

THE DISPLACEMENT EFFECTS OF DOMESTIC OUTSOURCING

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The Displacement Effects of Domestic Outsourcing*

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Abstract

Evidence that domestic outsourcing lowers pay comes largely from on-site transfers, in which workers move to a contractor but keep the same jobs. *Displacement* is rarely observed: whether workers lose their jobs, where they go, how earnings evolve. In Brazil's 1993–1994 pro-outsourcing reforms, which differentially affected security guards, such transfers were rare; firms instead used *occupational layoffs*, shedding their guards while keeping other workers. Displaced guards' employment recovered within five years, but many changed occupations and wages stayed about 12% lower. Lifetime losses average 1.2 to 1.5 years of pre-layoff earnings, concentrated among workers from high-wage firms, reflecting lost premia.

Keywords: domestic outsourcing, worker displacement, firm wage premia, job loss
JEL: J63, J31, L24

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1 Introduction

Firms across many countries have increasingly contracted out activities once performed in house (security, cleaning, catering, logistics) to specialized service providers, reshaping the boundaries of the firm and the organization of low- and middle-wage work (Weil, 2014). A growing body of evidence shows that this form of domestic outsourcing lowers the pay of the affected workers, who lose access to the wage premia that their former employers had shared with them (Goldschmidt and Schmieder, 2017; Dube and Kaplan, 2010; Drenik et al., 2023). Most of this evidence, however, is inferred from the wage gap between outsourced and in-house workers performing similar jobs. Direct evidence on the workers who are *displaced* when a firm decides to outsource—whether they lose their jobs, where they go, and how their earnings evolve—remains scarce, because the moment of displacement is rarely observed in the data used to study outsourcing.

This paper studies the incidence and worker-level consequences of firm outsourcing decisions, using Brazil’s 1993–1994 reform of security-service outsourcing regulation. In December 1993, the Superior Labor Court’s *Súmula* 331 made the outsourcing of non-core activities broadly lawful, and a 1994 federal statute (Lei 8.863) raised the cost of directly employing security guards relative to contracting them; together these reforms triggered a rapid expansion of specialized security firms. We describe the setting in Section 2; its market-level consequences are studied in Felix and Wong (2026). Our focus here is the firm-level event: which establishments shed their guards, and what happened to the incumbents they let go. Brazil’s matched employer–employee data (RAIS) record each worker’s establishment, occupation, and wage annually, allowing us both to identify outsourcing events at the establishment level and to follow affected workers over time.

Our first finding is about measurement: the outsourcing event the literature relies on barely occurs in our setting. That event is *on-site outsourcing*, a block of workers moving together from a client firm to a contractor while keeping the same job at the same site (Goldschmidt and Schmieder, 2017). Between 1990 and 2000 we identify only 107 on-site outsourcing events in the security guard occupation (and just 27 under the stricter definition of Goldschmidt and Schmieder (2017)), together affecting about 0.7% of the nation’s security guards. The common margin of adjustment was instead what we call an *occupational layoff*: an establishment that reduces or eliminates its guards while keeping its other workers in place. Occupational layoffs were an order of magnitude

more common, averaging 471 per year, affecting about 8.4% of security guards, and accounting for roughly 72% of the post-reform decline in direct-hire guard employment.¹ Studies that rely on on-site events therefore capture only a small and selected share of the worker separations that outsourcing sets in motion.

We next estimate the effect of occupational layoffs on the incumbents caught in them, using a matched difference-in-differences design that compares long-tenured guards displaced by an occupational layoff to otherwise similar guards at establishments that did not outsource. Displacement is large but employment recovers: in the year of the layoff, an affected guard is 50 percentage points less likely to be formally employed, yet the gap closes steadily thereafter, to about 2.5 percentage points by year five and to zero by the end of our window. Wages tell a different story. They fall by about 19% in the year after the layoff and remain roughly 12% below the counterfactual five years out, in line with the persistent earnings losses documented for mass layoffs in Brazil (Britto et al., 2022; Sachdev and Dal-Ri, 2025). This persistent loss is not an artifact of selection: by then the employment rates of the two groups have converged. Most displaced guards do not return to formal employment as guards; they reallocate to other occupations. Summing the discounted earnings losses over a long horizon, the implied present value is equivalent to 1.2 to 1.5 years of pre-layoff earnings, a magnitude comparable to estimates of the cost of job displacement in the United States (Davis and von Wachter, 2011).

