Domestic Outsourcing and Young-worker Entry into the Formal Sector*

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Abstract

We provide evidence that domestic outsourcing increases young-worker entry into the formal sector. Leveraging a pair of 1993–1994 Brazilian reforms that sharply reduced the relative cost of contracting out security guards, a triple-differences design shows that the reforms increased formal guard employment by 4% and hiring from unemployment or informality by 7%—with such entrants accounting for roughly half of the induced employment gains—while reallocating formal employment from older to younger workers and leaving demographic-adjusted wages unchanged. Census data corroborate the rise in formality, driven by the youngest cohorts. The compositional shift mirrors a general pattern in Brazil's matched employer–employee records: conditional on total firm size, employers with greater occupation-specific scale hire younger workers, paid less at entry and more likely to be entering formal employment for the first time. The evidence is most consistent with contract firms supplying at scale the capabilities needed to hire productive workers from outside the formal sector—a demand-side channel for increasing formal sector employment.

Keywords: domestic outsourcing, occupation-specific scale, informality
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1 Introduction

Labor markets in developing economies are characterized by high rates of informality and slippery job ladders (Ulyssea, 2020; Donovan et al., 2023). The growth of professionally managed firms may be a necessary step for helping young workers climb to better-paying, formal sector jobs (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 contract firms can increase young-worker entry into the formal sector by leveraging occupation-specific economies of scale in screening, training, and matching (Abraham and Taylor, 1996; Autor, 2009). Such scale is hard to build within any single firm when the activity is *non-core*: work like security or cleaning is peripheral to most employers, so demand for it is thin at any one firm but substantial once pooled across many clients.

We assess this theory by estimating the labor market impacts of pro-outsourcing reforms that sharply reduced the relative cost of non-core activity outsourcing in Brazil. Although the benefits of domestic outsourcing to workers have long been hypothesized, recent literature has largely focused on negative impacts on incumbent workers' wages, documenting that outsourcing excludes low-wage incumbents from firm wage premia and increases wage inequality (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2023). These approaches rely on *firm-level* variation in outsourcing choices, which does not capture effects on employment or on all workers in impacted occupations. Assessing the impact of outsourcing requires exogenous *market-level* variation in its adoption.

We estimate the market-level effects of domestic outsourcing using a pair of pro-outsourcing reforms enacted in Brazil in 1993 and 1994. On December 17, 1993, following an unanticipated series of events, Brazil's Superior Labor Court issued *Súmula 331*, which replaced a categorical precedent holding outsourcing to be illegal with the *atividade-meio* (non-core activity) doctrine: a firm could lawfully contract out activities outside its core business, with the outsourced worker deemed a legal employee of the intermediary rather than of the client firm. The ruling sharply reduced the *expected* legal cost of contracting out, and its practical reach was concentrated in the two activities it named explicitly—security and cleaning. Months later, Lei 8.863 of March 1994 amended Brazil's private-security

statute in two directions: it extended the lawful client universe of specialized security firms from banks to any public or private establishment, and it brought firms employing armed guards in-house—whatever their line of business—under the same federal regime as security firms. Because the 1994 revision was triggered by the 1993 ruling, we study the two as a pair and interpret our estimates as the joint effect of the 1993–1994 reforms.

Our analysis focuses on security guards, the only occupation explicitly named in both reforms. The second reform, Lei 8.863, applied to guards alone: it raised the cost of in-house hiring only for armed security, leaving cleaners—the only other occupation explicitly named, in the first reform (Súmula 331)—untouched. Cleaners, moreover, had little margin to absorb either reform. Their earnings bunched at the minimum wage and a large share worked informally, so outsourcing offered little labor cost to reduce; guards, by contrast, were paid well above the minimum wage and, owing to licensing and mandatory training administered by the Federal Police, were overwhelmingly formal. Consistent with this differential incidence, we show that the contract-firm employment share rose sharply after the reforms for guards but changed little for cleaners. Guards also constituted a sizable occupation, accounting for nearly 5% of formal private-sector male employment in 1992, and Brazil's employer–employee linked data (RAIS), which we use to measure worker outcomes, capture nearly the entire occupation.

Our identification strategy controls for confounding region-level and occupation-level shocks using a triple-differences design that compares guards to less-affected occupations in restrictive relative to permissive regions, before and after the reforms. The regional variation comes from pre-1993 court interpretation: southern regional labor courts had applied the predecessor doctrine restrictively, so the 1993 ruling lowered the effective legal cost of outsourcing disproportionately in their jurisdictions. We also weight permissive microregions to be similar to restrictive microregions in mean pre-reform characteristics, such as exposure to import competition, homicide rates, unemployment rates, and total formal employment.

We find that the reforms sharply increased outsourcing and, through it, young-worker entry into the formal sector. The outsourced share of guard employment rose by 4.5 percentage points on average across post-reform years, a 36% increase over the treated-group baseline, and total employment in the occupation rose persistently by roughly 4%. This

expansion was, first, a formalization of workers new to the sector: hiring from unemployment or informality rose by 7%, three times the increase from other formal jobs and, at baseline levels, roughly half of the reform-induced increase in employment. It was, second, a reallocation toward the young: employment of guards ages 18–24 rose by 42% while that of those ages 50–64 fell by roughly 12%, lowering the average guard's age by 1.4 years. The two connect in the Census, where the formal-employment gain is concentrated among the youngest cohort. The *demographic-adjusted* wage in the occupation of security guards as a whole did not change; instead, as-reported wages show a small and statistically insignificant decline, consistent with the compositional shift toward younger guards, who tend to earn lower wages.

These estimates are robust to several potential confounds: we detect no differential pre-reform trends; the results are stable across inverse propensity score weights, entropy balancing weights, and regression adjustment; and the impacts did not coincide with observable changes in homicide rates that may drive demand for security services. As a complementary check that also observes informal work, a Census-based triple difference shows formal guard employment rising differentially in restrictive microregions after the reforms, with the gain concentrated among the youngest cohorts. In the same design, informal earnings rise while informal employment and formal earnings show no detectable change—a pattern consistent with the reforms drawing workers out of the informal sector.

What drives these effects? One hypothesis is that contract firms have the scale to invest in occupation-specific fixed-cost capabilities—such as screening, matching, and training technologies—that improve the productivity of entrant workers. Consistent with this, we document that the compositional shift produced by the reforms is an instance of a general pattern of occupation-specific scale effects in hiring, present across all occupations in Brazil's employer–employee data. In firm-occupation-region-year regressions with firm fixed effects, a 10% increase in a firm's employment in an occupation—conditional on total firm employment—is associated with new hires into that occupation who are more likely to come from outside the formal sector (0.07 percentage points), less likely to have ever held a formal job (0.13 percentage points), younger (0.09 years), and paid lower starting wages (0.3%). The coefficients on firm-level scale have the reverse sign: larger firms hire workers with more

formal-sector history, more education, and higher wages, consistent with standard positive assortative matching (Song et al., 2019; Card et al., 2013; Alvarez et al., 2018). Occupation-specific scale thus draws marginal workers into formal employment, while firm-level scale concentrates already-formal workers.

To clarify the potential mechanisms involved, the appendix develops a screening model in the spirit of Jovanovic (1979) to illustrate how occupation-specific scale enables fixed-cost investments that make it profitable to hire productive workers from outside the formal sector whose quality the market cannot readily observe. In the model, a firm chooses between an experienced worker whose formal record the market already prices and a cheaper entrant from outside the formal sector who can be hired only after screening. Occupation-specific scale governs which firms pay the fixed cost of a screening technology, so higher-scale employers hire younger, record-less entrants at lower wages. Contract firms reach that scale by aggregating demand across many clients, and as outsourcing costs fall they extend screened-entrant hiring market-wide, drawing workers from outside the formal sector into formal employment. Screening represents a family of scale technologies—training, monitoring, reassignment—each discussed below. We also consider explanations that rely on no scale economy at all—rent-stripping, monopsony, and regulatory arbitrage—and find them hard to square with the post-reform stability of demographic-adjusted wages and auxiliary evidence such as a small outsourcing wage differential.

Literature. Our paper contributes primarily to the literature on labor markets in developing countries, which are characterized by high rates of informality and by information frictions that leave employers unable to effectively screen workers (Ulyssea, 2020; Donovan et al., 2023; Breza and Kaur, 2025). Explanations for high informality have centered on the regulatory and enforcement costs of formality (Ulyssea, 2018, 2020). We contribute evidence on a demand-side channel for increasing formal sector employment: the expansion of specialized contract firms—employers long hypothesized to benefit from occupation-specific economies of scale in hiring (Abraham and Taylor, 1996; Autor, 2001, 2009)—drew young workers from informality and non-employment into formal jobs, with no change in registration costs or enforcement, consistent with contract firms supplying at scale the screening

capacity that individual employers lack (Breza and Kaur, 2025).

We also contribute to the empirical literature on domestic outsourcing, which has largely emphasized regulatory arbitrage and rent extraction as the motives behind it: contract labor in India spread in response to statutory firing restrictions, reducing misallocation and raising aggregate manufacturing productivity (Bertrand et al., 2025); Mexico's recent outsourcing ban raised wages and reduced labor markdowns at establishments that had outsourced, consistent with sustained employer monopsony power (Estefan et al., 2024); and in high-income labor markets, outsourcing removes low-wage incumbents from firm wage premia (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2023) while raising output and compressing the labor share in general equilibrium (Bilal and Lhuillier, 2021). In our own setting, Felix and Wong (2026) document that the reforms we study imposed concentrated displacement costs on incumbent guards. Estimating the reforms' net effect on the occupation as a whole—incumbents, entrants, and non-adopting employers alike—we find that domestic outsourcing increased worker entry into the formal sector.

The hiring gradients underlying this entry—occupation-specific scale effects at hiring—speak to the literature on assortative matching and firm pay premia, which documents that larger firms employ more experienced, higher-paid, already-formal workers in the United States (Song et al., 2019), West Germany (Card et al., 2013), and Brazil itself (Alvarez et al., 2018). Looking inside firms, across their occupations, we find the opposite gradient: conditional on firm size, employers with greater occupation-specific scale draw their new hires from outside the formal sector. The reforms increased formal entry by reallocating employment toward such employers.

Finally, our findings add to the literature on firm professionalization (Chandler, 1977; Bloom and Van Reenen, 2007; Bloom et al., 2013; Hjort et al., 2022; Bassi et al., 2025; Engbom et al., 2024) by showing that the gains from specialization can be delivered through specialized intermediaries rather than within-firm professionalization, bridging this literature to work on standardization-driven scale (Argente et al., 2025) and task-specific scale as a driver of specialization (Arnarson et al., 2024).

2 Institutional Setting and Data

Our research design exploits a pair of legal reforms enacted in 1993 and 1994 that together altered the relative cost of outsourcing security guards. Súmula 331 of the Superior Labor Court (December 1993) lowered the expected legal cost of contracting out non-core activities, a change whose practical reach was concentrated among the two activities it named explicitly—security and cleaning. Lei 8.863 of 1994 then raised the cost of employing armed guards in-house, with no analogous change for cleaning or any other occupation. Below we describe the reforms and their differential incidence across activities (Section 2.1), the regional variation in pre-1993 court interpretation that our design exploits (Section 2.2), and the data (Section 2.3).

2.1 The 1993–1994 Reforms' Differential Incidence on Security

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 Súmula 256, a one-paragraph precedent stating that the Court understood the practice of outsourcing to be illegal except in cases permitted by legislation.Regional differences in judges' stances on the legality of outsourcing nevertheless persisted, a phenomenon we discuss in Section 2.2 and leverage for our empirical strategy in Section 3.1.

On December 17, 1993, following an unanticipated series of events, the Superior Labor Court issued Súmula 331,[1] which replaced the categorical rule of Súmula 256 with the

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

atividade-meio (non-core activity) doctrine. Under this doctrine a firm could lawfully contract out activities outside its core business, and the outsourced worker would be considered a legal employee of the intermediary rather than of the client firm.[2] Súmula 331 was not, however, a clean-slate legalization of all non-core outsourcing. It named security and cleaning explicitly, but reached other non-core services only where the work was free of *pessoalidade*, or personal rendering of services—a juridical concept in Brazilian law whereby the service for which an employee is hired must be performed by that employee, and cannot be delegated to someone else—and direct subordination. Professional services delivered under civil contracts—such as legal and accounting work—fell outside the labor-outsourcing frame entirely. Appendix Table A.2 summarizes the legal status of outsourcing by activity across the pre-1986, 1986–1993, and 1993–2017 regimes.

As a consequence of Súmula 331, the expected legal cost of outsourcing workers sharply fell. Prior to the reforms, 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 the reforms, the high expected legal costs likely discouraged many firms from doing so.

The 1993 reform triggered a second policy change that fell specifically on security. The supply of private security services had been regulated since 1983 by Lei 7.102, which authorized specialized security firms but restricted their lawful clients to financial institutions. Lei 8.863 of March 1994 amended Lei 7.102 along two dimensions. First, it extended the legal client universe of authorized security firms from banks to any public or private establishment, making operational in practice the trilateral security-outsourcing relationship that Súmula 331 had rendered lawful in principle. Second, through the new §4º of article 10, it brought firms that employ armed guards in-house—whatever their primary line of business—under the same federal regime as security firms: authorization by the Federal Police, a minimum-capital requirement, criminal-record screening, completion of federally-authorized training,

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

and a graduated penalty regime. Appendix Table A.3 lists the resulting regulatory bundle applying to any firm hiring armed guards.

The two reforms thus worked in the same direction for security but not for other activities. Súmula 331 lowered the legal cost of *contracting out* security and cleaning; Lei 8.863 additionally raised the legal cost of *directly hiring* armed guards, and did so nationally, with no analogous statute for cleaning or any other occupation. Because the extension of the security-firm regime to non-bank clients followed directly from the legalization of security outsourcing, we study the two as a pair and interpret our estimates as the joint effect of the 1993–1994 reforms rather than of Súmula 331 alone. Appendix Table A.1 summarizes the resulting de jure incidence by activity: armed security is the only activity for which the reforms shifted the legal cost structure both toward contracting and against direct-hiring.

We focus on the labor market for security guards because the 1993–1994 reforms were differentially binding for this occupation. As Appendix Table A.1 shows, Súmula 331 lowered the legal cost of contracting out both security and cleaning, but only for armed security did the paired Lei 8.863/1994 also raise the legal cost of hiring in-house. Guards are therefore the single occupation for which the reforms moved the cost of contracting and the cost of direct hiring in opposite directions, and we accordingly expect outsourcing to rise sharply among guards but not among cleaning or other *atividade-meio* activities. The contract-firm share by occupation offers a first, occupation-level check of this prediction.

Consistent with the differential legal incidence of the 1993–1994 reforms on security services, security guards were the only major occupation to experience a sharp increase in outsourcing after the reforms. Appendix Figure A.1, Panel B displays the trend in the share of private sector workers employed in contract firms (i.e., the "contract-firm share") for each occupation,[3] averaged across microregions. Before 1993, two occupations—security guards and cleaners—had contract-firm shares far exceeding those of other occupations. In

[3]We use two-digit occupation codes to identify broad occupational groups. Security guards are identified as private sector workers under *Classificação Brasileira de Ocupações* (CBO) two-digit occupation code 58, "Protection and security workers." We exclude police officers (CBO three-digit code 582) and other government-staffed protection occupations (e.g., firefighters and prison guards) from our definition of security guards, as these are public sector workers (see Appendix B). 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.

the years immediately following the reforms, the contract-firm share rose by more than 20 percentage points (p.p.) for security guards, from an average of 32%. By contrast, the contract-firm share for cleaners hardly changed: neither their outsourced employment share nor their employment concentration (HHI) increased after the reforms, consistent with the reforms having limited reach in occupations where most employment was already informal.

Outsourcing restrictions were also more enforceable for guards than for cleaners because guards are overwhelmingly formal workers subject to regulatory enforcement. Roughly 80% of all guards were in formal employment during this period, compared to 39–41% for the overall workforce. Guards also face strict licensing requirements[4] and must complete mandatory training administered by Brazil's *Polícia Federal*.[5] These features—high formality, licensing, and federally administered training—meant that court decisions on outsourcing legality were effectively enforceable for this occupation, making the 1993 reform a binding change in the regulatory environment.

Even though Súmula 331 named cleaning alongside security, the economic return to formalizing a cleaner through a contract firm was small to begin with. As Appendix Table B.2 shows, in the 1991 Census cleaner earnings bunched at or just above one minimum wage and a large share of cleaners worked informally, whereas guard earnings sat well above the minimum across most of the distribution and guards were overwhelmingly formal (Appendix Figure B.1 plots the two earnings distributions). A contract firm that formalizes a cleaner therefore captures little wage or compliance surplus relative to the inexpensive informal alternative, whereas for guards—whose informal direct hire is statutorily barred and whose pre-reform wages leave room for cost savings—the surplus from specialized contracting is larger. The muted cleaner response in Appendix Figure A.1 is thus consistent with this difference in returns rather than with any difference in the legal permissibility of cleaner outsourcing.

Finally, while our focus on security is driven by the differential incidence of the 1993–

[4]Guards must have no criminal records, be Brazilian and at least 21 years of age, have completed at least the 4th grade, and present proof of no pending obligations with either the electoral court (as voting is mandatory in Brazil) or with the military (as men are required to report for enlistment at age 18, though most are dismissed).

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

1994 reforms on security services, security guards are by no means a niche occupation in our context. Security guards accounted for 5% of total male private sector formal employment in 1992. Overwhelmingly male and relatively high-paid given Brazil's large informal sector, guards were employed across virtually every sector of the economy.[6] Appendix Figure A.2, Panel A plots 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.[7] In 1992, about 60% of wholesale establishments had at least one security guard on staff, and by 2002, fewer than 40% did. Panel B shows a concurrent increase in contract-firm employment, confirming that many of these firms contracted out their needed security services and thus no longer directly employed security guards.

2.2 Pre-Reform Regional Variation in Court 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 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 Súmula 256 as establishing a principle of illegality for outsourcing. This implied that even some exceptions listed in Súmula 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 were far more lenient. A union leader in São Paulo reported that "the high frequency of lawsuit losses ended up wearing down the Unions, because as we could not win lawsuits the employers made sure to promulgate: 'you see! The labor court

[6]See Appendix Table B.1 for more descriptive statistics of the occupations.

[7]An exception is manufacturing, whose share of establishments employing guards begins to decline just before 1993. This is likely related to concurrent trade liberalization, which disproportionately affected manufacturing establishments.

considers outsourcing legal!'" Appendix Tables A.5 and A.6 provide many more of these quotes, taken from transcripts of interviews with former regional court justices, judges, lawyers, and union leaders.[8]

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.[9] 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, 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),[10] 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 A, we define Southern regional courts (4th, 9th, 12th, and 15th) as restrictive toward outsourcing prior to the reforms and the remainder as permissive. Despite the 15th region's less clear stance, our results leveraging this regional variation are robust to dropping observations from the state of São Paulo (see Appendix Table C.5).

Aggregate trends confirm that the pro-outsourcing reforms had a much larger impact in restrictive jurisdictions. Appendix Figure A.1, Panel A shows that as of December 1992 (the year before the reforms), roughly 32% of security guards in restrictive regions were employed by contract firms, compared to 38% in permissive regions. By December 2000, this gap had been fully 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 sharp break from an otherwise flat trend. This

[8]Interview transcripts are from Biavaschi and Baltar (2013), who between 2008 and 2011 interviewed parties involved in key outsourcing lawsuits at various regional courts. We also separately interviewed a former Regional Court Justice and a current justice in the North. See Appendix A for details.

[9]Appeals to the Superior Labor Court are rare, especially on a specific topic such as outsourcing. That most appeals concerning outsourcing come from Southern courts indicates more active litigation of that topic in the region. Details on each appeal are provided in Appendix Table A.7.

[10]Interview evidence suggests the 15th region was less restrictive than Southern courts but still actively resisted outsourcing prior to Súmula 331. See Appendix Table A.5.

contrasts with the pattern observed in permissive regions, where outsourcing experienced secular growth throughout the period, with no apparent trend breaks following the reforms. Appendix Figure C.7 displays regional-court-specific trends, also showing clear trend breaks in each of the restrictive jurisdictions.

2.3 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.[11] This allows us to identify outsourced workers as those employed by 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 limitations. First, RAIS does not include information about informal employment. We can see whether a new hire ever had a formal sector job, and we can see whether they were hired directly from outside RAIS (from informality or unemployment), but we do not know what they were doing outside the formal sector. We therefore present in Appendix C results of the effects of the 1993–1994 reforms using data from the 1991 and 2000 Census datasets, and an otherwise similar empirical design. That is, we compare 1991-2000 changes in outcomes for security guards relative to the same control occupations, in restrictive versus permissive regions.

