Outsourcing, Labor Market Frictions, and Employment*

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Abstract

We estimate the labor market impacts of Brazil's 1993 outsourcing legalization using North-South variation in pre-legalization court permissiveness, and comparing security guards to less-affected occupations. We find that outsourcing legalization persistently reallocated jobs from older incumbent guards to younger entrants. Total employment of guards and their entry from informality persistently increased, while average demographic-adjusted wages remained constant. Meanwhile, a wave of occupational layoffs displaced some incumbent guards from high-wage firms. The evidence suggests that the rise of non-core activity outsourcing reduced labor market frictions, facilitated by firm-level economies of scale in human resources and spillovers to non-adopting firms.

Keywords: domestic outsourcing, labor market frictions, employment
JEL: J52, J58, L24, O17

1 Introduction

Labor markets in developing economies are hampered by search frictions and high rates of informality (Ulyssea, 2020; Donovan et al., 2023). The growth of professionally managed firms may be a necessary step for overcoming these labor market frictions and enabling economic development (Chandler, 1977; Bloom and Van Reenen, 2007; Bloom et al., 2013; Hjort et al., 2022; Engbom et al., 2024). In this paper, we explore the possibility that the rise of specialized non-core service contract firms—such as those for cleaning, security, IT, or human resource services—reduces labor market frictions. Since contract firms operate large and liquid internal labor markets, they gain economies of scale from hiring, screening, training, matching, and managing a large number of similar workers and may thereby facilitate specialization (Abraham and Taylor, 1996; Autor, 2009). We assess this theory by estimating the labor market impacts of an exogenous reduction in the cost of non-core activity outsourcing.

Although the benefits of domestic outsourcing to workers have long been hypothesized, recent literature on its effects has largely focused on negative impacts on incumbent workers' wages. Scholars have carefully documented that domestic outsourcing excludes low-wage incumbent workers from firm wage premia and increases wage inequality (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2023). However, these approaches rely on firm-level variation in outsourcing choices. The resulting firm-level estimates do not capture the effects of domestic outsourcing on employment nor on all workers in impacted occupations. Exogenous market-level variation in outsourcing adoption is therefore needed to assess the overall impacts of domestic outsourcing.

We begin our analysis by estimating the market-level effects of domestic outsourcing on employment and wages using an unexpected 1993 court ruling in Brazil that legalized outsourcing of all non-core activities by private sector firms. To form precise causal estimates, we focus on its impact on Brazil's large market for security guards, which accounted for 5% of total formal private sector male employment in 1992 and experienced the largest increase in outsourcing. To measure worker outcomes, we use Brazil's employer–employee linked data.

We control for confounding shocks using a triple-differences (DDD) regression specifica-

tion that compares guards to less-affected occupations in restrictive relative to permissive regions, before and after legalization. We also weight 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 find that outsourcing legalization led to positive employment and large reallocation effects. Total employment in the security guard occupation persistently increased by roughly 5%, with about half of this increase driven by workers coming from either unemployment or informality. The employment of security guards ages 18–24 increased and stayed about 50% higher, while the employment of those ages 55–64 persistently declined by roughly 15%. The average age of security guards persistently fell by two years, and their average employment tenure persistently declined by roughly half a year. The demographic-composition-adjusted wage in the occupation of security guards as a whole did not change; instead, *un*adjusted wages fell because the average age of security guards declined and younger security guards tend to earn lower wages.

These estimated market-level effects of outsourcing legalization are robust to several potential confounds. They are unlikely to be due to pre-existing differences in the evolution of local occupational labor markets, as we detect no differential pre-legalization trends. The estimates are also highly robust to using inverse propensity score weights, entropy balancing weights, and regression adjustment to account for potential confounding trends. Additionally, the impacts did not coincide with observable changes in crime rates that may drive demand for security services.

We next examine the generalizable mechanisms that may be driving these results. We begin by offering a simple conceptual framework. In the model, employers face search and informational frictions in the labor market and therefore favor incumbent workers. Contract firms aggregate labor demand from clients and thereby gain sufficient scale to invest in a technology to screen and match entrant workers to suitable clients. A reduction in the cost of domestic outsourcing increases total employment and reallocates employment from incumbent to entrant workers. This framework suggests that the observed reallocation of employment across demographic groups—such as the shift from older to younger workers—is driven by improved matching of workers' comparative advantage under reduced search fric-

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tions. Young men are more physically fit than older men for security jobs, but they often lack a track record to credibly signal reliability—a key desirable trait in security services. Contract firms mitigate this problem through economies of scale in screening and matching workers.[1]

We then present a set of descriptive findings consistent with our hypothesized mechanism. First, contract firms employ a much larger number of security guards than direct employers, suggesting they have occupation-specific economies of scale in human resources. Second, security guards in Brazil have high turnover rates, indicating that informational frictions in Brazilian labor markets during the study period were large. Third, outsourced workers on average have lower average tenure, lower returns to tenure, lower exit rates from formal employment, and lower rates of occupational switching. These findings suggest that outsourced workers have systematically different employment relations that may be attributable to the organizational features of contract firms.

We also test for spillovers of legalization-driven outsourcing adoption onto non-adopting direct-hire firms. While direct-hire employment decreased in the longer run, we find that it temporarily increased immediately after outsourcing was legalized, driven by a surge in younger worker employment. In fact, the demographic composition of direct-hire employment also persistently shifted younger: outsourcing legalization persistently *increased* the direct employment of younger workers while persistently decreasing that of older workers. These findings are inconsistent with a partial equilibrium view whereby outsourcing legalization only affects employers that switch to outsourcing. Rather, they align with a general equilibrium view whereby an increase in contract firms' large-scale recruitment attracts positively selected young workers into the security guard occupation. Given the highly frictional nature of the labor market for security services, such positively selected entry likely eased hiring constraints even for direct-hiring firms that did not adopt outsourcing after legalization.

Finally, we document the incidence and consequences of firm-level outsourcing events in

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[1] Field visits revealed that contract firms recruit primarily young men and screen them using physical fitness tests, psychological evaluations, and group interviews. Scale also allows firms to ensure reliability post-hiring—for instance, by maintaining a reserve pool of guards stationed at their base, ready to replace posted guards who fail to report due to illness or other disruptions.

the wake of outsourcing legalization, following the methods of Goldschmidt and Schmieder (2017). We find that outsourcing legalization led to a large wave of occupational layoffs—affecting 7%–9% of incumbent security guards. These occupational layoffs temporarily displaced incumbent workers from formal employment and persistently reduced their wages, by roughly 10%. This decline is largely explained by a loss of firm wage premia among workers initially employed at high-wage firms.

Importantly, unlike our market-level estimates, the estimates from firm-level events do not capture the effects of outsourcing legalization on newcomer guards nor its effects on a large number of incumbent security guards not affected by firm-level outsourcing events.[2] Rather, they show that the wages of the subset of incumbent workers who experienced firm-level outsourcing events fell more than the average worker in the occupation. This result cautions that firm-level event-based estimates—commonly used to assess the effects of domestic outsourcing—capture only a partial view of its broader labor market impacts.

Overall, the large reallocation and positive employment effects that we uncover, as well as the auxiliary evidence that we provide, are consistent with the hypothesis that the rise of specialized contract firms for non-core activity outsourcing reduced labor market frictions. The result was that entrant workers from outside the formal sector benefited, but some incumbent workers experienced concentrated harms as they were displaced from high-wage firms.

An important implication of our findings is that the market-level wage effects may differ from firm-level wage estimates commonly studied in the literature. Another is that the rise of domestic outsourcing did not simply alter worker rents, as emphasized by much recent evidence, but also dramatically altered employment patterns. These findings not only advance the literature on domestic outsourcing but also highlight how economies of scale in human resource management can help to overcome labor market frictions and spur economic development.

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[2]Despite the efficiency gains from scale in human resources of the kind we document, outsourcing is not a desirable contracting option for many firms. Field interviews revealed that firms seeking continuity in guard-client relationships—such as country clubs or interior security in shopping malls—preferred to hire guards directly, avoiding the frequent rotations typical of contract firms. By contrast, firms where guards had limited client interaction—such as exterior mall security or armed bank and transport protection—favored contract firms for their reliability and screening, even if the total contracting cost, including management fees, exceeded direct payroll expenses.

Related literature. Early literature on domestic outsourcing explores the determinants of firm-level decisions to outsource (Abraham and Taylor, 1996; Houseman, 2001; Autor, 2003; Berlingieri, 2014; Chaurey, 2015; Espinosa, 2020).[3] More recent studies estimate outsourcing wage differentials, holding worker characteristics and local labor market conditions constant (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2023). Other research measures the impact of changes in outsourcing regulations using firm-level variation (Bertrand et al., 2021; Estefan et al., 2024), and an emerging literature uses general equilibrium models to study the aggregate consequences of domestic outsourcing on output and inequality (Bilal and Lhuillier, 2021; Spitze, 2022; Bostanci, 2022).[4]

Our work differs from these studies in four main aspects. First, we focus on a medium-wage occupation with occupational licensing and a small outsourcing wage differential. In contrast, recent studies that estimate outsourcing wage differentials focus on low-wage occupations. They show that outsourced workers earn substantially lower wages due to the loss of firm-level wage premia (e.g., Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2023; Guo et al. 2024). Our focus on a higher-wage, licensed occupation may explain why we find that Brazil's outsourcing legalization did not lower demographic-adjusted wages.

Second, we focus on non-core activity outsourcing, while studies examining bans on outsourcing instead focus on core activity outsourcing. For example, Bertrand et al. (2021) show that the end of a ban on contract labor in India increased economic performance by allowing large manufacturing firms to avoid firing costs and scale up production. Estefan

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[3]Research shows that labor service outsourcing is more likely when firms have fluctuating labor demand (Abraham and Taylor, 1996; Houseman, 2001), require specialized services (Berlingieri, 2014; Espinosa, 2020), or face high firing costs (Lee, 1996; Autor, 2003; Chaurey, 2015). Autor (2001) documents that temporary help agencies screen and train workers. Battiston et al. (2021) studies the patterns of job rotation, experience accumulation, and talent poaching in a Colombian security service firm. Kalleberg (2000) surveys relevant research in sociology, while 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) and Abraham et al. (2018) discuss data challenges for measuring outsourced work and alternative work arrangements.

[4]Bilal and Lhuillier (2021) estimate a model of posted wages and on-the-job search and find that the rise of outsourcing in France reduced worker earnings and welfare but increased aggregate output. See also the working papers by Spitze (2022), who combines NLSY 1979 data with a search-and-bargaining model to understand the aggregate effects of domestic outsourcing in the U.S., and Bostanci (2022), who estimates a model of industry dynamics with labor adjustment frictions and find that domestic outsourcing increased both aggregate output and employment.

et al. (2024) find that Mexico's 2021 ban on outsourcing of core activities increased wages and non-wage compensation, reduced investment, and increased firm exit, with no detectable effects on employment, revenues, or the use of other inputs.[5] These studies largely focus on outsourcing as a means of avoiding regulatory requirements. We instead study the legalization of non-core activity outsourcing in a context where all firms—direct-hiring and contract firms—are subject to the same labor regulations. This allows us to focus on the role of scale economies at contract firms in explaining employment effects, as opposed to the role of regulatory arbitrage in explaining wage effects.[6]

Third, we focus on labor markets with significant regulation and large informal sectors. As such, the efficiency gains from labor market intermediation may be especially large in our setting. Guo et al. (2024) use the same data but study a different time period and find that outsourced cleaners and security guards in Brazil have fewer transitions to unemployment than comparable direct-hire workers, especially early in their employment tenure. They argue that contract firms ease worker reassignment across firms. Our findings closely complement theirs by providing evidence for non-wage effects of domestic outsourcing.

Fourth, and most importantly, we are the first to use exogenous market-level variation to estimate the effects of domestic outsourcing on labor markets. Prior studies generally use firm-level variation and focus on impacts on incumbent workers. In contrast, our market-level design accounts for potential effects on entrant workers. Using this market-level approach, we provide the first estimates of total employment effects at the occupational level. We are also the first to document that outsourcing reallocates jobs from older incumbent workers to younger entrant workers. This evidence is not only novel to the literature on domestic

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[5]In a working paper, Jiménez and Rendon (2022) use quarterly household survey data and a predictive model of worker-level treatment based on demographic variables to study Peru's 2022 ban on core activity outsourcing.

[6]It is unclear whether our findings for non-core services extend to core activities. Efficiency gains in our framework rely on scale in human resources, which is more feasible when labor demand is homogeneous—typical of non-core functions like security or IT. Core functions may involve more heterogeneous, firm-specific labor, limiting this mechanism. Brazil's 2017 labor reform (Law no. 13,429/2017), which legalized outsourcing of core activities, could help test generalizability. However, the reform also introduced major changes to the labor code, complicating causal interpretation. Analysis of the 2017 outsourcing reform is also complicated by the fact that core outsourcing in Brazil often occurs through "MEI" (Microempreendedor Individual) self-employment arrangements—introduced in 2008—rather than formal employment contracts, rendering it invisible in RAIS. This raises the possibility that the reform enabled regulatory arbitrage—similar to the Mexican case studied by Estefan et al. (2024)—alongside any potential efficiency gains from scale, as MEI arrangements bypass key labor obligations such as social security, severance, and paid leave.

outsourcing but also provides rare evidence for insider-outsider theories, which highlight how labor market institutions and imperfections may benefit incumbent workers at the expense of entrant workers (Lindbeck and Snower, 1989; Saint-Paul, 2002).

Our study also relates to the literature on labor market regulation. Scholars have 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). However, partial labor market reforms in Europe, which lifted constraints on fixed-term employment but maintained employment protection for workers under permanent contracts, had the perverse effect of increasing turnover among young workers without boosting employment (Bentolila and Saint-Paul, 1992; Cahuc and Postel-Vinay, 2002; Blanchard and Landier, 2002; García-Pérez et al., 2018; Daruich et al., 2020). The fact that Brazil's 1993 outsourcing legalization led to large losses for incumbent workers and large increases in employment, especially among the young, offers a useful counterpoint to the European experience. It suggests that reforms that allow outsourcing of *non-core* economic activities generate better outcomes for young workers and worse outcomes for incumbents than those that enable the use of fixed-term contracts without intermediation.

Roadmap. Section 2 provides a stylized conceptual framework for understanding the observed employment effects. Section 3 provides institutional background and descriptive evidence relevant for interpreting the estimates. Section 4 estimates the local labor market effects of outsourcing legalization, and Section 5 investigates the underlying mechanisms. Section 6 documents the incidence of firm-level outsourcing events around legalization and their effects on workers. Section 7 concludes.

2   Conceptual Framework

In this section, we provide a simple conceptual framework for interpreting the observed employment effects of domestic outsourcing legalization. The framework is a stylized partial equilibrium model with imperfect information and learning (a la Jovanovic 1979). In the model, only contract firms have sufficient scale to more efficiently screen and match workers.

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The rise of outsourcing therefore reallocates jobs from incumbent to entrant workers.

Setting. There is a set of workers indexed by i, a set of final-goods-producing firms indexed by j, and a set of contract firms. All firms face informational and search frictions in the labor market, but contract firms have sufficient scale—achievable by aggregating demand for non-core activities from final-goods firms—to invest in a fixed-cost human resources technology that can more efficiently screen and match workers.

Each final-goods firm has a publicly known productivity y_j that is drawn from distribution with full support between $[\underline{y}, \infty)$ for some $\underline{y}$. Each worker-firm pair has match productivity $y_{ij} \in \{0, y_j\}$, which is initially unknown and independently drawn for each pair, where π is the probability that $y_{ij} = y_j$. For simplicity, each final-goods firm can match with at most one worker.

Timing. Final-goods firms first choose whether to outsource or directly hire. Each firm then chooses whether to match with an entrant worker, an incumbent worker, or no worker at all. After matching, the worker and firm Nash bargain over expected total surplus, with the firm's outside option being no production and β as its bargaining power. All workers have outside option b. If successful, production occurs and output is realized.

Information. We think of outsourcing as a persistent organizational choice that shapes firm learning and adjustment. As such, we assume that match productivities and signals are drawn after the final-goods firm chooses between outsourcing and direct employment but before it chooses which worker to match with.

For the incumbent worker, the final-goods firm observes a match signal σ_{ij}, which is equal to y_{ij} with probability $p > 0$ and is uninformative otherwise (i.e., equal to y_j with probability π). This assumption captures the idea that incumbents are older workers who have previously worked for the firm, either as a direct hire or an outsourced worker, so the firm has some information about their match productivity.

If a final-goods firm chooses to direct hire, it does not have any information about the entrant worker's match productivity. The entrant worker's expected product is $\mathrm{E}[y_{ij}] = \pi y_j$. Therefore, if the incumbent worker's realized signal is $\sigma_{ij} = y_j$, then his expected product is $\mathrm{E}[y_{ij} \mid \sigma_{ij} = y_j] = [\pi + (1 - \pi) p] y_j$, so it is optimal for the firm to retain the incumbent worker. However, if $\sigma_{ij} = 0$, it is optimal for the firm to hire the entrant worker.

If a final-goods firm chooses to outsource, it pays a fee c to a contract firm.[7] In return, the contract firm operates a fixed cost technology for screening workers. This technology is not directly available to final-goods firms because they lack the economies of scale in human resources—which contract firms have because they can aggregate labor demand across multiple final-goods client firms—that are necessary to invest in the fixed cost screening technology.[8]

Because of the screening technology, the contract firm can observe signals s_{ij} for all entrant workers and firms, where s_{ij} is equal to y_{ij} with probability $q > 0$ and is uninformative otherwise (i.e., equal to y_j with probability π). We assume that the contract firm's screening technology is such that it always finds a worker i with observed signal $s_{ij} = y_j$ for any firm j that chooses to outsource.[9] The chosen entrant worker's expected product is thus always $\mathrm{E}\left[y_{ij} \mid s_{ij} = y_j\right] = \left[\pi + (1 - \pi)\, q\right] y_j$, so an outsourced entrant worker always has higher expected product than a directly hired entrant worker.[10]

Proposition 1. *As outsourcing cost c falls, the outsourced share of employed workers increases, the employment of entrant workers increases, the employment of incumbent workers decreases, and total employment weakly increases.*

Proof. See Appendix A. □

Figure 1 illustrates Proposition 1 by solving for firms' behavior in equilibrium as the exogenous cost of outsourcing c increases, holding fixed all other model parameters across simulations. The intuition for Proposition 1 is simple. A lack of information about the quality of entrant workers causes direct employers to favor incumbent workers. To redress the lack of information, contract firms provide screening and matching services to guarantee a minimum match quality. Therefore, as outsourcing cost falls, the employment of entrants rises. If the expected productivity of outsourced workers is higher than that of incumbent workers, the employment of incumbents also falls.

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[7] For simplicity, we assume that c is exogenous and satisfies the contract firm's break-even constraint.

[8] A micro-foundation for this assumption is that final-goods firms cannot screen on behalf of other end firms due to conflicts of interest and diseconomies of scope.

[9] For example, if the number of worker-firm pairs matched through the contract firm is large, then the law of large numbers guarantees that the contract firm can choose a worker with $s_{ij} = y_j$. Alternatively, the contract firm may maintain a pool of idle workers from which it can choose a worker with $s_{ij} = y_j$.

[10] To obtain interesting results, we assume that $\underline{y} < b/(\pi + (1 - \pi)q)$.

Figure 1: Illustration of Proposition 1
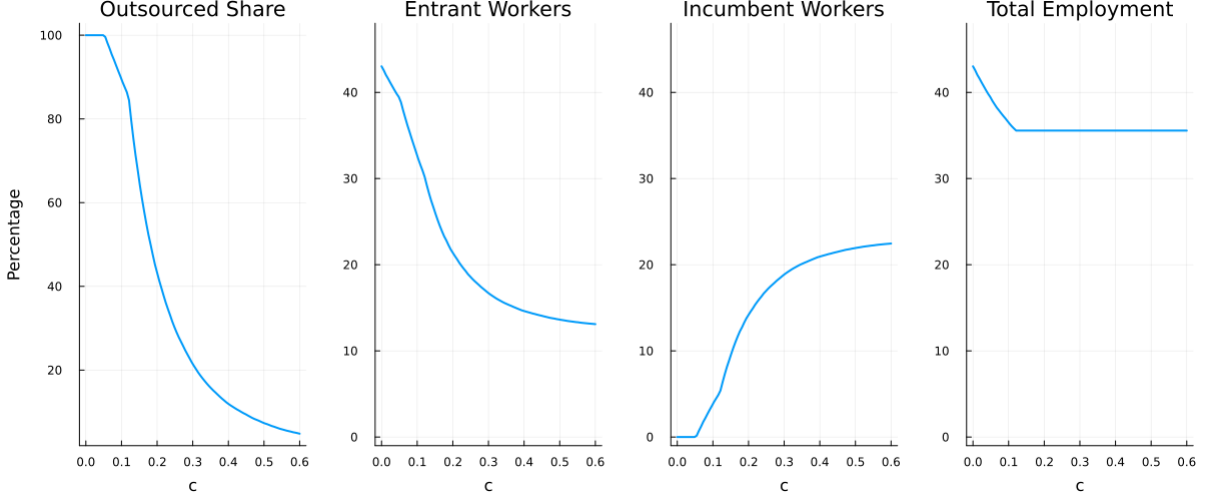
Note: This figure illustrates simulated employment patterns as the outsourcing cost c falls, fixing model parameters across simulations at $\pi = 0.6$, $p = 0.4$, $q = 0.6$, $b = 1$, $\beta = 0.5$, and $y_j \sim LogNormal(0,1)$. In the model, π is the ex-ante probability that a worker-firm match is productive; p and q are signal accuracies for incumbents and entrants, respectively; b is workers' outside option; β is firms' Nash bargaining weight; and y_j is the publicly known productivity of final-goods firm j.

Two limitations of the above model should be noted. First, the framework focuses on only one of many potential channels through which contract firms may redress labor market imperfections. By operating large and liquid internal labor markets, contract firms can achieve economies of scale not only in screening and matching but also in monitoring, reassignment, and training (e.g., Autor 2001; Guo et al. 2024; Li and Wong 2024). These alternative channels may also reallocate jobs from incumbent to entrant workers.

Second, the model captures only partial equilibrium employment effects and ignores the possibility that improvements in information can spillover across firms. It also assumes that domestic outsourcing does not alter wage setting. As shown below, we find that information spillovers are significant. We also find large wage effects for a subset of workers.

3 Institutional Setting and Data

In this section, we present institutional background and descriptive evidence relevant for interpreting our main results. We first recount the legal history of outsourcing in Brazil

and explain the variation in pre-legalization court legal interpretation. We then document trends in outsourcing, showing a disproportionate and sudden rise among security guards. Finally, we introduce the data and compare the observed characteristics of outsourced and direct-hire security guards.

3.1   History of Outsourcing in Brazil

Outsourcing emerged as a new business practice of uncertain legality in Brazil during the second half of the 20th century. In 1967, the Brazilian dictatorship issued Decree-Law 200, which allowed government bodies to outsource non-governmental functions but made no provisions for the legality of outsourcing by private sector firms. This legislative vacuum posed a major problem for lawsuits brought by workers who appeared to be outsourced, for which a key question was: who is the lawful employer—the contract firm or the client firm? The answer determines which firm is responsible for complying with Brazilian labor regulations regarding the outsourced worker's pay, benefits, and employment protection.

As lawsuits involving third-party contracting emerged, in 1986 the Superior Labor Court issued Enunciado 256, a one-paragraph precedent stating that the Court understood the practice of outsourcing to be illegal except in cases permitted by legislation.[11] Regional differences in judges' stances on the legality of outsourcing nevertheless persisted, a phenomenon we discuss in Section 3.2 and leverage for our empirical strategy in Section 4.1.

The uncertain legality of most outsourcing practices finally came to an end on December 17, 1993. Following an unanticipated series of events, the Superior Labor Court issued Súmula 331,[12] a detailed and sweeping precedent that declared the outsourcing of all non-core activities by any firm to be legal. Henceforth, outsourced workers would be considered legal employees of the intermediary firm so long as the service they provided was deemed a non-core activity of the client firm.[13]

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[11]The exceptions were a) outsourcing by the government (Decree-Law 200); b) temporary work of demonstrated need and no more than three months (Law 6.019 of 1974); and c) banking security (Law 7.102 of 1983, which required banks to offer safe storage and operational facilities to their clients).

[12]These events concerned a political crisis surrounding the investigation by the Labor Prosecution Office of the allegedly illegal outsourcing of typists by Banco do Brasil, the country's largest bank. See Biavaschi and Droppa (2011) for a historical account of the events leading to Súmula 331.

[13]In terms of compliance with Brazilian regulations, the client firm would only be liable for these obligations if the intermediary went bankrupt.

As a consequence of Súmula 331, the expected legal cost of outsourcing workers sharply fell. Prior to legalization, a firm in a region where judges considered outsourcing illegal was discouraged from outsourcing because, should an outsourced worker sue them for any reason, the firm could be found liable not only for the alleged damages but also for any penalties related to the illegal practice of outsourcing. While some firms might have still found it profitable to outsource before legalization, the high expected legal costs likely discouraged many firms from doing so.

3.2 Regional Variation in Pre-Legalization Interpretation

According to available records and the expert opinions of leading Brazilian jurists and scholars, there was a significant difference between Southern and other labor courts' interpretations of the legality of outsourcing prior to legalization by Súmula 331. Consider two regional courts at opposite sides of the legality debate: Rio Grande do Sul (restrictive) and São Paulo city (permissive).

On the restrictive side, judges interpreted Enunciado 256 as establishing a principle of illegality for outsourcing. This implied that even some exceptions listed in Enunciado 256—such as banks being allowed to outsource security—were also illegal. According to a regional labor court justice at the time, "security guards were being replaced by guards contracted now via these firms... Our understanding was that the exceptions made under 256 were not applicable here... so I recognized the employment link directly with banks."

On the permissive side, courts' understanding of outsourcing and general leniency toward it could not have been more different. In the words of a union leader in the city of São Paulo, "[T]he high frequency of lawsuit losses ended up wearing down the Unions, because as we could not win lawsuits the employers made sure to promulgate: 'you see! The labor court considers outsourcing legal!'" Appendix Tables B.2 and B.3 provide many more of these quotes, taken from transcripts of interviews with former regional court justices, judges, lawyers, and union leaders.[14]

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[14]The transcripts were generously provided by Magda Biavaschi (former Regional Court Justice and jurist) and Alisson Droppa (a legal historian), who between 2008 and 2011 interviewed judges, lawyers, and other parties involved in key lawsuits about outsourcing at various regional courts for a research project on regional differences in courts' stances on outsourcing prior to Súmula 331 (i.e., Barros Biavaschi and

The Southern courts' restrictiveness toward outsourcing is also reflected in their regional labor courts' legal precedents. Appeals concerning outsourcing made to the Superior Labor Court prior to Súmula 331 show that the Southern courts tended to recognize end firms as the legal employer.[15] They also indicate that outsourcing tended to be more frequently litigated in the South.

Combined, the interviews and legal precedents point to very restrictive interpretations in Brazil's geographic South,[16] including the states of Rio Grande do Sul (4th regional court), Paraná (9th), and Santa Catarina (12th), and a restrictive—though to a lesser extent—interpretation in the countryside of the state of São Paulo (15th region, Campinas),[17] in contrast with the permissive city of São Paulo (2nd region) as well as permissive interpretations in the rest of Brazil. Therefore, as further explained in Appendix B, we define Southern regional courts (4th, 9th, 12th, and 15th) as restrictive toward outsourcing prior to legalization and the remainder as permissive. Although we are uncertain about the correct classification of the 15th region, our local labor market results leveraging this regional variation are robust to dropping observations from the state of São Paulo (see Appendix Table D.5).

Aggregate trends confirm that outsourcing legalization had a much larger impact in restrictive jurisdictions. Figure 2, Panel A shows that as of December 1992 (the year before legalization), roughly 32% of security guards in restrictive regions were employed by contract firms, compared to 38% in permissive regions. By December 1999, this gap had been fully

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de Andrade Baltar 2013). We also separately interviewed Dr. Biavaschi and a current Regional Court Justice in a North region. Appendix B discusses how we learned of the regional differences in the courts' interpretations and gathered supporting evidence.

[15]Appeals to the Superior Labor Court are rare, especially on a specific topic such as outsourcing. That most appeals concerning outsourcing come from Southern courts indicates more active litigation of that topic in the region. Details on each appeal are provided in Appendix Table B.4.

[16]While evidence that Southern courts were more restrictive abounds, the reasons why are less clear. One possibility is the South's leftist legal tradition, with Rio Grande do Sul being the center of the Alternative Law Movement, an intellectual movement that emerged in the 1980s and was grounded on Marx's critical theory of law (Barreto and de Lyra, 2016). Another possibility is the influence of Italian and German immigration on the region's positive attitude toward labor unions (Batistella, 2009).

