The Reallocation Effects of Domestic Outsourcing*

Mayara Felix
Yale University and *NBER*

Michael B. Wong
HKU

August 2024

Abstract

This paper estimates the wage, employment, and reallocation effects of non-core activity outsourcing using Brazil's unexpected 1993 court-ordered outsourcing legalization. We leverage North-South variation in pre-legalization court permissiveness and compare security guards to less affected occupations. We find that older incumbent security guards were adversely impacted through occupational layoffs, loss of firm-level wage premia, and exit from the occupation. At the same time, increased numbers of younger workers entered the formal sector and became employed at contract firms. On net, legalization increased guard employment by 5%, led by a 50% increase in employment for guards aged 18-24, and had no effect on demographically-adjusted guard wages. The observed labor reallocation effects are explained by the fact that contract firms persistently employ demographically different workers than direct employers.

Keywords: domestic outsourcing, employment
JEL: J52, L24, J58

*First version: November 2021; previously circulated as "Labor market effects of domestic outsourcing: Evidence from legalization in Brazil." Felix: mayara.felix@yale.edu; Wong: mbwong@hku.hk. Michael Wong is very grateful to his advisors David Autor, Bob Gibbons, Robert Townsend, and John Van Reenen for steadfast guidance and support. Breno Gaspar provided excellent legal research assistance. We thank Brazilian jurists Magda Biavaschi and Sérgio Torres, and legal historian Alisson Droppa for providing guidance on court classifications, and we are grateful to Eduardo Carvalho for helping us arrange meetings on the field with security service firms, workers, lawyers, and union leaders. We thank Diego Daruich, Arindrajit Dube, Tomás Guanziroli, Simon Jäger, Claudia Macaluso, Raffaele Saggio, Breno Sampaio, Anna Stansbury, audience members at SOLE 2023, SED 2023, seminar participants at CityUHK, HKBU, HKU, Jinan, Princeton, Richmond Fed, UFPB, UFPE, and numerous former classmates and faculty at MIT for helpful comments and suggestions. We gratefully acknowledge funding from the National Science Foundation, the Kuok Foundation, and the George and Obie Shultz Fund at MIT.

1 Introduction

Around the world and across industries, firms increasingly rely on outsourced workers to provide labor services once performed by direct employees, such as cleaning, security, logistics, HR, and IT.[1] The rise of outsourcing is often said to have fundamentally altered labor markets (Autor, 2009; Weil, 2014; Appelbaum, 2017; Stansbury and Summers, 2020). Recent studies show that outsourcing excludes low-wage workers from firm-level rents and increases wage inequality (Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017; Drenik et al., 2023). Growing evidence also suggests that outsourcing enables firms to flexibly scale up and boosts productivity (Bertrand et al., 2021; Bilal and Lhuillier, 2021).

Despite the expanding literature, there remain many open questions. How does outsourcing affect the employment of different types of workers? Might some workers benefit, even as others are hurt? Does the outsourcing of *non-core* activity differ from that of *core* activities? Could the effects of outsourcing on workers be less negative in countries with a large informal sector? What explains the labor market effects of outsourcing?

In this paper, we study the wage, employment, and reallocation effects of domestic outsourcing using an unexpected court ruling in Brazil. In 1993, Brazil's Superior Labor Court legalized outsourcing of all *non-core* activities by private-sector firms. This exogenous legal change enables us to precisely estimate the effects of a large market-level reduction in the cost of outsourcing on different segments of workers.

We focus on Brazil's large market for security guards, which accounted for 3% of total formal private-sector employment in 1992 and whose wages are close to that of the median formal-sector worker in Brazil. The reason for our focus is that security guards are a licensed, heavily regulated, and mostly formal-sector occupation.[2] As such, legal restrictions against outsourcing before legalization were particularly binding for security guards, who upon legalization experienced the largest rise in outsourcing among all major occupations. We measure the impact of outsourcing legalization on the near universe of security guards using Brazil's comprehensive matched employee-employer data.

We uncover that outsourcing legalization substantially reallocated jobs from older in-

[1] See Dey et al. (2010); Berlingieri (2014); Bloom et al. (2018); Katz and Krueger (2016, 2019).
[2] See Appendix Figure C.1.

cumbent security guards to younger entrant workers. In particular, we find that a large subset of older incumbent workers experienced occupational layoffs, loss of firm-level wage premia, and exit from the occupation. At the same time, an increased number of younger workers entered from outside the formal sector and become security guards at contract firms. These reallocation effects are explained by the fact that outsourcing legalization increased employment at contract firms, which persistently employ demographically different workers than direct employers. Outsourcing legalization therefore resulted in persistent changes in the demographic composition of security guards in Brazil.

Section 2 provides institutional background. Section 3 documents the incidence and consequences of firm-level outsourcing events in the wake of outsourcing legalization, following the pioneering methods proposed by Goldschmidt and Schmieder (2017). Even though on-site outsourcing events have been the focus of the existing literature, we find that occupational layoffs are quantitatively a more important mode of outsourcing in Brazil.[3] While legalization led to a large wage of occupational layoffs—affecting 7-9 percent of incumbent security guards—it had a negligible effect on on-site outsourcing events, which are also rare throughout the period.

The effects of occupational-layoff outsourcing on incumbent workers were negative, large, and long-lasting. Occupational layoffs temporarily displaced incumbent workers from formal employment and persistently reduced their wages by roughly 10 percent. This decline in worker earnings is explained to a large extent by a loss of firm wage premia. Incumbents initially employed by higher-wage firms experience larger wage reductions, while incumbents in the bottom quartile of firms with occupational layoffs experienced little decline in wages. This loss of firm-specific wage premia explains about 46 percent of the total wage losses five years after an occupational layoff.

Section 4 estimates the local labor market effects of outsourcing legalization. We compare microregions in Brazil's South, where labor court judges tended to forbid outsourcing prior to legalization, with microregions in the rest of Brazil, where judges tended to be more permissive. To control for confounding shocks in the microregion, we use a triple-differences

[3]On-site outsourcing events are defined as large flows of workers from a direct employer to a contract firm, while occupational layoffs are sudden drops in security guard employment without corresponding drops in employment of other occupations at the establishment level.

regression specification that compares guards to less-affected occupations in restrictive versus permissive regions, before versus after legalization. We also reweigh permissive microregions to be similar to restrictive microregions in mean pre-legalization characteristics, such as crime rates, unemployment rates, and local exposure to concurrent tariff reductions.

We find that Brazil's outsourcing legalization persistently reallocated jobs from older incumbent workers to younger entrant workers. We estimate that outsourcing legalization caused the employment of security guards between the ages of 18-24 to persistently increase by roughly 50 percent. Meanwhile, the employment of security guards between ages 55-64 persistently declined by roughly 15 percent. To our knowledge, this is the first paper to show that outsourcing benefitted some workers, particularly the young, even as it may have harmed others within the same occupation.

These reallocation effects are not due to pre-existing differences in the evolution of local occupational labor markets, since there are no differential pre-legalization trends, and the increase did not coincide with changes in crime rates that may drive demand for security services. They are highly robust to using inverse propensity score weights, entropy-balancing weights, and regression adjustment to account for any potential confounding trends.

They are instead explained by the fact that contract firms persistently employ demographically different workers than direct employers. Even at the aggregate level, despite the enormous increase in outsourcing, the average age of outsourced guards is persistently roughly 5 years younger than that of direct-hire guards. This suggests that contract firms tend to hire and attract much younger workers due to their organizational structure.

In addition to large changes in employment composition, we find that Brazil's outsourcing legalization increased total employment of security guards by 5%. Roughly half of the employment increase was driven by workers coming from outside formality. Outsourcing legalization also did not reduce the composition-adjusted wage in the occupation of security guards as a whole.

Section 5 interprets these results through a stylized framework wherein older and younger workers supply labor to either the outsourcing or direct-hire sectors. In this model, security services in two sectors are imperfect substitutes in production and are produced with different fixed proportions technology. Since outsourcing legalization reduces the legal cost

of outsourcing, it induces substitution from the direct-hire sector to outsourced sector, and thereby shifts the demographic composition of the occupation as a whole. Given estimates of elasticities of substitution and demand, the model allows us to directly infer changes in worker, firm, and consumer surplus from our reduced-form estimates. For a broad range of elasticities, our results imply net welfare gains to consumers from outsourcing legalization, with only limited effects on net worker welfare despite significant job redistribution.

1.1 Related Literature

Our study contributes to a rapidly growing literature on the impact of regulatory changes to domestic outsourcing. Most relatedly, Bertrand et al. (2021) study a court-ordered relaxation of a ban against using contract labor in India, and show that it increased economic performance by allowing large manufacturing firms to avoid firing costs and scale up production. Estefan et al. (2024) examine Mexico's 2021 ban on outsourcing of *core* activities, which allowed firms to evade wage and employment regulations, and show that the reform increased wages and non-wage compensation, reduced investment, increased firm exit, with no detectable effects on employment, revenues, or the use of other inputs.[4]

Our work is different from the above two studies in three main aspects. First, we focus on the outsourcing of a *non-core* activity, for which there are more obvious efficiency advantages arising from economies of scale in human resources at the contract firm. Second, we use comprehensive worker-level administriative data rather than firm-level survey data. Third, we leverage geographical and occupational variation in the bindingness of regulations. Our methodological innovations enable us to estimate market-level effects on different worker segments with much greater precision.

Our finding of significant job reallocation from older incumbent workers to younger entrant workers is novel to the domestic outsourcing literature. While multiple studies have documented the role of firm-level premia in mediating the wage effects of outsourcing (e.g. Goldschmidt and Schmieder 2017; Drenik et al. 2023), the literature has not previously

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

shown that firm-level employment structure is also a mediator of the employment effects of outsourcing regulations. This finding shows that firm-level personnel policies can have market-level employment consequences. It also provides rare evidence for insider-outsider theories, which emphasizes that labor market institutions may benefit incumbent workers at the expense of entrant workers (Lindbeck and Snower, 1989; Saint-Paul, 2002).

Other strands of the literature on domestic outsourcing are also closely connected. For example, early literature explores the determinants of firm-level decisions to outsource (Abraham and Taylor, 1996; Houseman, 2001; Autor, 2003; Berlingieri, 2014; Chaurey, 2015; Espinosa, 2020).[5] A more recent literature estimates outsourcing wage differentials in low-wage occupations, holding worker employment and local labor market conditions constant (e.g., Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2023; Guo et al. 2024).[6] An emerging literature uses general equilibrium models to study the aggregate consequences of domestic outsourcing on output and inequality (Bilal and Lhuillier, 2021; Spitze, 2022; Bostanci, 2022).[7] None of this work, however, has considered the possibility that outsourcing may change the demographic composition of occupational labor markets.

Our study also relates to the literature on labor market regulation. Studies have shown that labor market regulation is associated with lower output, employment, and productivity (Besley and Burgess, 2004; Botero et al., 2004; Aghion et al., 2008). However, partial labor market reforms in Europe, which lifted constraints on fixed-term employment but maintained

[5]Research shows that labor service outsourcing is more likely when firms have fluctuating labor demand (Abraham and Taylor, 1996; Houseman, 2001), require specialized services (Berlingieri, 2014; Espinosa, 2020), or face high firing costs (Lee, 1996; Autor, 2003; Chaurey, 2015). Autor (2001) documents that temp-help agencies help screen and train workers. Battiston et al. (2021) studies the patterns of job rotation, experience accumulation, and talent poaching in a Columbian security service firm. Kalleberg (2000) surveys relevant research in sociology. Autor (2009) discuss a broader literature on labor market intermediation. Weil (2014) offers a detailed and largely qualitative analysis of the business practices of domestic outsourcing in the United States. Bernhardt et al. (2016) and Abraham et al. (2018) discuss data challenges for measuring outsourced work and alternative work arrangements.

[6]In a closely related working paper, Guo et al. (2024) use the same data but study a different time period, and show that outsourced cleaners and security guards in Brazil have reduced transitions to unemployment than comparable direct-hire workers; they attribute this finding to increased ease of reassigning workers across firms.

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

employment protection for workers under permanent contracts, had the perverse effect of increasing turnover among young workers without boosting employment (Bentolila and Saint-Paul, 1992; Cahuc and Postel-Vinay, 2002; Blanchard and Landier, 2002; García-Pérez et al., 2018; Daruich et al., 2020). The fact that Brazil's 1993 outsourcing legalization led to large losses for incumbent workers and large increases in employment, especially among the young, offers a useful counterpoint to the European experience. It suggests that reforms that allow outsourcing of *non-core* economic activities and provide indefinite employment through contract firms generates both larger efficiency gains and better outcomes for young workers than reforms that enable the use of fixed-term contracts without intermediation.

2 Institutional Setting and Data

2.1 History of Outsourcing in Brazil

Outsourcing emerged as a new business practice of uncertain legality in Brazil during the second half of 20th century. In 1967, the Brazilian dictatorship issued Law-Decree 200, which allowed government bodies to outsource non-governmental functions, but had no provisions about 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 to this question determines which firm is responsible for compliance with Brazilian labor regulations regarding the outsourced worker's pay, benefits, and employment protection.

As lawsuits involving third-party contracting emerged, in 1986 the Superior Labor Court issued *Enunciado 256*, a one-paragraph precedent stating that the Court understood the practice of outsourcing to be illegal except for cases permitted by legislation.[8] Regional differences in judges' stance on the legality of outsourcing nevertheless persisted, a phenomenon we discuss in Section 2.2 and leverage for our empirical strategy in Section 4.1.

The uncertain legality of most outsourcing practices finally came to an end on December

[8]The exceptions were: a) Outsourcing by government (Law-Decree 200); b) Temporary work of demonstrated need and no more than 3 months (Law 6.019 of 1974); and c) Banking security (Law 7.102 of 1983, which required banks to offer safe storage and operational facilities to its clients.)

17, 1993. Following an unanticipated series of events, the Superior Labor Court issued *Súmula 331*,[9] a detailed and sweeping precedent that declared outsourcing of all non-core activities by any firm to be legal. Henceforth, outsourced workers would be considered legal employees of the intermediary firm so long as the service provided by the worker was deemed a *non-core activity* of the client firm.[10]

As a consequence of *Súmula 331*, the *expected* legal cost of outsourcing workers sharply fell. Prior to legalization, a firm in a region where judges considered outsourcing illegal was discouraged from outsourcing because, should an outsourced worker sue them for *any* reason, the firm can 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 prior to legalization, the high *expected* legal costs of outsourcing likely discouraged many firms from doing so.

2.2 Regional Variation in Pre-legalization Interpretation

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

On the restrictive side, judges interpreted *Enunciado 256* as establishing a principle of illegality on outsourcing. This implied that even some exceptions listed in *Enunciado 256*—such as banks being allowed to outsource security—were also illegal. According to a regional labor court justice at the time:

> *"Security guards were being replaced by guards contracted now via these firms... Our understanding was that the exceptions made under 256 were not applicable here... so I recognized the employment link directly with banks."*

[9] 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.

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

On the permissive side, courts' understanding of outsourcing and general leniency towards it could not be more different. In the words of a union leader in the city of São Paulo:

> *"[T]he high frequency of lawsuit losses ended up wearing down the Unions, because as we could not win lawsuits the employers made sure to promulgate: 'you see! The labor court considers outsourcing legal!'* "

Appendix Tables A.2 and A.3 provide many more of these quotes, taken from transcripts of interviews with former regional court justices, judges, lawyers, and union leaders.[11]

Southern courts' restrictiveness towards outsourcing is also reflected on its regional labor courts' legal precedents. The appeals concerning outsourcing made to the Superior Labor Court prior to Súmula 331 show that Southern courts tended to recognize end-firms as the legal employer.[12] They also indicate that outsourcing tended to be more litigated in the South.

Combined, the interviews and legal precedents point to very restrictive interpretations in Brazil's geographic South,[13] including the states of Rio Grande do Sul (4th regional court), Paraná (9th), and Santa Catarina (12th), and a restrictive—though to a lesser extent—interpretation in the countryside of the state of São Paulo (15th region, Campinas),[14] in contrast with the permissive city of São Paulo (2nd region). We therefore classify regional

[11] Transcripts were generously provided by Magda Biavaschi (former Regional Court Justice and jurist) and Alisson Droppa (a legal historian), who between 2008 and 2011 interviewed judges, lawyers, and other parties involved in key lawsuits about outsourcing at various regional courts, for a research project on regional differences in courts' stance on outsourcing prior to Súmula 331 (i.e., Barros Biavaschi and de Andrade Baltar 2013). We also separately interviewed Dr. Biavaschi and a current Regional Court Justice a North region. Appendix A discusses how we learned of the regional differences in Courts' interpretation and gathered its supporting evidence.

[12] 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 is provided in Appendix Table A.4.

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

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

courts 4, 9, 12, and 15 as being restrictive towards outsourcing prior to legalization,[15] and the remaining regional labor courts as permissive.

Appendix Table A.1 summarizes our classification of Brazil's 24 regional labor courts. Columns (5) and (6) summarize the information from legal precedents,[16] while columns (7)-(9) summarize the information from available interview transcripts.[17] Columns (10)-(12) then show the prevalence of outsourcing of security guards before and following legalization for each region.

Figure 1 Panel A shows that, as of December 1992 (the year prior to legalization), roughly 32% of security guards in restrictive regions were employed by contract firms, compared to 38% at permissive regions. By December 1999, this gap had been fully closed, with contract firms accounting for 55% of all security guard employment in restrictive regions, compared to 53% at permissive regions. The increase in the prevalence of outsourcing in restrictive regions was a stark break from an otherwise flat trend. This is different from the pattern seen in permissive regions, where outsourcing experienced secular growth throughout the period, with no apparent trend breaks following legalization. Appendix Figure C.3 displays regional-court-specific trends.

2.3 Our Focus: Security Guards

We focus on security guards because restrictions on outsourcing were particularly binding for guards prior to its legalization. Relative to other occupations, guards are primarily formal,[18]

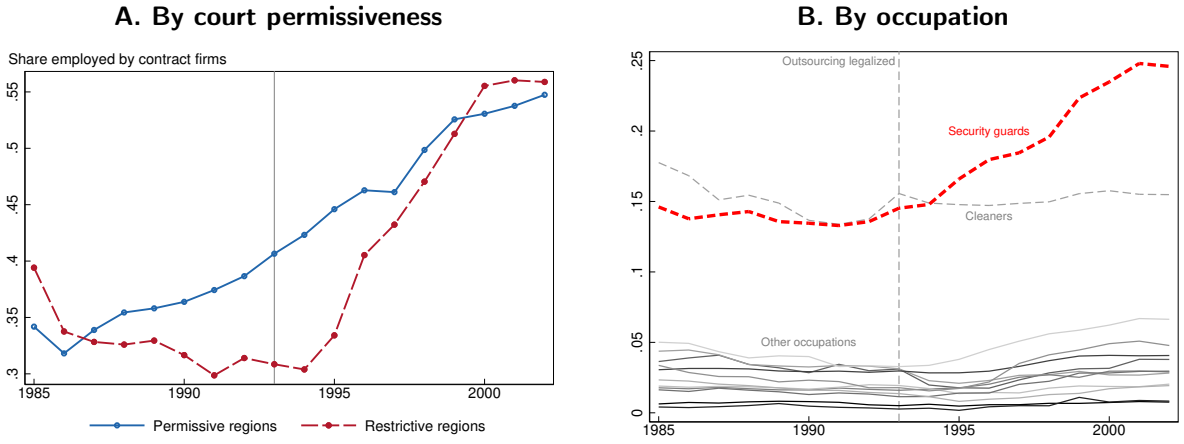
[15] Given our uncertainty about the correct classification of the 15th region, we show that our local labor market results are robust to dropping observations from the São Paulo state (see Appendix Table F.4).

[16] Based on our review of individual cases appealed to the Superior Court and cited by Súmula 331, which are publicly available. We petitioned all regional labor courts to obtain copies of individual case records concerning outsourcing and ruled in the years preceding legalization (1986-1993), but we could not obtain the records. For some courts the records no longer exit, for others they exist but are neither indexed by topic nor are they digitized. See Appendix A for details.

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

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

Figure 1: Trends in contract-firm share



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

face stricter licensing,[19] and have strict training requirements.[20] These factors facilitate monitoring of the occupation and ultimately the enforcement of local courts' decisions on security outsourcing.

