

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

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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, *unadjusted* 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-

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

¹Field 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.

²Despite 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

³Research 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.

⁴Bilal 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

⁵In 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.

⁶It 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.

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 $E[y_{ij}] = \pi y_j$. Therefore, if the incumbent worker's realized signal is $\sigma_{ij} = y_j$, then his expected product is $E[y_{ij} \mid \sigma_{ij} = y_j] = [\pi + (1 - \pi)p] y_j$, 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 $E[y_{ij} | s_{ij} = y_j] = [\pi + (1 - \pi)q]y_j$, 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.

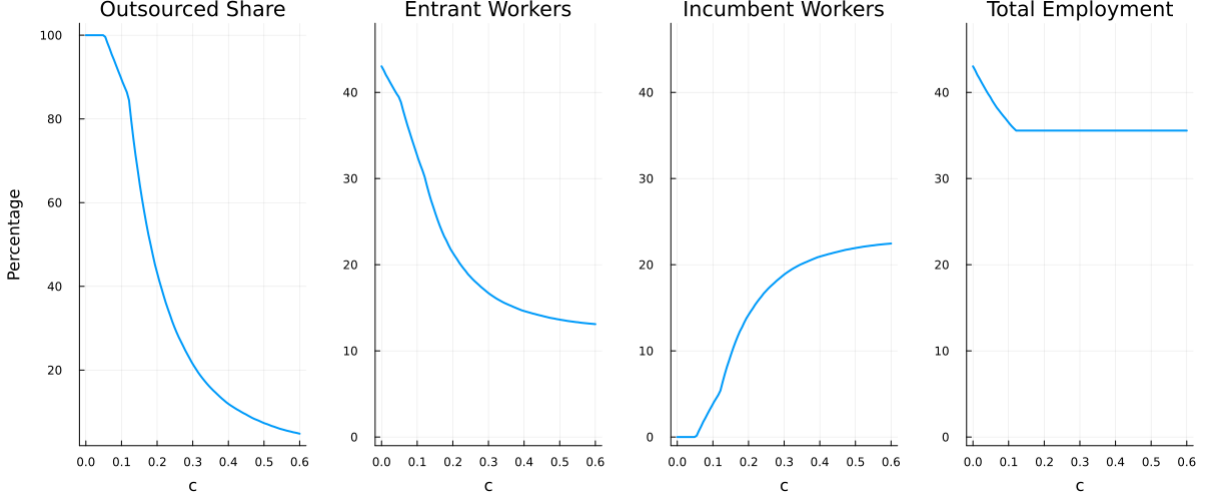
⁷For simplicity, we assume that c is exogenous and satisfies the contract firm’s break-even constraint.

⁸A 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.

⁹For 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 $s_{ij} = y_j$.

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

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 $y_j \sim \text{LogNormal}(0, 1)$. 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, re-assignment, 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

¹¹The 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).

¹²These 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*.

¹³In 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

¹⁴The 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

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.

¹⁵Appeals 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.

¹⁶While 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).

¹⁷Legal 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

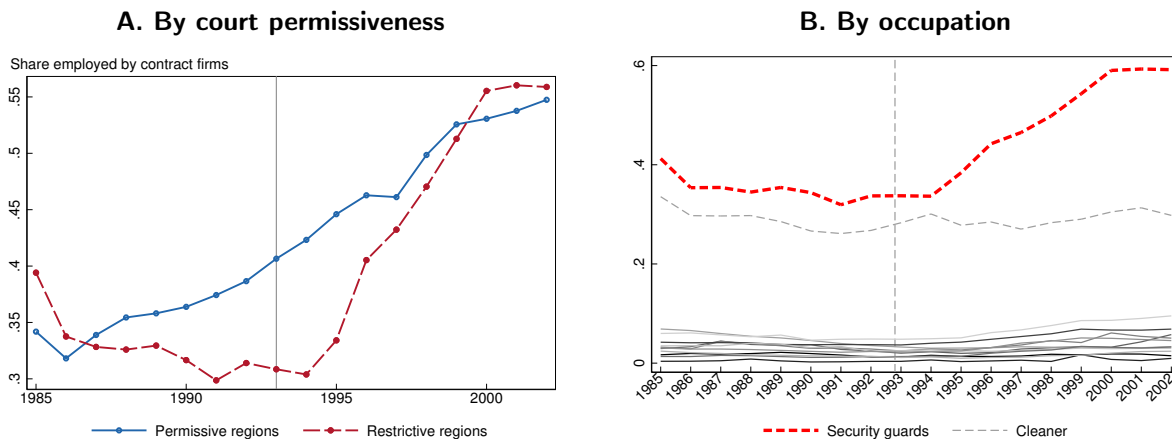
¹⁸Appendix Figure C.1 shows that roughly 80% of all guards were formal during this period (87% to 70% from the early 1980s to the early 2000s) versus 40%–44% for all workers (see Tables 4.8 and 4.9 in IBGE (2000), calculated including self-employed workers and excluding military, the government, and 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.

