq1992}^{2006} \alpha_\tau \left(T_o \times 1_{t=\tau}\right) + \delta_o + \delta_t + \varepsilon_{ort}, \tag{5}$$

where T_o is indicator variable equal to one if and only if occupation o is treated. We do so separately for subsamples that include either only restrictive microregions (in red) or only permissive microregion (in dark gray). We use entropy-balancing weights to reduce differences in the pre-legalization observable characteristics of permissive and restrictive microregions.

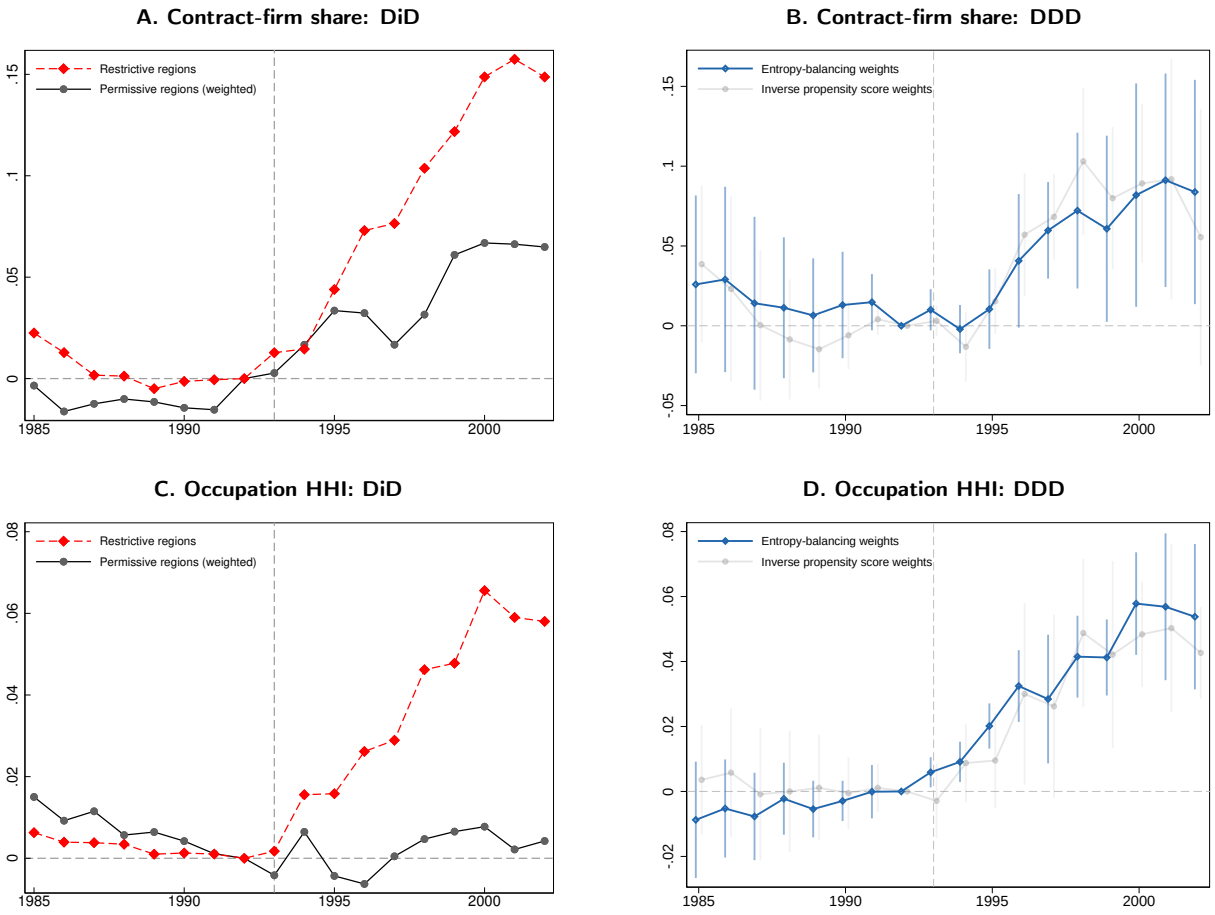
Second, we plot estimated β_τ coefficients from our triple-difference specification in equation (3). This specification is akin to taking the difference between the estimated α_τ's of the two regions, but with an additional full set of controls for occupation-year, microregion-year, and microregion-occupation fixed effects. To visualize the role of entropy-balancing in driving our estimates, we plot coefficients estimated from a regression with the inverse propensity score weights (in light gray) and entropy-balancing weights (in blue).

Figure 7 reveals that outsourcing prevalence sharply rose following legalization. Panel A shows that the difference in contract-firm share between the treated and comparison occupations over time, estimated using equation (5) separately for the subsamples of restrictive and permissive microregions. In both regions, the contract-firm share of security guards did not significantly change relative other occupations during the years prior to legalization, but began to grow immediately after outsourcing legalization. The growth is much larger in the restrictive microregions, where the relative contract-firm share in 2000 is roughly 15 p.p. higher than it was in 1992. By contrast, the relative contract-firm share is only about 6 p.p. higher than it was in 1992 in the (reweighted) permissive regions.

Panel B displays the corresponding triple-difference estimates. The stark divergence across regions survives this more demanding specification. There is a very gentle differential downward trend in the pre-legalization period, followed by a steep increase in the post-legalization period. The differential effect of outsourcing legalization on contract-firm share appears to plateau at roughly 7 p.p. higher. These effects are similar whether observations are weighted equally or using entropy-balancing weights.

Table 6 reports the pooled short-run (1-4 years) and long-run (5-9 years) effects of outsourcing legalization estimated using equation (4). Columns (1) and (2) use entropy-balancing weights and inverse propensity score weights, respectively. Column (3) uses regression adjustment with uniform weights. Columns (4) and (5) combine regression adjustment with entropy-balancing weights and inverse propensity score weights, respectively. All columns include controls for microregion-

Figure 7: Impact of outsourcing legalization on occupation structure



Note: Panels A and C plot the coefficients α_τ from the occupation-level difference-in-difference regression specification equation (3), separately estimated using restrictive and permissive regions. Panels B and D plot the coefficients β_τ from the triple-difference regression measuring the impact of outsourcing legalization in equation (3). The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

occupation linear trends.

Panel A shows that outsourcing legalization had a large and positive effect on the contract-firm share that is robust to alternative specifications. The pooled long-run (5-9 years) effects of outsourcing legalization on contract-firm share, estimated using entropy-balancing weights, vary between 6.2 - 7.1 p.p.. This represents an increase in contract-firm share of about 55-60 percent relative to the pre-legalization 1992 mean in restrictive microregions of 11.7 percent. The estimates are highly similar in the remaining columns.

Figure 7, Panel C visualizes the effect of outsourcing legalization on occupation concentration.

Since contract firms provide workers to multiple clients and require a minimum scale to operate, a rise in outsourcing should increase the concentration of workers among firms within an occupation. The data confirms this prediction. In the restrictive microregion, the occupation concentration of security guard exhibits little differential trend prior to legalization, but rose immediately after outsourcing legalization, and eventually reach roughly 5 p.p. higher roughly seven years after legalization, almost doubling the pre-legalization mean concentration in restrictive microregions of 6.0 percent in 1992.

Panel D confirms that the stark divergence in occupation concentration across regions survives the more demanding triple-difference specification. We find a very gentle differential upward trend in the pre-legalization period, followed by a much steeper increase in the post-legalization period. The effect of outsourcing legalization on contract-firm share plateaus at roughly 5 p.p. higher after five years. These effects are similar whether entropy-balancing weights or inverse propensity score weights are used.[33]

6.2 Reallocative Effects across Age Groups

We now show that outsourcing *reallocated* security guard employment away from older security guards to younger entrants.

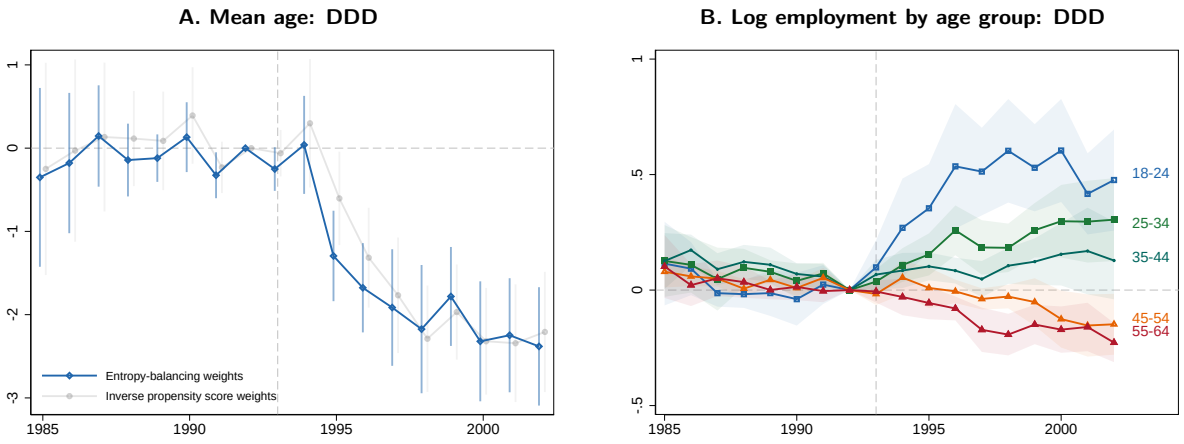
Figure 8, Panel A visualizes triple-difference estimates of the effect on mean age. In the figure, the mean age of security guards shows no differential trend in the pre-legalization period. However, it fell immediately after outsourcing legalization, eventually plateauing at roughly 2 years lower, relative to the pre-legalization mean of 43 years. These effects are similar whether entropy-balancing or inverse propensity score weights are used.

Table 6, Panel B shows that including controls for microregion-occupation-specific linear trends only somewhat attenuates the estimated decline in mean age. Furthermore, this finding is highly robust to alternative specifications, since all columns show that outsourcing legalization decreased the mean age by 1.5 - 1.9 years.

Figure 8, Panel B presents triple-difference estimates of the effect on log employment by age

[33] Table C.4, Panel B shows that the magnitude of the increase in occupation concentration is only somewhat weakened by the inclusion of controls for microregion-occupation-specific linear trends. The pooled long-run (5-9 years) effects of outsourcing legalization on occupation concentration, estimated using entropy-balancing weights, is 2.9 - 3.7 p.p. with linear trend controls and is statistically significant.

Figure 8: Impact of outsourcing legalization on employment composition



Note: Figure plots the coefficients β_τ from the triple-difference regression measuring the impact of outsourcing legalization in equation (3). Panel A shows the mean age as the outcome variable; Panel B shows the log employment in respective age groups. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

group. The figure shows little pre-legalization trends for employment in any of the included age groups. Immediately after legalization, however, there is a very large increase in employment of younger workers. Employment between the ages of 18-24 grew by almost 50 log points. Employment of workers in the 25-34 and 35-45 age groups increased by about 20 and 10 log points, respectively. By contrast, employment of workers in the 45-54 and 55-64 age group both declined. The decline is larger for the oldest age group of 55-64, at 15 log points.

These effects suggest that there was a large influx of young security guards into the occupation that coincided with increased exit of older workers. This result enriches our findings in Section 3. Previously, we document a large wave in occupational layoffs following legalization, which substantially displaced incumbent workers. The market-level specification here additionally reveals that outsourcing created not only losers, but also winners. Though the employment and wages of incumbents fells, the employment of younger workers grew as a consequence of job reallocation.

6.3 Effect on Employment

Even more strikingly, outsourcing legalization caused total security guard employment to increase by 8-15 percent.

Figure 9, Panel A plots the differences in log employment between security guards and other occupations over time, estimated using equation (5), separately for the subsamples of restrictive and permissive microregions. The trends in log employment of security guards were similar across the two regions prior to legalization, with both rising relative to the comparison occupations. Around 1992, log employment of security guards ceased to rise relative to comparison occupations in both regions, and began to fall. These secular trends are attributable to changes in national-level relative labor demand for security guards, which similarly affected police employment, as shown in Appendix Figure A.2.

After outsourcing legalization, however, the employment trends of the two regions diverged. During the first three years after legalization, log employment in restrictive region becomes consistently higher, with the gap initially growing. For remaining years that we measure, the gap remains large at a roughly 10 log point difference.

Figure 9, Panel B plots the estimated effect from equation (3) with log employment as the outcome variable. The divergence in log employment across regions survives this more demanding specification. Since we find a slight downward differential trend prior to legalization, the estimated rise in log employment is unlikely to be the result of pre-existing differences in the evolution of local occupational labor markets.

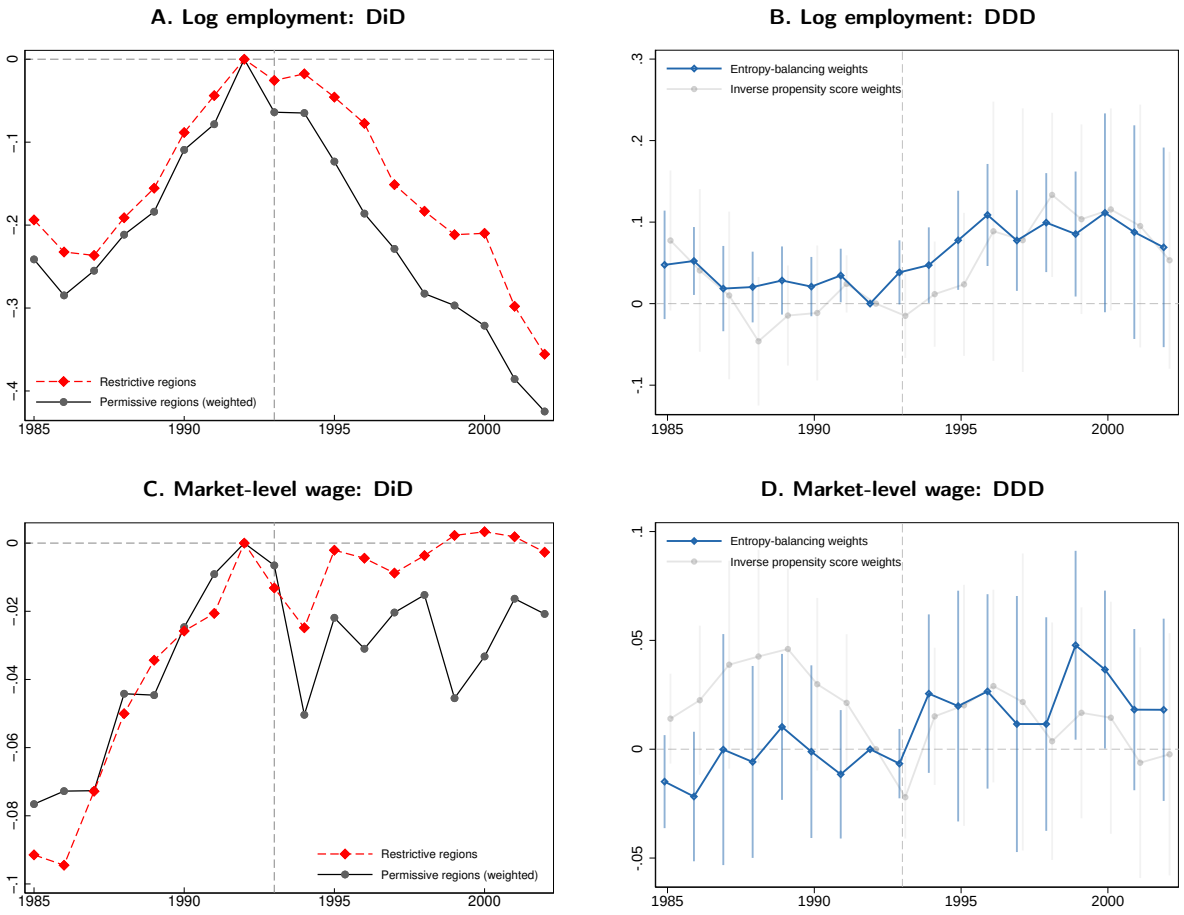
Table 6, Panel C shows that the positive effect of outsourcing legalization on employment survives controls for microregion-occupation-specific linear trends. The estimated effect of outsourcing legalization on employment varies between 8.5 - 15.1 log points and is statistically significant in all columns. The positive employment effect is therefore highly robust to alternative specifications.

6.4 Effect on Market-level Wage

Finally, outsourcing legalization increased market-level wages by 2-4 percent.

To measure the market-level wage, we estimate a worker-level regression of log monthly wage on microregion-occupation-year fixed effects with controls for observable worker demographics (including age, age squared, and age cubed interacted with four education levels and gender). The estimated microregion-occupation-year fixed effects is defined as the demographic-adjusted wage.

Figure 9: Impact of outsourcing on market-level wages and employment



Note: Panels A and C plot the coefficients α_τ from the occupation-level difference-in-difference regression specification equation (3), separately estimated using restrictive and permissive regions. Panels B and D plot the coefficients β_τ from the triple-difference regression measuring the impact of outsourcing legalization in equation (3). In Panels A and B, the outcome variable is log private-sector formal employment. In Panels C and D, the outcome variable is the estimated microregion-year fixed effect from a worker-level wage equation that includes worker demographic controls. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

Figure 9, Panel C plots differences in demographic-adjusted wage between security guards and other occupations over time, estimated using equation (5) separately for the subsamples of restrictive and permissive microregions. The trends in the demographic-adjusted wage of security guards were highly similar across the two regions, with both rising relative to the comparison occupations prior to legalization. After outsourcing legalization, however, the trends of the two regions diverged. The demographic-adjusted wage in restrictive region is consistently higher, by roughly 2 log point, for all of the years after outsourcing legalization that we measure.

Panel D shows that the divergence in market-level wage across regions after legalization survives the more demanding triple-difference specification. We find no differential trend in demographic-adjusted wages prior to legalization. Immediately after, our point estimates show an increase in the demographic-adjusted wage of about 2 log points, beginning in 1994. This increase persists even after nine years. However, we are unable to statistically distinguish the small positive effect from zero except in two years. As shown in the light gray series in Figure 9, Panel B, we observe substantial pre-trends when inverse propensity score weights are used instead.

The inclusion of controls for microregion-occupation-specific linear trends in our pooled specification increases the precision of the estimates. Table 6, Panel D shows our estimates of the short-run and long-run effects of legalization on demographic-adjusted wage using equation (4). As shown in Column (1), the estimated long-run effect using entropy-balancing weights alone is 2.5 log points and is marginally significant at the 10 percent level. The coefficients in Columns (2) and (3) are similar in magnitude, at 2.0 and 1.7 log points, respectively, but they are statistically indistinguishable from zero. In Columns (4) and (5), which combine regression adjustment and reweighting to be "doubly robust" to misspecification, the long-run effects are both statistically significant and sizeable, at 4.7 and 3.9 log points, respectively.

Estimates using alternative measures of the market-level wage generate similar results. Table C.5 reports the effects of outsourcing legalization on (unadjusted) mean log wage and on within-worker wage, which is computed analogously to the demographic-adjusted wage but with added controls for worker fixed effects. The corresponding long-run estimates using the combination of regression adjustment and entropy-balancing are 3.6 and 3.8 log points. Both are statistically significant at the 5 percent level.

These positive wage estimates are surprising because recent research has consistently found that domestic outsourcing is associated with lower wages at the firm level (Abraham 1990; Berlinski 2008; Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2020). Our worker-level analysis also suggests that firm outsourcing decisions reduced the wages of incumbent workers. Our finding therefore implies that outsourcing legalization generated positive market-level equilibrium wage effects. Section 7 provides a formalized interpretation of this finding.

6.5 Additional Robustness

As previously shown in Table 6, we obtain very similar estimates when using alternative weighting and regression adjustment methods to control for the confounding influence of occupation-specific trends related to six pre-legalization microregion characteristics. Table C.6 drops the controls for microregion-occupation-specific linear trends, which reduces the precision of the employment estimates and makes the wage estimates statistically indistinguishable from zero. Table C.7 uses entropy-balancing weights computed using subsets of the six microregion characteristics. As expected, the wage and employment effects become smaller and less precise when we do not control for differential exposure to concurrent trade liberalization. Table C.8 shows that our findings are robust to dropping observations from metropolitan São Paulo and the entire São Paulo state, respectively. Since we are uncertain about the correct classification of these regions (see Appendix C.1), we are reassured by the fact that these regions do not drive our main results. Figures C.10-C.13 show no sharp discontinuity for many outcomes at the border between restrictive and permissive jurisdictions and suggests potential geographical spillovers at the border.

7 Interpretative Framework

In the previous section, we found that outsourcing legalization raised market-level employment and wages. This final section provides a parametric economic interpretation using a simple union bargaining model wherein outsourcing legalization may alter both worker wage bargaining power and transactions costs. We combine this model with assumed structural parameters to infer changes in transactions costs, worker power, and welfare. We then perform a simple cost-benefit analysis to quantify the overall economic consequences of outsourcing.

7.1 Right-to-Manage Union Bargaining

To model the labor market for security guards in Brazil, where wages are determined partly through a sectoral bargaining system, we assume that workers are organized into a union. A representative firm and a representative union bargain over the wage w, with the disagreement outcome being the competitive wage w_c. The firm then chooses employment L to maximize profits. This model is

referred to in the literature as the "right-to-manage" model, since bargaining is only over wages and the firm has the authority to unilaterally set the level of employment.[34]

The firm's chosen employment is

$$L^D(w) = \arg\max_L pF(L) - (w+c)L, \tag{6}$$

where F is the firm's increasing and concave production function, p is the firm's product price, and the firm's unit labor cost consists of the wage w and transactions cost c. L^D denotes the firm's derived labor demand of security guards as a function of the wage w. When the union possesses nonzero bargaining power, the wage is set above the competitive wage, with a markup $\mu > 1$ such that

$$w = \mu w_c, \tag{7}$$

where μ increases in the union's bargaining power. The counterfactual competitive wage w_c is determined by the labor market clearing condition, given by

$$L^D(w_c) = L^S(w_c) \tag{8}$$

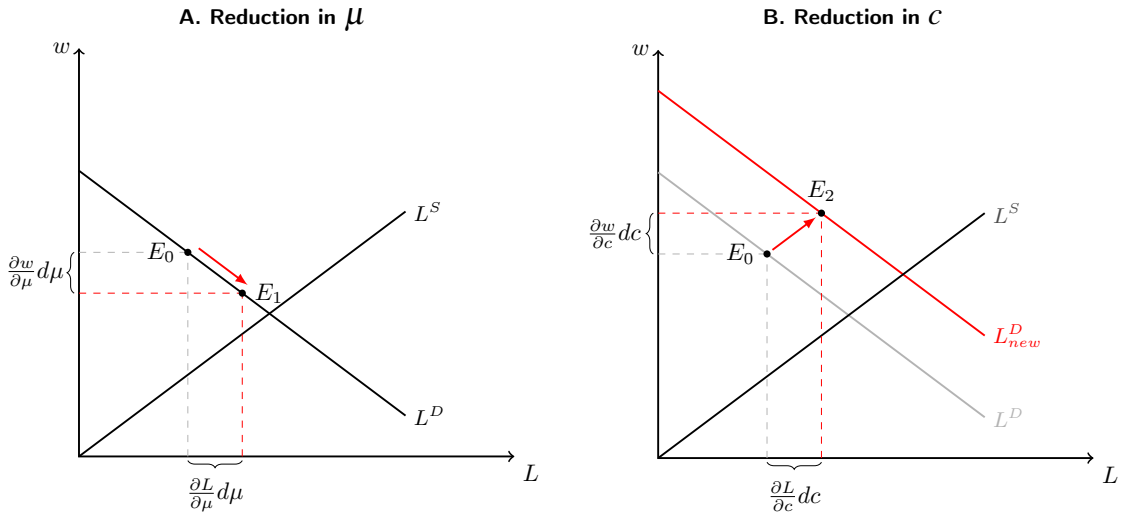
where L^S denotes the market-level labor supply of security guards as a function of wage w. Since labor demand is downward sloping and the union wage is elevated above the competitive wage, equilibrium employment is depressed below the competitive level. Together, equations (6), (7), and (8) fully describe the determination of w_c, w, and L in our model.