Consistent with particularly binding restrictions, guards were the only major occupation to experience a large rise in outsourcing after legalization, as shown in Figure 1 Panel B. Each line shows the trend in the share of private-sector workers employed in contract firms (i.e., the "contract-firm share") for each occupation,[21] averaged across microregions. Before 1993, there are two occupations – security guards and cleaners – whose contract-firm share

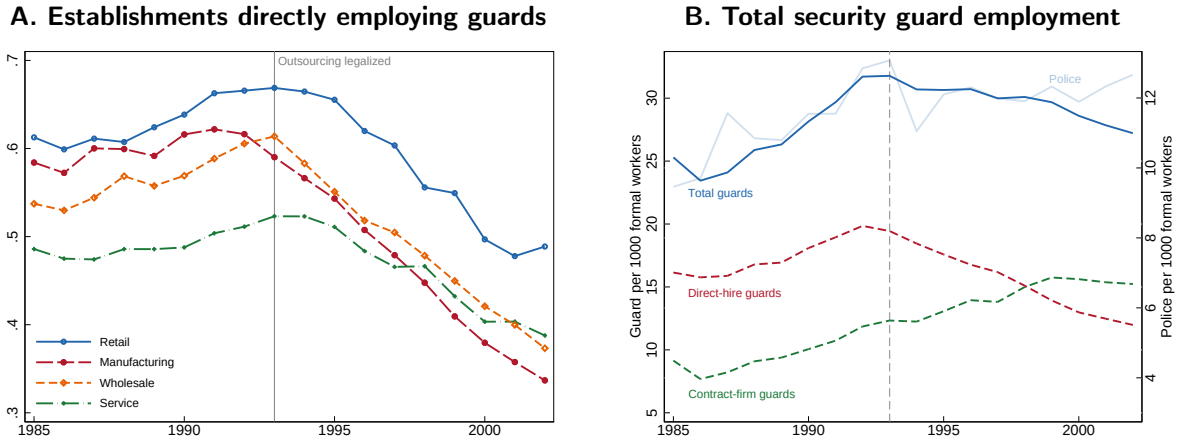
costs in Section 5.

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

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

[21]We use 2-digit occupation codes to identify broad occupational groups. Security guards are identified as private sector workers under CBO 2-digit occupation code 58, "Security and public safety workers." We exclude police officers (CBO 3-digit code 583) from our definition security guards as police officers are public sector workers. We use the CBO and CBO94 occupation codes that are consistent for the period 1985-2002, prior to a major revision in occupation codes in 2003 (CBO02 codes). See Appendix Table B.1 for the list of 2-digit large occupations included as comparison occupations in local labor market analyses.

Figure 2: Trends in security guard employment



Note: Panel BA plots the share of establishments with at 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 guard, the number of direct-hire guards, and the number of police (aged 18-64) per 1000 formal-sector workers in Brazil over time.

far exceeds that of other occupations. In the years immediately following legalization, the contract-firm share indeed rose by almost 10 p.p for security guards, from an average of 15 p.p. By contrast, the contract-firm share for cleaners hardly changed.[22]

Guards are also an economically important occupation in Brazil. An overwhelmingly male and relatively high-paid occupation,[23] guards accounted for 3.1 percent of total private-sector formal employment in 1993, being employed across various sectors. Figure 2 shows that all of these sectors appear to have been affected by the rise of security service outsourcing.

Panel A of Figure 2 plots the trend in the share of establishments with at 50 employees that employ at least one security guard, separately for the manufacturing, services, wholesale, and retail sectors. Across all four sectors, the share was generally steady before 1993, but began to fall sharply beginning around 1993.[24] In 1992, about 60 percent of wholesale

[22]Section 3 documents a large and long-lasting increase in the frequency of occupational layoffs for security guards after legalization, but the analogous increase for cleaners was smaller and short-lived. This could be due to cleaners being primarily employed in the informal sector, where regulatory changes are less binding.

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

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

establishment had at least one security guard on staff. By 2002, less than 40 percent did. Since Panel B shows a concurrent increase in contract-firm employment, many of these firms must have contracted out their needed security services, and thus no longer directly employed security guards.

Table 1 reveals that despite the enormous increase in outsourcing, the average ages of direct-hire and outsourced security guards, respectively, were essentially unchanged throughout. The average age of direct-hire guards is persistently 40 years old, while that of outsourced workers is persistently 35 years old. Outsourced workers are also persistently have fewer years of tenure. Moreover, we find that contract firms operate large internal labor markets with a large number of security guards, who are most likely flexibly allocated to different clients.

2.4 Data and Measurement of Outsourcing

We use Brazil's employee-employer 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), as well as detailed industry and occupation codes. Following standard practice in the literature, we focus on full-time private-sector workers aged 18-64.

Our measurement of outsourcing uses the fact that RAIS includes specific industry codes for contract firms.[25] This allows us to identify outsourced workers as those employed by contract firms, and direct-hires as those employed by any other private-sector firm. Finally, the high degree of formality among security guards allows us to track both incumbents and newcomers using employer-employee links.

Despite its richness, RAIS has three important limitations. First, while our analysis focuses on a primarily formal occupation—security guards—RAIS lacks information on workers

[25]See Appendix B for the 5-digit codes of contract firms, all of which fall under occupational class 74 "Serviços prestados principalmente às empresas." To consistently classify the industry of establishments over time, we use crosswalks along with our best judgement.

who are not formally employed. Thus, while we test whether outsourcing pushed incumbent workers out of the formal sector, we cannot discern whether this transition was to unemployment or to informality. Second, RAIS does not include information on where outsourced workers are posted. As a result, we cannot and do not focus on estimating "outsourcing premia," that is, the wage difference between being outsourced versus directly hired for the same worker performing the same job at the same firm.[26] Finally, RAIS lacks information on non-wage components of compensation (such as access to employer-based private health insurance).

3 Firm-level Outsourcing Events

In this section, we present three findings on firm-level outsourcing events in Brazil. First, outsourcing legalization in 1993 led to a large wave of occupational layoffs, while on-site outsourcing was rare throughout the study period. Second, occupational layoffs temporarily reduced the employment of incumbent security guards and persistently reduced their wages, resulting in an average loss equal to slightly more than one year of earnings. Most of these workers do not return to formal employment as security guards, being reallocated to other occupations. Third, high-wage firms were more likely to have occupational layoffs following legalization and the loss of firm wage premia substantially explains the decline in wages for incumbent workers affected by the layoffs. These findings reveal that outsourcing likely had displacement and reallocation effects that the previous literature has not documented.

3.1 Incidence of Occupational Layoffs and On-site Outsourcing

Since the existing evidence on outsourcing is based on firm-level outsourcing events,, we first ask: Did outsourcing legalization lead to a rise in firm-level outsourcing events? To identify outsourcing events from the data, we follow the methods proposed by Goldschmidt and Schmieder (2017) and examine both *on-site outsourcing events*, wherein a large number of workers flow from a direct employer to a contract firm but presumably continued to perform

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

the same job, and *occupational layoff*s, wherein an establishment drastically reduces their number of direct employees in a given occupation, while other occupations are seemingly unchanged.

We find that on-site outsourcing events were very rare in Brazil. Between 1990 and 2000, we identify a total of 107 on-site outsourcing events in the security guard occupation, defined as the flow of at least three security guards from a direct employer to a security services establishment.[27] These events affected 2,842 security guards, about 0.7 percent of the nearly half a million security guards in the country.

The rarity of on-site outsourcing is noteworthy, since prior literature uses on-site outsourcing events to estimate the effects of outsourcing. A potential reason for the rarity of on-site outsourcing events is that Brazil prohibits nominal wage reductions for continuing workers, which is generally understood to include the firing and rehiring workers through an intermediary to perform the same job but at a lower wage.[28]

Occupational layoffs are much more common and can account for a large share of the decline in direct-hire employment after outsourcing legalization. We define occupational layoffs as a two-thirds reduction in the number of workers in a specific occupation from an establishment with at least three workers in the occupation, excluding establishments where non-guard employment fell by more than 10 percent, as detailed in Appendix D.1.[29] Between 1990 and 2000, the number of occupational layoffs averaged 471 per year and affected a total of 35,544 security guards, about 8.4 percent of the security guards in the nation, and about 72 percent of the decline in direct-hire security guard employment during this time.

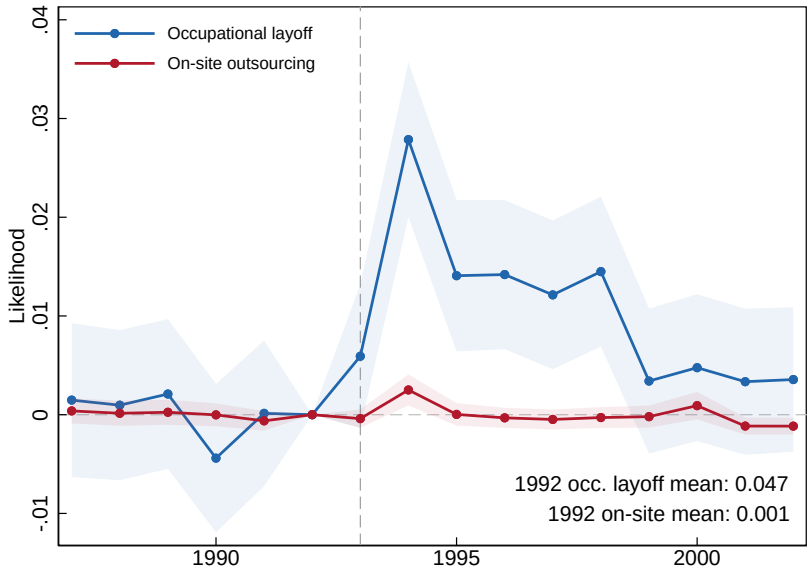
Figure 3 shows that there was a large wave of occupational layoffs immediately after legalization and hardly any increase in on-site outsourcing. It plots estimates of the establishment-level likelihoods of an occupational layoff or an on-site outsourcing event in each year. The likelihood of both are stable prior to 1993, the year of outsourcing legalization. The inci-

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

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

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

Figure 3: Occupational layoffs vs. on-site outsourcing events following legalization



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

dence of on-site outsourcing only mildly increases in the year after legalization. However, the incidence of occupational layoff rises sharply by 2.5 p.p. in 1994, the year after legalization. This increase is more than half the pre-legalization level. The elevated level persists for several years before slowly falling back towards baseline. As shown in Appendix Figure E.1, the increase was larger in Brazil's South, where courts were more restrictive towards outsourcing prior to legalization, than in the rest of Brazil.

3.2 Effects of Occupational Layoffs on Incumbent Workers

Since outsourcing in Brazil primarily takes the form of occupational layoffs, we next ask: How do occupational layoffs affect the employment and earnings of incumbent security guards? To answer this question, we compare long-tenured security guards who were directly affected by an occupational layoff between 1990 and 2000 to similar security guards who were unaffected

by such events. Our treatment group includes all security guards who did not continue their jobs as direct-hire security guards when their employer eliminated a large portion of such jobs. These treated workers could either separate from the establishment or be reassigned to another occupation within the establishment.[30] We construct a control group consisting of similar security guards in a similar non-outsourcing establishment using a matching algorithm, as described in Appendix D.2.

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

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

where y_{it} is the outcome of security guard i in year t (e.g., employment status or wage), D_i indicates if the security guard was outsourced in year t^*,[31] X_{it} are demographic controls, and ϵ_{it} is a residual term. Each coefficient δ_k is the effect of an occupational layoff on an incumbent direct-hire worker k years since the layoff, relative to their matched pair at an non-outsourcing firm. Note, therefore, that these do not capture the equilibrium effects of outsourcing legalization for the security guard occupation as a whole (which includes both non-outsourced incumbents and new workers entering the occupation), which we investigate in Section 4.

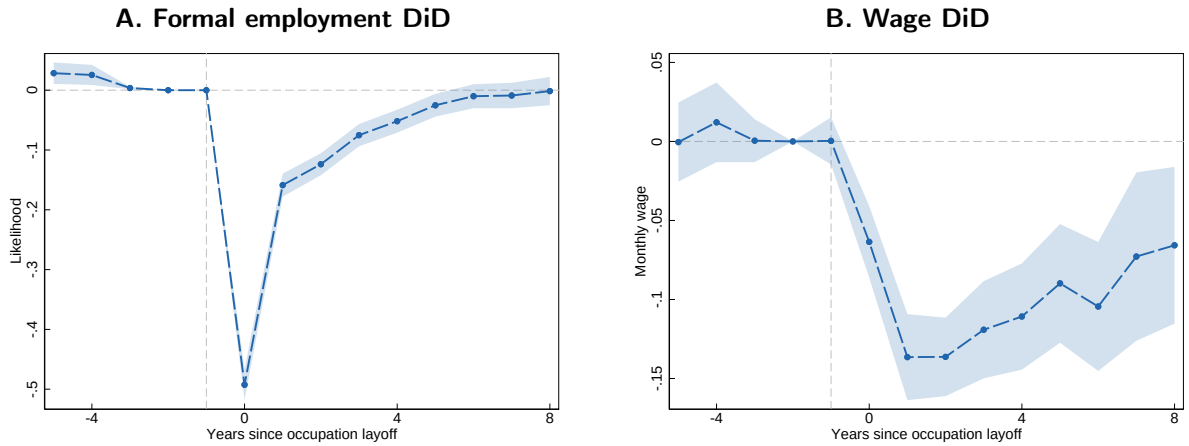
Figure 4 Panel A shows that outsourcing decisions significantly displace incumbent security guards from formal employment. In the year after an occupational layoff, an affected security guard is 49 p.p. less likely to be formally employed. However, the reduction in formal employment is transitory. Five years after an occupational layoff, there is no detectable effect on the likelihood of formal employment.

Panel B shows that occupational layoffs also persistently reduce the wages of incumbent security guards. Wages are hardest hit in the year following occupational layoffs, by 18

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

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

Figure 4: Effect of occupational layoffs on incumbent workers



Note: Panels A and B plots coefficients γ_τ from a difference-in-differences regression measuring the impact of an occupational layoff on incumbent direct-hire security guards, where the control group are similar workers in establishments that did not have an occupational layoff. Our sample includes all occupational layoffs, as identified by sudden drops in an establishment security guard count, between 1990 and 2000. We include controls for individual and year fixed effects, and time-varying demographics. Shaded bands indicate 95% confidence intervals, with standard errors clustered at the establishment level.

percent, and never recover to pre-layoff levels. Five years following an occupational layoff, security guards are still paid 12 percent less than they would have been counterfactually paid had the occupational layoff event not happened. Since by this time, the formal employment rates of the treated and control groups no longer exhibit any detectable differences, worker selection is unlikely to explain the drop in wages. As shown in Appendix Figures E.6 and E.7, these results are robust to alternative matching strategies and definitions of an occupational layoff. Finally, while legalization increased the frequency of outsourcing in Brazil's restrictive South by more than in the permissive North (e.g., see Appendix Figure E.1), Appendix Figure E.5 shows that outsourcing events have similar effects on incumbents' likelihood of employment and wages in either region.[32]

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

[32]See Appendix Figures E.6 through E.9 for event study effects using alternative matching strategies, alternative definitions of occupational layoffs, and effects of occupational layoffs on employment and wages of other occupations.

the same occupation. Even five years later, impacted workers are less likely to formally employed in the same occupation (by 12 p.p), though they are no longer less likely to be formally employed. By contrast, impacted workers are only 0.2 p.p more likely to be formally employed by a contract firm immediately after an occupational layoff. The effect of occupational layoffs on contract-firm employment rises to 7.7 p.p in the following year, and is 11.7 p.p five years after. Despite the large number of workers leaving the occupation, our finding of a persistent wage decline for incumbents is nearly all driven by workers who stay within the occupation, as shown in Appendix Figure E.2.

Appendix Table D.2 shows that the implied present discounted value (PDV) of earnings losses from experiencing an occupational layoff is roughly 1-1.4 years.[33] If we assume that workers earn nothing if unobserved, as described in Appendix D.3, then the workers lose 1.40 years of earnings of average pre-occupational-layoff earnings. If instead we assume they have the same earnings as observed workers, then they lose roughly 1.06 year of earnings. Regardless of the imputation method, the PDV of earning losses appear to be substantial. This suggests that the persistent wage reductions account for much of the total earnings losses. The magnitude of earnings losses is similar to that of job displacement in the U.S. provided by Davis and von Wachter (2011), who report earnings losses equivalent to 1.4 years of pre-displacement earnings in non-recession years.

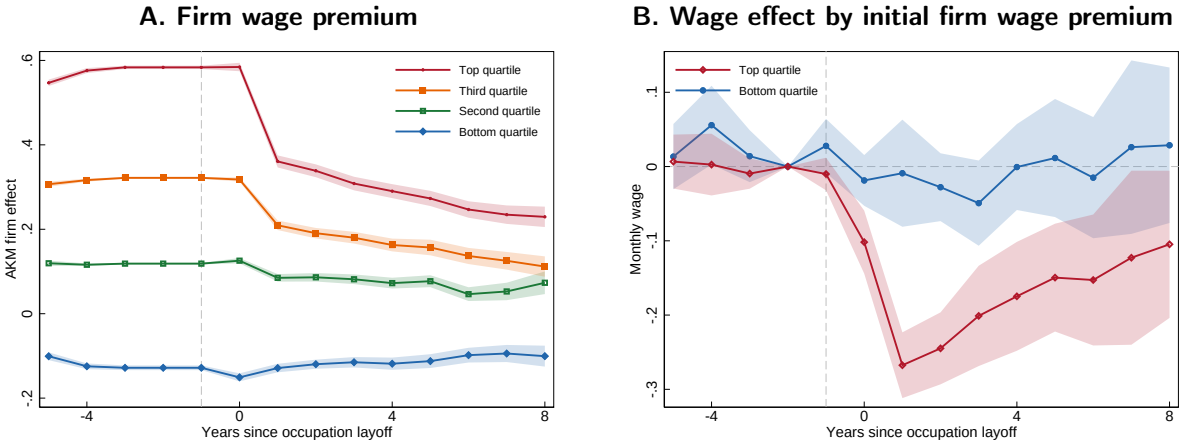
3.3 The Role of Firm-specific Wage Premia

A prominent idea in recent economic literature is that firms outsource in order to exclude workers from the wage premia they share with direct employees (Dube and Kaplan 2010; Weil 2014; Goldschmidt and Schmieder 2017). For example, some firms may be required by collective bargaining agreements, which are typically negotiated by sector and region in the Brazilian economy, to pay high wages to employees. Highly profitable firms may also face pressure to pay workers a wage premium in the interest of fairness or equity. A firm may avoid these requirements and pressures by moving workers outside its boundary.[34]

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

[34] Larry Katz articulates this view as follows: "When janitors work at Goldman Sachs as Goldman Sachs employees, they tend to share in the firm's huge productivity benefits and huge rents. But if they work for

Figure 5: Incumbent wage reduction is related to loss of firm wage premia



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

To investigate whether the desire to reduce firm-level worker rents drive firm outsourcing decisions, we first estimate firm-level wage premia using by decomposing log wages into worker and firm fixed effects following Abowd et al. (1999) (henceforth, AKM), as detailed in Appendix D.4. Then, we use the estimated firm fixed effects to answer three questions: Do workers experiencing outsourcing decisions transition to lower-wage firms? Do workers initially at high-wage firms experience larger wage declines when their employer decides to outsource? Are high-wage firms more likely to outsource?

Figure 5 Panel A shows that that incumbent workers earn generally stable AKM firm effects prior to occupational layoffs, but this changes after occupational layoffs. Incumbent workers initially at the top quartile transition to firms with much lower firm wage premia. Their average AKM firm effect falls by almost 20 percent. By contrast, workers initially at lower-wage firms do not experience a significant change in the firm wage premia they are paid.

Joe's Janitorial Services, they no longer share in those rents" (Clement 2017).

Panel B shows that workers initially employed by the top quartile firms experience a large level decline in wages following occupational layoffs.[35] We detect no statistically significant effect on wages for workers initially at establishments in the bottom quartile of the AKM firm effects distribution. The loss of firm-specific wage premia explains 42 percent of the total wage losses in the year of the occupational layoff, 43 percent one year after the occupational layoff, and 46 percent five years after (see last line of Table 2). These numbers suggest that changes in firm wage premia explain a substantial fraction of incumbent wage losses.[36]

High-wage establishments were also more likely to outsource. As shown in Appendix Table E.1, outsourcing decisions are more likely to be taken by firms with higher AKM firm effects, higher mean wage in 1993, and higher mean security-guard wage in 1993. These findings are broadly consistent with Goldschmidt and Schmieder (2017).