¹⁹Guards 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).

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

²¹We 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



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.

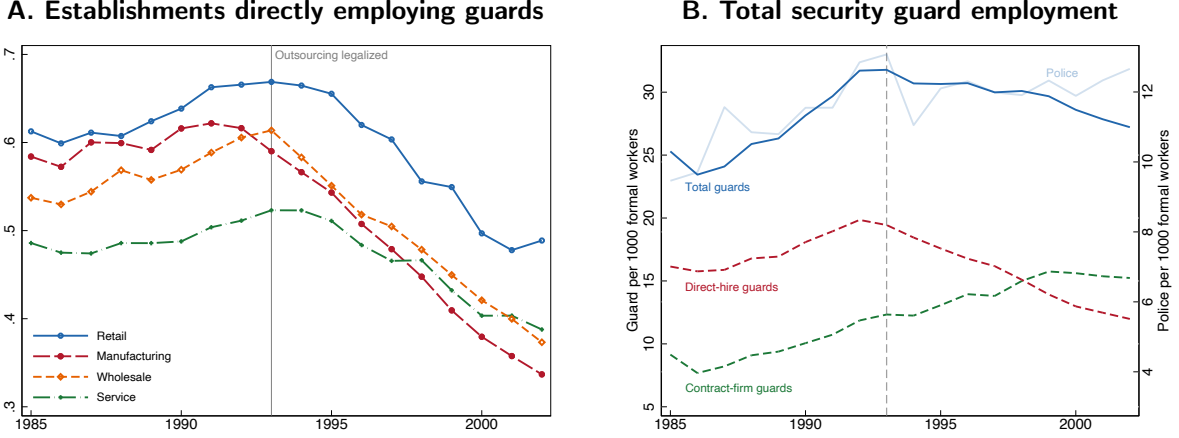
Figure 3, Panel A plots the trend in the share of establishments with at least 50 employees that employ at least one security guard, separately for the manufacturing, services, wholesale, and retail sectors. Across all four sectors, the share was generally steady before 1993 but began to fall sharply 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.

²²Section 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.

²³The 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.

²⁴An 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.

Figure 3: Trends in 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

²⁵See 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.

²⁶Recent 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

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

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:

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

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 services²⁷ 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

²⁷The 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).

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

²⁹Data 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,

³⁰We 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.

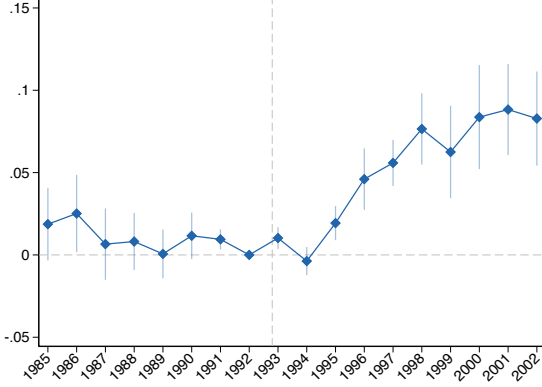
³¹“Roughly” and not “exactly” because the fully saturated fixed effects in the DDD specification (e.g., occupation-region, region-time, and occupation-time fixed effects) are more flexible controls than those the difference between DD estimates would impose (e.g., first DD identified off of occupation-time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

³²Yet 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.