Outsourcing legalization may have two effects in this representative firm model. The first is that outsourcing may reduce worker wage bargaining power by changing μ (i.e., strip workers of rents). The second is that outsourcing may reduce non-compensation cost c (i.e., improve cost efficiency). These two effects have different equilibrium consequences. A decline in worker bargaining power leads to a movement along the labor demand curve, so it increases employment but reduces wages. By contrast, a reduction in transactions costs effectively shifts out the labor demand curve, and therefore raises both wages and employment.

Figure 10 illustrates the effects of reducing the wage markup μ and reducing transactions cost

[34] See Kaufman (2004) for a review of union bargaining models. Also see Oswald (1985); Farber (1986).

Figure 10: Equilibrium effects in a right-to-manage union bargaining model



Note: Panel A and B plot the effects of reduced wage markup μ and reduced transactions cost c on equilibrium wage and employment in the right-to-manage union bargaining model.

c in separate panels. In Panel A, a decline in μ lowers the wage, but increases employment. In Panel B, a reduction in transactions cost raises the competitive wage, the union wage, as well as employment, since labor demanded by the firm increases at every wage level w. Figure D.1 shows the combined effect.

What is the interpretation of c and μ in our model? We think of μ as capturing the degree to which worker compensation is raised above the competitive level. The literature offers three reasons that outsourcing may reduce worker wages (Weil 2014; Goldschmidt and Schmieder 2017). First, outsourcing may reduce union bargaining power by allowing firms to circumvent the requirements of collective bargaining agreements. Second, outsourcing may weaken within-firm fairness norms by moving workers outside the boundary of the firm. Third, outsourcing may eliminate the need to pay efficiency wages due to improved monitoring technologies. We interpret a reduction in μ as potentially resulting from any of these effects.

We think of c as representing any non-compensation organizational costs necessary for employing a worker, such as administration, training, scheduling, hiring, or legal costs. The literature offers four reasons that transactions costs are lower when firms outsource. First, external providers may perform specialized tasks more cheaply or more effectively due to accumulated skills and economies of scale (Abraham and Taylor 1996). Second, outsourcing may also enable firms to

better respond to short-term fluctuations such as worker absences through access to a larger pool of substitute workers at the contract firm (Houseman 2001). Third, outsourcing allows firms to "contract around" employment protection provisions established in labor regulations, by allowing firms to reassign workers across firms with lowered firing costs (Lee 1996; Segal and Sullivan 1997; Autor 2003). Fourth, outsourcing may allow firm to pare back bloated management structures and focus on their "core competencies" (Prahalad and Hamel 1990). In addition, outsourcing legalization may reduce legal penalties that firms that outsource despite legal restriction may incur. These costs would also be captured by c.

7.2 Identification

Using the above right-to-manage model, we can use first-order approximations to identify changes in transactions cost and wage markup (dc, $d\mu$) from changes in employment and average wage (dL, dw). Specifically, suppose that all primitives in our model are held constant except for L, w, c and μ. We can thus write

$$\begin{aligned} dw &= \frac{\partial w}{\partial \mu} d\mu + \frac{\partial w}{\partial c} dc \\ dL &= \frac{\partial L}{\partial \mu} d\mu + \frac{\partial w}{\partial c} dc. \end{aligned}$$

By differentiating equations (6), (7), and (8), we obtain expressions for each of the above partial derivatives in terms of structural parameters. The above two equations can then be rewritten as

$$\begin{aligned} \frac{dw}{w} &= \frac{d\mu}{\mu} + \mu\phi \frac{dc}{w} \\ \frac{dL}{L} &= \varepsilon_D \frac{d\mu}{\mu} + \varepsilon_D \mu \left(1 - \mu\phi\right) \frac{dc}{w}, \end{aligned}$$

where $\phi = \varepsilon_D / \left(\varepsilon_D - \varepsilon_S\right)$ denotes the pass-through of transactions cost to the competitive wage, ε_D is the wage elasticity of occupational labor demand, and ε_S is the wage elasticity of occupational labor supply. Provided the necessary structural parameters $(\varepsilon_D, \varepsilon_S, \mu)$ and observed changes in employment and wage (dL, dw), this system of two linear equations with two unknowns (dc, $d\mu$) can be easily solved by algebraic manipulation.

We use observed changes in employment and wage (dL, dw) estimated using entropy-balancing weights without regression adjustment in Table 6 Column (1), which are the most conservative. We use values that previous research has considered plausible for the unknown structural parameters. For the labor demand elasticity ε_D, we consult the survey of Hamermesh (1996), which reports estimates ranging from -0.5 to -2. Since a more negative ε_D implies a smaller reduction in transactions cost, we follow Kleiner and Soltas (2019) and made the conservative choice to set $\varepsilon_D = -3$. Following estimates in Cortes and Gallipoli (2017), Traiberman (2019), and Hsieh et al. (2019) of the elasticity of substitution across occupations, we set the labor supply elasticity to $\varepsilon_S = 3$. We set the wage markup to be $\mu = 1.1$, since our reduced-form estimates shows that incumbent wages fall by roughly 10 percent after occupational layoffs.[35] The first five rows of Table 7 summarizes our chosen parameters. As we show below, our results are not very sensitive to alternative choices of parameters.

7.3 Parameter Estimates

The last two rows of Table 7 reports the estimated change in transactions cost and wage markup of the representative firm due to outsourcing legalization. The estimates show that outsourcing legalization reduced firm transactions costs of the representative firm by the equivalent of 5.1 percent of the worker's initial wage. By contrast, outsourcing only reduced the market-level wage markup by 0.3 p.p.. This reduction is statistically indistinguishable from zero.

These parameter estimates are intuitive given our reduced-form results. Since labor supply is assumed to be upward sloping, a pure reduction in transactions cost should increase both wages and employment. A pure reduction in the wage markup would reduce wages (and increase employment). Because the estimated wage and employment effects are both positive, the efficiency-enhancing effect of outsourcing must have been large relative its rent-stripping effect.

The small effect of outsourcing on the market-level wage markup may seem surprising in light of our earlier finding that incumbent guards experiencing occupational layoffs saw large wage reductions. For these two findings to be consistent, outsourcing legalization must have had a negligible effect on the wage markup of security guards that did not experience occupational layoffs. To

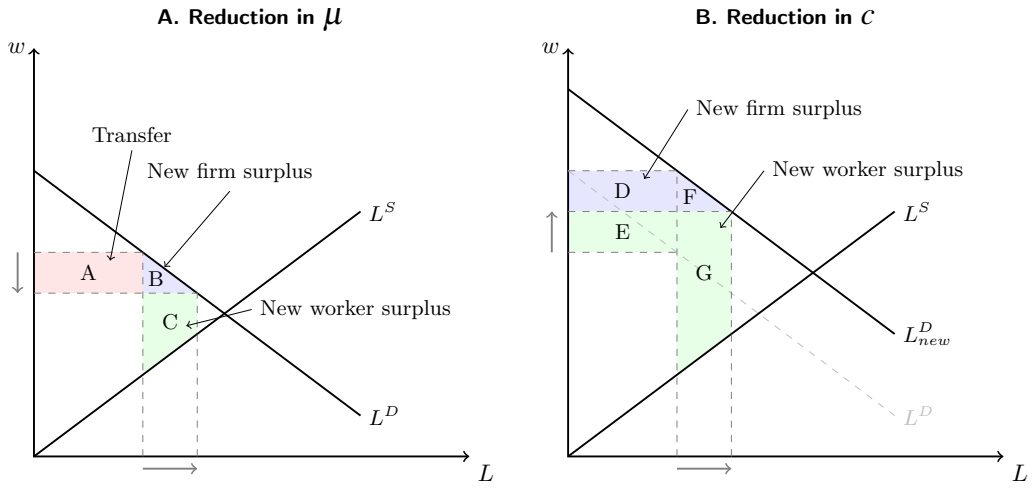
[35] We also consult a large literature on the union wage effect, which estimates a 10-25 percent union wage effect (Blanchflower and Bryson 2004)

see this, recall from our reduced-form estimates that total employment increased by 8.5 log points (or 8.9 percent), while the contract-firm share of employment rose by 6.2 percentage points. The decline in direct employment therefore was therefore roughly $108.9\% \times 6.2\% = 6.8\%$ of initial total guard employment. From our worker-level analysis, we found that less than 20 percent of incumbents remained security guards after occupational layoffs (see Appendix Figure B.5), and that these workers experienced an approximately 15% decline in wages (see Appendix Figure B.6). If we were to attribute this decline in wage entirely to a decline in the wage markup for those incumbents, and we assume the wage markup remained at initial level for all other remaining and entering security guards, then the employment-weighted wage markup fell by roughly $6.8\% \times 20\% \times 15\% = 0.2\%$, a number close to our estimated change in the market-level wage markup.

The small effect of outsourcing on the market-level wage markup is also consistent with the relatively small outsourcing wage differential for security guards in Brazil. Li and Wong (2021) document that contract-firm guards in Brazil earn only 3.6 log points less than direct hires after controlling for both observed and unobserved worker heterogeneity. They also find that the average firm-specific wage premia of contract-firm guards is very similar to that of direct-hire guards. This suggests the negative wage effects from the overall loss of firm-specific wage premia are smaller for Brazilian security guards than for low-wage occupations in high-income countries, where the outsourcing wage differential been shown to be much larger and attributable to a loss of firm-specific wage premia (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017).

Even though our null result on the wage markup differs from what might be expected given recent studies in other countries, our finding that outsourcing reduces transactions cost is likely to generalize to other occupations and settings. As mentioned above, the primary reasons that outsourcing generates productivity gains include improved worker training, reduced management costs, increased flexibility, and reduced litigation costs, all of which arise from economies of scale at the contract firm. These reasons are not specific to security guards in Brazil, so our finding of substantial efficiency gains due to outsourcing is likely general.

Figure 11: Welfare effects in a right-to-manage union bargaining model



Note: Panel A and B plots the effects of reduced wage markup μ and reduced transactions cost c on worker and firm surpluses.

7.4 Welfare Estimates

Figure 11 illustrates the welfare effects of outsourcing legalization in the right-to-manage model. Panel A plots the welfare effects of reduced worker power, which can be decomposed into three components. Area A denotes the transfer of worker surplus to the firm due to wage reduction for existing employment. Area B denotes new firm surplus due to new employment. Area C denotes new worker surplus due to new employment. The sum of areas A and B is the total change in firm surplus. The difference between areas C and A is the total change in worker surplus.

Panel B plots the welfare effects of reduced transactions cost, which can be decomposed into four components. Area D denotes the pass-through of reduced transactions cost to firms for existing employment. Area E denotes the analogous pass-through to workers. Area F denotes new firm surplus due to increased employment. Area G denotes new worker surplus due to increased employment. The sum of areas D and F is the total change in firm surplus. The sum of areas E and G is the total change in worker surplus.

Armed with the previously estimated effects of outsourcing on wage markup and transactions cost, we can now compute each of these areas described above using first-order approximations. The necessary calculations are derived in Appendix D.1.

Table 8 reports our welfare estimates. Because we do not impose functional form assumptions on the labor demand and labor supply curves and instead take first-order approximations, the levels

of the firm and worker surpluses are not identified in our parametric model, only the changes are. To normalize units, we report the implied changes in firm and worker surpluses as a percentage of the initial total security guard wagebill. We find that firm surplus increased by 2.7 percent of the initial security guard wagebill, while worker surplus increased by 4.1 percent of the initial security guard wagebill. Both of these increases are almost entirely attributable to reduced transactions cost.

To compute total surplus, we can either assume that transactions cost are dissipated as pure waste or redistributed in the economy. Transactions costs would be dissipated if, for example, they represent pure productivity increases that arise from economies of scale or specialized capabilities at the contract firm. In this case, we simply add up the change in worker and firm surpluses to compute the change in total surplus, which is estimated to be 6.7 percent of the initial security guard wagebill. Alternatively, the transactions cost can be thought of as a redistributive tax that is transferred to outside parties (such as to labor lawyers or managers). A reduction in transactions cost would therefore harm those parties. In this case, we would only include the change in dead-weight loss when computing the change in total surplus, and our estimates would instead imply that outsourcing increased total surplus by 1.6 percent of the initial security guard wagebill.[36]

7.5 Cost-Benefit Analysis

We now compare the increase in total surplus due to outsourcing legalization with the total earning losses of incumbent workers whose firms decided to outsource. A back-of-the-envelopment calculation suffices. As argued above, our reduced-form estimates suggest that direct employment fell by roughly 6.8% of initial total guard employment. The worker-level analysis suggests that firm outsourcing decisions reduced the earnings of incumbent workers by 1.24 years of their pre-outsourcing earnings, if we assume that unobserved workers earn the minimum wage. Multiplying these two numbers yields a total earnings losses for incumbent workers of roughly equal to $1.24 \times 6.8\% = 8.4\%$ of the initial security guard wagebill.

If the transactions cost is entirely dissipated as pure waste, then the total earnings losses is

[36] Importantly, our model assumes away the existence of non-wage benefits for workers, since data on non-wage benefits is not available. Non-wage benefits can be thought of a transaction cost for firms that is redistributed to workers. If non-wage benefits fall as a consequence of outsourcing legalization, then the model would overestimate the increase in worker surplus.

equivalent to $8.4\%/6.7\% = 1.3$ years of the annual long-run increase in total surplus from outsourcing. If the transactions cost is instead entirely redistributed, then the total earnings losses is equivalent to $8.4\%/1.6\% = 5.2$ years of the annual long-run increase in total surplus from outsourcing. Put differently, if incumbent workers were fully compensated for the present-value of their earnings losses and the long-run welfare gains of outsourcing were immediately realized, then the economic benefit of outsourcing would exceed its cost after 1.3 to 5.2 years.

7.6 Robustness: Alternative Parameter Choice

Table 9 investigates the sensitivity of our model estimates to alternative choices of elasticities and markup. We report the estimated effects on transactions cost, wage markup, firm surplus and worker surplus for $\varepsilon_S \in \{1,3,5\}$, $\varepsilon_D \in \{-1,-3,-5\}$, and $\mu \in \{1.05,1.1,1.2\}$. The results are intuitive. A less elastic labor supply implies a slightly larger reduction in the wage markup, but does not significantly alter the estimated changes in transactions cost or firm and worker surpluses. By contrast, a less elastic labor demand implies in a larger reduction in transactions cost as well as a larger increase in firm surplus, but hardly alters the estimated changes in worker surplus. A higher markup increases the pass-through of transactions costs to equilibrium wages, and hence implies a smaller increase in transactions cost and a large increase in worker surplus. Since the range of markups we consider plausible is small in relative terms, the estimated effects are largely insensitive to the alternative wage markups.

Across the range of alternative parameters that we consider, the welfare implications are largely similar: outsourcing legalization reduced transactions cost, had little effect on the wage markup, and increased both firm and worker surplus. The welfare estimates are insensitive to parameter choices because our reduced-form findings impose discipline on our parametric model. A substantial reduction in transactions cost raises both wages and employment, and is consistent with the findings. However, a substantial reduction in worker bargaining power lowers wages and increases employment, and therefore cannot rationalize the findings.

7.7 Robustness: Alternative Models

Finally, we compute the welfare effects of outsourcing legalization under two alternative models.

Strongly Efficient Bargaining. Table D.2 presents welfare estimates from a model in which union bargaining is "strongly efficient" (à la McDonald and Solow 1981). In this model, a risk-neutral firm and a risk-neutral union negotiate simultaneously over wages and employment in order to maximize the joint surplus of their economic activity. As such, there is no employment distortion from elevated union wages. We consider this framework to be a less realistic model of the Brazilian labor market, where collective bargaining agreements typically do not determine the firm's employment level. We include this alternative model mainly to test whether our results are sensitive to modeling assumptions.

The results are very similar to our baseline right-to-manage model. The estimated reductions in transactions cost and wage markup are both slightly larger under efficient bargaining than under right-to-manage bargaining. This result is intuitive, because any increase in employment would be solely attributed to reduced transactions cost in the strongly efficient bargaining model. This in turn implies that the wage markup must have fallen more to offset the pass-through of reduced transactions cost to wages. The estimated changes in firm and worker surplus are very similar across the two models.

Heterogeneous Firms and Endogenous Choice of Contractual Arrangements. Appendix D.3 presents a model wherein firms endogenously choose both the quantity of directly employed and outsourced labor, which are perfect substitutes in production. Firms face heterogeneous wage markups and transactions costs, which differ when they use outsourced workers instead of direct employees in production. Legalization reduces the legal penalty of outsourcing, and thus induces some firms to switch from direct to outsourced employment, and thereby endogenously alters the firms' wage markups and transactions costs. This richer model generates predictions that match not only our market-level wage and employment effects, but also our worker-level findings that outsourcing legalization led to a wave of layoffs and resulted in wage reductions related to loss of firm wage premia for laid-off incumbent workers.

In this heterogeneous firm model, the welfare effects of outsourcing legalization can be approximated from our market-level wage and employment estimates under two additional assumptions. Namely, we assume that each firm's labor demand is linear with identical slopes in order to aggregate across firms and that both wage markups and transactions costs are additive and

thus summable. Table D.2 shows the estimated sum of changes in transactions costs, the average markup, and the estimated changes in firm and worker surplus in this model. The results are once again highly similar as our baseline right-to-manage model. A slight difference is that change in worker surplus is smaller in this heterogeneous firm model, though still positive, since the firm-specific wage markups are assumed to be linear rather than multiplicative, so the pass-through from reduced transactions cost to worker wages is smaller.

8 Conclusion

The rise of domestic outsourcing is widely believed to have fundamentally altered the labor market to the detriment of workers (Weil 2014). In this paper, we leverage Brazil's unexpected legalization of domestic outsourcing in 1993 to estimate the overall effects of outsourcing on workers and labor markets. We focus on security guards, the only major occupation to experience a large increase in outsourcing following outsourcing legalization. To our knowledge, this paper is the first to use an exogenous rise in outsourcing to quantify the market-level consequences of domestic outsourcing.

Our results reveal that domestic outsourcing not only redistributed economic rents, as emphasized by recent studies, but also reallocated jobs and generated aggregate productivity gains. A wave of occupational layoffs followed outsourcing legalization. These layoffs temporarily displaced incumbent workers from formal employment, caused them to move to firms with lower firm wage premia, and persistently reduced their wages. At the same time, the employment of younger workers dramatically increased. Total employment of security guards rose by 8-15 percent, while their market-level wages rose by 2-4 percent. Interpreted through a parametric framework, these estimates imply that outsourcing legalization generated both substantial efficiency gains and positive welfare effects for some workers. If laid-off incumbent workers were fully compensated for their earnings losses, social breakeven would be achieved in one to five years.

Although our paper emphasizes the market-level wage and employment consequences of domestic outsourcing, the rise of domestic outsourcing is likely to have complex and multifaceted effects on the labor market. Two leading hypotheses for why outsourcing generates productivity gains are increased specialization of workers and firms and greater flexibility of labor allocation (e.g., Abraham and Taylor 1996). The rise of outsourcing could therefore affect not only equilib-

rium employment, employment composition, and wage levels, but also worker skill accumulation, job security, career advancement, and social mobility. We leave these important topics for future study.

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Table 1: Effects of occupational layoffs on incumbent workers

	Years since occupational layoff		
	Y0	Y1	Y5
Formally employed	-0.493	-0.159	-0.025
	(0.013)	(0.010)	(0.010)
Formally employed in same occupation	-0.761	-0.399	-0.117
	(0.007)	(0.011)	(0.010)
Formally employed in contract firm	0.002	0.077	0.041
	(0.001)	(0.005)	(0.005)
Monthly wage (relative to base year)	-0.064	-0.136	-0.090
	(0.012)	(0.014)	(0.019)
Log monthly wage	-0.078	-0.185	-0.112
	(0.010)	(0.011)	(0.017)
Firm wage effect	-0.032	-0.080	-0.052
	(0.005)	(0.006)	(0.011)
As fraction of wage losses	0.415	0.433	0.462

Notes: Entries give estimated effects of occupational layoff on the indicated outcome in Y0, Y1 and Y5 following the event. While employment outcomes are estimated on a balanced sample, wage effects are computed conditional on observation. Wage losses due to firm effects shown both as log points and as a percentage of wage losses (e.g., -.032/-0.78=0.415 in Y0). Standard errors are provided in parentheses.

Table 2: Present-value of earnings loss due to occupational layoffs

	PDV of earnings losses (as a multiple of annual pre-event earnings)
Assuming zero earnings for unobserved workers	1.40
Assuming unobserved workers earn half min wage	1.32
Assuming unobserved workers earn min wage	1.24
Using monthly wage estimates from observed workers	1.06

Notes: Entries provide estimated present-discounted values of earnings losses due to occupational layoffs, using a real interest rate of 5 percent, and summing the discounted losses over a 20-year period starting with the year of the occupational layoff. The four rows use alternative assumptions about earnings if the worker is unobserved.

Table 3: Establishment-level predictors of outsourcing decisions, 1993-1998

	Dependent variable: Outsourcing decision			
Log(estab size)	0.004			
	(0.005)			
Log(estab mean wage)		0.057***		
		(0.008)		
Log(estab mean guard wage)			0.018*	
			(0.009)	
AKM firm FE				0.088***
				(0.016)
N	7682	7682	7682	7682
R^2	0.04	0.05	0.04	0.04

Note: Sample includes establishments with at least 50 employees and at least three security guards in 1993. We exclude establishments whose total non-security guard headcount declines by more than 10 percent by 1998. The dependent variable takes indicates whether the number of security guards at the establishmnt dropped by more than two thirds in 1998 (and dropped to zero if the initial number of guards is fewer than six). We include controls for log number of security guards at the establishment in 1993, as well as microregion fixed effects.

Table 4: Classification of Regional Labor Courts

TRT Number	Jurisdiction	Contract-firm share (1992)	Change in contract-firm share (1992-9)	Chow test p-value	Classification
4	Rio Grande do Sul state	0.36	0.19	0.00	Restrictive
9	Paraná state	0.31	0.13	0.00	Restrictive
12	Santa Catarina state	0.36	0.20	0.00	Restrictive
15	São Paulo state excl. 2nd Region	0.30	0.23	0.00	Restrictive
1	Rio de Janeiro state	0.45	0.12	0.32	Permissive
2	São Paulo and Santos metro areas	0.36	0.20	0.29	Permissive
3	Minas Gerais state	0.32	0.14	0.02	Permissive
5	Bahia state	0.41	0.22	0.09	Permissive
6	Pernambuco state	0.41	0.06	0.74	Permissive
7	Ceará state	0.42	0.07	0.04	Permissive
8	Pará and Amapá states	0.50	0.09	0.07	Permissive
10	Distrito Federal and Tocantins state	0.51	0.09	0.01	Permissive
11	Amazonas and Roraima states	0.41	0.23	0.89	Permissive
13	Paraíba state	0.35	0.08	0.00	Permissive
14	Rondônia and Acre states	0.54	-0.03	0.00	Permissive
16	Maranhão state	0.49	0.03	0.34	Permissive
17	Espírito Santo state	0.42	0.02	0.02	Permissive
18	Goiás state	0.35	0.15	0.48	Permissive
19	Alagoas state	0.39	0.10	0.29	Permissive
20	Sergipe state	0.33	0.12	0.00	Permissive
21	Rio Grande do Norte state	0.40	0.04	0.21	Permissive
22	Piauí state	0.35	-0.01	0.02	Permissive
23	Mato Grosso state	0.36	0.09	0.09	Permissive
24	Mato Grosso do Sul state	0.43	-0.07	0.52	Permissive

Note: Table shows each of 24 regional courts, our classification of their stance on outsourcing, the share of guards in contract firms in their jurisdiction in 1992, and the change in the contract-firm share of guards in the following seven years.