[17]Legal historian Alisson Droppa reported that the "15th region had a more flexible view [than the 4th region]... but even so, in ... pre-331 period, [it] had a very strong focus on resist[ing] outsourcing." A lawyer from the 15th region at the time reported that the increase in outsourcing in the region "was intensified starting in 1994, at time of the Real Plan, but also the time when there was a change in the Superior Court's understanding of the topic. (...) [O]ne cannot underestimate the ability that legal decisions have to influence how firms behave." See Appendix Table B.2.

closed, with contract firms accounting for 55% of all security guard employment in restrictive regions, compared to 53% in permissive regions. The increase in the prevalence of outsourcing in restrictive regions was a stark break from an otherwise flat trend. This contrasts with the pattern observed in permissive regions, where outsourcing experienced secular growth throughout the period, with no apparent trend breaks following legalization. Appendix Figure C.3 displays regional-court-specific trends, also showing clear trend breaks in each of the restrictive jurisdictions.

3.3 Focus on Security Guards

We focus on security guards because restrictions on outsourcing were particularly binding for this occupation prior to legalization. Relative to other occupations, guards are primarily formal,[18] face stricter licensing,[19] and have strict training requirements.[20] These factors facilitate the enforcement of local courts' decisions on security outsourcing.

Consistent with these particularly binding restrictions, security guards were the only major occupation to experience a large rise in outsourcing after legalization, as shown in Figure 2, Panel B. Each line shows the trend in the share of private sector workers employed in contract firms (i.e., the "contract-firm share") for each occupation,[21] averaged across microregions. Before 1993, there are two occupations—security guards and cleaners—whose contract-firm share far exceeds that of other occupations. In the years immediately following

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[18]Appendix Figure C.1 shows that roughly 80% of all guards were formal during this period (87% to 70% from the early 1980s to the early 2000s) versus 40%–44% for all workers (see Tables 4.8 and 4.9 in IBGE (2000), calculated including self-employed workers and excluding military, the government, and domestic work). Manager accounts from field interviews also suggest a demand-driven reason for why formality might be higher among guards: formal contracts might help attract and retain guards who managers trust more, partially mitigating high recruitment costs to screen for trust and reliability.

[19]Guards must have no criminal records, be Brazilian and at least 21 years of age, have completed at least the 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 for enlistment at age 18, though most are dismissed).

[20]Guards must complete mandatory security services training administered by Brazil's Polícia Federal (equivalent to the Federal Bureau of Investigations in the United States).

[21]We use two-digit occupation codes to identify broad occupational groups. Security guards are identified as private sector workers under Congressional Budget Office (CBO) two-digit occupation code 58, "Security and public safety workers." We exclude police officers (CBO three-digit code 583) from our definition security guards as police officers are public sector workers. We use the CBO and CBO94 occupation codes that are consistent for the period 1985–2002, prior to a major revision in occupation codes in 2003 (CBO02 codes). See Appendix Table C.1 for a list of two-digit large occupations included as comparison occupations in local labor market analyses.

Figure 2: Trends in contract-firm share

A. By court permissiveness

B. By occupation
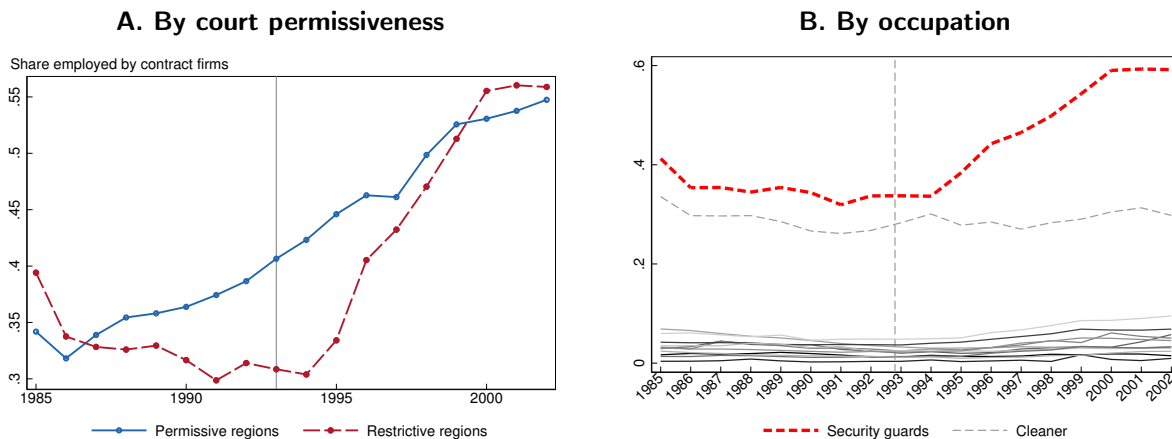
Note: Panel A plots the trend in the share of private sector security guards in the formal sector working for contract firms, separately for permissive and restrictive regions. Each line in Panel B shows the share of private sector workers employed in contract firms for an occupation in restrictive regions, including only major occupations and microregions that are in our estimation sample, which is described in Section 4 and tabulated in Table C.1.

legalization, the contract-firm share indeed rose by more than 20 percentage points (p.p.) for security guards, from an average of 32 p.p. By contrast, the contract-firm share for cleaners hardly changed.[22]

Security guards are also an economically important occupation in Brazil. Overwhelmingly male and relatively high-paid,[23] they accounted for 3.1% of total private sector formal employment in 1993 and were employed across various sectors.

Figure 3, Panel A plots the trend in the share of establishments with at least 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.[24] In 1992, about 60% of wholesale establishments had at least one security guard on staff, and by 2002, fewer than 40% did.

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[22]Section 6 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 smaller and short-lived. This could be due to cleaners being primarily employed in the informal sector, where regulatory changes are less binding.

[23]The vast majority of security guards in our data are also in indefinite-duration (as opposed to temporary) full-time contracts. See Table 1 for more descriptive statistics of the occupations.

[24]An 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.

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Figure 3: Trends in security guard employment

A. Establishments directly employing guards

B. Total security guard employment
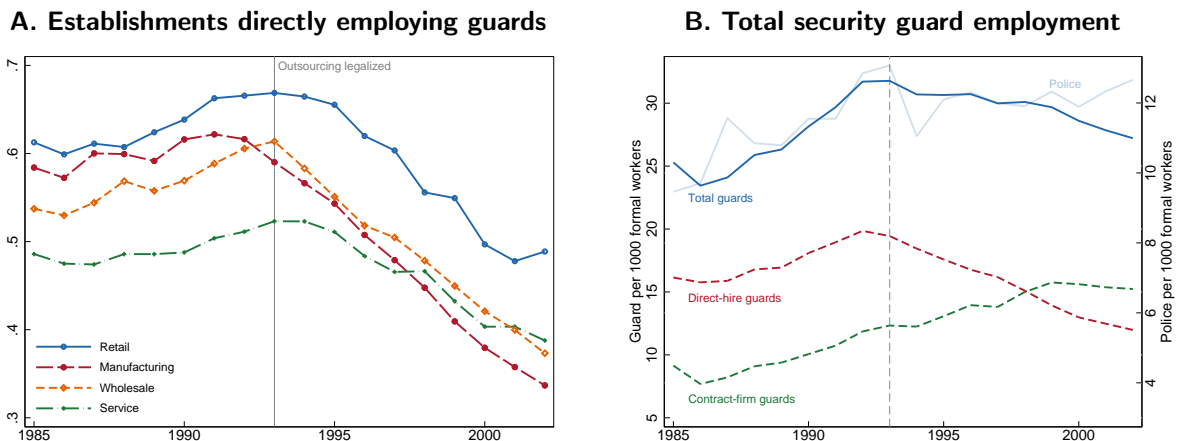
Note: Panel A plots the share of establishments with at least 50 employees in the respective sectors that employ at least one security guard. Panel B plots the total number of guards, the number of contract-firm guards, the number of direct-hire guards, and the number of police (aged 18–64) per 1,000 formal sector workers in Brazil over time.

Panel B shows a concurrent increase in contract-firm employment, confirming that many of these firms must have contracted out their needed security services and thus no longer directly employed security guards.

3.4 Data and Measurement of Outsourcing

We use Brazil's employer–employee matched administrative data, Relação Anual de Informações Sociais (RAIS), covering 1985–2002, which track the universe of Brazil's formal sector workers. For each matched worker-establishment pair, RAIS 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), and detailed industry and occupation codes. Following standard practice in the literature, we focus on full-time private sector workers aged 18–64.

Our measurement of outsourcing uses the fact that RAIS includes specific industry codes for contract firms.[25] This allows us to identify outsourced workers as those employed by

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[25]See Appendix C for the five-digit codes of contract firms, all of which fall under occupational class 74, "Serviços prestados principalmente às empresas." To consistently classify the industry of establishments over time, we use crosswalks along with our best judgment.

contract firms and direct hires as those employed by any other private sector firm. Finally, the high degree of formality among security guards allows us to track both incumbents and newcomers using employer–employee links.

Despite its richness, RAIS has three important limitations. First, while our analysis focuses on a primarily formal occupation—security guards—RAIS lacks data on workers who are not formally employed. Thus, while we test whether outsourcing pushed incumbent workers out of the formal sector, we cannot discern whether this transition was due to unemployment or informality. Second, RAIS does not include data on where outsourced workers are posted. As a result, we cannot focus on estimating "outsourcing premia," that is, the wage difference between being outsourced relative to being directly hired for the same worker performing the same job at the same firm.[26] Finally, RAIS lacks information on non-wage components of compensation (such as access to employer-based private health insurance).

4   Market-Level Effects of Outsourcing Legalization

In this section, we estimate the market-level effects of outsourcing legalization using cross-region and cross-occupation variation in the bindingness of legal restrictions against outsourcing. Unlike previous approaches, our novel strategy captures the effects of outsourcing legalization on the entire occupation. We find that legalization (i) increased the prevalence of outsourcing, (ii) increased guard employment, and (iii) reallocated guard jobs from older to younger workers, changing the occupation's demographic composition. We find no effects on demographic-adjusted wages and a very small but statistically insignificant negative effect on raw wages (i.e., wages without controlling for demographics or worker fixed effects). All of these effects are long-lasting.

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[26]Recent estimates of this kind for the Argentinian context have been reported by Drenik et al. (2023), who estimate that firms that typically pay 10% wage premia to their workers pay only 4.9% premia when the same worker is under a temp-agency contract instead.

4.1 Empirical Strategy

We exploit the fact that legalization was most binding for guards in restrictive regions to implement a DDD research design. That is, we compare the outcomes of guards relative to those for other occupations in restrictive versus permissive regions before and after legalization. Our main regression specification is

$$y_{ort} = \beta \left(T_{or} \times 1_{t>1992} \right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \epsilon_{ort}, \quad \text{(DDD)}, \tag{1}$$

where y_{ort} are the outcomes of interest (e.g., total log employment in occupation o in microregion r in year t), T_{or} is an indicator variable equal to one if occupation o is guards and microregion r is under the jurisdiction of a restrictive regional labor court, δ_{or} are microregion-occupation fixed effects, δ_{ot} are occupation-year fixed effects, and δ_{rt} are microregion-year fixed effects. Since treatment status depends on occupation and regional labor court, we two-way cluster the standard errors by these two dimensions.

The β coefficient in equation 1 is the average effect of legalization on guards in restrictive regions. We report this coefficient, estimated separately for various outcomes and under different sample weighting methods (more below), in Tables 2–6. As in standard DDD identification strategies, causal interpretation of β requires the assumption of parallel trends between treated and untreated units. That is, absent legalization, the outcomes of guards in restrictive regions would have followed similar trends as those in the control group.

While the parallel trends assumption cannot be tested on counterfactual outcomes, we use a standard year-by-year DDD regression to provide evidence that treated and untreated units followed parallel trends prior to the reform:

$$y_{ort} = \sum_{\tau=1985; \tau \neq 1992}^{2002} \beta_\tau \left(T_{or} \times 1_{t=\tau} \right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \epsilon_{ort}, \quad \text{(Dynamic DDD)}. \tag{2}$$

The β_τ coefficients in equation (2) break down the average treatment effect β from equation (1) into year-specific coefficients, presenting them as effects relative to the baseline year of 1992. These coefficients are reported in Figures 4–9.

It is worth highlighting the strengths of a DDD research design in the context of Brazil's

outsourcing legalization. One could have instead considered a simpler difference-in-differences (DD) design that compares restrictive relative to permissive regions but ignores the importance of comparing guards to other occupations. Alternatively, one could have considered a DD design that compares guards to other occupations but ignores the importance of comparing restrictive to permissive regions.

The issue with either DD design is the presence of national-level trends in demand for security services[27] and contemporaneous policies that differentially affected labor demand across regions, such as trade liberalization (1990–1994) and Plano Real (1994), a price stabilization reform.[28] Our DDD design allows us to control for differential trends in employment across occupations with occupation-year fixed effects δ_{ot} and for differential trends in employment across regions with region-year fixed effects δ_{rt}.

The key threat to identification in the DDD design is therefore the presence of contemporaneous policies that might have differentially affected guards in restrictive regions relative to other occupation-region pairs. In the Brazilian context, the main policy of concern is trade liberalization, which reduced employment and wages and temporarily increased crime in regions more exposed to import competition (e.g., see Kovak 2013 and Dix-Carneiro et al. 2018). Many of these regions were in the Southeast and South, overlapping with the jurisdiction of restrictive labor courts. While our findings suggest that outsourcing legalization increased employment and wages for guards in the South, some of this effect could have been driven by increased crime rates due to the negative effects of trade in the region.

To address this concern, we estimate equations (1) and (2) on a weighted sample, where the weights balance restrictive and permissive regions by their pre-liberalization exposure to import competition and other baseline covariates that might have induced differential effects of import competition exposure on the labor demand for guards in restrictive regions (i.e., homicide rates, unemployment rates, and total formal sector employment).[29] We do this by

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[27]The decade preceding legalization featured a secular increase in the employment of security guards and of police (see Panel B of Figure 3 and Appendix Figure C.2). Importantly, for our identification strategy, these differential occupational patterns were similar in restrictive and permissive regions (see Appendix Figure D.5).

[28]See Baumann (2001) for a review of these reforms. For regional effects of trade liberalization, see Kovak (2013) and Dix-Carneiro and Kovak (2017).

[29]Data on import competition exposure are from Felix (2021), and homicide rate data are from Dix-Carneiro et al. (2018).

first estimating entropy balancing weights, following the method proposed by Hainmueller (2012), and then weight each observation by its corresponding weight. Appendix Table D.1 reports summary statistics, showing that weighting observations reduces differences between restrictive and permissive microregions, even for characteristics that are not directly targeted. Appendix Figure D.1 shows the entropy balancing weights on a map.

For our weighted DDD regression, causal identification relies on the assumption of parallel trends, where treated and control units may differ in baseline levels of variables potentially correlated with treatment assignment (e.g., see Basri et al. 2021, whose method is weighted DD with entropy balancing weights). For robustness, we report estimates based on alternative weighting methods (inverse propensity weights) and alternative sets of balancing covariates. We also report estimates using a combination of weighting and regression adjustment (i.e., adding time-varying controls for pre-period microregion characteristics), which are known to be "doubly robust" to misspecification (Glynn and Quinn 2010).

Finally, because security guards are not employed in all microregions—many of which are rural—we estimate equations (1) and (2) on a balanced sample of 265 microregions and other large two-digit occupations present in all microregions.[30] Appendix Table C.1 lists these 11 comparison occupations and their characteristics. Our estimation sample covers roughly 400,000 security guards per year (or 98% of all formal sector guards) and roughly 8.5 million control occupation workers per year (over 50% of total private sector formal employment).

4.2 Findings

We now present our estimates of the market-level effects of outsourcing legalization based on the DDD design presented in Section 4.1. In Figures 4–9, Panel A plots coefficients β_τ of equation (2) for each respective outcome. To help visualize the identifying variation for these coefficients, Panel B plots the average outcome differences underlying them. Specifically,

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[30]We construct our balanced microregion-occupation estimation sample as follows. We select all microregions with at least 30 security guards and cleaners in every year. We then select all occupations with at least two workers in every year for all selected microregions. These restrictions yield a set of microregions with similar occupational composition and in which contract firms could have in principle operated, given the underlying demand for guards and cleaners throughout the period. Finally, we exclude cleaners due to the occupation's high contract-firm share prior to legalization but lack of law bindingness (as shown in Figure 2), potentially driven by high levels of informality.

note that each DDD point estimate corresponds roughly to the difference between two DD point estimates.[31] Panel B thus plots outcome differences between restrictive and permissive regions (i.e., the cross-region DD), separately for guards (in red) and other occupations (in gray). The difference between the red and gray lines is the key variation underlying the DDD estimates.[32] We interpret these cross-region DDs as "court restrictiveness" effects that affected guards relative to other occupations.

Legalization increased outsourcing

Figure 4 plots estimates of the effect of outsourcing legalization on the prevalence of outsourcing (Panel A) and its underlying variation (Panel B). We measure outsourcing prevalence as the outsourced share of employment, that is, the share of workers in the microregion-pair employed by a contract firm.

The figure shows that relative to permissive regions, the prevalence of outsourcing among guards sharply rose in restrictive regions following legalization, with no differential pre-trends. Prevalence increased steadily through 1999, leveling off afterwards. Table 2 reports the corresponding average effect over the post-legalization years (i.e., coefficient β in equation (1)). Column (2) presents the main specification, with entropy balancing weights. It shows that legalization increased the outsourced employment share by 4.2 p.p. per year on average (or a 135% increase relative to the baseline sample mean of 3.1%).[33] Columns (3)–(4) show that these effects are similar across alternative weighting methods. Appendix Figure D.2 shows that this increase in outsourcing prevalence did not meaningfully depend on distance to the border between restrictive and permissive regions.

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[31]"Roughly" and not "exactly" because the fully saturated fixed effects in the DDD specification (e.g., occupation-region, region-time, and occupation-time fixed effects) are more flexible controls than those the difference between DD estimates would impose (e.g., first DD identified off of occupation-time variation, conditional on occupation and time fixed effects; second DD identified off of region-time variation, conditional on region and time fixed effects).

[32]Yet another way to visualize the DDD would be to plot outcome differences between guards versus others, separately for restrictive and permissive regions. One could interpret these cross-occupation DDs as "law bindingness" effects that arose differently in restrictive relative to permissive regions. For brevity, Panel B presents the visualization that follows the "court restrictiveness" interpretation, but Appendix D presents both for all major outcomes.

[33]Control occupations have a very low prevalence of outsourcing, bringing the average baseline sample down toward zero (see Figure (2)). The 4.2 p.p. increase is a 162% increase relative to the baseline control sample mean of 2.6% (non-guards in permissive regions) and a 34% increase relative to the baseline treatment mean of 12.4% (guards in restrictive regions).

Figure 4: Effect of outsourcing legalization on outsourcing prevalence

A. Legalization effect on outsourced share (DDD): Guard vs other in restrictive vs permissive
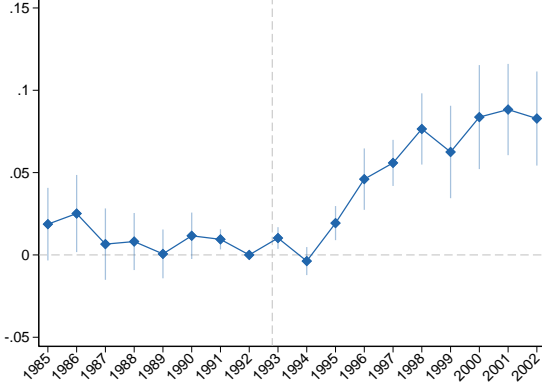
B. Restrictive vs permissive difference in outsourced share, separately plotted for guards vs other

Note: Panel A plots the coefficients β_τ from the DDD regression measuring the impact of outsourcing legalization in equation (2). Each observation is a microregion x occupation x year cell. Outsourced share is the share of all employment in a microregion-occupation pair working for a contract-firm. The omitted year is 1992. The sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation. Panel B plots α_τ coefficients from the DD regression $y_{rt} = \sum_{\tau=1985;\tau\neq1992}^{2002} \alpha_\tau (T_r \times 1_{t=\tau}) + \delta_r + \delta_t + \epsilon_{rt}$, where $T_r = 1$ if microregion r is under the jurisdiction of a restrictive regional labor court, and zero otherwise, and δ_r and δ_t are microregion and year fixed effects, respectively, separately estimated for guards and for other occupations in microregion r. A microregion's outcome for other occupations is an average across occupations, with equal weights. The resulting microregion-level regressions are weighted by microregion entropy balancing weights.

Legalization increased labor market concentration

Figure 5 plots estimates of the effect of outsourcing legalization on local labor market concentration as measured by the employment HHI (Panel A), along with the identifying variation for this effect (Panel B). Legalization increased concentration in local labor markets for security guards by 0.043 points (a 68% increase relative to the baseline mean of 0.063; see Table 2). Columns (3)–(4) show that these effects are similar across alternative weighting methods. The increase in HHI is attributable to the fact that contract firms tend to employ a large number of specialized workers (see Table 1), so a rise in outsourcing increased labor market concentration. This finding is consistent with our theory that contract firms benefit from economies of scale in human resources.

Figure 5: Effect of outsourcing legalization on employment concentration (HHI)

A. Legalization effect on HHI (DDD): Guard vs other in restrictive vs permissive

B. Restrictive vs permissive difference in HHI, separately plotted for guards vs other
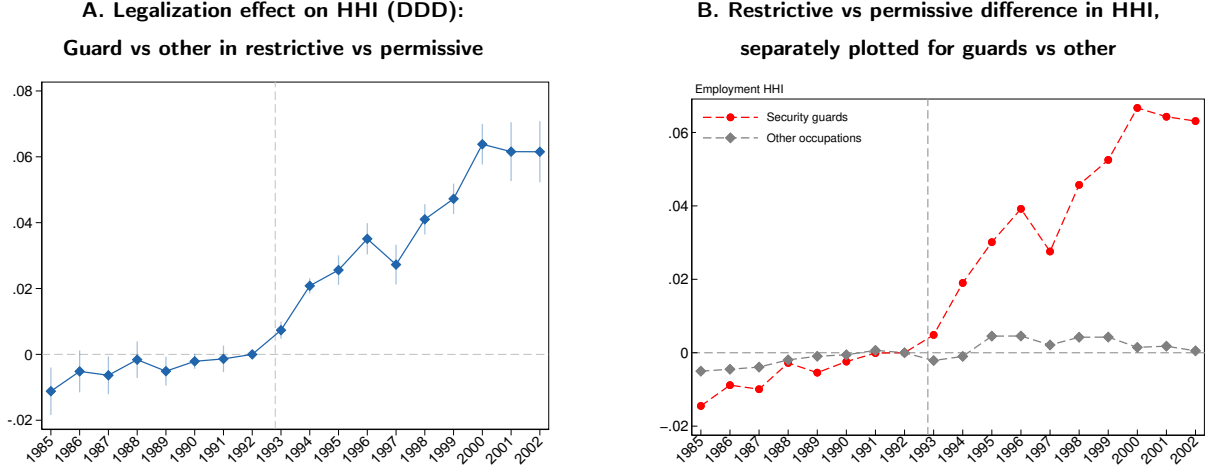
Note: See notes to Figure 4. The outcome variable is employment HHI, the Herfindahl-Hirschmann employment index (equal to the probability that any two workers work for the same firm) in a microregion-occupation pair.

Legalization increased worker entry and employment

Figure 6 plots coefficients β_τ of equation (2) for the effects of outsourcing legalization on worker entry (Panel A.1) and total employment (Panel A.2). Worker entry is measured as the log number of workers from unemployment or informality (i.e., workers who were not in the RAIS dataset in the previous year). Total employment is measured as the log total number of workers in each microregion-occupation pair, regardless of whether they are directly hired or work at contract firms.

Legalization increased worker entry from unemployment or informality into the security guard occupation by roughly 9% and total security guard employment by about 5%.[34] Panel C of Table 3 shows the precise point estimates and breaks down the total employment effect by employment origins (i.e., workers from outside RAIS compared with those already inside RAIS). It shows that the 9% entry effect on workers from unemployment or informality is three times larger than that from other formal sector jobs, of roughly 3%. Evaluated at each origin's mean baseline level, these proportional increases imply that 49% of all newcomers to the guard occupation came from unemployment or informality.

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[34]Pre-legalization effects are not jointly statistically significant.

Figure 6: Effect of outsourcing legalization on worker entry and total employment

Panel A: Dynamic DDD

A.1 Workers from unemployment or informality

A.2 Log employment

Panel B: Restrictive vs permissive differences, separately by occupation

B.1 Workers from unemployment or informality

B.2 Log employment
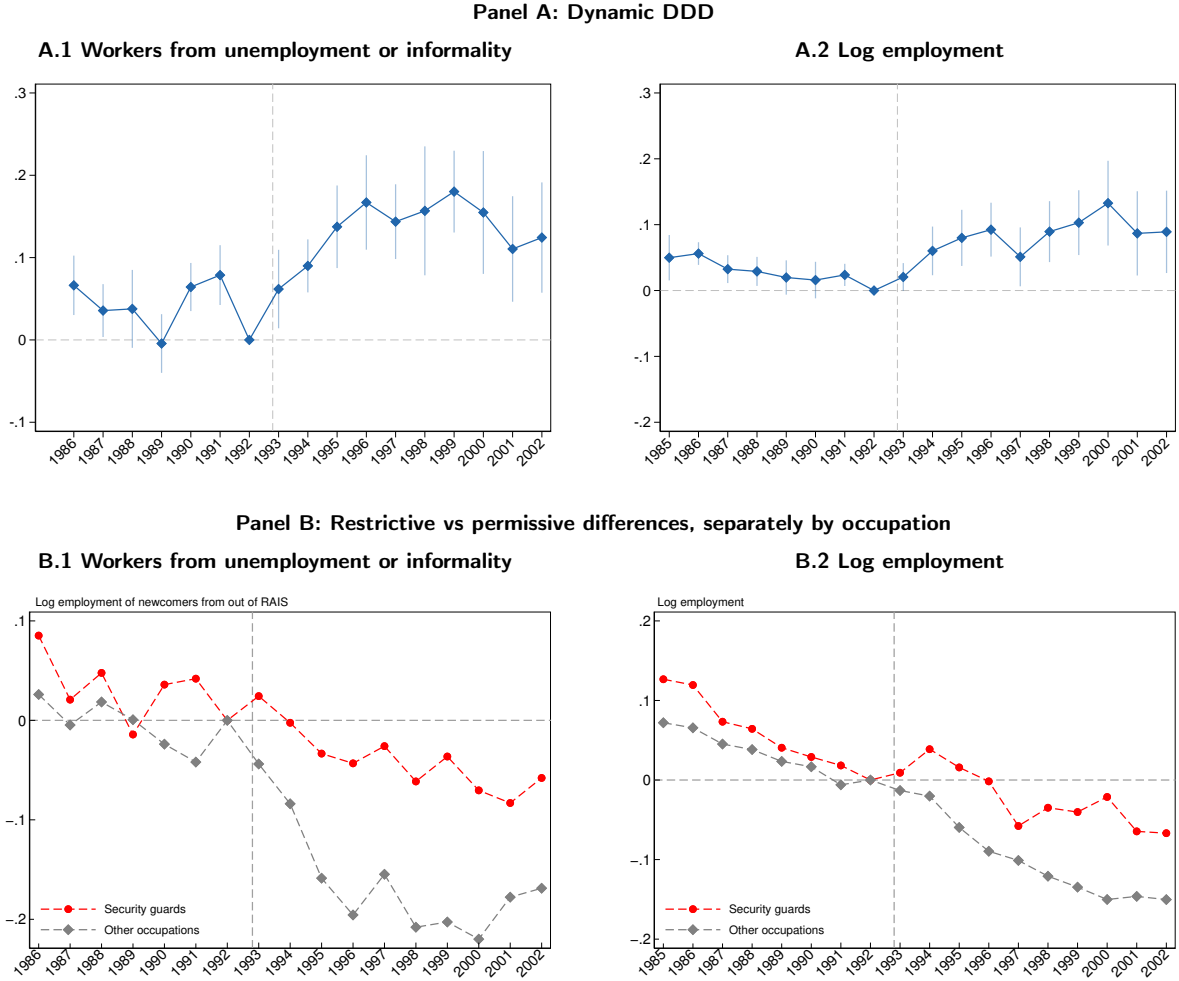
Note: See notes to Figure 4. The outcome variable in Panels A.1 and B.1 is log number of workers from outside of RAIS in the microregion-occupation pair, whereas the outcome in Panels A.2 and B.2 is log total employment in the microregion-occupation pair. Age groups are defined according to the age group variable consistently reported throughout the period.

The table shows two additional decompositions of legalization's effect on employment. First, the 5.2% increase in total guard employment was primarily driven by a large increase in outsourced guard employment,[35] as direct-hire employment declined by 4.8% (Panel B). These effects are robust to alternative weighting methods (columns (3)–(4)) and are qualita-

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[35]While our findings suggest a nearly 10% increase in outsourced guard employment, we cannot reject the null for this effect due to large standard errors, driven by many control microregion-occupation pairs having zero outsourced employment at baseline.

tively similar for alternative sets of the baseline controls used as inputs to construct balancing weights (Appendix Table D.2).

Legalization reallocated employment from older to younger workers

Figure 7, Panel A shows the effects of outsourcing legalization on guard age and employment by age groups. Legalization reduced the average age of security guards by two years (Table 4), an effect driven by a compositional shift toward younger workers. Employment increased by 42% among workers aged 18–24 and by 19% among workers aged 25–29 but decreased by 14% among those aged 50–64. As before, these effects are robust to alternative weighting methods (columns (3)–(4) of Table 4) and to alternative sets of the baseline controls used as inputs to construct balancing weights (Appendix Table D.3).