³³Control 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

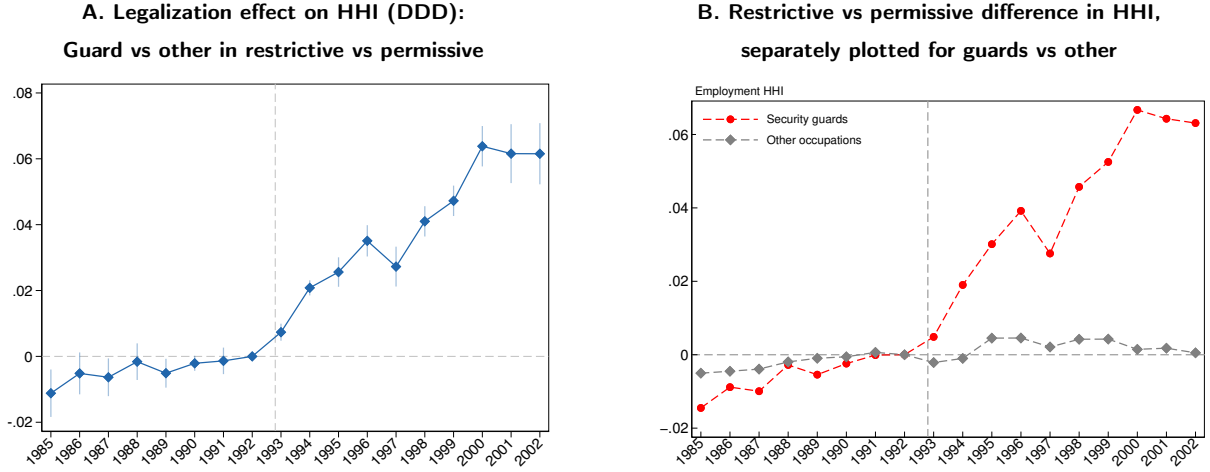


Note: Panel A plots the coefficients β_τ from the DDD regression measuring the impact of outsourcing legalization in equation (2). Each observation is a microregion \times occupation \times year cell. Outsourced share is the share of all employment in a microregion-occupation pair working for a contract-firm. 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 $y_{rt} = \sum_{\tau=1985; \tau \neq 1992} \alpha_\tau (T_r \times 1_{t=\tau}) + \delta_r + \delta_t + \epsilon_{rt}$, 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)



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.