These losses are concentrated, and tied to the pay policies of the firms that outsource. High-wage establishments were more likely to shed their guards, and the wage losses fall almost entirely on workers who began at high-paying firms. Incumbents from the top quartile of the firm wage-premium distribution move to firms that pay substantially less; those from lower-paying firms see no significant change. Decomposing wages into worker and firm components following Abowd et al. (1999), we find that the loss of firm-specific wage premia accounts for 41% to 47% of incumbents' wage losses. At the same time, the average firm wage premium in the occupation as a whole shows no break around the 1993 reform, underscoring that the displacement we measure is a selected, firm-level phenomenon rather than a market-wide repricing of guards.

¹Section 3 presents evidence that these patterns are guard-specific rather than an effect of Brazil's concomitant trade liberalization (Kovak, 2013; Dix-Carneiro and Kovak, 2017). A likely reason on-site transfers are rare is that Brazilian law prohibits nominal wage reductions for continuing workers, which is generally understood to bar firing and rehiring a worker through an intermediary to perform the same job at a lower wage. See Articles 453 and 468 of the *Consolidação das Leis do Trabalho*; Portaria MTB 384/1992; and Law 6.019, Article 5°-D.

Our findings speak to several literatures. The first studies the wage effects of domestic outsourcing, much of it building on [Goldschmidt and Schmieder \(2017\)](#), who identify outsourcing from the flow of a block of workers from a firm to a business-services contractor and show that the affected workers lose firm wage premia. This worker-flow design has since been applied across countries: to the United States by [Dorn et al. \(2018\)](#), to France by [Bergeaud et al. \(2025\)](#), and to Türkiye by [Gürer and Taymaz \(2026\)](#), with an analogous design used for temporary-agency work in Argentina by [Drenik et al. \(2023\)](#). Our contribution here is one of measurement. In Brazil, the on-site event these studies rely on is rare, accounting for about 0.7% of guards; the dominant margin is instead the occupational layoff, a separation that establishment-level data reveal but that worker-flow definitions miss. Because firm-level event studies follow only the workers who move to a contractor, they capture a selected slice of the separations that outsourcing sets in motion.

We contribute to two further literatures. First, we provide direct, worker-level evidence of *displacement*, following the incumbents an outsourcing firm lets go rather than inferring effects from cross-sectional gaps between outsourced and in-house workers ([Dube and Kaplan, 2010](#); [Weil, 2014](#)). By tracing their employment, wages, and occupational mobility, and by decomposing wages into worker and firm components, we connect outsourcing to the literature on the costs of job displacement ([Jacobson et al., 1993](#); [Davis and von Wachter, 2011](#); [Lachowska et al., 2020b](#); [Schmieder et al., 2020](#)). Second, because we study a court ruling that lowered the cost of outsourcing, we add to recent work on outsourcing regulation, which has mostly examined the opposite policy: bans and restrictions in Peru ([Jiménez and Rendón, 2025](#)), Mexico ([Estefan et al., 2024](#)), and Türkiye ([Aytun et al., 2026](#)). Evidence on a liberalizing reform is comparatively scarce.

Finally, this paper is the firm-level companion to [Felix and Wong \(2026\)](#), who study the same reform at the market level. The displacement we document is the distributional counterpart to those aggregate effects, and a reminder that firm-level event studies, which by construction follow only the workers a firm lets go, can diverge from a reform’s market-level consequences.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and data. Section 3 documents the incidence of outsourcing events, contrasting on-site outsourcing with occupational layoffs. Section 4 estimates the effects of occupational layoffs on incumbent workers. Section 5 examines the role of firm-specific wage premia. Section 6 concludes.

2 Institutional Setting and Data

This section describes the regulatory reform that drove the expansion of outsourcing in Brazilian security services, the administrative data we use, and how we measure outsourcing. We keep the treatment brief and refer the reader to [Felix and Wong \(2026\)](#) for the full institutional history and the market-level analysis.