Second, RAIS does not include data on where outsourced workers are posted. As a result,

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

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. The displacement effects of the 1993–1994 reforms in incumbent security guards are studied in Felix and Wong (2026).[12]

Finally, RAIS lacks information on non-wage components of compensation (such as access to employer-based private health insurance). As in recent studies (e.g., Goldschmidt and Schmieder 2017; Drenik et al. 2023), our wage effects do not account for non-wage compensation. Since non-wage benefits are positively correlated with wages (Taber and Vejlin 2020; Lamadon et al. 2022), our overall conclusions are unlikely to change substantially: incumbents displaced by outsourcing (Felix and Wong, 2026) likely lost amenities alongside the wage premia documented there, while entrants from unemployment or informality likely gained them.

3 Local Labor Market Effects of Outsourcing Reforms

We estimate the effects of the 1993–1994 outsourcing reforms on security guards at the local labor market level, using cross-region and cross-occupation variation in how binding the pre-reform legal restrictions against outsourcing had been. Unlike previous approaches, our strategy captures the effect of the reforms on an entire occupation. This variation reflects the joint effect of the two reforms described in Section 2.1, both of which fell on security guards far more than on other occupations. We find that the reforms (i) increased the prevalence of outsourcing, (ii) increased guard employment, and (iii) reallocated guard jobs from older to younger workers. We find no effects on demographic-adjusted wages and a small, statistically insignificant decline in as-reported wages. All of these effects are long-lasting.

3.1 Empirical Strategy

We exploit the fact that the 1993–1994 reforms were most binding for guards in restrictive regions to implement a triple-differences (henceforth DDD) research design. That is, we

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

compare the outcomes of guards relative to those for other occupations in restrictive versus permissive regions before and after the reforms. Our main regression specification is

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

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

The β coefficient in equation 1 is the average effect of the pro-outsourcing reforms on guards in restrictive regions. We report this coefficient, estimated separately for various outcomes and under different sample weighting methods (more below), in Tables 1, 2, and 3. As in standard DDD identification strategies, causal interpretation of β requires the assumption of parallel trends between treated and untreated units. That is, absent the reforms, 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:

$$y_{ort} = \sum_{\tau=1985;\tau\neq1992}^{2002} \beta_\tau \left(T_{or} \times 1_{t=\tau} \right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \epsilon_{ort}. \tag{2}$$

The β_τ coefficients in equation (2) break down the average treatment effect β from equation (1) into year-specific coefficients, presenting them as effects relative to the baseline year of 1992. These coefficients are reported in Figures 1–5. Our DDD design controls for national-level trends in demand for security services and regional-level effects of contemporaneous policies, such as trade liberalization (1990–1994), that would not be controlled for by simpler

DD designs.[13] The occupation-year fixed effects δ_{ot} absorb the former and the region-year fixed effects δ_{rt} absorb the latter.

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, and its key confounding channel is crime: liberalization temporarily increased crime in regions more exposed to import competition (Dix-Carneiro et al., 2018), many of which were in the Southeast and South, overlapping with the jurisdiction of restrictive labor courts. Because crime raises the demand for security services, part of the increase in guard employment that we document in restrictive regions could reflect trade-induced demand for guards rather than the pro-outsourcing reforms. Aside from its temporary effect on crime, liberalization reduced formal employment and wages in more-exposed regions (Dix-Carneiro and Kovak, 2017), whereas the effects on guard employment that we document are positive.

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, the share of employment in tradeable industries, and log total formal sector employment). We do this by first estimating entropy balancing weights, following the method proposed by Hainmueller (2012), and then weight each observation by its corresponding weight. Appendix Table C.2 reports 1992 summary statistics of guard markets in restrictive and permissive microregions. Appendix Figure C.1 shows the entropy balancing weights on a map. Appendix Figure C.9 shows no differential trends across restrictive and (weighted) permissive regions before or after the reforms.

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). We report estimates using alternative weighing meth-

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

ods as robustness. Finally, because security guards are not employed in all microregions, we estimate equations (1) and (2) on a balanced sample of 262 microregions and other large two-digit occupations present in all microregions.Appendix Table C.1 lists these 11 comparison occupations and their characteristics. Our estimation sample covers roughly 465,000 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).

3.2 Results

We now present our estimates of the market-level effects of the pro-outsourcing reforms based on the DDD design presented in Section 3.1. In Figures 1–5, Panel A plots coefficients β_τ of equation (2) for each respective outcome. Panel B plots the underlying identifying variation: outcome differences between restrictive and permissive regions (i.e., the cross-region DD), separately for guards (in red) and other occupations (in gray). Each DDD point estimate corresponds roughly to the difference between these two DD series.[14]

Pro-outsourcing reforms increased outsourcing

Figure 1 plots estimates of the effect of the pro-outsourcing reforms 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 the reforms, with no differential pre-

[14]"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. Appendix C presents both cross-region and cross-occupation DD visualizations for all major outcomes. The cross-region DD series plot α_τ coefficients from $y_{rt} = \sum_{\tau=1985;\tau\neq1992}^{2002} \alpha_\tau \left(T_r \times 1_{t=\tau}\right) + \delta_r + \delta_t + \epsilon_{rt}$, 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, estimated separately for guards and for other occupations; a microregion's outcome for other occupations is the equal-weighted average across occupations, and the microregion-level regressions are weighted by microregion entropy balancing weights. The corresponding cross-occupation DD series (Appendix C) plot α_τ from $y_{ot} = \sum_{\tau=1985;\tau\neq1992}^{2002} \alpha_\tau \left(T_o \times 1_{t=\tau}\right) + \delta_o + \delta_t + \epsilon_{ot}$, where $T_o = 1$ if occupation o is security guards and zero otherwise, and δ_o and δ_t are occupation and year fixed effects, estimated separately for restrictive and permissive regions; occupation-level outcomes are averages across microregions using microregion balancing weights, with occupations weighted equally.

trends. Prevalence increased steadily through 1999, leveling off afterwards. Table 1 reports the corresponding average effect over the post-reform years (i.e., coefficient β in equation (1)). Column (2), the main specification, shows that the reforms increased the outsourced employment share by 4.5 p.p. on average across post-reform years—a 36% increase relative to the baseline mean of 12.5% among treated cells (guards in restrictive regions).[15] These effects are robust to alternative weighting methods (columns (3)–(4)). Appendix Figure C.8 shows that this increase in outsourcing prevalence did not meaningfully depend on distance to the border between restrictive and permissive regions.

Pro-outsourcing reforms increased labor market concentration

Figure 2 plots estimates of the effect of the pro-outsourcing reforms on local labor market concentration as measured by the employment HHI (Panel A), along with the identifying variation for this effect (Panel B). Pro-outsourcing reforms increased concentration in local labor markets for security guards by 0.047 points (a 76% increase relative to the treated cells' baseline mean of 0.062; see Table 1). These effects are robust to alternative weighting methods. The increase is attributable to contract firms employing large numbers of specialized workers (Appendix Table B.1).

Pro-outsourcing reforms increased worker entry and employment

Figure 3 plots coefficients β_τ of equation (2) for the effects of the pro-outsourcing reforms 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.

Pro-outsourcing reforms increased worker entry from unemployment or informality into the security guard occupation by roughly 7% and total security guard employment by about

[15] Relative to lower baseline outsourcing levels elsewhere in the sample, the same 4.5 p.p. increase is larger in proportional terms: it is a 145% increase relative to the baseline sample mean of 3.1% and a 167% increase relative to the baseline mean of 2.7% among control cells (all cells other than guards in restrictive regions), both of which are low because control occupations have a very low prevalence of outsourcing (see Appendix Figure A.1).

4%.[16] Panel D of Table 1 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 7% entry effect on workers from unemployment or informality is three times larger than that from other formal sector jobs, of roughly 2%. Evaluated at each origin's mean baseline level, these proportional increases imply that roughly half of the reform-induced increase in guard employment came from unemployment or informality.

The table shows two additional decompositions of the pro-outsourcing reforms' effect on employment. First, the 3.9% increase in total guard employment was primarily driven by a large increase in outsourced guard employment,[17] as direct-hire employment declined by 6.8% (Panel C).[18] These effects are robust to alternative weighting methods (columns (3)–(4)).

Pro-outsourcing reforms reallocated employment from older to younger workers

Figure 4, Panel A shows the effects of the pro-outsourcing reforms on guard age and employment by age groups. Pro-outsourcing reforms reduced the average age of security guards by 1.4 years (Table 2), an effect driven by a compositional shift toward younger workers. Employment increased by 42% among workers aged 18–24 and by 16% among workers aged 25–29 but decreased by 12% among those aged 50–64.[19] These effects are robust to alternative weighting methods and balancing covariates (Table 2; Appendix Table C.3).

Panel B shows that restrictive regions were on a negative employment trend of similar magnitude for guards and other occupations prior to the reforms. Pro-outsourcing reforms

[16] Pre-reform effects are not jointly statistically significant.

[17] While our findings suggest a roughly 12% 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.

[18] Beyond these average effects, the dynamics display two further patterns, presented in Appendix D: total direct-hire guard employment and guard wages temporarily *rose* immediately after the reforms before returning to their longer-run paths (though the average post-reform effect on wages is statistically insignificant; see Table 3), and the demographic composition of direct-hire guard employment shifted toward younger workers even among firms that never adopted outsourcing. Both patterns are consistent with a frictional labor market: the availability of a larger pool of screened, occupation-committed young workers relaxed the hiring constraints faced by all employers, not just those that outsourced.

[19] Employment in an age group is zero in a small share of occupation–microregion–year cells, so these outcomes are inverse hyperbolic sine transforms, whose estimates depend on the value implicitly assigned to zero cells (Chen and Roth, 2024). Appendix Table C.4 shows the estimates are insensitive to this choice.

broke this trend for guards: employment sharply increased among young workers (Panel B.1) and decreased among older workers (Panel B.2) in restrictive regions, while control occupations continued their pre-legalization decline. Appendix Figure C.2 shows that these patterns are very different from the effect of the pro-outsourcing reforms on direct-hire incumbent guards, which are negative for most age groups, consistent with the negative effects of occupational layoffs documented in Felix and Wong (2026).

Finally, Table 2 shows that the reforms reduced the share of male workers by 0.8 percentage points and increased average education by 0.19 years; both changes are statistically significant.[20] 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.

Pro-outsourcing reforms had no effect on demographic-adjusted log earnings

Figure 5 plots coefficients β_τ of equation (2) for the effects of the pro-outsourcing reforms on guard wages, measured as real December log earnings, the standard measure for the RAIS dataset during this period (see Appendix B). Overall, we find that the pro-outsourcing reforms 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).

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.[21] Table 3 presents the corresponding average effects for these outcomes and for additional wage measures. While Panel A.1 suggests that the pro-outsourcing reforms might have induced a small reduction in as-reported wages (potentially driven by the compositional shift toward younger workers, who are typically paid less than older workers), this effect is neither large (a 0.6% reduction) nor statistically significant. We find similar near-zero and statistically insignificant point estimates with alternative weighting methods (columns (3)–(4)).

[20] Appendix Table C.6 reports other outcomes: the reforms increased the share of new hires (tenure below one year) by 1.4 percentage points, while average tenure declined by a statistically insignificant 0.10 years.

[21] Wages conditional on worker fixed effects are also conditional on dummies for gender × age-group × education cells, which vary within workers as they age or increase education)

Panel B of Figure 5 shows the corresponding cross-region DD series for the same wage outcomes—as reported and conditional on worker fixed effects. As in the DD visualization of employment effects shown in Panel B of Figure 3, there are differential trends in wages across regions (with near-identical magnitudes for guards and other occupations). However, unlike with employment, the pro-outsourcing reforms did not detectably break these trends.[22] These effects are robust to alternative weighting methods (Table 3).

Pro-outsourcing reforms increased formality and *in*formal earnings per Census

We use the 1991 and 2000 Censuses to estimate first-differenced regressions comparing 1991–2000 changes in outcomes for guards relative to control occupations, in restrictive versus permissive microregions. Formal employment of guards rises differentially in restrictive microregions (0.170 log points, $p < 0.01$); informal employment (0.076) and formal earnings (-0.029) show no significant change, while informal earnings rise (0.111, $p < 0.01$) (Appendix Table C.7). The formal-employment gain is concentrated among the youngest 1991 cohort and declines monotonically with age (Appendix Table C.8), consistent with the reforms drawing younger entrants into formal guard employment.

4 Mechanisms

What drives the young-worker entry documented in Section 3.2—the expansion of formal guard employment, driven by hiring from outside the formal sector and concentrated among the young? This section argues that these effects reflect internal occupation-specific scale economies in hiring, scaled up to the market level by pro-outsourcing policies that benefited the expansion of specialized security firms. We document that occupation-specific firm scale is robustly associated with the hiring of younger, less experienced workers new to the formal sector, not only among security guards but across all occupations in Brazil's employer–employee data. We then weigh alternative mechanisms: regulatory arbitrage and monopsony.

[22] With the exception of a potential break in the trends for as-reported wages, though this effect is not statistically significant, as shown in Table 3.

4.1 The Scale Mechanism

The central feature distinguishing contract firms from direct employers is scale in a single occupation. At a diversified direct employer, security guards are a peripheral function and hiring one is an infrequent event, so the employer has little reason to build recruitment infrastructure for the role; unable to assess a worker who arrives without a formal record, it fills guard positions with experienced workers whose records certify their reliability—older and more expensive, but known quantities. A specialized security firm makes the opposite choice: guards are its core business, and by aggregating guard demand across dozens or hundreds of client firms it reaches a volume of hiring at which fixed investments in human-resource functions are spread across many placements. Consistent with this, guards at contract firms are markedly younger than direct hires (about 34 versus 40 years old), and contract firms are specialized in the occupation in a way direct employers are not: the median contract firm employs about 480 workers, nearly all of them guards, whereas the median direct employer of guards has about 60 workers and only four guards (Appendix Table B.1).

These investments span the employment cycle. In *screening and matching*, we learned during field visits to security firms that contract firms administer standardized physical-fitness tests, psychological evaluations, and group interviews, maintain databases of vetted candidates, and develop expertise in assigning guards to client sites. In *training*, they build occupation-specific human capital beyond the legal minimum, an investment that pays off only at volume. In *monitoring and management*, they run centralized attendance and performance systems and hold reserve pools of guards, turning an idiosyncratic absence into a manageable portfolio problem (Goraya et al., 2025; Li and Wong, 2026). And in *reassignment*, they redeploy a poor-fit guard to another client site rather than absorb a costly separation (Guo et al., 2024). Each function carries an occupation-specific fixed cost that a direct employer hiring a handful of guards cannot justify but a firm placing hundreds can spread across its workforce.[23] This is the sense in which outsourcing a non-core activity

[23] Autor (2001) documents analogous screening and training investments by temporary-help agencies in the United States, and Battiston et al. (2021) studies job rotation and experience accumulation within a Colombian security firm.

differs from outsourcing a core one: the gains come not from circumventing labor regulation (Bertrand et al., 2025) but from a scale of human-resource operations that no single client could reach on its own (Abraham and Taylor, 1996; Autor, 2009).

The organizational difference determines which workers each type of employer hires. Facing a young applicant from outside the formal sector, a direct employer without screening cannot separate a good match from a bad one, and the safe choice is an experienced worker whose record already signals reliability—even when a younger entrant may be an equally good match at a lower wage. Contract firms overturn this experience advantage: their infrastructure lets them screen entrants at scale, hire young workers from outside the formal sector, and bring them to a common standard. A fall in the cost of outsourcing therefore shifts hiring from experienced workers toward younger entrants, the market-level reallocation documented in Section 3.

A screening model, presented in Appendix D.1, formalizes the argument and shows how within-firm occupation-specific economies of scale at hiring can raise formal sector occupational employment at the local labor market level. We formalize screening rather than training, monitoring, or reassignment because our evidence is about who firms hire, and screening is the member of this family that acts directly on the hiring margin; the others push in the same direction.

4.2 Occupation-Specific Scale Effects in Hiring

To provide evidence for this mechanism, Table 4 reports regression coefficients documenting the characteristics of newly hired workers as a function of occupation-specific firm scale from the following regression:

$$y_{zomt} = \beta \ln l_{zomt} + \gamma \ln l_{zmt} + \delta_{omt} + \mu_z + \varepsilon_{zomt}, \tag{3}$$

where each outcome y_{zomt} measures characteristics of new hires in year t into occupation o of firm z in microregion m—workers whose admission year is t and who are employed on

December 31 of year t.[24] The key regressors of interest are $\ln l_{zomt}$ (*occupation-specific scale*), measured as the log of firm z's occupation-o employment in microregion m at the end of year t, and $\ln l_{zmt}$ (*firm-level scale*), measured as the log of total firm employment in microregion m at the end of year t. Fixed effects δ_{omt} at the occupation $\times$ microregion $\times$ year level absorb differential trends in local composition of new hires common to all firms, while firm fixed effects μ_z absorb time-invariant firm-level variation in the composition of new workers hired by firm z that is common to all occupations and regions where firm z operates.

Panel A of Table 4 presents our estimates of scale effects for all occupations in the employer–employee linked data. The patterns for firm-level scale are consistent with widely documented patterns of positive assortative matching between high-wage workers and large, high-paying firms (for example, in the United States (Song et al., 2019), West Germany (Card et al., 2013), and Brazil (Alvarez et al., 2018)): new hires at larger firms are less likely to be hired from outside the formal sector (that is, they are more likely to transition directly between formal sector firms), more likely to have ever had a formal sector job, are older, more educated, and paid higher wages at hiring.

Occupation-specific scale effects feature the *opposite sign* as firm-level scale: Conditional on firm size, as the number of workers in a given occupation in the firm increases, new hires into that occupation are: (a) more likely to be hired directly from outside the data (i.e., from unemployment or informality); (b) more likely to have never had a formal sector job; (c) younger; (d) less educated; and (e) paid lower wages at hiring. These effects are of roughly the same magnitude, and sometimes much larger, than scale effects at the firm level.

Panel A of Table 4 implies that a ten percent increase in occupation-specific employment raises the share of new hires drawn from outside the formal sector by 0.07 percentage points, raises the share who had never held a formal job by 0.13 percentage points, lowers average age at hiring by 0.09 years, and lowers hiring wages by 0.3 percent. Panel B of Table 4 presents the same effects but restricting the set of occupation cells to guard cells only.[25] The patterns for guards are similar in sign and magnitude for all outcomes except years

[24] Appendix Table C.9 shows similar gradients for all occupations when scale is instead measured at the end of year $t-1$, before the new hires arrive. A same-year specification allows for a much bigger sample as it includes firms and occupation cells within firms in their first year of operations.

[25] That is, including only firm x (guard occupation) x microregion x year cells. Firm-level scale is of course still measured as total firm employment across all occupations.

of education, where guard scale increases the education of the marginal entrant, a pattern potentially driven by minimum education requirements for guards.

4.3 Alternative Mechanisms

The evidence in this section points to internal occupation-specific scale economies in hiring as the primary mechanism, scaled up to the market level by pro-outsourcing policies that benefited the expansion of specialized security firms, driving the workforce compositional shift and the employment and (null) demographic-adjusted wage effects at the market level documented in Section 3. Below we briefly discuss alternative mechanisms that could explain some, though not all, patterns we see, and which are more relevant to other settings, but less relevant to 1990s Brazil.

Rent-sharing. In many high-income settings, outsourcing lowers incumbent workers' pay by removing from their pay their direct employer's firm-specific wage premia (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017). This channel is present in our setting: Felix and Wong (2026) document that the pro-outsourcing reforms coincided with a wave of occupational layoffs, which imposed wage losses on incumbent guards. However, these losses are concentrated on laid-off incumbents initially at high-wage firms, and do not translate to occupation-level wage losses. Moreover, the average wage differential between outsourced and direct-hire security guards is small in Brazil. Conditional on worker characteristics, outsourced guards earn only 1.6–1.9% less than direct hires (Appendix Table D.1), far below the 10–25% penalties found for outsourced low-wage work in other settings. In the same data, Guo et al. (2024) estimate that the penalty is occupation-specific, 11% for outsourced cleaners against 1.3% for guards, consistent with rent stripping biting in lower-wage, less-regulated occupations but not in this medium-wage licensed one.

Monopsony. Outsourcing could also lower wages if contract firms wield greater labor market power than direct employers. Yet the expansion of contract-firm employment produced no market-level decline in demographic-adjusted wages. This is especially telling given the high labor market power documented for Brazil's formal sector in the 1990s (Felix, 2026): even in that environment, shifting employment toward contract firms did not compress pay. The pattern contrasts with Estefan et al. (2024), who find wage markdowns fell in Mexico

after outsourcing was *banned*, implying outsourcing there had raised employer power.