Table 5: Included occupations

Occupation	Contract-firm share	Mean log wage	Mean age	Mean schooling	Male	National employment
Security guards	0.37	7.05	41	5	0.99	458134
Technicians	0.03	7.92	33	10	0.87	370990
Electricians and electronics workers	0.03	7.51	33	6	0.96	254057
Cashiers and tellers	0.03	7.30	29	10	0.45	459240
Machine installers and mechanics	0.02	7.26	32	6	0.99	367353
Office administration	0.05	7.22	29	10	0.56	1849524
Drivers, sailers, conductors	0.02	7.22	37	5	1.00	630666
Secretaries and typists	0.05	6.88	29	11	0.15	163004
Food and beverage processing workers	0.00	6.80	31	5	0.80	294391
Other manual or uncommon occupations	0.05	6.70	32	5	0.83	1398107
Salesmen	0.01	6.59	28	8	0.60	781809
Cooks, waiters, bartenders	0.03	6.57	35	5	0.33	359621
Cleaners (excluded)	0.30	6.57	37	4	0.42	715719
National	0.06	7.13	33	8	0.74	14447147

Note: Table lists all 2-digit CBO occupations included in our main triple-difference specification and their mean characteristics in 1992.

Table 6: Effect of outsourcing legalization on market-level outcomes

	(1)	(2)	(3)	(4)	(5)
A. Contract-firm share					
Short run effect	0.012	0.043*	0.033**	0.013*	0.037*
	(0.014)	(0.022)	(0.012)	(0.007)	(0.020)
	[0.623]	[0.298]	[0.200]	[0.626]	[0.304]
Long run effect	0.062**	0.105***	0.081***	0.071***	0.100***
	(0.029)	(0.031)	(0.022)	(0.009)	(0.025)
	[0.106]	[0.157]	[0.068]	[0.083]	[0.165]
B. Mean age					
Short run effect	-0.767**	-0.684**	-0.706**	-0.815**	-0.680**
	(0.305)	(0.258)	(0.310)	(0.355)	(0.292)
	[0.128]	[0.146]	[0.138]	[0.130]	[0.251]
Long run effect	-1.512***	-1.948***	-1.675***	-1.705**	-1.887***
	(0.460)	(0.573)	(0.491)	(0.616)	(0.578)
	[0.114]	[0.125]	[0.068]	[0.098]	[0.161]
C. Log employment					
Short run effect	0.063*	0.075	0.071*	0.064	0.071
	(0.036)	(0.064)	(0.037)	(0.039)	(0.056)
	[0.423]	[0.363]	[0.297]	[0.431]	[0.396]
Long run effect	0.085***	0.144**	0.119**	0.104***	0.151**
	(0.029)	(0.066)	(0.049)	(0.035)	(0.056)
	[0.459]	[0.287]	[0.265]	[0.371]	[0.269]
D. Demographic-adjusted wage					
Short run effect	0.023	0.023	0.023	0.030	0.028
	(0.020)	(0.022)	(0.018)	(0.018)	(0.017)
	[0.497]	[0.557]	[0.479]	[0.337]	[0.401]
Long run effect	0.025*	0.020	0.017	0.047***	0.039**
	(0.014)	(0.020)	(0.019)	(0.016)	(0.015)
	[0.395]	[0.570]	[0.497]	[0.157]	[0.263]
1990 mreg features X occ X yr			X	X	X
	Entropy balancing weights	Propensity score weights	Uniform weights	Entropy balancing weights	Propensity score weights
Observations	57456	57456	57456	57456	57456

Notes: Estimates are from our main triple-difference specification. We report pooled coefficients for the short-run post-legalization (1994-1997) and long-run post-legalization (1998-2002). 1990 microregion features include log employment, unemployment rate, employment share in tradeable industries, import tariff reduction exposure, log employment of importers, and homicide rate (per 100K population). These same variable are used to compute propensity score and entropy balancing weights. Standard errors in parentheses are clustered at the TRT regional court level, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. All specifications include microregion-occupation linear trends. Randomization inference p-values using 1000 draws are reported in brackets.

Table 7: Effects of outsourcing legalization on labor market structure

	Estimate	S.E.
Pooled DDD estimates		
Change in guard wage (dw/w_0)	2.5%	(1.4%)
Change in guard employment (dL/L_0)	8.5%	(2.9%)
Assumed parameters		
Elasticity of occupational labor supply (ε_S)	3	
Elasticity of occupational labor demand (ε_D)	-3	
Initial wage markup (μ)	1.1	
Implied change in labor market structure		
Change in transactions cost (dc/w_0)	-5.1%	(1.5%)
Change in wage markup ($d\mu$)	-0.3%	(1.0%)

Notes: The first two rows repeats estimates from Table 6 Column (1). The middle three rows show the assumed values of structural parameters. The final two rows show the implied changes in transaction cost and wage markup.

Table 8: Welfare effects of outsourcing legalization

	% of initial guard wagebill	
Change in firm surplus	2.7%	(1.0%)
Due to reduced transactions cost	2.4%	(0.7%)
Due to reduced markup	0.3%	(0.9%)
Change in worker surplus	4.1%	(1.5%)
Due to reduced transactions cost	4.2%	(1.3%)
Due to reduced markup	-0.1%	(0.4%)
Change in total surplus		
Assume transactions cost is dissipated	6.7%	(1.9%)
Assume transactions cost is redistributed	1.6%	(0.6%)

Notes: Table reports the changes in firm, worker, and total surplus due to outsourcing legalization as a percentage of the initial wagebill.

Table 9: Robustness to alternative parameter choices

	Change in transactions cost (dc/w0)	Change in wage markup (dμ)	Change as % of initial guard wagebill	
			Firm Surplus	Worker Surplus
Baseline	-5.1%	-0.3%	2.7%	4.1%
$\varepsilon_S = 1$	-5.3%	-2.0%	4.5%	3.7%
$\varepsilon_S = 5$	-5.1%	0.5%	2.5%	4.0%
$\varepsilon_D = -1$	-10.3%	-0.3%	8.1%	3.6%
$\varepsilon_D = -5$	-4.1%	-0.3%	1.9%	4.2%
$\mu = 1.05$	-5.2%	-0.2%	2.9%	3.4%
$\mu = 1.2$	-5.0%	-0.5%	2.9%	4.7%

Notes: Table reports the changes in transactions cost, wage markup, and firm and worker surplus due to outsourcing legalization.

Appendices

A Data Definitions

Microregion definition. We use the "microregion" definition of the Brazilian Statistical Agency (IBGE), which groups together economically integrated contiguous municipalities (counties) with similar geographic and productive characteristics (IBGE 2002), to define the boundaries of local labor markets. To ensure that we consistently define microregions over time, we combine microregions whose boundaries changed during our sample period, following Kovak (2013). This process leads to a set of 494 consistently identifiable microregions within the period 1985-2006.

Sample restrictions. Our worker sample includes all individual between the ages of 18 and 64 who were employed as of January 1 of the reference year. We omit those working in public administration and those without valid information on their industry of employment. For our market-level analysis, we further restrict our sample to only include local labor markets with at least 25 security guards and 25 cleaners in every year between 1985-2002, yielding an analysis sample of 266 local labor markets. This analysis sample covers 98.6 percent of all security guards.

Variable definitions. We use the establishment's geographic location (municipality) and industry, and worker-level information including gender, age, education (nine categories), occupation, average monthly wages, and separation reasons. We measure worker wages using their average monthly wages during employed months in the reference year. Reported wages are gross and include regular salary payments, holiday bonuses, tips, performance-based bonuses, commission, and profit-sharing agreements. Worker transition rates are computed by comparing labor market statuses (e.g. formal employment, occupation, and so on) as of January 1 in adjacent years. Separation rates are calculated by examining whether an existing employment contract as of January 1 of the reference year terminates within the year.

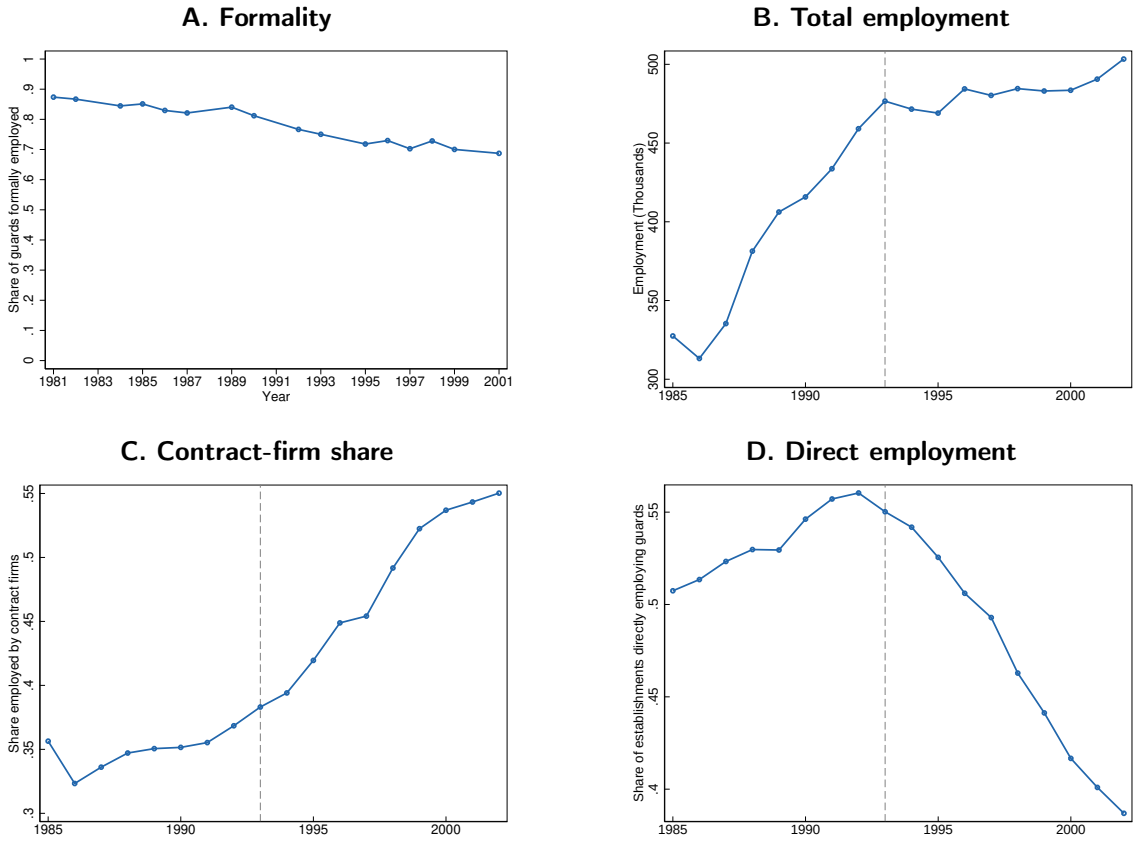
Measuring outsourcing using industry and occupation codes We use the 1994 *Código Brasileiro de Ocupações* (CBO) at the two-digit level to define occupations. We identify whether an establishment in the RAIS dataset is part of an contract firm based on the establishment's economic activity

code, which follows the *Classificação Nacional de Atividades Econômicas* (CNAE) system. We identified business service establishment as those with 1995 CNAE numbers 74608, 74160, 74500, 74705, and 74993.

Crime data. We use homicide rates available from the replication files of Dix-Carneiro et al. (2018).

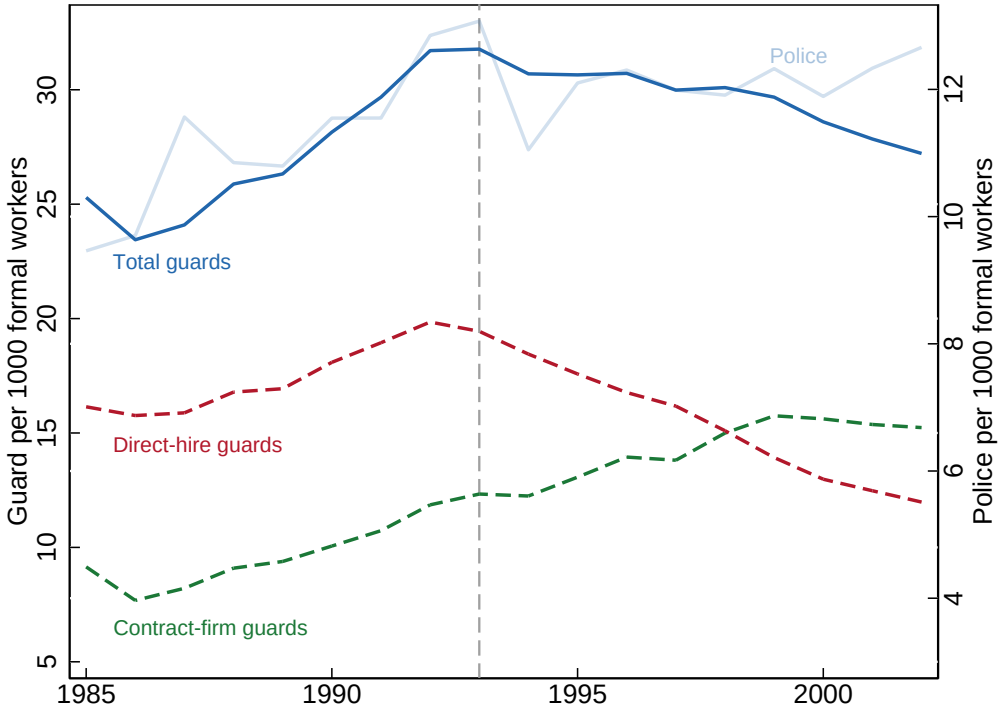
PNAD household survey. We compute the share of security guards that are formally employed using PNAD household survey.

Figure A.1: National trends, security guards



Note: Panel A plots the share of guards (aged 18-65) that are formally employed from PNAD. Panel B plots total number of formally employed private-sector security guards in Brazil (from RAIS). Panel C plots share of formally employed private-sector security guards at contract firms. Panel D plots share of establishments with ≥ 50 employees that directly employ at least one security guard.

Figure A.2: National employment trends, security guards



Note: Figure plots the total number of guards, the number of contract-firm guard, the number of direct-hire guards, and the number of police (aged 18-64) per 1000 formal-sector workers in Brazil over time.

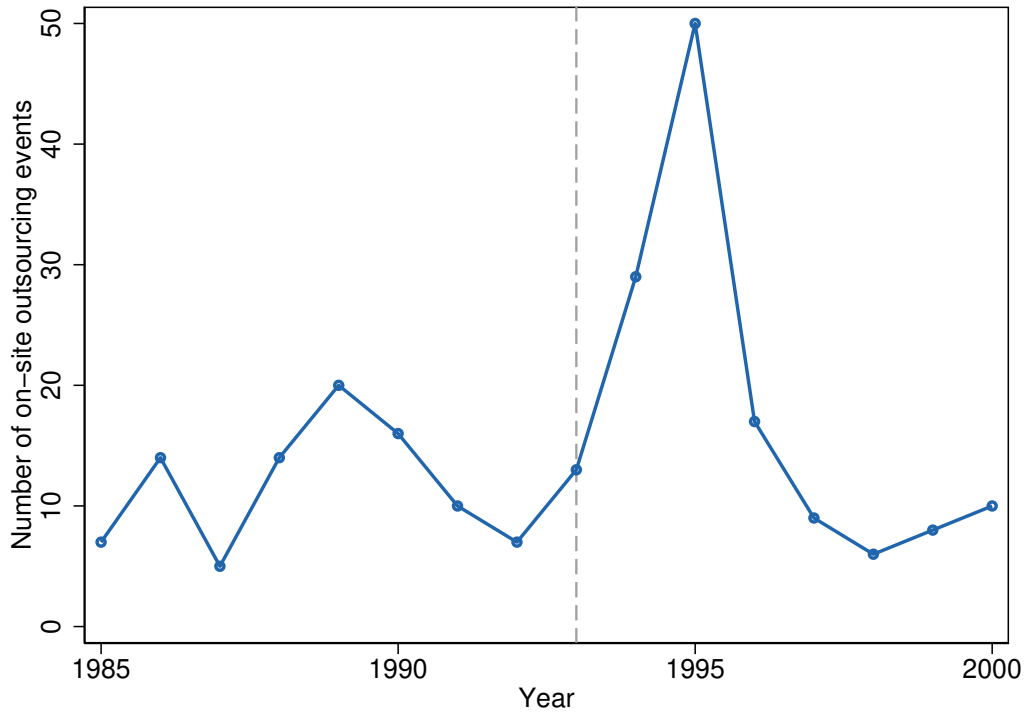
Table A.1: Summary statistics, security guards

	Direct hire			Contract-firm		
	1985-1993	1994-1996	1997-2002	1985-1983	1994-1996	1997-2002
Male	0.98	0.98	0.97	0.98	0.97	0.97
Age	40	40	40	34	34	35
Years of schooling	4.9	5.4	6.2	5.1	5.9	6.8
CLT urban indeterminate contract	0.98	0.95	0.96	0.99	0.98	0.99
Tenure	2.5	2.8	2.9	1.5	1.5	1.7
New hire (Tenure<1 years)	0.46	0.43	0.41	0.52	0.55	0.48
Real monthly earning (2017 $R)	1995	1753	1694	1481	1573	1665
	(1612)	(1476)	(1382)	(822)	(814)	(796)
	[1511]	[1321]	[1312]	[1285]	[1390]	[1525]
Contract hours		42.4	42.4		43.5	43.7
		[44]	[44]		[44]	[44]
Real wage (2017 $R)		43.5	41.8		37.1	38.4
		(55.9)	(49.3)		(34.6)	(23.2)
		[31]	[31]		[32]	[35]
Has multiple jobs	0.01	0.02	0.01	0.02	0.01	0.02
Employer size	885	692	568	1024	954	1729
	[146]	[83]	[59]	[586]	[460]	[478]
Number of guards at employer	67	66	54	822	781	808
	[8]	[5]	[4]	[490]	[422]	[432]
N	2220357	796814	1364634	1242514	596325	1506231

Notes: Sample includes all security guards aged 18-64 between 1985 and 2002. Standard deviations are presented in parentheses; medians are in brackets.

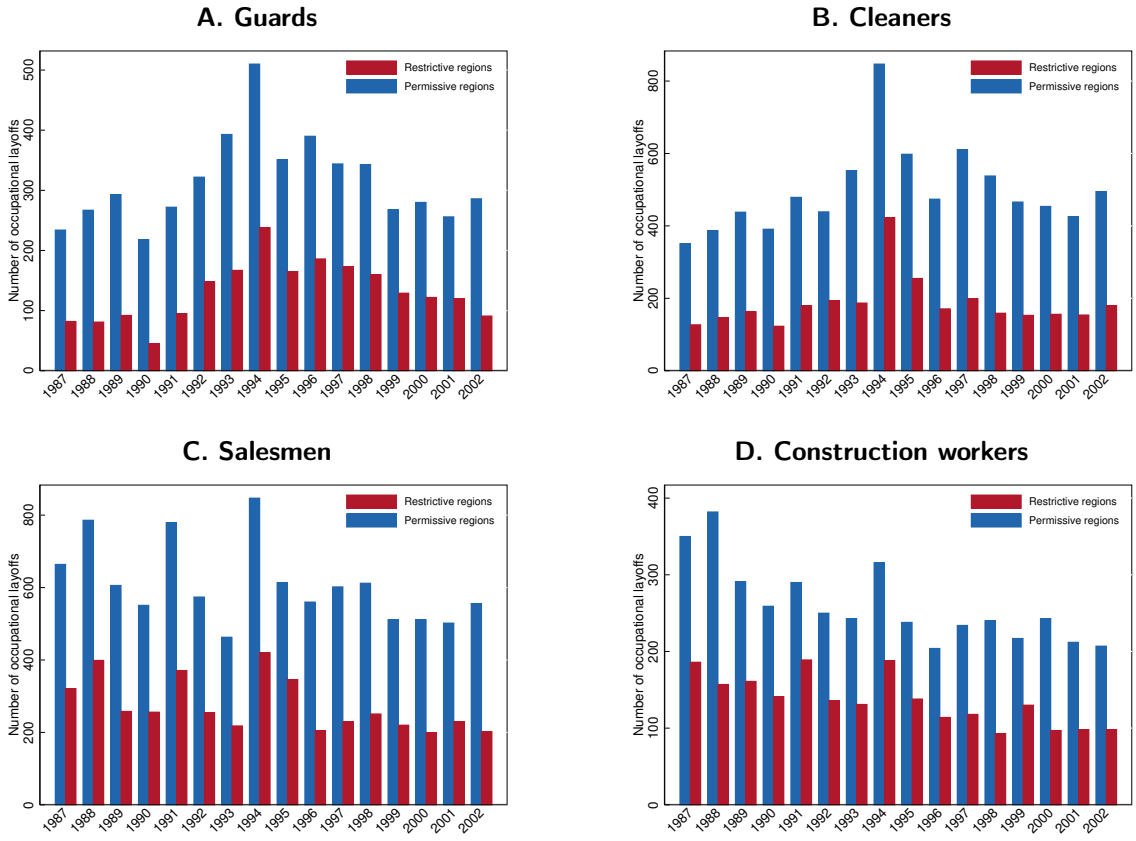
B Addendum: Effects of Outsourcing on Incumbent Workers

Figure B.1: Number of on-site outsourcing events



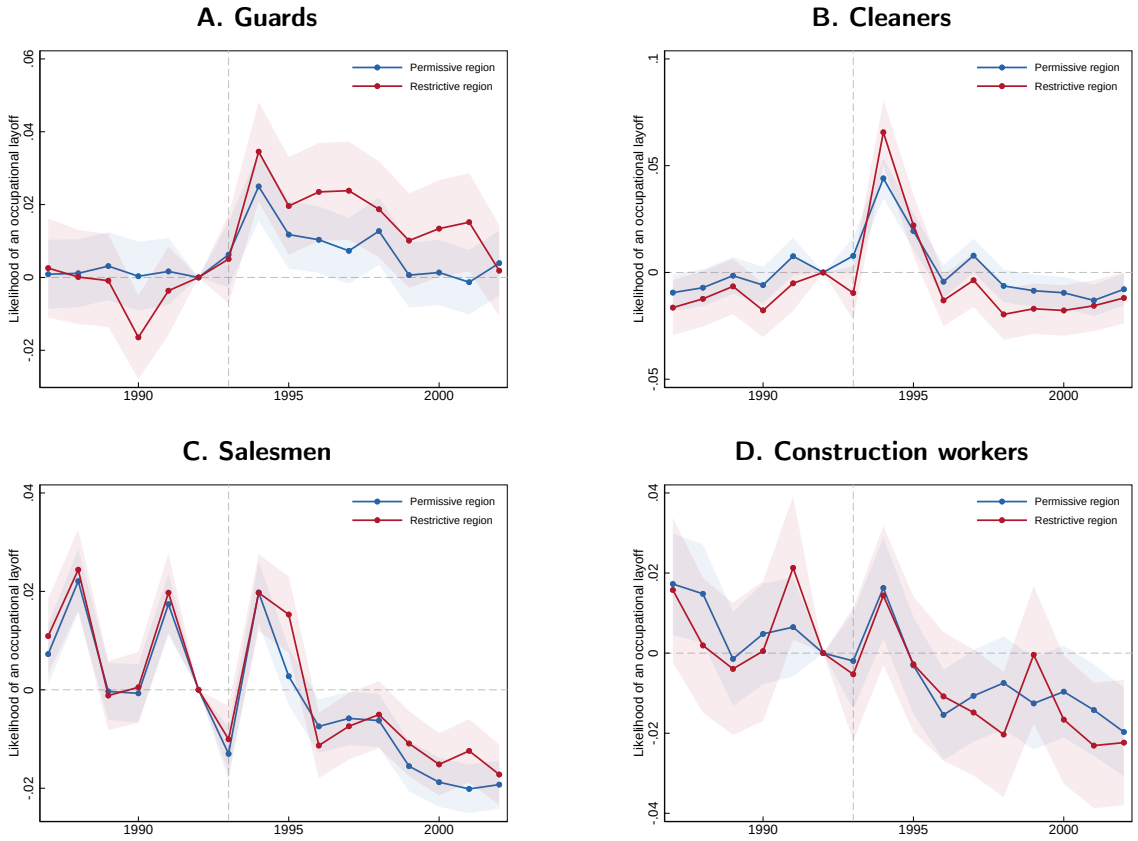
Note: Figure plots the number of on-site outsourcing events.