However, outsourcing legalization did not reduce the average firm wage premia in the security guard occupation. As shown in Appendix Figure E.4, the average firm effect among security guards was falling both before and after outsourcing legalization in both regions, but we do not detect any trend break in 1993, when outsourcing was declared legal. This finding suggest that the market-level effects may be different from effects estimated from firm-level outsourcing decisions. One reason for this discrepancy may be that the firms that experienced occupational layoffs, as identified from sudden drops in worker counts, tend to be larger and higher-wage firms, so their workers were more likely to be reallocated to lower-wage firms following the layoffs.

4 Market-level Effects of Outsourcing Legalization

This section presents our empirical strategy for and estimates of the effects of outsourcing legalization on local labor markets for guards. We find that legalization (i) increased the

[35] See Appendix Figure E.3 for effects on the likelihood of employment. Workers who were in the top quartile of the AKM fixed effect distribution at baseline are more likely to be disemployed (i.e., leave the data, either to informality or unemployment) on impact, but return to the formal sector at similar rates as workers who were in the bottom quartile at baseline.

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

prevalence of outsourcing, (ii) increased guard employment, and (iii) reallocated guard jobs from the old to the young, changing the occupation's demographic composition. We find no effects on demographic-adjusted wages, and a very small but statistically insignificant negative effect on raw wages (i.e., wages without controlling for demographics or worker fixed effects). All effects are long-lasting.

4.1 Empirical Strategy

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

$$y_{ort} = \beta \left(T_{or} \times 1_{t>1992} \right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \epsilon_{ort}, \quad \text{(DDD)} \tag{2}$$

where y_{ort} is 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 occupation-microregion 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 cluster standard errors two-ways by these two dimensions.

The β coefficient in equation 2 is the average effect of legalization on guards in restrictive regions. This is the coefficient we report, separately estimated for various outcomes and under different sample weighting methods (more below) in Tables 3 through 6. As in standard DDD identification strategies, causal interpretation of β requires the assumption of parallel trends between treated and untreated units. That is, absent legalization, the outcomes of guards in restrictive regions would have followed similar trends as the control group's.

While the parallel trends assumption cannot be tested on counterfactual outcomes, we provide evidence that treated and untreated units followed parallel trends prior the reform

using a standard year-by-year triple-differences regression:

$$y_{ort} = \sum_{\tau=1985;\tau\neq1992}^{2002} \beta_\tau \left(T_{or} \times 1_{t=\tau}\right) + \delta_{or} + \delta_{ot} + \delta_{rt} + \epsilon_{ort}, \quad \text{(Year-by-year DDD)} \qquad (3)$$

The β_τ coefficients in equation (3) break down the average treatment effect β from equation 2 into year-specific coefficients, presenting them as effects relative to the baseline year of 1992. These are the coefficients we report in Figures 6 through 8.

It is worth highlighting the strengths of a triple-differences research design in the context of Brazil's outsourcing legalization. One could have instead considered a simpler difference-in-differences design that compares restrictive versus permissive regions, but ignores the importance of comparing guards to other occupations. Alternatively, one could have considered a difference-in-differences design that compares guards to other occupations, but ignores the importance of comparing restrictive to permissive regions.

The issue with either difference-in-differences design is the presence of national-level trends in demand for security services,[37] and contemporaneous policies that differentially affected labor demand across regions, such as trade liberalization (1990-1994) and Plano Real (1994), a price stabilization reform.[38] We present these differential trends in Appendix Figures F.2 through F.4, discussing them in detail in Section 4.2. Instead, our triple-differences design allows to control for diffferential trends in employment across occupations with occupation-year fixed effects δ_{ot}, and for differential trends in employment across regions with region-year fixed effects δ_{rt}.

The key threat to identification in the triple-differences design is therefore the presence of contemporaneous policies that might have differentially affected *guards in restrictive regions* relative to other occupation-region pairs. In the Brazilian context, the main policy of concern is trade liberalization, which *reduced* employment and wages and temporarily increased crime in regions more exposed to import competition (e.g., see Kovak 2013 and Dix-Carneiro

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

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

et al. 2018). Many of these regions were in the Southeast and South, overlapping with the jurisdiction of restrictive labor courts. While our findings suggest that outsourcing legalization *increased* employment and wages for guards in the South, some of this effect could have been driven by increased crime rates due to the negative effects of trade in the region.

To address this concern, equations (2) and (3) are estimated on a weighted sample, where the weights balance restrictive and permissive regions on their pre-liberalization exposure to import competition and other baseline covariates that might have induced differential effects of import competition exposure on the labor demand for guards in restrictive regions (i.e., homicide rates, unemployment rates, and total formal sector employment).[39] We do this by first estimating entropy-balancing weights, following the method proposed by Hainmueller (2012),[40] and then weighting each observation by its corresponding weight. Our weighted triple-differences regression follows other papers where identification relies on parallel *trends*, but treated and control units differ on baseline *levels* on variables potentially correlated with treatment assignment (e.g., see Basri et al. 2021, whose method is weighted difference-in-differences with entropy-balancing weights).[41] We then report robustness to alternative weights and baseline covariate controls.[42]

Finally, because security guards are not employed in all microregions—many of which are rural—we estimate equations (2) and (3) on a balanced sample of 265 microregions and other large 2-digit occupations present in all microregions.[43] Appendix Table B.1 lists these 11 comparison occupations and their characteristics. Our estimation sample covers roughly 400,000 security guards per year (or 98 percent of all formal sector guards) and roughly 8.5

[39] Import competition exposure from Felix (2021) and homicide rates from Dix-Carneiro et al. (2018).

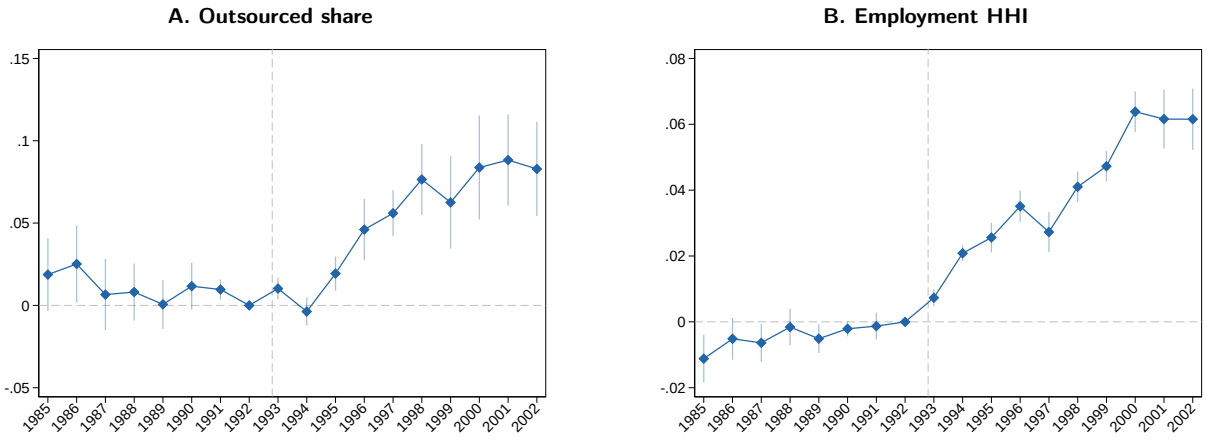
[40] See Appendix Figure F.1 for a display of the entropy-balancing weights on a map.

[41] Weighting observations reduces differences between restrictive and permissive microregions even for characteristics that are not directly targeted (see Appendix Table C.1).

[42] We also report estimates using a combination of weighting and regression adjustment, which are known to be "doubly robust" to misspecification (Glynn and Quinn 2010).

[43] We construct our balanced microregion-occupation estimation sample as follows. We select all microregions with at least 30 security guards and cleaners in every year. We then select all occupations with at least two workers in every year for all selected microregions. These restrictions yield a set of microregions with similar occupational composition and in which contract firms could have in principle operated, given the underlying demand for guards and cleaners throughout the period. Finally, we exclude cleaners due to the occupation's high contract-firm share prior to legalization but lack of law bindingness (as shown in Figure 1), potentially driven by high levels of informality.

Figure 6: Effect of outsourcing legalization on outsourcing prevalence



Note: This figure plots the coefficients β_τ from the triple-differences regression measuring the impact of outsourcing legalization in equation (3). 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.Employment HHI is the Herfindahl-Hirschmann employment index in a microregion-occupation pair. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation.

million control occupation workers per year (over 50 percent of total private sector formal employment).

4.2 Findings

Legalization effect on the prevalence of outsourcing

Figure 6 plots coefficients β_τ of equation (3) for the effects of outsourcing legalization on the prevalence of outsourcing. We consider two measures of outsourcing prevalence: a) the outsourced share of employment (Panel A), measured as the share of workers employed at a contract firm in a microregion-occupation pair; and b) the employment concentration (HHI) in a microregion-occupation pair (Panel B).[44] Both measures show that outsourcing prevalence sharply rose following legalization.

[44]While the outsourced share of employment is the most direct outcome for outsourcing prevalence, our measurement of its response to legalization might be slightly delayed if firms in restrictive regions take time to change their legal economic activity status to contract firms. We therefore also report effects on the concentration of employment (HHI) in a microregion-occupation pair, since this measure is not affected by each firm's economic activity status. HHI is a number ranging from zero (inifinitely many firms of similar size) to one (all employment is concentrated in a single firm).

Figure 6 shows that the prevalence of outsourcing among guards sharply rose in restrictive regions, relative to permissive regions, following legalization, with no differential pre-trends. Prevalence increased steadily through 1999, leveling off afterwards. Table 3 reports the corresponding average effect over the post-legalization years (i.e., coefficient β in equation (2)). Column (2) presents our main specification, with entropy-balancing weights. It shows that legalization increased the outsourced employment share increased by 4.2 percentage points per year on average (or a 135% increase relative to the baseline sample mean of 3.1%),[45] and increased restrictive local labor markets' guard employment HHI by 0.043 points (a 68% increase relative to the baseline mean of 0.063). Columns (3)-(4) show that these effects are similar across alternative weighting methods.

A helpful way to visualize the DDD effects in Figure 6 is to note that a DDD estimate is simply a more flexible (i.e., allowing for more controls) difference between DD estimates. We can therefore visualize it by plotting, for example, outcome differences between guards vs. others (i.e., the guard DD effect, or the "law bindingness" dimension of the legalization effect), separately for restrictive and permissive regions. This is one way to interpret the DDD effect, namely as the "law bindingness" effect in restrictive regions minus its corresponding effect in permissive regions. Alternatively, we can plot outcome differences between restrictive vs. permissive regions (i.e., the region DD, or the "court restrictiveness" dimension of the legalization effect), separately for guards and other occupations. This is yet another way to interpret the DDD effect, namely as the difference between the "court restrictiveness" effect on guards minus its corresponding effect on control occupations.[46]

Appendix Figure F.2 shows the DD visualizations corresponding to the outsourcing prevalence outcomes in Figure 6. The DDD effect on the outsourced employment share is the difference between the red and grey lines in either one of Panels A.1 or A.2.[47] Panels A.1

[45] Control occupations have very low prevalence of outsourcing, bringing the average baseline sample down towards zero (see Figure (1)). The 4.2 percentage point increase is a 162% increase relative to the baseline control sample mean of 2.6% (non-guards in permissive regions), and a 34% increase relative to the baseline treatment mean of 12.4% (guards in restrictive regions).

[46] This exercise also showcases the strengths of a DDD approach in this context because it allows us to check for any occupation-level trends or region-level trends, otherwise absorbed by occupation-year and region-year fixed effects in the DDD. While these trends are not present in measures of outsourcing prevalence, they are relevant for employment and wages, a point we return to below.

[47] Recall that DDD estimates include more flexible fixed effects than their difference-in-DD visualization. With the appropriate fixed effects, the DDD effect is the same regardless of whether it is interpreted as a

and B.1 show that the "court restrictiveness" effect steeply increased outsourcing prevalence among guards, with no effect on control occupations. Panels A.2 and B.2 show that the occupation "law bindingness" effect was much stronger in restrictive regions than in permissive ones, even if guard outsourcing also increased, relative to outsourcing in other occupations, in permissive regions following legalization.

Legalization effect on employment

Figure 7 plots coefficients β_τ of equation (3) for the effects of outsourcing legalization on guard employment. Panel A shows that legalization increased total guard employment,[48] whereas Panel B shows that this increase entailed a compositional shift in the occupation towards younger workers. Legalization increased total guard employment by 5.2% per year on average (Table 4), and this effect was monotonically decreasing in worker age (Table 5). Employment increased by 42% among workers aged 18-24 and by 19% among workers aged 25-29, but decreased by 14% among workers aged 50-64.[49] These effects are robust to alternative weighting methods (columns (3)-(4) of Tables 4 and 5) and are qualitatively similar for alternative sets of the baseline controls used as inputs to construct balancing weights (Appendix Tables F.1 and F.2).[50]

Appendix Figure F.3 shows the DD visualizations for total employment and employment of workers aged 18-24, two outcomes in Figure 7. As discussed above, these figures showcase the strengths of a DDD specification in this context by allowing us to see any trends in occupation-level outcomes that are common across regions, as well as trends in region-level outcomes that are common across occupations. These trends are absorbed by occupation-year and region-year fixed effects in the DDD, but are not absorbed in the DD visualizations.

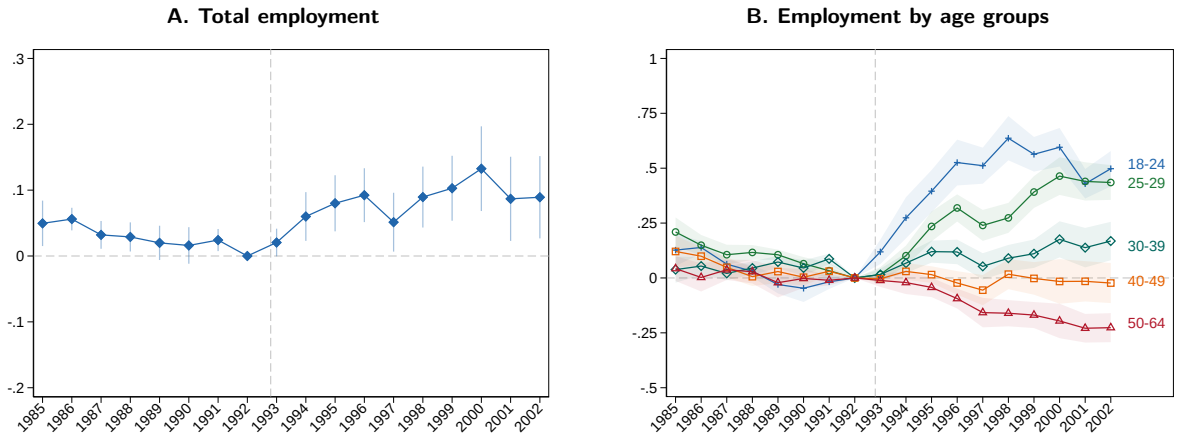
difference in guard effects or a difference in region effects.

[48] Despite the presence of mild pre-trends in early years, note that pre-legalization effects are small and not jointly statistically significant.

[49] These patterns are very different from the effect of legalization on direct-hire incumbent guards, which are negative for most age groups, consistent with the negative effects of occupational layoffs documented in Section 3. See Appendix Figure F.5.

[50] While our design's weights balance restrictive and permissive regions on baseline homicide rates, we also check that the increase in guard employment in restrictive regions relative to permissive regions was not driven by differential *trends* in crime rates. Appendix Figure F.6 shows no differences in homicide rates across regions before or after outsourcing legalization. The latter also suggests legalization did not affect homicide rates, although we lack data on more relevant crime outcomes for firms, such as robberies.These fall under each state's police jurisdiction, and are thus not consistently reported or available for all municipalities.

Figure 7: Effect of outsourcing legalization on employment



Note: See notes to Figure 6. The outcome variable in panel A is log employment, whereas the outcomes in panel B are log employment in different age groups. Age groups are defined according to the age group variable consistently reported throughout the period. The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation.

Panels A.1 and B.1 of Appendix Figure F.3 show the "court restrictiveness" dimension (i.e., cross-region) of the legalization effect, separately for guards (in red) and other (in grey) occupations. It shows that—for both guards and control occupations—restrictive regions were on a negative employment trend throughout the sample period. This negative employment trend is of very similar magnitude for guards and other occupations prior to legalization. However, legalization broke this negative trend for guards. After legalization, guard employment increased in restrictive regions, whereas employment in control occupations continued to follow its pre-legalization trend.

Panels A.2 and B.2 of Appendix Figure F.3 show the "law bindingness" dimension of the legalization effect, separately for restrictive (in red) and permissive (in grey) regions. It shows that guards were on an increasing employment trend, relative to other occupations, in both restrictive and employment regions until 1992. After 1992 this pattern is reversed, driven by a country-level steep increase in non-guard total formal sector employment and a slowdown in guard employment, documented earlier in Figure 2 and in Appendix Figure C.2.[51] Legalization attenuates the post-1992 relative decline in total guard employment in

[51]The most likely drivers of macro trends in formal employment in this period were trade liberalization

restrictive regions, and reverses it to a positive effect on employment of workers aged 18-24 in these regions. In other words, were it not for legalization, guard employment in restrictive regions would have followed the same downward trend as control occupations'.

Table 4 shows two additional decompositions of legalization's effect on employment. First, it shows that the 5.2% increase in total guard employment was primarily driven by a large increase in outsourced guard employment,[52] since direct-hire employment declined by 4.8% (Table 4, Panel B). Second, Table 4 breaks down the employment increase by employment origins (Panel C). Legalization increased the number of workers combing from outside of RAIS (that is, from unemployment, informality, or outside of the labor force) by 9.3% and the number of workers coming from other formal sector employment jobs by 3.2%. Evaluated at each origin's mean baseline level, these percent increases imply that 49% of all newcomers to the guard occupation came from unemployment, informality, or outside of the labor force.

Legalization effect on wages

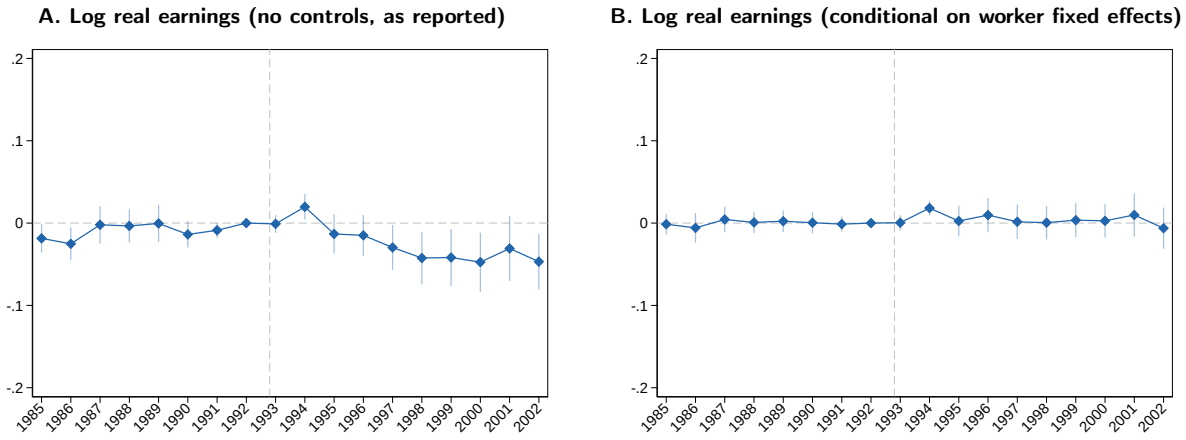
Figure 8 plots coefficients β_τ of equation (3) for the effects of outsourcing legalization on guard wages, measured as real December log earnings, the standard measure for the RAIS dataset during this period (see Appendix B). Overall, we find that outsourcing legalization had no effect on guard wages at the local labor market level (that is, inclusive of its effect on direct-hire incumbents who stay in the guard occupation and its effect on newcomers to the occupation).

Panel A of Figure 8 shows effects on wages as reported (that is, without any controls), whereas Panel B shows effects conditional on worker fixed effects and time-varying demographic controls. Table 6 presents the corresponding average effects for these outcomes and for additional wage measures. While Panel A of Figure 8 suggests that legalization might have induced a small reduction in raw wages (potentially driven by the compositional shift towards younger workers, who are typically paid less than older workers), this effect is neither

(1990-1994) and Plano Real (1994) (see Baumann (2001)), whereas the most likely drivers in macro trends in guard employment is aggregate demand for security provision, since similar patterns are seen for police employment (see Appendix Figure C.2).

