DOMESTIC OUTSOURCING AND YOUNG-WORKER ENTRY INTO THE FORMAL SECTOR

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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 reduced the relative cost of outsourcing security guards, a triple-differences design shows that the reforms increased formal guard employment by 4% and hiring from unemployment or informality by 7%, 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
JEL: J23, J24, J46, L24, O12, O17

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1 Introduction

Most employers need only a handful of security guards, cleaners, or cafeteria workers. Work of this kind is *non-core*: peripheral to the firms that demand it, so that demand for it is thin at any one employer but substantial once pooled across many. In high-income countries, such work is routinely contracted out to specialized service firms that do the pooling (Abraham and Taylor, 1996; Autor, 2009). In developing countries, by contrast, the lawful boundary of such contracting has been drawn narrowly and contested repeatedly, out of concern that outsourcing erodes the protections of the employment relationship in labor markets already characterized by high rates of informality and slippery job ladders (Ulyssea, 2020; Donovan et al., 2023).

Whether these restrictions matter for workers depends on what specialized firms do with the scale that pooling creates. Occupation-specific scale can function as a hiring technology, funding fixed-cost investments in screening, training, monitoring, and reassignment that make it profitable to hire young workers whom formal-sector employers cannot otherwise evaluate (Breza and Kaur, 2025). In this paper, we explore the possibility that the rise of specialized contract firms can, through this channel, increase young-worker entry into the formal sector.

We assess this theory in Brazil, where reforms enacted in 1993 and 1994 reduced the relative cost of outsourcing non-core activities. The setting is well suited to the question: under Brazilian law, outsourcing holds labor obligations fixed on both sides of the firm boundary—the client purchases a service, and the workers who provide it are formal employees of the specialized contractor, covered by the same labor code as any other formal worker. This institutional feature removes the regulatory-arbitrage motive present where contracted work falls under different labor or tax rules—as with contract labor in India (Bertrand et al., 2025) and personnel subcontracting in Mexico before its 2021 ban (Estefan et al., 2024)—and isolates the occupation-specific scale channel, for which we present direct evidence below.

Until 1993, Brazil lay at the restrictive extreme, with outsourcing presumed unlawful by superior court precedent outside narrow exceptions. On December 17, 1993, following an unanticipated series of events, Brazil's Superior Labor Court issued *Súmula 331*, which replaced that precedent 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 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 the private-security statute, raising the cost of employing armed guards in-house relative to contracting them. 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 the reforms moved on both margins: cheaper to contract out, costlier to keep in-house. Cleaners, the other activity named in the 1993 ruling, faced only the first margin—and had little room to respond to it. 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 predominantly formal. Consistent with this differential incidence, we show that the contract-firm employment share rose 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 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 cohorts. 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 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 pulling workers into the formal sector.

What drives these effects? We argue that the reforms' effects on the compositional shift and total formal employment within the guard occupation are a result of occupation-specific scale economies at contract firms. By aggregating demand across multiple clients, contract firms have the scale to invest in occupation-specific fixed-cost capabilities—such as screening, training, monitoring, and reassignment—that improve the productivity of entrant workers, resulting in a markedly different composition of new hires as a function of within-firm occupation-specific scale, a pattern we can directly measure in employer-employee linked data. The reforms reallocated the hiring of guards toward the employers with the greatest occupation-specific scale, extending market-wide the hiring technology that scale supports within firms.

Our argument is based on a novel pattern we document for all occupations in Brazil's employer-employee linked data: the hiring of inexperienced youth from outside the formal sector rises with employers' occupation-specific scale. Specifically, conditional on total firm size, the share of a firm's new hires into an occupation who come from outside the formal sector and the share who have never held a formal job are increasing in the firm's employment in that occupation (by 0.07 and 0.13 percentage points per 10% of scale), while new hires' average age and starting wages are decreasing in it (by 0.09 years and 0.3%). The same characteristics move in the opposite direction with total firm size: new hires at larger firms have more formal-sector history, are older, and are paid more at the start, the standard pattern of positive assortative matching (Song et al., 2019; Card et al., 2013; Alvarez et al., 2018). These gradients are conditional on occupation-by-microregion-by-year fixed effects, which absorb differential trends in the local composition of new hires common to all firms, and on firm fixed effects, which absorb time-invariant differences in the composition of a firm's new hires common to all occupations and regions where it operates.

We formalize this bridge from within-firm gradients to market-level effects in an appendix model in the spirit of Jovanovic (1979). 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 paying the fixed cost of screening. Occupation-specific scale determines which firms pay it, 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 to progressively smaller employers, raising formal employment in the occupation. Screening stands in for a family of scale capabilities, 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 the small outsourcing wage differential.