Regulatory arbitrage. Firms may also outsource to circumvent firing costs or other labor regulations (Bertrand et al., 2025). In our setting, this channel is unlikely to be the primary driver: both direct employers and contract firms are subject to the same Brazilian labor regulations. Outsourced positions also show lower turnover rather than worse conditions: their exit hazard from formal employment is about a percentage point lower at entry and converges with tenure (Appendix Table D.2). Moreover, outsourced guards switch occupations about 2.6 percentage points less per year, almost entirely within the firm, while being marginally less likely to move into management (Appendix Tables D.3 and D.4). Such patterns are not easily reconciled with pure regulatory arbitrage.

There is also a specialization-related reason why monopsony or arbitrage are less relevant in our setting: the effects we document concern the outsourcing of a *non-core* activity. Both the Mexican monopsony case of Estefan et al. (2024) and the Indian firing-cost case of Bertrand et al. (2025) concern *core* production labor, where the client already runs the outsourced occupation at scale. Our setting instead pools the demand for a *non-core* occupation, fragmented across many small employers, which may be why scale economies dominate here. The difference fits the occupation-specific scale mechanism we emphasize: here the growth of specialized firms draws workers into formal employment from informality and non-employment rather than transferring rents from incumbents to employers.

5 Conclusion

This paper asks what role specialized non-core service contract firms play in the labor markets of developing economies—in particular, whether their rise can increase young-worker formal-sector employment. Leveraging a pair of 1993–1994 Brazilian reforms that sharply reduced the relative cost of outsourcing security services, we estimate the reforms' market-level effects on the security guard occupation in a triple-differences design exploiting cross-region and cross-occupation variation in pre-reform restrictions.

The answer is yes: the reforms reallocated guard employment from older workers to younger workers and persistently expanded the occupation. Total employment rose by

roughly 4%; entry from unemployment or informality rose by 7%, accounting for about half of the reform-induced employment gains; and employment shifted from older to younger workers, while demographic-adjusted wages did not change. These effects mirror a general pattern in Brazil's employer–employee data: conditional on total firm size, employers with greater occupation-specific scale hire workers who are younger, paid less at entry, and more likely to be entering formal employment for the first time.

Our findings point to a demand-side channel of formalization: by reallocating employment toward the employers best positioned to hire workers new to formal work, domestic outsourcing increased formal sector employment among the young—one margin through which the growth of specialized firms can contribute to economic development.

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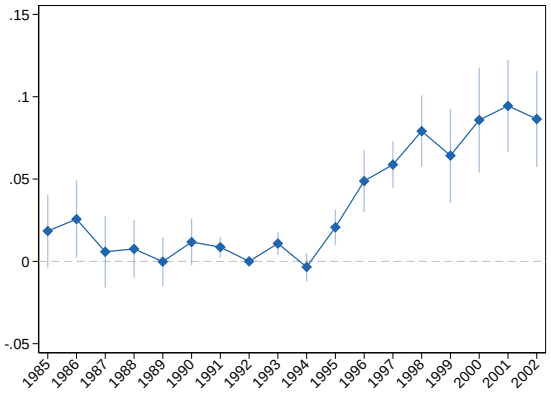
Figure 1: Effect of pro-outsourcing reforms on outsourcing prevalence

Panel A: DDD estimates

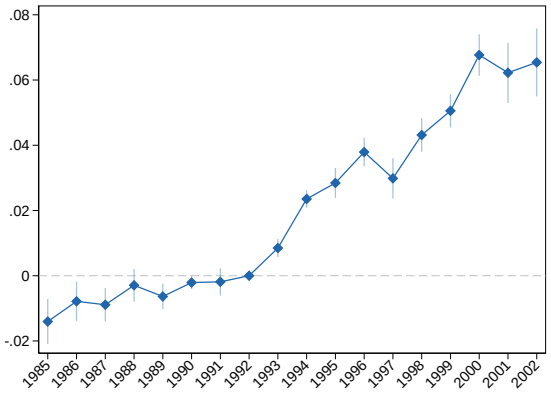


Panel B: Cross-region DDs, by occupation

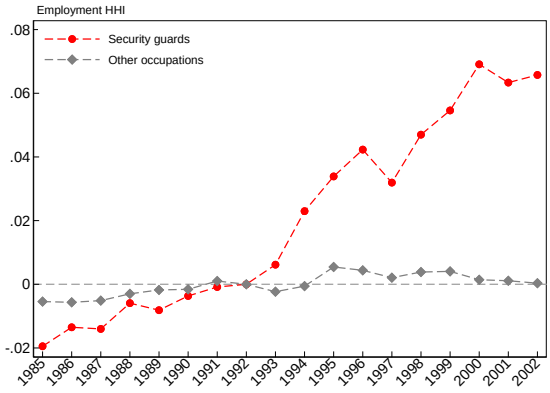
Note: Panel A plots the coefficients β_τ from the DDD regression measuring the impact of the pro-outsourcing reforms in equation (2), which compares guards to other occupations in restrictive versus permissive regions. Each observation is a microregion x occupation x year cell. Outsourced share is the share of all employment in a microregion-occupation pair working for a contract-firm. The omitted year is 1992. The sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation. Panel B plots the α_τ coefficients from the cross-region DD regression (the restrictive-versus-permissive difference in outsourced share), estimated separately for guards and for other occupations; the specification is given in footnote 14. A microregion's outcome for other occupations is an average across occupations, with equal weights, and the microregion-level regressions are weighted by microregion entropy balancing weights.

Figure 2: Effect of pro-outsourcing reforms on employment concentration (HHI)

Panel A: DDD estimates



Panel B: Cross-region DDs, by occupation

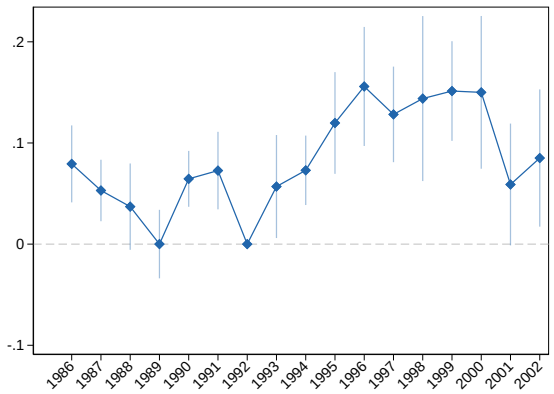


Note: See notes to Figure 1. The outcome variable is employment HHI, the Herfindahl-Hirschman employment index (equal to the probability that any two workers work for the same firm) in a microregion-occupation pair.

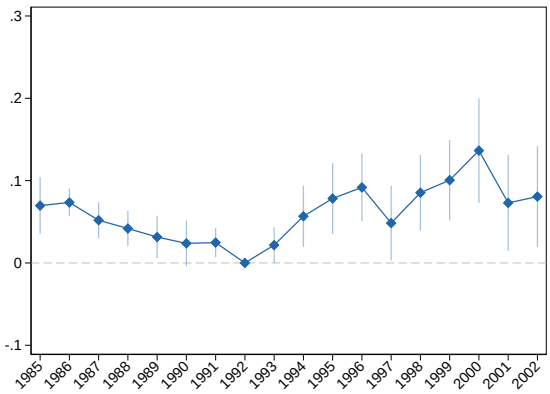
Figure 3: Effect of pro-outsourcing reforms on worker entry and total employment

Panel A: Year-by-Year DDD

A.1: Entry from outside RAIS

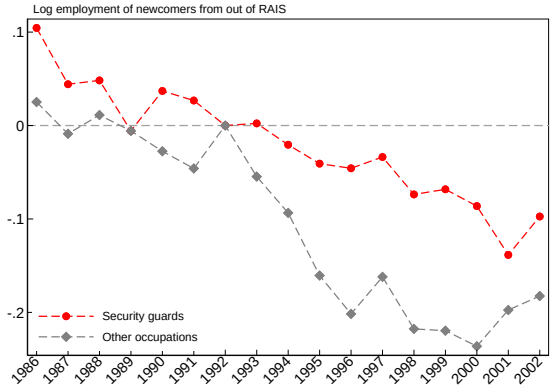


A.2: Log employment

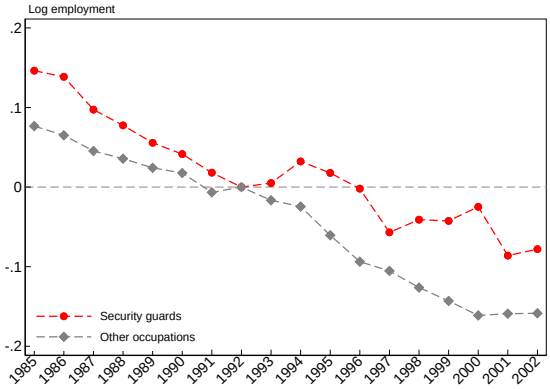


Panel B: Cross-region DDs, by occupation

B.1: Entry from outside RAIS



B.2: Log employment

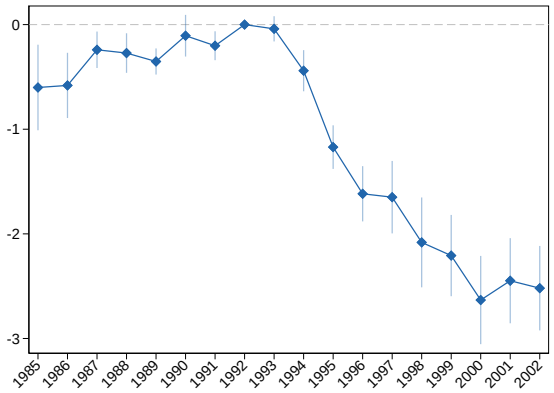


Note: See notes to Figure 1. The outcome variable in Panels A.1 and B.1 is log number of workers from outside of RAIS in the microregion-occupation pair, whereas the outcome in Panels A.2 and B.2 is log total employment in the microregion-occupation pair; employment counts enter as inverse hyperbolic sine transforms (see notes to Table 1). "Entry from outside RAIS" refers to workers not present in RAIS in the previous year, i.e., hired from unemployment, informality, or non-participation.

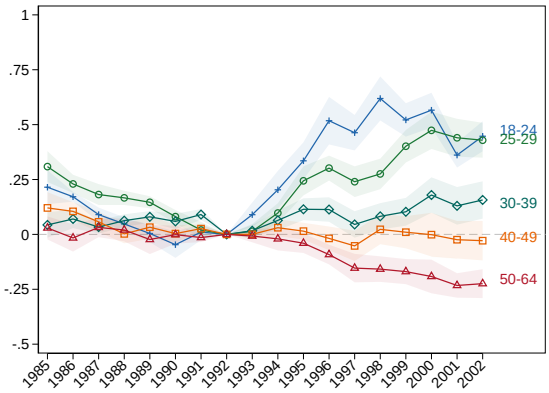
Figure 4: Effect of pro-outsourcing reforms on workforce age composition

Panel A: Year-by-Year DDD

A.1: Worker age

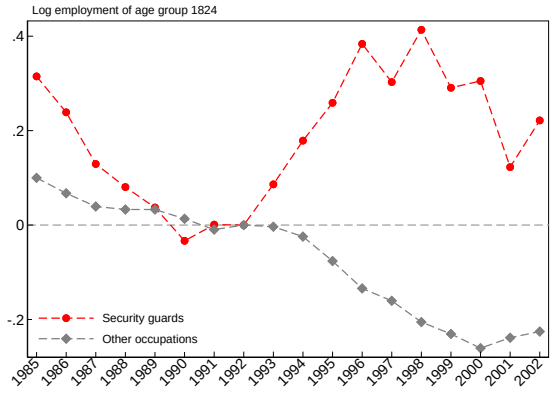


A.2: Log employment by age groups

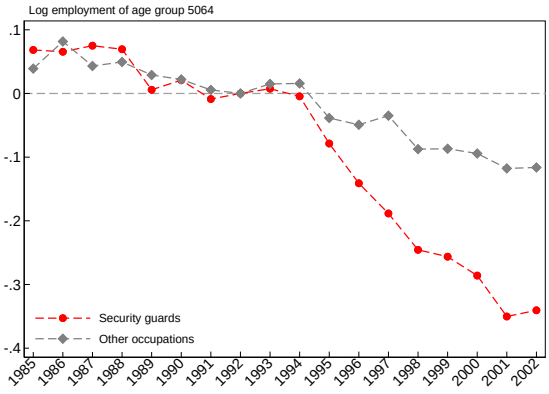


Panel B: Cross-region DDs, by occupation

B.1: Log employment, ages 18–24



B.2: Log employment, ages 50–64

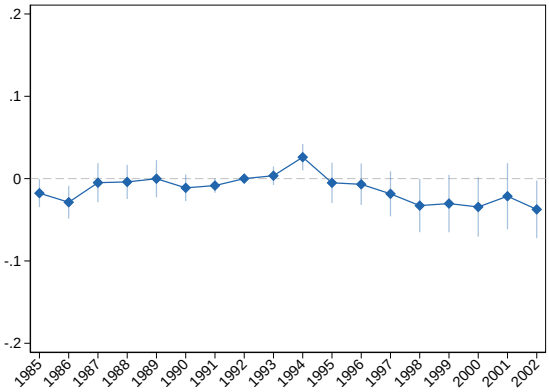


Note: See notes to Figure 1. The outcome variable in Panel A.1 is worker age, while the outcome variables in Panel A.2 are log employment by different age groups, as inverse hyperbolic sine transforms (see notes to Table 1). Age groups are defined according to the age group variable consistently reported throughout the period.

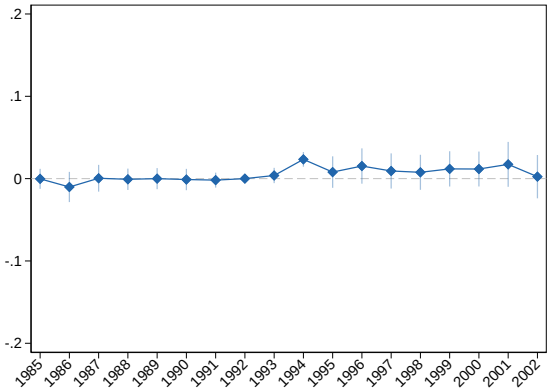
Figure 5: Effect of pro-outsourcing reforms on wages

Panel A: Year-by-Year DDD

A.1: Log earnings, as reported

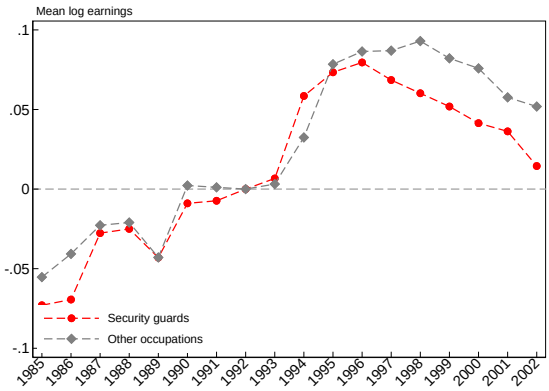


A.2: Log earnings, worker fixed effects

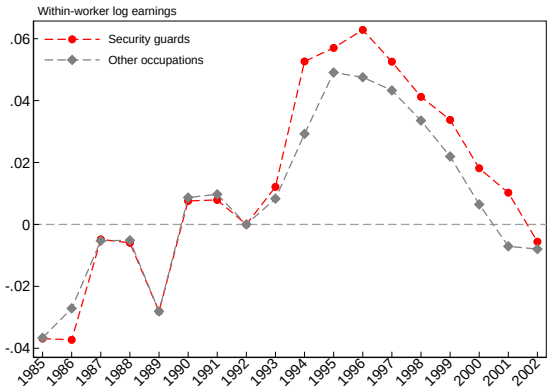


Panel B: Cross-region DDs, by occupation

B.1: Log earnings, as reported



B.2: Log earnings, worker fixed effects



Note: See notes to Figure 1. 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 gender × age-group × education-category cells. The demographic cells are not interacted with year; conditional on the worker fixed effects, they are identified by workers who change age group or education category over time.

Table 1: Effect of pro-outsourcing reforms on guard outsourcing and employment

	Baseline treated mean	Policy effect (DDD)			
	(Guards in Restrictive)	EB	IPW	EB+RA	Obs.
	(1)	(2)	(3)	(4)	(5)
Panel A: Prevalence of outsourcing					
Outsourced share	0.125	0.045***	0.057***	0.041***	56,592
		(0.013)	(0.010)	(0.009)	
Employment HHI	0.062	0.047***	0.037***	0.046***	56,592
		(0.003)	(0.002)	(0.003)	
Panel B: Log total employment					
Log total employment	6.763	0.038***	0.027*	0.036**	56,592
		(0.011)	(0.014)	(0.014)	
Panel C: Log employment by contract type					
Direct-hire	6.589	-0.070***	-0.070***	-0.066***	56,592
		(0.020)	(0.019)	(0.020)	
Outsourced	3.609	0.112	0.347**	0.077	56,592
		(0.145)	(0.130)	(0.133)	
Panel D: Log employment origins					
Workers from outside RAIS (unemp., informality, NILF)	5.253	0.069**	0.095***	0.061**	53,448
		(0.025)	(0.026)	(0.027)	
Workers from inside RAIS	6.501	0.022**	0.004	0.022**	53,448
		(0.009)	(0.016)	(0.009)	

Notes: This table shows effects of the pro-outsourcing reforms on the prevalence of outsourcing in a region-occupation pair (Panel A) and on employment (Panels B–D), estimated using equation 1's weighted DDD specification, which compares guards to other occupations in restrictive versus permissive regions before versus after the reforms. 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-Hirschman employment index within a region-occupation pair. Employment outcomes are inverse hyperbolic sine transforms of employment counts, which accommodate cells with zero employment; total employment is at least two workers in every cell by construction (see Appendix Table C.4 for robustness to the transform). Column (1) reports the 1992 baseline mean for treated cells (guards in restrictive regions); columns (2)–(4) report the DDD coefficient under entropy balancing (EB), inverse-propensity weighting (IPW), and entropy balancing with regression adjustment (EB+RA); column (5) reports the number of microregion-occupation-year observations. Column (1) is the unweighted mean across the treated guard × microregion cells in 1992 (entropy-balancing weights equal one in restrictive regions); the estimates in columns (2)–(4) are weighted by their respective specification's weights. All weights balance restrictive and permissive regions on baseline homicide rate, import competition exposure, share of employment in tradeable industries, log total formal employment, and unemployment rate. All regressions include microregion-occupation, microregion-year, and occupation-year fixed effects. The sample is balanced and includes 262 microregions and twelve 2-digit CBO94 occupational groups (security guards plus 11 major comparison groups, present in all microregions; cleaners are excluded). Standard errors are two-way clustered by Regional Labor Court and occupation and reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 2: Effect of pro-outsourcing reforms on guard characteristics

	Baseline treated mean (Guards in Restrictive)	Policy effect (DDD)			
		EB	IPW	EB+RA	Obs.
	(1)	(2)	(3)	(4)	(5)
Age	42.384	-1.386***	-1.334***	-1.436***	56,592
		(0.177)	(0.111)	(0.123)	
Direct-hire age	42.884	-0.962***	-0.779***	-1.027***	56,592
		(0.146)	(0.099)	(0.096)	
Outsourced age	40.445	0.668	-0.316	0.697*	43,370
		(0.403)	(0.353)	(0.380)	
Log employment by worker age group					
18–24	3.795	0.351***	0.311***	0.346***	56,592
		(0.032)	(0.023)	(0.025)	
25–29	4.526	0.150***	0.135***	0.155***	56,592
		(0.027)	(0.022)	(0.019)	
30–39	5.370	0.046*	0.036*	0.047**	56,592
		(0.022)	(0.019)	(0.019)	
40–49	5.383	-0.048	-0.043	-0.055	56,592
		(0.031)	(0.027)	(0.031)	
50–64	5.549	-0.132***	-0.135***	-0.140***	56,592
		(0.022)	(0.018)	(0.022)	
Log employment by highest attained education					
Less than High School	6.688	0.061***	0.039*	0.060***	56,592
		(0.013)	(0.020)	(0.014)	
Some High School	3.196	0.195***	0.080***	0.192***	56,592
		(0.019)	(0.023)	(0.028)	
High School graduate	2.944	0.250***	0.141***	0.259***	56,592
		(0.021)	(0.005)	(0.025)	
Some College	1.208	0.199***	0.170***	0.219***	56,592
		(0.051)	(0.052)	(0.048)	
College graduate or more	1.722	-0.019	-0.010	-0.026	56,592
		(0.032)	(0.019)	(0.039)	
Other workforce characteristics					
Male	0.989	-0.008***	-0.007***	-0.007**	56,592
		(0.002)	(0.002)	(0.002)	
Years of education	4.864	0.190***	0.130***	0.199***	56,592
		(0.029)	(0.019)	(0.027)	

Notes: This table shows the effect of the pro-outsourcing reforms on average worker age (overall and separately for direct-hire and outsourced workers), on log employment by age group and by education group, and on the male share and average years of education of the workforce, estimated with the weighted DDD specification. See notes to Table 1, which describe the inverse hyperbolic sine transform applied to all employment outcomes.