Figure B.2: Number of occupational layoffs, four occupations



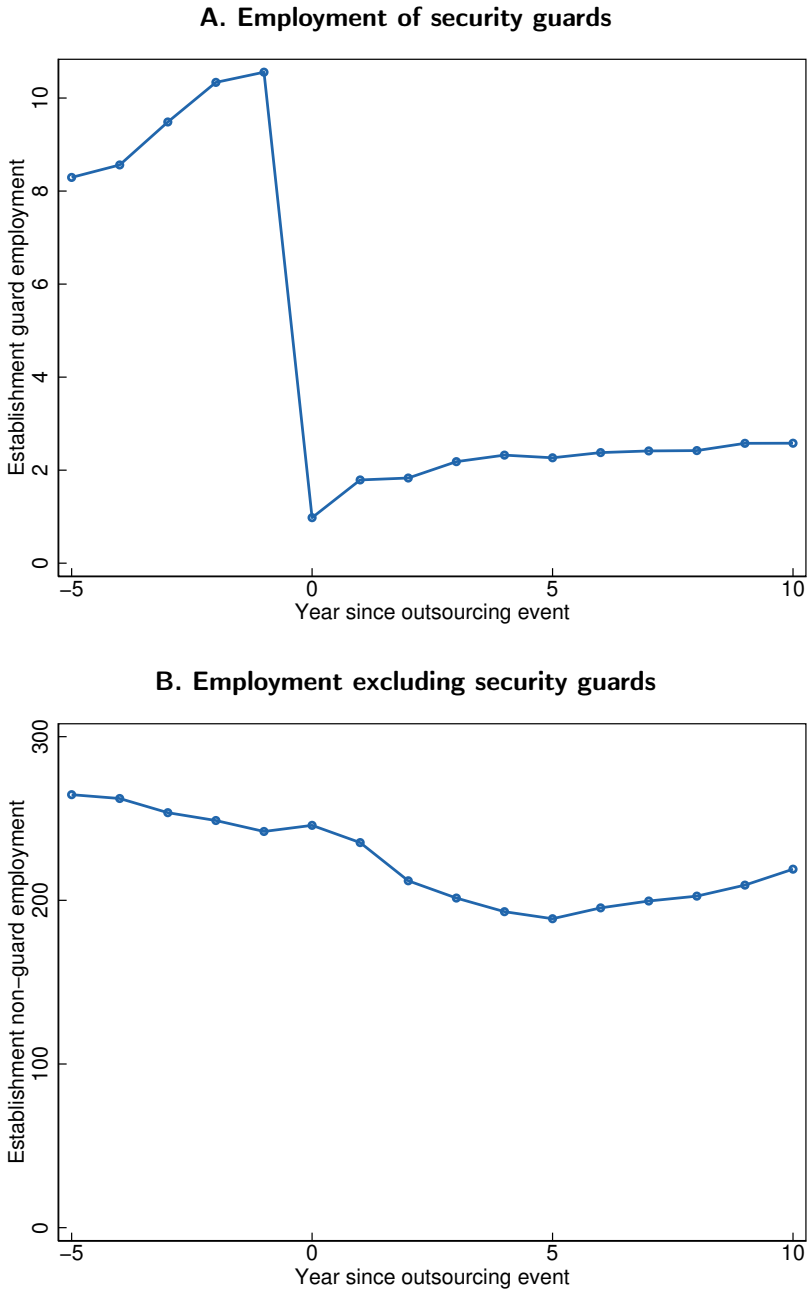
Note: Figure plots the number of occupational layoffs, separately for restrictive and permissive regions, for four occupations.

Figure B.3: Trend in likelihood of occupational layoffs, four occupations



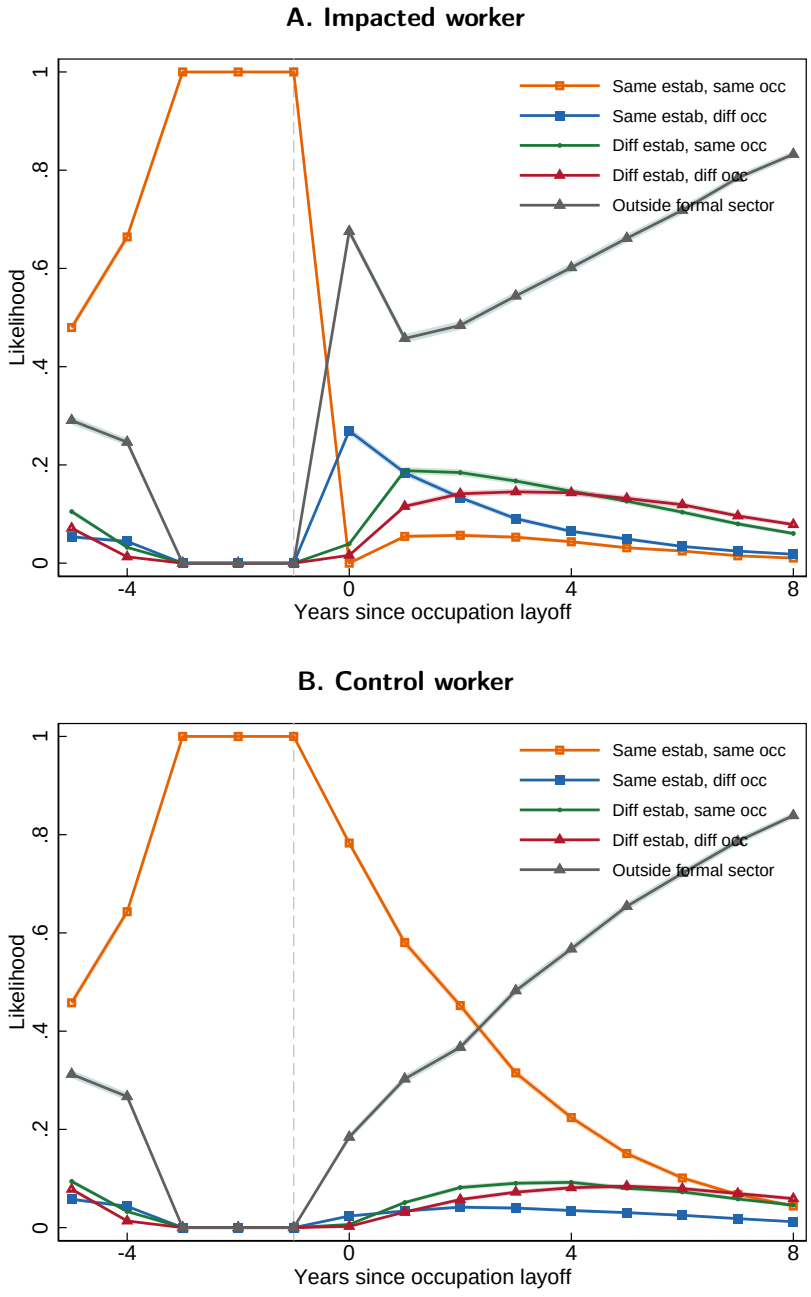
Notes: Each panel plots (for each occupation) coefficients from a linear probability model where we regress a dummy indicating the occurrence of an occupational layoff on separate year fixed effects for restrictive and permissive regions, relative to the omitted year of 1992, with controls for microregion fixed effects. Our sample includes all establishment-years where the establishment had at least 10 employees and 3 workers in the specified occupation and its remaining employment fell by less than 10 percent in the subsequent year. We exclude manufacturing establishments, because they are heavily affected by trade liberalization in the early 1990s.

Figure B.4: Establishment employment before and after occupational layoff



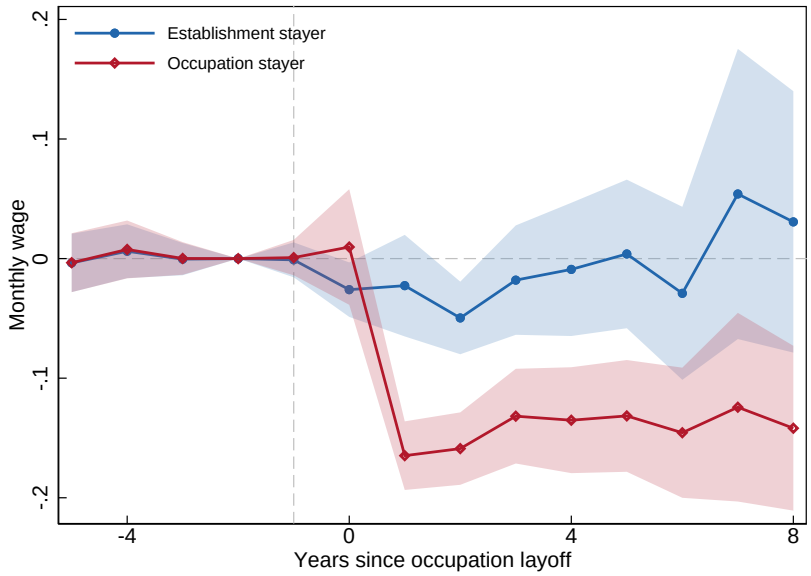
Note: Figure plots the number of security guards and non-security guard employees for the years before and after an occupational layoff.

Figure B.5: Employment status before and after occupational layoff



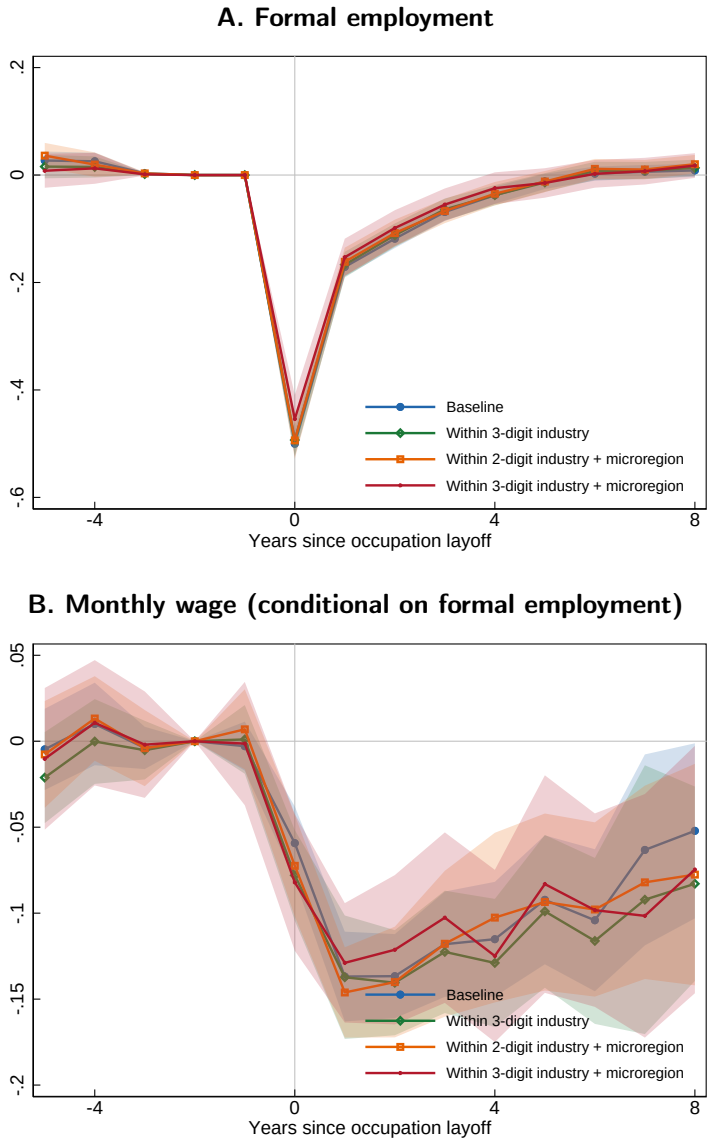
Note: Figure shows the raw likelihood of various employment statuses for workers affected by occupational layoffs and matched control workers.

Figure B.6: Effect of occupational layoffs on establishment and occupation stayers



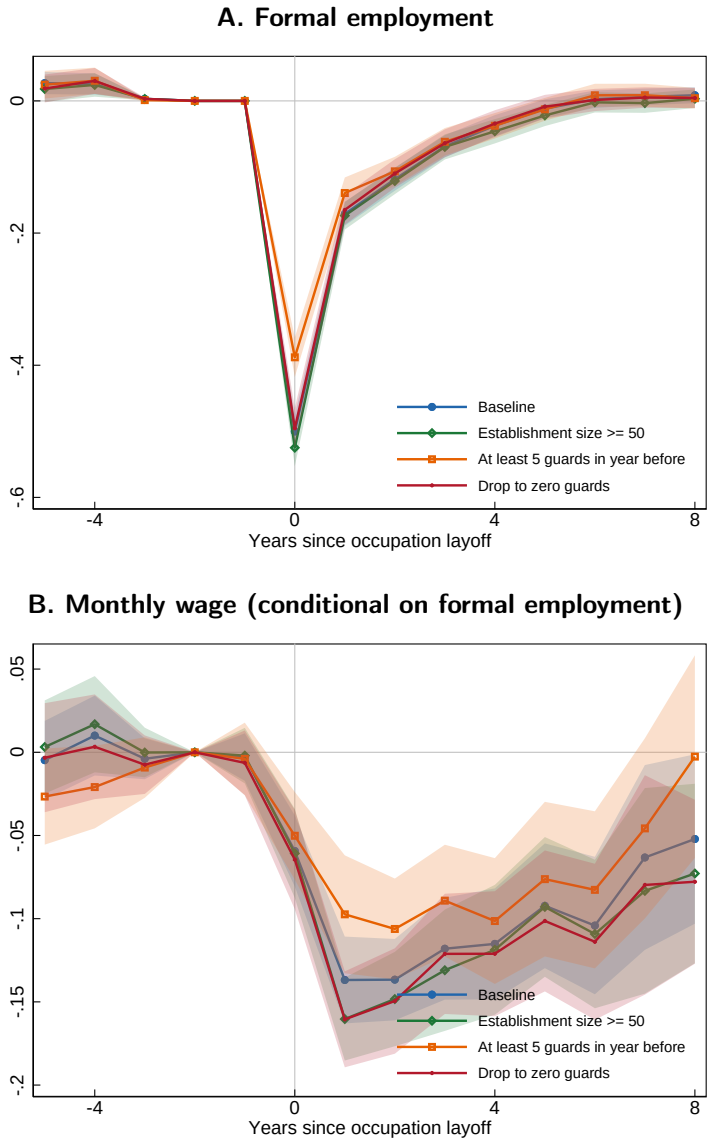
Note: Figure plots coefficient γ_τ from a difference-in-differences regression measuring the impact of a firm occupational layoff on incumbent direct-hire security guards, where the control group are similar workers in establishments that did not have an occupational layoff. The outcome variable is monthly wage, as measured as a fraction of wage two years prior to the outsourcing event, and observations are included only if either (a) the worker remains at the same establishment or (b) the worker remains in the same occupation. Our sample includes all occupational layoffs, as identified by sudden drops in an establishment security guard count, between 1990 and 2000. We include controls for individual and year fixed effects, and time-varying demographics. Shaded bands indicate 95% confidence intervals, with standard errors clustered at the establishment level.

Figure B.7: Effect of occupational layoff on incumbent guards, alternative matching strategies



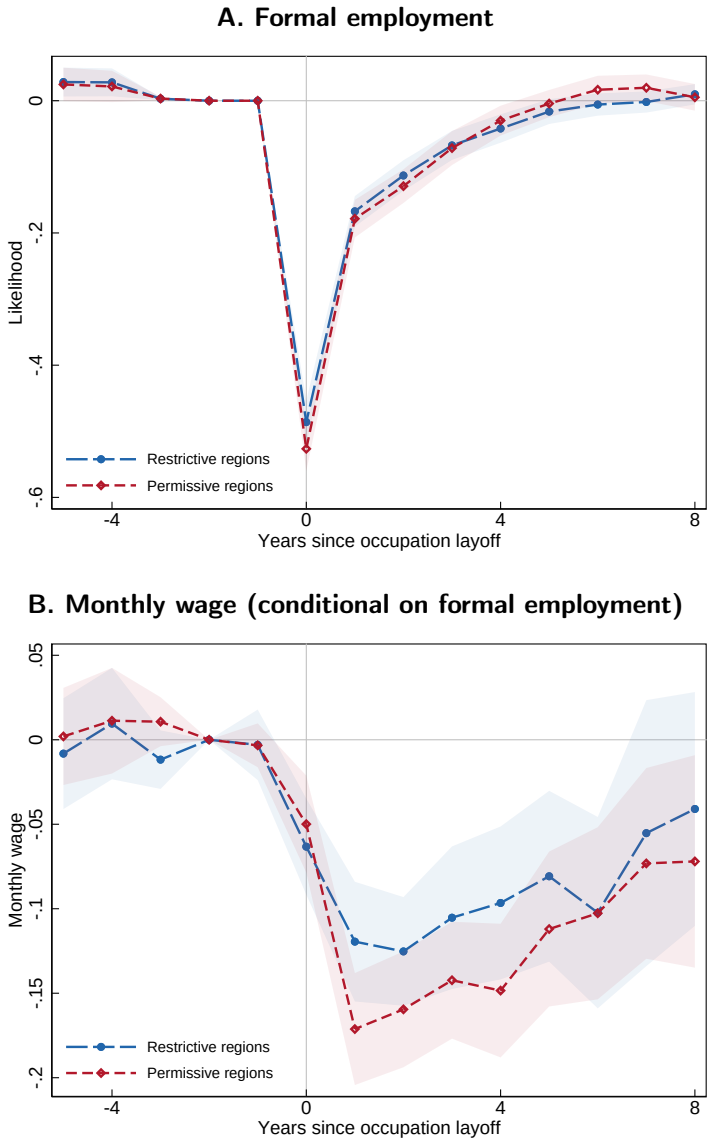
Note: Figure replicates Figure 3, Panels B and D with alternative matching strategies. The baseline specification matches workers within the 2-digit industry and local regional court jurisdiction. The alternative specifications matches within the 3-digit industry and local regional court jurisdiction (in green), within the 2-digit industry and microregion (in orange), and within the 3-digit industry and microregion (in red).

Figure B.8: Effect of occupational layoff on incumbent guards, alternative definitions



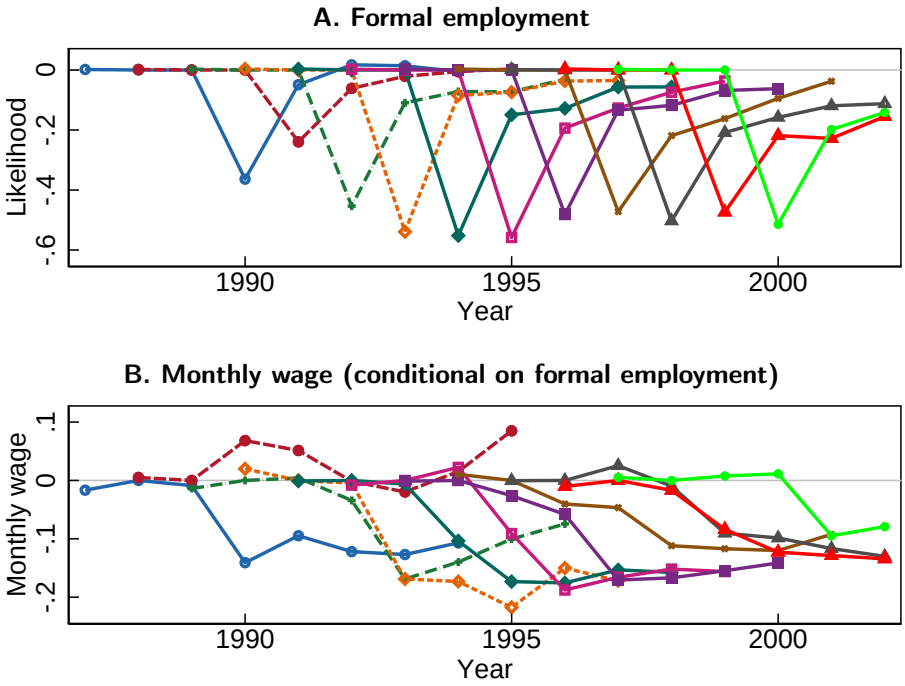
Note: Figure replicates Figure 3, Panels B and D with alternative definitions of occupational layoffs. The alternative specifications restricts to outsourcing establishments with at least 50 employees (in green), at least 5 security guards (in orange), or considers events where the number of security guards drops to zero (in red).