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). These estimates rely on *firm-level* variation in outsourcing choices, which does not capture effects on all workers in affected occupations; assessing them requires a market-level reform of the kind we study. In our own setting, Felix and Wong (2026) document that the reforms we study displaced 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., 2025) by showing that the gains from specialization can be delivered through specialized intermediaries rather than within-firm professionalization, linking it to work on standardization-driven scale (Argente et al., 2025) and task-specific scale as a specialization driver (Arnarson et al., 2026).

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.[1] 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 must comply with Brazilian labor regulations on the 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,[2] which replaced the categorical rule of Súmula 256 with the *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.[3] Súmula 331 was not, however, a clean-slate legalization

[1] Where the lawful boundary of outsourcing is drawn varies across developing countries. Ecuador (*Mandato Constituyente* 8, 2008) and Mexico (the 2021 federal labor reform) prohibit the subcontracting of personnel while leaving firm-to-firm contracting of specialized services outside the client's core activity lawful; India's Contract Labour Act (1970) empowers governments to prohibit contract labor in work that is perennial or incidental to an establishment's business (Bertrand et al., 2025); and Peru's Ley 29245 (2008) requires the contractor to supply an integral service at its own entrepreneurial risk (Jiménez and Rendón, 2025). On Mexico see Estefan et al. (2024).

[2] 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.

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

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.1 summarizes the legal status of outsourcing by activity across the pre-1986, 1986–1993, and 1993–2017 regimes.[4]

As a consequence of Súmula 331, the expected legal cost of outsourcing workers 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, and a graduated penalty regime. This bundle applied to any firm employing armed guards, and existing operators were given 120 days from the law's publication in March 1994 to comply.

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

[4] Contracting with an individual in place of an employee remained unlawful as disguised employment throughout our period; the arrangements that later blurred this boundary—such as *pejotização*, in which a worker incorporates and is contracted as a firm—became possible only under reforms enacted in 2017.

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.2 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. Table 1 reports descriptive statistics for the occupation and for the workers who hold these jobs. As Appendix Table A.2 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 among guards but not among cleaning or other *atividade-meio* activities.

Security guards are by no means a niche occupation in our context: they 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.[5] Figure 1, 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 around 1993.[6] 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 security services and no longer directly employed security guards.

Contract-firms' share by occupation offer a first 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. Figure 2, Panel B displays the trend in the share of private sector workers employed in contract firms

[5]See Table 1 for more descriptive statistics of the occupations.

[6]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.

(i.e., the "contract-firm share") for each occupation,[7] averaged across microregions in restrictive regions. Before 1993, two occupations—security guards and cleaners—had contract-firm shares far exceeding those of other occupations. In 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 predominantly 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[8] and must complete mandatory training administered by Brazil's *Polícia Federal*.[9] 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.1 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 predominantly formal (Figure 3 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 Figure 2 is thus consistent with

[7] 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 Table 2 for a list of two-digit large occupations included as comparison occupations in local labor market analyses.

[8] 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).

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

this difference in returns rather than with any difference in legal permissibility.

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 considers outsourcing legal!' " Appendix Tables A.3 and A.4 provide many more of these quotes, taken from transcripts of interviews with former regional court justices, judges, lawyers, and union leaders.[10]

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.[11] 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

[10]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.

[11]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.5.

of the state of São Paulo (15th region, Campinas),[12] 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.1).

Aggregate trends confirm that the pro-outsourcing reforms had a much larger impact in restrictive jurisdictions. Figure 2, 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 contrasts with the pattern observed in permissive regions, where outsourcing experienced secular growth throughout the period, with no apparent trend breaks following the reforms.

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 RAIS's specific industry codes for contract firms.[13] This allows us to identify outsourced workers as those employed by contract firms and direct hires as those at any other private sector firm. Finally, the high degree of formality among security guards

[12] 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.3.

[13] 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.

allows us to track both incumbents and newcomers.

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, we cannot estimate "outsourcing premia," the wage difference between outsourced and direct employment for the same worker, job, and firm. [14]

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 and labor market

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

concentration; (ii) increased worker entry into formal guard employment from unemployment or informality, expanding total 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 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.[15]

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 3, 4, and 5. As in standard DDD identification strategies, causal interpretation of β requires the assumption of parallel trends

[15]For Census outcomes only two waves are available (1991 and 2000), so we instead estimate $y_{omt} = \beta\left(\text{Guard}_o \times \text{Restrictive}_m \times 1_{t=2000}\right) + \delta_{om} + \delta_{ot} + \delta_{mt} + \epsilon_{omt}$, where y_{omt} is, in turn, log survey-weighted formal employment, log informal employment, and the weighted mean of log monthly earnings conditional on formal and 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 defined at the occupation-group level, the finest classification consistent across the two Censuses. Regressions are weighted by the same entropy-balancing microregion weights as the RAIS DDD, and standard errors are clustered by microregion. Appendix B gives the occupation crosswalk between the two Census coding systems.