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

Figure 8: Effect of outsourcing legalization on wages



Note: See notes to Figure 6. The outcome variable in Panel A is log real December earnings as reported by each worker, without any controls. The wage outcome variable in Panel B is log real December earnings conditional on worker fixed effects and time-varying demographic controls (i.e., education, age, and gender). The omitted year is 1992. Sample is weighted by entropy balancing weights. Standard errors for 95% confidence intervals are two-way clustered by regional labor court and occupation.

large (a 1.6% reduction) nor statistically significant. We find similar near-zero and statistically insignificant point estimates with alternative weighting methods (columns (3)-(4)) and with alternative sets of the baseline controls used as inputs to construct balancing weights (Appendix Table F.3).

Appendix Figure F.4 shows the DD visualizations for the wage outcomes in Figure 7 and for wages conditional on demographics only. As in the DD visualization of employment effects shown in Appendix Figure F.3, there are differential trends in wages across regions (i.e., Panels A.1-C.1, with near-identical magnitudes for guards and other occupations) and across occupations (i.e., Panels A.2-C.2, with near-identical magnitudes for restrictive and permissive regions). But unlike with employment, legalization did not break these trends.[53]

Importantly, note that the effect of legalization on guard wages at the local labor market level is different from the effect of an outsourcing event on incumbent guards, presented in Section 3, on two dimensions. First, the effect at the local labor market level includes effects on newcomer guards, while the effects in Section 3 do not. Second, the effect at the local

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

labor market level is restricted to the labor markets for guards, so it does not include wage declines driven by workers leaving the occupation., while the effects in Section 3 do.

Finally, similar to recent studies (e.g., Goldschmidt and Schmieder 2017; Drenik et al. 2023), we note that our wage effects do not account for the non-wage components of worker compensation, which are not available. However, since a growing literature shows that non-wage compensation is positively correlated with wages (e.g., Taber and Vejlin 2020; Lamadon et al. 2022), we believe our overall conclusions (and implications for welfare) are not likely to change if such data were available.[54]

5 Discussion

This section sketches an interpretive framework for our main findings: outsourcing legalization displaced older workers, increased employment of younger workers who enter from informality, and had little impact on wages. Appendix G provides a formal analysis.

In our setup, there are two types of security guards, young and old, and two security service sectors, direct-hire or outsourced. Each sector produces security services using old and young guards with different fixed proportions technologies. The security services from each sector are then combined with non-security labor to produce final goods for consumers. Wages are determined via "right-to-manage" union bargaining, with different worker bargaining power in each sector. Since we are interested in the outcomes of security guards only, employment and wages for non-security labor are assumed to be exogenous.

We allow outsourced and direct-hire security services to be imperfect production substitutes, since one or the other contracting form may be more efficient depending on production needs. For example, the advantages of outsourcing include reduced shift management costs, fast replacements for sick or absent worker, reduced monitoring costs, reduced recruitment cost, improved screening, specialized capabilities, contracting flexibility in response to variable labor demand, and an overall reduction in cognitive and attention costs (see, e.g.,

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

Abraham and Taylor 1996; Houseman 2001). These benefits arise from economies of scale at the intermediary firm. Disadvantages of outsourcing include weakened incentives for specific investments and an overhead required to compensate the intermediary firm for its services (see, e.g., Autor 2003; Li and Wong 2024).

We also allow each sector to have different levels of worker bargaining power and to employ the two types of security guards in different fixed proportions. These assumptions capture the idea that intermediary firms and direct-hire firms may have different wage policies as well as systematic differences in the types of workers they tend to employ. For example, we documented that intermediary firms tend to employ a large pool of similar workers. This allows them to flexibly reassign workers across firms. This improves employment security during the early years of tenure, even as it reduces tenure-based wage growth (Guo et al., 2024). Intermediation may therefore be more attractive to younger workers than older workers.

Outsourcing legalization eliminates a tax on outsourced security services. In response, the representative firm substitutes away from direct-hire services towards outsourced services. The magntitude of this adjustment depends on the substitutability of outsourced and direct-hire services, wage differential in the two sectors, and the elasticity of demand for the product.

Suppose, for example, that outsourced and direct-hire labor are perfect substitutes, that young and old workers used in the same proportions in the two sectors, and outsourced workers have signficantly less wage bargaining power. In this case, a large legal cost prevents the firm from employing outsourced workers, even though they are equally productive and require lower wages. Outsourcing legalization induces the firm to hire outsourced labor instead and thereby lowers overall wages.[55]

Next, suppose that outsourced and direct-hire labor are imperfect substitutes, that outsourcing sector favors young workers, and that outsourced and direct-hire workers are paid similarly. In this case, the legal cost distorts production away from the optimal mix of inputs. Outsourcing legalization increases production efficiency, displaces older workers, shifts employment composition towards younger workers, and has little impact on average wages.

Our findings show that Brazil's outsourcing legalization increased security guard employ-

[55]Legalization could also differentially affectwages of direct-hire and outsourced workers by affecting the size of quasi-rents. However, we assume that these effects are small relative to the outsourcing wage differential.

ment and had little effect on their average wages. Appendix G uses a first-order approximation to infer welfare effects from our reduced-form estimates. Interpreted through our framework, outsourcing legalization reduced consumer costs and had a small but positive effects on total worker welfare.

How generalizeable are these findings? The job reallocation effects that we uncover are likely to exist elsewhere, since outsourced workers tend to be demographically different from direct hires across countries and occupations. For instance, Dube and Kaplan (2010) document that outsourced cleaners and guards in the US are younger by 3.4 and 1.6 years, respectively. Drenik et al. (2023) document that the mean age of temp workers in Argentina are 28 years old, while it is 38 for all formal workers. These differences are likely arise from personnel policy differences between contract and direct-hire firms.

Cost efficiencies from *non-core* activity outsourcing are also likely to generalize beyond the security guard occupation in Brazil. Firms with limited and occasional need for any specialized service outside its core competency typically do not have human resources departments that can properly recruit, license, train, and/or monitor such workers. By aggregating demand for a specialized service across clients, contract firms can better absorb fixed costs involved in managing workers.

It is possible, however, that our estimated wage effects are smaller and less negative than would occur in other occupations and settings, since security guards in Brazil have a comparatively small outsourcing wage differential. For example, Guo et al. (2024) document thatoutsourced guards in Brazil earn only 1.3 percent less than direct hires after controlling for both observed and unobserved worker heterogeneity, while the wages of outsourced cleaners are 11 percent lower than similar direct hires. Outsourcing wage differentials estimated for low-wage occupations in high-income countries are also larger in magnitude, typically range from negative 10-25 percent (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017). A plausible explanation for this difference is that outsourcing wage differentials are less negative for higher-wage and more professionalized occupations, such as security guards in Brazil.[56]

[56]Consistent with this explanation, Spitze (2022) provides evidence from the U.S. that outsourced workers in higher-wage occupations tend to have a smaller wage differential.

6 Conclusion

This paper is the first to estimate the effects of a large and exogenous market-level reduction in the cost of *non-core* activity outsourcing using a comprehensive administrative employment panel and cross-market variation in the bindingness of labor market regulations. Our methodological innovations enable us to measure the market-level effects of outsourcing on different segments of workers with much greater precision than in prior studies.

We uncover a very large and persistent redistribution of jobs from older incumbent workers to younger entrant workers. First, we find that there was a wave of occupational layoffs in the wake of Brazil's 1993 outsourcing legalization. The impacted incumbent security guards experienced temporarily reduced employment, reallocation to other occupations and lower-wage firms, and—as a consequence—persistently reduced earnings. By contrast, on-site outsourcing events, which are a focus in prior literature, were very rare.

Second, we find that the employment of younger security guards persistently increased, even as the employment of older security guards persistently declined,. This shift in employment composition is attributable to the fact that contract firms persistently employ demographically different workers than direct employers. The total employment of security guards in local labor markets increased, with roughly half of the employment increase driven by workers coming from outside formality. At the same time, outsourcing legalization had little effect on average demographic-adjusted wages.

Our findings suggest that a reduction in the cost of *non-core* activity outsourcing benefits younger entrant workers, especially in higher-wage occupations and in economies with large informal sectors, even as it significantly hurts older incumbent workers. This result provides rare direct evidence for insider-outsider theories of the labor market. It highlights a role for firm personnel policies in mediating the employment impact of labor regulations. It also contrasts with the experience of European labor market reforms, which legalized fixed-term contracts instead of *non-core* activity outsourcing, and instead protected incumbent workers to the detriment of younger entrants. Given the startling findings uncovered here, further inquiry informed by our study may reveal much more about the interplay between domestic outsourcing and labor market structure.

References

Abowd, J. M., F. Kramarz, and D. N. Margolis (1999): "High Wage Workers and High Wage Firms," *Econometrica*, 67, 251–334.

Abraham, K. G., J. C. Haltiwanger, K. Sandusky, and J. R. Spletzer (2018): "Measuring the Gig Economy: Current Knowledge and Open Issues," Working Paper 24950, National Bureau of Economic Research.

Abraham, K. G. and S. K. Taylor (1996): "Firms' use of outside contractors: Theory and evidence," *Journal of Labor Economics*, 14, 394–424.

Aghion, P., R. Burgess, S. J. Redding, and F. Zilibotti (2008): "The Unequal Effects of Liberalization: Evidence from Dismantling the License Raj in India," *American Economic Review*, 98, 1397–1412.

Alvarez, J., F. Benguria, N. Engbom, and C. Moser (2018): "Firms and the Decline in Earnings Inequality in Brazil," *American Economic Journal: Macroeconomics*, 10, 149–89.

Andrews, M. J., L. Gill, T. Schank, and R. Upward (2008): "High Wage Workers and Low Wage Firms: Negative Assortative Matching or Limited Mobility Bias?" 171, 673–697.

Appelbaum, E. (2017): "Domestic Outsourcing, Rent Seeking, and Increasing Inequality," *Review of Radical Political Economics*, 49, 513–528.

Autor, D. H. (2001): "Why Do Temporary Help Firms Provide Free General Skills Training?" *The Quarterly Journal of Economics*, 116, 1409–1448.

——— (2003): "Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing," *Journal of Labor Economics*, 21, 1–42.

——— (2009): "Introduction," in *Studies of Labor Market Intermediation*, University of Chicago Press, 1–23.

Bargain, O. and P. Kwenda (2014): "The Informal Sector Wage Gap: New Evidence Using Quantile Estimations on Panel Data," *Economic Development and Cultural Change*, 63, 117 – 153.

Barreto, A. C. C. and U. M. B. de Lyra (2016): "O Direito Alternativo e a Instituição de uma Dogmática Emancipatória," *Revista Brasileira de Teoria Constitucional*, 2, 1230.

Barros Biavaschi, M. and P. de Andrade Baltar (2013): .

Basri, M. C., M. Felix, R. Hanna, and B. A. Olken (2021): "Tax administration versus tax rates: Evidence from corporate taxation in Indonesia," *American Economic Review*, 111, 3827–71.

Batistella, A. (2009): "O movimento operário e sindical em Passo Fundo (RS)–1920-1964," *Revista Eletrônica História em Reflexão*, 3.

Battiston, D., M. Espinosa, and S. Liu (2021): "Talent Poaching and Job Rotation," Working paper.

Baumann, R. (2001): "Brazil in the 1990s: an economy in transition," *CEPAL Review*.

Bentolila, S. and G. Saint-Paul (1992): "The macroeconomic impact of flexible labor contracts, with an application to Spain," *European Economic Review*, 36, 1013–1047.

Berlingieri, G. (2014): "Outsourcing and the Rise in Services," CEP Discussion Paper No. 1199.

Bernhardt, A., R. Batt, S. N. Houseman, and E. Appelbaum (2016): "Domestic Outsourcing in the United States: A Research Agenda to Assess Trends and Effects on Job Quality," Technical Report, Upjohn Institute.

Bertrand, M., C.-T. Hsieh, and N. Tsivanidis (2021): "Contract Labor and Firm Growth in India," NBER Working Paper.

Besley, T. and R. Burgess (2004): "Can Labor Regulation Hinder Economic Performance? Evidence from India*," *The Quarterly Journal of Economics*, 119, 91–134.

Biavaschi, M. B. and A. Droppa (2011): "A história da súmula 331 do tribunal superior do trabalho: a alteração na forma de compreender a terceirização," *Mediações-Revista de Ciências Sociais*, 16, 124–141.

Bilal, A. and H. Lhuillier (2021): "Outsourcing, Inequality and Aggregate Output," Working paper.

Blanchard, O. and A. Landier (2002): "The Perverse Effects of Partial Labour Market Reform: Fixed-term Contracts in France," *The Economic Journal*, 112, F214–F244.

Blanchflower, D. and A. Bryson (2004): "What Effect Do Unions Have on Wages Now and Would Freeman and Medoff Be Surprised?" *Journal of Labor Research*, 25, 383–414.

Bloom, N., A. Guo, and B. Lucking (2018): "Outsourcing, Occupational and Industrial Concentration," Working paper.

Bonhomme, S., K. Holzheu, T. Lamadon, E. Manresa, M. Mogstad, and B. Setzler (2020): "How Much Should we Trust Estimates of Firm Effects and Worker Sorting?" Working paper.

Bostanci, G. (2022): "Productivity Gains From Labor Outsourcing: the Role of Trade Secrets," Working paper.

Botero, J. C., S. Djankov, R. L. Porta, F. Lopez-de Silanes, and A. Shleifer (2004): "The Regulation of Labor," *The Quarterly Journal of Economics*, 119, 1339–1382.

Cahuc, P. and F. Postel-Vinay (2002): "Temporary jobs, employment protection and labor market performance," *Labour Economics*, 9, 63–91.

Chan, M. (2023): "How Substitutable Are Labor and Intermediates?" Tech. rep., Working paper.

Chaurey, R. (2015): "Labor regulations and contract labor use: Evidence from Indian firms," *Journal of Development Economics*, 114, 224–232.

Clement, D. (2017): "Interview with Lawrence Katz," https://www.minneapolisfed.org/article/2017/interview-with-lawrence-katz, accessed: 2020-02-03.

Cooney, S., D. du Toit, R. Fragale, R. Ronnie, and K. Sankaran (2015): "Building BRICS of success?" in *Comparative Labor Law*, ed. by M. Finkin and G. Mundlak, Edward Elgar Publishing, Incorporated.

Couch, K. A. and D. W. Placzek (2010): "Earnings Losses of Displaced Workers Revisited," *American Economic Review*, 100, 572–89.

da Cruz, L. G. R. (2009): "A terceirização trabalhista no Brasil: aspectos gerais de uma flexibilização sem limite." *Revista do Centro Acadêmico Afonso Pena*, 12.

Daruich, D., S. D. Addario, and R. Saggio (2020): "The Effects of Partial Employment Protection Reforms: Evidence from Italy," Development Working Papers 463, Centro Studi Luca d'Agliano, University of Milano.

Davis, S. J. and T. von Wachter (2011): "Recessions and the Costs of Job Loss," Tech. Rep. 2, Brookings Papers on Economic Activity.

Dey, M., S. Houseman, and A. Polivka (2010): "What do we know about contracting out in the United States? Evidence from household and establishment surveys," in *Labor in the new economy*, University of Chicago Press, 267–304.

Dix-Carneiro, R. and B. K. Kovak (2017): "Trade liberalization and regional dynamics," *American Economic Review*, 107, 2908–2946.

Dix-Carneiro, R., R. R. Soares, and G. Ulyssea (2018): "Economic Shocks and Crime: Evidence from the Brazilian Trade Liberalization," *American Economic Journal: Applied Economics*, 10, 158–95.

Drenik, A., S. Jäger, P. Plotkin, and B. Schoefer (2023): "Paying outsourced labor: Direct evidence from linked temp agency-worker-client data," *Review of Economics and Statistics*, 105, 206–216.

Dube, A. and E. Kaplan (2010): "Does outsourcing reduce wages in the low-wage service occupations? Evidence from janitors and guards," *Industrial and Labor Relations Review*, 63, 287–306.

Espinosa, M. (2020): "Labor Boundaries and Skills: The Case of Lobbyists," *Management Science*, 67, 1586–1607.

Estefan, A., R. Gerhard, J. P. Kaboski, I. O. Kondo, and W. Qian (2024): "Outsourcing Policy and Worker Outcomes: Causal Evidence from a Mexican Ban," Tech. rep., NBER working paper.

Farber, H. S. (1986): "The analysis of union behavior," Elsevier, vol. 2 of *Handbook of Labor Economics*, 1039 – 1089.

Felix, M. (2021): "Trade, Labor Market Concentration, and Wages," Job Market Paper.

García-Pérez, J. I., I. Marinescu, and J. Vall Castello (2018): "Can Fixed-term Contracts Put Low Skilled Youth on a Better Career Path? Evidence from Spain," *The Economic Journal*, 129, 1693–1730.

Gibbons, R. and L. Katz (1992): "Does Unmeasured Ability Explain Inter-Industry Wage Differentials?" *The Review of Economic Studies*, 59, 515–535.

Glynn, A. N. and K. M. Quinn (2010): "An Introduction to the Augmented Inverse Propensity Weighted Estimator," *Political Analysis*, 18, 36–56.

Goldschmidt, D. and J. F. Schmieder (2017): "The rise of domestic outsourcing and the evolution of the German wage structure," *The Quarterly Journal of Economics*, 132, 1165–1217.

Guo, N., D. Li, and M. Wong (2024): "Domestic Outsourcing and Employment Security," Working paper.

Hainmueller, J. (2012): "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies," *Political Analysis*, 20, 25–46.

Houseman, S. N. (2001): "Why Employers Use Flexible Staffing Arrangements: Evidence from an Establishment Survey," *Industrial and Labor Relations Review*, 55, 149–170.

IBGE (2000): "Estudos e Pesquisas, Informacao Demografica e Socioeconomica numero 4," *Rio de Janeiro*.

Jacobson, L. S., R. J. LaLonde, and D. G. Sullivan (1993): "Earnings Losses of Displaced Workers," 83, 685–709.

Jiménez, B. and S. Rendon (2022): "Labor Market Effects of Bounds on Domestic Outsourcing," Tech. rep., IZA Discussion Paper No. 15692.

Kalleberg, A. L. (2000): "Nonstandard employment relations: Part-time, temporary and contract work," *Annual Review of Sociology*, 26, 341–65.

Katz, L. F. and A. B. Krueger (2016): "The rise and nature of alternative work arrangements in the United States, 1995-2015," National Bureau of Economic Research working paper.

——— (2019): "Understanding trends in alternative work arrangements in the United States," National Bureau of Economic Research working paper.

Kaufman, B. E. (2004): "What Unions Do: Insights from Economic Theory," *Journal of Labor Research*, 25, 351–382.

Kovak, B. K. (2013): "Regional Effects of Trade Reform: What Is the Correct Measure of Liberalization?" *American Economic Review*, 103, 1960–76.

Lachowska, M., A. Mas, R. D. Saggio, and S. A. Woodbury (2020a): "Do Firm Effects Drift? Evidence from Washington Administrative Data," NBER Working paper.

Lachowska, M., A. Mas, and S. A. Woodbury (2020b): "Sources of Displaced Workers' Long-Term Earnings Losses," *American Economic Review*, 110, 3231–66.

Lamadon, T., M. Mogstad, and B. Setzler (2022): "Imperfect Competition, Compensating Differentials, and Rent Sharing in the US Labor Market," *American Economic Review*, 112, 169–212.

Lee, D. R. (1996): "Why Is Flexible Employment Increasing?" *Journal of Labor Research*, 17, 543–53.

Li, D. and M. B. Wong (2024): "A Theory of the Visible Hand: Intermediation and Coordination in Markets for Relational Contracts," Tech. rep., Working paper.

Lindbeck, A. and D. J. Snower (1989): *The Insider-Outsider Theory of Employment and Unemployment*, vol. 1 of *MIT Press Books*, The MIT Press.

Oswald, A. J. (1985): "The Economic Theory of Trade Unions: An Introductory Survey," *The Scandinavian Journal of Economics*, 87, 160–193.

Saint-Paul, G. (2002): "The Political Economy of Employment Protection," *Journal of Political Economy*, 110, 672–704.

Schmieder, J., T. von Wachter, and J. Heining (2020): "The Costs of Job Displacement over the Business Cycle and Its Sources: Evidence from Germany," Working paper.

Spitze, S. (2022): "The Equilibrium Effects of Domestic Outsourcing," Working paper.

Stansbury, A. and L. Summers (2020): "Declining Worker Power and American Economic Performance," Brookings Papers on Economic Activity.

Taber, C. and R. Vejlin (2020): "Estimation of a Roy/search/compensating Differential Model of the Labor Market," *Econometrica*, 88, pp. 1031–1069.