Table 3: Effect of pro-outsourcing reforms on wages

	Baseline treated mean	Policy effect (DDD)			
	(Guards in Restrictive)	EB	IPW	EB+RA	Obs.
	(1)	(2)	(3)	(4)	(5)
Panel A: Real December log earnings					
As reported (unconditional)	7.205	-0.006	-0.030*	-0.003	56,592
		(0.016)	(0.016)	(0.016)	
Conditional on worker demographics	-0.144	0.011	-0.012	0.014	56,592
		(0.015)	(0.015)	(0.014)	
Conditional on worker FEs and time-varying demographics	-0.023	0.013	-0.003	0.016	56,592
		(0.013)	(0.013)	(0.009)	
Panel B: Real December log earnings by contract type					
Direct-hire, as reported	7.209	0.001	-0.009	0.004	56,592
		(0.014)	(0.013)	(0.014)	
Direct-hire, cond. on demographics	-0.168	0.017	0.002	0.019	54,497
		(0.012)	(0.012)	(0.012)	
Direct-hire, cond. on worker FEs	-0.055	0.021**	0.013	0.021**	54,497
		(0.009)	(0.010)	(0.008)	

Notes: This table shows the effect of the pro-outsourcing reforms on wage outcomes, estimated with the weighted DDD specification. See notes to Table 1. 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: RAIS reports each worker's December earnings as multiples of that year's federal minimum wage throughout the sample period, whereas contract hours are only reported starting in 1995; we convert earnings to 2017 reais using each year's federal minimum wage from Brazil's Instituto de Pesquisa Econômica Aplicada (IPEA). Post-1995, direct-hire guards average 42.4 contract hours a week versus 43.6 for outsourced guards (see Appendix Table B.1). Earnings outcomes are expressed in natural logarithms.

Table 4: Characteristics of new hires as a function of occupation-specific and firm-level scale

Panel A: All occupations

	(1) Outside RAIS	(2) Ever formal	(3) Age	(4) Education	(5) Log wage
Log number of employees in occupation o at firm z in region m at end of year t	0.0071***	-0.0134***	-0.8914***	-0.1265***	-0.0317***
	(0.0002)	(0.0001)	(0.0041)	(0.0015)	(0.0003)
Log number of employees at firm z in region m at end of year t	-0.0152***	0.0064***	0.3151***	0.0330***	0.0391***
	(0.0004)	(0.0002)	(0.0055)	(0.0024)	(0.0006)
Occ×MMC×Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	22161387	22161387	22039063	21970115	22101204
Adj. R^2	0.138	0.221	0.288	0.585	0.691
Mean of outcome	0.7394	0.6266	29.6964	7.5503	6.9571

Panel B: Security guards

	(1) Outside RAIS	(2) Ever formal	(3) Age	(4) Education	(5) Log wage
Log number of employees in occupation o at firm z in region m at end of year t	0.0205***	-0.0126***	-1.3224***	0.0414***	-0.0334***
	(0.0015)	(0.0013)	(0.0403)	(0.0099)	(0.0017)
Log number of employees at firm z in region m at end of year t	-0.0241***	0.0115***	0.0498	0.1177***	0.0473***
	(0.0013)	(0.0012)	(0.0366)	(0.0085)	(0.0014)
MMC×Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	441,770	441,770	439,199	438,728	440,190
Adj. R^2	0.144	0.221	0.320	0.437	0.698
Mean of outcome	0.6780	0.7479	39.1121	4.9868	7.0549

Notes. This table shows regression coefficients on the firm-scale and occupation-specific firm scale regressors in equation (3). Occupations are measured at the CBO 2-digit level. Each observation is a firm × occupation × microregion × year cell, spanning years 1986–2000. Outcome variables are characteristics of new hires in year t (workers whose admission year is t and who are employed on December 31 of year t); the scale regressors are measured at the end of the same year t. Outside RAIS is a dummy indicating whether the worker was not in the RAIS dataset at year $t-1$. Ever formal is a dummy indicating whether the worker was ever seen in the RAIS dataset in any year prior to year t. Standard errors are clustered at the firm level and reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Online Appendices for

"Domestic Outsourcing and Young-worker Entry into the Formal Sector,"

by Mayara Felix and Michael B. Wong

A Institutional Setting

The fact that Brazil's regional courts differed in their permissiveness to outsourcing prior to Súmula 331 is well-known to industry insiders, but is poorly documented. During a field trip in September 2019, we conducted interviews with managers of security service firms, managers of businesses that either employed or outsourced security services, numerous security guards, labor lawyers, and staff of a security guard union in Recife. In one interview, we asked the CEO of a family-run security service firm incorporated in the 1970s how the legalization of outsourcing by Súmula 331 in 1993 affected her firm. She insistently responded that outsourcing had never been considered illegal. This response contradicted various legal articles that suggest outsourcing had been declared illegal by Súmula 256 in 1986 (da Cruz 2009; Biavaschi and Droppa 2011; Cooney et al. 2015).

We consulted several labor lawyers, who informed us that local courts in Brazil's South, which had historically been friendlier to labor, likely enforced the ban on outsourcing much more vigorously than courts in the rest of Brazil. To further investigate local court permissiveness toward outsourcing, we submitted formal requisitions to all 24 regional labor courts asking if: (1) lawsuits from 1985-1993 were available, (2) it would be possible for us to have a list of all lawsuits concerning outsourcing, and (3) they still had the cases listed as precedents for Súmula 256 and 331. Sadly, we received answers in the negative for almost all local courts. Most no longer kept cases before 1993.

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

Appendix Table A.4 summarizes our classification of Brazil's 24 regional labor courts. Columns (5) and (6) summarize the information from legal precedents, while columns (7)–(9)

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

One set of supporting documents for the classification comes from research performed by Magda Biavaschi and Alisson Droppa, two Brazilian legal historians who interviewed prominent judges and lawyers regarding the history surrounding Súmula 331, as well as a set of 28 interview transcripts that they generously provided. Appendix Tables A.5 and A.6 display all quotes from these interviews concerning Courts' interpretation on the legality of outsourcing.

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

The interviews and legal precedents point to very restrictive interpretations in Brazil's geographic South,[26] including the states of Rio Grande do Sul (4th regional court), Paraná (9th), and Santa Catarina (12th), and a restrictive—though to a lesser extent—interpretation in the countryside of the state of São Paulo (15th region, Campinas), in contrast with the permissive city of São Paulo (2nd region).

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

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

courts were permissive.

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

Table A.1: De jure regulatory environment for outsourcing vs. direct-hiring, by activity

	Legal cost of outsourcing		Legal cost of direct-hiring	
Activity	Pre-1994	Post-1994	Pre-1994	Post-1994
	(1)	(2)	(3)	(4)
Armed security	**High** (Súmula 256, 1986)	Low (Súmula 331, Dec 1993)	Low (Lei 7.102/1983 applies to banking security only)	**High** (Lei 8.863/1994 extends Lei 7.102 to all firms directly hiring armed security)
Cleaners	**High** (Súmula 256, 1986)	Low (Súmula 331, Dec 1993; cleaning named explicitly)	Low (no statutory requirements; generic CLT)	Low (no statutory requirements; generic CLT)
Other *atividade-meio* (reception, telephony, copying, on-site typing)	**High** (Súmula 256, 1986)	Medium (Súmula 331 generic clause: outsourcing allowed only if *pessoalidade* is absent)	Low (no statutory requirements; generic CLT)	Low (no statutory requirements; generic CLT)

Notes. This table summarizes the de jure legal cost of outsourcing and of directly hiring workers in each activity, before and after the 1993–1994 reforms. "High"/"Medium"/"Low" refer to the de jure cost imposed by the cited statutory or jurisprudential instrument. "Generic CLT" refers to the general labor framework of Decreto-Lei 5.452/1943. See Table A.2 for the detailed outsourcing column and Table A.3 for the post-1994 direct-hiring regime applicable to armed security.

Table A.2: Legal status of outsourcing by activity, pre-1986 to 2017

Activity	**Pre-1986**	**1986–1993 (Súmula 256)**	**1993–2017 (Súmula 331)**
Panel A: Permitted nationally throughout period			
Temporary work via agency	Allowed since 1974 (Law 6.019/1974[a])	Allowed (statutory exception)	Allowed (item I)
Bank security	Allowed since 1983 (Law 7.102/1983[b])	Allowed (statutory exception)	Allowed (item III)
External legal, accounting, audit, consulting	Allowed (civil/commercial contract, not labor)	Allowed (civil/commercial contract, not labor)	Allowed under generic clause; *pessoalidade* absent
Panel B: Legalized nationally in 1993 (Súmula 331)			
Non-bank private security	Doctrinally contested; some courts treated as illegal *locação de mão-de-obra*	Illegal under 256 (case law contested)	Allowed (item III, after Law 8.863/1994[c])
Cleaning and conservation	Civil contract in principle, but courts often re-classified as direct employment when on-site/personal	Illegal under 256 (case law contested)	Allowed (item III, named explicitly)
Reception, telephony, copying, typing (on-site)	Civil contract in principle; risk of reclassification	Illegal under 256 (case law contested)	Allowed under generic *atividade-meio* clause only if *pessoalidade* absent
Panel C: Forbidden nationally throughout period			
Core production tasks (assembly line; teachers in a school; pulp-wood cutting in paper firms)	Civil contract on paper; courts often pierced	Illegal under 256	Illegal (*atividade-fim*)[d]

Notes. This table shows the legal status of outsourcing by activity under the pre-1986, 1986–1993 (Súmula 256), and 1993–2017 (Súmula 331) regimes. Cells under "Pre-1986" and "1986–1993" summarize the most common labor-court resolutions, not statutory rules, except where a statute is cited (Panel A). Cited laws: [a] Lei 6.019/1974; [b] Lei 7.102/1983; [c] Lei 8.863/1994; [d] Lei 13.467/2017.

Table A.3: Regulatory bundle applying to any firm hiring armed guards (vigilantes) after Lei 8.863/1994

Category	**Requirement**	**Statutory source**
Authorization	Federal operating authorization (Ministério da Justiça, via Departamento de Polícia Federal)	Lei 7.102 art. 20, I; extended to in-house guards by Lei 8.863 art. 2 (§4º of art. 10)
Authorization	Annual review/renewal of operating authorization	Lei 7.102 art. 20, X (added by Lei 8.863 art. 5)
Capital	Minimum paid-in capital: 1,000 × MVR (1983–1994); 100,000 UFIR (1995–)	Lei 7.102 art. 13; Decreto 89.056/1983; later amended by Lei 9.017/1995
Ownership	No foreign ownership or administration	Lei 7.102 art. 11
Personnel–screening	Criminal-record screening of directors and all employees	Lei 7.102 arts. 12 and 16, VI
Personnel–vigilante prerequisites	Brazilian nationality; minimum age 21; minimum schooling; psychological + health + physical aptitude exams; clean criminal record; electoral and military obligations current	Lei 7.102 art. 16, I–VII
Training	Approved completion of a *curso de formação de vigilante* at a federally authorized training establishment	Lei 7.102 art. 16, IV (tightened by Lei 8.863 art. 4)
Worker registration	Prior registration of the vigilante with the Departamento de Polícia Federal	Lei 7.102 art. 17
Operational	Firm-level ownership and responsibility for firearms; restriction to .32 / .38 revolver in service	Lei 7.102 arts. 21–22
Penalty	Warning; fine; temporary prohibition of operation; cancellation of registration	Lei 7.102 art. 23 (parts I–IV)
Penalty	Criminal referral; borrower firm exposed when employing unauthorized vigilantes	Lei 7.102 art. 23 + applicable Penal Code provisions
Compliance window	120 days from publication (29 March 1994) for existing operators to adapt	Lei 8.863 art. 6

Notes. This table shows the regulatory requirements that apply to any firm employing armed guards after Lei 8.863/1994, together with each requirement's statutory source. "Vigilante" is the legal term for an armed private-security worker (Lei 7.102 art. 15, as amended by Lei 8.863 art. 3). These requirements do not attach to unarmed building-service workers (porteiros, recepcionistas, controladores de acesso). Sources: Lei 7.102/1983; Lei 8.863/1994; Decreto 89.056/1983; Lei 9.017/1995.

Table A.4: Classification of regional labor courts

				Count summary of Regional Labor Court classification materials							
				Regional Labor Court rulings on outsourcing appealed to Superior Labor Court before Súmula 331		Interviews about Regional Labor Courts' rulings on outsourcing before Súmula 331			Prevalence of security guard outsourcing across Regional Labor Court's jurisdictions		
Regional Labor Court (1)	States under jurisdiction (2)	Classification (3)	Geographic Region (4)	Total appeals (5)	Appeals where Regional Labor Court ruled the end-firm as the employer (6)	Total Interviews (7)	Interviews where legality of outsourcing is discussed (8)	Court's interpretation of outsourcing described as Restrictive (9)	1992 contract-firm share of guard employment (10)	1992-1999 change in contract-firm share (11)	Chow test p-value (12)
4	Rio Grande do Sul	Restrictive	South	3	2	8	7	7	0.37	0.20	0.00
9	Paraná	Restrictive	South	3	3	6	4	4	0.32	0.14	0.00
12	Santa Catarina	Restrictive	South	1	0	1	1	1	0.39	0.19	0.00
15	Campinas (São Paulo state, excl. capital)	Restrictive	Southeast	0		5	4	3	0.32	0.25	0.00
1	Rio de Janeiro	Permissive	Southeast	1	0	0			0.45	0.16	0.32
2	São Paulo (capital)	Permissive	Southeast	2	1	3	3	0	0.40	0.18	0.29
3	Minas Gerais	Permissive	Southeast	0		0			0.34	0.13	0.02
5	Bahia	Permissive	Northeast	0		0			0.42	0.20	0.09
6	Pernambuco	Permissive	Northeast	0		1	1	0	0.40	0.06	0.74
7	Ceará	Permissive	Northeast	1	1	0			0.41	0.07	0.04
8	Amapá and Pará	Permissive	North	0		0			0.51	0.09	0.07
10	Distrito Federal and Tocantins	Permissive	Central-West	0		0			0.52	0.10	0.01
11	Amazonas and Roraima	Permissive	North	0		0			0.42	0.23	0.89
13	Paraíba	Permissive	Northeast	0		0			0.37	0.08	0.00
14	Acre and Rondônia	Permissive	North	0		1	1	0	0.54	-0.03	0.00
16	Maranhão	Permissive	Northeast	0		0			0.48	0.03	0.34
17	Espírito Santo	Permissive	Southeast	0		0			0.41	0.10	0.02
18	Goiás	Permissive	Central-West	0		0			0.36	0.17	0.48
19	Alagoas	Permissive	Northeast	0		0			0.39	0.10	0.29
20	Sergipe	Permissive	Northeast	0		0			0.35	0.10	0.00
21	Rio Grande do Norte	Permissive	Northeast	0		0			0.39	0.04	0.21
22	Piauí	Permissive	Northeast	0		0			0.35	-0.01	0.02
23	Mato Grosso	Permissive	Central-West	0		0			0.37	0.09	0.09
24	Mato Grosso do Sul	Permissive	Central-West	0		0			0.43	-0.04	0.52

Notes: This table shows, for each regional labor court, counts of the qualitative materials (legal precedents and interviews) underlying our restrictive/permissive classification, along with the prevalence of security guard outsourcing in its jurisdiction. Column (5) lists the number of regional court rulings on outsourcing brought to the Superior Labor Court between 1986 (Súmula 256 in place) and December 1993 (Súmula 331 in place). See Appendix Table A.7 for a summary of each ruling. Column (7) lists the number of interviews (with regional labor court justices, jurists, lawyers, circuit court judges, and union leaders) conducted by the authors and/or by Brazilian jurist and former regional labor court justice Magda Biavaschi and legal historian Alisson Droppa, concerning Regional Courts' interpretation of the legality of outsourcing prior to Súmula 331. See Appendix Tables A.5 and A.6 for summary quotes, and Biavaschi and Baltar (2013) for the legal research documenting regional disparities across regional labor courts' rulings on outsourcing prior to Súmula 331. Columns (10)–(12) show the prevalence of security guard outsourcing in each regional labor court's jurisdiction, as measured by the baseline share of all guards that are employed by contract-firms in column (10), the 1992-1999 change in that share in column (11), and the p-value on the Chow test detecting a trend break in that share after 1993.

Table A.5: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331

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

Notes: This table shows quotes from interviews concerning regional labor courts' interpretation of the legality of outsourcing prior to Súmula 331, from interviewees who worked in restrictive regions. Source 1 in Column (5) is interviews conducted by the authors, whereas Source 2 is transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table A.5: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331 (Cont.)

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

Notes: This table shows quotes from interviews concerning regional labor courts' interpretation of the legality of outsourcing prior to Súmula 331, from interviewees who worked in restrictive regions. Source 1 in Column (5) is interviews conducted by the authors, whereas Source 2 is transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table A.5: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331 (Cont.)

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

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Table A.5: Quotes from interviewees who worked in Restrictive regions prior to Súmula 331 (Cont.)

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

Notes: This table shows quotes from interviews concerning regional labor courts' interpretation of the legality of outsourcing prior to Súmula 331, from interviewees who worked in restrictive regions. Source 1 in Column (5) is interviews conducted by the authors, whereas Source 2 is transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table A.6: Quotes from interviewees who worked in Permissive regions prior to Súmula 331

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

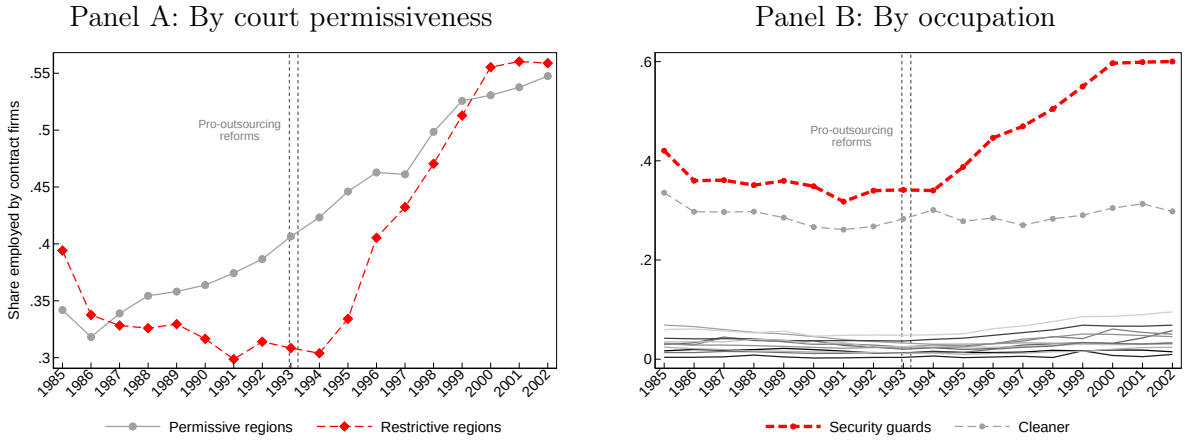
Notes: This table shows quotes from interviews concerning regional labor courts' interpretation of the legality of outsourcing prior to Súmula 331, from interviewees who worked in permissive regions. Source 1 in Column (5) is interviews conducted by the authors, whereas Source 2 is transcripts from interviews conducted by (now retired) Regional Labor Court Justice Magda Biavaschi and legal historian Alisson Droppa.

Table A.7: Variation in dissent over legality of outsourcing across regional labor court precedents appealed to the Superior Labor Court prior to Súmula 331

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

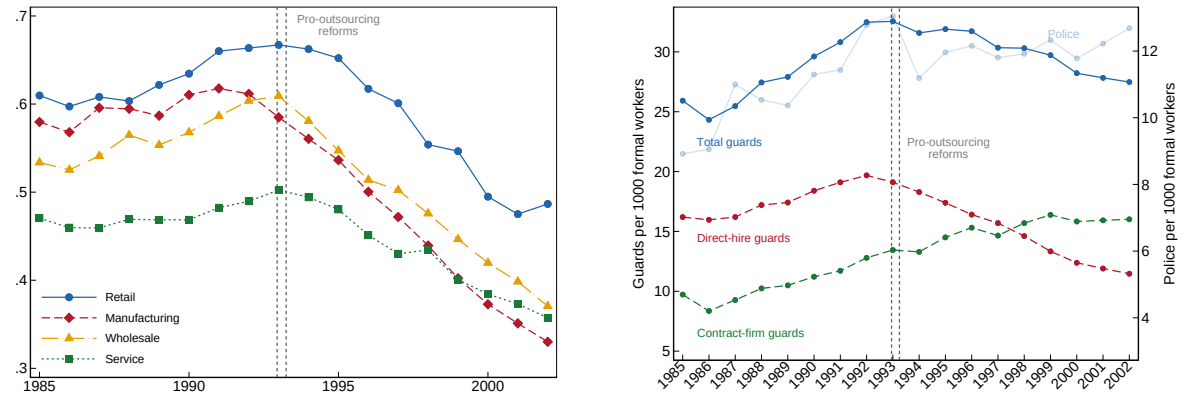
Notes: This table shows regional labor court rulings on outsourcing that were appealed to the Superior Labor Court prior to Súmula 331, including each regional court's finding on the direct employment link and whether the Superior Labor Court dissented. [1] The outsourcing firm appealed this case to the Superior Labor Court because the regional labor court found the outsourcing firm, Orbram Organizacao E Brambilla Ltda, jointly liable for damages the worker was suing for, despite having established a direct employment link with the end employer. The Superior Court found the outsourcing firm solely liable as it dissented from the regional labor court's decision on which firm had the direct employment link with the worker.