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:

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

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 4–8. 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.[16] The occupation-year fixed effects δ_{ot} absorb the former and the region-year effects δ_{rt} 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

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

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 weighting each observation with entropy balancing weights (Hainmueller, 2012). In 1992, before the reforms, guard markets in restrictive and permissive jurisdictions were similar on the balanced covariates. Appendix Figure C.1 maps the entropy balancing weights. Appendix Figure C.4 shows no differential homicide trends across restrictive and (weighted) permissive regions.

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 methods 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 11 large two-digit occupations present in all of them.Table 2 lists them and their characteristics. Our estimation sample covers roughly 465,000 security guards per year (or 98% of formal sector guards) and roughly 8.5 million control occupation workers per year (over half of 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 4–8, 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.[17]

[17]"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 earnings. 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

Pro-outsourcing reforms increased outsourcing

Figure 4 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: the share of workers in the microregion-pair at contract firms.

The figure shows that relative to permissive regions, the prevalence of outsourcing among guards rose in restrictive regions following the reforms, with no differential pre-trends. Prevalence increased steadily through 1999, leveling off afterwards. Table 3 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).[18] These effects are robust to alternative weighting methods (columns (3)–(4)).

Pro-outsourcing reforms increased labor market concentration

Figure 5 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 3). These effects are robust to alternative weighting methods. The increase is attributable to contract firms employing large numbers of specialized workers (Table 1).

Pro-outsourcing reforms increased worker entry and employment

Figure 6 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

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permissive regions; occupation-level outcomes are averages across microregions using microregion balancing weights, with occupations weighted equally.

[18] 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 low prevalence of outsourcing (see Figure 2).

dataset in the previous year). Total employment is measured as the log total number of workers in each microregion-occupation pair, whether directly hired or 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 4%.[19] Panel D of Table 3 shows the precise point estimates and breaks down the total employment effect by employment origins (i.e., workers from outside RAIS compared with those already inside RAIS). It shows that the 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,[20] as direct-hire employment declined by 6.8% (Panel C).[21] These effects are robust to alternative weighting methods (columns (3)–(4)).

Pro-outsourcing reforms reallocated employment from older to younger workers

Figure 7, 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 4), 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.[22] These effects are robust to alternative weighting methods and balancing covariates (Table 4; Appendix Table C.3).

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

[20] 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.

[21] 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 5), 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.

[22] 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.2 shows the estimates are insensitive to this choice.

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 broke this trend for guards: employment 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 differ 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 4 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.[23] 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 8 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.[24] Table 5 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)).

Panel B of Figure 8 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

[23]The reforms also 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.

[24]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)

employment effects shown in Panel B of Figure 6, there are differential trends in wages across regions (with near-identical magnitudes for guards and other occupations). However, unlike with employment, the pro-outsourcing reforms did not detectably break these trends.[25] These effects are robust to alternative weighting methods (Table 5).

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, $p < 0.01$) (Table 6). The formal-employment gain is concentrated among the youngest 1991 cohorts and declines monotonically with age (Table 7), consistent with the reforms drawing in young entrants.

4 Mechanisms

This section examines the mechanism behind the young-worker entry documented in Section 3.2: internal occupation-specific scale economies in hiring, scaled up to the market level by pro-outsourcing policies that favored specialized security firms. We document that the hiring of younger, less experienced workers new to the formal sector is increasing in occupation-specific firm scale, not only among security guards but across all occupations in Brazil's employer–employee data. We then weigh alternative mechanisms: rent-stripping, monopsony, and regulatory arbitrage.

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

[25] 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 5.

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, already in the pre-reform period (1985–1993) guards at contract firms were younger than direct hires (about 34 versus 40 years old), and contract firms were specialized in the occupation in a way direct employers were not: the median contract firm employed about 580 workers, of whom about 490 were guards, whereas the median direct employer of guards had about 150 workers and only seven guards (Table 1).

These investments span the employment cycle. In *screening*, 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*, 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., 2026). 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.[26] This is how outsourcing a non-core activity differs from outsourcing a core one: the gains come not from circumventing labor regulation (Bertrand et al., 2025) but from human-resource operations at a scale no single client could reach (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

[26] Autor (2001) documents analogous screening and training investments by temporary-help agencies in the United States, and Battiston et al. (2025) studies job rotation and experience accumulation within a Colombian security firm.