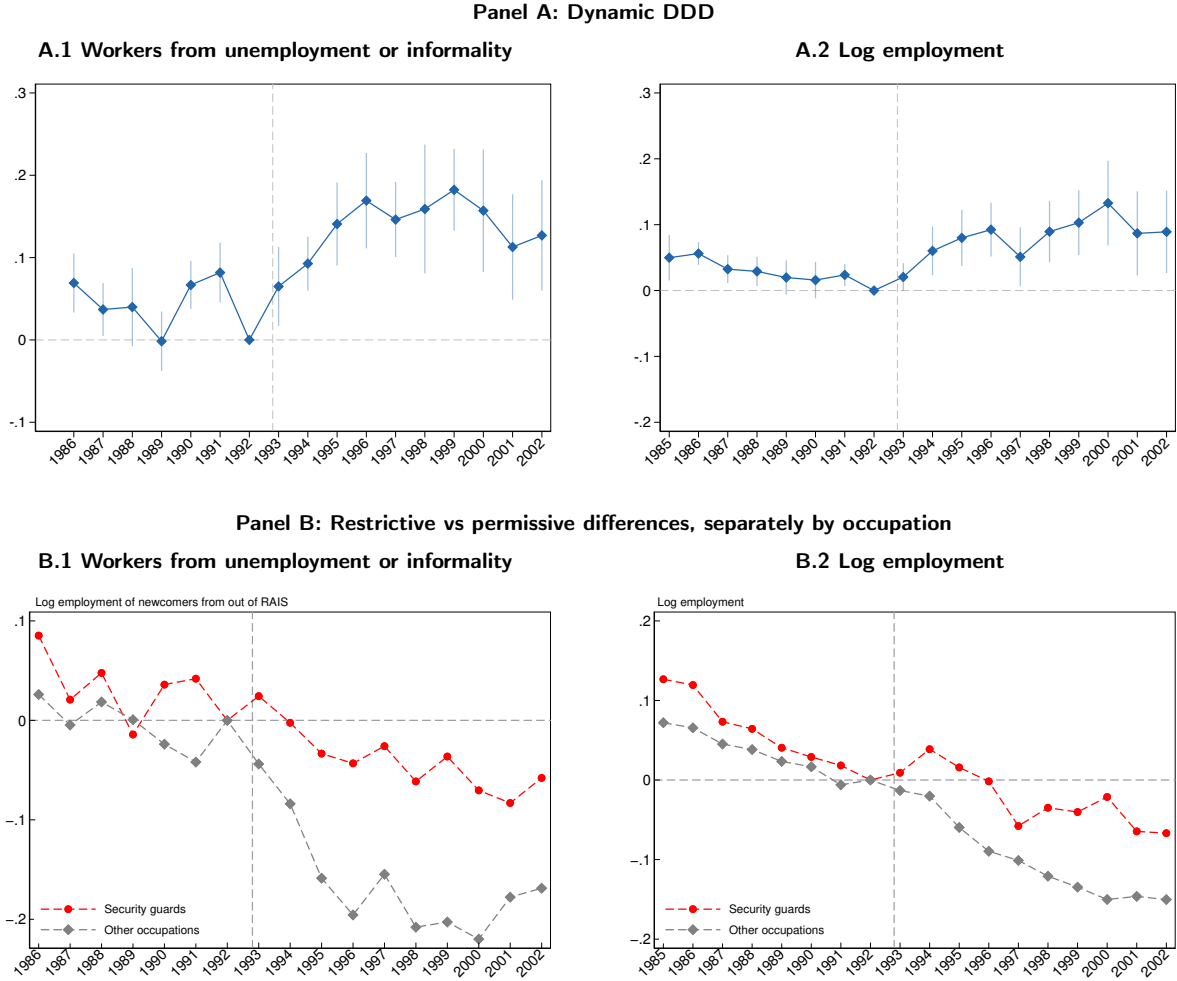
Legalization increased worker entry and employment

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.

³⁴Pre-legalization effects are not jointly statistically significant.

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

³⁵While 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).