Figure A.1: Trends in contract-firm share



Note: This figure plots trends in contract-firm employment shares; the two dashed vertical lines mark Súmula 331 (December 1993) and Lei 8.863 (March 1994). 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 3 and tabulated in Appendix Table C.1.

Figure A.2: Trends in security guard employment



Note: Source: Felix and Wong (2026). This figure plots national trends in security guard employment; the two dashed vertical lines mark Súmula 331 (December 1993) and Lei 8.863 (March 1994). 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.

B Data

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

Entropy-balancing covariates. The entropy-balancing weights used in the market-level analysis balance restrictive and permissive microregions on baseline values of log total formal employment, the homicide rate, the unemployment rate, the share of employment in tradeable industries, and import competition exposure. Data on import competition exposure and the tradeable employment share are from Felix (2026); homicide rate data are from Dix-Carneiro et al. (2018); the unemployment rate is computed from the 1991 Census; and total formal employment is computed from RAIS.

RAIS sample restrictions. Our sample includes all workers between the ages of 18 and 64 who were employed as of December 31 of each year. If a worker is seen working for more than one firm, we keep the employment link with the highest-paying firm, and drop employment records with missing earnings data. We drop workers in public administration and those without valid information on their industry of employment. We also drop two government occupation groups—diplomats and civil servants (CBO94 two-digit codes `22` and `31`)—and, within the protection-and-security group, the government-staffed occupations described below. For our analysis of local labor markets for security guards, we restrict our sample to the large occupational groups listed in Table C.1—the two-digit groups with at least two workers in every included market and year—and only include local labor markets with at least 30 security guards and 30 cleaners in every year between 1985-2002, yielding an analysis sample of 262 local labor markets, covering 98 percent of all security guards. These restrictions yield a set of microregions with similar occupational composition and in which

contract firms could in principle have operated, given the underlying demand for guards and cleaners throughout the period. Given the occupation's high contract-firm share prior to the reforms despite the lack of law bindingness (Appendix Figure A.1), potentially driven by high levels of informality, we exclude cleaners from estimation of treatment effects for security guards.

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

RAIS measurement of outsourcing. We use the 1994 *Código Brasileiro de Ocupações* (CBO 94) at the two-digit level to define occupations, which is consistent with predecessor (CBO) occupational codes; the construction of the security-guard treatment group and the occupation aggregation we apply are detailed in the two paragraphs below. We identify whether an establishment in the RAIS dataset is part of a contract firm based on the establishment's economic activity code, which follows the *Classificação Nacional de Atividades Econômicas* (CNAE) system. We identified contract firms as those with CNAE95 numbers 74608 ("Atividades de investigação, vigilância e segurança"), 74160 ("Atividades de assessoria em gestão empresarial"), 74500 ("Seleção, agenciamento e locação de mão-de-obra"), 74705 ("Atividades de limpeza em prédios e domicílios"), and 74993 ("Outras atividades de serviços prestados principalmente às empresas"), which fall under industry class 74 "Serviços prestados principalmente às empresas." For firms that exited prior to 1995 and therefore do not have a CNAE code, we used a concordance between "IBGESUBATIVIDADE" codes (reported prior to 1995) and CNAE codes, constructed using firms that are present in the data before and after 1995 and report the former code before, the latter after.

Defining the security-guard occupation. We define the treated occupation—private-sector security guards—using CBO94. The two-digit group 58 ("Trabalhadores de serviços de proteção e segurança") bundles five three-digit subgroups, only one of which is private security, so we restrict group 58 to the private-sector-eligible protection occupations and drop those staffed by government. We keep CBO94 codes 58320 ("guarda de segurança"), 58330 ("vigia"), 58340 ("agente de segurança de aeroporto," an outsourceable activity), 58390 ("outros guardas de segurança"), and the residual 58990 ("outros trabalhadores de proteção e segurança n.c."). We drop firefighters (subgroup 581), police and detectives (582), municipal traffic guards (584), prison guards (58930), and lifeguards (58950, who in Brazil are typically members of the *Corpo de Bombeiros*), all of which are public-sector occupations not affected by the outsourcing reforms. The restriction is applied at the worker level, before any aggregation, so the two-digit security cell used throughout the analysis contains only private-sector security workers; private security accounts for 85–93% of group-58 employment in every sample year.

The cleaner group. The excluded cleaner comparison group is the two-digit CBO94 group 55 ("Trabalhadores de serviços de administração, conservação e limpeza de edifícios e logradouros públicos"), which bundles building-administration and cleaning occupations in a single two-digit code; unlike the security group, we apply no within-group restriction. Its three-digit subgroup 551 covers building administration and services: building managers (55115), caretakers (55120, *zeladores*), doormen (55125, *porteiros*), garage attendants (55135), sacristans (55140), elevator operators (55150, *ascensoristas*), and a residual category (55190). Subgroup 552 covers cleaning: cleaners (55220, *faxineiros*), window cleaners (55230), street sweepers (55250, *garis*), garbage collectors (55260, *lixeiros*), and a residual category (55290). In the Census analysis the corresponding codes are 804 and 844 (1991) and 5142 (2000).

Occupation aggregation. We measure occupations with CBO94 at the two-digit level. To keep occupation cells comparable over time we make two adjustments using the official RAIS occupation dictionary. First, 187 five-digit codes that were phased in or out over the

sample period, and are therefore not consistently reported across years, are assigned to a residual category (code 99). Second, whose definitions are very similar and whose counts shift across the boundary between them over time: medical and nursing occupations (6 into 7), teaching occupations (13 into 14), sales and commerce occupations (43 and 49 into 45), housekeeping, food-service and attendant occupations (52 and 53 into 54), and agricultural occupations (61–66 into 67). The control occupations in the triple-difference design are therefore consistently defined two-digit groups, parallel to the two-digit security treatment group.

Table B.1: Descriptive statistics of security guards by contract type

	Direct hire						Contract firm					
	(1) 85–93		(2) 94–96		(3) 97–02		(4) 85–93		(5) 94–96		(6) 97–02	
Male	0.98	(0.13)	0.98	(0.15)	0.97	(0.17)	0.98	(0.13)	0.97	(0.17)	0.96	(0.19)
Years of schooling	4.88	(2.86)	5.35	(3.09)	6.18	(3.38)	5.10	(2.23)	5.88	(2.55)	6.83	(2.84)
Age	39.62	(11.58)	40.16	(11.48)	39.97	(11.31)	34.13	(9.92)	34.02	(9.02)	34.59	(8.50)
CLT urban indeterminate contract	0.98	(0.13)	0.95	(0.22)	0.96	(0.21)	0.99	(0.09)	0.98	(0.13)	0.99	(0.11)
Tenure	2.46	(4.63)	2.82	(4.86)	2.91	(4.81)	1.51	(3.26)	1.50	(3.37)	1.66	(3.27)
New hire (tenure $<$ 1 year)	0.46	(0.50)	0.43	(0.50)	0.41	(0.49)	0.52	(0.50)	0.54	(0.50)	0.48	(0.50)
Long-tenured (tenure $\geq$ 10 years)	0.08	(0.27)	0.09	(0.29)	0.10	(0.30)	0.04	(0.19)	0.03	(0.18)	0.04	(0.19)
Real monthly earnings (2017 R$)	1994.54	(1612.45)	1753.45	(1476.20)	1693.68	(1381.73)	1481.18	(822.43)	1573.06	(813.93)	1665.08	(795.56)
Contract hours	.	(.)	42.38	(4.45)	42.42	(4.31)	.	(.)	43.53	(2.83)	43.67	(2.07)
Real hourly wage (2017 R$)	.	(.)	43.46	(55.95)	41.85	(49.30)	.	(.)	37.11	(34.61)	38.38	(23.24)
Exit from formality	0.33	(0.47)	0.33	(0.47)	0.41	(0.49)	0.35	(0.48)	0.35	(0.48)	0.43	(0.49)
Occupation switching	0.06	(0.24)	0.05	(0.22)	0.04	(0.20)	0.03	(0.17)	0.02	(0.15)	0.02	(0.12)
Managerial promotion	0.00	(0.05)	0.00	(0.04)	0.00	(0.04)	0.00	(0.03)	0.00	(0.04)	0.00	(0.02)
Employer size, mean	886		690		566		1019		950		1696	
Employer size, median	147		83		59		582		459		478	
Number of guards at employer, mean	65		63		52		817		777		774	
Number of guards at employer, median	7		5		4		488		422		430	

Notes: This table shows means (standard deviations in parentheses) of worker- and job-level characteristics for private-sector security guards aged 18–64 in RAIS, separately for direct hires and contract-firm employees (workers at establishments with contract-firm industry codes; see Appendix B), in three periods: 1985–1993, 1994–1996, and 1997–2002. Monthly earnings and hourly wages are expressed in 2017 reais; contract hours are reported in RAIS only from 1995 onward. Exit from formality indicates that the worker does not appear in RAIS in the following year; occupation switching indicates a change of two-digit occupation in the following year, and managerial promotion indicates holding a managerial occupation code in the following year; both are coded zero for workers who do not appear in RAIS in the following year. 1997–2002 columns use the estimation sample of the outsourcing wage differential regressions, which excludes workers with imputed age. The final rows, set off by the dashed separator, describe the workers' employers, reporting the mean and the median of employer size (total employment at the establishment) and of the number of security guards employed there.

Census data. We complement RAIS with the 1991 and 2000 Brazilian Demographic Censuses, using the extracts assembled by Dix-Carneiro and Kovak (2017). Unlike RAIS, the Census covers formal and informal workers alike. We classify an employed respondent as a formal wage worker if they report employment with a signed work card (*carteira assinada*) and as an informal wage worker if they report wage employment without a signed card; self-employment is reported as its own Census category. "Informal employment" pools informal wage work and self-employment. We keep respondents aged 18–64, excluding public administration; descriptive statistics additionally condition on employment and non-missing earnings, while the triple-difference sample below keeps all labor-force participants (employed or unemployed). The Census reports monthly earnings in the main job as multiples of the contemporaneous minimum wage; we convert earnings to 2000 reais using the July 1991 minimum wage expressed in 2000 R$ (R$126.51) and the 2000 minimum wage (R$151), and trim earnings below the 1st and above the 99th percentile within year before taking logs. All statistics use Census person weights, and municipalities are aggregated to the same time-consistent microregions as in the RAIS analysis, following Kovak (2013).

The two censuses code occupations under different household classifications, neither of which is the CBO94 system used in RAIS: the 1991 Census uses the three-digit 1991 Census occupation code (`cbodom91`), and the 2000 Census uses the four-digit CBO-Domiciliar code (`cbodom00`); the mapping between the two systems is not one-to-one. We define security guards as codes `843` (*vigias*) and `869` (*guardas vigias de organizações particulares*) in 1991, and as codes `5173` (*vigilantes e guardas de segurança*) and `5174` (*guardas e vigias*, which includes porters) in 2000. Cleaners are codes `804` (*faxineiras*) and `844` (*serventes*) in 1991 and code `5142` (building maintenance and conservation workers) in 2000; Table B.2 reports each of these codes as separate rows. year-specific Census codes into the guard treatment group and into the RAIS control-occupation groups of Table C.1 via a year-specific crosswalk (ten of the eleven RAIS two-digit control groups can be matched in the Census coding; cleaners are excluded), and we define fine-grain occupation cells using each year's two-digit code (the first two digits of `cbodom91` and of `cbodom00`). The crosswalk is as follows (CBO-94 control group—Census codes [1991; 2000]): 03 Technicians [1991: 112, 131, 133, 163, 168, 183, 191, 192, 302, 401, 402, 403, 405, 406; 2000: 3000–3999]; 32 Secretaries and typists

[1991: 56, 59; 2000: 2523, 4121]; 33 Cashiers and tellers [1991: 53, 603; 2000: 4211, 4212]; 39 Office administration [1991: 40, 52, 60, 63, 64; 2000: 4110, 4122, 4131, 4132, 4142, 4151, 4221]; 45 Salesmen [1991: 602, 621; 2000: 3541, 5201, 5211, 5241, 5242, 5243]; 54 Attendants [1991: 812, 813, 814, 815, 818, 821, 822; 2000: 5132, 5134, 5135, 5151, 5162]; 77 Food and beverage processing [1991: 535, 537, 539, 545; 2000: 7842, 8401, 8411, 8412, 8413, 8421, 8482, 8491, 8492, 8493]; 84 Machine installers and mechanics [1991: 419, 420, 422, 423, 424, 425; 2000: 7250, 7251, 7411, 9111, 9112, 9113, 9131, 9141, 9142, 9143, 9144, 9193, 9503]; 85 Electricians and electronics workers [1991: 502, 503, 506; 2000: 7156, 7301, 7311, 7321, 8611, 8612, 9501, 9502, 9511, 9531]; and 98 Drivers, sailors, and conductors [1991: 725, 742, 751, 752; 2000: 5112, 7820, 7823, 7824, 7825, 7826, 7828]. The remaining RAIS control group (99, Other manual occupations) has no clean Census analog and is excluded. Because the 1991 and 2000 coding systems differ, all specifications include occupation-by-year fixed effects, which absorb any year-specific coding differences that are common across regions.

Census DDD specification. To corroborate the RAIS triple difference with data that cover informal work, we aggregate the Census to microregion × occupation × year cells and estimate

$$y_{omt} = \beta \left(\text{Guard}_o \times \text{Restrictive}_m \times 1_{t=2000}\right) + \delta_{om} + \delta_{ot} + \delta_{mt} + \epsilon_{omt}, \tag{4}$$

where y_{omt} is, in turn, log survey-weighted formal employment, log informal employment, the weighted mean of log monthly earnings conditional on formal employment, and the weighted mean of log monthly earnings conditional on informal employment, in occupation o, microregion m, and year $t \in \{1991, 2000\}$. The fixed effects mirror equation (1): δ_{ot} are two-digit occupation-by-year effects, δ_{mt} are microregion-by-year effects, and δ_{om} are occupation-group-by-microregion effects, which absorb the $\text{Guard}_o \times \text{Restrictive}_m$ interaction, so that only the triple-difference coefficient β is identified. The two fixed effects use different occupation classifications by necessity: the two-digit codes are the finest classification within each Census but are not comparable across the 1991 and 2000 coding systems, so the time-invariant δ_{om} is instead defined at the occupation-group level—the finest classification that

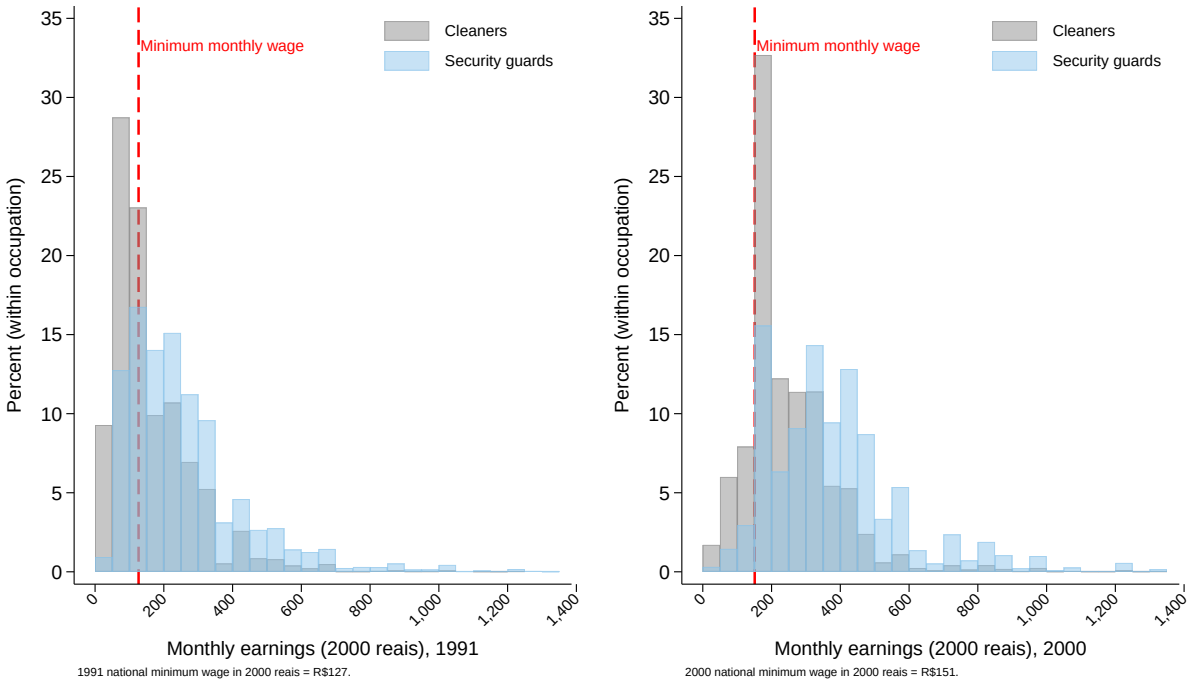
is consistent across the two Censuses, and the level at which the guard treatment group itself is defined. Regressions are weighted by the same entropy-balancing microregion weights as the RAIS DDD, and standard errors are clustered by microregion. Estimates are reported in Appendix Table C.7 and, separately by 1991 age cohort, in Appendix Table C.8.

Table B.2: Employment composition and monthly earnings by occupation, 1991 and 2000 Censuses

				Employment shares		Demographics		Wage percentiles (mult. min wage)				
Year	Code	Occupation label	Tot. emp.	Formal	Informal	% Male	Educ (yrs)	p10	p25	p50	p75	p90
Panel A: All workers (aged 18–64, employed, non-missing wages, excl. public admin.)												
1991	–	All workers	42,370,596	0.413	0.587	70.9	5.39	0.46	0.69	1.38	2.77	5.53
2000	–	All workers	52,447,250	0.390	0.610	62.3	7.10	0.79	1.13	1.99	3.97	7.95
Panel B: Plausible cleaning-related occupation codes												
1991	804	Faxineiras	241,830	0.000	1.000	4.6	3.52	0.41	0.58	1.05	1.66	2.65
1991	844	Serventes	592,676	0.867	0.133	40.7	3.63	0.47	0.64	1.11	1.52	2.21
2000	5142	Trab. manut. e conservacao de edificios	1,067,923	0.615	0.385	42.5	4.69	0.86	1.00	1.40	1.99	2.65
Panel C: Plausible security-guard-related occupation codes												
1991	843	Vigias	398,238	0.866	0.134	98.6	3.96	0.64	1.04	1.66	2.49	3.59
1991	869	Guardas vigias de organizacoes particulares	261,140	0.921	0.079	98.2	5.53	0.83	1.22	1.94	2.77	4.15
2000	5173	Vigilantes e guardas de seguranca	627,508	0.756	0.244	97.5	6.58	1.00	1.72	2.52	3.31	4.64
2000	5174	Guardas e vigias (incl. porteiros)	461,503	0.747	0.253	96.2	5.26	1.00	1.39	1.99	2.74	3.97

Notes. This table shows total employment, formal and informal employment shares, demographics, and wage percentiles for cleaner- and guard-related occupation codes in the 1991 and 2000 Censuses. Sample: workers aged 18–64, employed, with non-missing monthly wages, excluding public administration. Earnings expressed in 2000 R$; wage percentiles computed from `mw_ymain` (monthly earnings as multiples of that year's minimum wage), trimmed at the 1st/99th percentiles.

Figure B.1: Monthly earnings distribution for cleaners and security guards, 1991 and 2000



Notes. This figure plots overlaid histograms of monthly earnings for cleaners and for security guards, separately for the 1991 and 2000 Censuses. Monthly earnings in 2000 R$. The vertical dashed line in each panel marks that Census year's national minimum wage in 2000 R$ (R$126.51 for 1991; R$151 for 2000).