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. The hiring gradients do not distinguish among screening, training, monitoring, and reassignment, so the model formalizes the representative one; Appendix D adds post-hire evidence supportive of this family of investments: outsourced guards are paid close to comparable direct hires, exit less in early tenure, and switch occupations less.

4.2 Occupation-Specific Scale Effects in Hiring

To provide evidence for this mechanism, Table 8 reports the characteristics of newly hired workers as a function of occupation-specific firm scale, estimated 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.[27] 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 × microregion × 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 8 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

[27] Appendix Table C.4 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.

(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 8 implies that the share of new hires drawn from outside the formal sector and the share who had never held a formal job are increasing in occupation-specific employment—by 0.07 and 0.13 percentage points per ten percent of scale—while average age at hiring and hiring wages are decreasing in it, by 0.09 years and 0.3 percent. Panel B of Table 8 presents the same effects but restricting the set of occupation cells to guard cells only.[28] The patterns for guards are similar in sign and magnitude for all outcomes except years of education, where he education of the marginal entrant is instead increasing in guard scale, a pattern potentially driven by minimum education requirements for guards.

4.3 Alternative Mechanisms

The evidence above points to occupation-specific scale economies in hiring as the primary mechanism behind the market-level effects 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-stripping. 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,

[28]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.

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. (2026) 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). Outsourced guards also switch occupations 2.6 percentage points less per year, almost entirely within the firm (Appendix Table D.3). 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—whether their rise can increase young workers' formal employment. Leveraging a pair of 1993–1994 Brazilian reforms that 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 that exploits regional and occupational 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 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 often 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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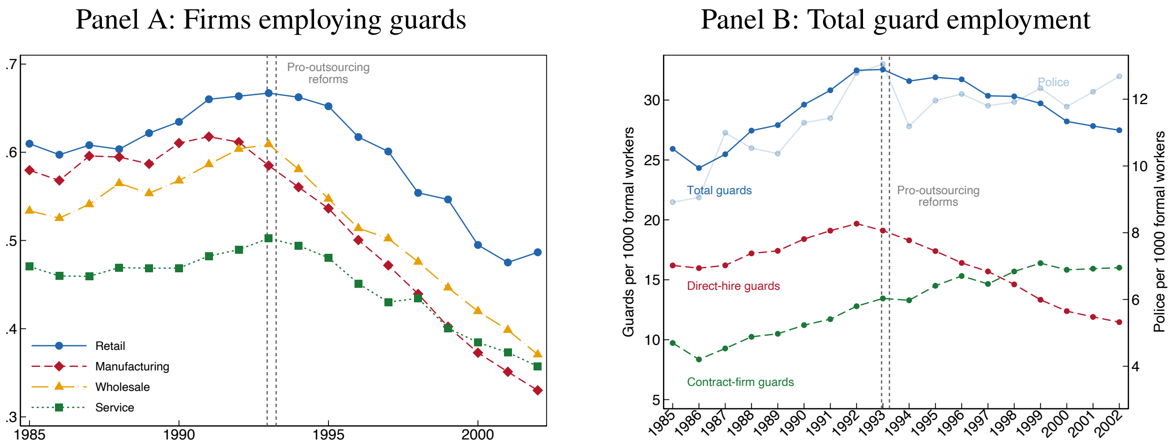
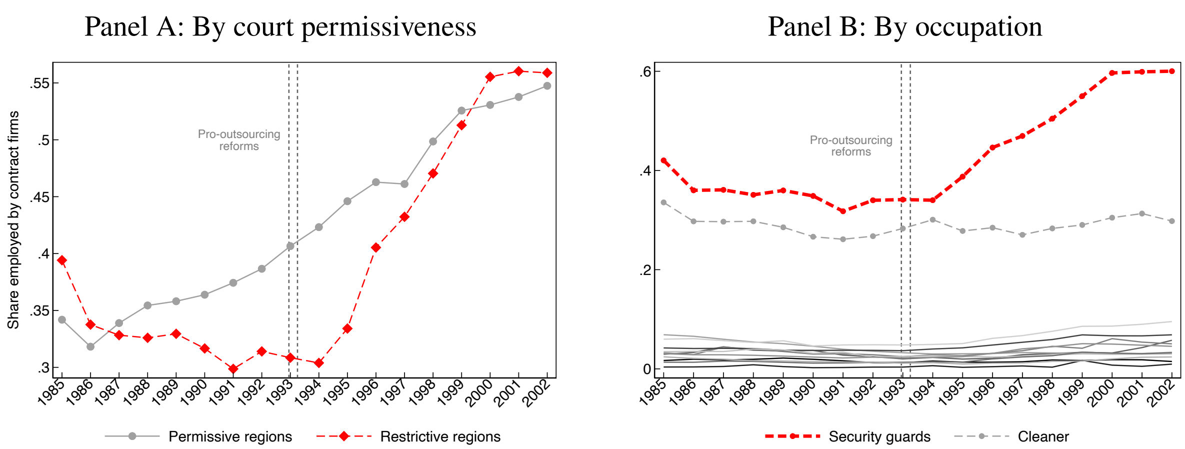
Figure 1: Trends in security guard employment