Figure B.9: Effect of occupational layoff on incumbent guards, by region



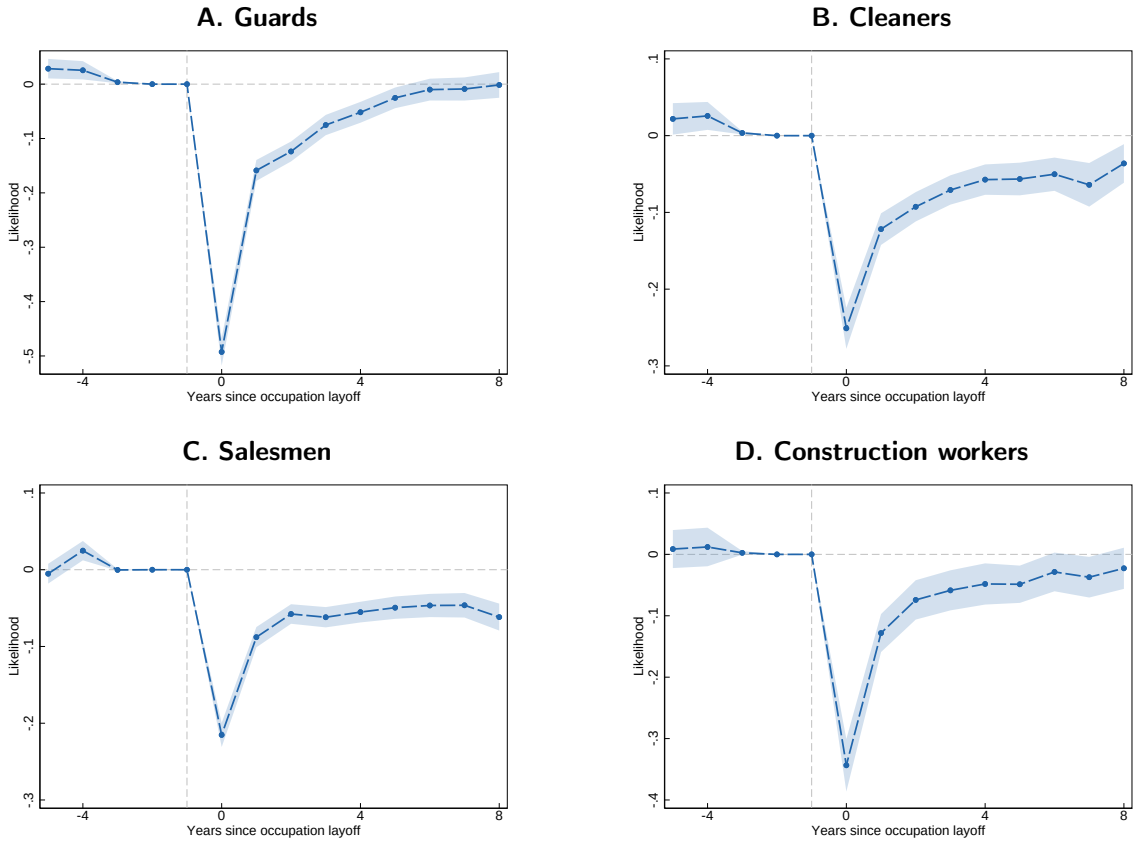
Note: Figure replicates Figure 3, Panels B and D using subsamples including only restrictive and permissive microregions, respectively.

Figure B.10: Effect of occupational layoffs on incumbent guards, by year



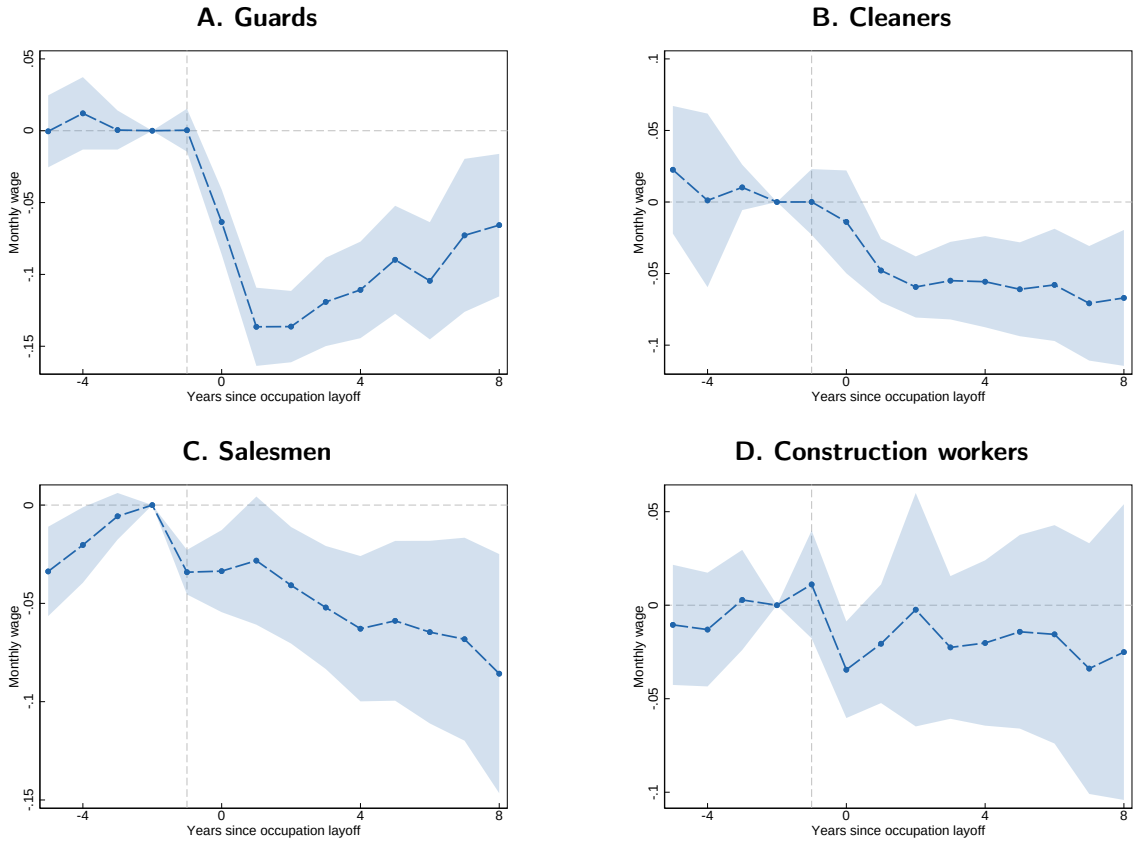
Note: Figure reports coefficients from Equation (1) for year $t-3$ to $t+4$, estimated for each occupational layoff year.

Figure B.11: Effect of occupational layoff on incumbent employment, other occupations



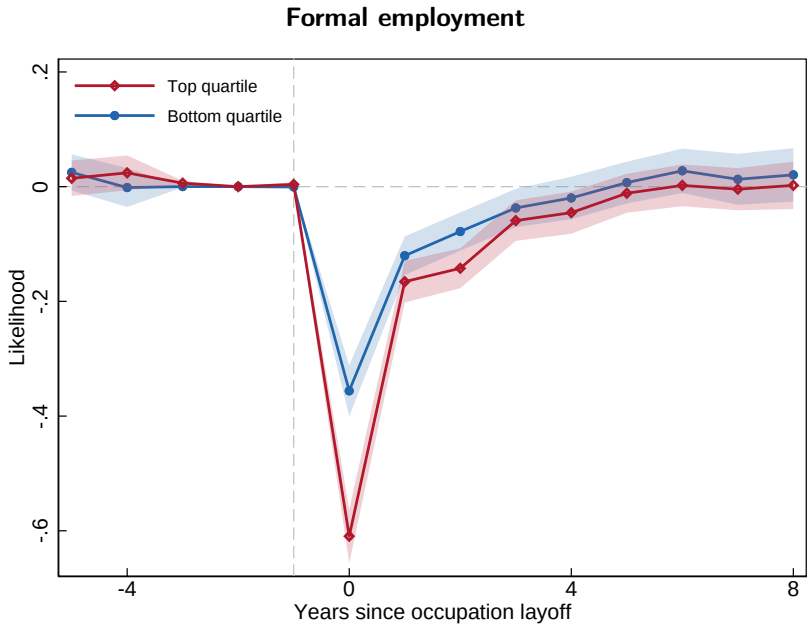
Note: Figure replicates Figure 3, Panel B for occupational layoffs in other occupations.

Figure B.12: Effect of occupational layoff on incumbent wage, other occupations



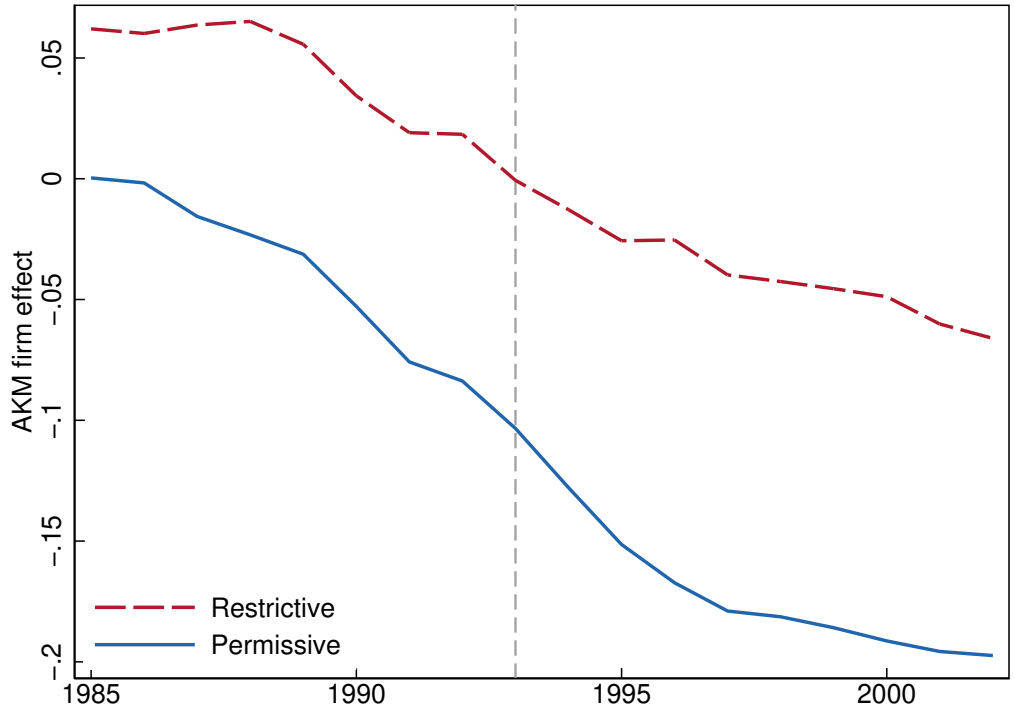
Note: Figure replicates Figure 3, Panel D for occupational layoffs in other occupations.

Figure B.13: Effect of occupational layoff on incumbent employment, by initial firm wage premia



Note: Figure replicates Figure 3, Panel B using subsamples that include only workers initially in the top and bottom quartile of the AKM firm effects distribution, respectively.

Figure B.14: Trends in average firm-specific wage premia among security guards



Note: Figure plots the trend in the mean AKM firm effect among security guards, averaged over microregions in our estimation sample with equal weights, separately for permissive and restrictive regions.

Table B.1: Descriptive Statistics, Matched worker sample

Worker characteristics in year t-1	Impacted worker	Control worker
Male	0.97	0.98
	(0.2)	(0.1)
Years of schooling	5.4	5.1
	(2.8)	(2.8)
Age	41.6	42.6
	(10.2)	(10.7)
Tenure	5.1	4.9
	(4.6)	(4.5)
Average monthly wage (2017 $R)	2363	2301
	(1359)	(1419)
Establishment Size	462	468
	(720)	(964)
Firm FE	0.23	0.21
	(0.28)	(0.3)
Sector:		
Manufacturing	0.32	0.32
Industrial utility	0.13	0.13
Retail	0.12	0.12
Wholesale	0.04	0.04
Finance	0.00	0.00
Service	0.11	0.10
Medical	0.03	0.03
Mining	0.03	0.03
Construction	0.04	0.04
Real estate and transportation	0.13	0.14
Other	0.05	0.05
N	12443	12443

Notes: Sample includes all matched security guards used to estimate the effects of occupational layoffs.

Table B.2: Effects of occupational layoffs on incumbent workers, other occupations

		Guards	Cleaners	Salemen	Construction
Formally employed	Y0	-0.493	-0.251	-0.215	-0.343
	Y1	-0.159	-0.122	-0.088	-0.128
	Y5	-0.025	-0.057	-0.049	-0.049
Formally employed in same occupation	Y0	-0.761	-0.777	-0.691	-0.708
	Y1	-0.399	-0.500	-0.398	-0.398
	Y5	-0.117	-0.216	-0.166	-0.171
Formally employed in contract firm	Y0	0.002	0.008	0.001	0.003
	Y1	0.077	0.032	0.003	0.008
	Y5	0.041	0.020	0.005	0.005
Monthly wage (relative to base)	Y0	-0.064	-0.014	-0.034	-0.034
	Y1	-0.136	-0.048	-0.028	-0.021
	Y5	-0.090	-0.061	-0.059	-0.014
Log monthly wage	Y0	-0.078	-0.034	-0.036	-0.041
	Y1	-0.185	-0.058	-0.053	-0.041
	Y5	-0.112	-0.059	-0.042	-0.032
Firm wage effect	Y0	-0.032	-0.019	-0.006	-0.009
	Y1	-0.080	-0.036	-0.012	-0.016
	Y5	-0.052	-0.030	-0.001	-0.006
As fraction of wage losses	Y0	0.415	0.563	0.166	0.215
	Y1	0.433	0.616	0.219	0.379
	Y5	0.462	0.508	0.032	0.177

Notes: Entries give estimated effects of occupational layoff on the indicated outcome in Y0, Y1 and Y5 following the event, for four occupations (guards, cleaners, salesmen, and construction workers). While employment outcomes are estimated on a balanced sample, wage effects are computed conditional on observation. Wage losses due to firm effects shown both as log points and as a percentage of wage losses (e.g., -.032/-0.78=0.415 in Y0).

C Addendum: Market-level Effects of Outsourcing

C.1 Classifying Regional Courts

In this section, we describe our efforts to construct a measure of pre-legalization court permissiveness and the rationales for our eventual classification.

We discovered that Brazil's regional courts differed in their permissiveness to outsourcing prior to Súmula 331 during a weeklong field trip in September 2019. During this trip, we conducted interviews with managers of security service firms, managers of businesses that either employed or outsourced security services, numerous security guards, labor lawyers, and staff of a security guard syndicate in Recife. In one interview, we asked the CEO of a family-run security service firm incorporated in the 1970s how the legalization of outsourcing by Súmula 331 in 1993 affected her firm. To our surprise, she insistently responded that outsourcing had never been considered illegal. This response contradicted various historical articles that suggest outsourcing had been declared illegal by Súmula 256 in 1985 (da Cruz 2009; Biavaschi and Droppa 2011; Cooney et al. 2015). Puzzled, we consulted several labor lawyers, who informed us that local courts in Brazil's South, which had historically been friendlier to labor, likely enforced the ban on outsourcing much more vigorously than courts in the rest of Brazil.

This finding motivated us to seek legal records in order to construct a finer and more comprehensive measure of local court permissiveness towards outsourcing. Frustratingly, this proved impossible. We submitted formal requisitions to all 24 Regional Labor Courts asking if: (1) lawsuits from 1985-1993 were available, (2) it would be possible for us to have a list of all lawsuits concerning outsourcing, and (3) they still had the cases listed as precedents for Súmula 256 and 331. We received answers in the negative for almost all local courts. Most no longer kept cases before 1993. In the few cases where records still existed, they were neither digitalized nor indexed, and hence required in-person archival visitations. Visitations were frustrated by the COVID-19 pandemic. Even the precedents we specifically asked for were not found, because the record numbers used by the local courts differed from the Superior Court.

Given these obstacles, we resorted to scouring available primary and secondary sources to glean as much information on the stances of regional courts as possible. We rely heavily on research performed by Magda Biavaschi and Allison Droppa, two Brazilian legal historians who interviewed

prominent judges and lawyers regarding the history surrounding Súmula 331, as well as a set of 28 interview transcripts that they generously provided. We also studied a set of publicly available legal cases cited by Súmula 331.

Ample evidence suggests that the 4th region (Rio Grande do Sul) was strongly restrictive towards outsourcing prior to 1993. According to Eduardo Antunes Parmeggiani, prosecutor for the District Attorney's Labor Office in the region, "the understanding of Súmula 256 was *stricto sensu*: [outsourcing] was only allowed for banking security services and for temporary work." In an interview, Biavaschi, a former labor judge in the region, revealed that some judges had an even stricter interpretation, "we understood that ... the exception made in Súmula 256 could not be applied to banks... So I... would recognize the employment link [of security guards] directly with banks... [And] I didn't rule alone. We had a group, we used to discuss and rule this way." We also find evidence that outsourcing was heavily litigated in the region. Biavaschi revealed that "in Rio Grande do Sul, we investigated about 900 cases filed against the company RIOCELL ... of which about 400 questioned outsourcing." Of the three cases from the 4th region that were cited by Súmula 331, two were restrictive decisions subsequently reversed by the Superior Labor Court. We thus classify the 4th region as restrictive.

The 9th region (Paraná) appears to have had a similarly restrictive view. Luiz Salvador, a union lawyer in Paraná, reported that he "litigated complaints... arguing that those who performed doormen services were bankers [and] these lawsuits were successful." From archival work, Droppa reported that "a good part of the decisions of the 9th Region were based on judgments of the Regional Labor Court of the 4th Region." Of the three cases from the 9th region that were cited by Súmula 331, all three were restrictive decisions subsequently reversed by the Superior Labor Court. We thus classify the 9th region as restrictive.

We have some limited but suggestive evidence that the stance of the 15th region (São Paulo state excluding 2nd region) was also restrictive. Droppa reported that the "15th region had a more flexible view [than the 4th region]... but even so, in ... pre-331 period, had a very strong focus on resist[ing] outsourcing." Given our uncertainty about the correct classification of the 15th region, we show that our market-level results are robust to (and, if anything, larger when) dropping observations from the São Paulo state (see Table C.8).

There is compelling evidence that the 2nd region (metropolitan São Paulo) was much more

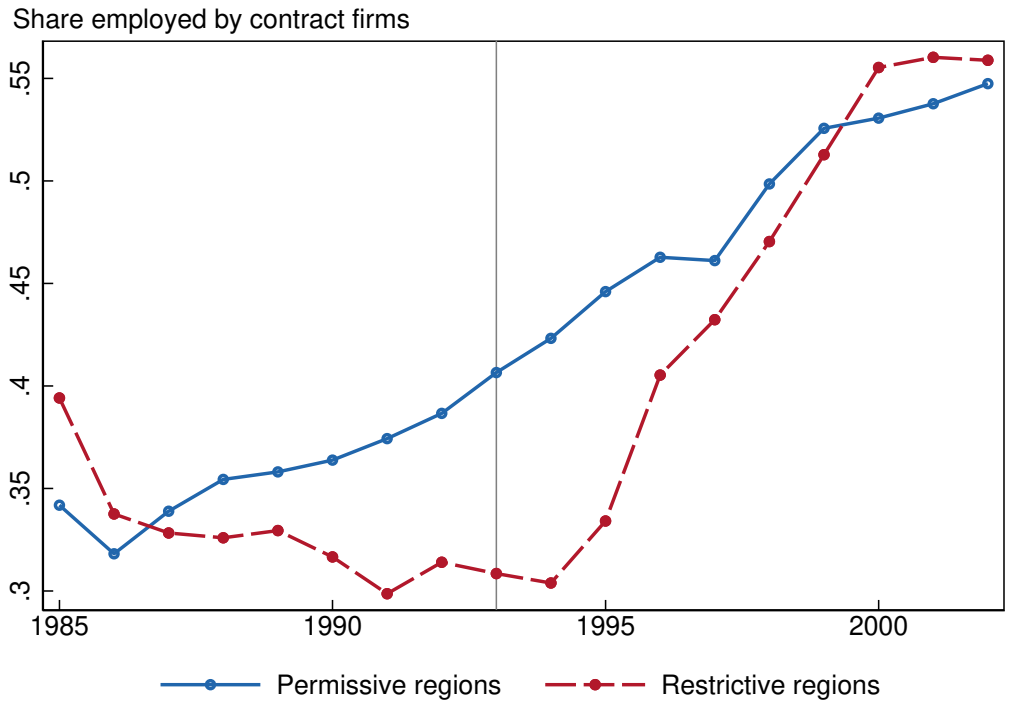
permissive towards outsourcing, however. Iduigues Ferreira Martins, a union leader, reported that the issue of outsourcing was "brought to the Labor Court at certain times, but the successive losses at court discouraged the Unions. Since we didn't win the lawsuits the unions brought about and the bosses were keen on using this very fact as propaganda - 'see! the Labor court considers outsourcing legal'." Judge Jorge Luiz Souto Maior reported that "[the law] allowed for security services outsourcing for banks [but] ended up being extended to other activities" and that "[s]everal theorists, mainly from Escola Paulista ... criticized TST's Súmula 256." We thus classify the 2nd region as permissive.

We find an almost complete absence of scholarship and written record about the legality of outsourcing during the pre-legalization period beyond the regions considered in the preceding paragraphs. We suspect the reason for this absence is that outsourcing was simply much less litigated outside Brazil's South. For example, only three of the 11 cases cited by Súmula were from outside the 2nd, 4th, and 9th regions. We classify the 12th region (Santa Catarina) as restrictive, simply because it physically lay between the 4th and 9th region, the two regions we know to be unambiguously restrictive. We classify all of the remaining courts as permissive. For the 6th region (Pernambuco), we were able to secure an interview with a labor judge, who confirmed to us that before Súmula 331 "we would consider [outsourcing] be legal if the company, the intermediate company was a company specialized in armed security. So he could be outsourcing to a bank or to any other sort of company and it would be legal."

Why did the stances of local courts in the South differ from the rest of Brazil? A number of interviews suggest that judges in the South may have been influenced by a leftist legal education. Brazil's South was the intellectual center of the Alternative Law Movement, which emerged in the late 1980s among jurists in Rio Grande do Sul under the strong influence of the Marxist critical theory of law (Barreto and de Lyra 2016). Biavaschi recounted that "all of us ... at that time were trained in a law school that looked at labor law as a special right, as a profoundly social right, born out of workers' demands in the 19th century." When asked about why court in were more permissive on outsourcing, Souto Maior responded "Influence of the Paulista School of Law? A very rigid School, very close to restricted positivism, authentic legalism ... This view is very present in the training of professionals and, consequently, there is a deficiency with regard to a critical analysis of the Law."