2.1 The 1993–1994 reform of security-service outsourcing

The legality of domestic outsourcing in Brazil was unsettled for much of the twentieth century. In 1986, the Superior Labor Court issued *Enunciado* 256, which treated the hiring of workers through an interposed firm as unlawful except in narrowly enumerated cases: temporary work (Law 6.019 of 1974) and banking security (Law 7.102 of 1983). This changed on December 17, 1993, when the Court issued *Súmula* 331, a sweeping precedent declaring the outsourcing of non-core (*atividade-meio*) activities lawful for any firm, with the contract firm recognized as the legal employer of the outsourced workers. The expected legal cost of contracting out non-core functions such as security fell.

A second, less-noticed change worked in the same direction for security guards specifically. In March 1994, Lei 8.863 amended the federal statute governing private security (Lei 7.102 of 1983), extending its licensing, training, and authorization requirements to establishments that employ armed guards in house. The reform thus raised the cost of *directly* employing guards at the same time that *Súmula* 331 lowered the cost of *contracting* them. Together, the two reforms increased the relative cost of in-house security and set off a rapid expansion of specialized security firms.

Pre-existing regional differences in how labor courts viewed outsourcing shaped where the reform bit hardest. Courts in Brazil’s South—Rio Grande do Sul, Paraná, and Santa Catarina, together with the Campinas region of São Paulo—had interpreted the pre-1993 rules restrictively, while courts elsewhere were more permissive. While we find that layoffs were slightly more prevalent in restrictive regions (Appendix Figure [A.2](#)), this regional variation is most relevant for the market-level effects studied in [Felix and Wong \(2026\)](#), as opposed to estimating the effects of outsourcing events on incumbents, whose trajectories we can compare against matched workers, as standard in this literature ([Goldschmidt and Schmieder, 2017](#)).

2.2 From in-house to contracted security

The aggregate consequence of the reform was a pronounced substitution away from in-house guards. [Felix and Wong \(2026\)](#) show that the share of large establishments directly employing at least one security guard fell steadily after 1993 across every major sector, from roughly 60% in 1992 to less than 40% in 2002, while the share of guards employed by contract firms rose steadily. Behind this aggregate shift lie the establishment-level decisions (firms letting go of the guards they had employed directly) whose incidence and worker-level consequences we study in this paper.

2.3 Data and measurement of outsourcing

We use Brazil’s matched employer–employee administrative data, the *Relação Anual de Informações Sociais* (RAIS), covering 1985–2002. RAIS records, for each worker–establishment pair and year, the worker’s occupation, average monthly wage, demographics, and the establishment’s detailed industry. We follow standard practice and focus on full-time private-sector workers aged 18–64. Security guards are a large, overwhelmingly formal, medium-wage occupation, accounting for about 3% of private-sector formal employment in the early 1990s. They are well suited to a study of displacement: with roughly four in five formally employed, we can track both the incumbents an establishment lets go and the workers who replace them through the employer–employee links in RAIS.

We identify outsourcing using the fact that RAIS assigns contract firms to specific industry codes (industry class 74). A worker employed by a contract firm is classified as outsourced; a worker employed by any other private-sector firm is classified as a direct hire. Because RAIS does not record where an outsourced worker is posted, we cannot observe the client–contractor relationship directly; instead, we identify outsourcing from establishment-level changes in direct-hire employment, as described in [Section 3](#). Further detail on the data, occupation codes, and outsourcing measure is provided in [Felix and Wong \(2026\)](#).

3 Incidence of Outsourcing Events

Much of what we know about the worker-level effects of outsourcing comes from firm-level *outsourcing events*. We therefore begin by asking what such events looked like in Brazil after the reform.

3.1 Defining outsourcing events

We distinguish two types of event. An *on-site outsourcing* event is one in which a group of workers flows together from a direct employer to a contract firm, presumably continuing to perform the same job at the same site; this is the event studied by [Goldschmidt and Schmieder \(2017\)](#) and much of the subsequent literature ([Dorn et al., 2018](#); [Bergeaud et al., 2025](#); [Gürer and Taymaz, 2026](#)). We identify an on-site event as a group of at least three guards who are employed at a non-business-service establishment with at least ten employees in one year and at a business-service (contract) establishment the next, excluding cases in which the origin establishment’s non-guard employment fell by more than 10 percent, which would signal a broader contraction. [Goldschmidt and Schmieder \(2017\)](#) use a stricter variant, additionally requiring the predecessor establishment to have at least 50 employees, to survive and not shrink by more than half, and the flow to represent less than 30 percent of its workforce.