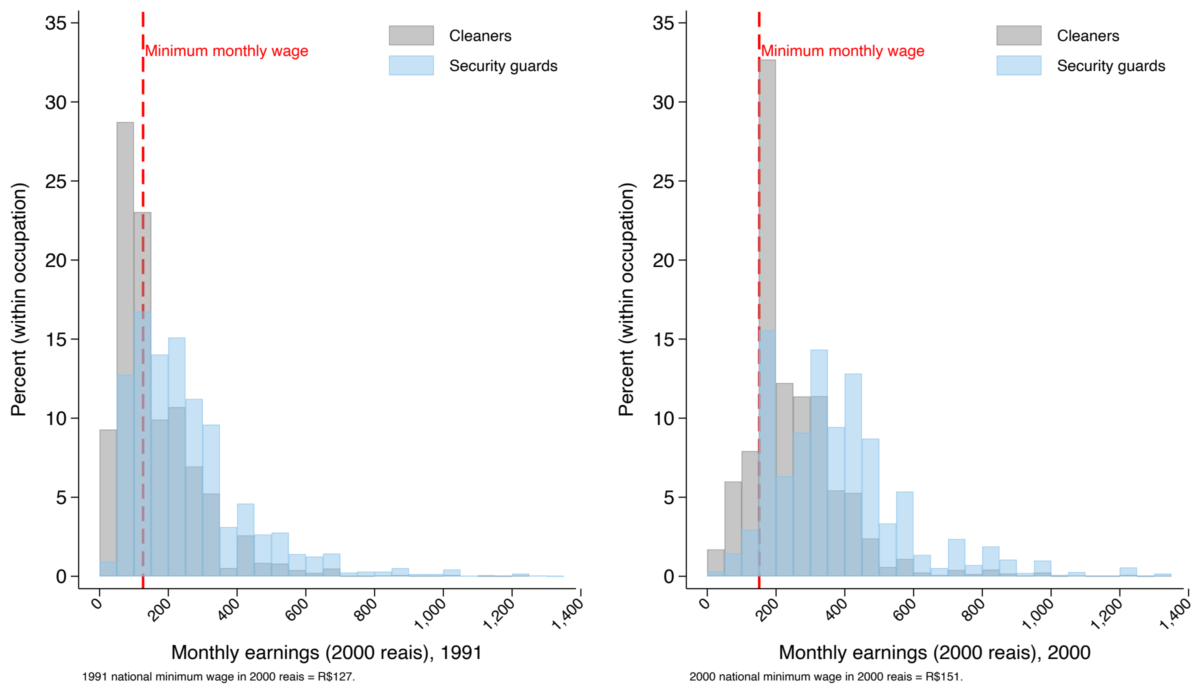
Note: This figure plots national trends in security guard employment; the two dashed vertical lines mark Súmula 331 (December 1993) and Lei 8.863 (March 1994). Panel A plots the share of establishments with at least 50 employees in the respective sectors that employ at least one security guard. Panel 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.

Figure 2: 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 Table 2.

Figure 3: Monthly earnings distribution for cleaners and security guards, 1991 and 2000

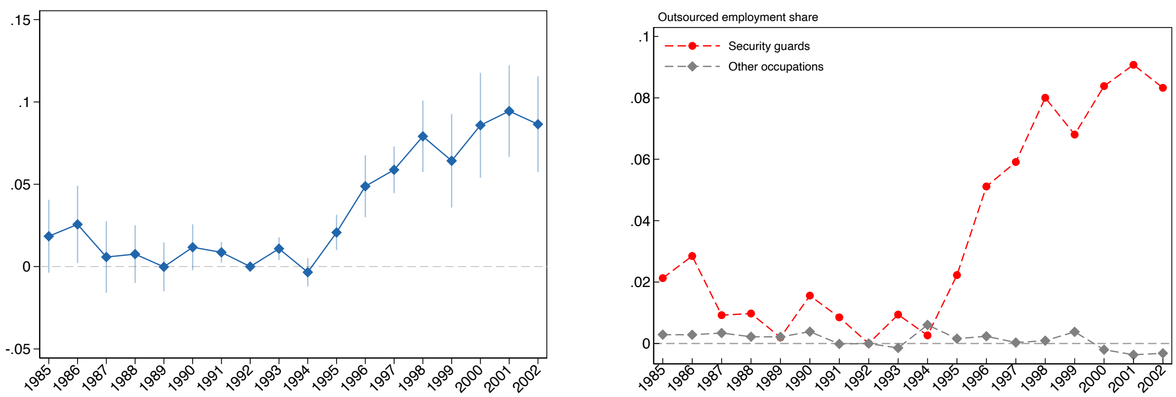


Notes. This figure plots overlaid histograms of monthly earnings for cleaners and for security guards, separately for the 1991 and 2000 Censuses. Monthly earnings in 2000 R$. The vertical dashed line in each panel marks that Census year's national minimum wage in 2000 R$ (R$126.51 for 1991; R$151 for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