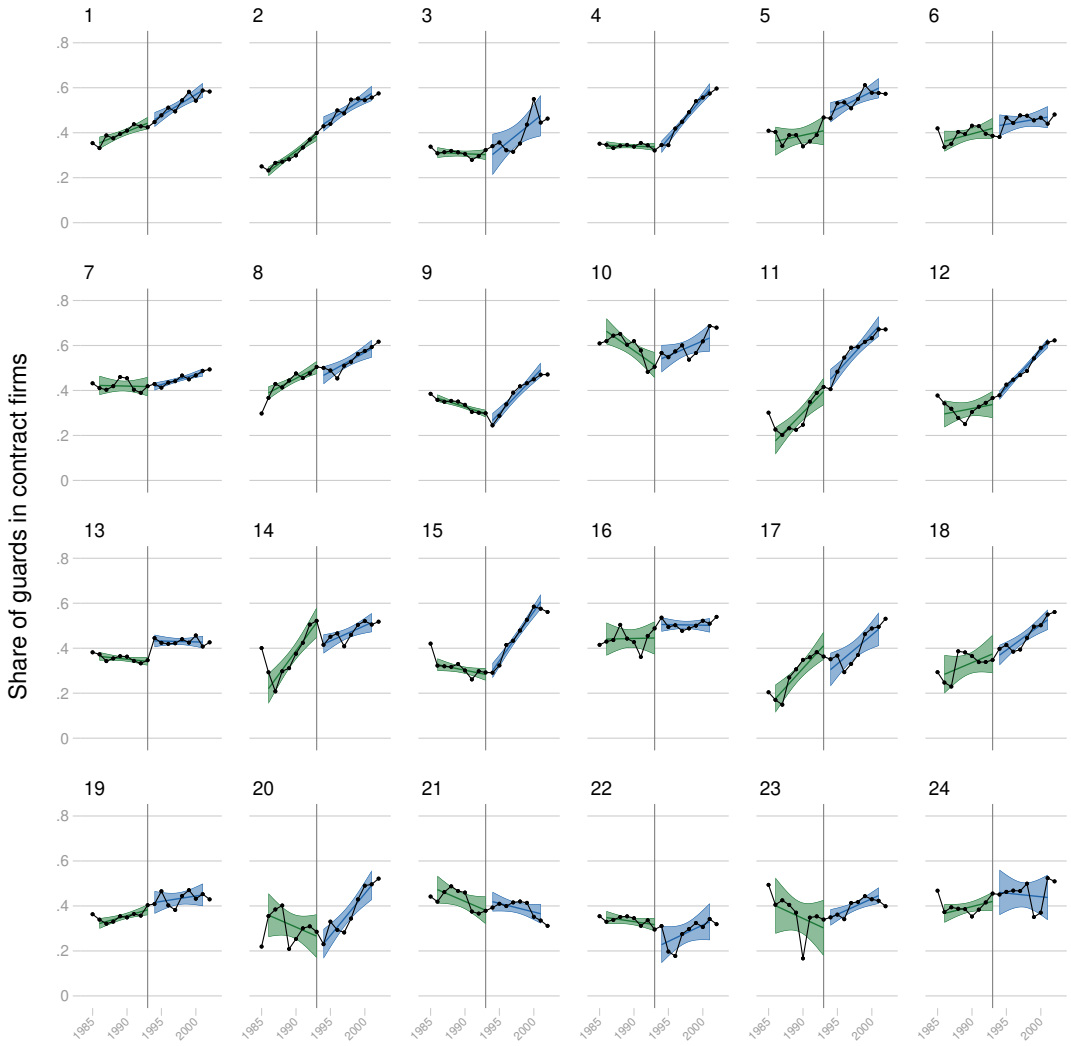
C.2 Additional Figures and Tables

Figure C.1: Trends in outsourcing across regions



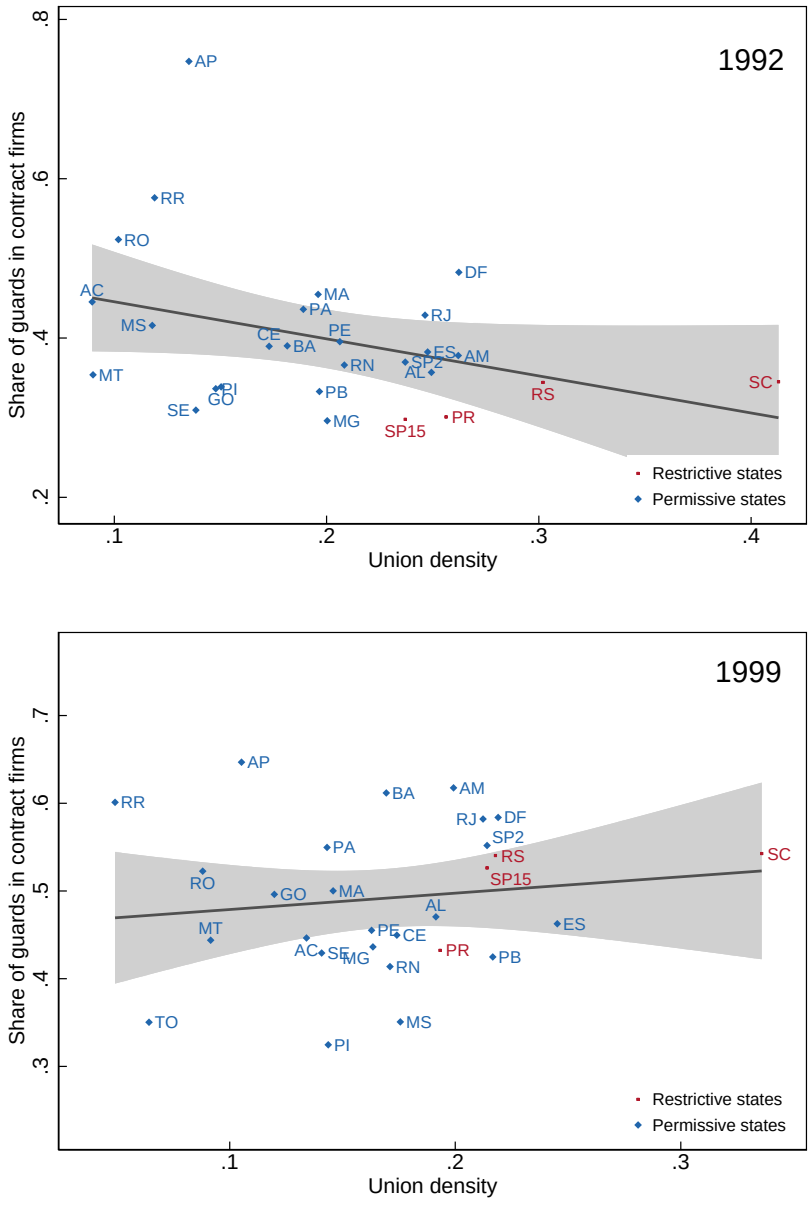
Note: Figure plots the trend in the share of private-sector security guards in the formal sector working for contract firms, separately for permissive and restrictive regions.

Figure C.2: Trends in security outsourcing, by regional labor court



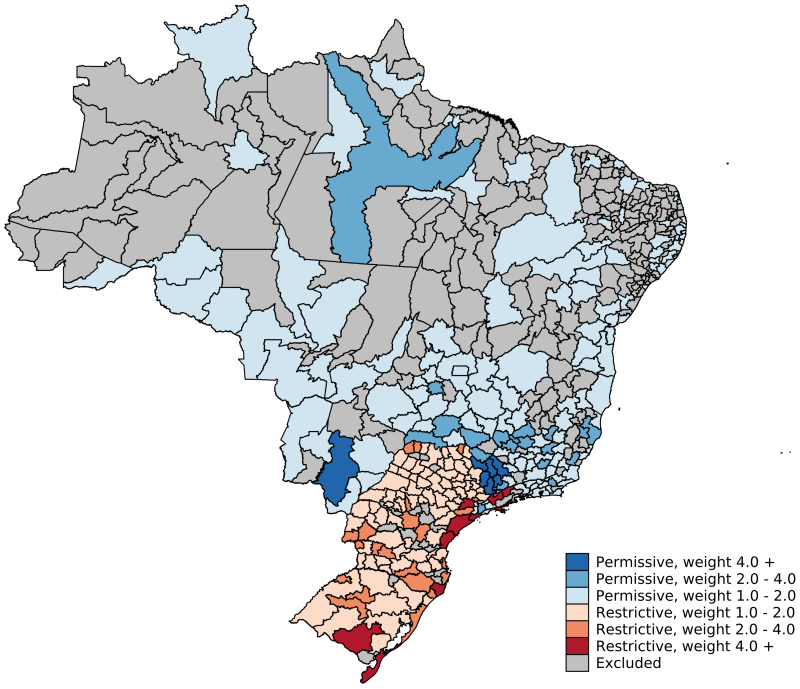
Note: Figure plots the trend in outsourcing prevalence, as measured by the share of security guards in the formal sector working for contract firms, separately for each jurisdiction of the 24 regional labor courts.

Figure C.3: Union density and security outsourcing across states, 1992 and 1999



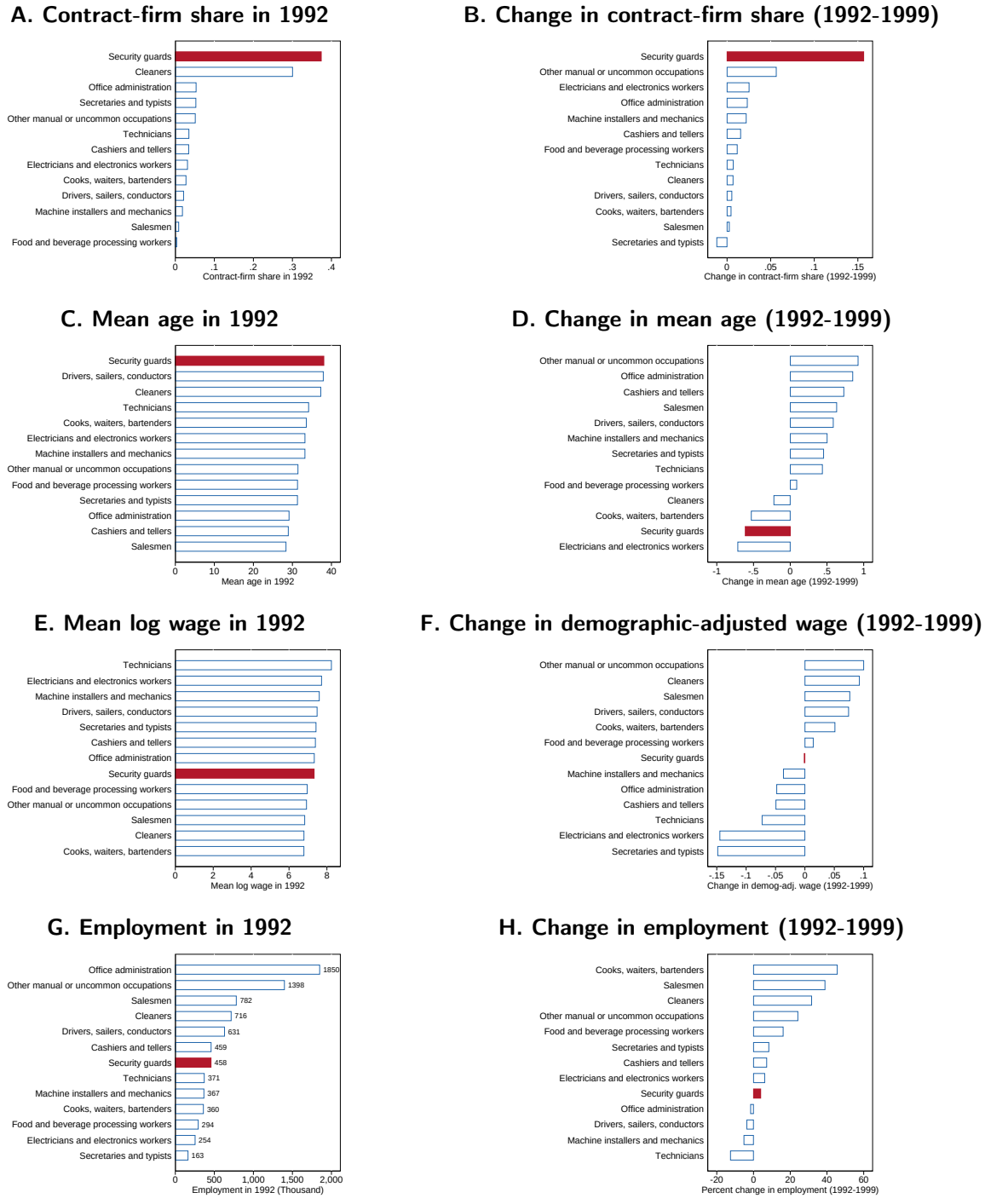
Note: Figure plots outsourcing prevalence against union density for each Brazilian state, in 1992 and 1999. Union density is measured by the share of all workers with union density in household survey PNAD. We include regression lines, computed with average state-level formal-sector employment between 2000-2006 as sample weights. States with a stronger presence of unions, such as those in the South, have a lower presence of outsourcing in 1992, but the relationship reversed by 1999.

Figure C.4: Map of microregions with inverse propensity score weights



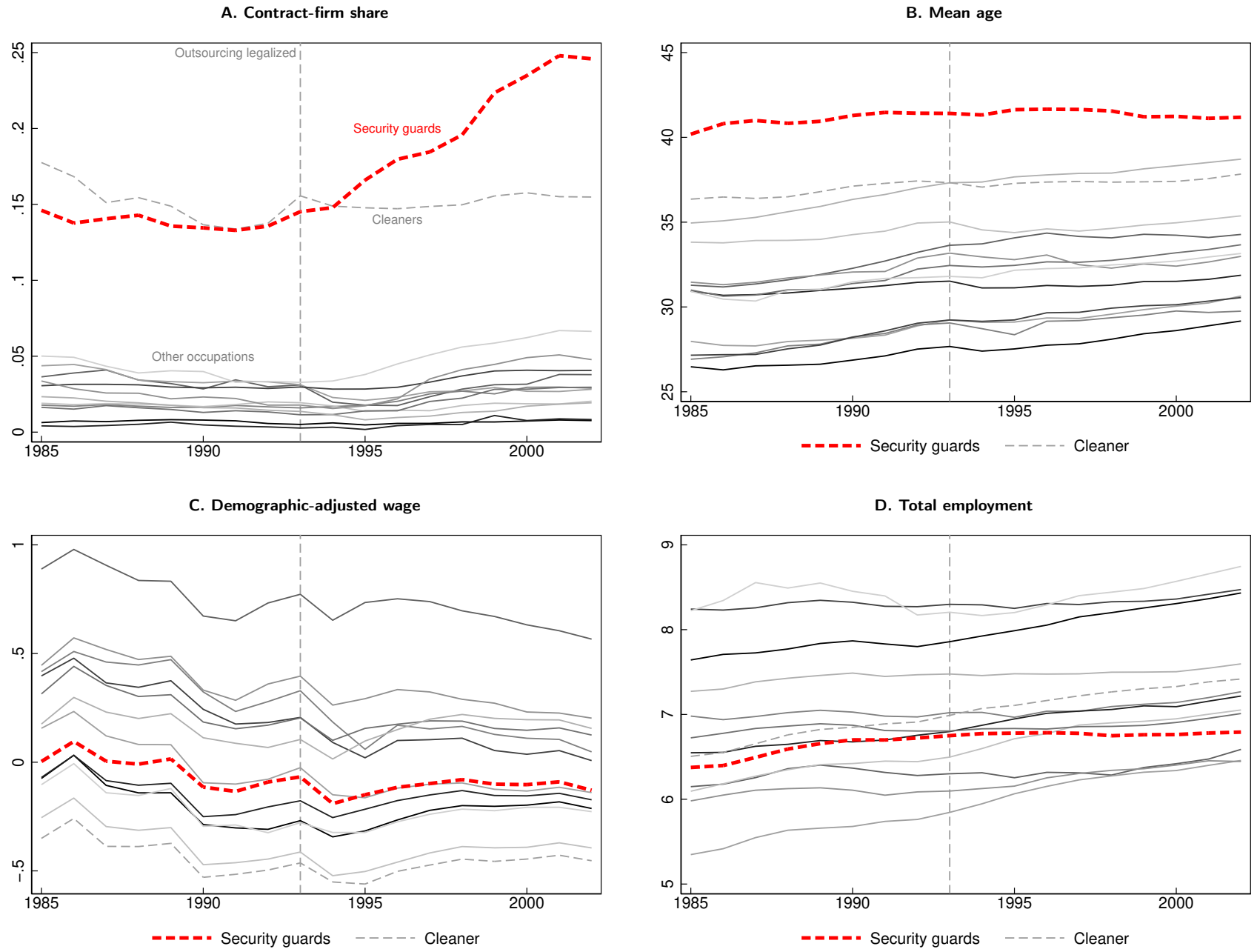
Note: Map shows microregions with inverse propensity score weights. Inverse propensity score weights are computed using log employment, homicide rate, unemployment rate, share of employment in tradable industries, log formal employment of importers, and local import tariff competition exposure in 1992.

Figure C.5: Outcomes by occupation, national



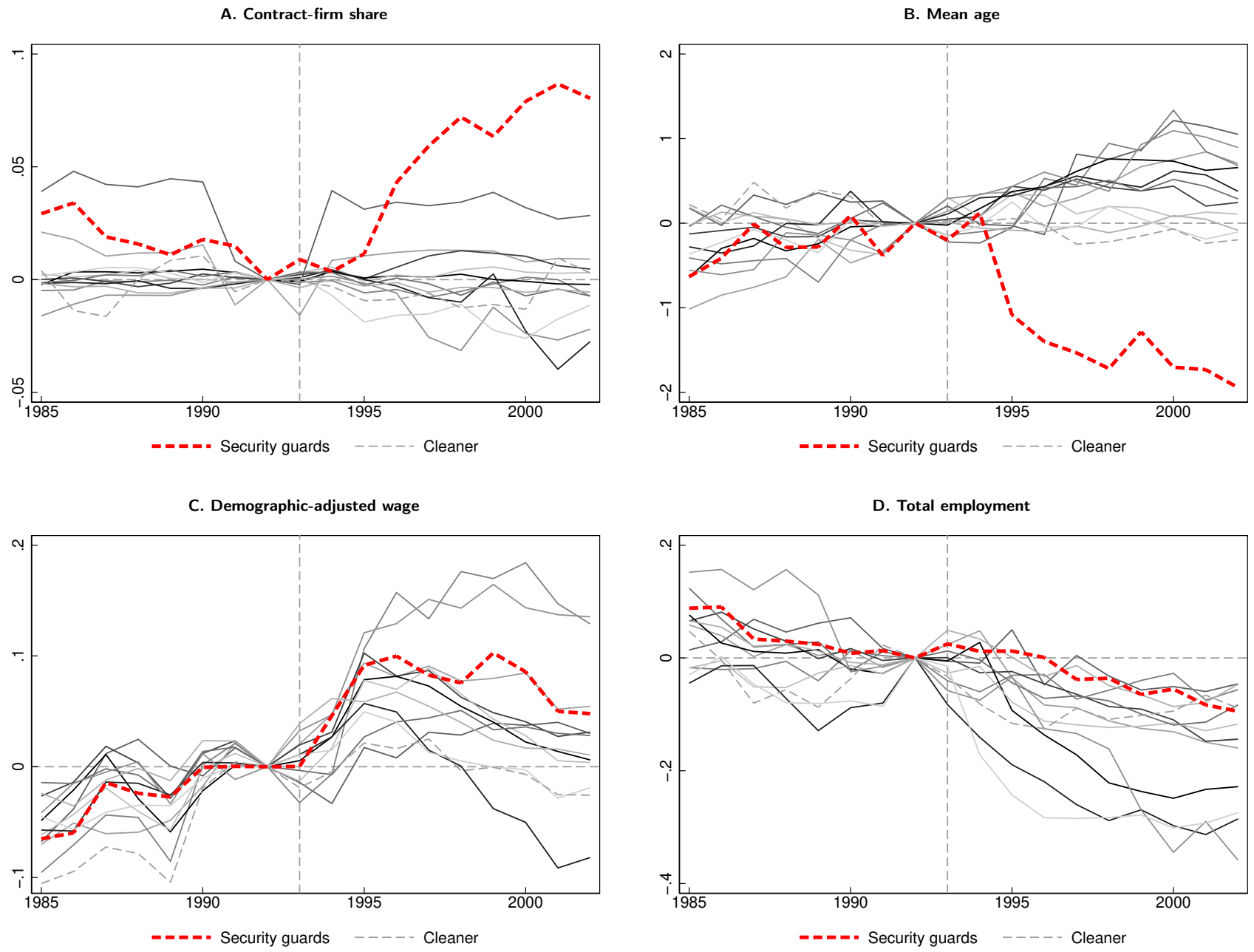
Note: Figure provides summary statistics for all included occupations at the national level.

Figure C.6: Trends by occupation



Note: Figure plots the trend in various outcome variables, averaged over microregions in our estimation sample with equal weights.

Figure C.7: Differences between restrictive and permissive regions by occupation

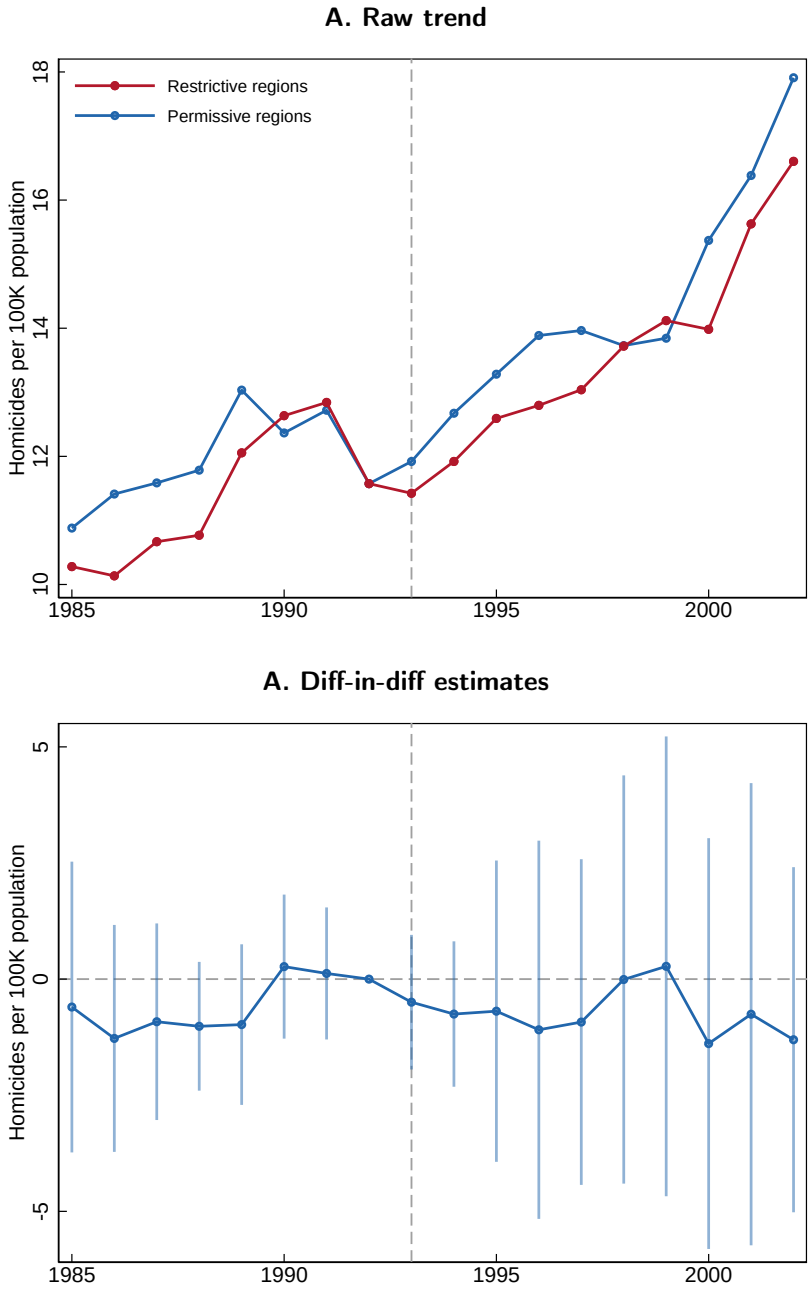


Note: Each line plots the coefficients from a difference-in-differences regression that comparing restrictive microregions to permissive microregions, with controls for year and microregion fixed effects, separately estimated for each occupation in our estimation sample. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