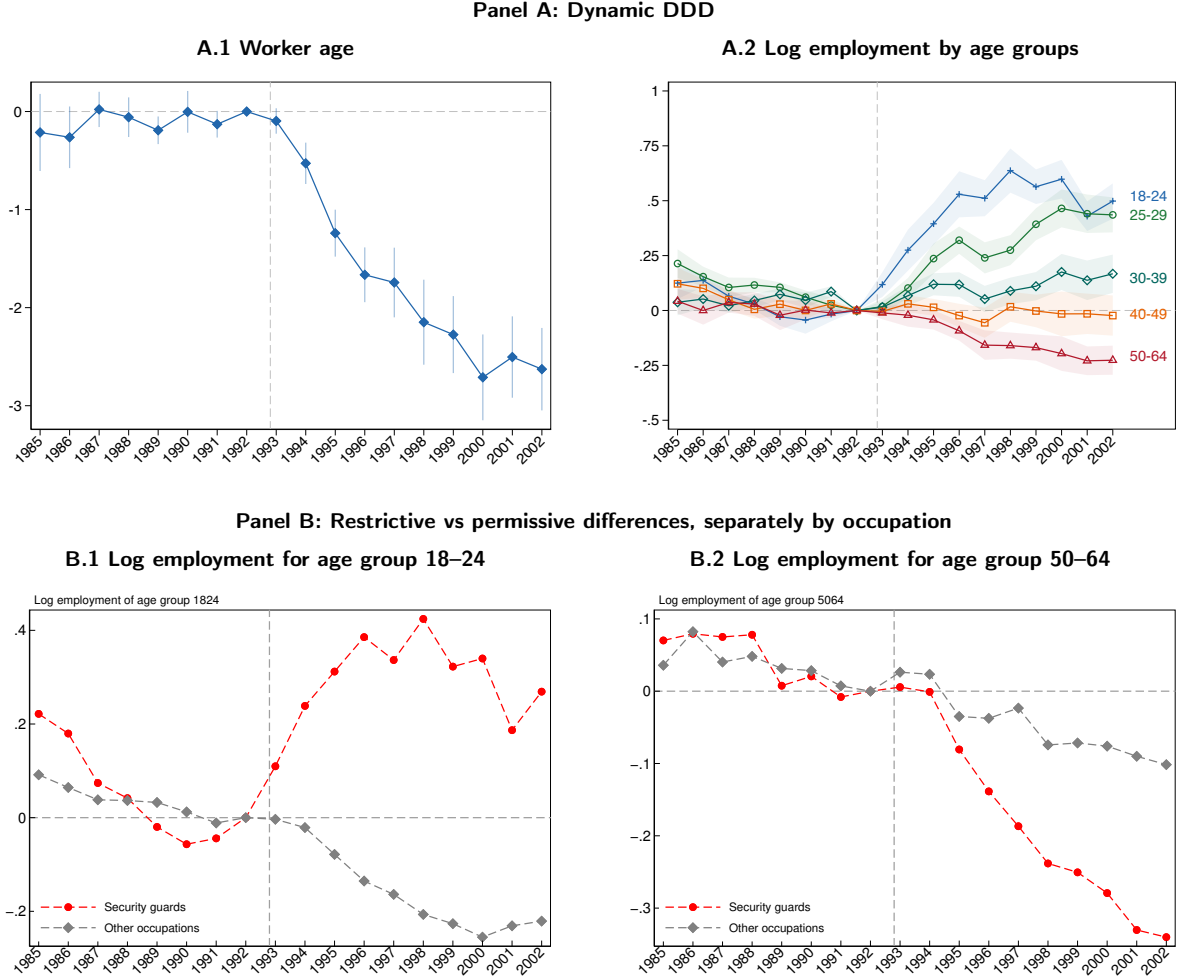
Panel B shows that for both guards and control occupations, restrictive regions were on a negative employment trend throughout the sample period, and this negative trend was of a very similar magnitude for guards and other occupations prior to legalization. However, legalization broke this negative trend 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