Figure 4: Effect of pro-outsourcing reforms on outsourcing prevalence

Panel A: DDD estimates

Panel B: Cross-region DDs, by occupation

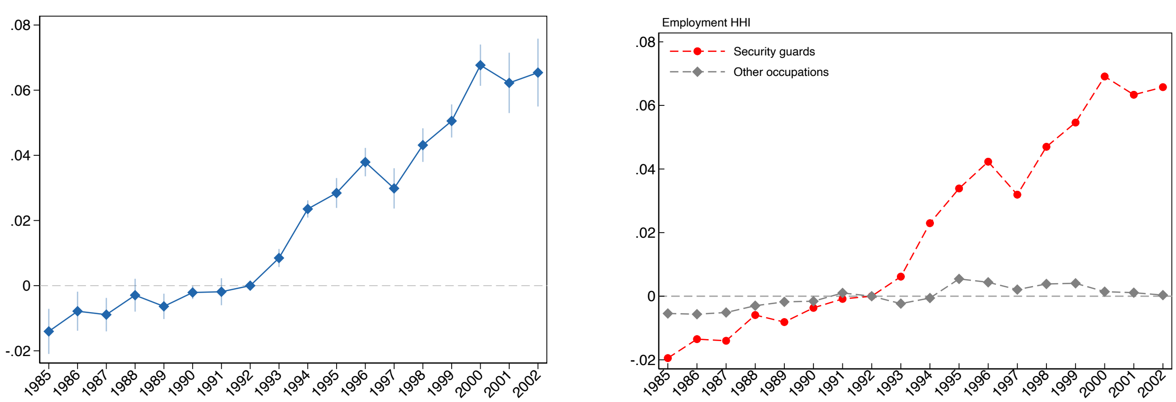


Note: Panel A plots the coefficients β_τ from the DDD regression measuring the impact of the pro-outsourcing reforms in equation (2), which compares guards to other occupations in restrictive versus permissive regions. Each observation is a microregion x occupation x year cell. Outsourced share is the 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 17. 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 5: Effect of pro-outsourcing reforms on employment concentration (HHI)

Panel A: DDD estimates

Panel B: Cross-region DDs, by occupation

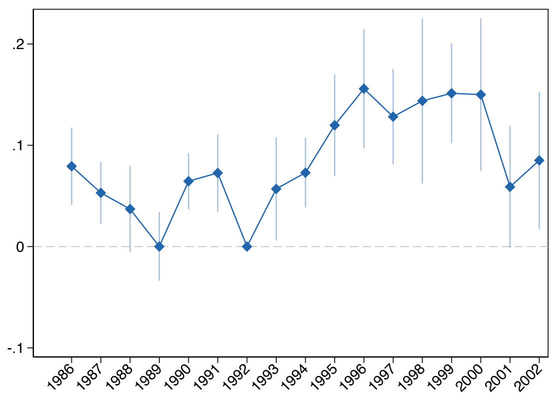


Note: See notes to Figure 4. The outcome variable is employment HHI, the Herfindahl-Hirschman employment index (equal to the probability that any two workers work for the same firm) in a microregion-occupation pair.

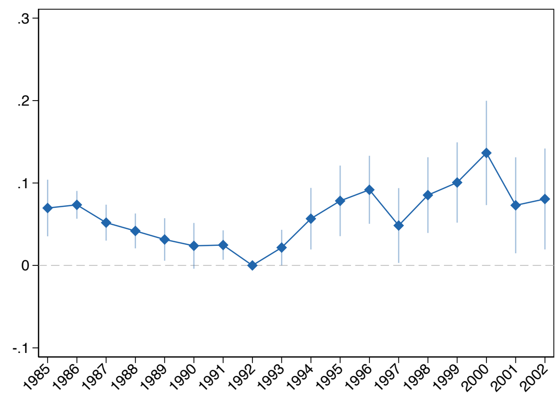
Figure 6: Effect of pro-outsourcing reforms on worker entry and total employment