Panel B shows that for both guards and control occupations, restrictive regions were on a negative employment trend throughout the sample period, and this negative trend was of a very similar magnitude for guards and other occupations prior to legalization. However, legalization broke this negative trend for guards. After legalization, guard employment sharply increased among young workers (Panel B.1) and sharply decreased among older workers (Panel B.2) in restrictive regions. Employment in control occupations continued its pre-legalization negative trend for both age groups.

Appendix Figure D.7 shows that these patterns are very different from the effect of legalization on direct-hire incumbent guards, which are negative for most age groups, consistent with the negative effects of occupational layoffs documented in Section 6.

Effects on other workforce characteristics and employment outcomes

Table 5 shows the effects of outsourcing legalization on average workforce characteristics. Legalization reduced average tenure by 0.14 years, increased the share of male workers by 0.007, and increased average education by 0.22 years. All of these changes are statistically significant. Additionally, the increase in years of education is driven by a larger increase in the employment of workers with some high school or more compared to those with less than a high school education.

Figure 7: Effect of outsourcing legalization on workforce age composition

Panel A: Dynamic DDD

A.1 Worker age

A.2 Log employment by age groups

Panel B: Restrictive vs permissive differences, separately by occupation

B.1 Log employment for age group 18–24

B.2 Log employment for age group 50–64
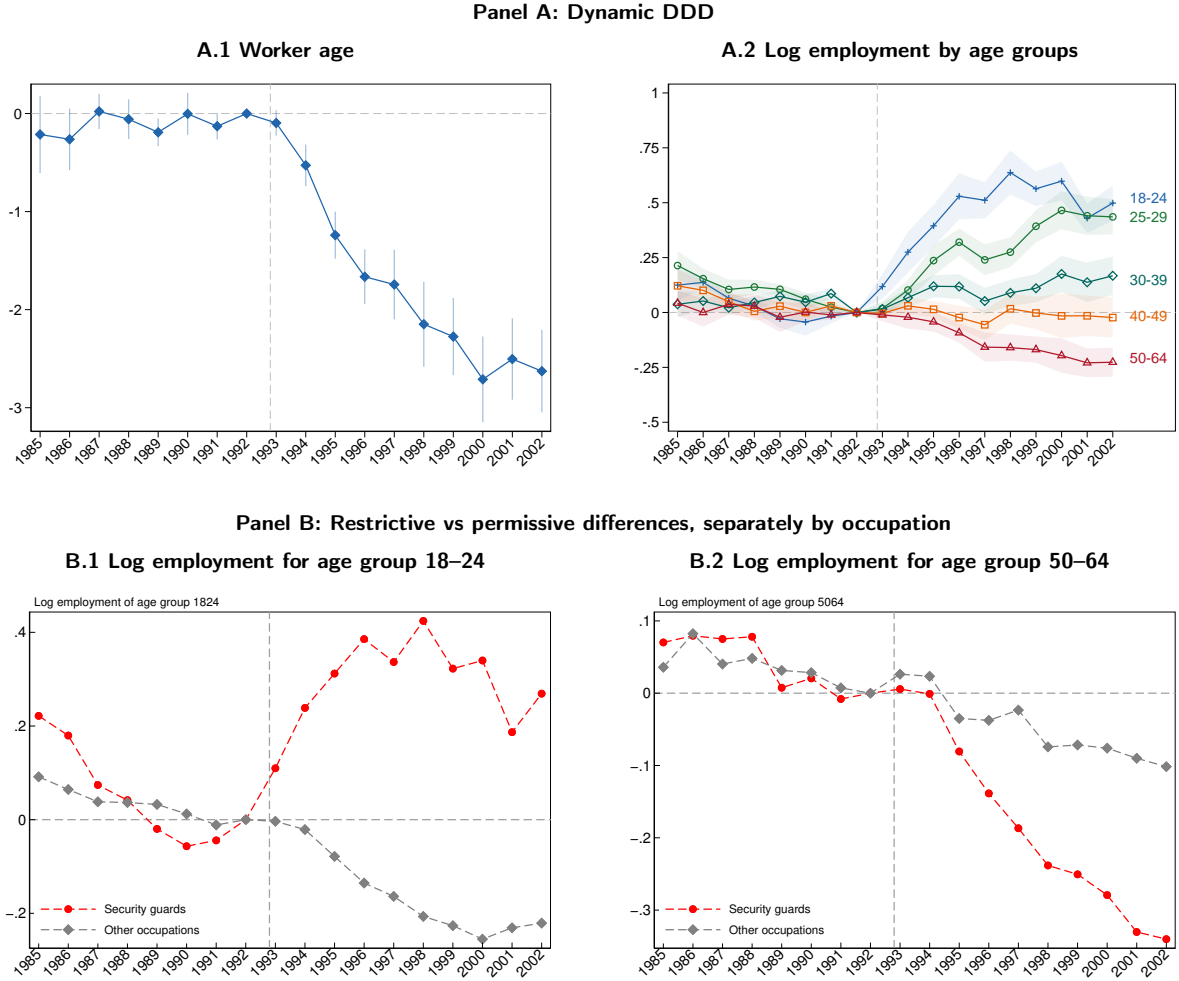
Note: See notes to Figure 4. The outcome variable in Panel A.1 is worker age, while the outcome variables in Panel A.2 are log employment by different age groups. Age groups are defined according to the age group variable consistently reported throughout the period.

Legalization had no effect on composition-adjusted log earnings

Figure 8 plots coefficients β_τ of equation (2) for the effects of outsourcing legalization on guard wages, measured as real December log earnings, the standard measure for the RAIS dataset during this period (see Appendix C). Overall, we find that outsourcing legalization had no effect on guard wages at the local labor market level (i.e., inclusive of its effect on direct-hire incumbents who stay in the guard occupation and its effect on newcomers to the

occupation).

Figure 8: Effect of outsourcing legalization on wages

Panel A: Dynamic DDD

A.1 Log real earnings (no controls, as reported)

A.2 Log real earnings (conditional on worker fixed effects)

Panel B: Restrictive vs permissive differences, separately by occupation

B.1 Log real earnings (no controls, as reported)

B.2 Log real earnings (conditional on worker fixed effects)
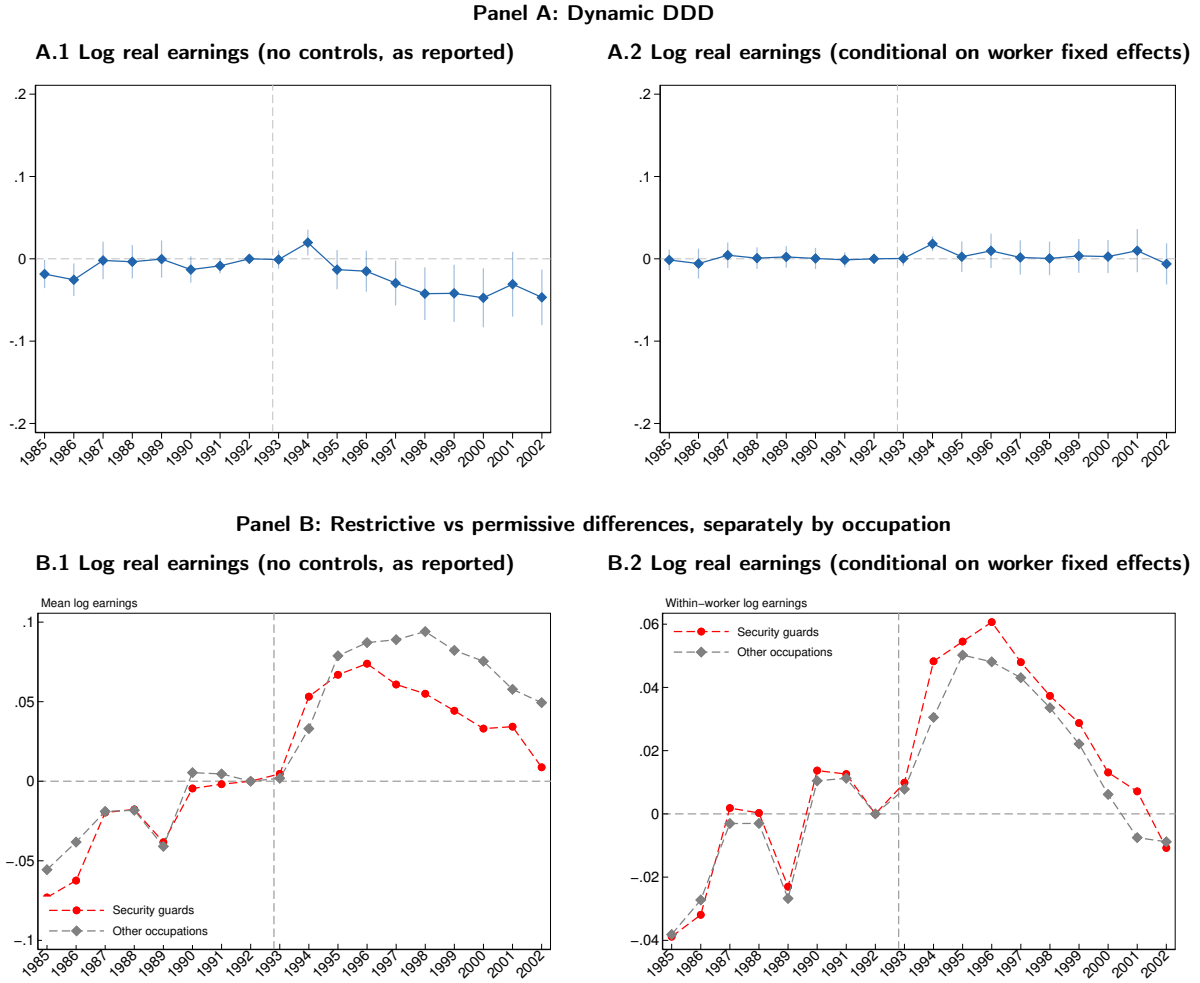
Note: See notes to Figure 4. The outcome variable in Panels A.1 and B.1 is log real December earnings as reported by each worker, without any controls. The wage outcome variable in Panels A.2 and B.2 is log real December earnings, conditional on worker fixed effects and time-varying demographic controls (i.e., education, age, and gender).

Panel A.1 shows effects on wages as reported (i.e., without any controls), whereas Panel A.2 shows effects conditional on worker fixed effects and time-varying demographic controls. Table 6 presents the corresponding average effects for these outcomes and for additional wage measures. While Panel A.1 suggests that legalization might have induced a small reduction in raw wages (potentially driven by the compositional shift toward younger workers, who are typically paid less than older workers), this effect is neither large (a 1.6% reduction)

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nor statistically significant. We find similar near-zero and statistically insignificant point estimates with alternative weighting methods (columns (3)–(4)) and with alternative sets of the baseline controls used as inputs to construct balancing weights (Appendix Table D.4).

Panel B of Figure 8 shows the DD visualizations for the wage outcomes in Figure 7 and for wages conditional on demographics only. As in the DD visualization of employment effects shown in Panel B of Figure 6, there are differential trends in wages across regions (with near-identical magnitudes for guards and other occupations). However, unlike with employment, legalization did not detectably break these trends.[36] Once again, these effects are robust to alternative weighting methods (columns (3)–(4) of Table 6) and to alternative sets of the baseline controls used as inputs to construct balancing weights (Appendix Table D.4).

Similar to recent studies (e.g., Goldschmidt and Schmieder 2017; Drenik et al. 2023), we note that our wage effects do not account for the non-wage components of worker compensation, which are not available. However, since a growing literature shows that non-wage compensation is positively correlated with wages (e.g., Taber and Vejlin 2020; Lamadon et al. 2022), our overall conclusions (and implications for welfare) may not change much if these data were available.[37]

Effects on worker transition rates and within-worker wage growth

We examine the effects of outsourcing legalization on worker transitions and within-worker wage growth. First, we measure the impact on the average likelihood that guards transitioned to informality or unemployment, across firms, and across occupations in the following year, with and without controls for observable worker demographics. We next measure the impact on yearly within-worker change in log wage, either conditional on remaining in formal sector or on remaining in the same firm. Appendix Figures D.8 and D.9 present present the

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[36]With the exception of a potential break in the trends for raw wages, though this effect is not statistically significant, as shown in Table 6.

[37]That is, we expect that incumbent guards experiencing occupational layoffs would have lost not only wage premia but also access to better workplace amenities. At the same time, half of the young men who benefited from legalization due to increased employment would likely have benefited from better amenities relative to their counterfactual (of unemployment or informal employment), whereas the other half might not have experienced any changes in workplace amenities given the null effect on wages.

results. Though we lack statistical power to rule out small effects, we do not find any clear occupation-wide impact on either worker transition rates or within-worker wage growth.

There were no differential trends in observed crime rates

Finally, we check that the increase in guard employment in restrictive regions relative to permissive regions was not driven by differential trends in crime rates. Appendix Figure D.10 shows no differences in homicide rates across regions before or after outsourcing was legalized, suggesting it did not affect homicide rates.[38]

5 Mechanisms

Having estimated the market-level effects of outsourcing legalization, in this section we provide auxiliary evidence to illuminate the underlying economic mechanisms. We first show that there are significant differences in the personnel practices of contract firms, which can partly explain why a rise in outsourcing led to persistent changes in employment structure. We then show that the increase in domestic outsourcing also altered employment composition among direct employers, suggesting that the reform also induced changes in their personnel practices.

5.1 Personnel and Wage Policies of Contract Firms

As shown in the previous section, outsourcing legalization persistently reallocated jobs from older to younger workers. A potential explanation is that contract firms persistently attract and retain a different set of workers through their personnel and wage practices. This subsection confirms that there are systematic differences between direct employers and contract firms in terms of workforce composition as well as wage and personnel practices.

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[38]Data on more relevant crime outcomes for firms, such as robberies, fall under each state's police jurisdiction and are thus not consistently reported or available for all municipalities.

Differences in firm-level workforce composition

Table 1 shows that contract firms employ a large number of security guards and few workers of other occupations, both before and after legalization. For example, before legalization, direct-hire guards worked in establishments with a median of roughly 160 employees, of whom a median of 9 were security guards. Meanwhile, contract-firm guards worked in establishments with a median of roughly 634 employees, of whom a median of 535 were security guards. These numbers strongly suggest that unlike direct-hire employers, contract firms operate large and specialized internal labor markets.

The table also shows that the mean age of outsourced guards is much lower than that of direct-hire guards, throughout the study period (34–35 versus 40 years old), both before and after legalization. Moreover, the mean tenure of direct-hire guards is 3.0–3.6 years, while that of outsourced guards is 2.1–2.3 years, and a larger share of outsourced guards have less than one year of tenure (0.4 versus 0.35).

Differences in wages

To test whether contract firms have different wage policies, we follow Dube and Kaplan (2010) and estimate the following worker-level regression using the universe of security guards during the post-legalization period (1997–2002):

$$y_{it} = \gamma O_{it} + X_{it}\beta + \tau_t + \alpha_i + \epsilon_{it}, \tag{3}$$

where y_{it} are worker outcomes such as log wage and transitions, O_{it} denotes contract-firm employment, X_{it} denotes worker characteristics (such as age and education), τ_t denotes year fixed effects, and α_i denotes worker fixed effects.

Appendix Table E.1 shows that the wages of outsourced security guards are only about 1% to 2% less than that of direct hires, after controlling for both observed and unobserved worker heterogeneity.[39] However, outsourced guards experience substantially less tenure-

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[39]These estimates are smaller and less negative than those found in other occupations and settings. For example, outsourcing wage differentials estimated for low-wage occupations in high-income countries are also larger in magnitude, typically ranging from –10% to –25% (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017). This is potentially attributable to the fact that security guards are a medium-wage, highly professionalized occupation. Using the same data but for a later period (2003–2010), Guo et al.

based wage growth, of about one-third. These findings are consistent with the idea that contract firms can easily find replacement workers to satisfy labor demand and are therefore less reliant on backloaded wage payments to incentivize retention.

Differences in retention and occupational switching

To test whether contract firms have systematically different personnel policies, we follow Guo et al. (2024) and estimate the following linear probability model using the universe of security guards during the post-legalization period (1997–2002):

$$y_{it+1} = \gamma O_{it} + X_{it}\beta + \tau_t + \epsilon_{it} \mid e_{it} = 1, \tag{4}$$

where y_{it+1} is a dummy indicating whether a transition occurred (such as exit from formal sector, or occupation switching), O_{it} denotes contract-firm employment, X_{it} denotes worker characteristics (such as age and education as well as unobserved ability, as proxied by AKM worker effects), τ_t denotes year fixed effects, and e_{it} indicates being employed in year t.

The results, presented in Appendix Table E.2, confirm that outsourced workers face different personnel policies. Outsourced workers exit from the formal sector at a roughly similar rate, after controlling for observed worker characteristics. However, outsourcing is associated with lower exit among workers during their early years of tenure. As argued by Guo et al. (2024), these patterns are consistent with the idea that contract firms enable the flexible reallocation of workers across firms, thereby reducing the need to retain incumbent workers.

Appendix Table E.3, Column (1) shows that outsourced workers have 2.6 p.p. lower annual rates of occupation switching after controlling for worker characteristics. Column (2) shows that the difference is not related to tenure, while Columns (3) and (4) show that it is entirely driven by workers who remain in the same firm. Appendix Table E.4 shows that outsourced workers also have a lower probability of becoming managers. These differences are consistent with the idea that contract firms are highly specialized and therefore offer fewer opportunities for occupation change and promotions.

(2024) estimates that outsourced guards in Brazil earn only 1.3% less than direct hires, while the wages of outsourced cleaners are 11% lower than those of similar direct hires.

Figure 9: Effect of outsourcing legalization on direct-hire employment

A.1 Log direct-hire employment by age group

A.2. Log direct-hire employment
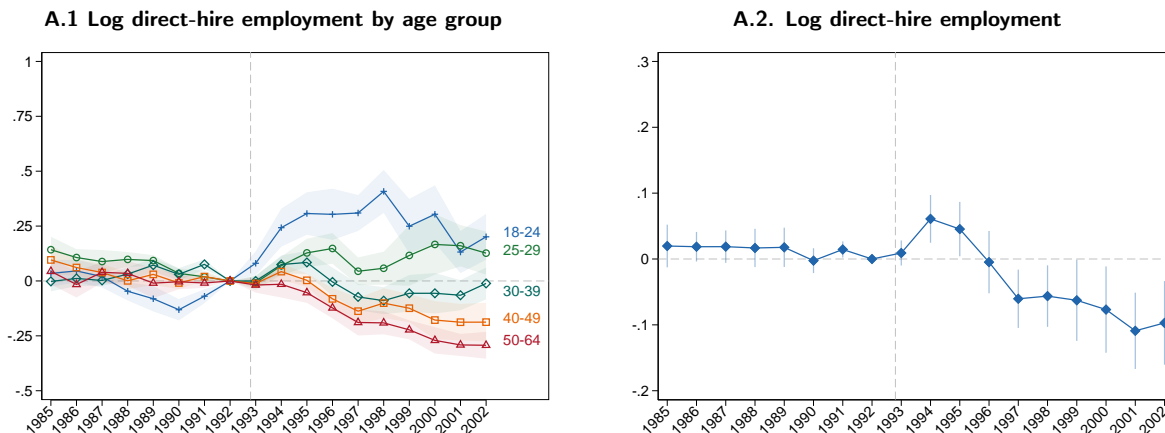
Note: See notes to Figure 4. The outcome variables in Panel A.1 are log direct-hire employment by different age groups, while the outcome variable in Panel A.2 is log direct-hire employment.

5.2 Employment Spillovers to Non-Adopting Firms

The previous subsection showed systematic differences in the outcomes of outsourced and direct-hire workers not explained by worker characteristics. These firm-level differences may partly explain the observed market-level effects. However, outsourcing legalization may also alter the personnel composition in direct employers since the increase in contract firms' large-scale recruitment appears to have attracted positively selected young workers into the security guard occupation.

We assess the relevance of such spillovers by examining whether outsourcing legalization altered the demographic composition of direct-hire workers. Panel A of Figure 9 shows that outsourcing legalization persistently increased the direct employment of younger workers and reduced that of older workers. This finding is inconsistent with the simplistic view that outsourcing legalization only affected employers that switched to outsourcing. Rather, they suggest the presence of spillovers to non-adopting firms: the rise of domestic outsourcing indirectly eased the hiring constraints even for direct employers.

Panel B shows a temporary increase in total direct-hire employment immediately after outsourcing legalization, before direct employment fell in the longer run. This result is sur-

prising but robust to alternative definitions of contract-firm employment (Appendix Figure D.11). It is likely attributable to the fact that worker entry into the occupational labor market immediately increased, thereby indirectly filling vacancies of direct employers.

6 Firm-Level Outsourcing Events

This section supplements the above results by analyzing the incidence and impact of firm-level outsourcing events in Brazil, as identified using accepted empirical methods. Three main findings are reported. First, outsourcing legalization led to a large wave of occupational layoffs, while on-site outsourcing was rare. Second, occupational layoffs temporarily reduced the employment of some incumbent security guards and persistently reduced their wages, resulting in an average loss equal to slightly more than one year of earnings. Most of these workers did not return to formal employment as security guards, being reallocated to other occupations. Third, high-wage firms were more likely to have occupational layoffs following legalization, and the loss of firm wage premia substantially explains the decline in wages for incumbent workers affected by the layoffs.

These findings confirm that outsourcing legalization had significant reallocation effects. In particular, they reveal that outsourcing legalization resulted in a large negative impact on a subset of incumbent workers, in large part due to the loss of firm-specific wage premia. They also caution that firm-level wage estimates can differ substantially from market-level wage effects, especially if they focus on a highly selected sample of workers.

6.1   Incidence of Occupational Layoffs and On-Site Outsourcing

Much existing evidence on outsourcing is based on firm-level outsourcing events (e.g., Goldschmidt and Schmieder 2017). Motivated by this literature, we examine whether outsourcing legalization led to a rise in firm-level outsourcing events. To identify outsourcing events from the data, we follow the methods proposed by Goldschmidt and Schmieder (2017) and examine both *on-site outsourcing events*, wherein a large number of workers flow from a direct employer to a contract firm but presumably continue to perform the same job, and *occupational layoffs*, wherein an establishment drastically reduces their number of direct employees

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in a given occupation, while other occupations are seemingly unchanged.

We find that on-site outsourcing events were very rare in Brazil. Between 1990 and 2000, we identify 107 on-site outsourcing events in the security guard occupation, defined as the flow of at least three security guards from a direct employer to a security services establishment.[40] These events affected 2,842 security guards, about 0.7% of the nearly half a million security guards in the country.

The rarity of on-site outsourcing is noteworthy as prior literature uses on-site outsourcing events to estimate the effects of outsourcing. 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 of workers through an intermediary to perform the same job but at a lower wage.[41]

Occupational layoffs are much more common and can account for a large share of the decline in direct-hire employment after outsourcing legalization. We define occupational layoffs as a two-thirds reduction in the number of workers in a specific occupation from an establishment with at least three workers in the occupation, excluding establishments where non-guard employment fell by more than 10%, as detailed in Appendix F.1.[42] Between 1990 and 2000, the number of occupational layoffs averaged 471 per year and affected 35,544 security guards, about 8.4% of security guards in the nation, and about 72% of the decline in direct-hire security guard employment during this time.

Figure 10 plots estimates of the establishment-level likelihoods of an occupational layoff or an on-site outsourcing event in each year. It shows a large wave of occupational layoffs immediately after legalization and hardly any increase in on-site outsourcing. Both are stable prior to 1993, the year of outsourcing legalization. In the year after legalization, the incidence of on-site outsourcing increases only mildly, whereas the incidence of occupational layoff rises sharply by 2.5 p.p. in 1994—more than half the pre-legalization level. The

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[40]Appendix F.1 details our definition, which ensures that the outsourcing event did not coincide with firm-wide layoffs. Our definition is less stringent than that of Goldschmidt and Schmieder (2017). Using their definition, we identify only 27 on-site outsourcing events in the security service industry between 1990 and 2000. These events affected only 1,061 security guards, less than 0.25% of security guards in the nation.

[41]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.

[42]See Appendix Figure F.1 for the establishment-level employment of guards (Panel A) and other occupations (Panel B) in the years leading to and following an outsourcing event.

Figure 10: Occupational layoffs vs. on-site outsourcing events following legalization
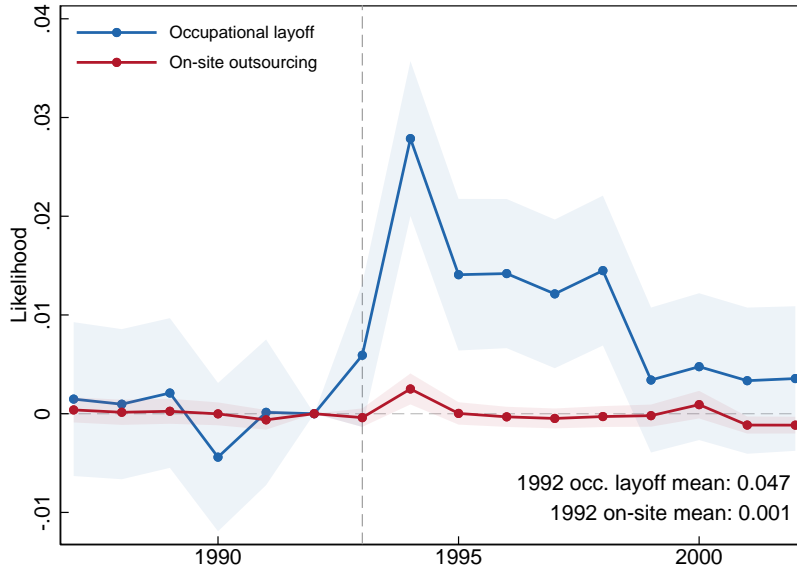
Notes: This figure plots coefficients from a linear probability model where we regress a dummy indicating the occurrence of an occupational layoff or an on-site outsourcing event on year fixed effects, relative to the omitted year of 1992, with controls for microregion fixed effects. Our sample includes 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% in the subsequent year. We exclude manufacturing establishments because they were heavily affected by trade liberalization in the early 1990s. Standard errors are clustered at the establishment level.

elevated layoff rate persists for several years before slowly returning toward baseline. As shown in Appendix Figure F.2, the increase was larger in Brazil's South, where courts were more restrictive toward outsourcing prior to legalization, than in the rest of Brazil.

6.2 Effects of Occupational Layoffs on Incumbent Workers

Since the detected firm-level outsourcing events in Brazil primarily took the form of occupational layoffs, we next examine how these layoffs affect the employment and earnings of incumbent security guards. We compare long-tenured security guards who were directly affected by an occupational layoff between 1990 and 2000 to similar security guards who were unaffected by such events. Our treatment group includes all security guards who did not continue their jobs as direct-hire security guards when their employer eliminated a large portion of such jobs. These workers could either separate from the establishment or be re-

assigned to another occupation within the establishment.[43] We construct a control group consisting of security guards in non-outsourcing establishments that are similar to those in the treatment group, using a matching algorithm as described in Appendix F.2.

We use the following worker-level DD specification to estimate the effects of occupational layoffs on incumbent security guards:

$$y_{it} = \sum_{k=-4, k\neq-1}^{8} \delta_k \left(D_i \times I_{t=t^*+k}\right) + \alpha_i + \tau_t + X_{it}\beta + \epsilon_{it}, \tag{5}$$

where y_{it} is the outcome of security guard i in year t (e.g., employment status or wage), D_i indicates if the security guard was outsourced in year t^*,[44] X_{it} are demographic controls, and ϵ_{it} is a residual term. Each coefficient δ_k is the effect of an occupational layoff on an incumbent direct-hire worker k years since the layoff, relative to their matched pair at an non-outsourcing firm. Note, therefore, that these do not capture the equilibrium effects of outsourcing legalization for the security guard occupation as a whole (which includes both non-outsourced incumbents and new workers entering the occupation), which we investigate in Section 4.

Figure 11, Panel A shows that outsourcing decisions significantly displace incumbent security guards from formal employment. In the year after an occupational layoff, an affected security guard is 49 p.p. less likely to be formally employed. However, five years after an occupational layoff, there is no detectable effect on the likelihood of formal employment.

Panel B shows that occupational layoffs also persistently reduce the wages of incumbent security guards. Wages are hardest hit in the year following occupational layoffs, by 18%, and never recover to pre-layoff levels. Five years following an occupational layoff, security guards are still paid 12% less than they would have been paid had the occupational layoff event not happened. Since by this time the formal employment rates of the treated and control groups no longer exhibit any detectable differences, worker selection is unlikely to

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[43]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).

[44]Measured by an occupational layoff occurring at the guard i's firm in year t^*.