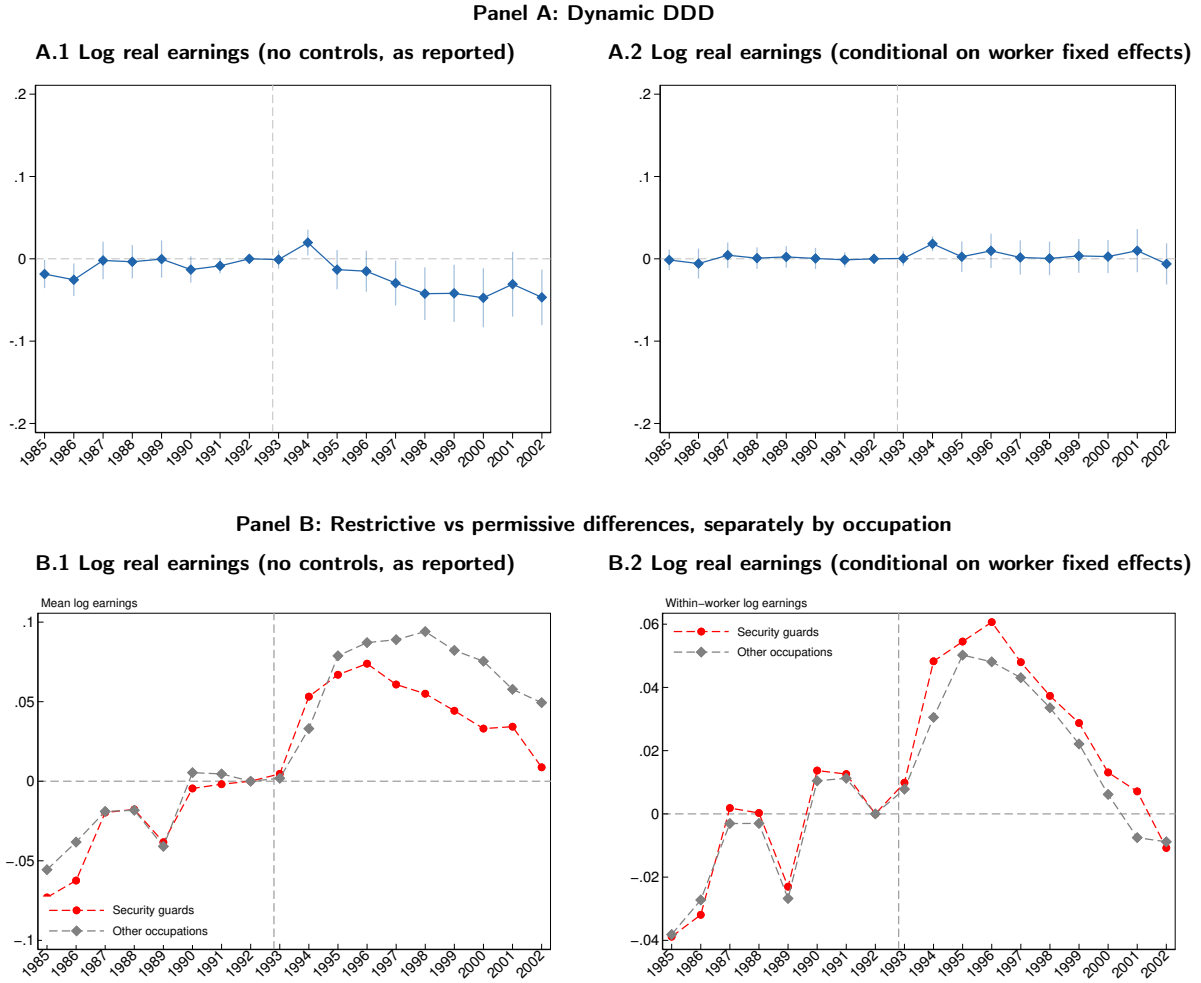
Note: See notes to Figure 4. The outcome variable in Panel A.1 is worker age, while the outcome variables in Panel A.2 are log employment by different age groups. Age groups are defined according to the age group variable consistently reported throughout the period.

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



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)

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 Vejin 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 the

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

³⁷That 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.

³⁸Data 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):

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

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-

³⁹These 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):

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

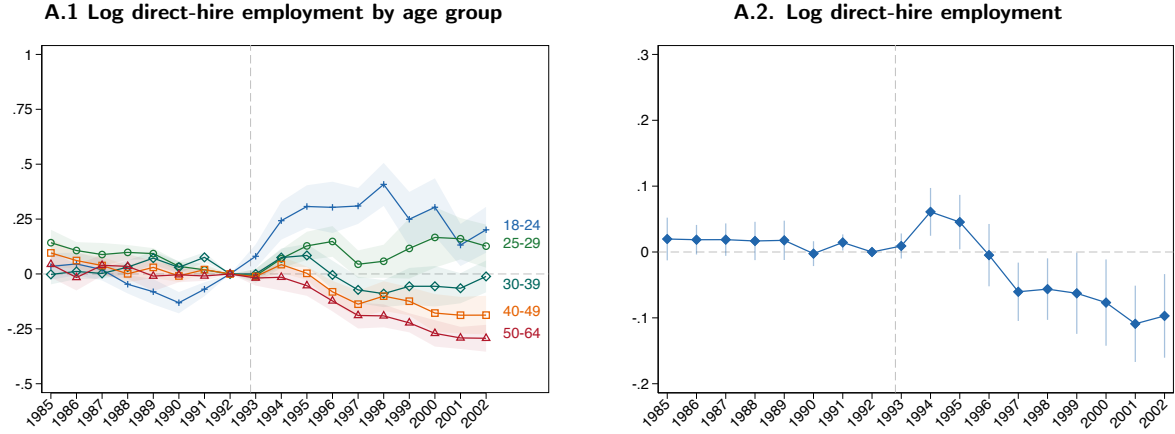
where y_{it+1} is a dummy indicating whether a transition occurred (such as exit from formal sector, or occupation switching), O_{it} denotes contract-firm employment, X_{it} denotes worker characteristics (such as age and education as well as unobserved ability, as proxied by AKM worker effects), τ_t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