Panel A: Year-by-Year DDD

A.1: Entry from outside RAIS

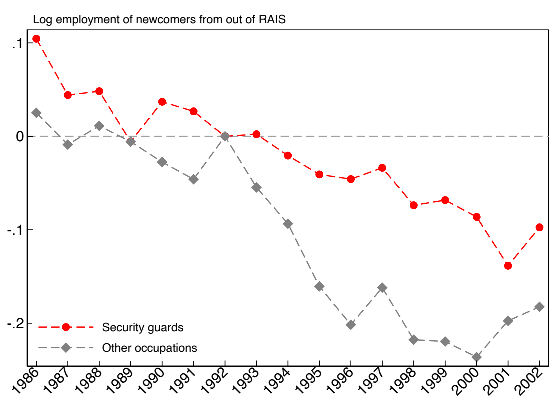


A.2: Log employment

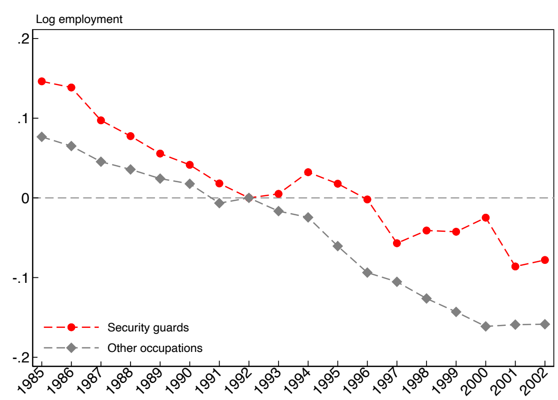


Panel B: Cross-region DDs, by occupation

B.1: Entry from outside RAIS



B.2: Log employment

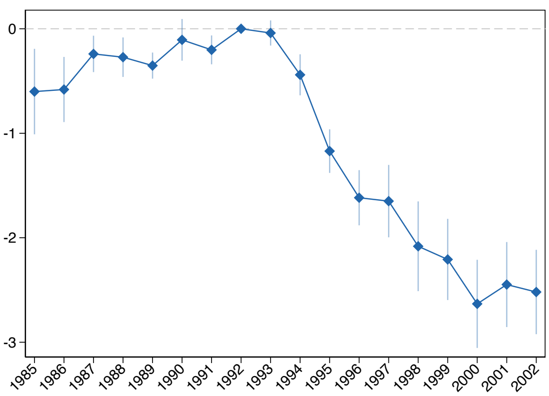


Note: See notes to Figure 4. The outcome variable in Panels A.1 and B.1 is log number of workers from outside of RAIS in the microregion-occupation pair, whereas the outcome in Panels A.2 and B.2 is log total employment in the microregion-occupation pair; employment counts enter as inverse hyperbolic sine transforms (see notes to Table 3). "Entry from outside RAIS" refers to workers not present in RAIS in the previous year, i.e., hired from unemployment, informality, or non-participation.

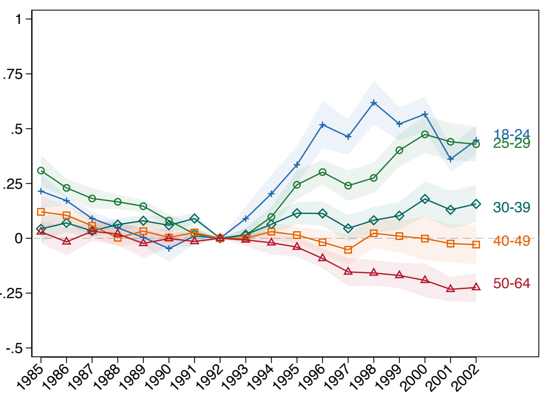
Figure 7: Effect of pro-outsourcing reforms on workforce age composition