Weil, D. (2014): *The Fissured Workplace*, Harvard University Press.

Table 1: Descriptive statistics of security guards by contract type

	Direct hire			Contract-firm		
	1985-1993	1994-1996	1997-2002	1985-1993	1994-1996	1997-2002
Male	0.98	0.98	0.97	0.98	0.97	0.97
Age	40	40	40	34	34	35
Years of schooling	4.9	5.4	6.2	5.1	5.9	6.8
CLT urban indeterminate contract	0.98	0.95	0.96	0.99	0.98	0.99
Tenure	2.5	2.8	2.9	1.5	1.5	1.7
New hire (Tenure<1 years)	0.46	0.43	0.41	0.52	0.55	0.48
Real monthly earning (2017 $R)	1995	1753	1694	1481	1573	1665
	(1612)	(1476)	(1382)	(822)	(814)	(796)
	[1511]	[1321]	[1312]	[1285]	[1390]	[1525]
Contract hours		42.4	42.4		43.5	43.7
		[44]	[44]		[44]	[44]
Real wage (2017 $R)		43.5	41.8		37.1	38.4
		(55.9)	(49.3)		(34.6)	(23.2)
		[31]	[31]		[32]	[35]
Has multiple jobs	0.01	0.02	0.01	0.02	0.01	0.02
Employer size	885	692	568	1024	954	1729
	[146]	[83]	[59]	[586]	[460]	[478]
AKM firm effect	0.15	0.04	-0.02	0.12	0.11	0.10
	(0.366)	(0.365)	(0.359)	(0.202)	(0.205)	(0.206)
Number of guards at employer	67	66	54	822	781	808
	[8]	[5]	[4]	[490]	[422]	[432]
N	2220357	796814	1364634	1242514	596325	1506231

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

Table 2: Effects of occupational layoffs on incumbent workers

	Years since occupational layoff		
	Y0	Y1	Y5
Formally employed	-0.493	-0.159	-0.025
	(0.013)	(0.010)	(0.010)
Formally employed in same occupation	-0.761	-0.399	-0.117
	(0.007)	(0.011)	(0.010)
Formally employed in contract firm	0.002	0.077	0.041
	(0.001)	(0.005)	(0.005)
Monthly wage (relative to base year)	-0.064	-0.136	-0.090
	(0.012)	(0.014)	(0.019)
Log monthly wage	-0.078	-0.185	-0.112
	(0.010)	(0.011)	(0.017)
Firm AKM FE effect	-0.032	-0.080	-0.052
	(0.005)	(0.006)	(0.011)
As fraction of wage losses	41%	43%	46%

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

Table 3: Effect of legalization on prevalence of guard outsourcing

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

Notes: This table shows effects of outsourcing legalization on the prevalence of outsourcing in a region-occupation pair, estimated using equation 3's weighted triple-differences specification, which compares guards to other occupations in restrictive versus permissive regions before versus after legalization. Outsourced share is the share of total labor market employment that is outsourced (that is, hired by a contract firm). Employment HHI is the Herfindahl-Hirschmann employment index within a region-occupation pair. Columns (2)-(4) present estimates according to different econometric weighing methods. All weights balance restrictive and permissive regions on baseline homicide rate, import competition exposure, total formal employment, and unemployment rate. All regressions include microregion-occupation, microregion-year, and occupation-year fixed effects. The sample is balanced and includes 216 microregions and thirteen 2-digit CBO94 occupational groups (security guards plus 12 major comparison groups, present in all microregions). Standard errors are two-way clustered by Regional Labor Court and occupation.

Table 4: Effect of legalization on employment

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

Notes: See notes to Table 3. Log employment is expressed in natural logs.

Table 5: Effect of legalization on employment composition

		Legalization effect (Weighted DDD)			
	Mean at baseline (1992)	Entropy-balancing weights (EB)	Inverse propensity score weights	EB with regression adjustment	Observations (region x occup x year)
	(1)	(2)	(3)	(4)	(5)
Log employment by worker age group					
18-24	5.685	0.422	0.400	0.421	57240
		(0.036)	(0.029)	(0.028)	
25-29	5.579	0.193	0.188	0.201	57240
		(0.031)	(0.025)	(0.020)	
30-39	5.993	0.060	0.058	0.062	57240
		(0.023)	(0.019)	(0.019)	
40-49	5.254	-0.050	-0.042	-0.056	57240
		(0.031)	(0.026)	(0.031)	
50-64	4.280	-0.140	-0.145	-0.148	57240
		(0.023)	(0.018)	(0.021)	

Notes: See notes to Table 3. Log employment is expressed in natural logs.

Table 6: Effect of legalization on wages

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

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

Online Appendices

A Classifying Regional Labor Courts

In this section, we describe the process we followed to classify Brazil's Regional Labor Court by pre-legalization court permissiveness and the source materials on which we relied for our classification, summarized below in Appendix Table A.1.

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

This finding motivated us to seek legal records in order to construct a finer and more comprehensive measure of local court permissiveness towards outsourcing. We submitted formal requisitions to all 24 regional labor courts asking if: (1) lawsuits from 1985-1993 were available, (2) it would be possible for us to have a list of all lawsuits concerning outsourcing, and (3) they still had the cases listed as precedents for Súmula 256 and 331. We received answers in the negative for almost all local courts. Most no longer kept cases before 1993. Given the obstacles in obtaining individual rulings within each regional labor court, we gathered available primary and secondary sources to glean as much information on the stances of regional courts as possible.

First, we were able to retrieve and review all regional labor court precedent rulings that were appealed to the Superior Labor Court between 1986 (when Súmula 256 was in place)

and 1993 (when Súmula 331 took place), as these were centrally stored at the Superior Labor Court's archives. Appendix Table A.4 summarizes the key decisions in each ruling. It shows that Southern regional labor courts tended to find a direct employment link with the end-firm, meaning that it did not recognize the legality of the outsourcing arrangement. In contrast, Column (7) in Appendix Table A.4 shows that the Superior Labor Court tended to reverse these regional courts' decisions, finding the direct employment link with the contract-firm instead.

Finally, we relied heavily on research performed by Magda Biavaschi and Alisson Droppa, two Brazilian legal historians who interviewed prominent judges and lawyers regarding the history surrounding Súmula 331, as well as a set of 28 interview transcripts that they generously provided. We also studied a set of publicly available legal cases cited by Súmula 331. Appendix Tables A.2 and A.3 display all quotes from these interviews concerning Courts' interpretation on the legality of outsourcing.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

B Data Appendix

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

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

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

Table B.1: Included occupations

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

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

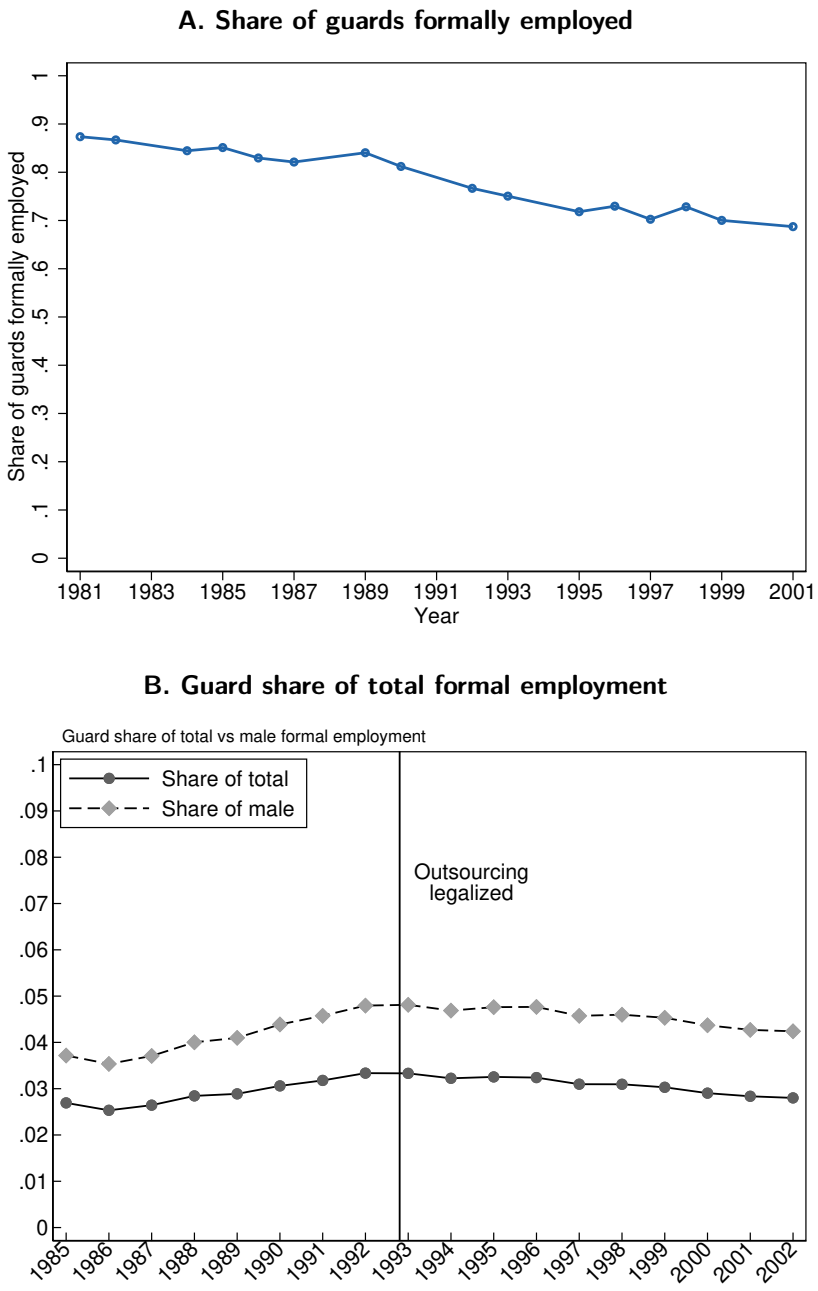
Measuring outsourcing using industry and occupation codes We use the 1994 *Código Brasileiro de Ocupações* (CBO 94) at the two-digit level to define occupations, which is consistent with predecessor (CBO) occupational codes. We identify whether an establishment in the RAIS dataset is part of an contract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

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

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

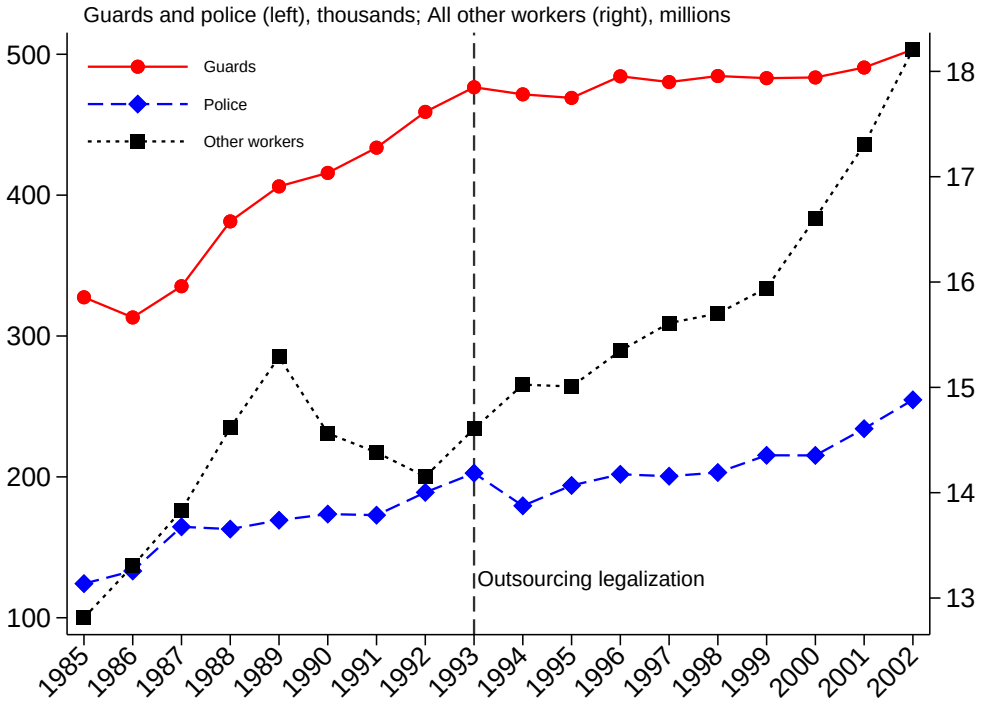
C Descriptive Statistics and Trends

Figure C.1: Share of security guards formally employed



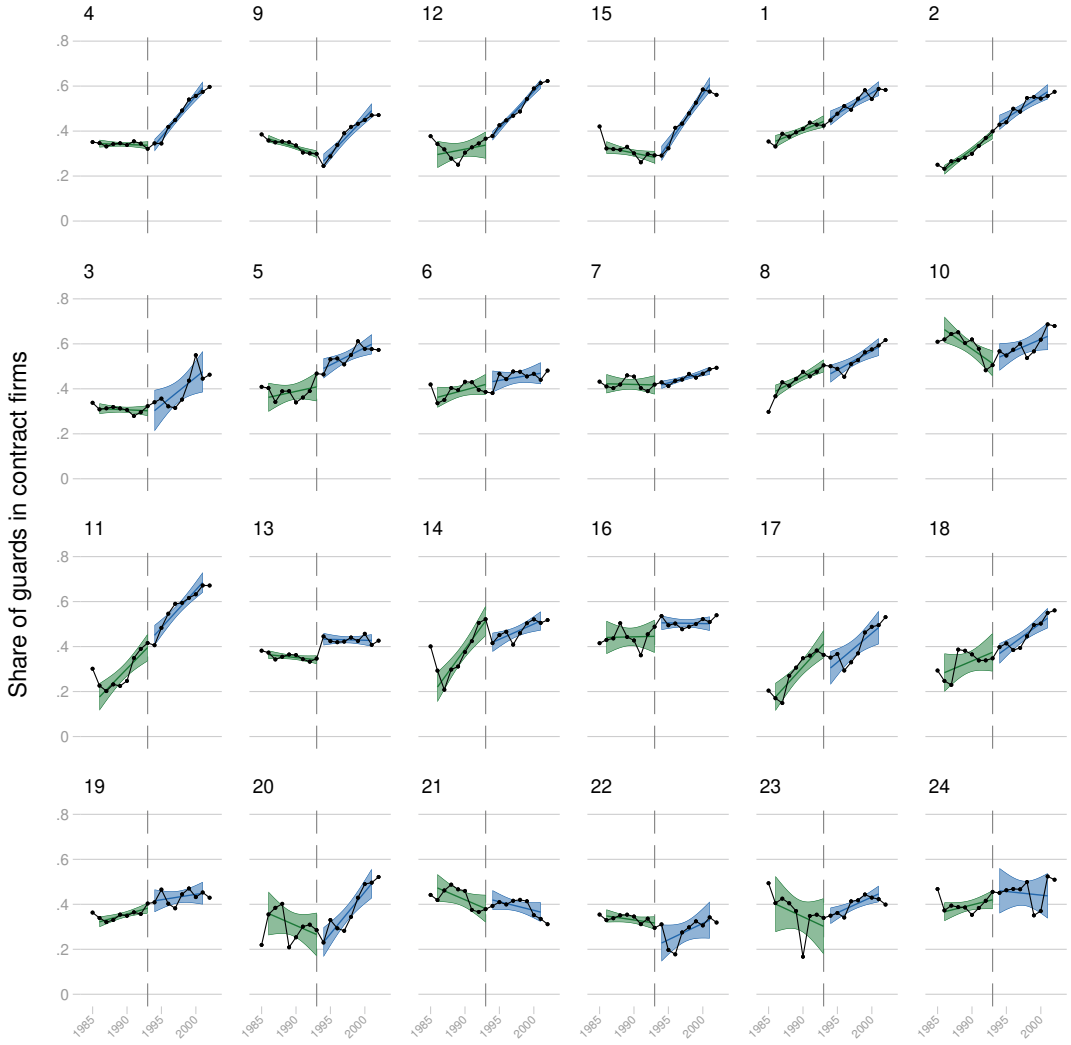
Note: Panel A plots the share of guards (aged 18-65) that are formally employed based on Brazil's Pesquisa Nacional por Amostra de Domicílios (PNAD) household survey. Pabel B plots the guard share of all total private-sector formal employment, separately for all workers versus for male workers only,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

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



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

Figure C.3: Trends in security outsourcing, by regional labor court



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

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

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

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

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

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

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

D Firm-level Outsourcing Events: Definitions

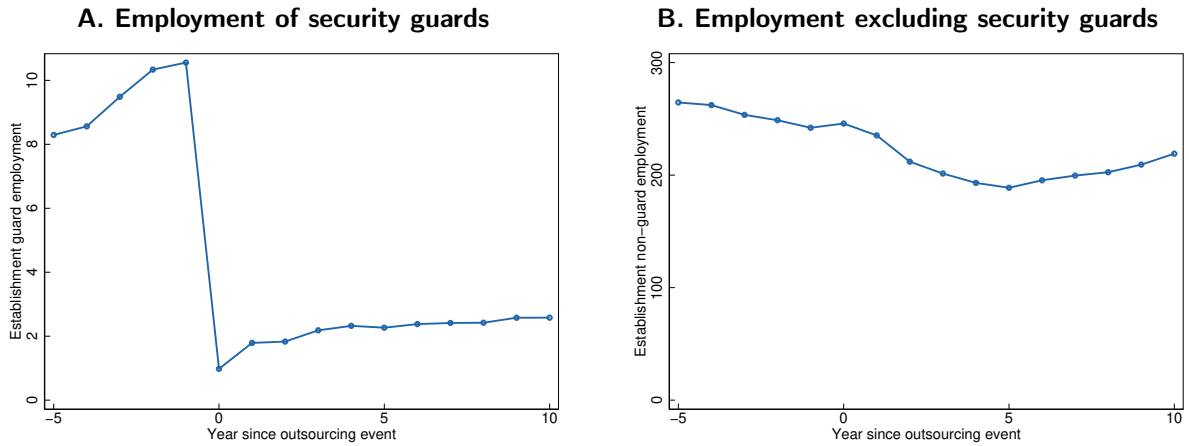
D.1 On-site and Occupational Layoff Outsourcing Events

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

Goldschmidt and Schmieder (2017) use a more stringent definition for on-site outsourcing. In their definition, the predecessor establishment must have at least 50 employees in the year prior to the event, continue to exist in the following year, and not shrink by more than 50%. The flow must also represent less than 30% of employment in the predecessor in the previous year, such that outsourced workers represent only a small part of the predecessor's business.

We say that establishment j had an "occupational layoff" in year t if: (1) establishment j had at least 10 total employees; (2) establishment j employed at least three guards in years $t-1$ and $t-2$; (3) the number of guards at establishment j in year t fell by at least two-thirds compared to year $t-1$; (4) the number of guards fell to zero in year t if j employed fewer than six guards in year $t-1$; (5) establishment j's non-guard employment shrinks by less than 10 percent between $t-1$ and t; and (6) establishment j was not a contract firm nor government entity in year $t-1$. Note that to avoid misclassifying normal fluctuations in headcount, we require that the number of security guards must fall to zero for establishments initially with 5 or fewer guards. To avoid misclassifying mass layoffs, we also exclude establishments who non-guard employment fell by more than 10 percent. Figure D.1 visualizes this definition by plotting the average number of security guards and non-security guard employees at the establishment level during the years before and after an occupational layoff.

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



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

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

We define non-outsourcing establishments as those with no occupational layoffs between 1990 and 2000. For each treated worker, we take the set of workers employed by non-outsourcing establishments in the same 2-digit industry, 5-digit occupation, and regional court jurisdiction to be our potential control group. For both treated and control groups, we restrict to workers who were employed at the same establishment as a security guard for three consecutive years prior to outsourcing. We then estimate a probit regression of whether a worker is experienced an occupational layoff, controlling for wages two and three years prior, as well as tenure and AKM firm effect in the year prior to outsourcing. For each treated worker, we then choose the non-outsourced worker with the closest propensity score to the comparison worker. Table D.1 presents summary statistics for the matched worker sample.