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

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.

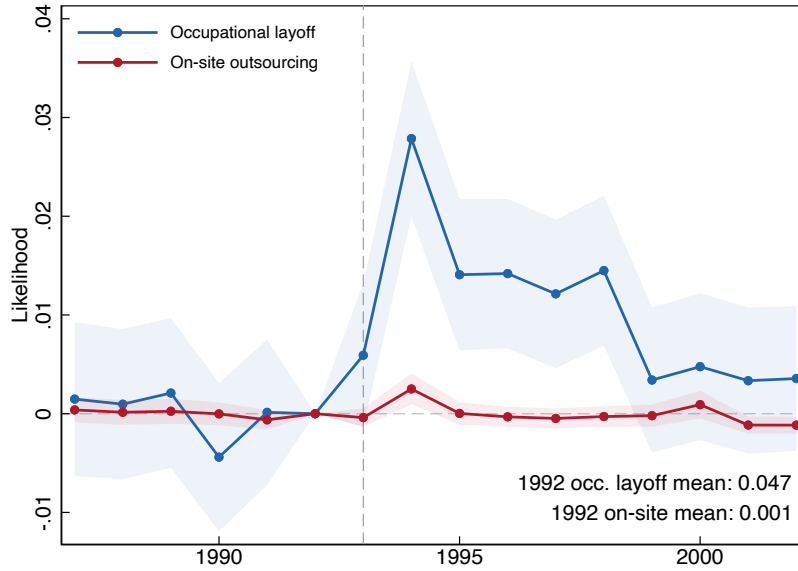
Figure 10 plots estimates of the establishment-level likelihoods of an occupational layoff or an on-site outsourcing event in each year. It shows a large wave of occupational layoffs immediately after legalization and hardly any increase in on-site outsourcing. Both are stable prior to 1993, the year 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

⁴⁰Appendix 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.

⁴¹See Articles 453 and 468 of *Consolidação das Leis do Trabalho*. See also *Portaria MTB 384/1992* and Law 6.019 Article 5^o-D.

⁴²See 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:

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

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 a 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.

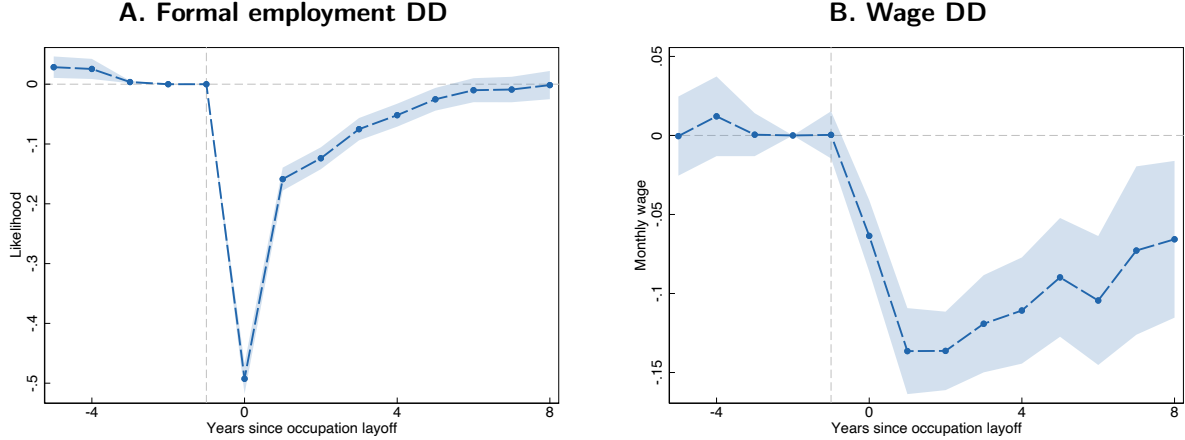
Figure 11, Panel A shows that outsourcing decisions significantly displace incumbent security guards from formal employment. In the year after an occupational layoff, an affected security guard is 49 p.p. less likely to be formally employed. However, five years after an occupational layoff, there is 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