An *occupational layoff* is one in which an establishment reduces or eliminates its employment in a single occupation while its other workers remain in place. We classify establishment j as having an occupational layoff in year t if it had at least ten employees, employed at least three guards in years $t - 1$ and $t - 2$, and its guard headcount fell by at least two-thirds from $t - 1$ to t while its non-guard employment fell by less than 10 percent, and it was neither a contract firm nor a government entity.² The restriction on non-guard employment distinguishes a firm shedding a single function from a firm-wide contraction or mass layoff.

²To avoid misclassifying normal headcount fluctuations, for establishments with five or fewer guards we additionally require the guard count to fall to zero.

3.2 Frequency of the two event types

On-site outsourcing was rare. Between 1990 and 2000 we identify only 107 on-site outsourcing events in the security guard occupation. These events affected 2,842 guards, about 0.7% of Brazil's security guards. Applying the stricter definition of [Goldschmidt and Schmieder \(2017\)](#) yields just 27 events affecting 1,061 guards, less than 0.25% of guards nationally. The rarity of on-site outsourcing is notable, because it is the event that prior work uses to estimate the effects of outsourcing on workers. A likely explanation is that Brazilian law prohibits nominal wage reductions for continuing workers, which is generally understood to bar the practice of firing and rehiring a worker through an intermediary to perform the same job at a lower wage.³

Occupational layoffs were the common margin. By contrast, occupational layoffs were an order of magnitude more common. Between 1990 and 2000 they averaged 471 per year and affected 35,544 security guards, about 8.4% of the nation's guards and roughly 72% of the total decline in direct-hire guard employment over the period.⁴ Appendix Figure [A.1](#) illustrates the event: at establishments we classify as having an occupational layoff, guard employment collapses in the event year while employment in other occupations continues along its prior path, the signature of a firm shedding a function rather than contracting overall.

The reform shifted the margin. Figure [1](#) plots the establishment-level likelihood of each event type by year. Both series are flat before 1993. After the reform, the incidence of guard occupational layoffs jumps by about 2.7 percentage points in 1994 (more than half the pre-reform level) and remains elevated for several years, while the incidence of on-site outsourcing barely moves.

Importantly, the patterns in Figure [1](#) exclude manufacturing firms, which were directly affected by Brazil's concomitant trade liberalization ([Kovak, 2013](#); [Dix-Carneiro and Kovak, 2017](#); [Felix, 2026](#)), the effects of which one might be concerned could confound the effects of the guard-directed pro-outsourcing reform we study. Additional evidence points to the patterns in Figure [1](#) being guard-specific and different from the overall effect of trade liberalization on formal employment.

³See Articles 453 and 468 of the *Consolidação das Leis do Trabalho*; Portaria MTB 384/1992; and Law 6.019, Article 5°-D. Direct, like-for-like substitution of an incumbent by an outsourced worker at the same site is the transaction these provisions discourage.

⁴We compute this share as the number of guards displaced by occupational layoffs divided by the net fall in direct-hire guard employment over 1990–2000.

First, looking within establishments over time, the decline in the share of establishments directly employing guards is equally clear among establishments that survived trade liberalization, namely those observed continuously from 1988 through 2002 (Appendix Figure A.3). Second, looking across occupations, the decline in the share of establishments directly employing each occupation is much larger for guards than for other comparable two-digit occupations (Appendix Figure A.4).

Finally, looking within establishment-occupation pairs over time, guard layoffs increased by 2.7 percentage points more after the reform than layoffs in other occupations at the same establishment in the same year (Appendix Table A.1). The establishment-year fixed effects in this specification absorb the establishment’s exposure to Brazil’s trade liberalization, as well as any other shock shared across its occupations.

4 Effects of Occupational Layoffs on Incumbents

Since the outsourcing events we detect take overwhelmingly the form of occupational layoffs, we now ask how these layoffs affected the incumbent guards caught in them. We document four facts: the displaced were pushed out of formal employment but recovered within a few years (Sections 4.1–4.2); their wages fell persistently (Section 4.2); the losses were borne by workers who stayed in the occupation even as most others left it (Section 4.3); and the implied lifetime losses were large (Section 4.4).