Table D.1: Descriptive Statistics, Matched worker sample

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

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

D.3 Estimation of Present Discounted Value of Earnings Losses

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

$$PDV_{Loss} = \sum_{k=0}^{8} \hat{\delta}_k \frac{1}{(1+r)^k} + \sum_{k=8}^{19} \hat{\delta}_8 \frac{\left(1+\hat{\lambda}\right)^{k-8}}{(1+r)^k} \tag{4}$$

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

A complication in our setting is that we do not observe earnings for workers who are employed in the informal sector. To impute earnings for missing observations, we use a range of methods that make different assumptions about what an unobserved worker would have earned.Appendix Table D.2 summarizes our results according to these various methods:

Table D.2: PDV of earnings losses, as multiple of baseline earnings

Assuming zero earnings for unobserved workers	1.40
Assuming unobserved workers earn half min wage	1.32
Assuming unobserved workers earn min wage	1.24
Using monthly wage estimates from observed workers	1.06

The first method assumes that workers earn nothing if they unobserved in our data. The second method assumes that unobserved workers earn half the minimum wage. The third method assumes that unobserved workers earn exactly the minimum wage. The final method assumes that unobserved workers had the same earnings as observed workers, so we simply

use the monthly wage estimates from the non-missing data that we reported in the previous section as the estimates for earnings losses. While the first method yields a strict upper bound on total earnings losses, the final method is likely to understate them, since earnings in the informal sector are lower on average (Bargain and Kwenda, 2014).

D.4 Measuring Firm Wage Premia using AKM Decomposition

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

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

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

To ensure that firm and worker fixed effects are identified, we restrict our analysis to the largest connected set of firms that are linked by workers moving between them. Identification of the AKM model also requires that workers do not move across firm in a manner that is systematically correlated with unmeasured productivity (Gibbons and Katz 1992). Alvarez et al. (2018) provide evidence that this assumption is justified in Brazilian RAIS data. A further concern when estimating the AKM model is limited mobility bias, which may generate misleading variance decompositions, as discussed by Andrews et al. (2008). However, limited mobility bias is likely to be small in our setting since we use a long panel (Lachowska et al. 2020a; Bonhomme et al. 2020).

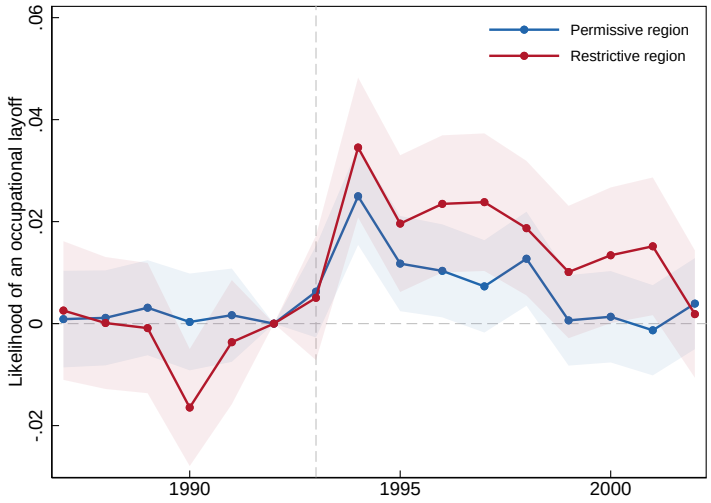
E Firm-level Outsourcing Events: Additional results

Table E.1: Establishment-level predictors of security guard outsourcing decisions, 1993-1998

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

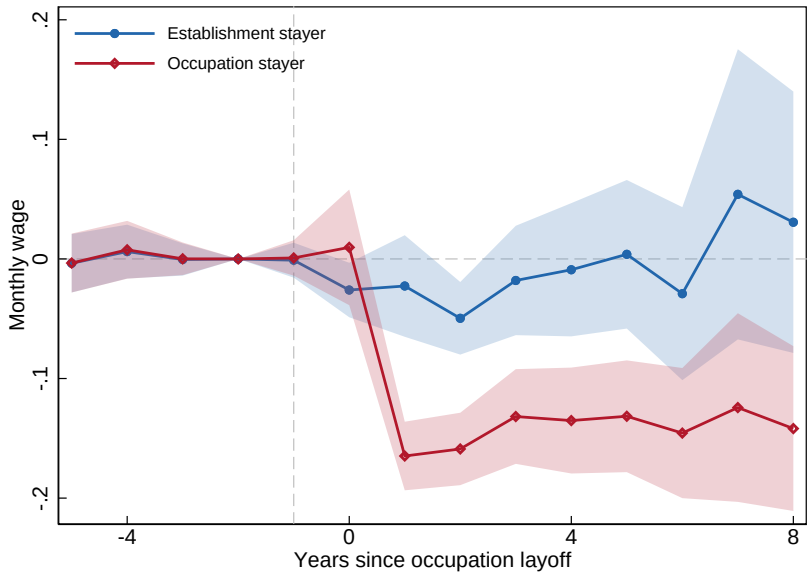
Note: Sample includes establishments with at least 50 employees and at least three security guards in 1993. We exclude establishments whose total non-security guard headcount declines by more than 10 percent by 1998. The dependent variable takes indicates whether the number of security guards at the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

Figure E.1: Frequency of occupational layoffs by region restrictiveness



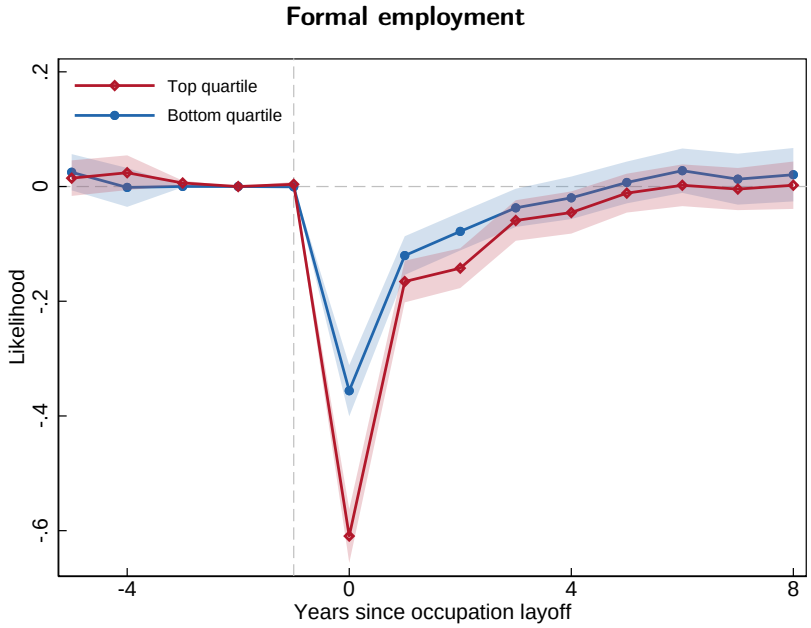
Note: This figure plots coefficients from a linear probability model with separate year fixed effects for restrictive and permissive regions. We cluster standard errors at the establishment level.

Figure E.2: Effect of occupational layoffs on establishment and occupation stayers



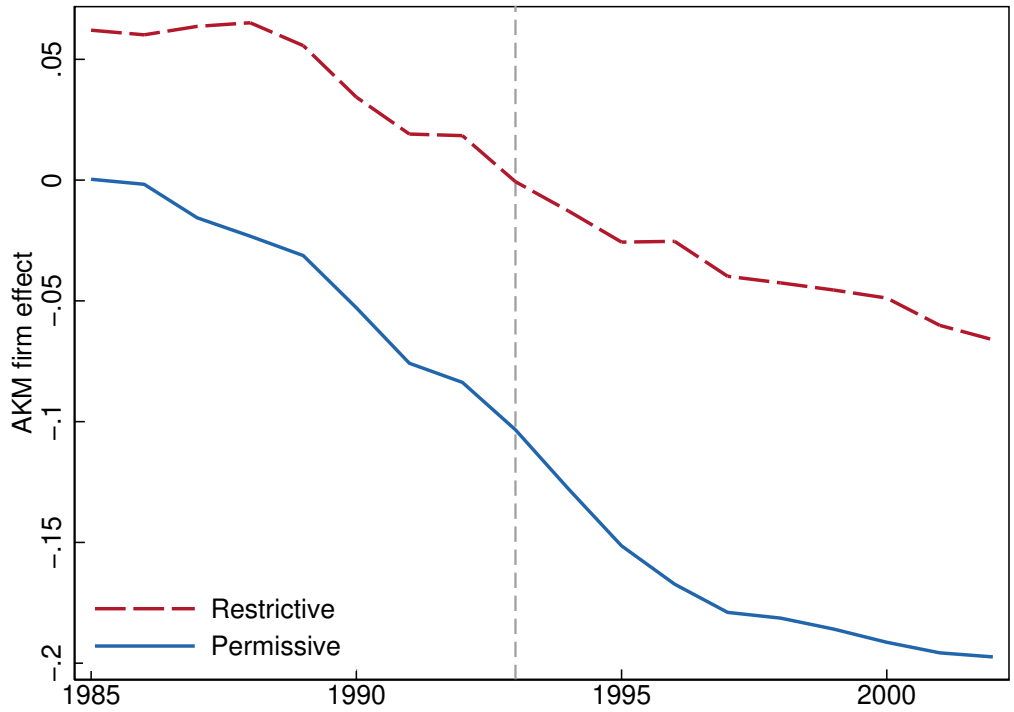
Note: Figure plots coefficient γ_τ from a difference-in-differences regression measuring the impact of a firm occupational layoff on incumbent direct-hire security guards, where the control group are similar workers in establishments that did not have an occupational layoff. The outcome variable is monthly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

Figure E.3: Effect of occupational layoff on incumbent employment, by initial firm wage premia



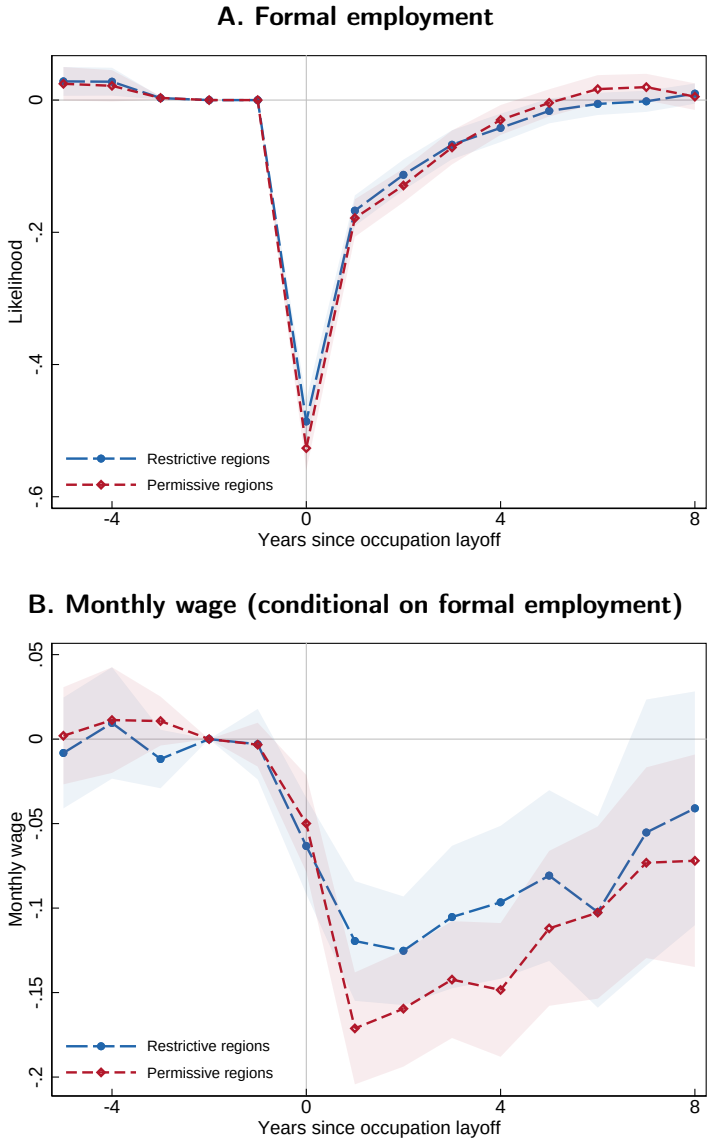
Note: Figure replicates Figure 4, Panel B using subsamples that include only workers initially in the top and bottom quartile of the AKM firm effects distribution, respectively.

Figure E.4: Trends in average firm-specific wage premia among security guards



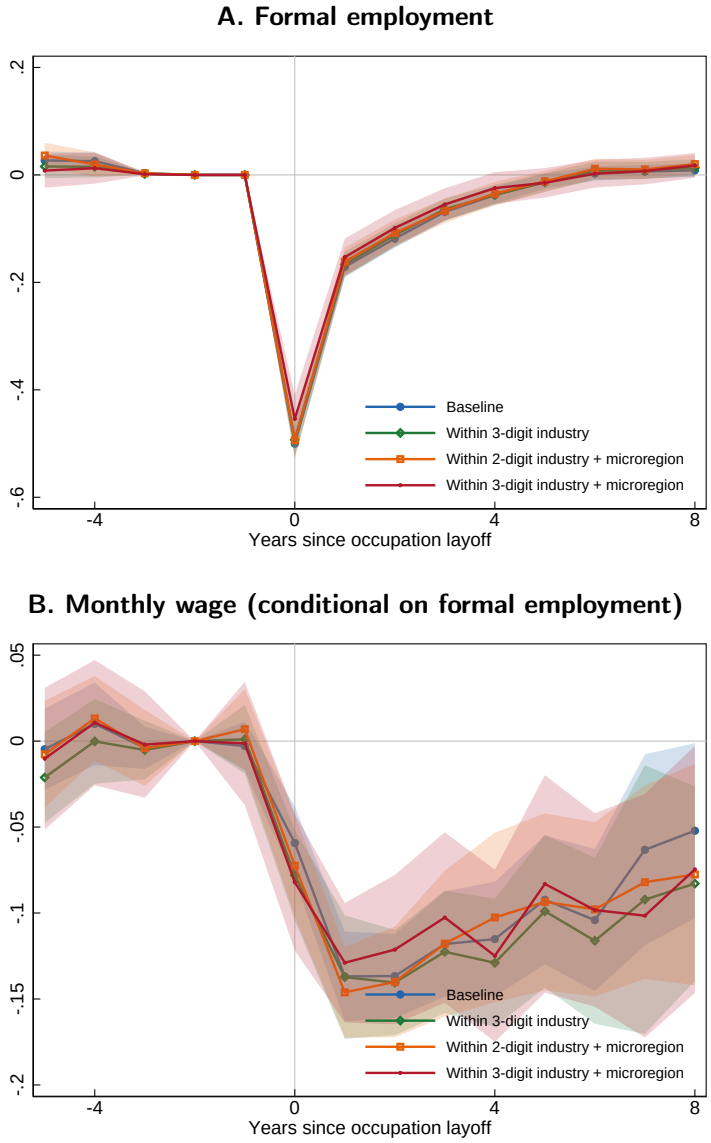
Note: Figure plots the trend in the mean AKM firm effect among security guards, averaged over microregions in our estimation sample with equal weights, separately for permissive and restrictive regions.

Figure E.5: Effect of occupational layoff on incumbent guards, by region



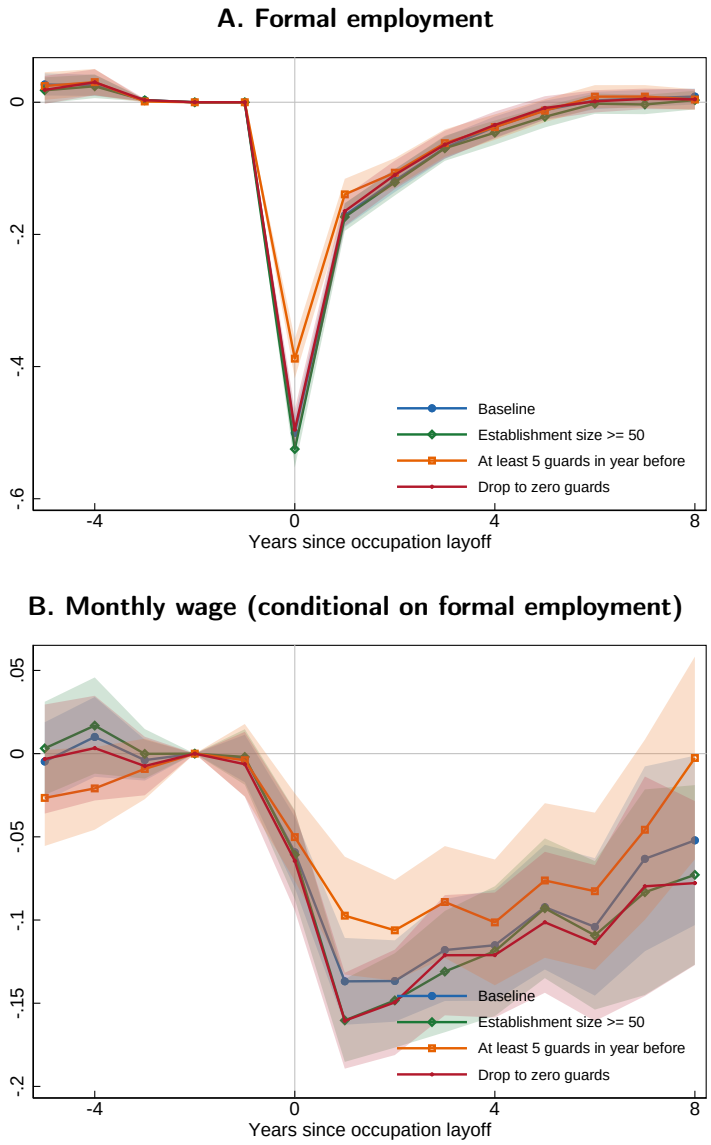
Note: Figure replicates Figure 4, Panels B and D using subsamples including only restrictive and permissive microregions, respectively.

Figure E.6: Effect of occupational layoff on incumbent guards, alternative matching strategies



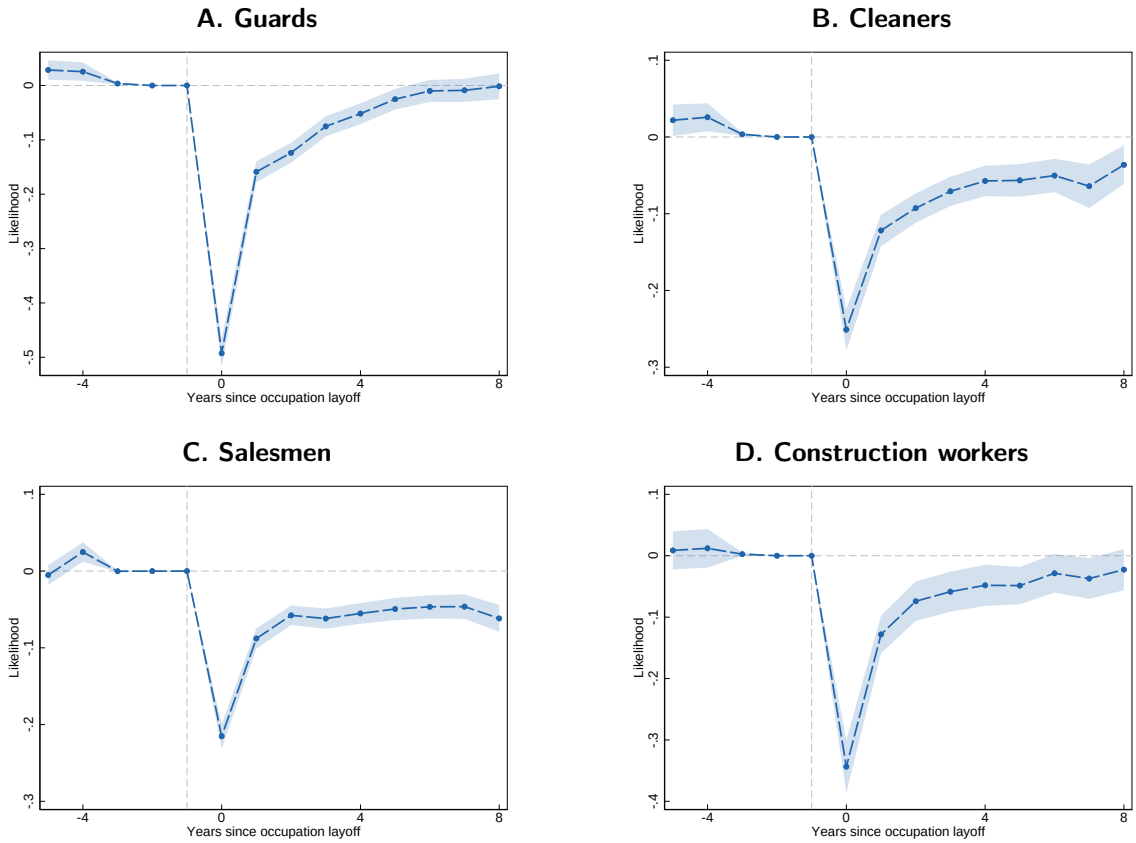
Note: Figure replicates Figure 4, Panels B and D with alternative matching strategies. The baseline specification matches workers within the 2-digit industry and local regional court jurisdiction. The alternative specifications matches within the 3-digit industry and local regional court jurisdiction (in green), within the 2-digit industry and microregion (in orange), and within the 3-digit industry and microregion (in red).