Figure 11: Effect of occupational layoffs on incumbent workers

A. Formal employment DD

B. Wage DD
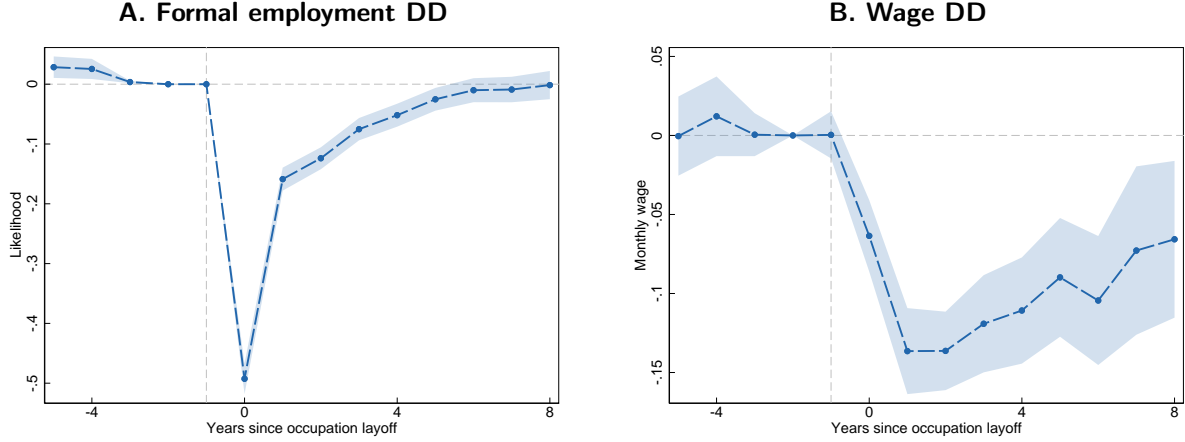
Note: Panels A and B plot coefficients γ_τ from a DD regression measuring the impact of an occupational layoff on incumbent direct-hire security guards, where the control group consists of similar workers in establishments without an occupational layoff. Our sample includes all occupational layoffs, 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.

explain the drop in wages.[45]

The effect of a firm-level outsourcing event on incumbent guards is different from the effect of legalization on guard wages at the local labor market level, presented in Section 4, for two reasons. First, the effect at the local labor market level includes effects on newcomer guards, while the effects in this section focus only on impacted incumbents. Second, the effect at the local labor market level is restricted to the labor markets for guards, so it does not include wage declines driven by incumbent workers leaving the occupation.

Appendix Table F.2 shows that a large fraction of affected workers transition to other occupations, while only a small proportion end up in contract-firm employment. Immediately after an occupational layoff, impacted workers are 76 p.p. less likely to be formally employed

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[45]Additional related results are shown in the appendix. First, while legalization increased the frequency of outsourcing in Brazil's restrictive South by more than in the permissive North (e.g., see Appendix Figure F.2), Appendix Figure F.3 shows that outsourcing events have similar effects on incumbents' likelihood of employment and wages in either region. Second, as shown in Appendix Figures F.4 and F.5, these results are robust to alternative matching strategies and definitions of an occupational layoff. Third, the effects of occupational layoffs on employment and wages in other occupations are shown in Appendix Figures F.6–F.7 and are broadly similar.

in the same occupation. Even five years later, these workers are less likely to be formally employed in the same occupation (by 12 p.p.), though they are no longer less likely to be formally employed. By contrast, immediately after a layoff, impacted workers are only 0.2 p.p. more likely to be formally employed by a contract firm. The effect of occupational layoffs on contract-firm employment rises to 7.7 p.p. in the following year and increases 11.7 p.p. five years after. Despite the large number of workers leaving the occupation, our finding of a persistent wage decline for incumbents is nearly all driven by workers who stay within the occupation, as shown in Appendix Figure F.8.

Appendix Table F.3 shows that the implied present discounted value (PDV) of earnings losses from experiencing an occupational layoff is roughly 1–1.4 years.[46] If we assume that workers earn nothing if unobserved, as described in Appendix F.3, then the workers lose 1.40 years of earnings of average pre-occupational-layoff earnings. If instead we assume they have the same earnings as observed workers, then they lose roughly 1.06 years of earnings. Regardless of the imputation method, the PDV of earning losses appears to be substantial. This suggests 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.

6.3   The Role of Firm-Specific Wage Premia

Recent literature shows that firms often outsource in order to exclude workers from the wage premia they share with direct employees (Dube and Kaplan 2010; Weil 2014; Goldschmidt and Schmieder 2017). For example, some firms may be required by collective bargaining agreements, which are typically negotiated by sector and region in the Brazilian economy, to pay high wages to employees. Highly profitable firms may also face pressure to pay workers a wage premium in the interest of fairness or equity. A firm may avoid these requirements and pressures by moving workers outside its boundary.[47]

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[46]Based on security guards with at least 3 years of positive earnings at an employer with at least 10 workers.

[47]Clement 2017 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

Figure 12: Incumbent wage reduction is related to loss of firm wage premia

A. Firm wage premium

B. Wage effect by initial firm wage premium
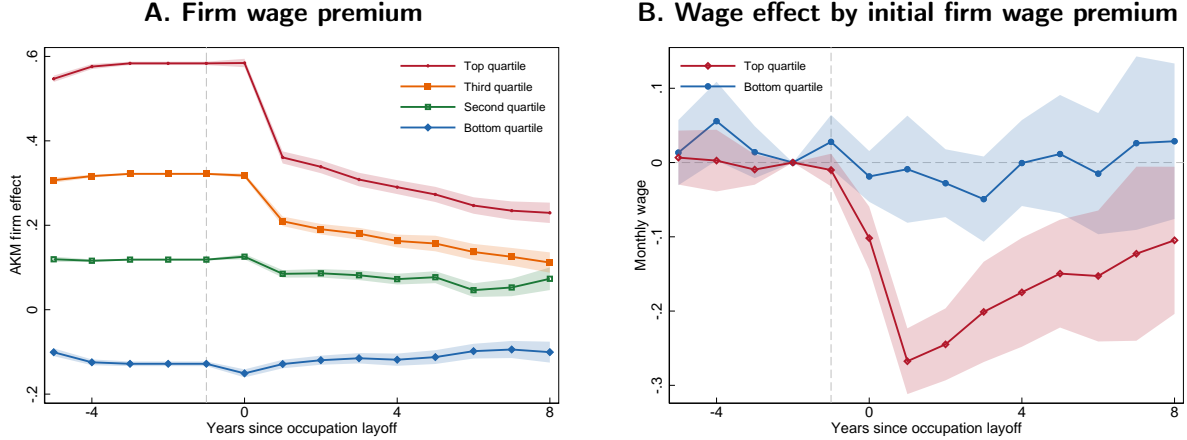
Note: Panel A plots the average AKM firm effect of incumbent direct-hire security guards in the years before and after firm outsourcing decisions, conditional on their remaining formally employed. Panel B shows DD regression estimates for the effect of outsourcing decisions on incumbent monthly wage (as a fraction of wage two years before the outsourcing event) conditional on formal employment, separately for high- 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.

This subsection confirms that the desire to reduce firm-level worker rents was an important mediator of the wage effects of firm-level outsourcing decisions. First, we estimate firm-level wage premia by decomposing log wages into worker and firm fixed effects following Abowd et al. (1999) (henceforth AKM), as detailed in Appendix F.4. Then, we use the estimated firm fixed effects to answer three questions: Do workers experiencing outsourcing decisions transition to lower-wage firms? Do workers initially at high-wage firms experience larger wage declines when their employer decides to outsource? Are high-wage firms more likely to outsource?

Figure 12, Panel A shows that incumbent workers have generally stable AKM firm effects prior to occupational layoffs, but this changes after such layoffs. Incumbent workers initially in the top quartile transition to firms with much lower firm wage premia, and their average AKM firm effect falls by almost 20%. By contrast, workers initially at lower-wage firms do not experience a significant change in firm-specific wage premia.

Panel B shows that workers initially employed at top quartile firms experience a large

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work for Joe's Janitorial Services, they no longer share in those rents."

decline in wages following occupational layoffs.[48] We detect no statistically significant effect on wages for workers initially employed at establishments in the bottom quartile of the AKM firm effects distribution. The loss of firm-specific wage premia accounts for 42% of the total wage losses in the year of the occupational layoff, 43% one year after, and 46% five years after (see the last line of Table F.2). These figures suggest that changes in firm wage premia explain a substantial share of the wage losses experienced by incumbents.[49]

High-wage establishments were also more likely to have outsourcing events. As shown in Appendix Table F.4, outsourcing events are more likely to be occur at firms with higher AKM firm effects, higher mean wages in 1993, and higher mean security guard wages in 1993. These findings are broadly consistent with those of Goldschmidt and Schmieder (2017).

However, outsourcing legalization did not reduce the average firm wage premia in the security guard occupation. As shown in Appendix Figure F.10, the average firm effect among security guards declined both before and after outsourcing legalization in both regions, but we do not detect any trend break in 1993. This finding confirms, once again, that the market-level effects of outsourcing can differ substantially from those estimated from firm-level outsourcing events, since the latter approach uses a highly selected sample of workers.

7   Conclusion

This paper investigates whether the rise of professional service firms and non-core activity outsourcing reduced labor market frictions in Brazil. Our estimates leverage Brazil's 1993 legalization of non-core activity outsourcing, which sharply reduced the cost of such outsourcing. We provide precise market-level estimates of its effects on employment and wages in a highly impacted occupation, focusing on security guards and using both cross-region and cross-occupation variation in the bindingness of labor market regulations in a

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[48]See Appendix Figure F.9 for effects on the likelihood of employment. Workers who were in the top quartile of the AKM fixed effect distribution at baseline are more likely to be disemployed (i.e., leave the data, either to informality or unemployment) on impact but return to the formal sector at similar rates as workers who were in the bottom quartile at baseline.

[49]By comparison, Lachowska et al. (2020b) estimate that firm effects explain 17% of wage losses from job displacement in the U.S., while Schmieder et al. (2020) estimate that firm effects account for 75% of wage losses from job displacement in Germany. The underlying sources of differences in the importance of firm wage premia across countries remain an open question in the literature.

triple-difference design.

We find that outsourcing legalization resulted in a large and persistent redistribution of jobs from older to younger workers. Total employment of security guards increased by 5%, with roughly half of the increase driven by workers coming from unemployment or the informal sector. However, legalization had little effect on average demographic-adjusted wages in the occupation. Importantly, while our framework's insight of reallocation from incumbent to new entrant workers is general, it is unclear whether this demographic shift is generalizable to other occupations or contexts. For example, in the context of cleaning services in the United States, contract firms may instead draw on a different form of comparative advantage by recruiting immigrants, who may not be younger but may be more efficient at cleaning than native workers. This suggests that the specific demographic reallocation depends on the nature of the task and the dimensions of comparative advantage that matter most.

We document systematic differences in personnel and wage policies between contract firms and direct employers, which help to explain our market-level results, as well as evidence consistent with increased economies of scale in managing specialized workers. Moreover, we find that legalization increased the direct employment of young workers, suggesting that hiring constraints eased throughout the occupational labor market, even for firms that directly employ. In addition, legalization induced a wave of occupational layoffs, which resulted in displacement and large wage losses for some incumbent workers initially at high-wage firms.

Our findings suggest that the rise of specialized contract firms is an important step toward overcoming labor market frictions and enabling economic development. When assessing the overall welfare consequences of outsourcing, economists and policymakers should more seriously consider the employment effects of non-core activity outsourcing alongside its wage and rent-stripping effects, especially in settings with substantial labor market frictions, such as in developing contexts.

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Table 1: Descriptive statistics of security guards by contract type

	Direct hire			Contract-firm		
	1985-1993	1994-1996	1997-2002	1985-1993	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
Long-tenured (Tenure>10 years)						
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]
Exit from formality	0.33	0.33	0.41	0.35	0.35	0.43
Occupation switching	0.06	0.05	0.04	0.03	0.02	0.02
Managerial promotion	0.002	0.002	0.001	0.001	0.001	0.001
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.

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Table 2: Effect of legalization on prevalence of guard outsourcing

| | Mean at baseline (1992) | Legalization effect (Weighted DDD) | | | Observations (region x occup x year) |
| | | Entropy-balancing weights (EB) | Inverse propensity score weights | EB with regression adjustment | |
	(1)	(2)	(3)	(4)	(5)
Outsourced share	0.031	0.042 (0.013)	0.053 (0.010)	0.038 (0.009)	57240
Employment HHI	0.063	0.043 (0.003)	0.032 (0.002)	0.042 (0.003)	57240

Notes: This table shows effects of outsourcing legalization on the prevalence of outsourcing in a region-occupation pair, estimated using equation 2's weighted DDD specification, which compares guards to other occupations in restrictive versus permissive regions before versus after legalization. Outsourced share is the share of total labor market employment that is outsourced (that is, hired by a contract firm). Employment HHI is the Herfindahl-Hirschmann employment index within a region-occupation pair. Columns (2)–(4) present estimates according to different econometric weighing methods. All weights balance restrictive and permissive regions on baseline homicide rate, import competition exposure, total formal employment, and unemployment rate. All regressions include microregion-occupation, microregion-year, and occupation-year fixed effects. The sample is balanced and includes 216 microregions and thirteen 2-digit CBO94 occupational groups (security guards plus 12 major comparison groups, present in all microregions). Standard errors are two-way clustered by Regional Labor Court and occupation.

Table 3: Effect of legalization on employment

	Mean at baseline (1992) (1)	Legalization effect (Weighted DDD)			Observations (region x occup x year) (5)
		Entropy-balancing weights (EB) (2)	Inverse propensity score weights (3)	EB with regression adjustment (4)	
Panel A: Log total employment					
Log total employment	7.213	0.052 (0.011)	0.047 (0.014)	0.051 (0.014)	57240
Panel B: Log employment by contract type					
Direct-hire	7.176	-0.048 (0.019)	-0.042 (0.018)	-0.044 (0.019)	57240
Outsourced	2.642	0.099 (0.145)	0.327 (0.131)	0.062 (0.133)	57240
Panel C: Log employment origins					
Workers from outside RAIS (e.g., unemployment, informality, nilf)	5.763	0.093 (0.026)	0.121 (0.027)	0.087 (0.027)	54060
Workers from inside RAIS	6.913	0.032 (0.010)	0.023 (0.015)	0.033 (0.010)	54060

Notes: See notes to Table 2. Log employment is expressed in natural logs.

Table 4: Effect of legalization on workforce age composition

	Mean at baseline (1992)	Legalization effect (Weighted DDD)			Observations (region x occup x year)
		Entropy-balancing weights (EB)	Inverse propensity score weights	EB with regression adjustment	
	(1)	(2)	(3)	(4)	(5)
Age	32.793	-1.649 (0.196)	-1.664 (0.125)	-1.712 (0.125)	57240
Log employment by worker age group					
18-24	5.685	0.422 (0.036)	0.400 (0.029)	0.422 (0.028)	57240
25-29	5.579	0.195 (0.031)	0.190 (0.025)	0.203 (0.020)	57240
30-39	5.993	0.060 (0.023)	0.058 (0.019)	0.061 (0.018)	57240
40-49	5.254	-0.050 (0.031)	-0.042 (0.027)	-0.056 (0.031)	57240
50-64	4.280	-0.140 (0.023)	-0.145 (0.018)	-0.148 (0.021)	57240

Notes: See notes to Table 2. Log employment is expressed in natural logs.

Table 5: Effect of legalization on additional employment outcomes and workforce characteristics

| | Mean at baseline (1992) | Legalization effect (Weighted DDD) | | | Observations (region x occup x year) |
		Entropy-balancing weights (EB)	Inverse propensity score weights	EB with regression adjustment	
	(1)	(2)	(3)	(4)	(5)
Panel A: Other employment outcomes					
Tenure	3.689	-0.142	-0.188	-0.122	57240
		(0.061)	(0.071)	(0.060)	
Newhire share (Tenure < 1 year)	0.268	0.017	0.017	0.014	57240
		(0.005)	(0.006)	(0.003)	
Panel B: Other workforce characteristics					
Male	0.705	-0.007	-0.007	-0.007	57240
		(0.002)	(0.002)	(0.002)	
Years of education	7.198	0.222	0.173	0.230	57240
		(0.030)	(0.020)	(0.028)	
Log employment by education					
Less than High School	6.657	0.074	0.057	0.074	57240
		(0.013)	(0.019)	(0.014)	
Some High School	4.645	0.254	0.156	0.252	57240
		(0.018)	(0.023)	(0.027)	
High School graduate	4.768	0.266	0.163	0.274	57240
		(0.024)	(0.008)	(0.028)	
Some College	2.757	0.200	0.189	0.221	57240
		(0.047)	(0.046)	(0.045)	
College graduate or more	3.308	-0.001	0.021	-0.008	57240
		(0.034)	(0.015)	(0.040)	

Notes: See notes to Table 2.

Table 6: Effect of legalization on wages

	Mean at baseline (1992)	Legalization effect (Weighted DDD)			Observations (region x occup x year)
		Entropy-balancing weights (EB)	Inverse propensity score weights	EB with regression adjustment	
	(1)	(2)	(3)	(4)	(5)
Panel A: Real December log earnings					
As reported (i.e., unconditional)	7.116	-0.016 (0.015)	-0.038 (0.016)	-0.013 (0.015)	57240
Conditional on worker demographics	-0.172	0.003 (0.015)	-0.019 (0.015)	0.006 (0.013)	57240
Conditional on worker FEs and time-varying demographics	-0.206	0.004 (0.012)	-0.010 (0.012)	0.007 (0.009)	57240
Panel B: Real December log earnings by contract type, as reported					
Direct-hire	7.117	-0.011 (0.014)	-0.020 (0.013)	-0.008 (0.014)	57240
Outsourced	6.937	0.003 (0.020)	0.045 (0.015)	0.004 (0.023)	43628

Notes: See notes to Table 2. Following the literature using RAIS data prior to 1995, we measure a worker's wage as their salary for the month of December of each year in their highest-paying job. That is because RAIS consistently reports each worker's total earnings, as multiples of that year's federal minimum wage, for the month of December throughout the sample period, whereas additional data on contract hours is only available starting in 1995. Post-1995 data on hours show that direct-hire guards work on average 42.4 hours a week, compared to 43.6 hours a week on average for outsourced guards. See Appendix Table 1 for additional summary statistics. We construct real December earnings by multiplying the RAIS multiples-of-the-minimum-wage earnings variable by each year's federal minimum wage, available from Brazil's Instituto de Pesquisa Econômica Aplicada (IPEA) and expressed in 2017 reais. Log earnings are expressed in natural logs.

Online Appendices

A   Conceptual Framework: Details

Proof of Proposition 1

Let $y_p = b/(\pi + (1 - \pi)p)$, $y_q = b/(\pi + (1 - \pi)q)$, and $\bar{y} = b/\pi$.

Suppose $p < q$. There are four cases:

1. Suppose $y_j < y_q$. It is never profitable for the firm to hire a worker.

2. Suppose $y_j \in (y_q, y_p)$. It is never profitable to hire an incumbent or a direct-hire entrant. If the firm hires an outsourced entrant worker, the firm's profit is $\beta\left([\pi + (1 - \pi)q]y_j - b\right) - c$. It is optimal for the firm to outsource if and only if $\beta\left([\pi + (1 - \pi)q]y_j - b\right) > c$.

3. Suppose $y_j \in (y_p, \bar{y})$. Under direct employment, firm j contracts with incumbent worker with $\sigma_{ij} = y_j$ but hires no worker if $\sigma_{ij} = 0$. The firm's profit is $\pi\beta\left([\pi + (1 - \pi)p]y_j - b\right)$. However, under outsourcing, firm i always hires the entrant worker. The firm's profit is $\beta\left([\pi + (1 - \pi)q]y_j - b\right) - c$. It is optimal for the firm to outsource if and only if $\beta\left([\pi + (1 - \pi)q]y_j - b\right) - \pi\beta\left([\pi + (1 - \pi)p]y_j - b\right) > c$.

4. Suppose $y_j > \bar{y}$. Under direct employment, firm j contracts with incumbent worker with $\sigma_{ij} = y_j$ and hires entrant worker if incumbent has signal $\sigma_{ij} = 0$. Under direct employment, firm j's profit is $\pi\beta\left([\pi + (1 - \pi)p]y_j - b\right) + (1 - \pi)\beta\left(\pi y_j - b\right)$. Under outsourcing, firm j's profit is $\beta\left([\pi + (1 - \pi)q]y_j - b\right) - c$. It is optimal for the firm to outsource if and only if $\beta\left([\pi + (1 - \pi)q]y_j - b\right) - \pi\beta\left([\pi + (1 - \pi)p]y_j - b\right) - (1 - \pi)\beta\left(\pi y_j - b\right) > c$.

In this case, as c falls, firms switch to outsourcing, the employment of entrant workers increases, and the employment of total workers increases. The employment of incumbent workers falls.

Now suppose $p > q$. There are four cases:

1. Suppose $y_j < y_p$. It is never profitable for the firm to hire a worker.

1

2. Suppose $y_j \in (y_p, y_q)$. It is strictly optimal that a firm j contracts with incumbent worker with $\sigma_{ij} = y_j$ and hires no worker otherwise. Under direct employment, the firm's profit is $\pi\beta\left([\pi + (1-\pi)p]y_j - b\right)$. Under outsourcing, its profit is $\pi\beta\left([\pi + (1-\pi)p]y_j - b\right) - c$. It is strictly optimal to directly hire. Suppose $y_j \in (y_q, \bar{y})$. Under direct employment, firm j contracts with incumbent worker with $\sigma_{ij} = y_j$ and hires no worker otherwise. The firm's profit is $\pi\beta\left([\pi + (1-\pi)p]y_j - b\right)$. However, under outsourcing, the firm contracts with incumbent worker with $\sigma_{ij} = y_j$ and hires an entrant worker otherwise. The firm's profit is $\pi\beta\left([\pi + (1-\pi)p]y_j - b\right) + (1-\pi)\beta\left([\pi + (1-\pi)q]y_j - b\right) - c$. It is optimal for the firm to outsource if and only if $(1-\pi)\beta\left([\pi + (1-\pi)q]y_j - b\right) > c$.

3. Suppose $y_j > \bar{y}$. Under direct employment, firm j contracts with incumbent worker with $\sigma_{ij} = y_{ij}$ and hires an entrant worker if incumbent has signal $\sigma_{ij} = 0$; firm i's profit is $\pi\beta\left([\pi + (1-\pi)p]y_j - b\right) + (1-\pi)\beta\left(\pi y_j - b\right)$. Under outsourcing, firm j incumbent worker with $\sigma_{ij} = y_{ij}$ and hires an entrant worker otherwise; firm i's profit is $\beta\left([\pi + (1-\pi)p]y_j - b\right) + (1-\pi)\beta\left([\pi + (1-\pi)q]y_j - b\right) - c$. It is optimal for the firm to outsource if $(1-\pi)^2\beta q y_j > c$.

In this case, as c falls, firms switch to outsourcing, the employment of entrant workers increases, and the employment of total workers increases. The employment of incumbent workers remains the same.

B Institutional Setting: Details

The fact that Brazil's regional courts differed in their permissiveness to outsourcing prior to Súmula 331 is well-known to industry insiders, but is poorly documented. During a field trip in September 2019, 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 union 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. She insistently responded that outsourcing had never been considered illegal. This response contradicted various legal articles that suggest outsourcing had been declared illegal by Súmula 256 in 1985 (da Cruz 2009; Biavaschi and Droppa 2011; Cooney et al. 2015).

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. To further investigate local court permissiveness toward outsourcing, 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. Sadly, we received answers in the negative for almost all local courts. Most no longer kept cases before 1993.

Given the obstacles in obtaining individual rulings within each regional labor court, we gathered available primary and secondary sources to glean as much information on the stances of regional courts as possible. The materials include interviews and review of individual cases appealed to the Superior Court and cited by Súmula 331. We classify the regional courts based on our best reading of these materials. We say that a court is restrictive if the materials suggest that the regional courts consistently ruled against the legality of the outsourcing arrangement. We say that a court is permissive either if they did not, or if materials are unavailable.

Appendix Table B.1 summarizes our classification of Brazil's 24 regional labor courts. Columns (5) and (6) summarize the information from legal precedents, while columns (7)–(9)

summarize the information from available interview transcripts. Columns (10)–(12) then show the prevalence of outsourcing of security guards before and following legalization for each region.

One set of supporting documents for the classification comes from research performed by Magda Biavaschi and Alisson 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. Appendix Tables B.2 and B.3 display all quotes from these interviews concerning Courts' interpretation on the legality of outsourcing.

Another set of supporting documents are regional labor court precedent rulings that were appealed to the Superior Labor Court between 1986 (when Súmula 256 was in place) and 1993 (when Súmula 331 took place), as these were centrally stored at the Superior Labor Court's archives. Appendix Table B.4 summarizes the key decisions in each ruling. Column (6) shows that Southern regional labor courts tended to find a direct employment link with the end-firm, meaning that it did not recognize the legality of the outsourcing arrangement. In contrast, Column (7) shows that the Superior Labor Court tended to reverse these regional courts' decisions, finding the direct employment link with the contract-firm instead.

The interviews and legal precedents point to very restrictive interpretations in Brazil's geographic South,[50] including the states of Rio Grande do Sul (4th regional court), Paraná (9th), and Santa Catarina (12th), and a restrictive—though to a lesser extent—interpretation in the countryside of the state of São Paulo (15th region, Campinas), in contrast with the permissive city of São Paulo (2nd region).

At the same time, we found little scholarship and written record about the legality of outsourcing during the pre-legalization period for most regions ultimately classified as permissive. The lack of written record is likely driven by outsourcing being less contested/litigated outside Brazil's South or major urban centers, like the city of São Paulo (2nd region), where courts were permissive.

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[50]While evidence that Southern courts were more restrictive abound, the reasons why are less clear. One possibility is the South's leftist legal tradition, Rio Grande do Sul being the center of the Alternative Law Movement, an intellectual movement that emerged in the 1980s and was grounded on Marx's critical theory of law (Barreto and de Lyra, 2016). Another possibility is the influence of Italian and German immigration on the region's positive attitude toward labor unions (Batistella, 2009).

Based on these findings, we define regional courts 4, 9, 12, and 15 as restrictive toward outsourcing prior to legalization, while the remaining regional courts are defined as permissive toward outsourcing. As mentioned in the main text, we are uncertain about the correct classification of the 15th region, but our local labor market results are robust to dropping observations from the São Paulo state (see Appendix Table D.5).

Table B.1: Classification of regional labor courts: count summary of qualitative classification materials

| | | | | Count summary of Regional Labor Court classification materials | | | | | | | |
| | | | | Regional Labor Court rulings on outsourcing appealed to Superior Labor Court before Súmula 331 | | Interviews about Regional Labor Courts' rulings on outsourcing before Súmula 331 | | | Prevalance of security guard outsourcing across Regional Labor Court's jurisdictions | | |
Regional Labor Court (1)	States under jursidiction (2)	Classification (3)	Geographic Region (4)	Total appeals (5)	Apeals where Regional Labor Court ruled the end-firm as the employer (6)	Total Interviews (7)	Interviews where legality of outsourcing is discussed (8)	Court's interpretation of outsourcing is described as Restrictive (9)	1992 contract-firm share of guard employment (10)	1992-1999 change in contract-firm share (11)	Chow test p-value (12)
4	Rio Grande do Sul	Restrictive	South	3	2	8	7	7	0.36	0.19	0.00
9	Paraná	Restrictive	South	3	3	6	4	4	0.31	0.13	0.00
12	Santa Catarina	Restrictive	South	1	0	1	1	1	0.36	0.20	0.00
15	Campinas (São Paulo state, excl. capital)	Restrictive	Southeast	0		5	4	3	0.30	0.23	0.00
1	Rio de Janeiro	Permissive	Southeast	1	0	0			0.45	0.12	0.32
2	São Paulo (capital)	Permissive	Southeast	2	1	3	3	0	0.36	0.20	0.29
3	Minas Gerais	Permissive	Southeast	0		0			0.32	0.14	0.02
5	Bahia	Permissive	Northeast	0		0			0.41	0.22	0.09
6	Pernambuco	Permissive	Northeast	0		1	1	0	0.41	0.06	0.74
7	Ceará	Permissive	Northeast	1	1	0			0.42	0.07	0.04
8	Amapá and Pará	Permissive	North	0		0			0.50	0.09	0.07
10	Distrito Federal and Tocantins	Permissive	Central-West	0		0			0.51	0.09	0.01
11	Amazonas and Roraima	Permissive	North	0		0			0.41	0.23	0.89
13	Paraíba	Permissive	Northeast	0		0			0.35	0.08	0.00
14	Acre and Rondônia	Permissive	North	0		1	1	0	0.54	-0.03	0.00
16	Maranhão	Permissive	Northeast	0		0			0.49	0.03	0.34
17	Espírito Santo	Permissive	Southeast	0		0			0.42	0.02	0.02
18	Goiás	Permissive	Central-West	0		0			0.35	0.15	0.48
19	Alagoas	Permissive	Northeast	0		0			0.39	0.10	0.29
20	Sergipe	Permissive	Northeast	0		0			0.33	0.12	0.00
21	Rio Grande do Norte	Permissive	Northeast	0		0			0.40	0.04	0.21
22	Paiuí	Permissive	Northeast	0		0			0.35	-0.01	0.02
23	Mato Grosso	Permissive	Central-West	0		0			0.36	0.09	0.09
24	Mato Grosso do Sul	Permissive	Central-West	0		0			0.43	-0.07	0.52

Notes: Column (5) lists the number of regional court rulings on outsourcing brought to the Supreme Labor Court between 1986 (Súmula 256 in place) and December 1993 (Súmula 331 in place). See Appendix Table A1 for a summary of each ruling. Column (7) lists the number of interviews (with regional labor court justices, jurists, lawyers, circuit court judges, and union leaders) conducted by the authors and/or by Brazilian jurist and former regional labor court justice Magda Biavaschi and legal historian Alisson Droppa, concerning Regional Courts' interpretation of the legality of outsourcing prior to Súmula 331. See Appendix Tables B.2 and B.3 for summary quotes, and Barros Biavaschi and de Andrade Baltar (2013) for the legal research documenting regional disparities across regional labor courts' rulings on outsourcing prior to Súmula 331. Columns (10)–(12) show the prevalence of security guard outsourcing in each regional labor court's jurisdiction, as measured by the baseline share of all guards that are employed by contract-firms in column (10), the 1992-1999 change in that share in column (11), and the p-value on the chow-test detecting a trend break in that share after 1993.