Figure C.8: Effect of outsourcing legalization on alternative wage measures

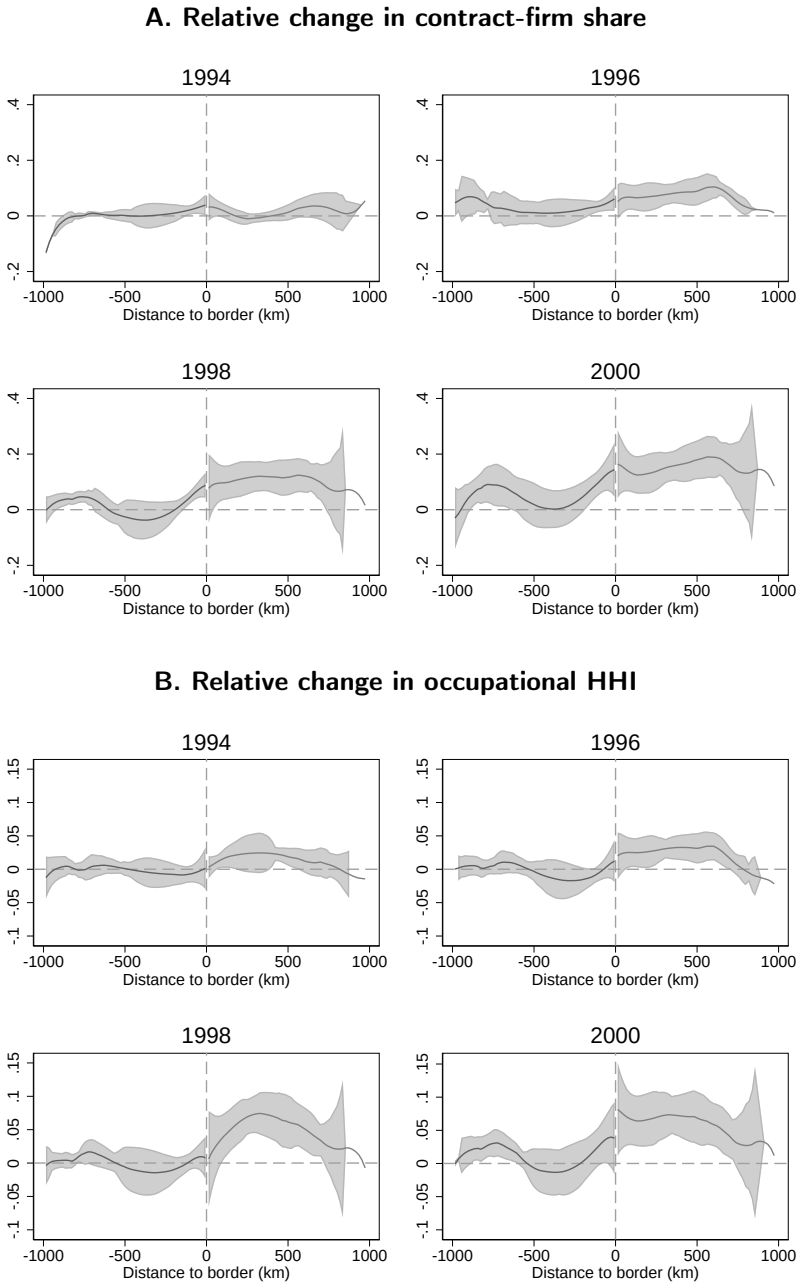
Note: Figure plots the coefficients β_τ from the triple-difference regression measuring the impact of outsourcing legalization in equation (3). The red series uses the mean log wage as the outcome variable. The purple uses the estimated microregion–occupation-year fixed effects from a worker-level wage equation that includes worker demographic controls. The blue uses the estimated fixed effects from a specification that additionally controls for worker fixed effects. The omitted year is 1992. Sample of microregions is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

Figure C.9: Regional differences in homicide rates



Note: Panel A plots the mean homicide rate for restrictive and permissive microregions, respectively. Panel B plots the coefficients from a difference-in-differences regression that comparing restrictive microregions to permissive microregions, with controls for year and microregion fixed effects. The omitted year is 1992. Sample includes all microregions with at least 50 security guards in every year between 1985-2006 and is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

Figure C.10: Changes in outsourcing prevalence and distance to the state border

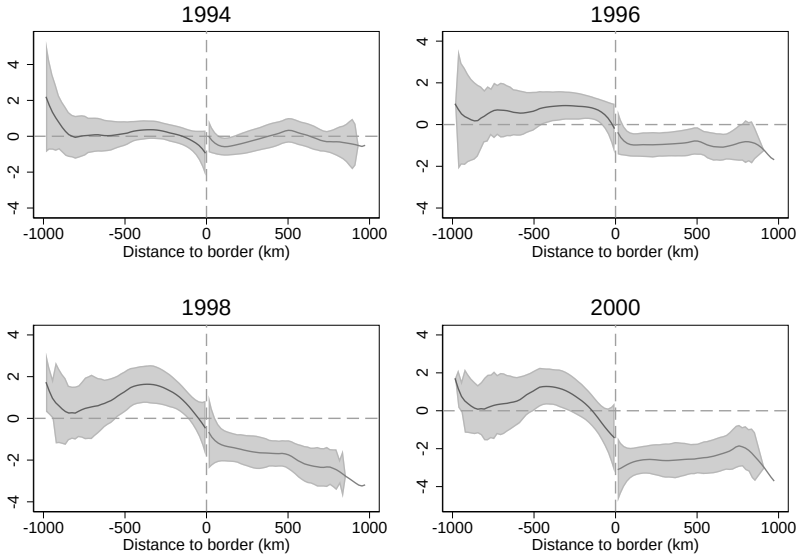


Note: We estimate kernel-weighted local polynomial regressions of the relative change of an outcome in year X on distance to border, using the Epanechnikov kernel, polynomials of degree 2, and a bandwidth of 200km, separately for restrictive and permissive microregions. We define the relative change of an outcome in year X = (outcome in year X - outcome in year 1992) – (mean outcome among comparison occupations in year X - mean outcome among comparison occupations in year 1992). Sample includes all microregions within 1000 km of the border between restrictive and permissive jurisdictions, but excludes the 2nd region. We display a graph of the smoothed values with 95% confidence bands, where restrictive regions given positive distance and permissive regions are given negative distance.

Figure C.11: Changes in employment by age group and distance to the state border

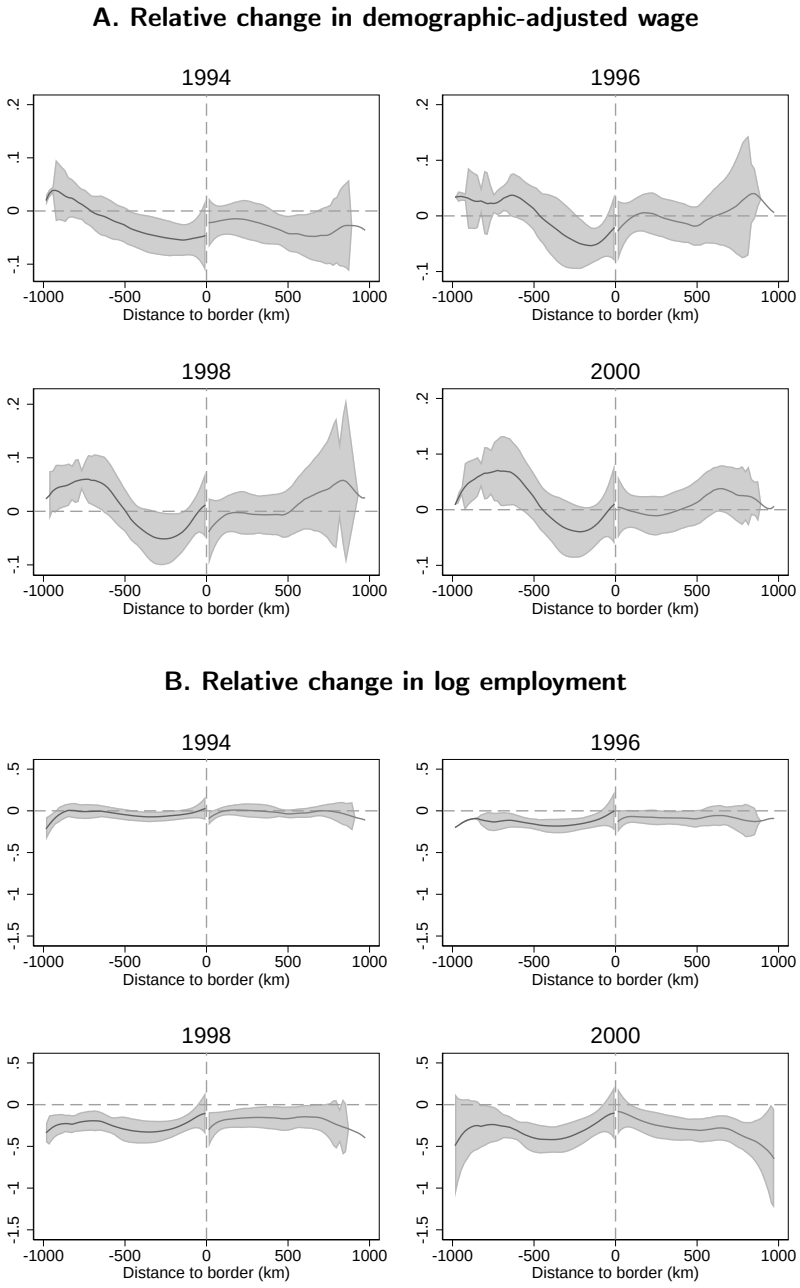
Note: We estimate kernel-weighted local polynomial regressions of the relative change of an outcome in year X on distance to border, using the Epanechnikov kernel, polynomials of degree 2, and a bandwidth of 200km, separately for restrictive and permissive microregions. We define the relative change of an outcome in year X = (outcome in year X - outcome in year 1992) – (mean outcome among comparison occupations in year X - mean outcome among comparison occupations in year 1992). Sample includes all microregions within 1000 km of the border between restrictive and permissive jurisdictions, but excludes the 2nd region. We display a graph of the smoothed values with 95% confidence bands, where restrictive regions given positive distance and permissive regions are given negative distance.

Figure C.12: Relative change in mean age and distance to the state border



Note: We estimate kernel-weighted local polynomial regressions of the relative change of an outcome in year X on distance to border, using the Epanechnikov kernel, polynomials of degree 2, and a bandwidth of 200km, separately for restrictive and permissive microregions. We define the relative change of an outcome in year X = (outcome in year X - outcome in year 1992) – (mean outcome among comparison occupations in year X - mean outcome among comparison occupations in year 1992). Sample includes all microregions within 1000 km of the border between restrictive and permissive jurisdictions, but excludes the 2nd region. We display a graph of the smoothed values with 95% confidence bands, where restrictive regions given positive distance and permissive regions are given negative distance.

Figure C.13: Changes in employment and wage and distance to the state border



Note: We estimate kernel-weighted local polynomial regressions of the relative change of an outcome in year X on distance to border, using the Epanechnikov kernel, polynomials of degree 2, and a bandwidth of 200km, separately for restrictive and permissive microregions. We define the relative change of an outcome in year X = (outcome in year X - outcome in year 1992) – (mean outcome among comparison occupations in year X - mean outcome among comparison occupations in year 1992). Sample includes all microregions within 1000 km of the border between restrictive and permissive jurisdictions, but excludes the 2nd region. We display a graph of the smoothed values with 95% confidence bands, where restrictive regions given positive distance and permissive regions are given negative distance.

Table C.1: Judges' interpretations of the legality of security service outsourcing before Súmula 331

Interpretation	Example quote
Allowed for any firm	The parameter we would use [to judge outsourcing cases] was, a bank could have as an employee, an armed guard, but **we would consider [outsourcing] be legal if the company, the intermediate company was a company specialized in armed security. So he could be outsourcing to a bank or to any other sort of company and it would be legal.**" - Sérgio Torres, appellate judge (TRT 6)
	[The practice of outsourcing] started in 1974 with the temporary work law, and was extended later to security services, which allowed for security services outsourcing for banks. The idea was exactly this... [but] **it didn't restrict itself to banks. It ended up being extended to other activities.**" - Jorge Luiz Souto Maior, appellate judge (TRT 2)
	"**When the Union questioned [the outsourcing], the firm would say that it needed people specialized in certain activities... They would say the same for security services, train guards, deal with gun law issues, so it was ideal to hire a security services firm... Yes there were issues brought to the Labor Court at certain times, but the successive losses at court discouraged the Unions**, since we didn't win the lawsuits the unions brought about and the bosses were keen on using this very fact as propaganda - 'see! the Labor court considers outsourcing legal'." - Iduigues Ferreira Martins, union leader (TRT 2)
Allowed only for banks	"The understanding of Enunciado 256 was stricto sensu: [outsourcing] was **only allowed for banking security services and for temporary work.**" - Eduardo Antunes Parmeggiani, prosecutor for the District Attorney's Labor Office (TRT 4)
Not allowed even for banks	"Regarding bankers, there was a specific situation. Banks had a legal obligation to hire security services given the nature of the banks' business activity. Therefore, **we understood that, because of that, the exception made in Precedent 256 could not be applied to banks... So I... would recognize the employment link [of security guards] directly with banks.** Many of my decisions were confirmed by the [Regional] Court and then by the Supreme Labor Cour[.] [And] I didn't rule alone. We had a group, we used to discuss and rule this way." - Magda Biavaschi, retired appellate judge (TRT 4) and professor
	"In the 1970s and 1980s, I advised watchmen and security guards, who at the time did not have any specific legislation [concerning outsourcing]. At that time, security guards worked at banks' front doors as watchman, and performed various tasks. In truth, they were doormen: they provided information, helped clients open bank accounts, helped with archiving. So, **I started to litigate complaints... arguing that who performed doormen services was a banker... Since [this practice] was illegal, these lawsuits were successful.**" - Luiz Salvador, labor lawyer (TRT 9)

Table C.2: Predictors of microregion outsourcing prevalence, 1992 and 1999

Dependent variable: Microregion security guard contract-firm share in year *t*				
	t =1992		*t* =1999	
Log(mkt size in year *t*)	0.090***	0.090***	0.129***	0.130***
	(0.007)	(0.007)	(0.007)	(0.007)
Restrictive region		-0.037*		0.045**
		(0.020)		(0.021)
Obs	266	266	266	266
R^2	0.38	0.39	0.55	0.56

Notes: Sample is all microregions with at least 25 security guards and 25 cleaners, weighted by average microregion formal-sector employment between 1985-2006. Market size is measured by the number of private-sector security guards in the formal sector. Standard errors are clustered at the microregion level and presented in parentheses, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level.

Table C.3: Descriptive Statistics, Microregions, 1992

Microregion characteristics in 1992	Restrictive		Permissive		
Share of guards employed by contract firms	0.117	0.098	0.149	0.122	0.136
	(0.177)	(0.14)	(0.218)	(0.178)	(0.202)
Establishments directly hiring guards	242	204	379	784	526
	(343)	(263)	(1323)	(2632)	(1883)
HHI	0.060	0.054	0.072	0.059	0.065
	(0.08)	(0.06)	(0.08)	(0.06)	(0.076)
Average log(wage), guards	7.20	7.22	6.95	7.03	6.97
	(0.21)	(0.22)	(0.29)	(0.23)	(0.27)
Average log(wage), all formal workers	7.08	7.12	6.96	6.95	6.95
	(0.2)	(0.21)	(0.27)	(0.27)	(0.27)
Guards per 1000 formal workers	22.07	23.56	30.15	22.40	26.85
	(23.93)	(23.11)	(19.97)	(11.54)	(17.25)
By employment type:					
Direct-hire guards	17.00	19.31	22.29	18.06	20.56
Contract-firm guards	5.08	4.25	7.86	4.35	6.29
By age group:					
Age 18-24	1.62	2.03	2.92	1.92	2.48
Age 25-34	5.73	6.32	8.94	6.06	7.67
Age 35-44	5.27	5.61	7.64	5.67	6.80
Age 45-54	4.77	4.91	5.67	4.48	5.19
Age 55-64	3.91	3.93	4.20	3.56	3.96
Used for entropy balancing:					
Log formal-sector employment	10.1	9.9	9.7	10.1	9.8
Homicide rate (per 100K population)	11.6	13.4	16.6	11.6	14.2
Unemployment rate	0.027	0.037	0.045	0.027	0.037
Log formal employment of importers	9.1	9.0	8.7	9.1	8.8
Share of employment in tradeable sector	0.50	0.45	0.43	0.50	0.45
Import tariff competition exposure	-0.048	-0.043	-0.040	-0.048	-0.043
Weights	Uniform	Inv. prop. Score	Uniform	Entropy-balancing	Inv. prop. Score
TRT regional courts	4	4	20	20	20
N	107	107	159	159	159

Notes: Sample includes all microregions with at least 25 guards and 25 cleaners in all years between 1985 and 2006 . Column (2) is weighted by entropy balancing weights. Standard deviations are in parentheses.

Table C.4: Effect of outsourcing legalization on alternative measures of outsourcing prevalence

	(1)	(2)	(3)	(4)	(5)
A. Contract-firm share					
Short run effect	0.012	0.043*	0.033**	0.013*	0.037*
	(0.014)	(0.022)	(0.012)	(0.007)	(0.020)
Long run effect	0.062**	0.105***	0.081***	0.071***	0.100***
	(0.029)	(0.031)	(0.022)	(0.009)	(0.025)
B. Occupational HHI					
Short run effect	0.011	0.018	0.013*	0.013**	0.018*
	(0.008)	(0.013)	(0.006)	(0.006)	(0.010)
Long run effect	0.029***	0.041**	0.034***	0.037***	0.046***
	(0.009)	(0.017)	(0.008)	(0.008)	(0.014)
C. IHS(Number of direct employers)					
Short run effect	-0.002	-0.015	-0.001	-0.005	-0.014
	(0.010)	(0.014)	(0.014)	(0.011)	(0.012)
Long run effect	-0.059	-0.077	-0.046	-0.069	-0.071*
	(0.045)	(0.045)	(0.043)	(0.042)	(0.036)
1990 mreg features X occ X yr			X	X	X
	Entropy balancing weights	Propensity score weights	Uniform weights	Entropy balancing weights	Propensity score weights
Observations	57456	57456	57456	57456	57456

Notes: Estimates are from our main triple-difference specification. We report pooled coefficients for the short-run post-legalization (1994-1997) and long-run post-legalization (1998-2002). 1990 microregion features include log employment, unemployment rate, employment share in tradeable industries, import tariff reduction exposure, log employment of importers, and homicide rate (per 100K population). These same variable are used to compute propensity score and entropy balancing weights. Standard errors in parentheses are clustered at the TRT regional court level, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. All specifications include microregion-occupation linear trends.

Table C.5: Effect of outsourcing legalization on alternative wage measures

	(1)	(2)	(3)	(4)	(5)
A. Mean log wage					
Short run effect	0.022	0.024	0.021	0.029	0.030
	(0.024)	(0.028)	(0.020)	(0.018)	(0.019)
Long run effect	0.016	0.016	0.010	0.036**	0.034**
	(0.019)	(0.026)	(0.016)	(0.013)	(0.014)
B. Demographic-adjusted wage					
Short run effect	0.023	0.023	0.023	0.030	0.028
	(0.020)	(0.022)	(0.018)	(0.018)	(0.017)
Long run effect	0.025*	0.020	0.017	0.047***	0.039**
	(0.014)	(0.020)	(0.019)	(0.016)	(0.015)
C. Within-worker wage					
Short run effect	0.021	0.028	0.024	0.027*	0.032**
	(0.013)	(0.017)	(0.015)	(0.014)	(0.014)
Long run effect	0.023**	0.025	0.024	0.038**	0.036**
	(0.011)	(0.015)	(0.019)	(0.015)	(0.014)
1990 mreg features X occ X yr			X	X	X
	Entropy balancing weights	Propensity score weights	Uniform weights	Entropy balancing weights	Propensity score weights
Observations	57456	57456	57456	57456	57456

Notes: Estimates are from our main triple-difference specification. We report pooled coefficients for the short-run post-legalization (1994-1997) and long-run post-legalization (1998-2002). 1990 microregion features include log employment, unemployment rate, employment share in tradeable industries, import tariff reduction exposure, log employment of importers, and homicide rate (per 100K population). These same variable are used to compute propensity score and entropy balancing weights. Standard errors in parentheses are clustered at the TRT regional court level, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. All specifications include microregion-occupation linear trends.

Table C.6: Effect of outsourcing legalization on market-level outcomes, without microregion-occupation linear trend controls

	(1)	(2)	(3)	(4)	(5)
A. Contract-firm share					
Short run effect	0.013	0.030**	0.035**	0.024	0.027***
	(0.022)	(0.011)	(0.015)	(0.015)	(0.009)
Long run effect	0.064	0.083**	0.079***	0.085***	0.082***
	(0.038)	(0.030)	(0.023)	(0.020)	(0.014)
B. Mean age					
Short run effect	-1.090***	-0.954***	-1.342***	-1.210***	-0.947***
	(0.275)	(0.280)	(0.246)	(0.217)	(0.210)
Long run effect	-2.059***	-2.404***	-2.661***	-2.252***	-2.272***
	(0.412)	(0.343)	(0.362)	(0.366)	(0.279)
C. Log employment					
Short run effect	0.049**	0.048	0.068**	0.059**	0.055
	(0.022)	(0.035)	(0.029)	(0.021)	(0.037)
Long run effect	0.062	0.099	0.104	0.084**	0.114***
	(0.056)	(0.059)	(0.061)	(0.031)	(0.039)
D. Demographic-adjusted wage					
Short run effect	0.027	0.004	0.025	0.029	0.006
	(0.029)	(0.030)	(0.026)	(0.026)	(0.026)
Long run effect	0.032	-0.010	0.010	0.036	-0.009
	(0.029)	(0.037)	(0.027)	(0.022)	(0.033)
1990 mreg features X occ X yr			X	X	X
	Entropy balancing weights	Propensity score weights	Uniform weights	Entropy balancing weights	Propensity score weights
Observations	57456	57456	57456	57456	57456

Notes: Estimates are from our main triple-difference specification. We report pooled coefficients for the short-run post-legalization (1994-1997) and long-run post-legalization (1998-2002). 1990 microregion features include log employment, unemployment rate, employment share in tradeable industries, import tariff reduction exposure, log employment of importers, and homicide rate (per 100K population). These same variable are used to compute propensity score and entropy balancing weights. Standard errors in parentheses are clustered at the TRT regional court level, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level.

Table C.7: Effect of outsourcing legalization, alternative entropy-balancing targets

	(1)	(2)	(3)	(4)
A. Contract-firm share				
Short run effect	0.027*	0.017	0.009	0.012
	(0.014)	(0.011)	(0.011)	(0.014)
Long run effect	0.087***	0.057**	0.053**	0.062**
	(0.022)	(0.026)	(0.022)	(0.029)
B. Mean age				
Short run effect	-0.654**	-0.711**	-0.749**	-0.767**
	(0.288)	(0.318)	(0.292)	(0.305)
Long run effect	-1.863***	-1.516***	-1.473***	-1.512***
	(0.510)	(0.503)	(0.435)	(0.460)
C. Log employment				
Short run effect	0.038	0.047	0.058	0.063*
	(0.044)	(0.036)	(0.035)	(0.036)
Long run effect	0.058	0.069*	0.080***	0.085***
	(0.057)	(0.033)	(0.025)	(0.029)
D. Demographic-adjusted wage				
Short run effect	0.008	0.010	0.021	0.023
	(0.022)	(0.020)	(0.020)	(0.020)
Long run effect	-0.016	-0.007	0.021	0.025*
	(0.017)	(0.012)	(0.013)	(0.014)
Entropy-balancing targets				
Log employment		X	X	X
Unemployment rate		X	X	X
Emp share in tradeable industries			X	X
Import tariff reduction exposure			X	X
Log employment of importers			X	X
Homicide rate (per 100K population)				X
Observations	57456	57456	57456	57456

Notes: Estimates are from our main triple-difference specification. We report pooled coefficients for the short-run post-legalization (1994-1997) and long-run post-legalization (1998-2002). Treatment group means in the pre-legalization year of 1992 are presented. Standard errors in parentheses are clustered at the TRT regional court level, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. All specifications include microregion-occupation linear trends.