Figure E.7: Effect of occupational layoff on incumbent guards, alternative definitions



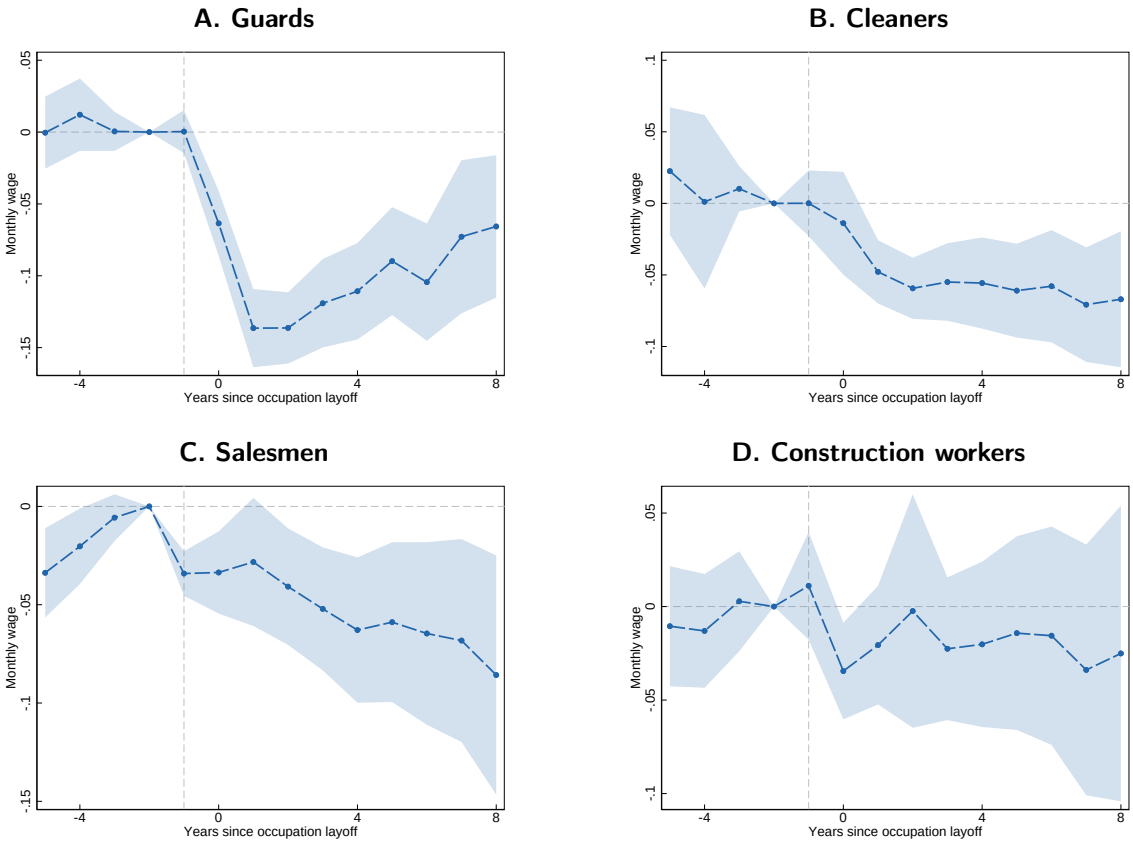
Note: Figure replicates Figure 4, Panels B and D with alternative definitions of occupational layoffs. The alternative specifications restricts to outsourcing establishments with at least 50 employees (in green), at least 5 security guards (in orange), or considers events where the number of security guards drops to zero (in red).

Figure E.8: Effect of occupational layoff on incumbent employment, other occupations



Note: Figure replicates Figure 4, Panel B for occupational layoffs in other occupations.

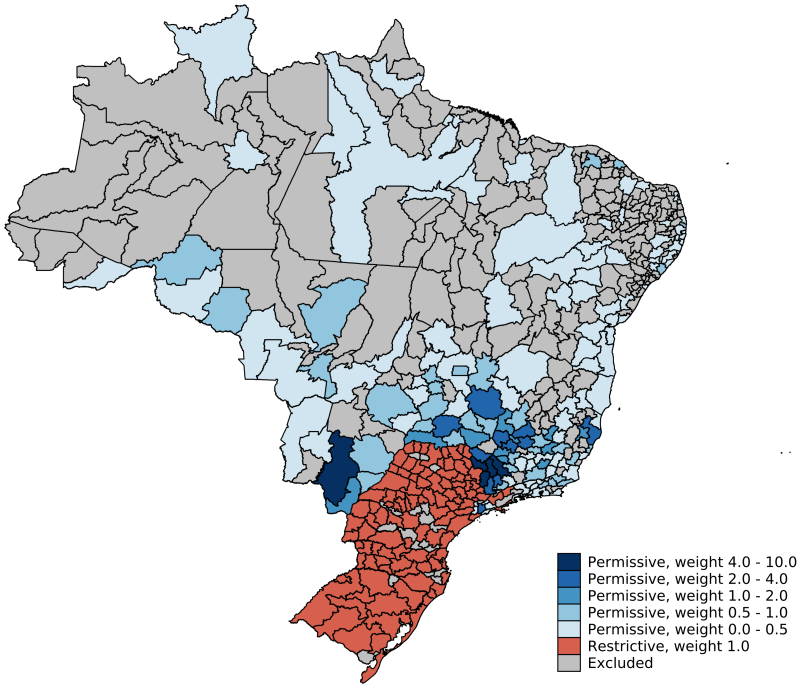
Figure E.9: Effect of occupational layoff on incumbent wage, other occupations



Note: Figure replicates Figure 4, Panel D for occupational layoffs in other occupations.

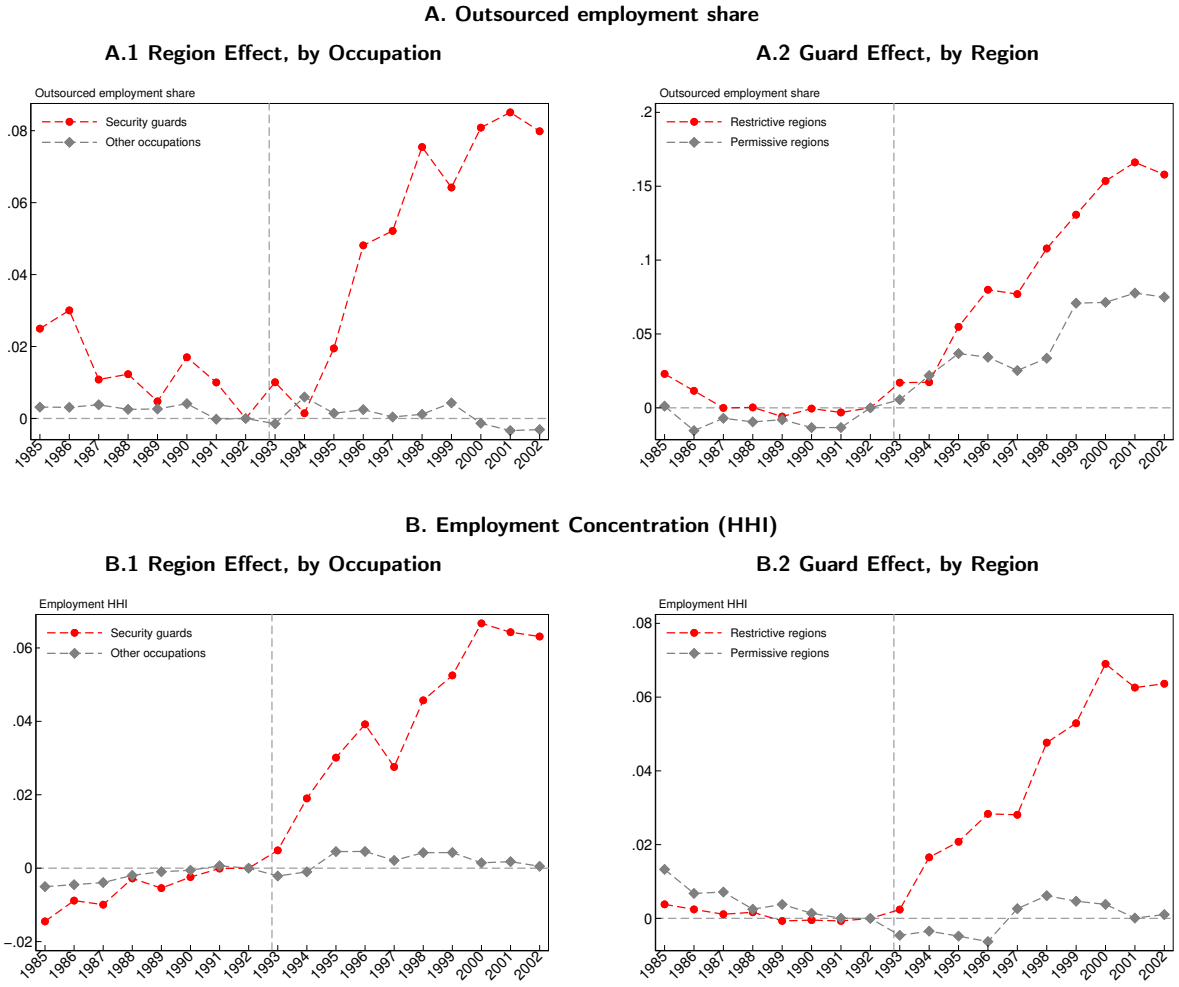
F Market-level effects of legalization: Additional results

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



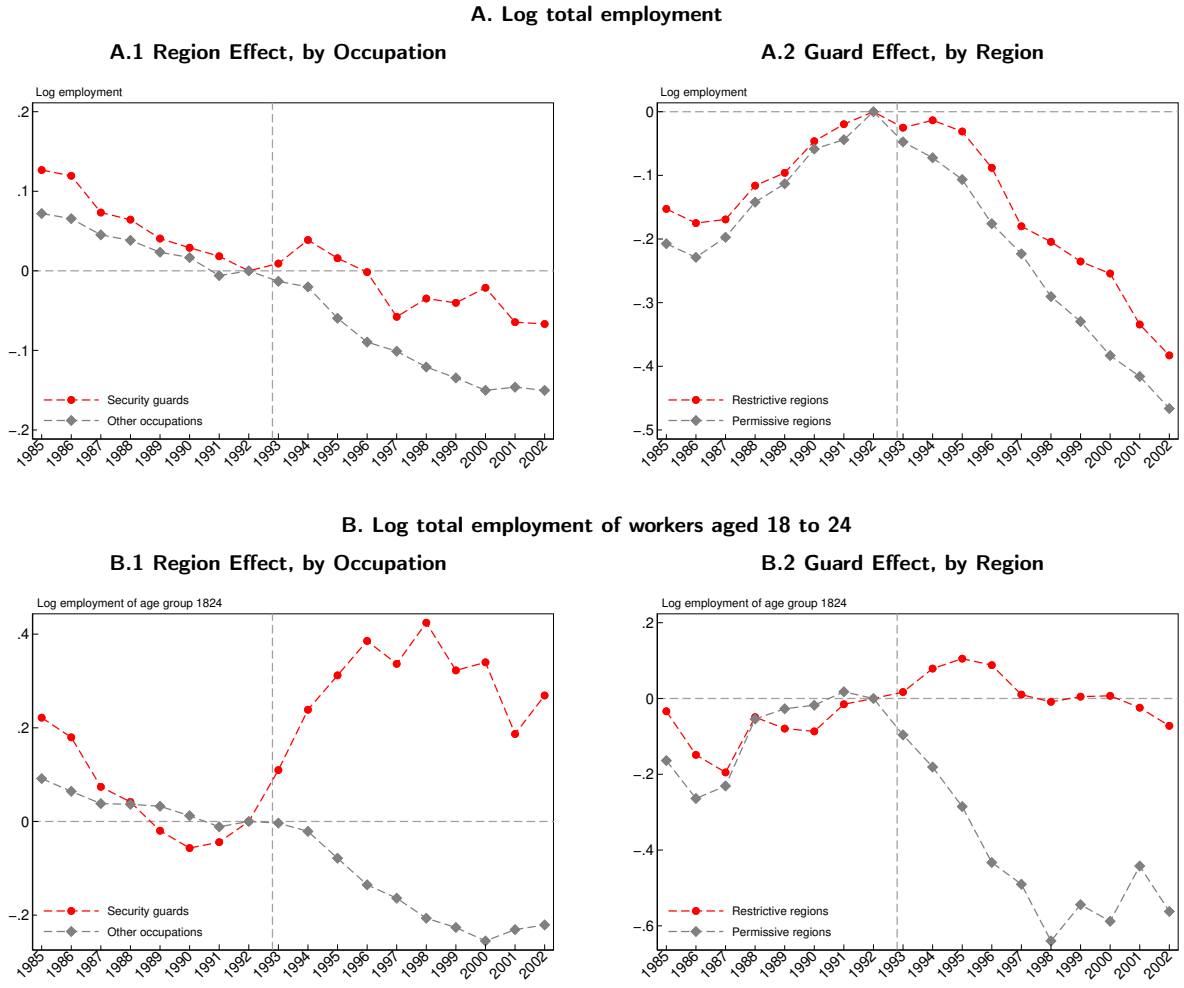
Note: Map shows microregions used in our main specifications, where observations are weighted using entropy balancing weights. Entropy balancing weights are computed using log employment, homicide rate, unemployment rate, share of employment in tradable industries, log formal employment of importers,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

Figure F.2: Differences in outsourcing prevalence: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").



Note: The left panel plots α_τ coefficients from the difference-in-differences regression $y_{rt} = \sum_{\tau=1985;\tau\neq1992}^{2002} \alpha_\tau \left(T_r \times 1_{t=\tau}\right) + \delta_r + \delta_t + \epsilon_{rt}$, where $T_r = 1$ if microregion r is under the jurisdiction of a restrictive regional labor court, zero otherwise, δ_r and δ_t are microregion and year fixed effects, respectively, separately estimated for guards versus for other occupations in microregion r. A microregion's outcome for other occupations are averages across occupations, with equal weights. The resulting microregion-level regressions are weighted by microregion entropy balancing weights. The right panel plots α_τ coefficients from the difference-in-differences regression $y_{ot} = \sum_{\tau=1985;\tau\neq1992}^{2002} \alpha_\tau \left(T_o \times 1_{t=\tau}\right) + \delta_o + \delta_t + \epsilon_{ot}$ where $T_o = 1$ if occupation o is security guards, zero otherwise, δ_o and δ_t are occupation and year fixed effects, respectively, separately estimated for the occupations in restrictive versus permissive microregions. Occupation-level outcomes are averages across microregions, with microregion balancing weights. The resulting occupation-level regressions weigh occupations equally. Results in both panels are based on a balanced sample of 265 microregions and 12 large 2-digit occupational groups, with positive employment in all microregions in all years, accounting for over 98% of all security guards in the data. See Appendix B for details. Legalization's baseline year of 1992 is omitted in both panels.

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



Note: See footnotes to Figure F.2.

Figure F.4: Differences in wages: Region Effect (Restrictive vs. Permissive, "Court Restrictiveness") and Guard Effect (Guards vs. Others, "Law Bindingness").

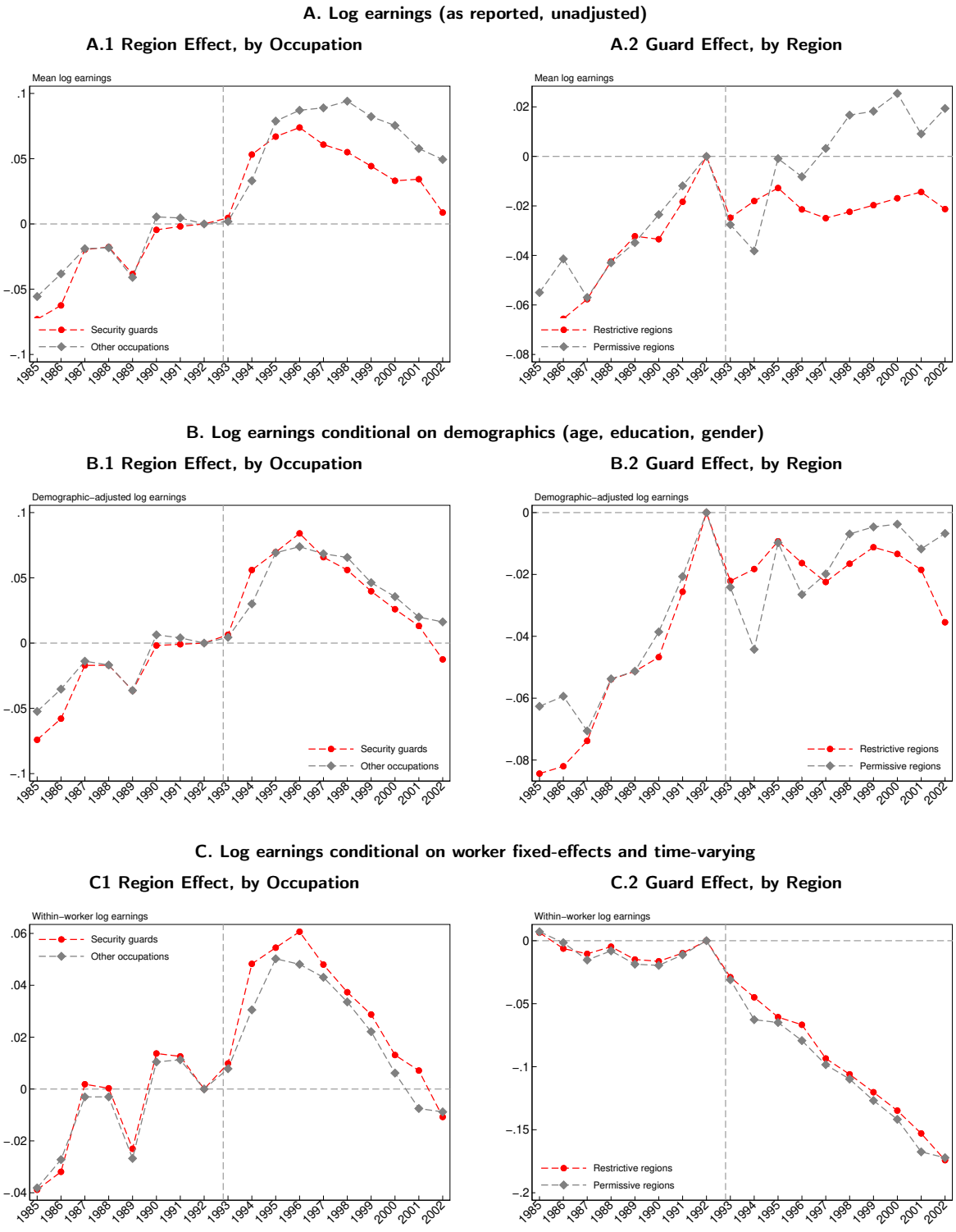
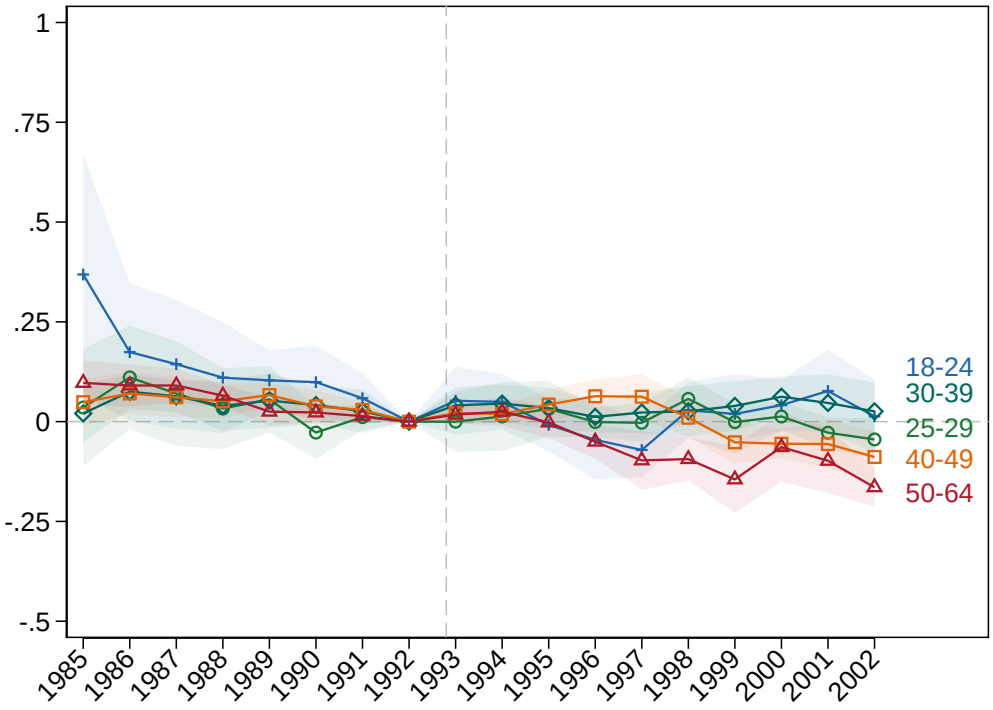
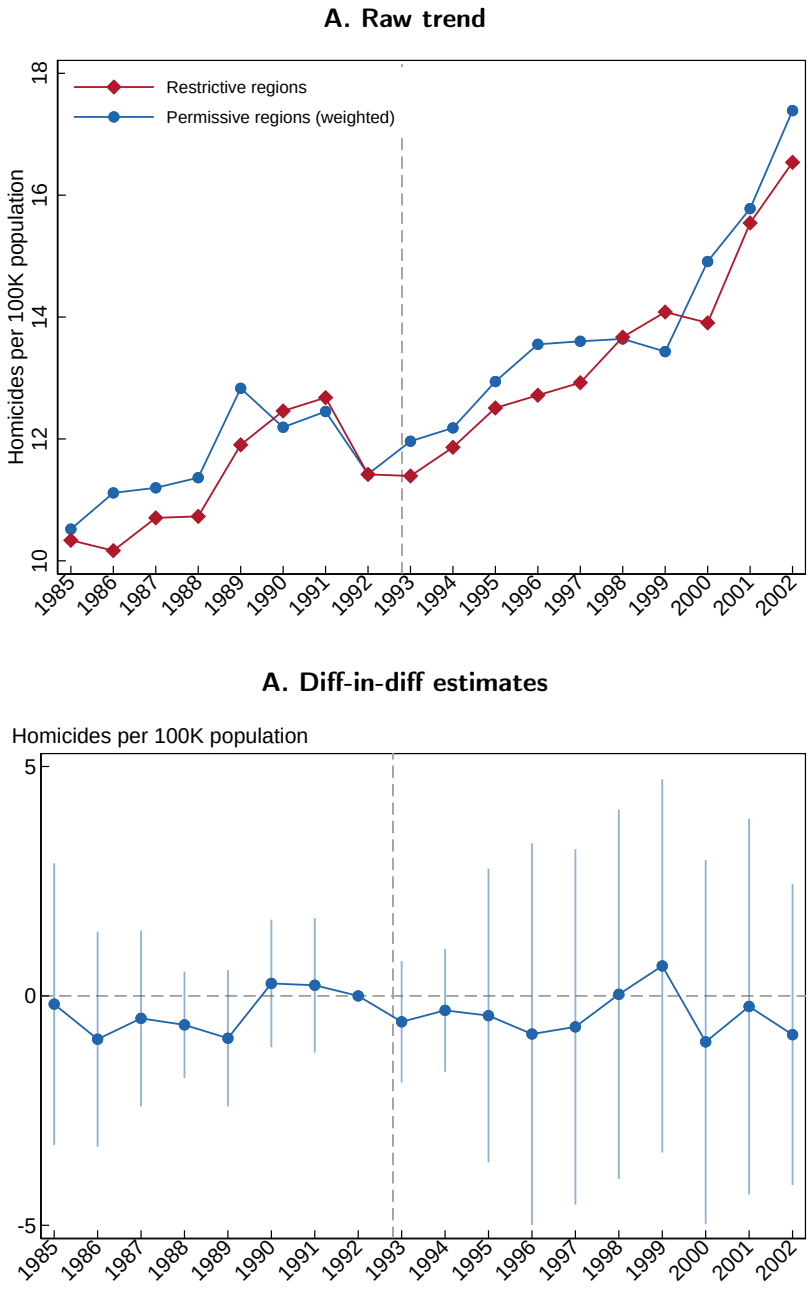