Table B.2: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331

Interviewee's position at the time (1)	Regional Labor Court (2)	Original quote (3)	English translation (4)	Interpretation (5)	Interview date (6)	Source (7)
Regional Labor Court Justice	4	"Na época, os juízes da 4ª Região aplicavam - seguindo a linha do magistério do mestre de todos nós, João Antônio Guillembernard Pereira Leite – o entendimento da Súmula 256 do TST que levava ao reconhecimento da relação de emprego direta entre trabalhador e tomador dos serviços, seu real beneficiário. E, dessa forma, reconhecia-se a produção dos efeitos jurídicos que a fraude visava a impedir, retirando-se a máscara e responsabilizando-se diretamente a empresa tomadora dos serviços."	"At the time, judges at the 4th Regional Labor Court upheld - in line with the juridical practice of our master João Antônio Guillembernard Pereira Leite - the understanding that the Superior Court's Súmula 256 recognized a direct employment relationship between the worker and the services-contracting firm, which is the actual beneficiary [of the employment relationship]. And, in this way, we accepted as valid the legal consequences that the fraud[lent] [outsourcing relationship] aimed to avoid, taking off its mask and ruling the services-contracting firm to be directly responsible [for such legal consequences]."	Restrictive	Feb 2008	2
Prosecutor for the District Attorney's Labor Office	4	"E o entendimento era o stricto sensu do Enunciado 256: [a terceirização] só era permitida no serviço de vigilância bancária e no trabalho temporário. Adotava-se integralmente esse entendimento."	"And the interpretation of Enunciado 256 was strictu sensu: [outsourcing] was only allowed in the case of security services for banks and for temporary work. That interpretation was wholeheardetly adopted."	Restrictive	Jul 2008	2
Regional Labor Court Justice	4	"... naquele momento tínhamos o Enunciado 256 do TST que só excluía aquele trabalho temporário e, depois, os vigilantes [bancários]."	"... at that time we had Enunciado 256 from the Superior Labor Court which only made exceptions for temporary work and, afterwards, for [banking] security."	Restrictive	Jul 2008	2
Regional Labor Court Justice	4	"Os vigilantes do quadro estavam sendo substituídos por vigilantes contratados via agora essas empresas... Nós entendíamos que aqui a legislação ressalvada pela 256 não se aplicava... Então eu econhecia a natureza do vínculo de emprego diretamente com os bancos. Muitas dessas minhas decisões foram confirmadas pelo Tribunal e depois no Tribunal Superior do Trabalho, que era quem editava a súmulas. Se foram reformadas, eu não sei porque não acompanhava... nós não nos preocupávamos muito em acompanhar o andamento das nossas decisões, mas não eu não julgava sozinha."	"Security guards were being replaced by guards contracted now via these firms... Our understanding was that the exceptions made under 256 were not applicable here... so I recognized the employment link directly with banks. Many of my decisions were upheld by the Regional Labor Court and then by the Superior Labor Court, which edited súmulas. If they were revised, I do not know because I did not follow... we did not concern ourselves with what happened after our decisions, but no I did not rule alone."	Restrictive	Apr 2020	1
Labor judge	4	"Nas minhas decisões, eu não me apego muito à teoria, à doutrina... Gosto de analisar o caso concreto. E naquele processo [de 1991], ao analisar a forma pela qual havia sido feita a terceirização, (...) o que me levou a entender que o que estava sendo praticado (...) não era correto (...) é o fato de que não poderia admitir que um empregado que estava integrado no corpo da empresa, galgando uma carreira, com esperança de subir na vida, fosse, de repente, excluído daquele processo. Uma empresa deve crescer, mas é obrigatório que leve seus empregados a crescer junto com ela. Não aceito outra forma de empresariado. (...) Colocava-me na pele de um empregado, por exemplo, na pele do chefe do departamento de pessoal. Que havia, enfim... Era um excelente empregado, merecia estar no quadro de empregados, com todas as vantagens decorrentes, sendo um bom empregado." "Então, não posso dizer que no meu íntimo seja favorável à terceirização. Não sou. A empresa deve ser uma grande família em que todos evoluam juntos. Todos. "	"In my decisions, I don't cling much to theory, to doctrine... I like to analyze each specific case. And that [1991] case, in analyzing the way in which outsourcing had been done, (...) what made me understand that what was actually being practiced (...) was not right (...) is the fact that I could not accept that an employee who had been part of the staff, who was building a career, with hopes to ascend in life, could, out of a sudden, be excluded from that process. A company needs to grow, but it is compulsory that it allows it brings its employees to grow with it. I cannot accept any other form of business. (...) I put myself in the employee's shoes, for example, (...) in the shoes of the human resources department's head. That had, anyway... He was an excellent employee, he deserved to be in the employee roster, with all its advantages, being a good employee." "So, I cannot say that deep inside I am favorable to outsourcing. I am not. The firm must be a great family in which all grow together. All."	Restrictive	May 2008	2

Notes: Source 1 in Column (5) are interviews conducted by authors, whereas Source 2 are transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table B.2: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331 (Cont.)

Interviewee's position at the time (1)	Regional Labor Court (2)	Original quote (3)	English translation (4)	Interpretation (5)	Interview date (6)	Source (7)
Journalist and lawyer. Representative for Worker Syndicates at RS court (1985-1991), Representative of Worker Syndicates at Superior court (1993 onwards)	4	"Mas penso que no Rio Grande do Sul (e aqui não tem bairrismo ou gauchismo) temos uma visão mais esclarecida sobre a questão. Até sobre o papel da Justiça do Trabalho. Esta existe justamente para assegurar aos trabalhadores os direitos previstos na CLT, para que a lei seja cumprida. Se querem modificar a decisão da Justiça do Trabalho, mudem-se as leis, no Congresso Nacional, democraticamente. Mas o que não pode acontecer é que determinadas interpretações sejam complacentes." "Pessoalmente, como advogado e como pessoa que conhece a Justiça do Trabalho, como eu conheci, que conhece as relações de trabalho, não vejo como alguém pode se beneficiar com a locação de mão-de-obra, com a terceirização. Algum passe mágico nessa contratação acontece. Não posso compreender que uma empresa que deixa de contratar telefonistas para contratá-las por uma terceira, locadora de mão-de-obra, pague para essa locadora X e ela pague o seu empregado. O que ela vai pagar ao empregado seu? Qual o lucro? Por evidente, há lucro nessa intermediação. Mas quem ganha? Quem perde? Alguém sai lesado. O trabalhador, possivelmente. (...) É uma matemática que não fecha. É a minha conclusão. Por isso, não consigo entender, ainda, a razão de ser da terceirização."	"But I think that in Rio Grande do Sul (and here I mean no state chauvinism) we have a much clearer understanding of this topic. Even of the role of Labor Courts. The courts exist precisely to assure workers of their rights according to labor laws, so that the law be followed. If they want to change the Labor Courts' decision, then they must change the laws, in Congress, democratically. But what cannot happen is that certain interpretations be complacent." "Personally, as a labor lawyer and as someone who understands the Labor Courts as I do, that knows labor relationships, I don't see how someone can benefit from the contracting of labor, from outsourcing. Some magical step in this form of contracting must happen. I cannot understand how one firm that stops to directly hire phone operators to outsourcing them from an outsourcing firm instead, can pay this outsourcing firm X and then this firm pays its employee. What is the outsourcing firm going to pay its employee? What is the profit? Evidently, there is profit in this intermediation. But who wins? Who loses? Someone gets hurt. The worker, possibily. (...) This is math that doesn't make sense. This is my conclusion. For that reason, I cannot understand, still, outsourcing's raison-dêtre."	Restrictive	Jul 2008	2
Jerônimo Leiria, laywer for large end-firm in Rio Grande do Sul; Creator of word "terceirização" to denote outsourcing, with the goal of avoiding explicit violations of Súmula 256	4	"E como havia o entendimento da Súmula 256 do TST, que proibia a contratação de serviços, era necessário que houvesse uma nomenclatura para essas atividades contratadas que não recebesse o título de prestação de serviços. E, ainda, que não tivesse uma tradução em qualquer outra língua. Era necessário achar uma palavra oca, que não tivesse significado nenhum... Aí eu criei umas 40 palavras: [dentre elas a] terceirização (a partir da idéia de que se contrato uma empresa, vêm os empregados de outra empresa, eles são alheios, terceiros)... Então se procedeu a uma votação... Daí a palavra mais simpática e mais "nada a ver" foi terceirização. "O que é que vocês estão fazendo?", perguntariam. E responderíamos: "Estamos fazendo terceirização". Como a fábula roupa nova do rei (...) Como era uma palavra nova, que não estava registrada em lugar nenhum, eu media os centímetros quadrados quando a mesma era publicada, pois tinha sido inventada por nós. Tenho o registro de que ela [a palavra] foi publicada pela primeira vez em 23 de janeiro de 1991, pela revista Exame." "Pergunta: Mas havia resistências internamente a Justiça do Trabalho, não? Resposta: Sim. Pergunta: Eram importantes, eram significativas essas resistências na época? Resposta: Sim, era a totalidade."	"Because there was an understanding from Súmula 256, which forbade the contracting of services, it was necessary for that to be a nomenclature for contracted activities that did not use that [exact] term. And, still, it had to be a term that did not have a translation from any other language. It was necessary to find a hollow word, devoid of meaning... So I created 40 words: [among which was terceirização (starting from the idea that if I contract with a company, the employees from that company are the ones who come, they are third-parties)... Then we coted... and the more sympathetic word and the one that had 'nothing to do with anything' was terceirização [, which can be translated as outsourcing]. They would ask: 'What are you guys doing?', and we would answer 'We are outsourcing'. It is like the fable of the Emperor's New Clothes (...) Because it was a new word, that was not registered anywhere, I was able to track whenever it was published, as the word had been created by us. I have records that show that the first time it [the word] was published was on Januart 23, 1991, by the Exame magazine." "Question: But was there important internal resistance from the Labor Courts? Answer: Yes. Question: Were they important, were this resistence significant at the time? Answer: Yes, they were all there was.	Restrictive	Sep 2008	2
Regional Labor Court Justice	9	"A Terceirização é um processo que veio para ficar, em que a classe trabalhadora se subdividiu e se fracionou, dificultando a ação sindical... buscando uma acomodação com o movimento do capital que continua avançando em seu sistema de exploração... Pela análise daquele processo, cuja cópia digital vocês me enviaram antes da entrevista, percebi que a Juíza de primeiro grau... concluía pala ilegalidade da Terceirização... No Tribunal, mantínhamos a ilegalidade e [reconhecíamos] o vínculo de emprego."	"Outsourcing is a process that is here to stay, in which the working class got subdivided and fractiored, making it harder for unions to operate... searching for its place with the capital movement that keeps advancing in its exploitation system... According to [my] analysis of that case, whose three digital copies you sent me before this interview, I noticed that the district judge... ruled that outsourcing was ilegal. At the Regional Court, we would uphold this ilegality and [recognized] the employment link with the end employer."	Restrictive	May 2011	2

Notes: Source 1 in Column (5) are interviews conducted by authors, whereas Source 2 are transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table B.2: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331 (Cont.)

Interviewee's position at the time (1)	Regional Labor Court (2)	Original quote (3)	English translation (4)	Interpretation (5)	Interview date (6)	Source (7)
Labor judge	9	"Então a terceirização, a meu ver, não deixa de ser uma precarização fruto dessa modernização onde se abrem as portas para um novo formato da prestação de serviço. (...) [E]u diria que 99% das ações questionavam sim a terceirização e buscavam vinculo com a [tomadora de serviços]... bom, não preciso nem te dizer que todas as sentenças foram no sentido de reconhecer o vinculo e a responsabilização ali eu acho que dei solidaria em todos, devido ao ato ilícito, a fraude, eu 'canetiei' pesado, as sentenças foram confirmadas pelo Tribunal, não sei se chegou ao TST, mas eu lembro que não teve nenhuma dúvida"	"Outsourcing, in my view, is nothing but a worsening of working conditions that is a product of this modernization where doors are open to a new way of services contracting (...) [I] would say that 99% of cases did question outsourcing and wanted a direct employment link with [the end-firm]... well, I don't even need to say that all of our rulings were to recognize the direct employment link and find the end-employer jointly liable ['responsabilidade solidária'], I think I found joint liability on all cases, due to the ilicit nature, the fraud, I 'ruled' heavily, the rulings were upheld by the Regional Court, not sure if they were brought to the Superior Court, but I remember we had no doubts about it."	Restrictive	Nov 2011	2
Labor lawyer	9	"Nos idos de 1979, 1980, assessorava a categoria dos vigias e vigilantes que, à época, não tinha legislação especifica... Nessa época, os vigilantes trabalhavam nas portas dos bancos como vigilantes e exerciam diversas funções. Na verdade, eles eram porteiros: davam informações, ajudavam a abrir conta bancária, fichários, arquivos. Então, comecei a ajuizar reclamatórias... invocando que quem exercia serviços de portaria [em bancos] era bancário... Em decorrência desse trabalho, as serventes que trabalhavam em bancos começaram a me procurar. Comecei, assim, a ajuizar reclamatórias contra os bancos advogando a tese de que, na realidade, essas serventes eram bancárias e não "locadas". Sendo a locação de mão de obra ilegal, essas ações começaram a ser vitoriosas."	"Around 1979, 1980, I assisted security guards, for which, at the time, there was no specific legislation... At that time, security guards worked in front of banks as security guards and took on a variety of tasks. In truth, they were doormen: they gave out information, helped customers open accounts, filing, archival. So, I began filing lawsuits... arguing that who exercised doorman services [at banks] was a bank employee... Because of this work, servers who worked in banks started seeking me out. I started, then, filing lawsuits against banks arguing that, in reality, these servers were bank employees and not "outsourced". Given that outsourcing was ilegal, I started winning these lawsuits."	Restrictive	Dec 2012	2
Laywer and Congressman. Helped found Workers Party with Lula.	9	"Pergunta: Então, vimos que nas sentenças da Vara, da Junta, eram nesse sentido. Reconheciam o vínculo direto com [uma grande empresa do Paraná]. O Tribunal, em regra, a mantinha. Mas [essa empresa] recorria para o TST, não mais questionando sua condição de empregadora, mas o mérito. [Por quê?] Resposta: Não adiantava, porque ela ia perder."	"Question: So, we saw that the District Court rulings were in line with this. They would recognize the direct link with [a large firm in Paraná]. The Regional Court, in general, upheld that decision. But [the firm] would appeal to the Superior Court, no longer questioning that it was the direct employer, but only [questioning] the allaged damages. [Why?] Answer: Because it was to no avail, the firm knew it would lose."	Restrictive	May 2011	2
Layer of large end-firm in Paraná and Santa Catarina	12	"[A empresa] passou nesse tempo por processos de terceirização, nessa década de 1980 e início de 2000, mas verificasse o seguinte as terceirizações que foram para baratear não deram certo, as terceirizações que foram para adquirir tecnologia, essa deram certo e estão ai até hoje... [T]emos [essa discusão sobre terceirização] em Santa Catarina... porque [lá houveram] várias terceirizações anuladas, é um problema isso. "	[The firm] went over a process of outsourcing during this time, between the 1980s and early 2000, but we found out the following: the outsourcing decisions made to cut costs didn't work out, the outsourcing made to acquire technology did and are here through today... [W]e have [this discussion about outsourcing] in Santa Catarina... because [there, there were] many nulled outsourcing decisions, this is a problem."	Restrictive	Aug 2011	2
President of worker syndicate	15	"Depois da regulamentação...we could outsource em algumas atividades. Por exemplo, a vigilância que, hoje, é legal; não era. A vigilância, a alimentação, a limpeza, todas atividades da nossa categoria. (...) Terceirização, eu acho, é uma fraude nas relações de trabalho. Ela maquia a relação. As empresas dela fazem uso para reduzir custo e, também, para se eximirem da responsabilidade trabalhista frente àqueles funcionários. (...) Em todas as convenções reivindicamos o fim da terceirização..."	"After legalization...we could outsource in some activities. For example, security services which, today, is legal, was not. Security services, food services, cleaning, all activities our our syndicate. (...) Outsourcing, I think, is a fraud to work relationships. It masks the relationship. Its firms make use of it to reduce costs and, also, to bypass labor laws for those workers. (...) In all conventions we pushed for the end of outsourcing..."	Restrictive	Apr 2009	2
Prosecutor for the state of São Paulo from 1990 to 1993	15	"O 256 era perfeito. Havia dois tipos de trabalho que poderiam ser terceirizado: vigilância [bancária] e de caráter temporário, em casos excepcionais, tudo documentado, fundamentado, perfeito. Quanto veio a Súmula 331 ela abriu "a porteira" para o empregador usar um instrumento de administração e de excelência, como se fosse uma ferramenta de precarização[.]"	"256 was perfect. There were two types of jobs that could be outsourced: [banking] security and temporary work, in exceptional cases, everything documented, argued, perfect. When Súmula 331 arrived it opened the "flood gates" for the employer to use an exceptional administrative provision as a tool for worsening labor conditions[.]"	Restrictive	Jun 2009	2

Notes: Source 1 in Column (5) are interviews conducted by authors, whereas Source 2 are transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table B.2: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331 (Cont.)

Interviewee's position at the time (1)	Regional Labor Court (2)	Original quote (3)	English translation (4)	Interpretation (5)	Interview date (6)	Source (7)
Labor judge	15	"Na carreira de magistrado deparei-me com... pouquíssimos processos questionando a terceirização, buscando o reconhecimento do vínculo com a tomadora do serviço. Isso é muito raro. Nem me recordava, aliás, do processo em que atuei, cujas cópias vocês me encaminharam previamente e, muito menos, da decisão que então prolatei. Não me recordava porque são muito esporádicos."	"In my career as a judge I enountered very few lawsuits questioning outsourcing, asking for recognition of the employment link with the end firm. That is very rare. In fact, I did not even remember the lawsuit I ruled on, whose copies you previously forwarded me, let alone the decision I ruled on it then. I did not remember because they are very few and far-between."	Unclear	Apr 2009	2
Clerk (1987-1991), laywer (1991-1995), judge (1995 onwards)	15	"Por princípio, penso que a terceirização é negativa, porque descategoriza e precariza os trabalhadores, além de prejudicar a própria dimensão da responsabilidade patrimonial pelos créditos trabalhistas."	"In principle, I think that outsourcing is negative, because it makes worker groups more disconnected and fragile, in addition to harming the very dimension of employer responsibility for labor claims."	Restrictive	Jul 2007	2
		"Por aqui na 15ª, a impressão que eu tenho é que, com a mudança da Súmula (da 256 para 331), as pessoas passaram a assimilar a ideia de que a terceirização era sempre lícita, sem atentar para as próprias ressalvas feitas pelo TST. "	"Here at the 15th, the impression I have is that, once the Súmula changed (from 256 to 331), people started to accept the idea that outsourcing was always licit, even disregarding the exceptions made by the Superior Labor Court."			
		"[E]m 1985, o Estado de São Paulo contava com 60.476 trabalhadores terceirizados, número que ampliado para 129.951 em 1993 e saltou para 179.836 em 1994... Esse aumento foi intensificado a partir de 1994, quando da implantação do Plano Real mas também época em que houve a modificação do entendimento predominante no TST a respeito do tema. (...) É claro que esse não foi o único fator que levou a esse fenômeno amplificador da terceirização, mas não se pode subestimar a capacidade que as decisões judiciais possuem de influenciar as diretrizes de atuação empresarial."	"[I]n 1985, the State of São Paulo had 60.476 outsourcing workers, a number that expanded to 129.951 in 1993 and jumped to 179.836 in 1994... This increase was intensified starting in 1994, at time of the Real Plan, but also the time when there was a change in the Superior Court's understanding of the topic. (...) Of course this was not the only factor that led to the expansion of outsourcing, but one cannot underestimate the ability that legal decisions have to influence how firms behave."			

Notes: Source 1 in Column (5) are interviews conducted by authors, whereas Source 2 are transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table B.3: Quotes from interviewees who worked in Permissive regions prior to Súmula 331

Interviewee's position at the time (1)	Regional Labor Court (2)	Original quote (3)	English translation (4)	Interpretation (5)	Interview date (6)	Source (7)
Regional Labor Court Justice	6	"The parameter we would use (to judge outsourcing cases) was [that] a bank could have as an employee, an armed guard, but we would consider in terms of outsourcing it to legal or not 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."	N/A	Permissive	May 2020	1
Public Prosecutor's Labor Office	14	"Para mim, o problema não está na terceirização em si, como disse antes, há hipótese em que ela é admissível e, até certo ponto, necessária em momentos muito específicos da atividade empresarial. O problema está na terceirização da atividade fim, o que acontece com muita freqüência."	"To me, the problem is not with outsourcing per se, as I said before, there are cases in which it is admissible and, up to a certain point, needed at very specific moments of firms' [lifecycles]. The problem is with outsourcing of end-activities, which happens frequently."	Permissive	Mar 2011	2
Labor judge	2	"Na verdade, isso começou em 1974 com a lei do trabalho temporário, alastrando-se, depois, para a vigilância [bancária], o que possibilitou a "Terceirização" no trabalho bancário dos vigilantes. A idéia era exatamente essa e ela não se vinculou apenas aos bancos. Acabou se estendendo para outras atividades [econômicas]."	"Actually, this started in 1974 with the temporary law work, spreading, afterwards, to [banking] security, which allowed "Outsourcing" of security services for banks. The idea was exactly this and it did not restrict itself just for banks. It ennded up spreading to other [economic] activities."	Permissive	Sep 2009	2
Regional Labor Court Justice	2	"[A] terceirização é importante para maior eficiência da empresa, para maior produtividade da empresa, para baratear custo... É preciso haver uma justificativa para a terceirização: a busca da maior produtividade, da maior eficiência e não do menor custo da mão de obra... O gaúcho tem uma postura mais contenciosa mesmo em relação à vida e as coisas... O paulista tem talvez uma visão mais econômica, mais pragmática..."	"Outsourcing is important for firm efficiency, for more productivity, for cutting costs... There must be a justification for the outsourcing: a search for productivity, for more efficiency and not cutting labor costs [specifically]... the Southerner has a more contencious view even with regards to other things in life... The paulista perhaps has a more economical, more pragmatic view..."	Permissive	May 2009	2
Union leader	2	"Quando o Sindicato questionava, a empresa dizia que precisa de pessoas especializadas em determinadas atividades... O mesmo diziam para a vigilância, treinar guardas, ter problemas com porte de arma, então seria ideal que tivesse uma empresa de segurança... Houve sim questionamentos na Justiça do Trabalho em alguns momentos, mas as sucessivas derrotas judiciais serviram para desanimar os Sindicatos, uma vez que nós não conseguimos êxito nas ações que sindicatos ingressavam e os patrões faziam questão de propagandear isso – 'está vendo! A Justiça do Trabalho considera legal a terceirização'."	"When the Union questioned, the firm said it needed specialized people in certain activities... They said the same for security, train guards, face issues with gun licensing, so it would be ideal if there was a security services firm... Yes, the Labor Court questioned this at times, but the high frequency of lawsuit losses ended up wearing down the Unions, because as we could not win lawsuits the employers made sure to promulgate -- 'you see! The Labor Court considers outsourcing legal!'"	Permissive	Apr 2009	2

Notes: Source 1 in Column (5) are interviews conducted by authors, whereas Source 2 are transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table B.4: Variation in dissent over legality of outsourcing across regional labor court precedents appealed to the Superior Labor Court prior to Súmula 331

Regional Precedent Number	Regional Court (1)	Year of Superior Court ruling (2)	What type of job did the plaintiff worker do? (3)	Party that appealed Regional Court's ruling to Superior Court (4)	End employer type (5)	Rulings: Which firm was found to have the direct employment link?	
						Regional Court (6)	Superior Court (7)
Panel A: Precedents by Regional Labor Courts classified as Restrictive							
RR 35607-78.1991.5.04.5555	4	1992	Cleaning services	End employer	Federal Agency	End employer	Outsourcing Firm
RR 45956-68.1992.5.09.5555	9	1992	Security services	End employer	Federal Agency	End employer	Outsourcing Firm
RR 41486-28.1991.5.09.5555	9	1992	Cleaning services	End employer	Federal Government	End employer	Outsourcing Firm
RR 24086-98.1991.5.09.5555 [1]	9	1992	Cleaning services	Outsourcing firm	Bank	End employer	Outsourcing Firm
RR 41974-21.1991.5.04.5555	4	1993	Cleaning services	End employer	State Government	End employer	Outsourcing Firm
RR 43279-06.1992.5.04.5555	4	1993	Security services	Worker	Federal Agency	Outsourcing Firm	Outsourcing Firm
ERR 211-52.1990.5.12.5555	12	1993	Cleaning services	Worker	Bank	Outsourcing Firm	Outsourcing Firm
Panel B: Precedents by Regional Labor Courts classified as Permissive							
RR 226-34.1989.5.02.5555	2	1989	Cleaning services	Worker	Bank	Outsourcing Firm	Outsourcing Firm
RR 42286-78.1991.5.01.5555	1	1992	Cleaning services	End employer	Federal Agency	Outsourcing Firm	Outsourcing Firm
RR 44058-74.1992.5.07.5555	7	1992	Not mentioned	End employer	State-owned firm	End employer	Outsourcing Firm
RR 62835-48.1992.5.02.5555	2	1993	Not mentioned	End employer	City Government	End employer	Outsourcing Firm

Notes: [1] The outsourcing firm appealed this case to the Superior Labor Court because the regional labor court found the outsourcing firm, Orbram Organizacao E Brambilla Ltda, jointly liable for demages the worker was suing for, despite the fact that the court established a direct employment link was established with the end employer. The Superior Court found the outsourcing firm solely liable as it dissented from the regional labor court's decision on which firm had the direct employment link with the worker.

C   Data and Descriptive Statistics: Details

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 sample includes all workers between the ages of 18 and 64 who were employed as of December 31 of each year. If a worker is seen working for more than one firm, we keep the employment link with the highest-paying firm, and drop employment records with missing earnings data. We drop workers in public administration and those without valid information on their industry of employment. For our analysis of local labor markets for security guards, we restrict our sample to the large occupational groups listed in Table C.1, and only include local labor markets with at least 30 security guards and 30 cleaners in every year between 1985-2002, yielding an analysis sample of 265 local labor markets, covering 98 percent of all security guards.. Given the high prevalence of outsourcing among cleaners, we exclude them from estimation of treatment effects for security guards.

Variable definitions. We use the establishment's geographic location (municipality) and industry, and worker-level information including gender, age group (five categories), education (nine categories), occupation, and December earnings, reported as multiples of that year's federal minimum wage. To convert those into total real earnings, we multiply the RAIS multiples-of-the-minimum-wage earnings variable by each year's federal minimum wage, available from Brazil's Instituto de Pesquisa Econômica Aplicada (IPEA) and converted to 2017 reais. Reported earnings include regular salary payments, holiday bonuses, tips, performance-based bonuses, commission, and profit-sharing agreements.

Table C.1: Included occupations

Occupation	Contract-firm share	Mean log wage	Mean schooling	Male	National employment
Security guards	0.39	6.91	4	0.99	494776
Technicians	0.04	7.82	10	0.87	384837
Electricians and electronics workers	0.03	7.50	6	0.97	265208
Cashiers and tellers	0.04	7.49	11	0.50	481360
Machine installers and mechanics	0.05	7.39	10	0.58	1845970
Office administration	0.02	7.19	6	0.99	377638
Drivers, sailers, conductors	0.02	7.08	5	1.00	689230
Secretaries and typists	0.06	6.80	11	0.15	167401
Food and beverage processing workers	0.00	6.69	5	0.84	307684
Other manual or uncommon occupations	0.05	6.64	5	0.83	1471090
Salesmen	0.01	6.51	8	0.66	1099564
Cooks, waiters, bartenders	0.03	6.49	5	0.31	466541
Cleaners (excluded)	0.31	6.49	4	0.42	746155
National	0.06	7.14	8	0.74	14789180

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

Measuring outsourcing using industry and occupation codes We use the 1994 *Código Brasileiro de Ocupações* (CBO 94) at the two-digit level to define occupations, which is consistent with predecessor (CBO) occupational codes. 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 contract firms as those with CNAE95 numbers 74608 ("Atividades de investigação, vigilância e segurança"), 74160 ("Atividades de assessoria em gestão empresarial"), 74500 ("Seleção, agenciamento e locação de mão-de-obra"), 74705 ("Atividades de limpeza em prédios e domicílios"), and 74993 ("Outras atividades de serviços prestados principalmente às empresas"), which fall under occupational class 74 "Serviços prestados principalmente às empresas." For firms that exited prior to 1995 and therefore do not have a CNAE code, we used a concordance between "IBGESUBATIVIDADE" codes (reported prior to 1995) and CNAE codes, constructed using firms that are present in the data before and after 1995 and report the former code before, the latter after.