4.1 Design: a matched difference-in-differences

We compare long-tenured security guards who were directly affected by an occupational layoff between 1990 and 2000 to similar guards who were not. The treatment group consists of guards who did not continue in their jobs as direct-hire guards when their employer eliminated a large share of such jobs; these workers may either have separated from the establishment or been reassigned to another occupation within it.⁵ We restrict both the treated and comparison pools to guards employed

⁵This definition is natural for studying occupational layoffs but differs from the mass-layoff literature, in which 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](#)). We also do not condition on treated workers being transferred to a contract firm, so our estimates are conceptually different from those of [Goldschmidt and Schmieder \(2017\)](#).

at the same establishment for at least three consecutive years before the event, so that we study long-tenured incumbents. For each treated guard we form potential controls from establishments with no occupational layoff in the same two-digit industry, five-digit occupation, and regional court jurisdiction, and select the one with the closest propensity score from a probit of layoff exposure on wages two and three years prior, tenure, and the firm wage premium (the AKM firm effect introduced in Section 5.1). The matched sample comprises 12,199 displaced guards and an equal number of controls, and the resulting covariate balance appears in Appendix Table A.2, columns (1) and (2).

We estimate the dynamic effect of an occupational layoff on incumbents with the worker-level difference-in-differences specification

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

where y_{it} is an outcome (formal employment or wages) for guard i in year t , D_i indicates that the guard was affected by an occupational layoff at their establishment in year t^* , α_i and τ_t are worker and year fixed effects, and X_{it} are time-varying demographics. Each coefficient δ_k measures the effect of the layoff k years after the event, relative to the worker's matched control. Throughout, event year zero (Y0) denotes the calendar year of the layoff itself, that is, the first year in which the establishment's guard count has fallen; year one (Y1) is the year after the layoff, and so on. We emphasize that these are partial-equilibrium effects on displaced incumbents; they are not the effects of the reform on the guard occupation as a whole, which include non-displaced incumbents and new entrants and which [Felix and Wong \(2026\)](#) study at the market level.

The identifying assumption is that, absent the layoff, displaced guards would have followed the same employment and wage path as their matched controls. Two design choices support it. Each treated guard is compared to a control drawn from establishments that never had an occupational layoff and aligned in event time, so the estimates do not rely on the contaminated comparisons that bias two-way fixed-effects estimators under staggered timing; and the event-study form of equation (1) lets us read pre-trends directly.

Two points of interpretation follow. Our estimand is the cost borne by the guards a firm releases, not the cost a randomly chosen guard would bear, and which guards are released need not be random

within the firm. The wage effects, in turn, are estimated for guards who remain formally employed; because the displaced who fare worst are the most likely to leave the formal sector, this conditioning, if anything, understates the average wage loss.

4.2 Displacement and persistent wage losses

Figure 2, Panel A shows that occupational layoffs displaced incumbents from formal employment. The pre-event coefficients are small, no larger than three percentage points, so the treated and control groups were on similar paths before the layoff. In the year of the layoff, an affected guard is 50 percentage points less likely to be formally employed. The effect then fades: the gap narrows to about 2.5 percentage points by year five and closes by the end of our window.

Wages, by contrast, fall persistently (Panel B). Log wages fall most in the year following the layoff, by about 19% (Appendix Table A.3, column 2), and never return to their pre-layoff path: five years after the event, displaced guards still earn roughly 12% less than they would have absent the layoff (column 3). Panel B, which plots wages relative to their pre-event level, shows the same pattern with somewhat smaller magnitudes, about 14% at the trough and 9% at year five. These persistent losses are comparable to the large and lasting earnings losses documented for mass layoffs in Brazil (Britto et al., 2022; Dal-Ri et al., 2024; Sachdev and Dal-Ri, 2025), and they complement a growing literature on the consequences of Brazilian mass layoffs for reallocation, entrepreneurship, and informality (Van Doornik et al., 2023; Da Mata et al., 2023; Corado, 2023; Martins-Neto et al., 2024; Dal-Ri, 2024). Because the employment gap is small by this horizon, differential selection out of employment is unlikely to account for a wage gap of this size, and the persistent loss is concentrated among guards who stay in the occupation rather than among those who exit (Section 4.3).⁶