Figure F.5: Effect of legalization on employment of direct-hire incumbent guards



Note: See notes to Figure 7. The sample includes all workers in guard and control group occupations who were directly employed in 1992.

Figure F.6: Regional differences in homicide rates



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

Table F.1: Effect of legalization on employment: Robustness to balancing baseline covariates

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

Notes: See notes to Table 3. Log employment is expressed in natural logs.

Table F.2: Effect of legalization on employment composition: Robustness to balancing baseline covariates

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

Notes: See notes to Table 3. Log employment is expressed in natural logs.

Table F.3: Effect of legalization on wages: Robustness to balancing baseline covariates

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

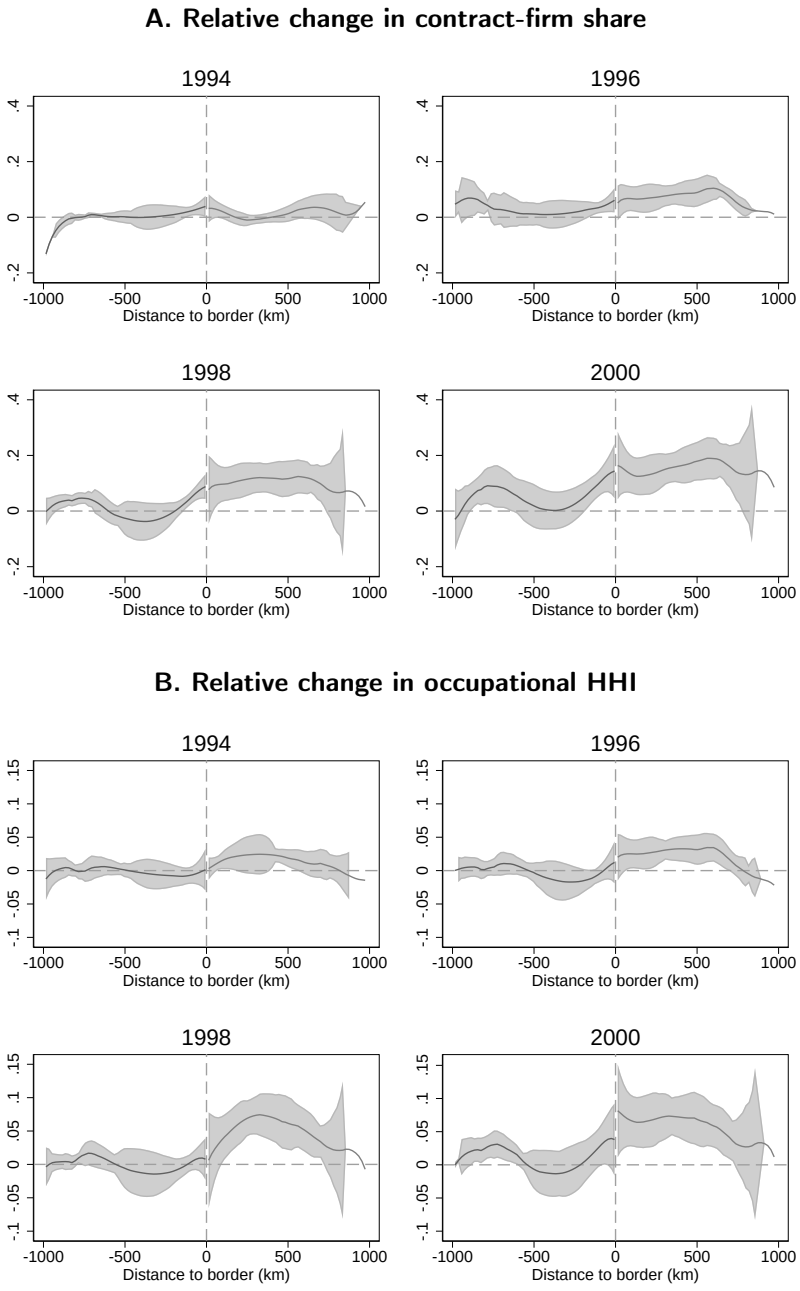
Notes: See notes to Table 6.

Table F.4: Effect of legalization on wages: Robustness to excluding São Paulo

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

Notes: See notes to Table 6.

Figure F.7: Changes in outsourcing prevalence and distance to the state border



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

G Interpretive Framework

In this appendix, we use a stylized framework to provide an interpretation of our empirical findings. We then use the framework to assess the welfare effects of Brazil's 1993 outsourcing legalization.

G.1 Model

There are two types of security guards $i \in \{N, I\}$, e.g., entrant and incumbent. There are two sectors $s \in \{E, O\}$, direct-hire or outsourced. In each sector, wages are determined via "right-to-manage" union bargaining with different levels of worker bargaining power.[57] Each sector also uses labor input to produces security services with different fixed proportions technologies. Security services from each sector are then combined with non-security labor to produce goods to consumers.

The timing is as follows: For each s, a union bargains with a firm over wages w_s, taking the wage in the other sectors as given. If no agreement is made, each party receives their outside option. Then, the firms choose the quantity of vacancies (L_O, L_E) to open. Due to fixed proportions technology, we have $L_{Ns} = a_s L_s$ and $L_{Is} = (1 - a_s) L_s$. There is an excess of workers, so all vacancies are filled. All unmatched workers earn reservation wage R.

We assume that outsourced and direct-hire labor are imperfect production substitutes because one or the other contracting form may be more efficient depending on production needs. Advantages of outsourcing include reduced shift management costs, fast replacements for sick or absent worker, reduced monitoring costs, reduced recruitment cost, improved screening, specialized capabilities, contracting flexibility in response to variable labor demand, and an overall reduction in cognitive and attention costs (see, e.g., Abraham and Taylor 1996; Houseman 2001). These benefits arise from economies of scale at the intermediary firm. Disadvantages of outsourcing include weakened incentives for specific investments and an

[57] This model is referred to in the literature as the "right-to-manage" model because bargaining is only over wages and the firm has the authority to unilaterally set the level of employment. See Kaufman (2004) for a review of union bargaining models. Also see Oswald (1985); Farber (1986).

overhead required to compensate the intermediary firm for its services (see, e.g., Autor 2003; Li and Wong 2024).

Specifically, we assume that each firm's production function is given by a constant-returns-to-scale CES function:

$$q\left(L_E, L_O, \tilde{L}\right) = \left(L_E^{\frac{\sigma-1}{\sigma}} + L_O^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}} \tilde{L}, \tag{5}$$

where q denotes the output produced, $\tilde{L}$ denotes non-security labor inputs, and $\sigma \in (0, \infty)$ is the elasticity of substitution.[58] For simplicity, we take $\tilde{L}$ as exogenous and normalize $\tilde{L} = 1$. Inverting this, the firm's cost per unit of q is:

$$c\left(w_E, w_O, \tau\right) = \left[w_E^{1-\sigma} + \left(w_O + \tau\right)^{1-\sigma}\right]^{\frac{1}{1-\sigma}}, \tag{6}$$

where τ denotes the legal cost of outsourcing per unit labor.

The firms compete monopolistically in the product market. As such, each faces a downward-sloping inverse firm-level demand function $p^f(q)$. The firm's profit is given by

$$\pi\left(w_E, w_O\right) = p^f(q)\, q - w_E L_E - \left(w_O + \tau\right) L_O.$$

Each union's payoffs is $u_s\left(w_s\right) = \left(w_s - R\right) L_s$. In the absence of agreement, the union workers earn their reservation wage R, so $u_E^0 = u_O^0 = 0$, and the firm earns zero profits, so $\pi_E^0 = \pi_O^0 = 0$.

Collective bargaining maximizes a generalized Nash objective. Let $\beta_E \in [0, 1]$ and $\beta_O \in [0, 1]$ denote the relative bargaining power of direct-hire and outsourced union, respectively. In the direct-hire sector, the Nash objective is:

$$\left[u_E\left(w_E\right) - u_E^0\right]^{\beta_E} \left[\pi\left(w_E, w_O\right) - \pi_E^0\right]^{1-\beta_E}.$$

[58] Direct-hire and outsourced labor are assumed to be symmetric in the production function. This assumption is natural in our setting since the outsourcing wage differential is small and the share of security guards who are outsourced after legalization is roughly one-half.

Analogously, the Nash objective in the outsourced sector is:

$$\left[u_O\left(w_O\right) - u_O^0\right]^{\beta_O}\left[\pi\left(w_E, w_O\right) - \pi_O^0\right]^{1-\beta_O}.$$

The optimal wages split the gains from trade, implying that bargaining outcomes are:

$$w_s = R + \beta_s \frac{S_s}{L_s},$$

where the "quasi-rents" associated with reaching agreement, given our assumptions, are:

$$\begin{aligned} S_E &= p^f\left(q\right)q - RL_E - \left(w_O + \tau\right)L_O \\ S_O &= p^f\left(q\right)q - w_E L_E - \left(\tau + R\right)L_O \end{aligned}$$

The firm then chooses q to maximize profits, taking wages as given:

$$\max_q p^f\left(q\right)q - c\left(w_E, w_O, \tau\right)q.$$

The firm's first-order condition is

$$p = \left(1 - \frac{1}{\epsilon_D^f}\right)c, \tag{7}$$

where ϵ_D^f is the firm-level price elasticity of product demand. Market-level demand is given by $D\left(p\right)$. Total employment is $L = L_E + L_O$ and the expected wage is $\overline{w} = sw_O + \left(1 - s\right)w_E$, where $s = L_O/L$.

Outsourcing legalization eliminates the tax on outsourced labor, τ. In response, the representative firm substitutes away from direct-hire labor towards outsourced labor. The magntitude of this adjustment depends on the substitutability of outsourced and direct-hire labor, the difference in labor input use in the two sectors, and the elasticity of demand for the product.

Suppose, for example, that outsourced and direct-hire labor are perfect substitutes (i.e., $\sigma = \infty$), the input shares are the same in the two sectors ($a_O = a_E$), and outsourced

workers have signficantly less wage bargaining power (i.e., $w_E \gg w_O$). In this case, a large legal cost τ prevents the firm from employing outsourced workers, even though they are equally productive and require lower wages. Outsourcing legalization induces the firm to hire outsourced labor instead and thereby lowers overall wages.[59]

Next, suppose that outsourced and direct-hire labor are imperfect substitutes (i.e., $\sigma \ll \infty$) and the two types of labor require the same wage (i.e., $w_E = w_O$). In this case, the legal cost τ distorts production away from the optimal mix of inputs and causes outsourced labor to have a higher marginal product than direct-hire labor. Outsourcing legalization increases production efficiency and has little impact on wages.[60]

G.2 Quantification

First-order approximations to the above model can be used to infer the changes in worker, firm, and consumer surplus from the reduced-form estimates in Section 4.2. For simplicity, we assume that legalization does not affect the reservation wage R.

Totally differentiating (5) around $\tau = 0$ yields the percentage change in product output as a linear function of the observed changes in employment:

$$\frac{dq}{q} = \frac{L_E^{\frac{\sigma-1}{\sigma}} \frac{dL_E}{L_E} + L_O^{\frac{\sigma-1}{\sigma}} \frac{dL_O}{L_O}}{L_E^{\frac{\sigma-1}{\sigma}} + L_O^{\frac{\sigma-1}{\sigma}}}.$$

Similarly, Equation (7) implies that the percentage change in product unit cost is given by:

$$\frac{dc}{c} = \frac{1}{\epsilon_D^m} \frac{dq}{q}$$

where ϵ_D^m denotes the market-level demand elasticity. Equation (6) then implies that the change in legal cost as a fraction of the outsourced wage can be obtained by incorporating

[59] Theoretically, legalization could alter alter w_E and w_O through affecting the size of quasi-rents. However, we assume that these effects are small relative to the difference between w_E and w_O. Therefore, the primary effect of outsourcing on wages is through reallocation of labor from the higher-wage direct-hire sector into the lower-wage outsourced sector.

[60] Once again, outsourcing legalization may affect w_E and w_O by altering firm quasi-rents. Here we assume that this effect is small.

information about the observed change in wages:

$$\frac{d\tau}{w_O} = \frac{\left(w_E^{1-\sigma} + w_O^{1-\sigma}\right)\frac{dc}{c} - w_E^{1-\sigma}\frac{dw_E}{w_E}}{w_O^{1-\sigma}} - \frac{dw_O}{w_O}.$$

The welfare effects of outsourcing legalization are then as follows. The change in consumer surplus as a fraction of consumer expenditure is:

$$\frac{dCS}{pq} = -\frac{1}{\epsilon_D^m}\left(1 + \frac{1}{2}\frac{dq}{q}\right)\frac{dq}{q}.$$

The percentage change in firm profit is:

$$\frac{d\pi}{\pi} = \frac{1 + \epsilon_D^m}{\epsilon_D^m}\frac{dq}{q}$$

The change in worker surplus as a fraction of total wagebill is:

$$\frac{dWS}{\overline{w}L} = \frac{d\overline{w}}{\overline{w}} + \frac{dL}{L}\left(1 - \frac{R}{\overline{w}}\right).$$

To form estimates of the above quantities, we use the estimated dL/L, dw/w, dL_E/L_E, dw_E/w_E in Table 4 and 6. For wage and employment levels, we use national-level averages between 1994 and 1996. For the unknown structural parameters, we use values that previous research has considered plausible. For the market-level demand elasticity ϵ_D^m, we use $\epsilon_D^m = -\frac{3}{2}$ as our main estimate and report a range from $-\frac{3}{4}$ to -3. Following estimates in Chan (2023) of the elasticity of substitution between direct-hire and outsourced workers, we set the substitution elasticity to $\sigma = 2$ and report a range from 1 to 4. We set the inverse wage markup to be $R/\overline{w} = 0.9$, since our reduced-form estimates shows that incumbent wages fall by roughly 10 percent after occupational layoffs, and these effects seem to be driven by loss of firm rents.[61] Note that none of the above estimates depend on assumptions regarding the firm-level demand elasticity ϵ_D^f.

[61] The literature also estimates a 10-25 percent union wage effect (Blanchflower and Bryson 2004).

Table F.5: Welfare effects of outsourcing legalization

	Main specification	σ=1	σ=4	ε_D^m=-3/4	ε_D^m=-3
	(1)	(2)	(3)	(4)	(5)
Final good consumption (dq/q)	0.061	0.069	0.056	0.061	0.061
	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)
Unit cost of final good (dc/c)	-0.040	-0.046	-0.038	-0.081	-0.020
	(0.008)	(0.009)	(0.008)	(0.016)	(0.004)
Legal cost of outsourcing (dτ/wo)	-0.104	-0.120	-0.090	-0.180	-0.065
	(0.051)	(0.051)	(0.049)	(0.051)	(0.052)
Consumer surplus (dCS/cq)	0.042	0.048	0.039	0.083	0.021
	(0.009)	(0.009)	(0.008)	(0.017)	(0.004)
Firm profit (dπ/π)	0.020	0.023	0.019	-0.020	0.040
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Worker surplus (dWS/wL)	0.008	0.008	0.008	0.008	0.008
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)

Notes: This table displays the estimated changes in equilibrium outcomes. Our main specification assumes that $\epsilon_D^m = -3/2$ and $\sigma = 2$.

G.3 Results

Table F.5, Column (1) shows the estimates. They suggest that outsourcing legalization caused output to increase by roughly 6 percent. Unit cost correspondingly fell by 4 percent. The legal cost of outsourcing fell by 10 percent of the outsourced worker wage. Consumer surplus increased by 4 percent of the product expenditure. Firm profit increased by 2 percent. Worker surplus increased by less than one percent of the occupational-level wagebill. Since we do not detect overall changes in average wage, the increase in worker surplus is entirely attributable to the increase in total employment. The change in worker surplus, however, is not statistically significant.

The remaining columns display estimations results with alternative assumptions regarding σ and ϵ_D^m. If demand were less elastic, the implied decrease in unit cost and increase in consumer surplus are both larger. Similarly, if outsourced and direct-hire workers were less

substitutable, then the implied increase in consumer surplus is larger and more statistically significant. The implied change in worker welfare does not depend on σ or ϵ_D^m.