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 C.1: Share of security guards formally employed, over time

A. Share of guards formally employed

B. Guard share of total formal employment
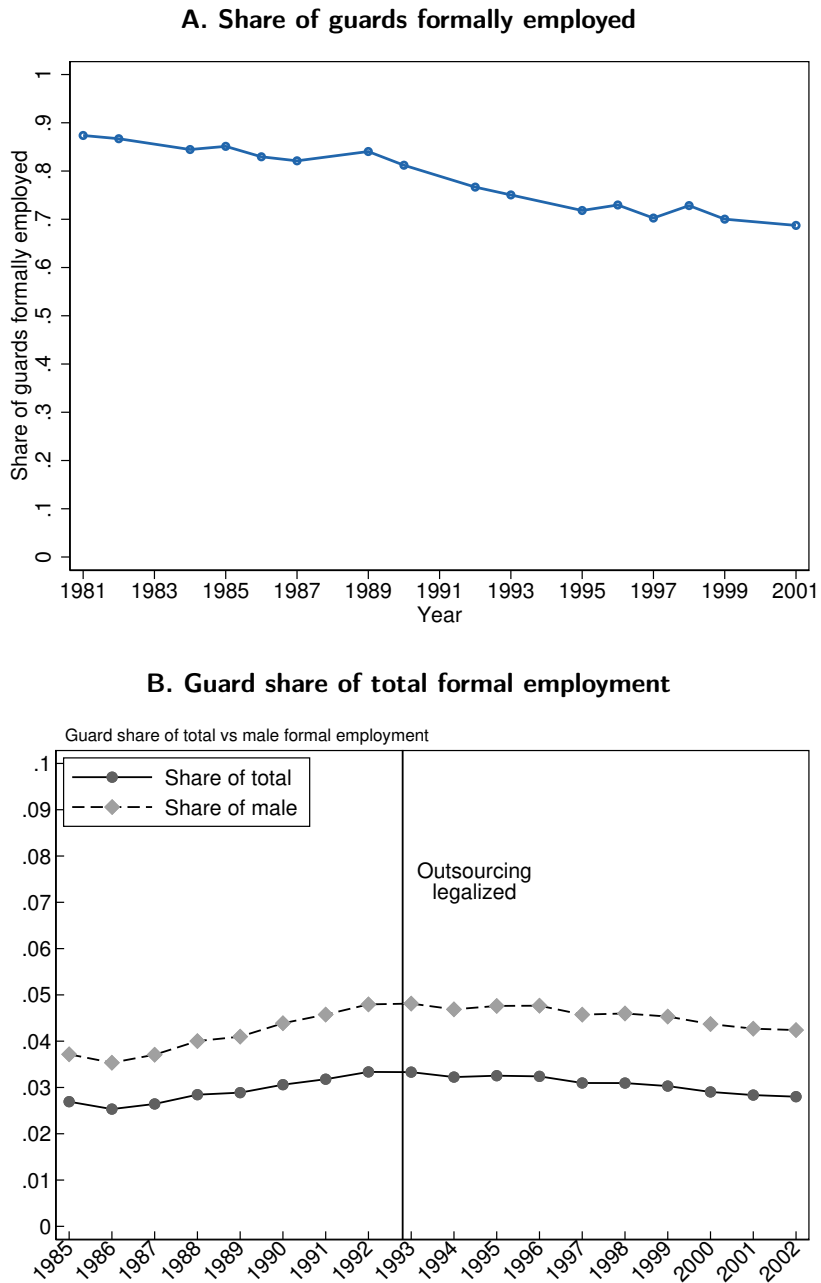
Note: Panel A plots the share of guards (aged 18-65) that are formally employed based on Brazil's Pesquisa Nacional por Amostra de Domicílios (PNAD) household survey. Pabel B plots the guard share of all total private sector formal employment, separately for all workers versus for male workers only, based on RAIS.

Figure C.2: Total formal employment of security guards, police, and other private-sector occupations

Guards and police (left), thousands; All other workers (right), millions
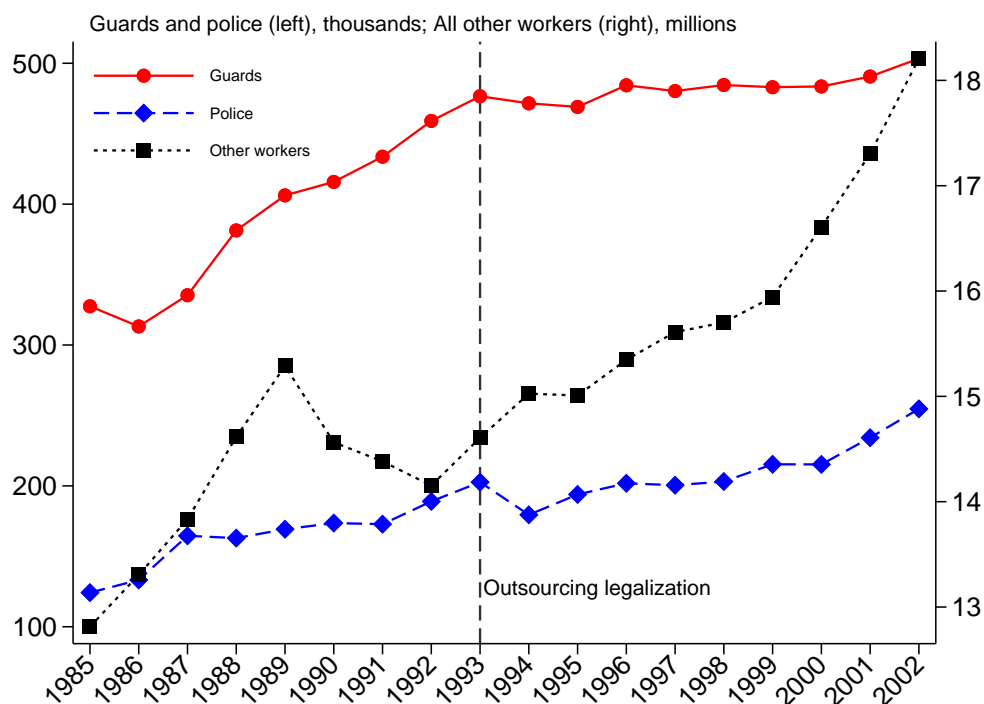
Note: This figure plots total formal employment of security guards (CBO 2-digit code 58, excluding CBO 3-digit code 583), police officers (CBO 3-digit code 583), and other formal sector occupations based on RAIS.

Figure C.3: Trends in security outsourcing, by regional labor court
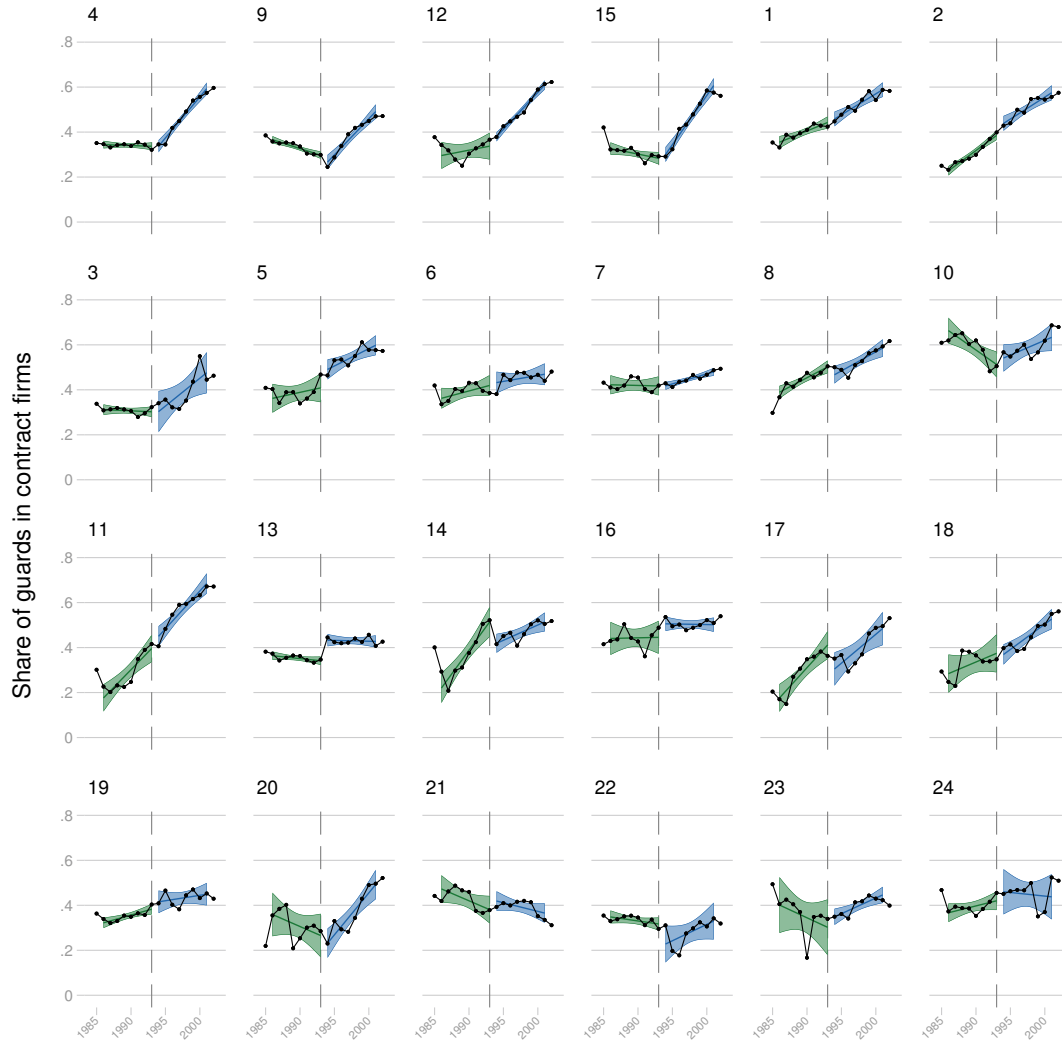
Note: Figure plots the trend in outsourcing prevalence, as measured by the share of security guards in the formal sector working for contract firms, separately for each jurisdiction of the 24 regional labor courts. Courts 4, 9, 12, and 15 are classified as restrictive.

Table C.2: Predictors of outsourcing prevalence in a microregion, 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.

D Market-level effects of legalization: Details

Table D.1: Baseline descriptive statistics of analyzed microregions by regional labor court restrictiveness

Microregion characteristics in 1992	Restrictive		Permissive		
Outsourced share of guards	0.124	0.111	0.155	0.141	0.150
	(0.182)	(0.154)	(0.224)	(0.193)	(0.212)
Establishments directly hiring guards	239	220	377	647	497
	(337)	(283)	(1317)	(2177)	(1728)
HHI	0.062	0.056	0.074	0.063	0.068
	(0.08)	(0.066)	(0.09)	(0.06)	(0.078)
Average log(wage), guards	7.21	7.22	6.96	6.99	6.97
	(0.21)	(0.22)	(0.29)	(0.23)	(0.27)
Guards per 1000 formal workers	23.14	24.18	31.28	24.43	28.69
	(25.56)	(22.12)	(20.92)	(13.28)	(18.66)
By employment type:					
Direct-hire guards	17.51	19.54	22.77	18.87	21.29
Contract-firm guards	5.64	4.63	8.51	5.57	7.40
By age group:					
Age 18-24	1.84	2.15	3.18	2.27	2.83
Age 25-29	3.41	3.51	5.45	3.77	4.81
Age 30-39	6.23	6.62	9.11	6.63	8.19
Age 40-49	5.48	5.64	6.92	5.61	6.43
Age 50-64	6.19	6.25	6.63	6.15	6.43
Used for entropy balancing:					
Log formal-sector employment	10.7	10.6	10.4	10.7	10.5
Homicide rate (per 100K population)	11.6	13.2	16.4	11.6	14.5
Unemployment rate	0.026	0.036	0.043	0.026	0.036
Share of employment in tradeable sector	0.46	0.42	0.42	0.46	0.43
Import tariff competition exposure	-0.145	-0.146	-0.146	-0.145	-0.146
Weights	Uniform	Inv. prop. Score	Uniform	Entropy-balancing	Inv. prop. Score
TRT regional courts	4	4	20	20	20
N	107	107	158	158	158

Notes: Sample includes all microregions with at least 30 guards and 30 cleaners in all years between 1985 and 2006. Standard deviations are in parentheses.

Figure D.1: Map of microregions with entropy-balancing weights
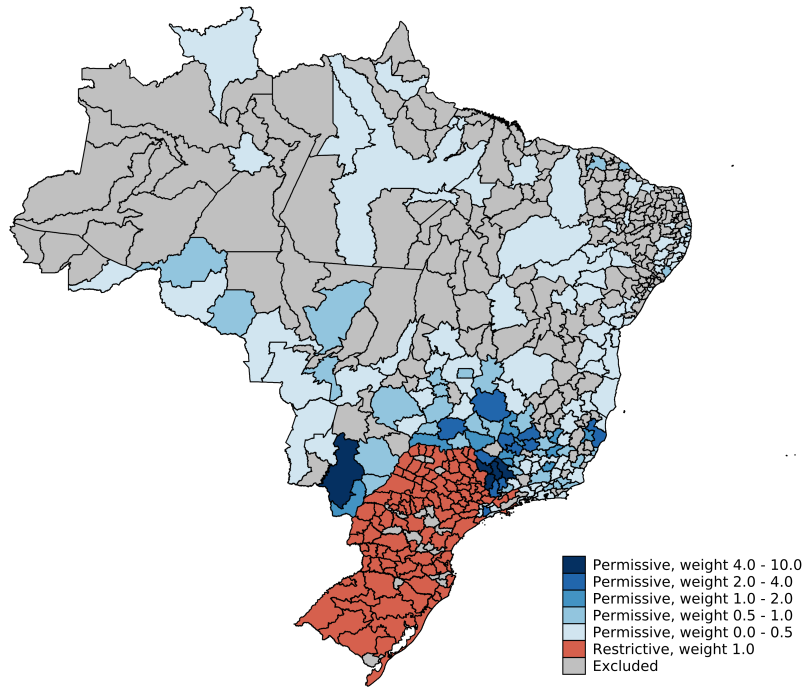
Note: Map shows microregions used in our main specifications, where observations are weighted using entropy balancing weights. Entropy balancing weights are computed using log employment, homicide rate, unemployment rate, share of employment in tradable industries, log formal employment of importers, and local import tariff competition exposure in 1992.

Figure D.2: Changes in outsourcing prevalence and distance to the state border

A. Relative change in contract-firm share

B. Relative change in occupational HHI
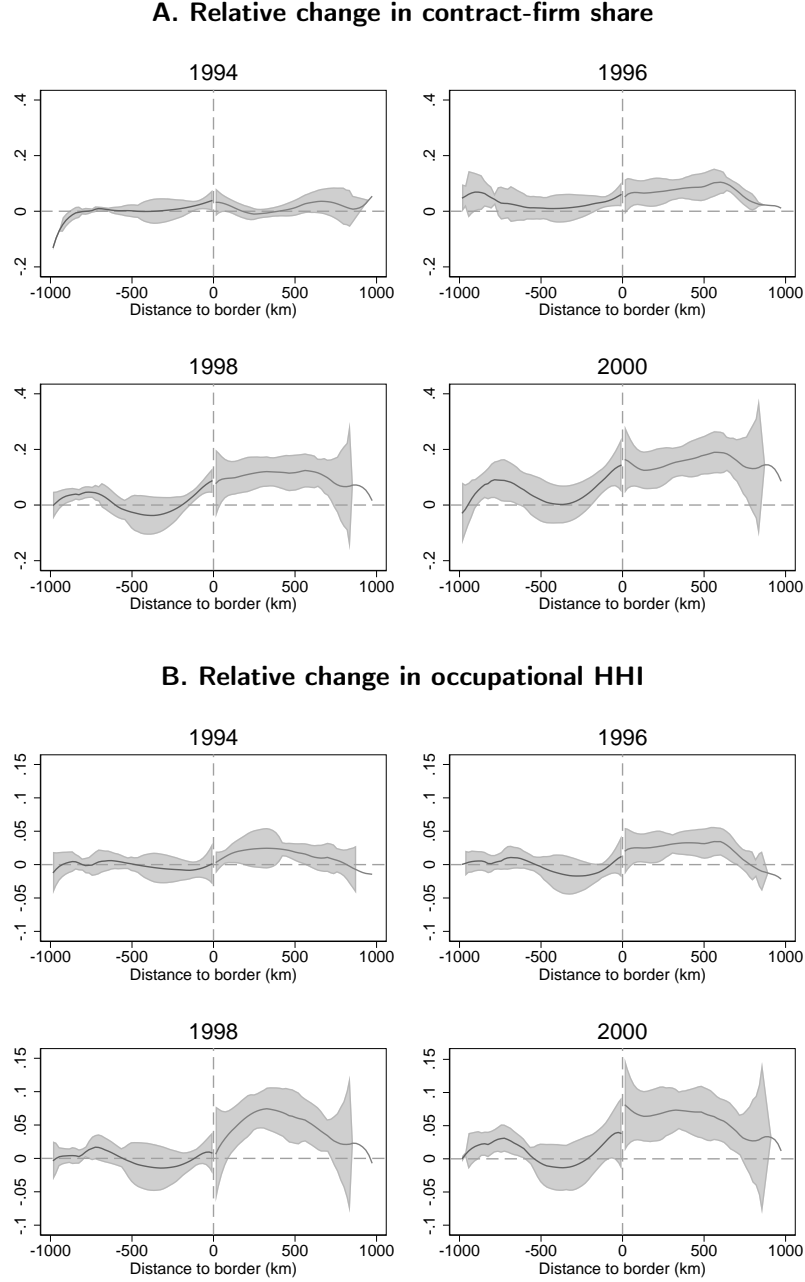
Note: We estimate kernel-weighted local polynomial regressions of the relative change of an outcome in year X on distance to border, using the Epanechnikov kernel, polynomials of degree 2, and a bandwidth of 200km, separately for restrictive and permissive microregions. We define the relative change 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 D.3: Differences in outsourcing prevalence: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

1. Outsourced employment share

B.1 Region Effect, by Occupation **C.1 Guard Effect, by Region**

2. Employment Concentration (HHI)

B.1 Region Effect, by Occupation **C.2 Guard Effect, by Region**
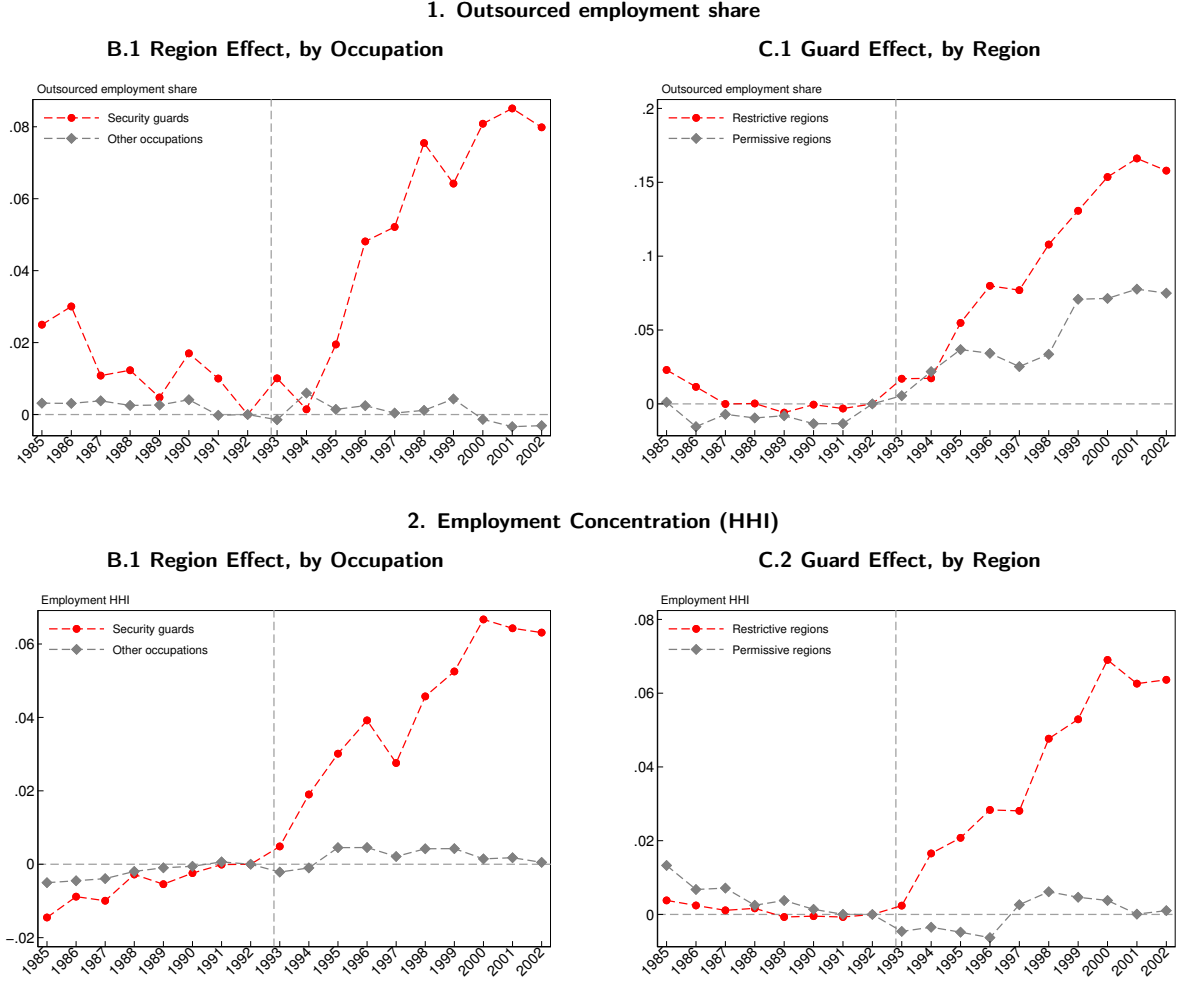
Note: Panel B repeats Figure 4 Panel B. Panel C plots α_τ coefficients from the DD regression $y_{ot} = \sum_{\tau=1985; \tau \neq 1992}^{2002} \alpha_\tau (T_o \times 1_{t=\tau}) + \delta_o + \delta_t + \epsilon_{ot}$ where $T_o = 1$ if occupation o is security guards, zero otherwise, δ_o and δ_t are occupation and year fixed effects, respectively, separately estimated for the occupations in restrictive versus permissive microregions. Occupation-level outcomes are averages across microregions, with microregion balancing weights. The resulting occupation-level regressions weigh occupations equally. Results in both panels are based on a balanced sample of 265 microregions and 12 large 2-digit occupational groups, with positive employment in all microregions in all years, accounting for over 98% of all security guards in the data. See Appendix C for details. Legalization's baseline year of 1992 is omitted in both panels.

Figure D.4: Differences in employment by age groups: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

1. Log total employment of workers aged 18 to 24

B.1 Region Effect, by Occupation

C.1 Guard Effect, by Region

2. Log total employment of workers aged 50 to 64

B.2 Region Effect, by Occupation

C.2 Guard Effect, by Region
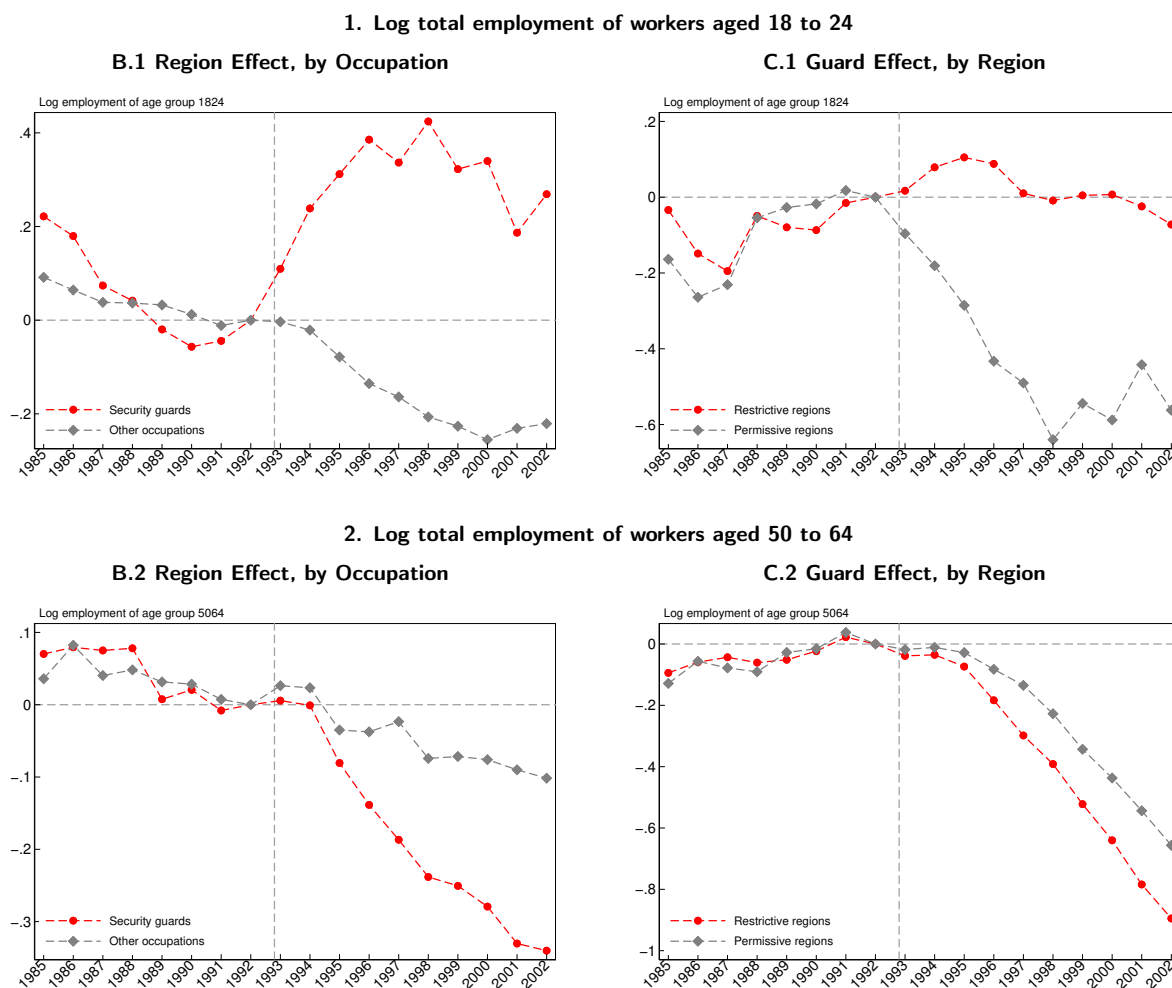
Note: See footnotes to Figure D.3.

Figure D.5: Differences in employment: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

1. Log number of workers from unemployment or informality

B.1 Region Effect, by Occupation

C.1 Guard Effect, by Region

2. Log total employment

B.2 Region Effect, by Occupation

C.2 Guard Effect, by Region
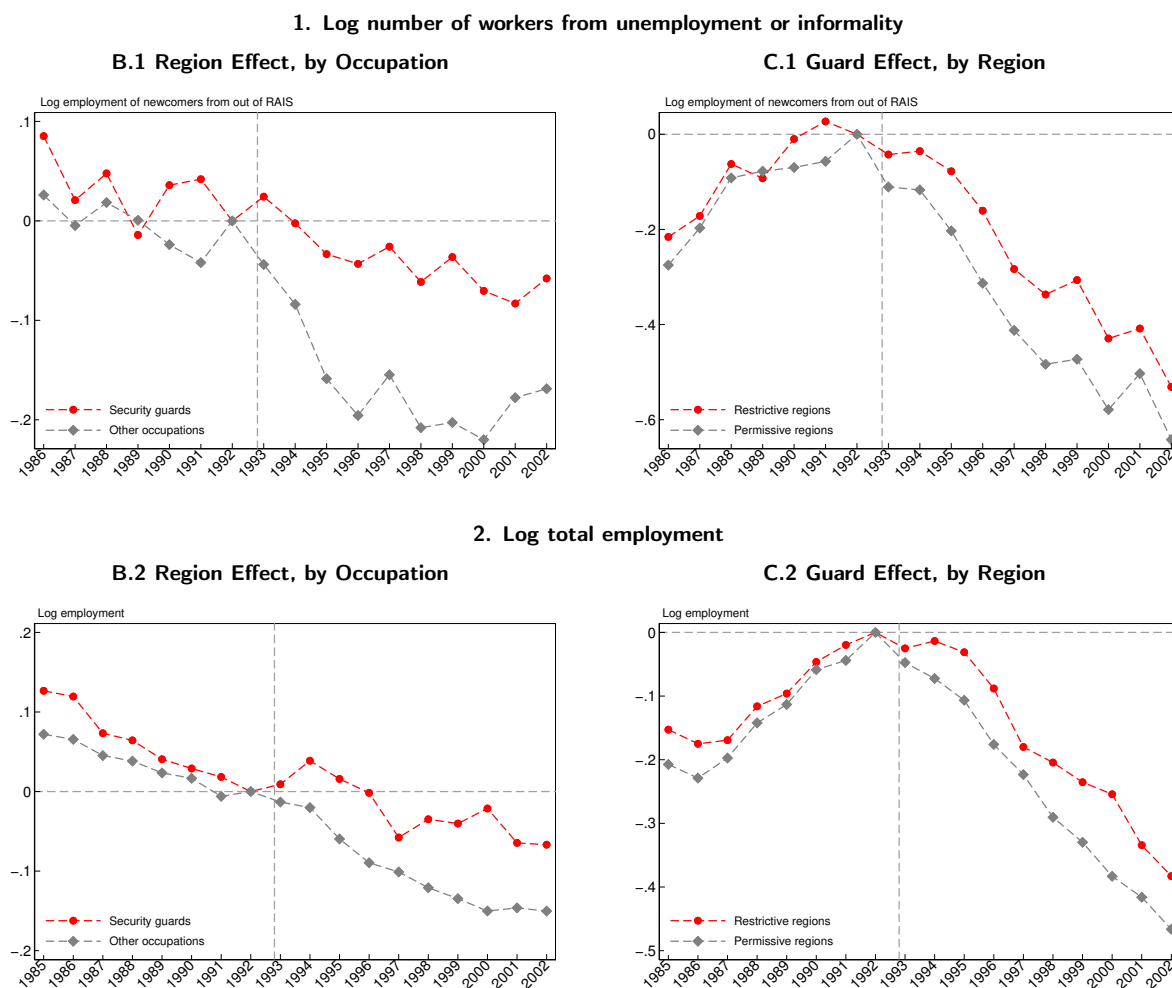
Note: See footnotes to Figure D.3.