4.3 Occupational reallocation and the role of stayers

Where did the displaced go? Appendix Table A.3 shows that most did not return to formal employment as guards. In the year of the layoff, affected workers are 76 percentage points less likely to be formally employed in the same occupation (column 1). Five years later they remain 12

⁶The results are robust to alternative matching strategies and alternative definitions of an occupational layoff (Appendix Figures A.5 and A.6).

percentage points less likely (column 3). By then, though, the gap in overall formal employment has narrowed to about 2.5 percentage points: they have moved to other occupations rather than left formal work. Few move to contract firms on impact: contract-firm employment among the displaced rises by only 0.2 percentage points in the year of the layoff, climbs to 7.5 percentage points the following year (column 2), and eases to 4.1 percentage points five years out.

The persistent wage loss is borne almost entirely by the minority of workers who *stay* in the guard occupation rather than the majority who leave it. Appendix Figure A.7 decomposes the wage effect and shows that it is concentrated among occupation stayers, which means the loss is not an artifact of compositional change among those who exit. The displacement pattern is, moreover, not specific to guards: occupational layoffs in other occupations—cleaners, salesmen, and construction workers—produce broadly similar employment and wage effects on their incumbents (Appendix Figures A.8 and A.9).

4.4 The magnitude of lifetime losses

To gauge the overall cost of displacement, we compute the present discounted value (PDV) of earnings losses, following Davis and von Wachter (2011). For our baseline specification, we sum the estimated annual losses over a twenty-year horizon at a real interest rate of 5%, extrapolating beyond the eighth post-event year (the last we observe) at a constant decay rate:

$$PDV_{Loss} = \sum_{k=0}^8 \hat{\delta}_k \frac{1}{(1+r)^k} + \sum_{k=9}^{19} \hat{\delta}_8 \frac{(1+\hat{\lambda})^{k-8}}{(1+r)^k}, \quad (2)$$

where $\hat{\delta}_k$ is the estimated earnings loss in year k from equation (1) and $\hat{\lambda}$ is the average annualized decay in the loss between years five and eight. A complication is that we do not observe earnings in the informal sector, so we report the PDV under a range of assumptions about the earnings of workers we no longer observe.⁷ Column (1) of Table 1 reports our baseline results, which use a 5% interest rate, a twenty-year horizon, and the estimated decay rate $\hat{\lambda}$. Across the four assumptions, the PDV of an occupational layoff is equivalent to roughly 1.2 to 1.5 years of pre-

⁷We use four assumptions that bracket the truth: that unobserved workers earn nothing (a strict upper bound on losses), half the minimum wage, the full minimum wage, or as much as the workers who remain observed (a lower bound, since informal-sector earnings are lower on average, Bargain and Kwenda 2014).

layoff earnings, comparable to the cost of job displacement estimated for the United States by [Davis and von Wachter \(2011\)](#). Standard errors, shown in parentheses, come from a pairs cluster bootstrap that resamples establishments and re-estimates equation (1) in each replication. Columns (2)–(5) vary the discounting and extrapolation parameters one at a time. The persistent wage reductions documented above, rather than the temporary loss of employment, account for most of this total.

5 The Role of Firm-Specific Wage Premia

A leading explanation for why outsourcing lowers pay is that it severs workers from firm-specific wage premia: firms outsource peripheral functions to avoid sharing rents with the workers who perform them ([Dube and Kaplan, 2010](#); [Weil, 2014](#); [Goldschmidt and Schmieder, 2017](#)). We assess this channel directly by decomposing wages into worker and firm components.