Table C.8: Effect of outsourcing legalization, alternative samples

	(1)	(2)	(3)
A. Contract-firm share			
Short run effect	0.012	0.020	0.017
	(0.014)	(0.022)	(0.018)
Long run effect	0.062**	0.082**	0.065*
	(0.029)	(0.038)	(0.034)
B. Mean age			
Short run effect	-0.767**	-1.168***	-0.741**
	(0.305)	(0.389)	(0.324)
Long run effect	-1.512***	-2.366***	-1.419***
	(0.460)	(0.435)	(0.492)
C. Log employment			
Short run effect	0.063*	0.121***	0.061
	(0.036)	(0.039)	(0.039)
Long run effect	0.085***	0.100**	0.074**
	(0.029)	(0.040)	(0.034)
D. Demographic-adjusted wage			
Short run effect	0.023	0.052*	0.027
	(0.020)	(0.026)	(0.021)
Long run effect	0.025*	0.048**	0.032*
	(0.014)	(0.021)	(0.016)
Sample	Main	Excl SP state	Excl SP metro
Observations	57456	45360	55944

Notes: Estimates are from our main triple-difference specification. We report pooled coefficients for the short-run post-legalization (1994-1997) and long-run post-legalization (1998-2002). Treatment group means in the pre-legalization year of 1992 are presented. Standard errors in parentheses are clustered at the TRT regional court level, with * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. All specifications include microregion-occupation linear trends.

D Addendum: Interpretive Framework

Section D.1 derives the changes in worker and firm surplus due to outsourcing legalization under the right-to-manage union bargaining model. Section D.2 provides derivations for the strongly efficient union bargaining model. Section D.3 presents a model with heterogeneous firms that make endogenous choices of contractual arrangements. Section D.4 presents additional figures and tables.

D.1 Welfare Changes under Right-to-Manage Union Bargaining

We first consider the right-to-manage union bargaining model. We calculate the effects due to reduced worker bargaining power and reduced transactions cost, separately. First consider a decrease in wage markup μ from a weakened union. There are three resulting effects on welfare, as depicted in Figure 11, Panel A.

1. There is a transfer of worker surplus to the firm due to reduced wage for existing workers, given by

$$A = L \times (-w_c d\mu).$$

2. There is new firm surplus from new employment, given by

$$B = \frac{1}{2}\left(-\frac{dL^D}{dw} w_c d\mu\right) \times (w_c d\mu).$$

3. There is new worker surplus from new employment, given by

$$C = \left(-\frac{dL^D}{dw} w_c d\mu\right) \times \left[\left(1 - \frac{\varepsilon_D}{\varepsilon_S}\right) w_c (\mu - 1 + d\mu) + \frac{1}{2}\left(-\frac{\varepsilon_D}{\varepsilon_S} w_c d\mu\right)\right].$$

Next consider an increase in transactions cost c. There are four resulting effects, as depicted in Figure 11, Panel B.

1. There is the pass-through of reduced transactions cost to firms (holding the employment level fixed), given by

$$D = L \times (1 + \mu\phi)(-dc).$$

2. There is the pass-through of reduced transactions cost to workers (holding the employment level fixed), given by

$$E = L \times (-\mu\phi)(-dc).$$

3. There is an increase in firm surplus due to new employment, given by

$$F = \frac{1}{2}\left[-\frac{dL^D}{dw}(1+\mu\phi)(-dc)\right] \times (1+\mu\phi)(-dc).$$

4. There is an increase in worker surplus due to new employment, given by

$$G = \left[-\frac{dL^D}{dw}(1+\mu\phi)(-dc)\right] \times \left[\left(1-\frac{\varepsilon_D}{\varepsilon_S}\right)(\mu-1)(w_c+dw_c) + \frac{1}{2}\frac{-\varepsilon_D}{\varepsilon_S}(1+\mu\phi)(-dc)\right].$$

The combined effects on firm surplus can be rewritten in terms of structural parameters as follows.

$$d(FS) = -w\frac{d\mu}{\mu}\left[1-\varepsilon_D\left\{\frac{1}{2}\frac{d\mu}{\mu}\right\}\right] - L(1+\mu\phi)\,dc\left[1+\frac{1}{2}\varepsilon_D(1+\mu\phi)\frac{dc}{w}\right].$$

Similarly, the effect on worker surplus is given by

$$\begin{aligned} d(WS) = {} & Lw\frac{d\mu}{\mu}\left[1+\varepsilon_D\left\{\left(1-\frac{\varepsilon_D}{\varepsilon_S}\right)\frac{\mu-1+d\mu}{\mu} - \frac{1}{2}\frac{\varepsilon_D}{\varepsilon_S}\frac{d\mu}{\mu}\right\}\right] \\ & + Ldc\left\{\mu\phi + (1+\mu\phi)\varepsilon_D\left[\left(1-\frac{\varepsilon_D}{\varepsilon_S}\right)(\mu-1)\left(\frac{1}{\mu}+\phi\frac{dc}{w}\right) + \frac{1}{2}\frac{\varepsilon_D}{\varepsilon_S}(1+\mu\phi)\frac{dc}{w}\right]\right\}. \end{aligned}$$

D.2 Strongly Efficient Union Bargaining

We next characterize welfare effects in the strongly efficient union bargaining model. In this model, the union and firm bargain over both wage and employment. The union has "risk-neutral" preferences and equal weights across all workers. The employment level therefore coincides with competitive allocation.

The wage level is given by

$$w = \mu w_c \tag{9}$$

Employment is given by market-clearing

$$L^D(w_c+m) = L^S(w_c) \tag{10}$$

Thus we can write

$$\frac{\partial w}{\partial c} = \mu \frac{\partial w_c}{\partial c} = \mu\phi \tag{11}$$

and

$$\frac{\partial L}{\partial c} = \frac{\partial\left(L^D(w_c+m)\right)}{\partial c} = \frac{\partial L^D}{\partial c}\left(\frac{\partial w_c}{\partial c}+\frac{\partial c}{\partial c}\right) = \frac{\partial L^D}{\partial w_c}(\phi+1) \tag{12}$$

Totally differentiation of (9) and (10) and substitution of (11) and (12) yields:

$$\begin{aligned}\frac{dw}{w} &= \frac{d\mu}{\mu}+\mu\phi\frac{dc}{w}\\ \frac{dL}{L} &= \mu\varepsilon_D(\phi+1)\frac{dc}{w}.\end{aligned}$$

Rearranging, we have that

$$\begin{aligned}\frac{dc}{w} &= \frac{\frac{dL}{L}}{\mu\varepsilon_D(\phi+1)}\\ \frac{d\mu}{\mu} &= \frac{dw}{w}-\mu\phi\frac{dc}{w}.\end{aligned}$$

The change in total surplus can be computed as

$$d(TS) = -Ldc\left(1+\frac{dL}{2L}\right)$$

The change in firm surplus is comprised of two parts:

$$d(FS) = B-A,$$

where

$$A = dw\times L\left(1+\varepsilon^D\left(\frac{\mu-1}{\mu}+\frac{1}{2}\frac{dw}{w}\right)\right)$$

and

$$B = -dc \times L\left(1 + \varepsilon^D\left(\frac{\mu - 1}{\mu} + \frac{dw}{w} + \frac{1}{2}\frac{dc}{w}\right)\right).$$

D.3 Heterogeneous Firm Model

In this model, legalization reduces the legal penalty of outsourcing, and thus induces some firms to switch from direct to outsourced employment. This richer model generates predictions that match both our worker-level and firm-level reduced-form findings, as well as aggregate wage and employment effects that are highly similar as our main representative firm model.

D.3.1 Setup

Consider a finite set of firms indexed by $j \in \mathscr{J}$. There are two contractual arrangements $a \in \{E, O\}$, which respectively denote direct employment and outsourcing. There are two periods $t \in \{0, 1\}$, representing the time before and after outsourcing legalization.

In each period t, firm j maximizes profit by choosing l_{Ejt} and l_{Ojt}, the number of direct-hire and contract-firm workers. The firm's profit is given by

$$\pi_{jt} = \max_{\{l_{Ejt}, l_{Ojt}\}} R_j\left(l_{Ejt} + l_{Ojt}\right) - \sum_{a \in \{E,O\}} \left(w_{ajt} + c_{ajt}\right) l_{ajt}$$

where $R_j(\cdot)$ denotes a continuous, increasing, and concave firm-specific revenue function, $w_{ajt} \geq 0$ is a firm-arrangement-specific wage paid to the worker in period t, and $c_{ajt} \geq 0$ is a firm-arrangement-specific non-compensation cost component. Direct-hire and outsourced labor are assumed to be perfect substitutes in the revenue function, but may differ in their firm-specific wages and non-wage labor costs.

The wage w_{aj} can be decomposed into two additive parts:

$$w_{ajt} = w_t^c + \mu_{aj},$$

where w_t^c denotes the competitive wage in period t and $\mu_{aj} \geq 0$ denotes a time-invariant firm-arrangement-specific wage markup. The non-compensation cost component c_{ajt} can also be de-

composed as follows:

$$c_{ajt} = \begin{cases} m_{Oj} + T_j & \text{if } a = O \text{ and } t = 0 \\ m_{Oj} & \text{if } a = O \text{ and } t = 1 \\ m_{Ej} & \text{if } a = E, \end{cases}$$

where $m_{aj} \geq 0$ is a firm-arrangement-specific transactions cost and $T_j > 0$ is a firm-specific legal penalty for outsourcing before legalization.

Because contract-firm and direct-hire workers are perfect substitutes in the revenue function, each firm's choice of contractual arrangement is separable from its choice of employment. We can therefore let $a_{jt} = \arg\min_a \left(w_{ajt} + c_{ajt}\right)$ denote the firm's choice of contractual arrangement. Before legalization, firm j outsources if and only if

$$\mu_{Ej} + m_{Ej} > \mu_{Oj} + m_{Oj} + T_j,$$

while after legalization, firm j outsources if and only if

$$\mu_{Ej} + m_{Ej} > \mu_{Oj} + m_{Oj}.$$

We assume that $\mu_j^E > \mu_j^O$, but m_{Ej} could be greater or less than m_{Oj}. In other words, when a firm decides to outsource, its wage premium unambiguously falls, but its transactions cost could either rise or fall.

The firm's employment choice is determined by profit-maximization. Letting $\mu_{jt} = \mu_{a_{jt}j}$ and $c_{jt} = c_{a_{jt}jt}$, we can write each firm's employment as a function of the unit labor cost by inverting the firm's first-order condition:

$$l_{jt} = R_j'^{-1}\left(w_t^c + \mu_{jt} + c_{jt}\right).$$

Summing over firms, total employment in period t is given by

$$L_t \quad = \quad \sum_{j \in \mathscr{J}} R_j'^{-1}\left(w_t^c + \mu_{jt} + c_{jt}\right),$$

while the average wage in period t is given by

$$\overline{w}_t = w_t^c + \sum_{j \in \mathscr{J}} s_{jt} \mu_{jt},$$

where $s_{jt} = l_{jt}/L_t$ denotes the employment share of firm j in period t.

To close the model, we assume that labor market equilibrium is a set $\left\{ w_t^c, \left\{ l_{ajt} \right\}_{a \in \{E,O\}, j \in \mathscr{J}} \right\}_{t \in \{0,1\}}$ such that (i) each firm j chooses $\left\{ l_{ajt} \right\}_{a \in \{E,O\}}$ to maximize profit in each period t and (ii) all workers with a reservation wage below w_{ct} are employed, and no worker with a reservation wage above w_{ct} is. In notation, $\sum_{j \in \mathscr{J}} l_j^D \left(w_t^c + \mu_{jt} + c_{jt} \right) = L^S \left(w_t^c \right)$, where $\mu_{jt} + c_{jt}$ is the unit labor cost net of the competitive wage (henceforth, "net labor cost"), and $L^S(w)$ is the total labor supply at competitive wage w.

D.3.2 Discussion

The model allows for two firm-level effects of domestic outsourcing that are frequently discussed in the literature. First, outsourcing may reduce firm-specific wage premia paid to workers. Second, outsourcing may generate firm-specific cost efficiencies.

Wage markup μ_{aj} captures the degree to which worker compensation at firm j is raised above the competitive level under arrangement a. For simplicity, we assume that the firm-arrangement-specific wage markup is time-variant and does not depend on the legality of outsourcing.[37] We also assume that wage markups are higher under direct employment, because the literature has consistently found that outsourced workers are paid lower wages (see, e.g., Goldschmidt and Schmieder 2017; Drenik et al. 2020).

Transactions cost c_{aj} represents any non-compensation organizational costs that firm j faces for employing a worker under contractual arrangement a, such as administration, training, scheduling, hiring, and monitoring costs. Once again, we assume that the firm-arrangement-specific transactions cost is time-variant and does not depend on the legality of outsourcing.[38]

Legal penalty T_{Oj} captures a firm j's cost of violating legal restrictions against outsourcing in

[37] This assumption would be violated if the increased threat of outsourcing after outsourcing legalization reduced direct-hire wage markups.

[38] This assumption would be violated if, for example, outsourcing legalization allows contract firms to expand, thus creating new economies of scale and leading transactions costs for firms that outsource to fall.

period 0. These costs could include legal penalties, litigation costs, as well as reputational damage that may result from engaging in activities prohibited by labor courts.

D.3.3 Effects of Outsourcing Legalization

When outsourcing becomes legalized, legal penalties for outsourcing fall to zero. For firms that outsourced even before legalization, net labor costs (i.e., $\mu_{jt} + c_{jt}$) unambiguously fall, since they no longer incur any legal penalty from outsourcing. For firms that always directly employ, net labor costs are unchanged. For firms that are induced to switch from direct employment to outsourcing, net labor costs also fall, since their net labor costs would have remained the same if they did not switch. Furthermore, cost minimization implies that there are no firms that switch from outsourcing to direct employment.

Since net cost unambiguously falls for some firms and remain constant for others, total labor demanded by firms shifts out. This in turn raises total employment as well as the competitive wage.

The resulting change in average wage is comprised of two additive parts:

$$\overline{w}_1 - \overline{w}_0 \quad = \quad \underbrace{(w_{c1} - w_{c0})}_{>0} + \sum_{j \in \mathscr{J}} \left(s_{j1}\mu_{j1} - s_{j0}\mu_{j0} \right).$$

The first term is the change in the competitive wage following outsourcing legalization, which is unambiguously positive. The second term is the change in the employment-weighted average firm-specific wage premia, which can be further decomposed into two components

$$\sum_{j \in \mathscr{J}} \left(s_{j1}\mu_{j1} - s_{j0}\mu_{j0} \right) = \underbrace{\sum_{j \in SW} s_{j0} \left(\mu_{Oj} - \mu_{Ej} \right)}_{<0} + \underbrace{\sum_{j \in \mathscr{J}} \left(s_{j1} - s_{j0} \right) \mu_{a_{j1}j}}_{?},$$

where *SW* denotes the set of firms that switch to outsourcing. The first term represents the "direct" effect of reduced wage premia at firms that switch to outsourcing. This component is negative since the wage premia of switchers unambiguously falls after outsourcing. The second term is an "indirect" component due to the changes in the firm employment shares, holding wage premia fixed, and has ambiguous sign.

This model therefore explains our empirical findings that outsourcing legalization increased

both employment and average wages. The substantial increase in market-level employment implies that net labor costs substantially fell after outsourcing legalization. However, the small increase in average wages implies that the average firm-specific wage premia did not fall enough to offset the rise in competitive wage.

A simple extension explains the observed effects of outsourcing legalization on incumbent workers. Suppose that all employed workers in period 0 continue to be employed at the same firm in period 1 unless fired, and that the wages of incumbent workers are downward rigid and cannot be reduced. In order to switch from direct employment to outsourcing achieve reductions in wage markups, firm must therefore fire incumbent workers and hire new replacement workers. As discussed in Section 3, this assumption is natural because Brazilian law prohibits nominal wage reductions, including through firing and rehiring the same worker through a contract firm to perform the same job.

Outsourcing legalization would therefore induce a wave of layoffs. Laid-off incumbent workers experience a spell of unemployment, and suffer persistent wage reductions related to the loss of firm-specific wage premia. If firing costs are high, the firm may choose to instead reassign some workers to other jobs within the firm to avoid firing them, even though those jobs may be less well suited for the incumbent worker. As shown in Section 3 and 4, all of these predictions are borne out in our data.

D.3.4 Special case: Linear labor demand and supply

Estimation of this model requires information about firm-specific transactions costs, legal penalties, and labor demand. These data that are not available to us, since we neither observe firm-specific legal penalties, transactions costs, and the client-specific wages and employment of contract-firm workers.

However, under two simplifying assumptions, the estimated changes in market-level security guard employment can be written as linear functions of firm-level wage premia and transactions cost differentials. The assumptions are:

1. Each firm j's labor demand is linear with identical slopes, i.e., $l_{jt}\left(c_{jt}\right) = l_j - \beta_D\left(c_{jt}\right)$;

2. Labor supply is linear, i.e. $L^S\left(w_c\right) = L_0^S + \frac{\beta_S}{|\mathscr{J}|} w_c$;

Given these assumptions, the change in competitive wage of security guards is equal to

$$w_1^c - w_0^c = \theta \Delta \bar{c},$$

where $\theta = \beta_D / (\beta_S + \beta_D)$ is the pass-through of reduced net labor costs onto equilibrium wages and $\Delta \bar{c}$ is the average non-wage cost savings due to outsourcing legalization, given by

$$\Delta \bar{c} = \frac{1}{|\mathscr{J}|} \left[\sum_{j \in AO} T_j + \sum_{j \in SW} \left(m_{Ej} - m_{Oj} + \mu_{Ej} - \mu_{Oj} \right) \right].$$

Note that $\Delta \bar{c}$ can be decomposed into two terms. The first term is the sum of the legal penalty savings of always-outsourcers. The second term is the sum of the transactions cost and wage premia savings for switchers.

The change in employment of security guards per firm is then equal to

$$\begin{aligned} \bar{l}_1 - \bar{l}_0 &= \beta_D \left(w_{c1} - w_{c0} \right) \\ &= \beta_D \theta \Delta \bar{c}, \end{aligned} \tag{13}$$

where $\bar{l}_t = L_t / |\mathscr{J}|$. The change in average wage of security guards is equal to

$$\overline{w}_1 - \overline{w}_0 = \theta \Delta \bar{c} + \Delta \bar{\mu},$$

where

$$\Delta \bar{\mu} = \sum_{j \in \mathscr{J}} \left(s_{j1} \mu_{j1} - s_{j0} \mu_{j0} \right)$$

denotes the change in the employment-weighted average firm-specific wage premia. Therefore, with assumed values of β_S and β_D, it is possible to identify $\Delta \bar{c}$ and $\Delta \bar{\mu}$ from $\bar{l}_1 - \bar{l}_0$ and $\overline{w}_1 - \overline{w}_0$.

D.3.5 Welfare Analysis

Having identified $\Delta \bar{c}$ and $\Delta \bar{\mu}$, we can approximate changes in total firm surplus and worker surplus due to outsourcing legalization. To do this, we assume that the employment-weighted average transactions cost and wage premia are assumed to approximately equal to their respective

unweighted average across firms.

The change in firm surplus is then given by

$$\begin{aligned}
\Delta FS &= -\sum_j \frac{1}{2}\left(l_{j0}+l_{j1}\right)\left(c_{j1}-c_{j0}\right) \\
&\approx -\frac{L_0+L_1}{2}\sum_j \left(c_{j1}-c_{j0}\right) \\
&= \frac{L_0+L_1}{2}\left[\Delta\bar{c}-(w_{c1}-w_{c0})\right] \\
&= \frac{L_0+L_1}{2}(1-\theta)\,\Delta\bar{c} \\
&= \frac{L_0+L_1}{2}\frac{\left(\bar{l}_1-\bar{l}_0\right)}{\beta_D}.
\end{aligned}$$

The approximate change in firm surplus can therefore be computed by simply dividing from the change in employment by the slope of labor demand.

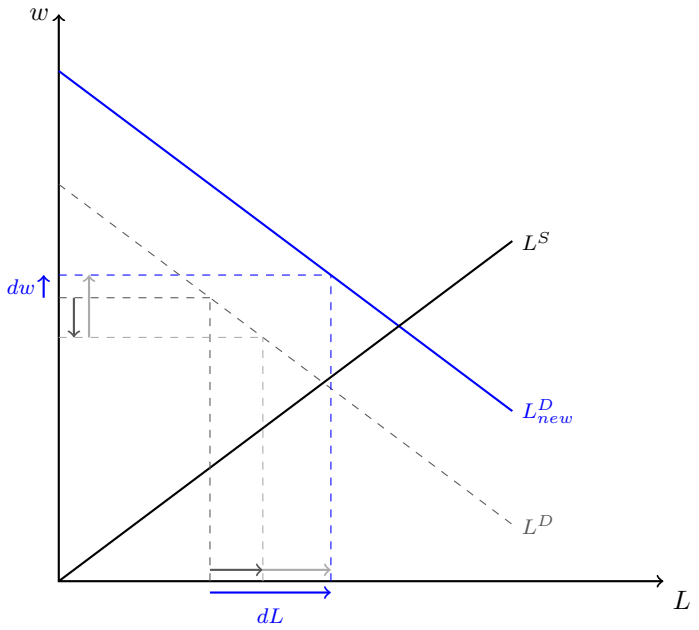
The change in worker surplus is given by

$$\begin{aligned}
\Delta WS &= \sum_j \frac{1}{2}\left(l_0+l_1\right)\left(w_{c1}-w_{c0}\right)+\sum_j l_{j1}\mu_{a_{j1}j}-\sum_j l_{j0}\mu_{a_{j0}j} \\
&\approx \sum_j \frac{1}{2}\left(l_0+l_1\right)\left(w_{c1}-w_{c0}\right)+\frac{L_0+L_1}{2}\left(\sum_j s_{j1}\mu_{a_{j1}j}-\sum_j s_{j0}\mu_{a_{j0}j}\right) \\
&= \frac{L_0+L_1}{2}\left(\theta\Delta\bar{c}+\Delta\mu\right) \\
&= \frac{L_0+L_1}{2}\left(\overline{w}_1-\overline{w}_0\right).
\end{aligned}$$

The change in worker surplus is therefore approximately the change in total wages paid.

D.4 Additional Figures and Tables

Figure D.1: Wage and employment effects in a right-to-manage union bargaining model



Note: Figure plots the combined effects of reduced wage markup μ and reduced transactions cost m on wage and employment in the model presented in section 7.

Table D.1: Decomposition of welfare effects under right-to-manage model

	% of initial guard wagebill	
Change in firm surplus	2.7%	(1.0%)
Due to reduced transactions cost	2.4%	(0.7%)
For existing employment (D)	2.3%	(0.7%)
From new employment (F)	0.1%	(0.0%)
Due to reduced markup	0.3%	(0.9%)
For existing employment (A)	0.3%	(0.9%)
From new employment (B)	0.0%	(0.0%)
Change in worker surplus	4.1%	(1.5%)
Due to reduced transactions cost	4.2%	(1.3%)
For existing employment (E)	2.8%	(0.8%)
From new employment (G)	1.4%	(0.4%)
Due to reduced markup	-0.1%	(0.4%)
For existing employment (-A)	-0.3%	(0.9%)
From new employment (C)	0.2%	(0.5%)

Notes: Table decomposes changes in firm, worker, and total surplus due to outsourcing legalization into components due to existing employment and new employment.

Table D.2: Effects of outsourcing legalization on structural parameters under efficient bargaining and heterogeneous firm models

	Right-to-manage	Efficient bargaining	Heterogeneous firm model
Change in transactions cost (dc/w_0)	-5.1%	-5.7%	-5.7%
Change in markup ($d\mu$)	-0.3%	-0.7%	-0.3%
Change in firm surplus	2.7%	2.5%	2.9%
Change in worker surplus	4.1%	3.5%	2.6%

Notes: Table reports the changes in transactions cost, wage markup, and firm and worker surplus due to outsourcing legalization under alternative models.