Figure D.6: Differences in wages: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

1. Log earnings (as reported, unadjusted)

B.1 Region Effect, by Occupation

C.1 Guard Effect, by Region
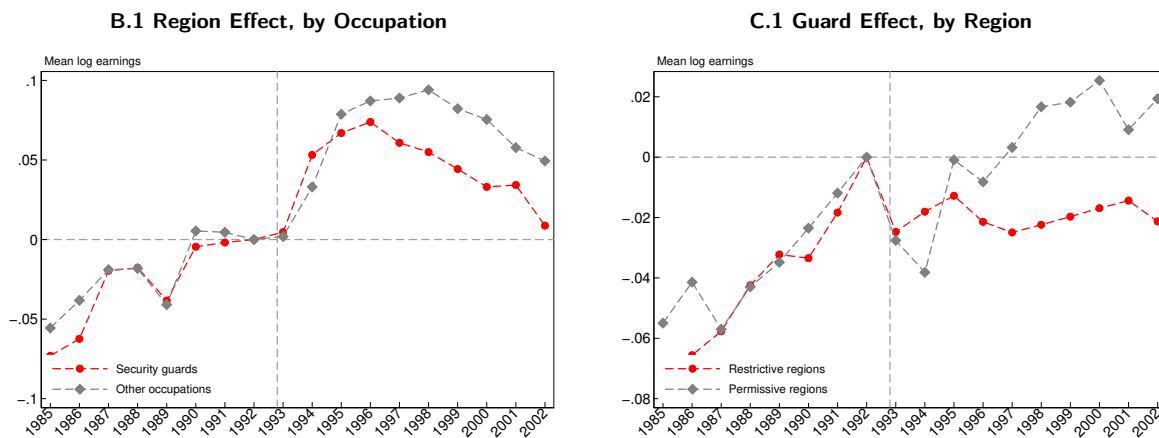
B. Log earnings conditional on demographics (age, education, gender)

B.2 Region Effect, by Occupation

C.2 Guard Effect, by Region

3. Log earnings conditional on worker fixed-effects and time-varying

B.3 Region Effect, by Occupation

C.3 Guard Effect, by Region
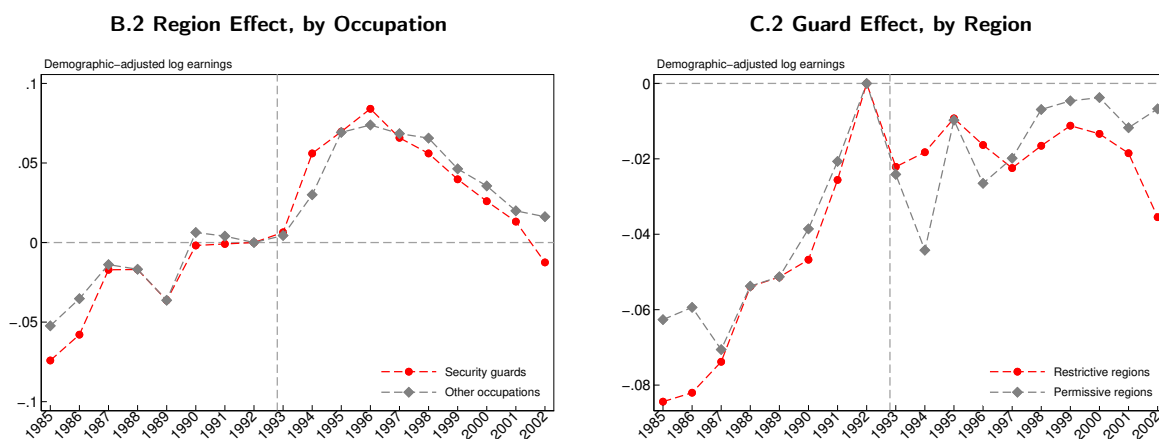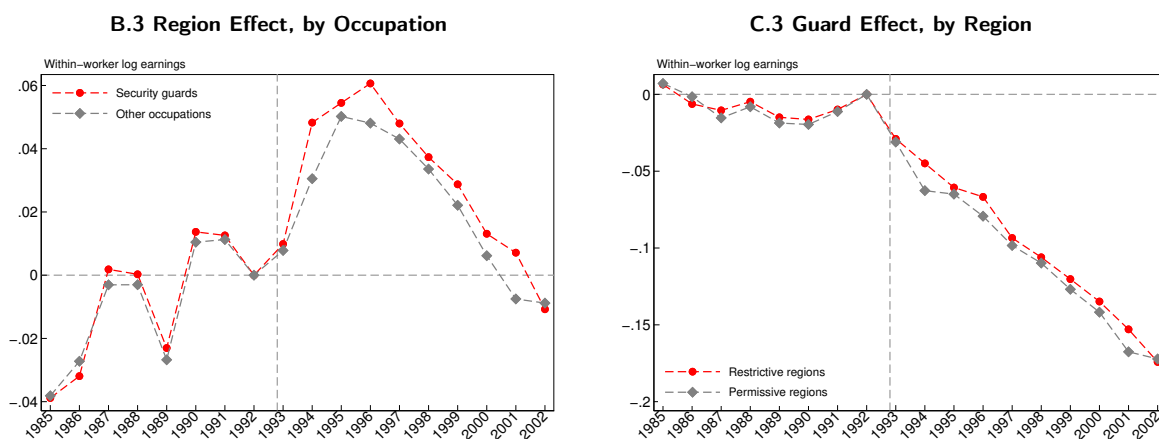
Table D.2: Robustness to balancing baseline covariates: Effect of legalization on employment

	Legalization effect (Weighted DDD)				
	Main specification	Adding balancing on informality	Not balancing on unemp. rate	Not balancing on trade exposure	Observations (region x occup x year)
	(1)	(2)	(3)	(4)	(5)
Panel A: Log total employment					
Log total employment	0.052	0.059	0.031	0.028	57240
	(0.011)	(0.020)	(0.009)	(0.013)	
Panel B: Log employment by contract type					
Direct-hire	-0.048	-0.054	-0.061	-0.067	57240
	(0.019)	(0.024)	(0.018)	(0.021)	
Outsourced	0.099	-0.373	0.193	0.136	57240
	(0.145)	(0.195)	(0.145)	(0.144)	
Panel C: Log employment origins					
Workers from outside RAIS (e.g., unemployment, informality, nilf)	0.093	0.048	0.044	0.075	54060
	(0.026)	(0.024)	(0.022)	(0.026)	
Workers from inside RAIS	0.032	0.061	0.023	0.004	54060
	(0.010)	(0.022)	(0.013)	(0.013)	

Notes: See notes to Table 2. Log employment is expressed in natural logs.

Table D.3: Robustness to balancing baseline covariates: Effect of legalization on employment composition

	Legalization effect (Weighted DDD)				
	Main specification	Adding balancing on informality	Not balancing on unemp. rate	Not balancing on trade exposure	Observations (region x occup x year)
	(2)	(2)	(3)	(4)	(5)
Log employment by worker age group					
18-24	0.422	0.289	0.410	0.379	57240
	(0.036)	(0.031)	(0.025)	(0.030)	
25-29	0.193	0.218	0.202	0.153	57240
	(0.031)	(0.022)	(0.023)	(0.025)	
30-39	0.060	0.066	0.043	0.034	57240
	(0.023)	(0.037)	(0.023)	(0.023)	
40-49	-0.050	-0.058	-0.106	-0.081	57240
	(0.031)	(0.046)	(0.026)	(0.032)	
50-64	-0.140	-0.159	-0.198	-0.144	57240
	(0.023)	(0.036)	(0.006)	(0.020)	

Notes: See notes to Table 2. Log employment is expressed in natural logs.

Table D.4: Robustness to balancing baseline covariates: Effect of legalization on wages

| | Legalization effect (Weighted DDD) | | | | |
	Main specification	Adding balancing on informality	Not balancing on unemp. rate	Not balancing on trade exposure	Observations (region x occup x year)
	(2)	(2)	(3)	(4)	(5)
Panel A: Real December log earnings					
As reported (i.e., unconditional)	-0.016	-0.002	-0.007	-0.026	57240
	(0.015)	(0.022)	(0.014)	(0.016)	
Conditional on worker demographics	0.003	0.019	0.013	-0.006	57240
	(0.015)	(0.018)	(0.014)	(0.015)	
Conditional on worker FEs	0.004	0.020	-0.005	-0.005	57240
and time-varying demographics	(0.012)	(0.012)	(0.011)	(0.012)	
Panel B: Real December log earnings					
by contract type, as reported					
Direct-hire	-0.011	-0.007	0.005	-0.016	57240
	(0.014)	(0.016)	(0.013)	(0.014)	
Outsourced					
	0.003	-0.036	0.011	-0.008	43628
	(0.020)	(0.029)	(0.020)	(0.020)	

Notes: See notes to Table 6.

Table D.5: Robustness to excluding São Paulo: Effect of legalization on key outcomes

	Legalization effect (Weighted DDD)		
	Main specification	Exclude São Paulo metro	Exclude São Paulo state
	(1)	(2)	(3)
Outsourced share	0.052	0.056	0.035
	(0.011)	(0.011)	(0.011)
Log employment	0.052	0.056	0.035
	(0.011)	(0.011)	(0.011)
Log employment by worker age group			
18-24	0.422	0.427	0.344
	(0.036)	(0.037)	(0.044)
50-64	-0.140	-0.138	-0.120
	(0.023)	(0.024)	(0.026)
Log real December log earnings	0.003	-0.007	0.034
conditional on worker demographics	(0.015)	(0.016)	(0.010)

Notes: See notes to Table 6.

Figure D.7: Effect of legalization on employment of direct-hire incumbent guards
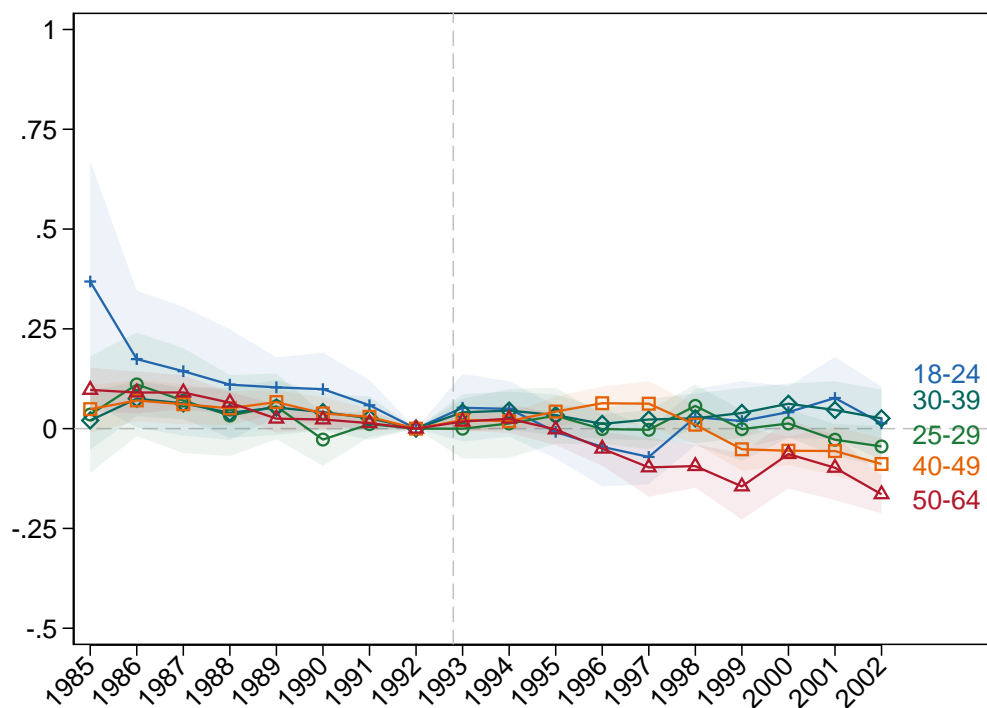
Note: See notes to Figure 7. The sample includes all workers in guard and control group occupations who were directly employed in 1992.

Figure D.8: Effect of outsourcing legalization on worker transitions
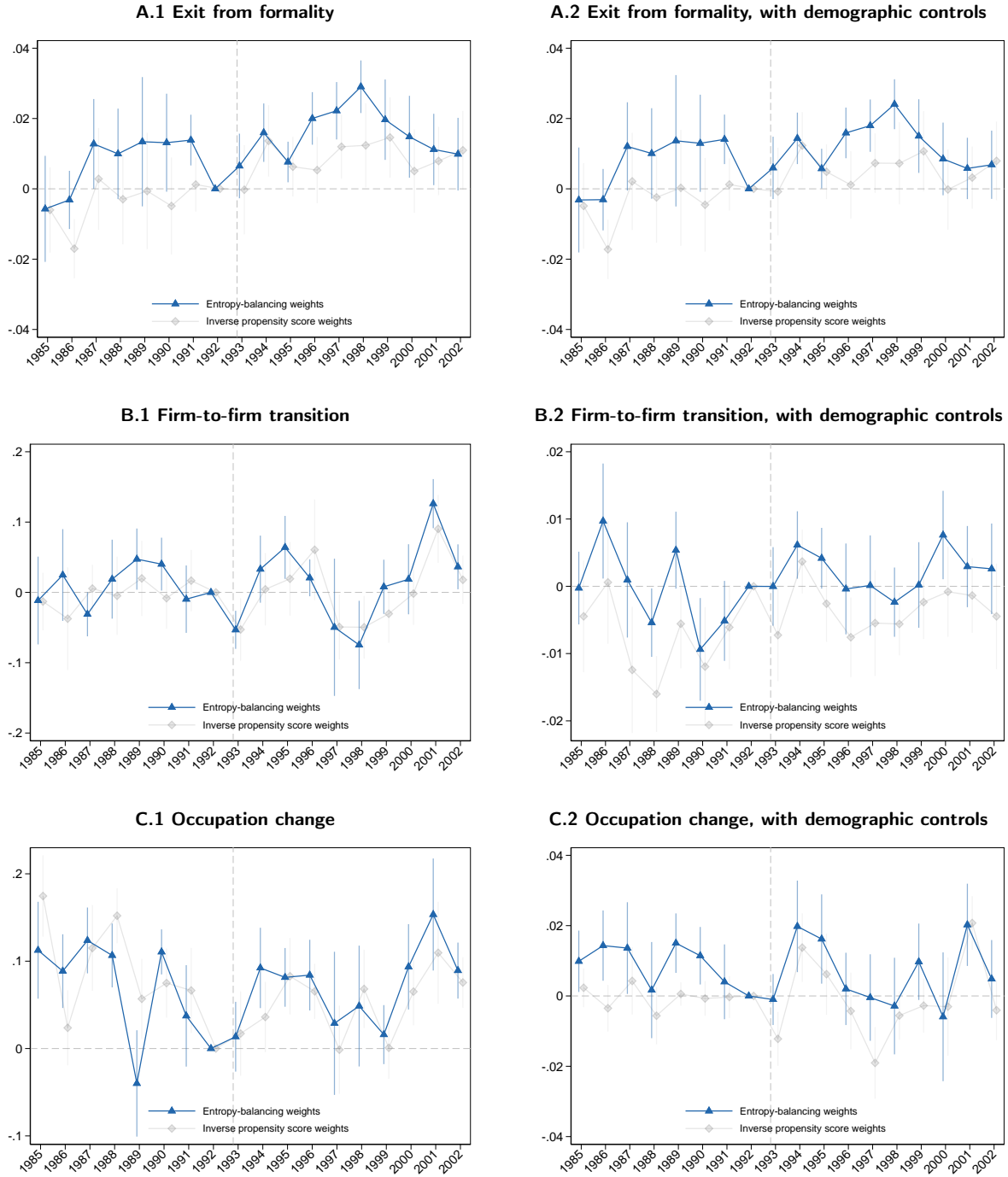
A.1 Exit from formality

A.2 Exit from formality, with demographic controls

B.1 Firm-to-firm transition

B.2 Firm-to-firm transition, with demographic controls

C.1 Occupation change

C.2 Occupation change, with demographic controls

Note: See notes to Figure 4. The outcome variable in panel A is worker age.The outcomes variables in panel B are log employment by different age groups. Age groups are defined according to the age group variable consistently reported throughout the period. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation. XXXXX Edits XXXXX

Figure D.9: Effect of outsourcing legalization on within-worker wage growth

A.1 Wage growth, conditional on formal employment
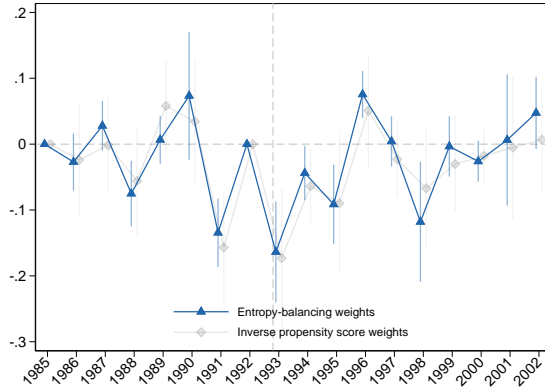
A.2 Wage growth, conditional on formal employment, with demographic controls
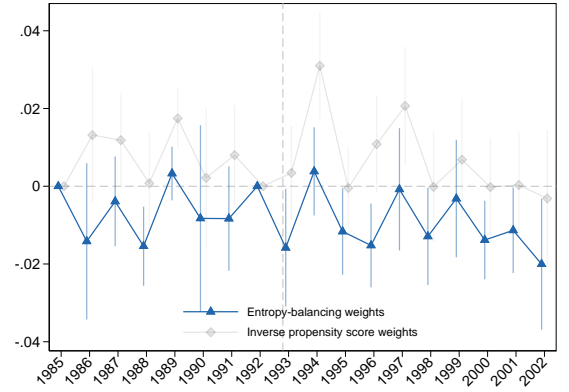
B.1 Within-firm wage growth
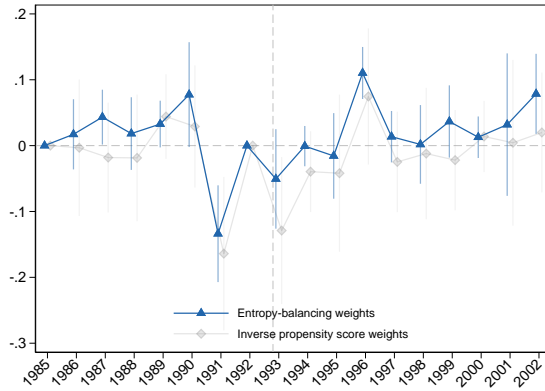
B.2 Within-firm wage growth, with demographic controls
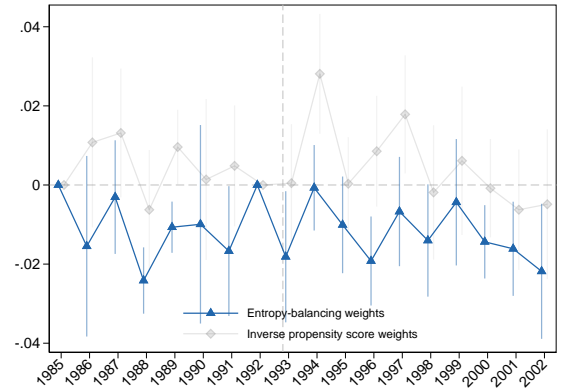
Note: See notes to Figure 4. The outcome variable in panel A is worker age.The outcomes variables in panel B are log employment by different age groups. Age groups are defined according to the age group variable consistently reported throughout the period. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation. XXXXX Edits XXXXX

Figure D.10: Regional differences in homicide rates

A. Raw trend

A. Diff-in-diff estimates
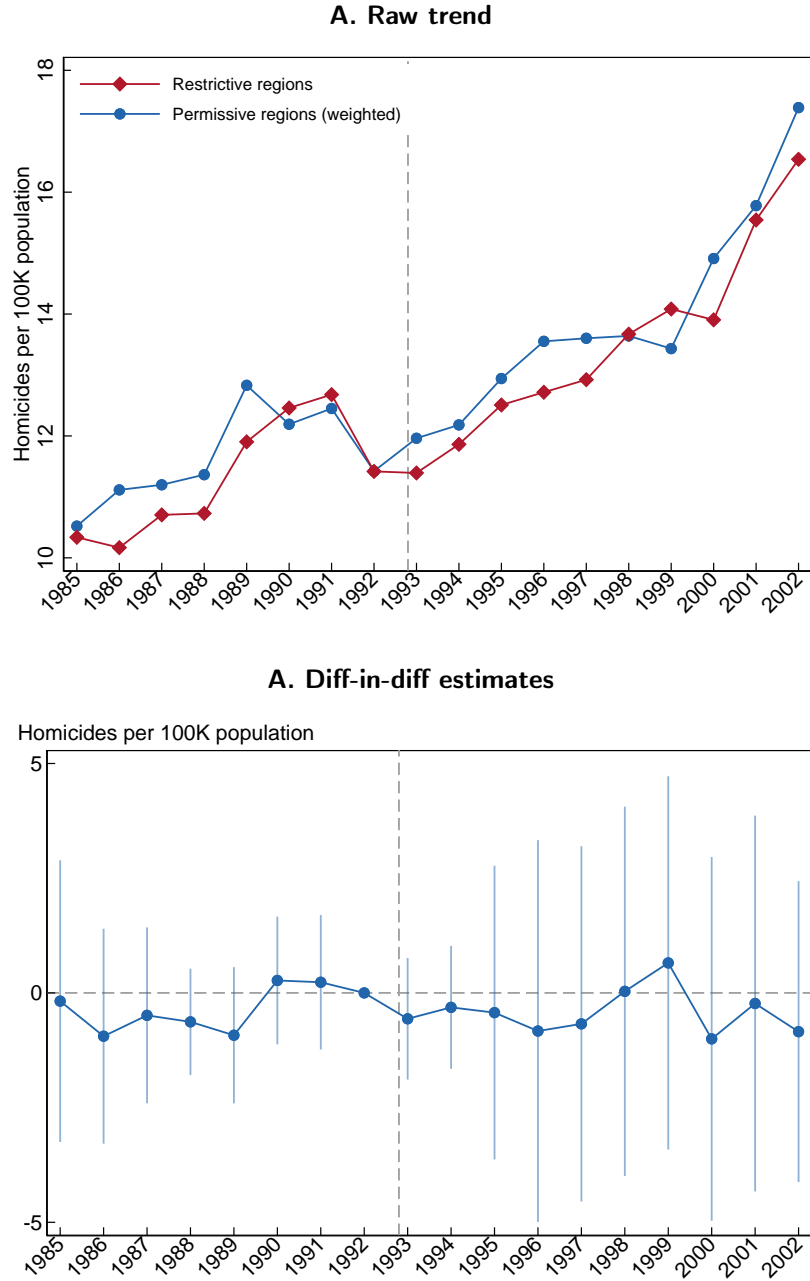
Note: Panel A plots the mean homicide rate for restrictive and permissive microregions, respectively. Panel B plots the coefficients from a DD regression that comparing restrictive microregions to permissive microregions, with controls for year and microregion fixed effects. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

Figure D.11: Effect of legalization on direct-hire employment, alternative definitions
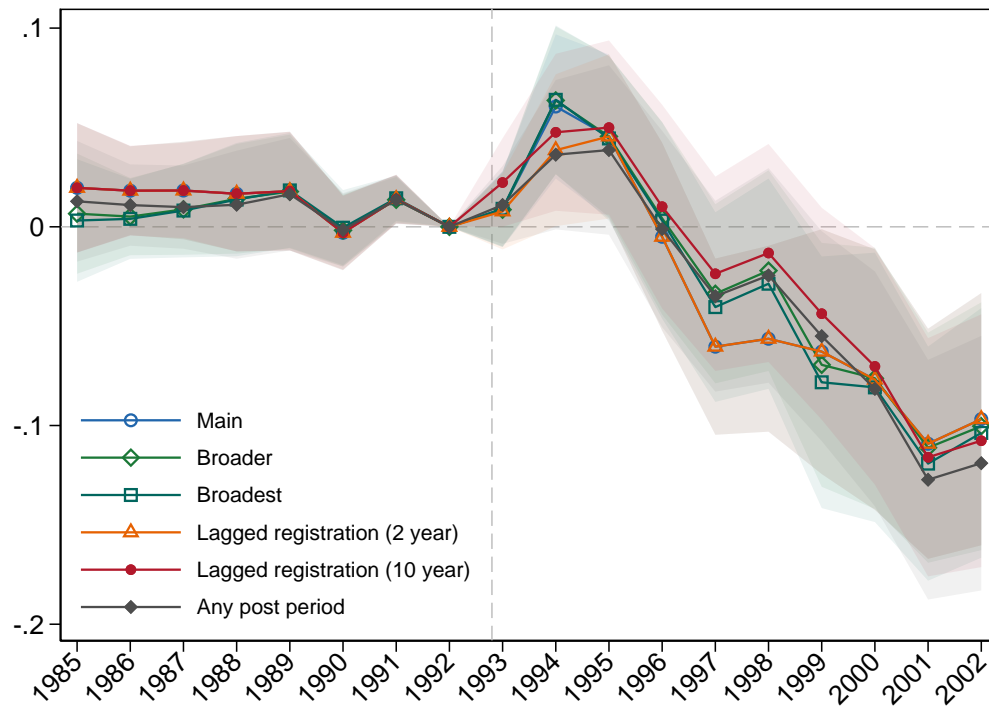
Note: See notes to Figure 7. "Main" uses the same contract firm definition used throughout the paper. "Broader" uses a broader definition, including a larger set of 6-digit CNAE codes. "Broadest" uses an even broader definition, including almost all 6-digit codes within the CNAE class 72 and 74. "Lagged registration (2 year)" allows for up to 2 years of delay in registration as business service firm" (since CNAE95 starts two years after legalization). "Lagged registration (10 year)" allows for up to 10 years of delay in registration as business service firm. "All post period" defines contract firms as any firm that registers as a business service firm in any post-period year.

E   Differences in Wages and Transitions between Outsourced and Direct-hire Workers

Table E.1: Outsourcing wage differential

	Dependent variable: Log real monthly wage			
	(1)	(2)	(3)	(4)
Outsourced	-0.016	-0.019	0.059	-0.007
	(0.001)	(0.001)	(0.001)	(0.001)
Outsourced X Tenure			-0.020	-0.008
			(0.000)	(0.000)
Tenure			0.050	0.026
			(0.000)	(0.000)
Observations	2870865	2456800	2870865	2456800
R2	0.34	0.91	0.45	0.91

Note: Sample is all guards between ages 18-64 employed between 1997-2002. All regressions include controls for year FE, microregion FE, and education level interacted with age, age squared and age cubed. Standard errors are clustered at the individual level and presented in parentheses.

Table E.2: Outsourcing hazard differential

	(1)	(2)	(3)	(4)
Dependent variable: Exit from formality				
Outsourced	0.004	0.003	-0.010	-0.007
	(0.001)	(0.001)	(0.001)	(0.001)
Outsourced X Tenure			0.003	0.002
			(0.000)	(0.000)
Tenure			-0.012	-0.009
			(0.000)	(0.000)
AKM Worker FE		X		X
Observations	2870865	2836925	2870865	2836925
R2	0.30	0.31	0.31	0.32

Note: Sample is all guards between ages 18-64 employed between 1997-2002. All regressions include controls for year FE, microregion FE, and education level interacted with age, age squared and age cubed. Standard errors are clustered at the individual level and presented in parentheses. The dependent variable is equal to one if and only if the worker does not remain formally employed in the following year.