5.1 Measuring firm wage premia

To isolate the firm-specific component of pay, we follow [Abowd et al. \(1999\)](#) (henceforth AKM) and decompose log wages as

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

where $\psi_{J(i,t)}$ is a fixed effect for the worker’s firm, α_i is a worker fixed effect capturing general productivity, θ_t is a year effect, and X_{it} are time-varying characteristics such as age and education. The estimated firm effect $\hat{\psi}_j$ measures the time-invariant wage premium a firm pays to all its workers, and is our measure of the rents a worker stands to lose when displaced. We estimate the decomposition on all formal workers in RAIS over 1985–2002, restricting to the largest connected set of firms linked by worker mobility. The key identifying assumption is that workers’ moves between firms are uncorrelated with unobserved productivity.⁸

⁸This exogenous-mobility condition follows [Gibbons and Katz \(1992\)](#); [Alvarez et al. \(2018\)](#) provide evidence that it is justified in Brazilian RAIS data. A further concern, limited mobility bias ([Andrews et al., 2008](#)), is likely small in our setting given the long panel we use ([Lachowska et al., 2020a](#); [Bonhomme et al., 2020](#)).

5.2 Firm premia and incumbents' wage losses

Figure 3 shows that the incumbents' wage losses are indeed tied to the loss of firm premia, and that they are concentrated among workers who began at high-paying firms. Panel A plots the average AKM firm effect of displaced guards before and after the event, separately by initial quartile of the firm-premium distribution. Workers initially in the top quartile move to firms paying substantially less: their average firm effect falls by slightly more than 20% on impact (from about 0.6 to 0.38) and drifts further down thereafter. Workers in the third quartile show a smaller but still visible decline (from about 0.33 to 0.21), while the average premia of workers who began in the bottom half of the distribution change little. Panel B shows the corresponding wage effects: the persistent wage decline is large for workers from top-quartile firms and statistically indistinguishable from zero for workers from bottom-quartile firms.

Quantitatively, the loss of firm-specific wage premia accounts for 41% of the total wage loss in the year of the occupational layoff, 43% one year later, and 47% five years later (Appendix Table A.3, columns (1)–(3)). The loss of firm premia is thus a substantial mediator of incumbents' wage declines.⁹

5.3 Which firms outsource

That high-wage firms are central to the story also shows up on the *decision* margin. Appendix Table A.4 shows that establishments with higher mean wages in 1993 (column 2) and higher AKM firm effects (column 4) were more likely to undertake an occupational layoff after the reform; the coefficient on 1993 mean guard wages is also positive but not statistically significant (column 3), and the coefficient on establishment size is small and not statistically significant (column 1). This selection of high-paying firms into outsourcing is consistent with the pattern documented by Goldschmidt and Schmieder (2017). The displacement effects, accordingly, are concentrated among workers who began at these firms: Appendix Figure A.10 shows that incumbents initially in the top quartile of the firm-premium distribution are more likely to be pushed out of formal employment on impact than those in the bottom quartile, though both groups return to formal

⁹For comparison, Lachowska et al. (2020b) attribute 17% of the wage losses from job displacement in the United States to firm effects, while Schmieder et al. (2020) attribute 75% in Germany. The sources of these cross-country differences remain an open question.

employment at similar rates.

Finally, we caution that these firm-level losses do not imply a market-wide repricing of guards. Appendix Figure A.11 plots the average firm wage premium among security guards over time: it declined gradually both before and after the reform, in both restrictive and permissive regions, with no trend break around 1993. In other words, while outsourcing relocated a selected set of incumbents from high- to low-premium firms, it did not lower the average firm premium in the occupation as a whole, reinforcing that the losses we document are a selected, firm-level phenomenon rather than a market-wide repricing of guards.