C DDD Appendix Tables and Figures

Table C.1: Occupations present in all microregions (inclusion criterion for DDD)

Occupation	Contract-firm share (1)	Mean log wage (2)	Mean schooling (3)	Male (4)	National employment (5)
Security guards	0.394	6.909	4.41	0.988	479,644
Drivers, sailors, conductors	0.022	7.079	5.35	0.996	689,211
Machine installers and mechanics	0.020	7.187	5.81	0.989	377,652
Electricians and electronics workers	0.033	7.504	6.52	0.971	265,185
Technicians	0.036	7.823	9.79	0.875	384,875
Food and beverage processing workers	0.004	6.693	4.90	0.842	307,701
Other manual or uncommon occupations	0.054	6.640	4.99	0.826	1,470,967
Salesmen	0.014	6.511	7.63	0.655	1,099,606
Office administration	0.055	7.393	9.53	0.585	1,845,952
Cashiers and tellers	0.036	7.493	10.09	0.505	481,410
Attendants	0.029	6.492	5.00	0.314	466,502
Secretaries and typists	0.056	6.795	10.17	0.154	167,417
Cleaners (excluded)	0.310	6.489	4.47	0.423	746,136
National	0.060	7.136	7.49	0.744	14,773,995

Notes: This table shows the two-digit CBO94 occupation groups that satisfy the DDD inclusion criterion (at least two workers in every sample microregion in every year; see Section 3.1), with their 1992 characteristics: the share of the occupation's employment at contract firms, mean log real December earnings, mean years of schooling, male share, and national formal employment. Security guards are listed first; the remaining occupations are ordered by male share (column (4)). Cleaners, separated by the dotted line, satisfy the inclusion criterion but are excluded from estimation, given their high baseline outsourcing prevalence and informality. Each row is one two-digit CBO94 group; Appendix B details the specific occupations in the security-guard and cleaner groups. The "National" row reports the corresponding totals and means across all occupations in RAIS.

Table C.2: Baseline descriptive statistics of analyzed microregions by regional labor court restrictiveness

	(1) Restrictive		(2) Permissive	
Outsourced share of guards	0.240	(0.207)	0.373	(0.169)
Establishments directly hiring guards	813.680	(742.248)	5460.065	(5436.288)
Employment HHI	0.041	(0.064)	0.037	(0.061)
Average log(wage), guards	7.375	(0.188)	7.307	(0.317)
Guards per 1000 formal workers	25.632	(21.665)	35.582	(12.500)
Direct-hire guards	16.960	(5.829)	20.890	(5.069)
Contract-firm guards	8.672	(18.315)	14.692	(9.821)
Age 18–24	2.164	(3.274)	4.016	(1.947)
Age 25–29	4.263	(6.271)	7.714	(3.547)
Age 30–39	7.590	(7.343)	11.432	(4.231)
Age 40–49	5.866	(3.322)	6.813	(2.302)
Age 50–64	5.750	(2.143)	5.608	(2.173)

Notes: This table shows 1992 means (standard deviations in parentheses) of microregion-level characteristics, computed from the guard cells of the estimation sample, separately for microregions under restrictive regional labor courts (jurisdictions 4, 9, 12, and 15) and under permissive courts (the remaining 20 jurisdictions). Means are weighted by microregion formal employment. Guards per 1,000 formal workers and its decompositions are expressed relative to total microregion formal employment. The sample includes all microregions with at least 30 guards and 30 cleaners in every year.

Table C.3: Robustness to balancing baseline covariates: Effect of pro-outsourcing reforms on employment composition

	Policy effect (DDD)				
	Main	+Informality	No unemp. bal.	No trade bal.	Obs.
	(1)	(2)	(3)	(4)	(5)
Log employment by worker age group					
18–24	0.351***	0.243***	0.317***	0.285***	56,592
	(0.032)	(0.028)	(0.022)	(0.025)	
25–29	0.150***	0.193***	0.149***	0.092***	56,592
	(0.027)	(0.019)	(0.024)	(0.021)	
30–39	0.046*	0.053	0.019	0.011	56,592
	(0.022)	(0.036)	(0.025)	(0.023)	
40–49	-0.048	-0.074	-0.109***	-0.081**	56,592
	(0.031)	(0.045)	(0.027)	(0.032)	
50–64	-0.132***	-0.158***	-0.193***	-0.134***	56,592
	(0.022)	(0.037)	(0.006)	(0.019)	

Notes: This table shows the effect of the pro-outsourcing reforms on employment composition, estimated with the weighted DDD specification under alternative sets of balancing baseline covariates. See notes to Table 1. As in Table 1, employment outcomes are inverse hyperbolic sine transforms of employment counts.

Table C.4: Robustness of DDD employment effects to the valuation of zero-employment cells

	Main	Chen–Roth corrections				
	Arcsinh	Calibrated log, $m(0) = -x$			Extensive	
	DDD	$x = 0$	$x = 1$	$x = 3$	$1\{Y > 0\}$	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Log total employment	0.038***	0.038***	0.038***	0.038***	0.000	56,592
	(0.011)	(0.011)	(0.011)	(0.011)	(0.000)	
Log employment by worker age group						
18–24	0.351***	0.356***	0.355***	0.352***	-0.001	56,592
	(0.032)	(0.032)	(0.032)	(0.034)	(0.001)	
25–29	0.150***	0.152***	0.149***	0.144***	-0.002*	56,592
	(0.027)	(0.027)	(0.027)	(0.028)	(0.001)	
30–39	0.046*	0.046*	0.046*	0.046*	0.000	56,592
	(0.022)	(0.022)	(0.022)	(0.022)	(0.000)	
40–49	-0.048	-0.050	-0.047	-0.042	0.003	56,592
	(0.031)	(0.032)	(0.031)	(0.029)	(0.002)	
50–64	-0.132***	-0.135***	-0.130***	-0.122***	0.004***	56,592
	(0.022)	(0.020)	(0.022)	(0.024)	(0.000)	

Notes: This table re-estimates the pooled DDD effects on log total employment and log employment by age group under alternative treatments of cells with zero employment, following Chen and Roth (2024). All columns use the weighted DDD specification of Table 1 (entropy balancing weights, the same fixed effects and sample, standard errors two-way clustered by regional labor court and occupation). Column (1) reports the main specification, in which outcomes are inverse hyperbolic sine transforms of employment counts, used because employment is zero in a small share of occupation-microregion-year cells. Columns (2)–(4) instead use $m_x(Y) = \ln Y$ for $Y \geq 1$ and $m_x(0) = -x$, so that a transition from zero to one worker is explicitly valued at $100x$ log points, for $x \in \{0, 1, 3\}$. Column (5) reports the DDD effect on an indicator for positive employment, estimated by OLS. Column (6) reports observations. Total employment has no zero-employment cells in the estimating sample by construction. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Robustness to excluding São Paulo: Effect of pro-outsourcing reforms on key outcomes

	Policy effect (DDD)		
	Main	Excl. SP metro	Excl. SP state
	(1)	(2)	(3)
Outsourced share	0.045***	0.048***	0.021
	(0.013)	(0.013)	(0.016)
Log total employment	0.038***	0.041***	0.024*
	(0.011)	(0.011)	(0.011)
Log employment 18–24	0.351***	0.358***	0.289***
	(0.032)	(0.033)	(0.041)
Log employment 50–64	-0.132***	-0.131***	-0.110***
	(0.022)	(0.023)	(0.025)
Log earnings (cond. on demographics)	0.011	0.001	0.041***
	(0.015)	(0.016)	(0.010)

Notes: This table shows the effect of the pro-outsourcing reforms on key outcomes when São Paulo observations are excluded from the sample. See notes to Table 3. Columns report the weighted DDD estimate for the main sample, excluding the São Paulo metropolitan area, and excluding the state of São Paulo.

Table C.6: Effect of pro-outsourcing reforms on job tenure

	Baseline treated mean	Policy effect (DDD)			
	(Guards in Restrictive)	EB	IPW	EB+RA	Obs.
	(1)	(2)	(3)	(4)	(5)
Tenure	3.113	-0.099	-0.154*	-0.077	56,592
		(0.060)	(0.070)	(0.059)	
Direct-hire tenure	3.225	0.107**	0.084	0.117*	56,592
		(0.047)	(0.060)	(0.056)	
Contract-firm tenure	2.153	-0.126	-0.012	-0.113	43,370
		(0.149)	(0.100)	(0.139)	
New-hire share (tenure $<$ 1 year)	0.274	0.014**	0.015**	0.011**	56,592
		(0.005)	(0.006)	(0.004)	

Notes: This table shows the effect of the pro-outsourcing reforms on average job tenure (overall and by contract type) and on the share of new hires (tenure below one year), estimated with the weighted DDD specification. See notes to Table 1.

Table C.7: DDD on Census cell-level outcomes, 1991–2000

	Log formal employment	Log informal employment	Log formal earnings	Log informal earnings
Guard × Restrictive × Post-1993	0.1699***	0.0762	-0.0289	0.1110***
	(0.0579)	(0.0771)	(0.0224)	(0.0409)
Occupation (2-digit) × Year FE	Yes	Yes	Yes	Yes
Microregion × Year FE	Yes	Yes	Yes	Yes
Occupation group × Microregion FE	Yes	Yes	Yes	Yes
1991 mean: Guard, Restrictive	5.3286	3.4985	5.4108	5.0241
Cells (N)	43,543	42,543	43,495	41,901
Adj. R^2	0.525	0.437	0.539	0.503

Notes. Cells are (microregion x year x fine-grain occupation code). All regressions include two-digit-occupation x year, microregion x year, and occupation-group x microregion fixed effects. Two-digit occupation codes are year-specific (the 1991 and 2000 Censuses use different coding systems, which the occupation x year fixed effects absorb); occupation groups are the cross-year-consistent CBO-94 groups of Appendix Table C.1, and the occupation-group x microregion FE absorbs the Guard x Restrictive main-effect interaction, so only the triple-difference coefficient is identified and reported. Sample restricted to ages 18-64 in the labor force (employed or unemployed); public administration excluded. Outcomes: Log formal (informal) employment = log survey-weighted formal (informal) employment count; Log formal (informal) earnings = weighted mean log monthly earnings (R$ 2000) conditional on formal (informal) employment. The guard treatment codes and the year-specific crosswalk from Census occupation codes to the CBO-94 control groups are documented in Appendix B; cleaners are excluded throughout. All regressions are weighted by the microregion entropy-balancing weight (eweight2). The reported 1991 mean is the unweighted mean across guard cells in restrictive regions (the treated cells, where the entropy-balancing weights equal one), using the same sample as each column's regression. Standard errors are clustered at the microregion level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: DDD on Census cell-level outcomes by 1991-age cohort, 1991–2000

	Age cohort (in 1991)				
	C1 (18-24)	C2 (25-29)	C3 (30-39)	C4 (40-49)	C5 (50+)
Panel A: Log formal employment					
Guard × Restrictive × Post-1993	0.5175***	0.2172**	0.1801**	0.0481	-0.2010
	(0.1083)	(0.0887)	(0.0777)	(0.0829)	(0.1323)
1991 mean: Guard, Restrictive	3.3937	3.6989	4.1901	4.0711	4.2261
Cells (N)	30,366	26,789	29,637	19,235	8,704
Adj. R^2	0.504	0.498	0.485	0.505	0.545
Panel B: Log informal employment					
Guard × Restrictive × Post-1993	0.3982***	0.0552	0.0150	0.1598	0.1957*
	(0.1487)	(0.1430)	(0.1462)	(0.1231)	(0.1001)
1991 mean: Guard, Restrictive	2.6251	2.5869	2.7031	2.6902	3.0423
Cells (N)	29,422	24,872	28,304	21,512	13,362
Adj. R^2	0.406	0.393	0.417	0.437	0.449
Panel C: Log formal earnings					
Guard × Restrictive × Post-1993	0.0756	-0.0628	-0.0027	0.0219	0.0073
	(0.0477)	(0.0386)	(0.0358)	(0.0443)	(0.0555)
1991 mean: Guard, Restrictive	5.3494	5.5468	5.4887	5.4083	5.2872
Cells (N)	30,347	26,754	29,583	19,156	8,662
Adj. R^2	0.594	0.538	0.526	0.523	0.451
Panel D: Log informal earnings					
Guard × Restrictive × Post-1993	0.1200	-0.1241	0.0241	0.0753	0.0550
	(0.0880)	(0.0888)	(0.0824)	(0.0744)	(0.0725)
1991 mean: Guard, Restrictive	5.0454	5.2346	5.2088	5.1152	4.9348
Cells (N)	28,821	24,405	27,715	20,926	12,900
Adj. R^2	0.532	0.440	0.433	0.393	0.322

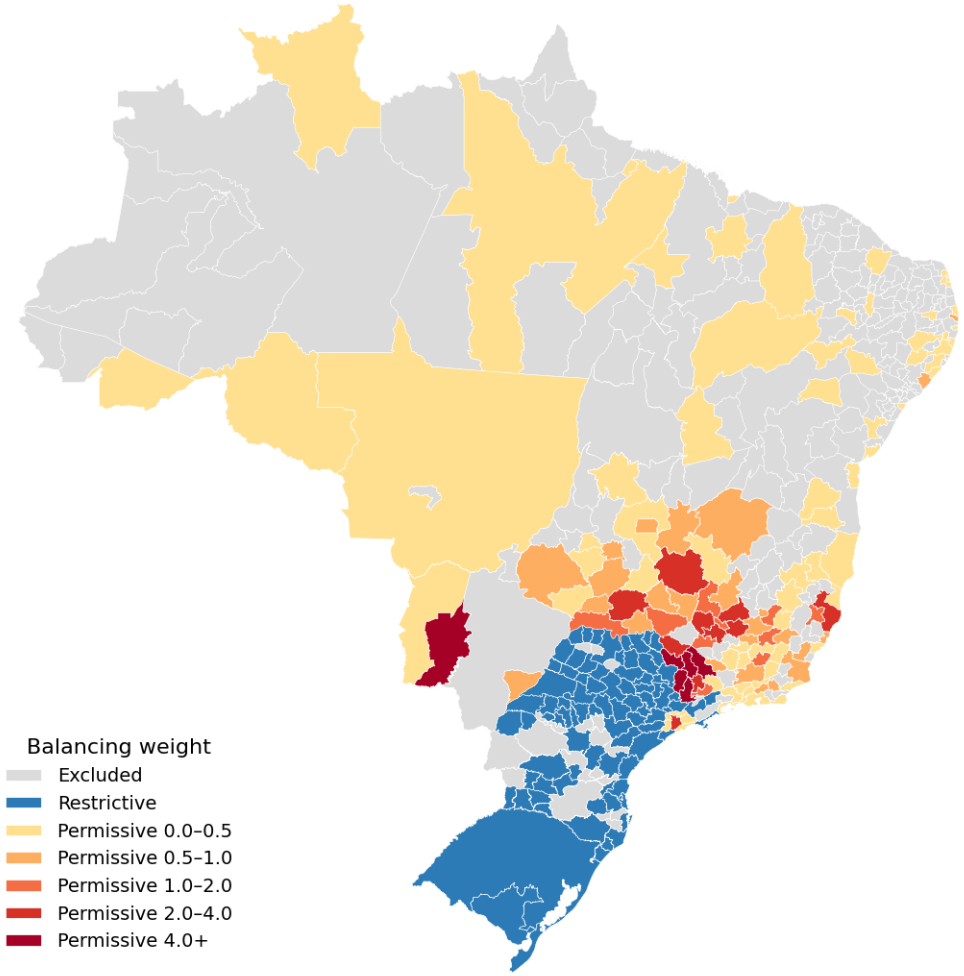
Notes. Cohorts (age in 1991, mirrors RAIS DDD age bins): C1 = 18-24 (27-33 in 2000); C2 = 25-29 (34-38 in 2000); C3 = 30-39 (39-48 in 2000); C4 = 40-49 (49-58 in 2000); C5 = 50+ (59-64 in 2000). Cells are (microregion x year x fine-grain occupation code) – the SAME cells as in the pooled main table – with cohort-stratified counts stored as side-by-side variables. All panels include two-digit-occupation x year, microregion x year, and occupation-group x microregion fixed effects (two-digit codes are year-specific; occupation groups are the cross-year-consistent CBO-94 groups of Appendix Table C.1); the occupation-group x microregion FE absorbs Guard x Restrictive, so only the triple-difference coefficient is identified. The guard treatment codes and the year-specific crosswalk from Census occupation codes to the CBO-94 control groups are documented in Appendix B; cleaners are excluded throughout. All regressions are weighted by the microregion entropy-balancing weight (eweight2). The reported 1991 mean is the unweighted mean across guard cells in restrictive regions (the treated cells, where the entropy-balancing weights equal one), using the same sample as each panel's regression. Standard errors are clustered at the microregion level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Characteristics of new hires as a function of occupation-specific and firm-level scale, with scale measured the year before hiring

	(1) Outside RAIS	(2) Ever formal	(3) Age	(4) Education	(5) Log wage
Log number of employees in occupation o					
at firm z in region m at year $t-1$	0.0030***	-0.0104***	-0.6736***	-0.0935***	-0.0236***
	(0.0002)	(0.0002)	(0.0050)	(0.0020)	(0.0004)
Log number of employees at firm z					
in region m at year $t-1$	-0.0175***	0.0077***	0.3242***	0.0524***	0.0362***
	(0.0005)	(0.0003)	(0.0073)	(0.0032)	(0.0008)
Occ×MMC×Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	11635985	11635985	11592336	11565755	11610714
Adj. R^2	0.142	0.208	0.303	0.620	0.712
Mean of outcome	0.7078	0.6700	29.5740	7.5819	7.0101

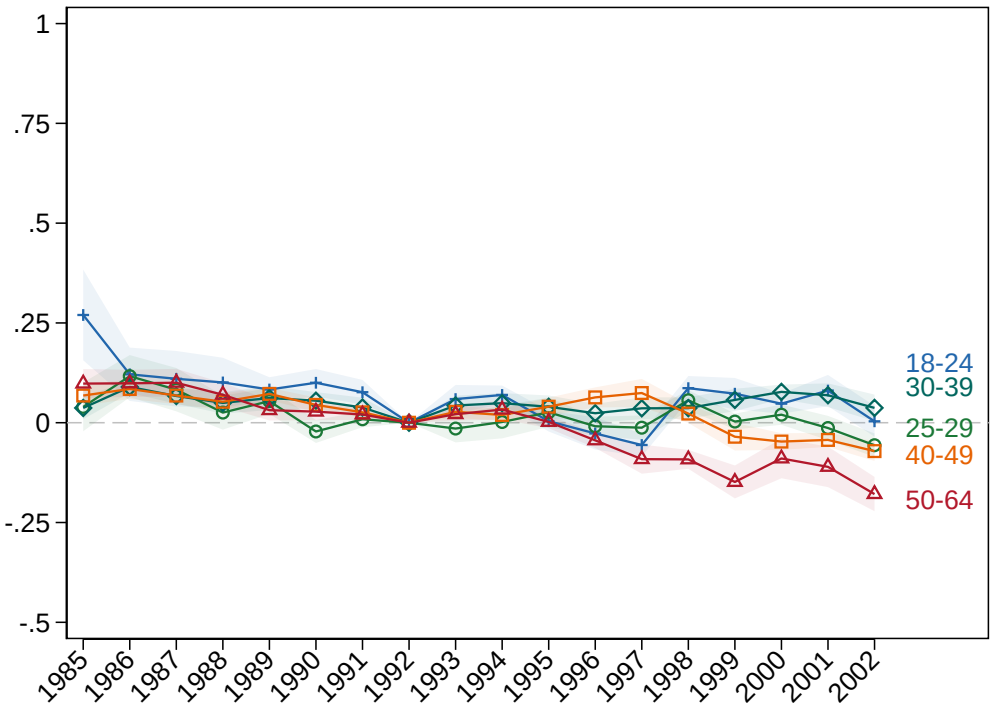
Notes. This table shows the all-occupations panel of Table 4 with the two scale regressors measured at the end of year $t-1$, before the year-t new hires arrive, rather than at the end of year t. The sample therefore includes only firm × occupation × microregion cells that also exist at $t-1$. All other details as in Table 4.

Figure C.1: Map of microregions with entropy-balancing weights



Note: This figure plots a map of the microregions used in our main specifications, where observations are weighted using entropy balancing weights. Entropy balancing weights are computed using 1992 values of log total formal employment, the homicide rate, the unemployment rate, the share of employment in tradeable industries, and import competition exposure.

Figure C.2: Effect of pro-outsourcing reforms on employment of direct-hire incumbent guards



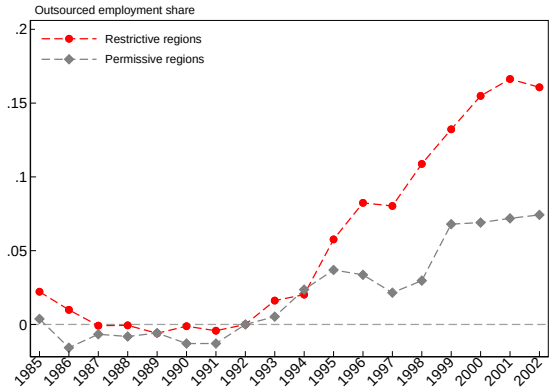
Note: This figure plots year-by-year DDD estimates of the effect of the pro-outsourcing reforms on log employment by age group among direct-hire incumbents. See notes to Figure 4. The sample includes all workers in guard and control group occupations who were directly employed in 1992.