⁴³While 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).

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

Figure 11: Effect of occupational layoffs on incumbent workers



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

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.

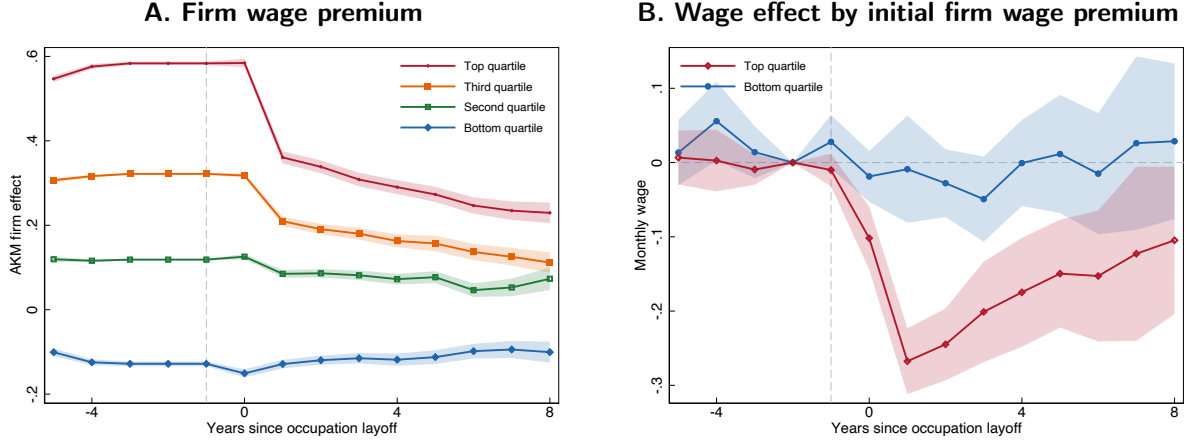
6.3 The Role of Firm-Specific Wage Premia

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

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

⁴⁷Clement 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



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

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 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

⁴⁸See 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.

⁴⁹By 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 (1612) [1511]	1753 (1476) [1321]	1694 (1382) [1312]	1481 (822) [1285]	1573 (814) [1390]	1665 (796) [1525]
Contract hours		42.4 [44]	42.4 [44]		43.5 [44]	43.7 [44]
Real wage (2017 \$R)		43.5 (55.9) [31]	41.8 (49.3) [31]		37.1 (34.6) [32]	38.4 (23.2) [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 [146]	692 [83]	568 [59]	1024 [586]	954 [460]	1729 [478]
Number of guards at employer	67 [8]	66 [5]	54 [4]	822 [490]	781 [422]	808 [432]
<i>N</i>	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.

Table 2: Effect of legalization on prevalence of guard outsourcing

	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)	
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

		Legalization effect (Weighted DDD)			
	Mean at baseline (1992) (1)	Entropy- balancing weights (EB) (2)	Inverse propensity score weights (3)	EB with regression adjustment (4)	Observations (region x occup x year) (5)
<i>Panel A: Log total employment</i>					
Log total employment	7.213	0.052 (0.011)	0.047 (0.014)	0.051 (0.014)	57240
<i>Panel B: Log employment by contract type</i>					
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
<i>Panel C: Log employment origins</i>					
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

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

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)
<i>Panel A: Real December log earnings</i>					
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
<i>Panel B: Real December log earnings by contract type, as reported</i>					
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.