Table E.3: Outsourcing occupation switching differential

	(1)	(2)	(3)	(4)
	Any firm		Same firm	Different firm
Outsourced	-0.026	-0.026	-0.027	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Outsourced X Tenure		-0.000		
		(0.000)		
Tenure		-0.000		
		(0.000)		
Observations	2870865	2870865	2870865	2870865
R2	0.02	0.02	0.02	0.00

Dependent variable: Occupation switching

Note: Sample is all guards between ages 18-64 employed between 1997-2002. All regressions include controls for year FE, microregion FE, and education level interacted with age, age squared and age cubed. Standard errors are clustered at the individual level and presented in parentheses. In Columns (1) and (2), the dependent variable is equal to one if and only if the worker remains employed and the 2-digit CBO94 occupation code in the following year is different from the current year. In Column (3), the dependent variable is equal to one if the worker additionally remains in the same firm. In Column (4), the dependent variable is equal to one if the worker instead moves to a different firm.

Table E.4: Outsourcing managerial promotion differential

	(1)	(2)	(3)	(4)
	Dependent variable: Managerial promotion			
	Any firm		Same firm	Different firm
Outsourced	-0.001	-0.001	-0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Outsourced X Tenure		0.000		
		(0.000)		
Tenure		0.000		
		(0.000)		
Observations	2870865	2870865	2870865	2870865
R2	0.00	0.00	0.00	0.00

Note: Sample is all guards between ages 18-64 employed between 1997-2002. All regressions include controls for year FE, microregion FE, and education level interacted with age, age squared and age cubed. Standard errors are clustered at the individual level and presented in parentheses. In Columns (1) and (2), the dependent variable is equal to one if and only if the worker remains employed and the 2-digit CBO94 occupation code in the following year is a managerial occupation. In Column (3), the dependent variable is equal to one if the worker additionally remains in the same firm. In Column (4), the dependent variable is equal to one if the worker instead moves to a different firm.

F Firm-level Outsourcing Events: Details

F.1 On-site and Occupational Layoff Outsourcing Events

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 to perform the same job.

Goldschmidt and Schmieder (2017) use a more stringent definition for on-site outsourcing. 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.

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 $t-1$. Note that 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 also exclude establishments who non-guard employment fell by more than 10 percent. Figure F.1 visualizes this definition by plotting the average number of security guards and non-security guard employees at the establishment level during the years before and after an occupational layoff.

Figure F.1: Establishment employment before and after occupational layoff

A. Employment of security guards

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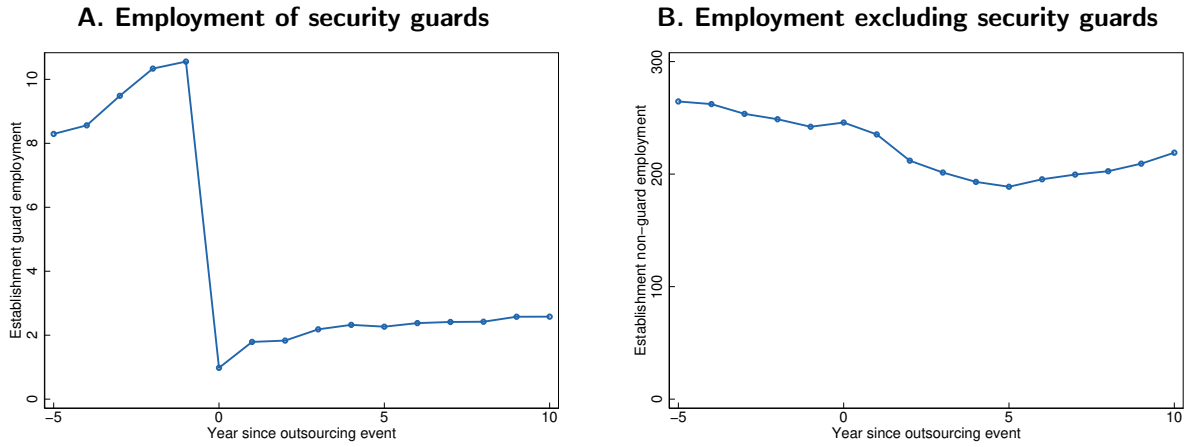
Note: Figure plots the number of security guards and non-security guard employees for the years before and after an occupational layoff.

Figure F.2: Frequency of occupational layoffs by region restrictiveness
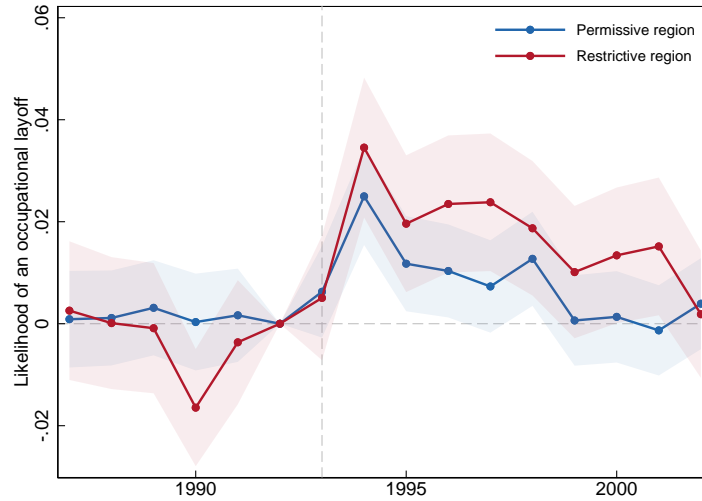
Note: This figure plots coefficients from a linear probability model with separate year fixed effects for restrictive and permissive regions. We cluster standard errors at the establishment level.

F.2 Matching Workers Affected by Occupational Layoff to a Control Group

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. For each treated worker, we then choose the non-outsourced worker with the closest propensity score to the comparison worker. Table F.1 presents summary statistics for the matched worker sample.

Table F.2: Effects of occupational layoffs on incumbent workers

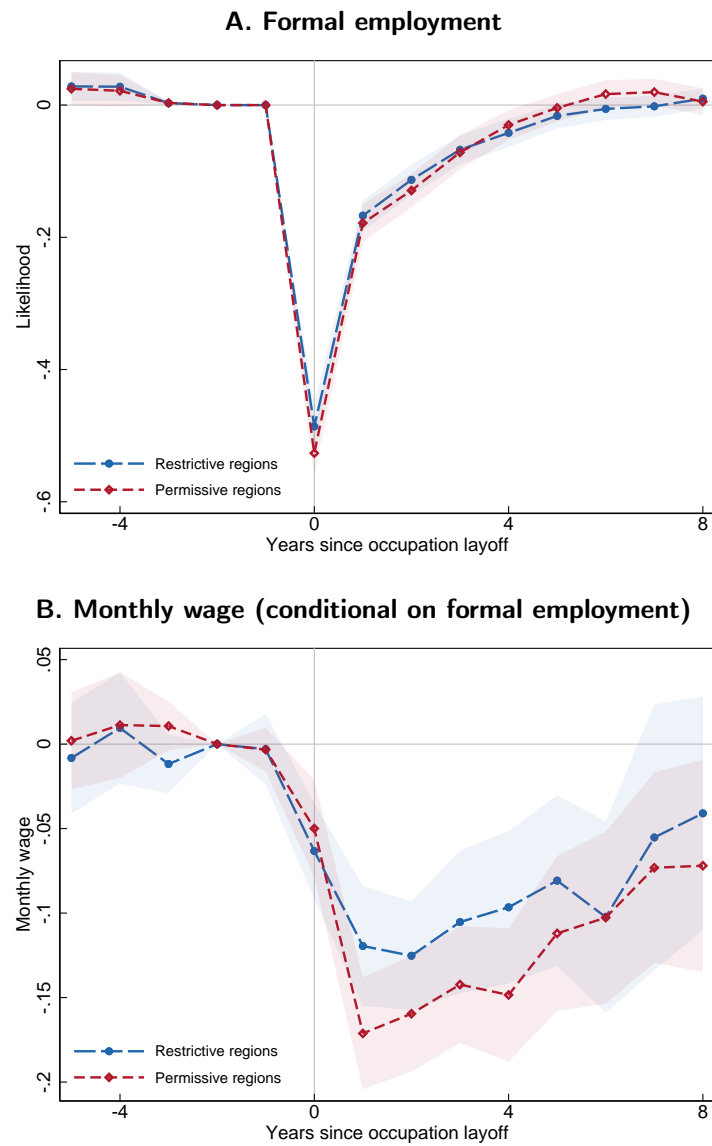|	Years since occupational layoff		
	Y0	Y1	Y5
Formally employed	-0.493	-0.159	-0.025
	(0.013)	(0.010)	(0.010)
Formally employed in same occupation	-0.761	-0.399	-0.117
	(0.007)	(0.011)	(0.010)
Formally employed in contract firm	0.002	0.077	0.041
	(0.001)	(0.005)	(0.005)
Monthly wage (relative to base 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 AKM FE effect	-0.032	-0.080	-0.052
	(0.005)	(0.006)	(0.011)
As fraction of wage losses	41%	43%	46%

Notes: Entries give estimated effects of occupational layoff on the indicated outcome in years zero (Y0), one (Y1) and five (Y5) after the event. Employment effects are estimated on a balanced sample of incumbents and their matched control workers. Wage effects are estimated conditional on workers remaining in RAIS. Wage losses due to firm effects shown both as log points and as a percentage of wage losses (e.g., -.032/-0.78 = 41% in Y0). Standard errors clustered at the firm level are in parentheses.

Table F.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)
Imputed 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.

Figure F.3: Effect of occupational layoff on incumbent guards, by region

A. Formal employment

**B. Monthly wage (conditional on formal employment)**

Note: Figure replicates Figure 11, Panels B and D using subsamples including only restrictive and permissive microregions, respectively.

Figure F.4: Effect of occupational layoff on incumbent guards, alternative matching strategies

A. Formal employment

**B. Monthly wage (conditional on formal employment)**
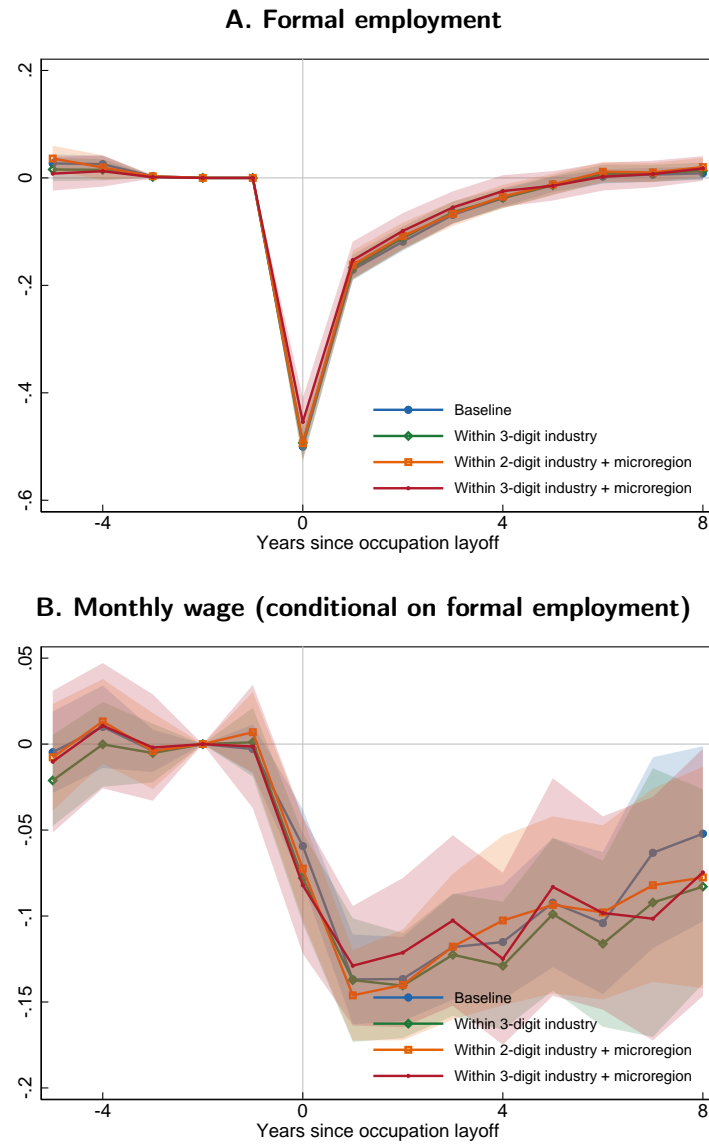
Note: Figure replicates Figure 11, Panels B and D with alternative matching strategies. The baseline specification matches workers within the 2-digit industry and local regional court jurisdiction. The alternative specifications matches within the 3-digit industry and local regional court jurisdiction (in green), within the 2-digit industry and microregion (in orange), and within the 3-digit industry and microregion (in red).

Figure F.5: Effect of occupational layoff on incumbent guards, alternative definitions

A. Formal employment

**B. Monthly wage (conditional on formal employment)**
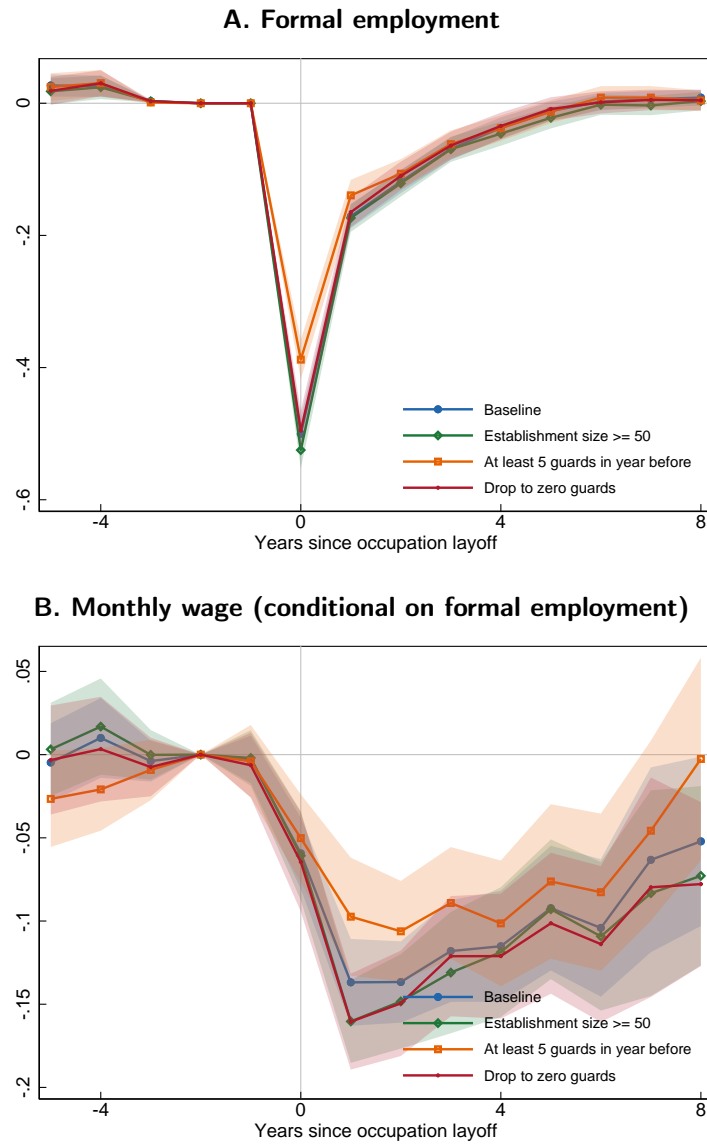
Note: Figure replicates Figure 11, Panels B and D with alternative definitions of occupational layoffs. The alternative specifications restricts to outsourcing establishments with at least 50 employees (in green), at least 5 security guards (in orange), or considers events where the number of security guards drops to zero (in red).

Figure F.6: Effect of occupational layoff on incumbent employment, other occupations
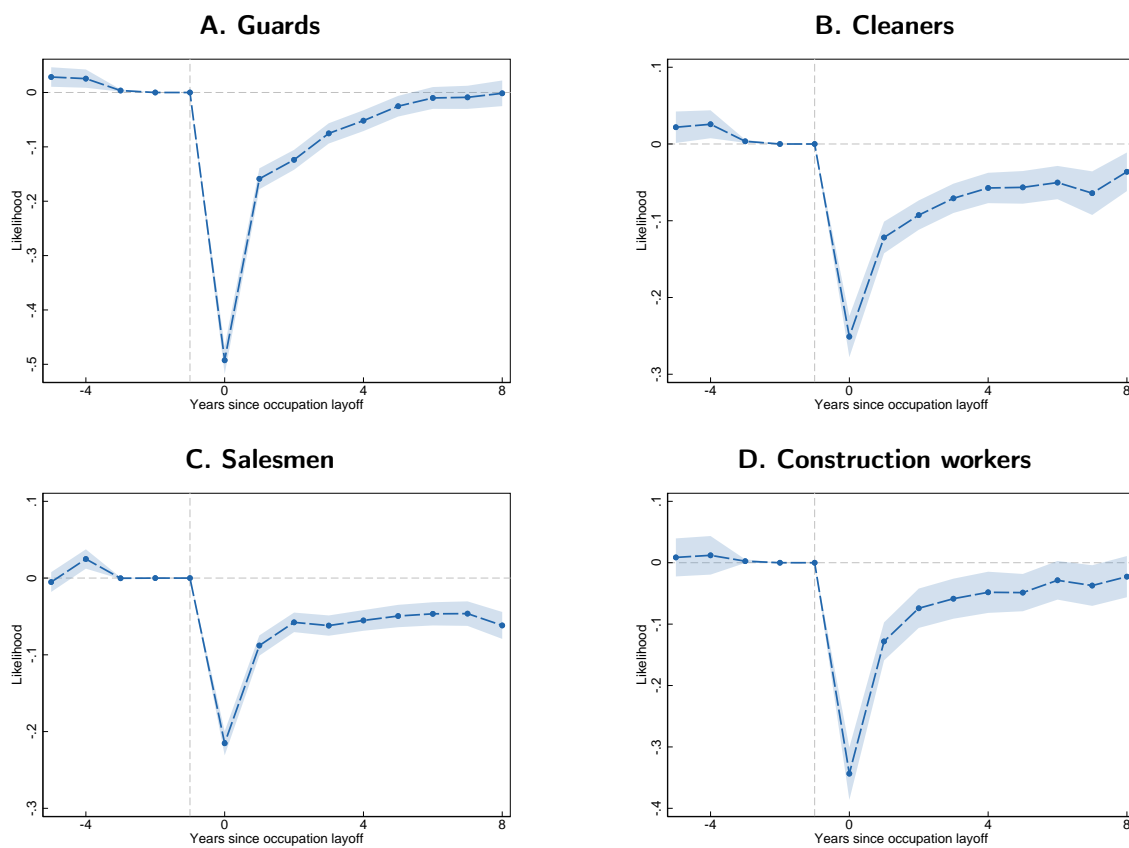

Note: Figure replicates Figure 11, Panel B for occupational layoffs in other occupations.

Figure F.7: Effect of occupational layoff on incumbent wage, other occupations

A. Guards

B. Cleaners

C. Salesmen

D. Construction workers
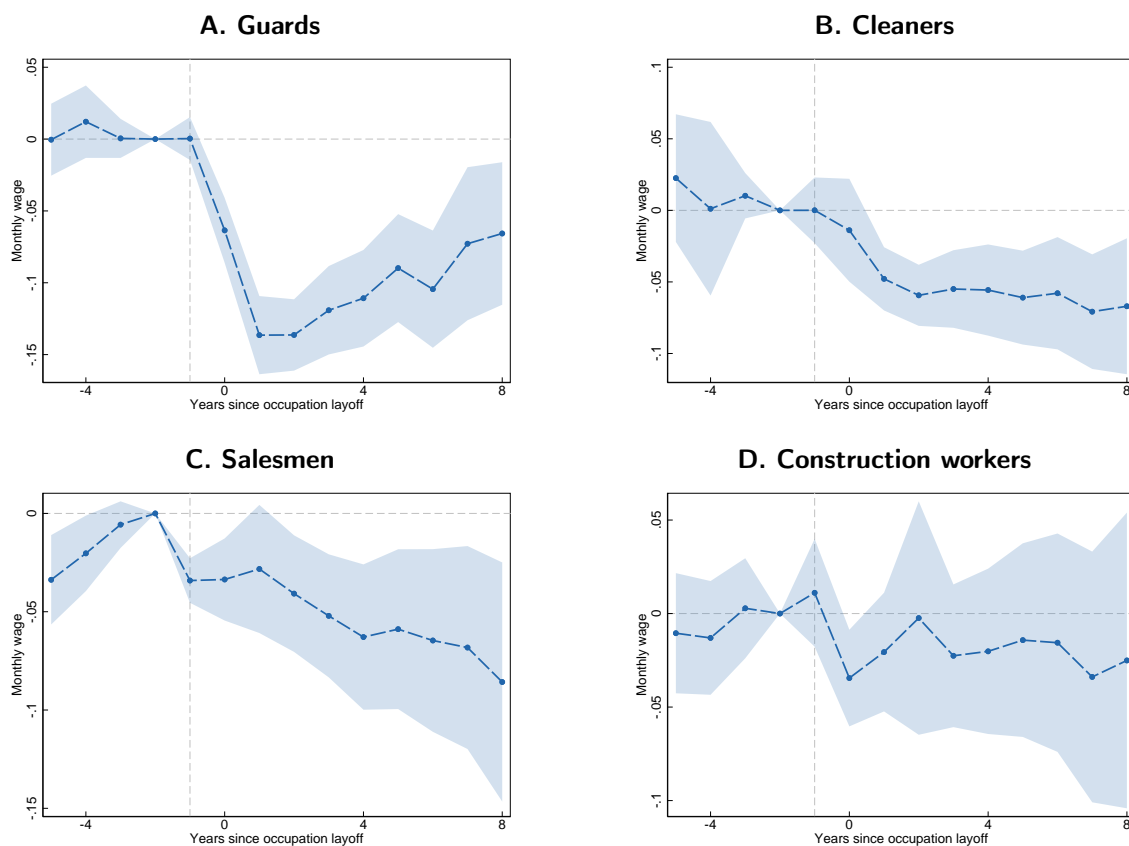
Note: Figure replicates Figure 11, Panel D for occupational layoffs in other occupations.

Figure F.8: Effect of occupational layoffs on establishment and occupation stayers
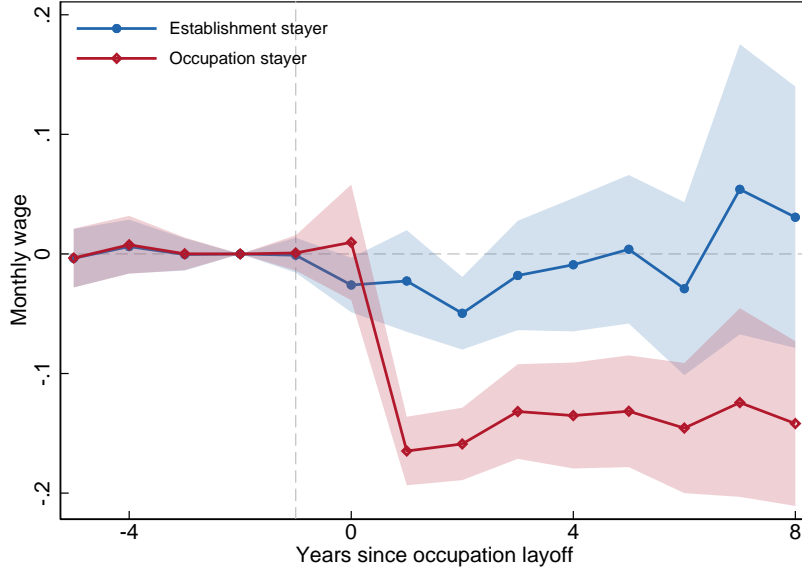
Note: Figure plots coefficient γ_τ from a DD regression measuring the impact of a firm occupational layoff on incumbent direct-hire security guards, where the control group are similar workers in establishments that did not have an occupational layoff. The outcome variable is monthly wage, as measured 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.

F.3 Estimation of Present Discounted Value of Earnings Losses

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

$$PDV_{Loss} = \sum_{k=0}^{8} \hat{\delta}_k \frac{1}{(1+r)^k} + \sum_{k=8}^{19} \hat{\delta}_8 \frac{\left(1+\hat{\lambda}\right)^{k-8}}{(1+r)^k} \tag{6}$$

where $\hat{\delta}_k$ is the average estimated earnings loss in year k after occupational layoff, estimated using equation (5), 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.Appendix Table F.3 summarizes our results according to these various methods:

Table F.3: PDV of earnings losses, as multiple of baseline 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

The first method 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).

F.4   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:

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

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

Table F.4: Establishment-level predictors of security guard 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 establishment 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.

Figure F.9: Effect of occupational layoff on incumbent employment, by initial firm wage premia

Formal employment
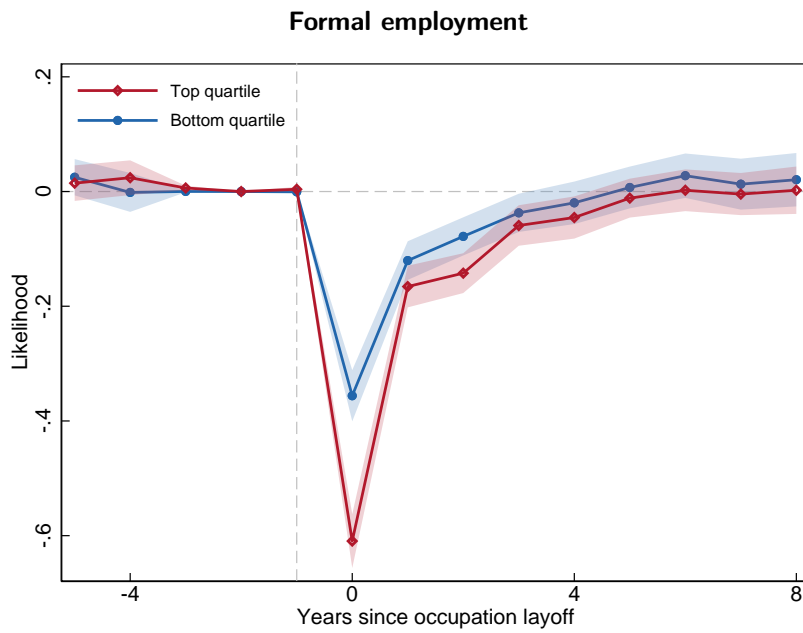
Note: Figure replicates Figure 11, Panel B using subsamples that include only workers initially in the top and bottom quartile of the AKM firm effects distribution, respectively.

Figure F.10: Trends in average firm-specific wage premia among security guards
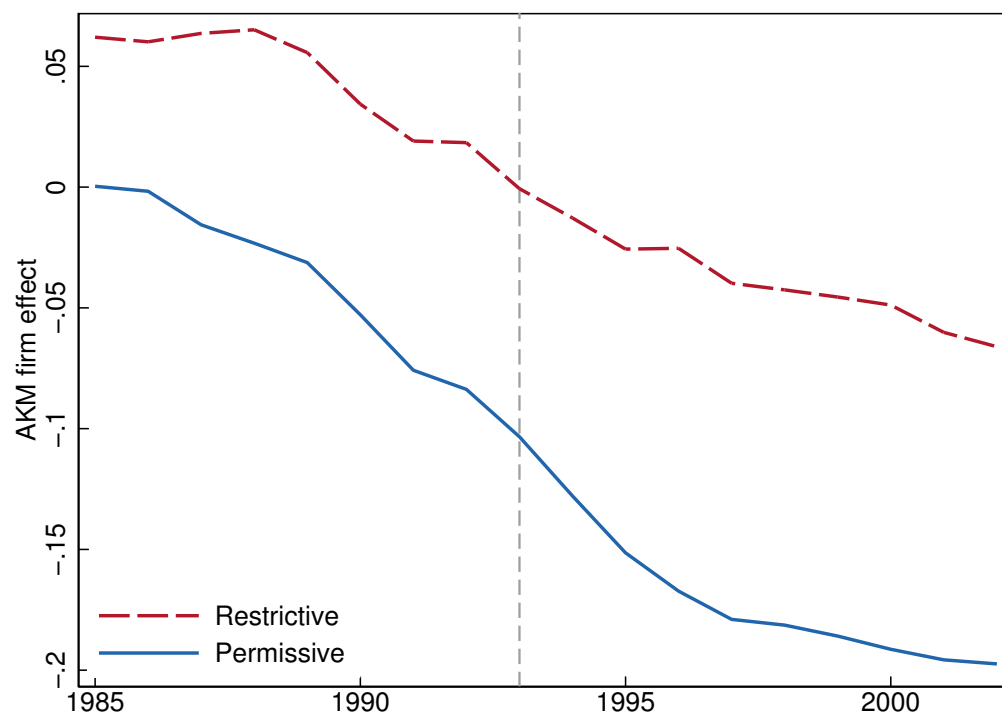
Note: Figure plots the trend in the mean AKM firm effect among security guards, averaged over microregions in our estimation sample with equal weights, separately for permissive and restrictive regions.