Figure C.3: Differences in outsourcing prevalence: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

1. Outsourced employment share

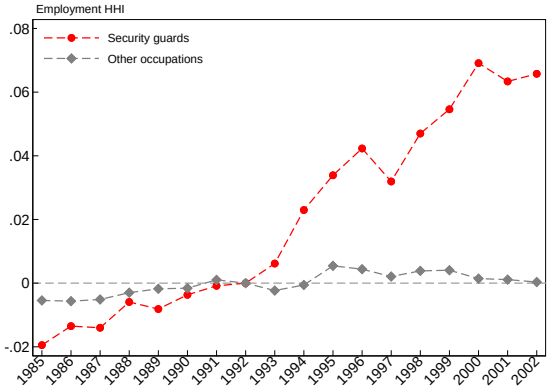
B.1: Cross-region DD

C.1: Cross-occupation DD

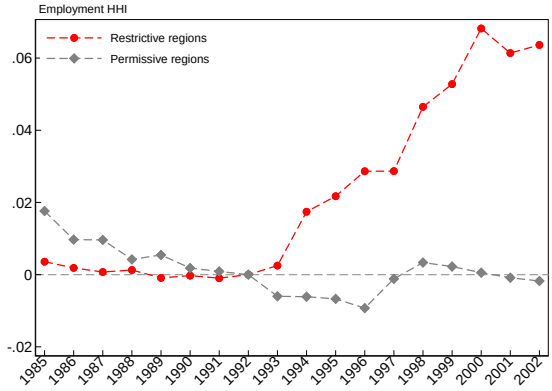


2. Employment Concentration (HHI)

B.2: Cross-region DD



C.2: Cross-occupation DD



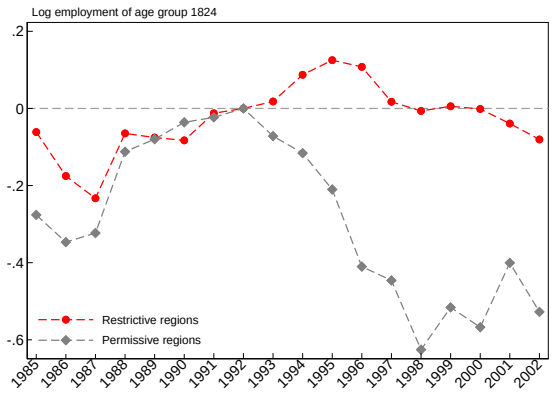
Note: This figure plots the two difference-in-differences series underlying the DDD estimates for outsourced employment share and employment HHI: the cross-region DD, i.e., the restrictive-versus-permissive difference, plotted separately by occupation (Panel B), and the cross-occupation DD, i.e., the guard-versus-other-occupations difference, plotted separately by region (Panel C); the specifications of both DD series are given in footnote 14. Panel B.1 repeats Figure 1 Panel B; Panel B.2 repeats Figure 2 Panel B. Panel C plots the α_τ coefficients from the cross-occupation DD regression, estimated separately for restrictive and permissive regions. Results in both panels are based on a balanced sample of 262 microregions and 12 large 2-digit occupational groups, with at least two workers in all microregions in all years, accounting for over 98% of all security guards in the data. See Appendix B for details. Legalization's baseline year of 1992 is omitted in both panels.

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

1. Log total employment of workers aged 18 to 24

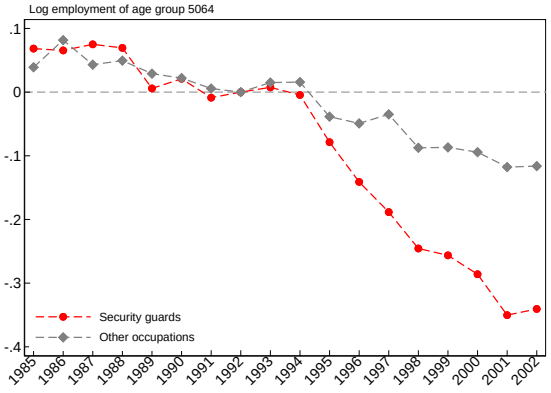
B.1: Cross-region DD

C.1: Cross-occupation DD

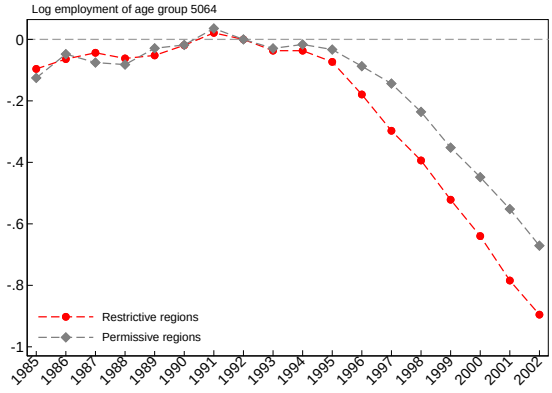


2. Log total employment of workers aged 50 to 64

B.2: Cross-region DD



C.2: Cross-occupation DD

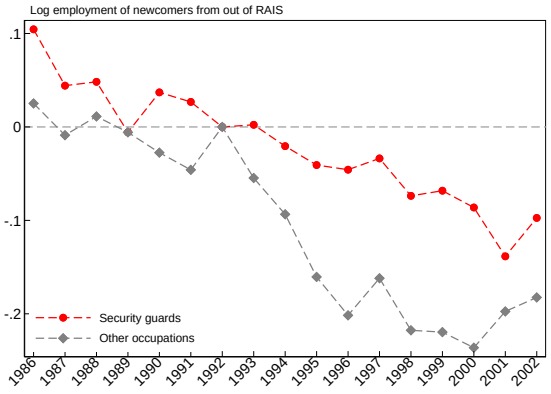


Note: This figure plots the cross-region DD series (restrictive versus permissive, by occupation; Panel B) and the cross-occupation DD series (guards versus other occupations, by region; Panel C) for log employment of workers aged 18–24 and of workers aged 50–64. See notes to Figure C.3.

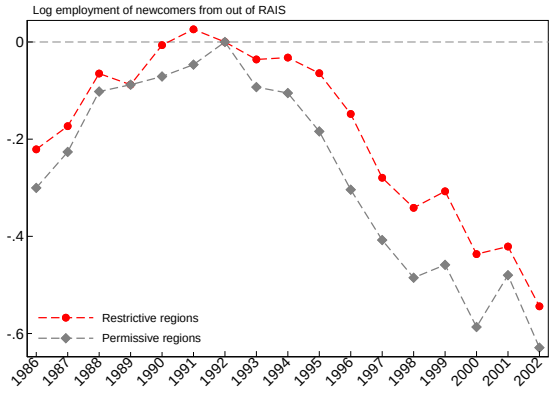
Figure C.5: Differences in employment: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

1. Log number of workers from unemployment or informality

B.1: Cross-region DD

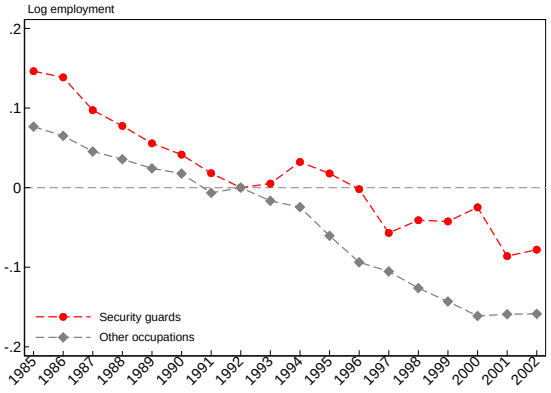


C.1: Cross-occupation DD

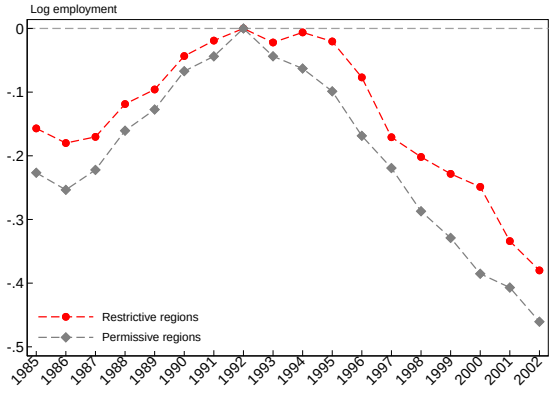


2. Log total employment

B.2: Cross-region DD



C.2: Cross-occupation DD



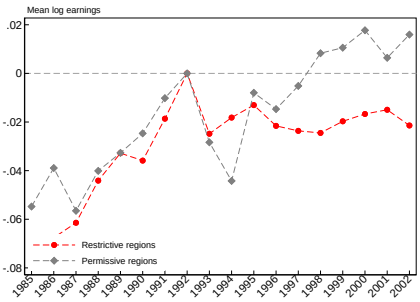
Note: This figure plots the cross-region DD series (restrictive versus permissive, by occupation; Panel B) and the cross-occupation DD series (guards versus other occupations, by region; Panel C) for the log number of workers from unemployment or informality and for log total employment. See notes to Figure C.3.

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

1. Log earnings (as reported)

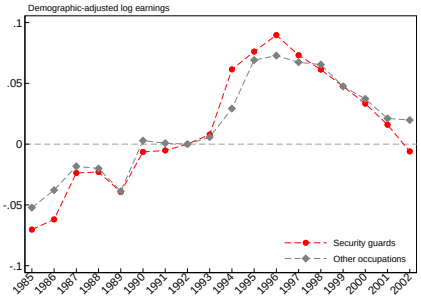
B.1: Cross-region DD

C.1: Cross-occupation DD

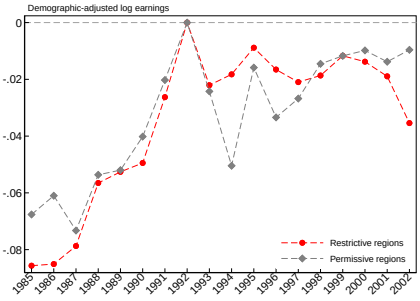


2. Log earnings conditional on demographics (age, education, gender)

B.2: Cross-region DD

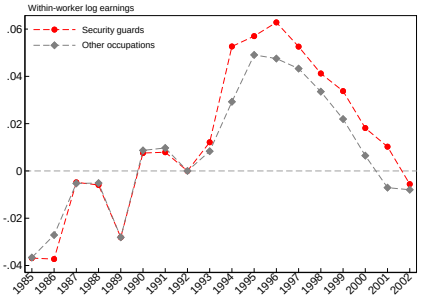


C.2: Cross-occupation DD

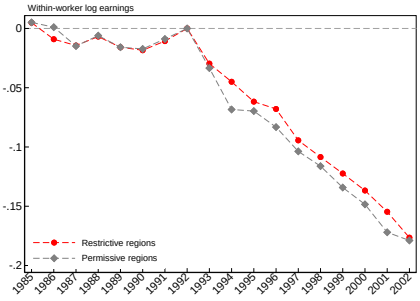


3. Log earnings conditional on worker fixed-effects and demographics

B.3: Cross-region DD

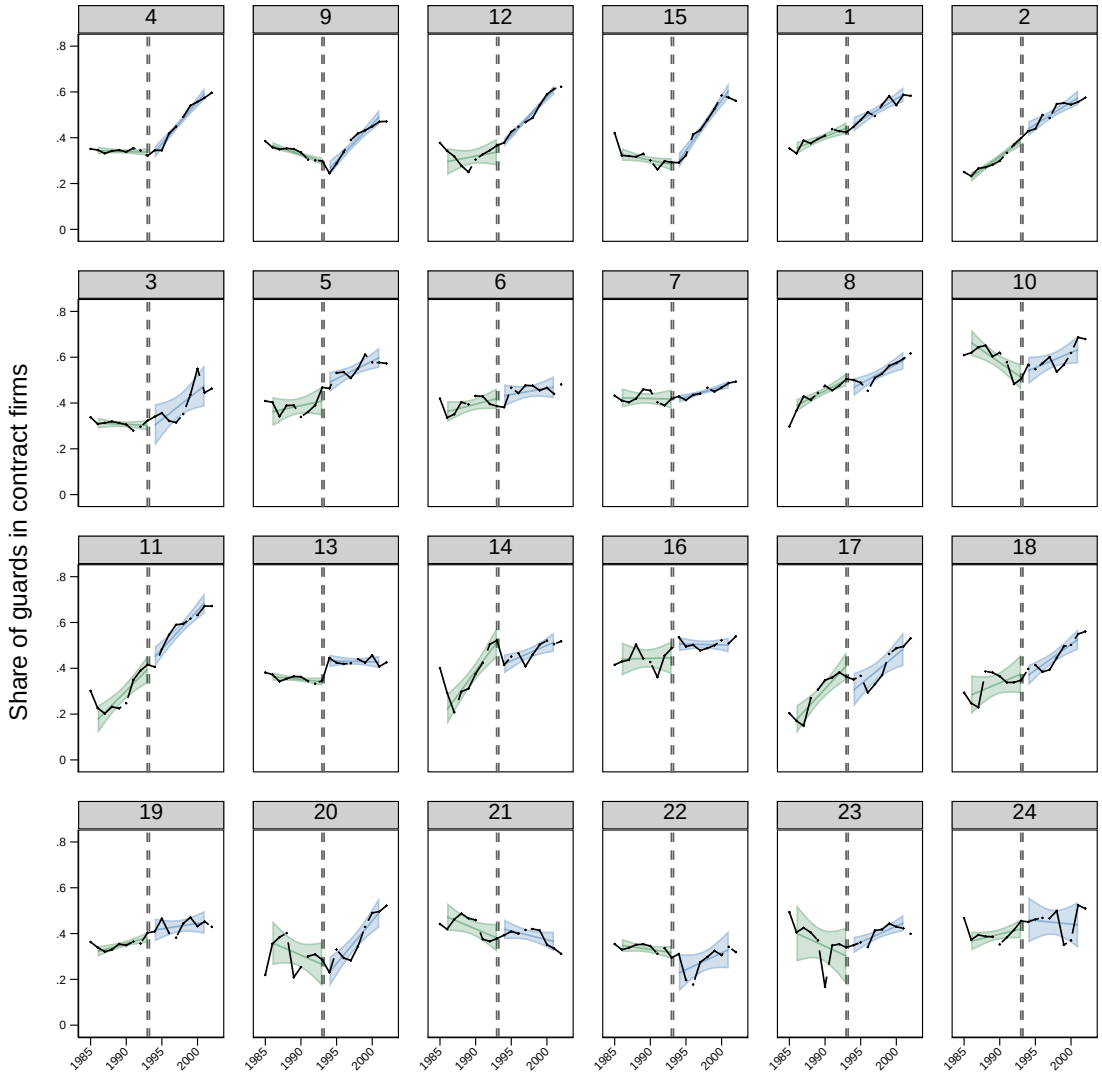


C.3: Cross-occupation DD



Note: This figure plots the cross-region DD series (restrictive versus permissive, by occupation; Panel B) and the cross-occupation DD series (guards versus other occupations, by region; Panel C) for log real December earnings, as reported and adjusted for demographics and worker fixed effects. See notes to Figure C.3. Outcomes are cell-level average log real December earnings for the microregion-occupation pair: as reported, without adjustment (row 1); demographic-adjusted, netting out gender × age-group × education-category fixed effects (row 2); and additionally netting out worker fixed effects (row 3). Adjusted outcomes are computed on the sample excluding public-sector workers.

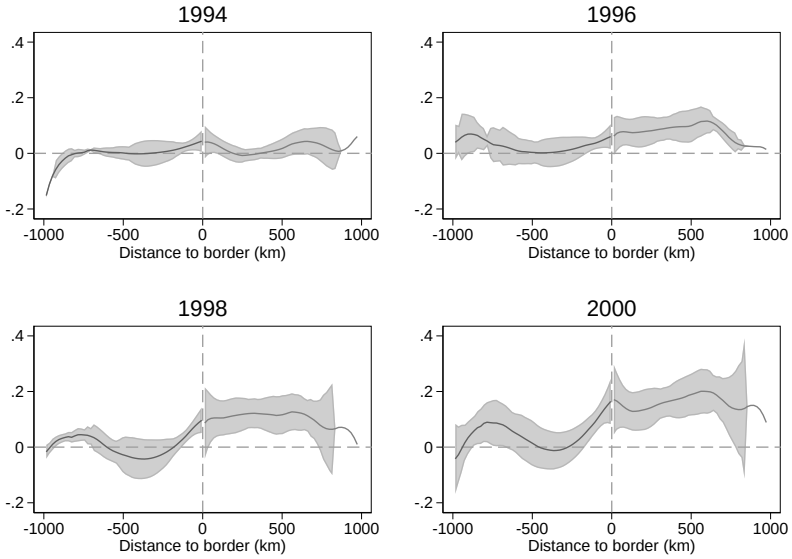
Figure C.7: Trends in security outsourcing, by regional labor court



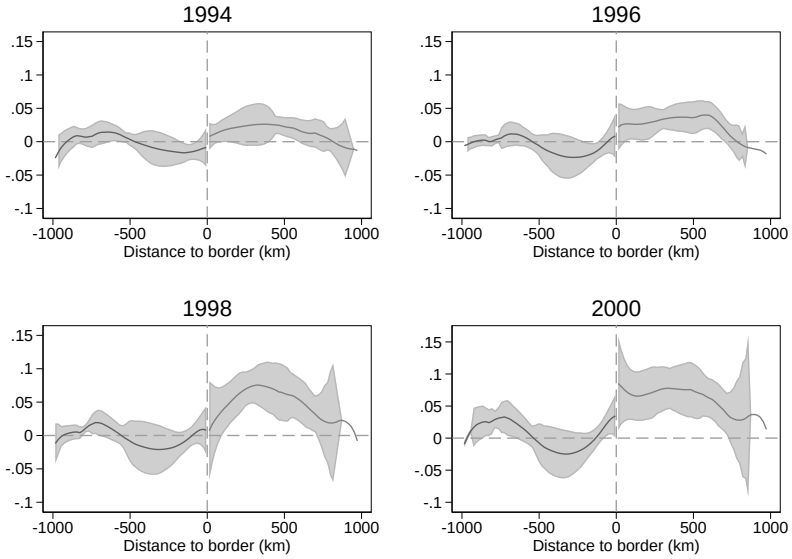
Note: This figure plots the trend in outsourcing prevalence, as measured by the share of security guards in the formal sector working for contract firms, separately for each jurisdiction of the 24 regional labor courts. Courts 4, 9, 12, and 15 are classified as restrictive. The two dashed vertical lines mark Súmula 331 (December 1993) and Lei 8.863 (March 1994).