Panel A: Year-by-Year DDD

A.1: Worker age



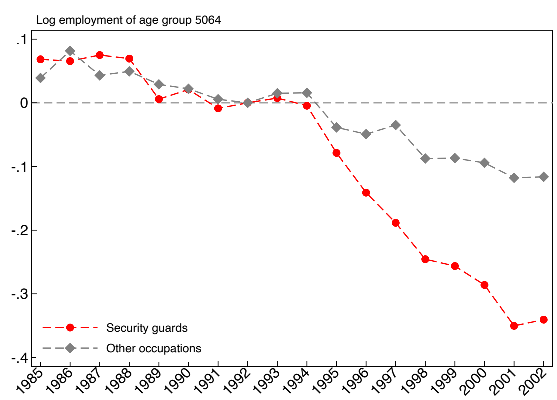
A.2: Log employment by age groups



Panel B: Cross-region DDs, by occupation

B.1: Log employment, ages 18–24

B.2: Log employment, ages 50–64

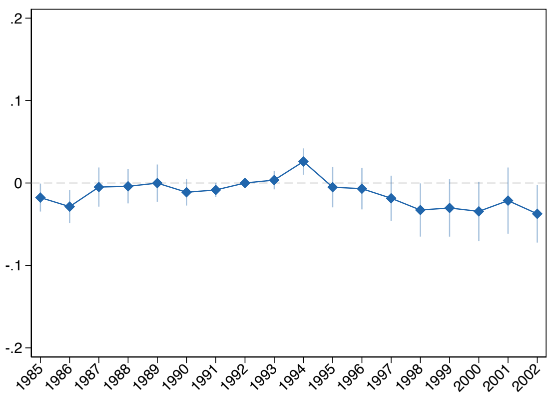


Note: See notes to Figure 4. The outcome variable in Panel A.1 is worker age, while the outcome variables in Panel A.2 are log employment by different age groups, as inverse hyperbolic sine transforms (see notes to Table 3). Age groups are defined according to the age group variable consistently reported throughout the period.

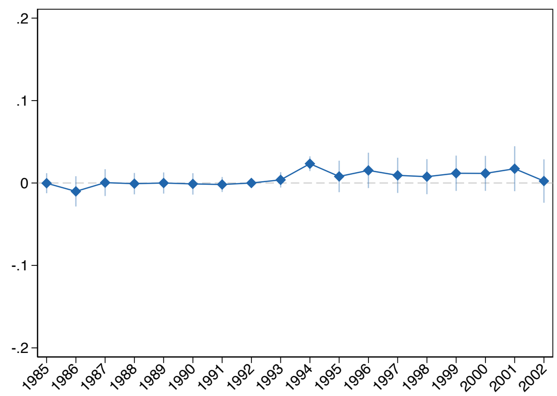
Figure 8: Effect of pro-outsourcing reforms on wages

Panel A: Year-by-Year DDD

A.1: Log earnings, as reported

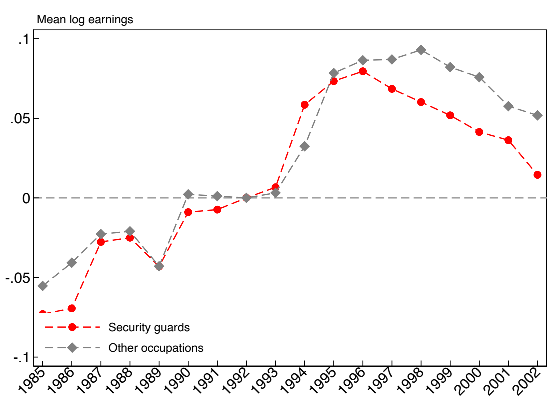


A.2: Log earnings, worker fixed effects

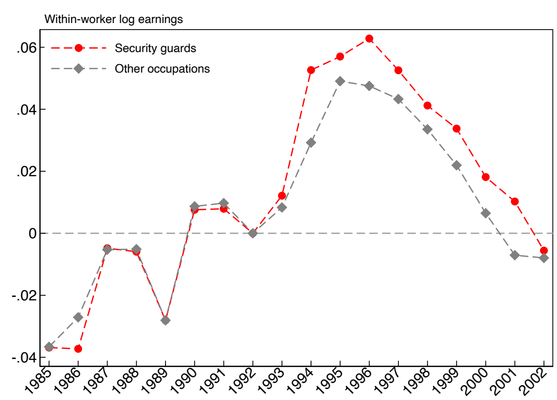


Panel B: Cross-region DDs, by occupation

B.1: Log earnings, as reported



B.2: Log earnings, worker fixed effects



Note: See notes to Figure 4. The outcome variable in Panels A.1 and B.1 is log real December earnings as reported by each worker, without any controls. The wage outcome variable in Panels A.2 and B.2 is log real December earnings, conditional on worker fixed effects and 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: 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.

Table 2: 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 3: Effect of pro-outsourcing reforms on guard outsourcing and employment

	Baseline treated mean (Guards in Restrictive) (1)	Policy effect (DDD) EB (2)	IPW (3)	EB+RA (4)	Obs. (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.2 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 4: Effect of pro-outsourcing reforms on guard characteristics

	Baseline treated mean	Policy effect (DDD)			
	(Guards in Restrictive)	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 3, which describe the inverse hyperbolic sine transform applied to all employment outcomes.

Table 5: Effect of pro-outsourcing reforms on wages

	Baseline treated mean (Guards in Restrictive)	Policy effect (DDD)			
		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 3. 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 Table 1). Earnings outcomes are expressed in natural logarithms.

Table 6: 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 7: 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 8: 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.