Figure C.8: Changes in outsourcing prevalence and distance to the state border

Panel A: Relative change in contract-firm share



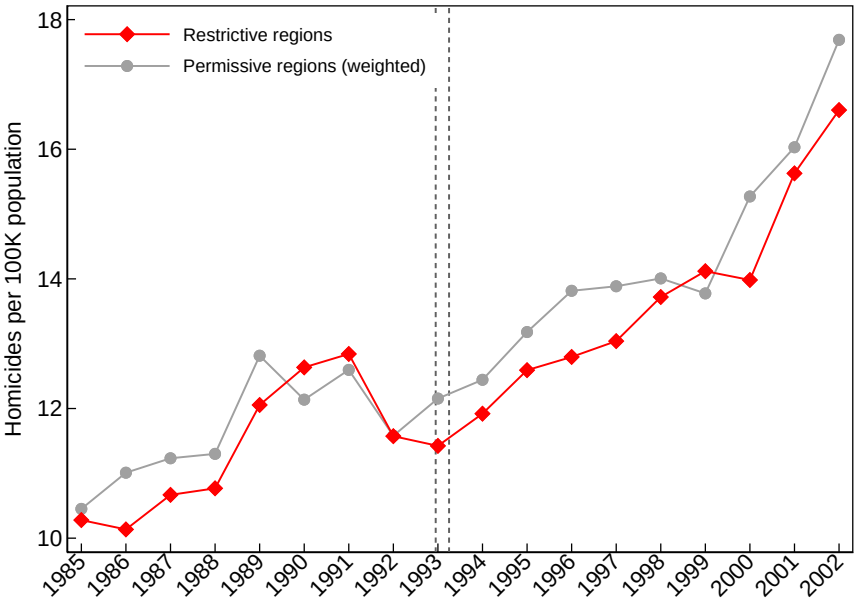
Panel B: Relative change in occupational HHI



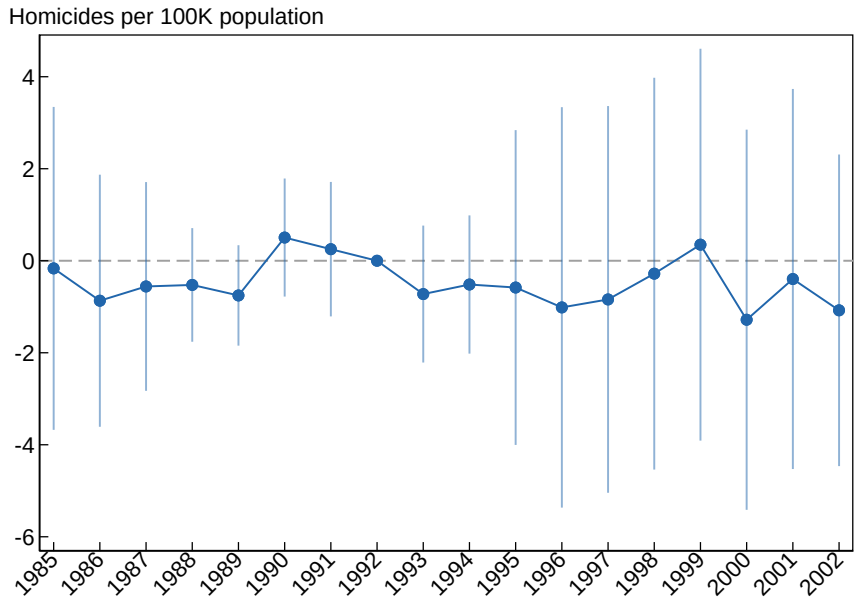
Note: This figure plots the relative change in the contract-firm share (Panel A) and in occupational HHI (Panel B) against distance to the border between restrictive and permissive jurisdictions. We estimate kernel-weighted local polynomial regressions of the relative change of an outcome on distance to border, using the Epanechnikov kernel, polynomials of degree 2, and a bandwidth of 200km, separately for restrictive and permissive microregions. Within each panel, the four sub-panels correspond to years 1994, 1996, 1998, and 2000 (as titled); for sub-panel year t, the relative change is (guard outcome in year t − guard outcome in 1992) − (mean outcome among comparison occupations in year t − the same mean in 1992). Sample includes all microregions within 1000 km of the border between restrictive and permissive jurisdictions, but excludes the jurisdiction of the 2nd Regional Labor Court (the São Paulo and Santos metropolitan areas). We display a graph of the smoothed values with 95% confidence bands, where restrictive regions are given positive distance and permissive regions are given negative distance.

Figure C.9: Regional differences in homicide rates

Panel A: Raw trend



Panel B: Diff-in-diff estimates



Note: This figure plots homicide rates in restrictive versus permissive microregions over time. Data on homicide rates are from the replication files of Dix-Carneiro et al. (2018). Panel A plots the mean homicide rate for restrictive and permissive microregions, respectively. Panel B plots the coefficients from a DD regression comparing restrictive microregions to permissive microregions, with controls for year and microregion fixed effects. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors are clustered at the regional labor court level.

D Mechanism: Additional Evidence and Model

Section 4 shows that, conditional on total firm size, employers with greater occupation-specific scale hire disproportionately from outside the formal sector—younger workers without a formal record—and at lower hiring wages. This appendix interprets those gradients, and the market-level effects of Section 3, through a single model. The model has one evidence-based key assumption: an entrant from outside the formal sector is cheaper than an experienced worker but can be hired only after the firm pays an occupation-specific fixed cost. Scale determines who pays that cost, and hence who hires entrants; contract firms pay it for many clients at once. That fixed cost stands for a family of occupation-specific investments a specialized firm can amortize and a small employer cannot: screening record-less entrants, training beyond the statutory minimum, centralized monitoring, and reassignment across client sites. The hiring gradients do not distinguish these functions, so we formalize the representative one, screening.

The model delivers three results, built up in turn. First, occupation-specific scale governs which firms pay the fixed cost and hence whether they hire experienced workers or entrants—negative assortative matching between scale and worker experience (Proposition D.1). Second, the entrants are hired at strictly lower wages, at no lower productivity (Proposition D.2). Third, contract firms reach the required scale by aggregating demand across many clients, so a policy that lowers the cost of outsourcing weakly raises formal-sector employment (Proposition D.3). Two further features of the policy response, short-run dynamics and spillovers to non-adopting firms, are consistent with the same friction (Section D.4).

D.1 A Screening Model of Occupation-Specific Scale in Hiring

We build the model up one piece at a time: a single position, then a firm of many positions, then the market of contract firms. It is partial equilibrium, in the spirit of Jovanovic (1979).

Setup. Consider the labor market for one occupation. Firms are indexed by j. Firm j has productivity y_j, drawn from a distribution with full support on $[\underline{y}, \infty)$, and needs $n_j \geq 1$ workers in the occupation; n_j is its occupation-specific scale, the object measured in the data conditional on total firm size. A filled position produces y_j.

A position can be filled by an *experienced worker* or an *entrant*, equally productive once hired but differing in their outside option and in whether hiring them requires screening.

- An *experienced worker* carries a portable formal record and can be hired directly by any firm. His outside option is b_X.
- An *entrant* is young, from informality or non-employment, and has no record; he cannot be hired until the firm runs a *screening* technology. His outside option is $b_E < b_X$.

The gap $b_X - b_E$ is an experience premium in outside options, not in output: a portable record lets rival employers bid for an experienced worker, raising his outside option above the entrant's. An entrant is therefore cheaper, not worse.

Screening is a fixed-cost technology. A firm that pays K can hire entrants for all n_j of its positions; equivalently it buys screening from a contract firm at a fee c per position (contract firms enter in §D.3).

Timing. Each period has three steps.

1. *Screening.* Each firm decides whether to operate screening—paying K itself, or the fee c per position—or not.
2. *Hiring.* A screening firm fills its positions with entrants; a non-screening firm fills them with experienced workers where that is profitable, and otherwise leaves them empty.
3. *Wages.* Each hired worker and the firm split the match surplus—output minus the worker's outside option—by Nash bargaining, the firm keeping a share $\beta \in (0,1)$. A worker of output y_j and outside option b is therefore paid $w = b + (1-\beta)(y_j - b)$, leaving the firm $\beta(y_j - b)$.

Standing assumption. Throughout, $0 \leq b_E < b_X$ and $\underline{y} < b_E$, so the least productive firms cannot profitably fill a position with either worker.

One position: the gain from screening. Take a single position at a firm of productivity y_j. Filled by an experienced worker it earns the firm

$$U(y_j) = \max\{\beta(y_j - b_X),\, 0\}; \tag{5}$$

filled by a screened entrant it earns

$$W(y_j) = \max\{\beta(y_j - b_E),\, 0\}. \tag{6}$$

Since the entrant is cheaper and equally productive, $W \geq U$, and the per-position gain from screening is

$$\bar{g}(y_j) \;\equiv\; W(y_j) - U(y_j) \;=\; \begin{cases} 0, & y_j \leq b_E, \\ \beta\,(y_j - b_E), & b_E \leq y_j \leq b_X, \\ \beta\,(b_X - b_E), & y_j \geq b_X. \end{cases} \tag{7}$$

The gain is zero at unproductive firms (no worker is worth hiring), rises over the range where only an entrant is worth hiring ($b_E < y_j < b_X$), and flattens at the wage saving $\beta(b_X - b_E)$ once an experienced worker is profitable too ($y_j > b_X$). It is continuous and nondecreasing.

D.2 The Firm: Scale, Screening, and Hiring Wages

Screening is paid for once and used for all n_j positions, so a firm operates it exactly when the total gain covers the cost, $n_j\,\bar{g}(y_j) \geq K$. Scale, not productivity alone, is what makes screening worthwhile; because n_j is set by demand for the occupation's services, a firm cannot inflate it to harvest the gain.

Proposition D.1 (Occupation-specific scale and screening)**.** *There is a threshold $n^*(y_j) = K/\bar{g}(y_j)$, weakly decreasing in y_j, such that a firm screens if and only if $n_j \geq n^*(y_j)$. A screening firm hires entrants; a non-screening firm hires experienced workers where profitable, else leaves positions unfilled. Higher occupation-specific scale therefore shifts new hires from experienced workers to entrants—in the data, younger workers hired from outside the formal sector, without a formal record.*

Proof. See Appendix D.6. □

This is negative assortative matching between occupation-specific scale and worker experience: the mirror image, within firms across occupations, of the positive assortative matching

between large firms and high-wage workers documented at the level of the whole firm (Song et al., 2019; Card et al., 2013; Alvarez et al., 2018). It is the sign pattern of Table 4.

Proposition D.2 (Hiring wages)**.** *At any firm, an experienced worker is paid* $w_X = b_X + (1-\beta)(y_j - b_X)$ *and a screened entrant* $w_E = b_E + (1-\beta)(y_j - b_E)$*, so the entrant is cheaper by exactly*

$$w_X - w_E = \beta\,(b_X - b_E) \;>\; 0. \tag{8}$$

Proof. Immediate from the wage rule; see Appendix D.6. □

Higher-scale firms therefore pay lower hiring wages because they hire from a cheaper pool, not because they mark wages down: given a worker's output and outside option, bargaining pays the same wage at any firm. The lower entry wage is a compositional shift toward workers the market cannot yet price, so the as-reported hiring wage falls with scale while the demographic-adjusted wage does not—consistent with the near-zero outsourcing wage differential conditional on worker fixed effects (Table D.1) and the flat demographic-adjusted wage at the market level (Section 3).

D.3 The Market: Contract Firms and Aggregate Employment

A firm with $n_j < n^*(y_j)$ cannot cover K on its own. A contract firm can: operating screening once over the positions it fills for many clients, it reaches a scale at which the fixed cost clears even though no single client does, and it sells access at the fee c. A client outsources a position whenever the fee is below the per-position gain, $c < \bar{g}(y_j)$; a policy that lowers the cost of outsourcing lowers c and extends entrant hiring to progressively smaller, lower-productivity clients.

Proposition D.3 (Contract firms and aggregate employment)**.** (i) *A contract firm serving clients at fee* c *operates screening once its served scale* $N = \sum_j n_j$ *satisfies* $cN \geq K$*, even when* $n_j\,\bar{g}(y_j) < K$ *for every individual client.* (ii) *A position is outsourced if and only if* $c < \bar{g}(y_j)$*; so as* c *falls, the outsourced share of employment rises, entrant employment weakly rises, experienced-worker employment weakly falls, and total employment weakly rises.*

Proof. See Appendix D.6. □

The two employment margins are visible in $\bar{g}$. At firms with $y_j > b_X$ a cheaper entrant replaces an experienced worker: the composition shifts, total employment does not. At firms with $b_E < y_j < b_X$, where an experienced worker was too costly to hire, a screened entrant fills a position that was empty: total employment rises. This is the market-level counterpart of Proposition D.1: the reforms of Section 3 raised the aggregate scale of screening by reallocating guard employment to contract firms, and the experienced workers displaced are the older, formally-recorded guards the market-level results document.

Two limitations. First, the model is partial equilibrium: entrant supply is elastic at b_E and wages do not clear, so the level of the total-employment effect is not a general-equilibrium claim. Second, being static, it speaks to neither the short-run transition path nor the spillovers to non-adopting firms; both are documented next (§D.4) and lie outside the model.

D.4 Additional Policy Effects: Short-Run Dynamics and Spillovers

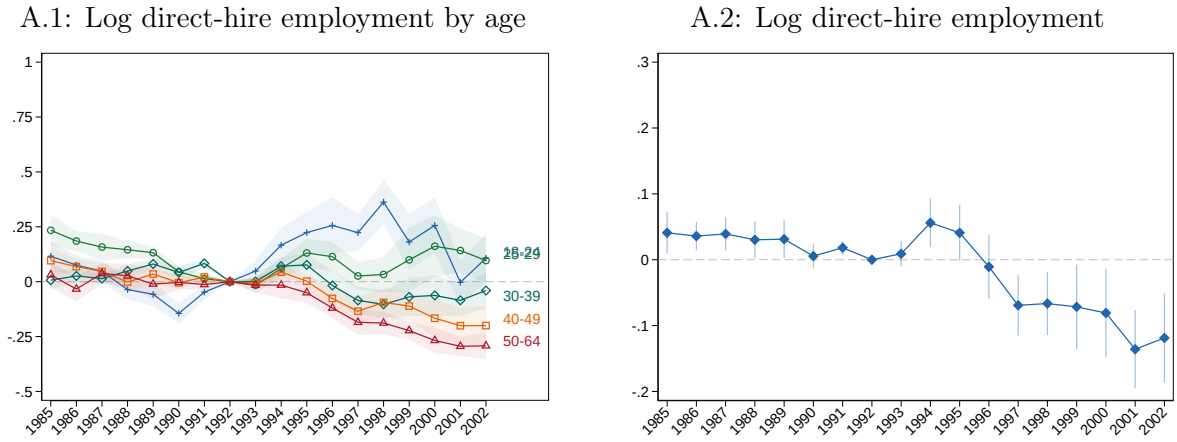
Two additional patterns of the policy response are consistent with the information friction that occupation-specific scale overcomes. First, the short-run adjustment path. If entrants from outside the formal sector could be hired as easily as experienced guards, the reforms would have reallocated employment smoothly: direct-hire employment would fall as outsourcing expanded, and wages would not move. Unlike this smooth path, total direct-hire employment *temporarily increased* after the reforms, then permanently declined (Figure D.1, Panel A.2). Similarly, guard wages temporarily rose before returning to their prior path (Figure 5), though the average post-reform effect on wages is statistically insignificant (Table 3).

These short-run dynamics are consistent with the information friction binding while the technology that relaxes it scaled up: workers from outside the formal sector could not fill positions until screened and certified, and the capacity to screen and certify them—the contract firms that supply it—took time to build. In the interim, employers competed for the only workers the market could already price, the existing stock of experienced guards, temporarily raising both their direct-hire employment and their wages; both effects dissipated as conversion capacity and entry caught up.

Second, spillovers to firms that never adopted outsourcing. Figure D.1, Panel A.1, shows

that the pro-outsourcing reforms persistently increased the direct employment of younger guards and reduced that of older guards in restrictive relative to permissive regions—mirroring the pattern in total employment but occurring among firms that did not adopt outsourcing. This is inconsistent with the reform's effects being confined to adopting firms, and consistent with the friction being relaxed at the level of the market: entrants screened and certified through the contract-firm pipeline form a pool that any employer can hire from, so the recomposition toward young entrants appears among direct-hire employers as well.

Figure D.1: Effect of pro-outsourcing reforms on direct-hire employment



Note: This figure plots year-by-year DDD estimates of the effect of the pro-outsourcing reforms on log direct-hire employment by age group (Panel A.1) and on total log direct-hire employment (Panel A.2). See notes to Figure 1.

D.5 Differentials

This section reports auxiliary evidence in support of the occupation-specific scale mechanism we discuss in Section 4. Specifically, we report differences between contract-firm and direct-hire guards in the post-reform period (1997–2002), conditioning on demographics and, in some specifications, worker fixed effects.

These differentials are the signature the scale efficiency reading predicts: if contract firms substitute fixed-cost investments for the experienced worker's record rather than stripping rents, outsourced guards should be paid close to comparable direct hires and be no worse off after hire. Outsourced guards earn only 1.6–1.9% less than comparable direct hires, with tenure–wage growth about one-third flatter rather than steeper (Table D.1); their exit hazard

from formal employment is about one percentage point lower at entry and converges with tenure (Table D.2); and they switch occupations 2.6 percentage points less per year, entirely among within-firm stayers, while being marginally less likely to move into management (Tables D.3 and D.4). Section 4.3 draws on these differentials in weighing scale economies against rent-sharing and monopsony: a rent-extraction account would predict the large wage penalties and more precarious jobs found for outsourcing elsewhere, which we do not see.

Table D.1: Outsourcing wage differential

	(1)	(2)	(3)	(4)
Contract firm	-0.016***	-0.019***	0.059***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Contract firm × tenure			-0.020***	-0.008***
			(0.000)	(0.000)
Tenure			0.050***	0.026***
			(0.000)	(0.000)
Observations	2870865	2456800	2870865	2456800
R^2	0.34	0.91	0.45	0.91

Standard errors in parentheses

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

Notes: Worker-level regressions of log real hourly wages on a contract-firm indicator, using all private-sector security guards aged 18–64 in RAIS during 1997–2002 (workers with imputed age excluded). All columns absorb microregion and year fixed effects and education-category-specific cubics in age. Columns (2) and (4) additionally absorb worker fixed effects; columns (3) and (4) add tenure and its interaction with the contract-firm indicator.

Table D.2: Outsourcing hazard differential

	(1)	(2)	(3)	(4)
Contract firm	0.004***	0.003***	-0.010***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Worker FE		-0.128***		-0.093***
		(0.001)		(0.001)
Contract firm × tenure			0.003***	0.002***
			(0.000)	(0.000)
Tenure			-0.012***	-0.009***
			(0.000)	(0.000)
Observations	2870865	2836925	2870865	2836925
R^2	0.30	0.31	0.31	0.32

Standard errors in parentheses

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

Notes: Worker-level regressions in which the outcome is an indicator for exiting formal employment (not appearing in RAIS) the following year. Sample and baseline controls as in Table D.1. Columns (2) and (4) control for an estimated worker fixed effect, whose coefficient is reported; columns (3) and (4) add tenure and its interaction with the contract-firm indicator.

Table D.3: Outsourcing occupation switching differential

	(1)	(2)	(3)	(4)
Contract firm	-0.026***	-0.026***	-0.027***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Contract firm × tenure		-0.000***		
		(0.000)		
Tenure		-0.000***		
		(0.000)		
Observations	2870865	2870865	2870865	2870865
R^2	0.02	0.02	0.02	0.00

Standard errors in parentheses

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

Notes: Worker-level regressions in which the outcome is an indicator for switching two-digit occupation the following year, coded zero for workers who do not appear in RAIS the following year. Sample and controls as in Table D.1. Column (2) adds tenure and its interaction with the contract-firm indicator; columns (3) and (4) split the outcome into within-firm and across-firm switches.

Table D.4: Outsourcing managerial promotion differential

	(1)	(2)	(3)	(4)
Contract firm	-0.001***	-0.001***	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Contract firm × tenure		0.000		
		(0.000)		
Tenure		0.000		
		(0.000)		
Observations	2870865	2870865	2870865	2870865
R^2	0.00	0.00	0.00	0.00

Standard errors in parentheses

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

Notes: Worker-level regressions in which the outcome is an indicator for holding a managerial occupation code the following year, coded zero for workers who do not appear in RAIS the following year. Sample and controls as in Table D.1. Column (2) adds tenure and its interaction with the contract-firm indicator; columns (3) and (4) split into within-firm and across-firm promotions.

D.6 Proofs

Proof of Proposition D.1. From (7), $\bar{g}$ is continuous, nondecreasing (slopes 0, β, 0), and strictly positive for $y_j > b_E$; hence $n^*(y_j) = K/\bar{g}(y_j)$ is weakly decreasing where $\bar{g} > 0$, and $n_j \bar{g}(y_j) \geq K \iff n_j \geq n^*(y_j)$. A firm that has paid K earns $W(y_j) \geq U(y_j)$ per position and so fills every profitable position ($y_j > b_E$) with an entrant; a firm that has not cannot hire entrants and fills a position with an experienced worker when profitable ($y_j > b_X$), else leaves it empty. Thus screening firms hire entrants and non-screening firms hire the experienced, and raising n_j past $n^*(y_j)$ switches the firm's new hires from experienced workers to entrants. □

Proof of Proposition D.2. With output y_j common to both workers, the wage rule gives $w_X = b_X + (1-\beta)(y_j - b_X)$ and $w_E = b_E + (1-\beta)(y_j - b_E)$; subtracting, $w_X - w_E = \beta(b_X - b_E) > 0$. □

Proof of Proposition D.3. *(i)* A client outsources a position iff $W(y_j) - c > U(y_j)$, i.e. $c < \bar{g}(y_j)$, and then pays c on each of its n_j positions; a contract firm serving such clients collects $c \sum_j n_j = cN$ against the one-time cost K and operates once $cN \geq K$. A large

enough base of clients with $\bar{g}(y_j) > c$—each possibly sub-threshold, $n_j \bar{g}(y_j) < K$—meets this, since it is the fee revenue cN, not the total surplus $\sum_j n_j \bar{g}(y_j)$, that must cover K. *(ii)* Fix c. A client with $n_j \geq n^*(y_j)$ already screens and hires entrants regardless of c, so it is invariant to c and set aside. Among clients with $n_j < n^*(y_j)$, a position is outsourced iff $c < \bar{g}(y_j)$; since $\bar{g}$ is nondecreasing, the outsourcing set $\{y_j : \bar{g}(y_j) > c\}$ is an interval of higher-productivity firms that expands as c falls. A position entering it with $y_j > b_X$ was filled by an experienced worker, now an entrant (composition shifts, total unchanged); one with $b_E < y_j < b_X$ was empty, now an entrant (total rises). Hence as c falls the outsourced share and entrant and total employment weakly rise while experienced employment weakly falls. $\square$