6 Conclusion

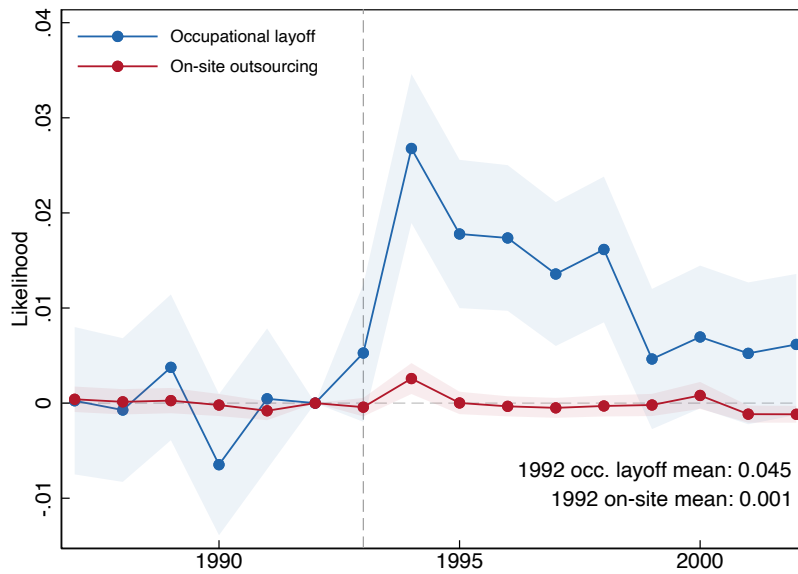
We have studied the incidence and worker-level consequences of firm outsourcing decisions, using Brazil's 1993–1994 reform of security-service outsourcing. Two features distinguish our analysis from much of the existing literature. First, on the measurement side, the on-site outsourcing events on which that literature relies turn out to be rare in our setting; the dominant margin of adjustment is instead the occupational layoff, in which an establishment sheds an entire occupation while keeping its other workers in place. Second, because our data let us observe the moment of displacement and follow workers afterward, we can measure the incidence and impact of outsourcing on the workers it displaces, rather than infer it from wage gaps alone.

The displaced bore real and persistent costs. Incumbent guards caught in an occupational layoff were pushed out of formal employment, but their employment largely recovered within about five years. Their wages did not: they remained roughly 12% lower five years out, and most reallocated to other occupations rather than returning as guards. The implied lifetime losses amount to 1.2 to 1.5 years of pre-layoff earnings. They fell on workers from high-paying firms and stemmed largely from the loss of firm-specific wage premia, which account for more than two-fifths of the wage decline.

These losses are, however, only one side of the ledger. They were concentrated on a selected set of incumbents at high-wage outsourcing firms, and the average firm wage premium in the occupation as a whole did not change around the reform. Studying the same reform at the market level, [Felix and Wong \(2026\)](#) find that it raised employment in the guard occupation overall,

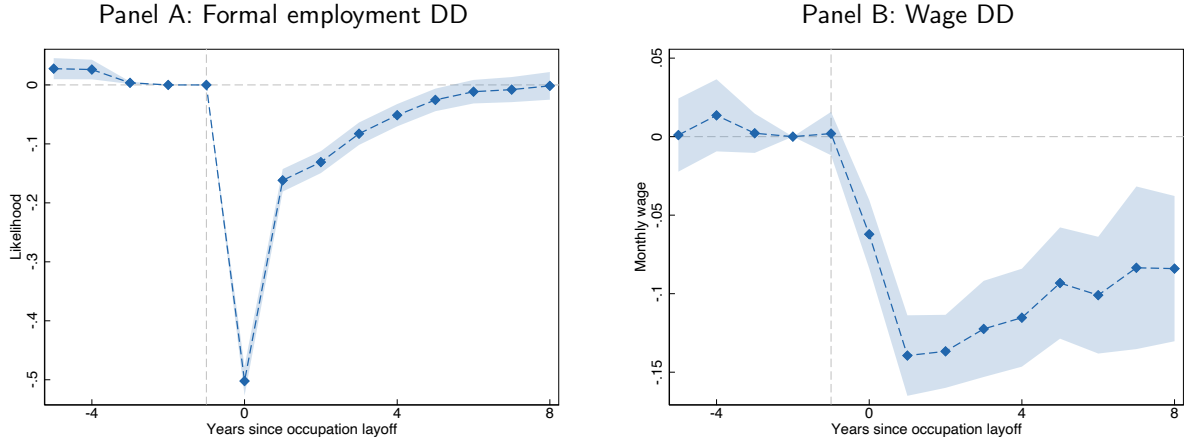
drawing workers in from informality and leaving composition-adjusted wages roughly unchanged. The two findings are complementary: the displacement documented here is the distributional counterpart to those aggregate gains, not a verdict on the net effect of outsourcing. The contrast also carries a methodological caution: firm-level event studies, which by construction follow the workers who are let go, can diverge from a reform's market-level effects. A full welfare assessment of domestic outsourcing must weigh the concentrated losses of displaced incumbents against the broader employment and reallocation effects that operate at the level of the market.

Figure 1: Occupational layoffs vs. on-site outsourcing events following legalization



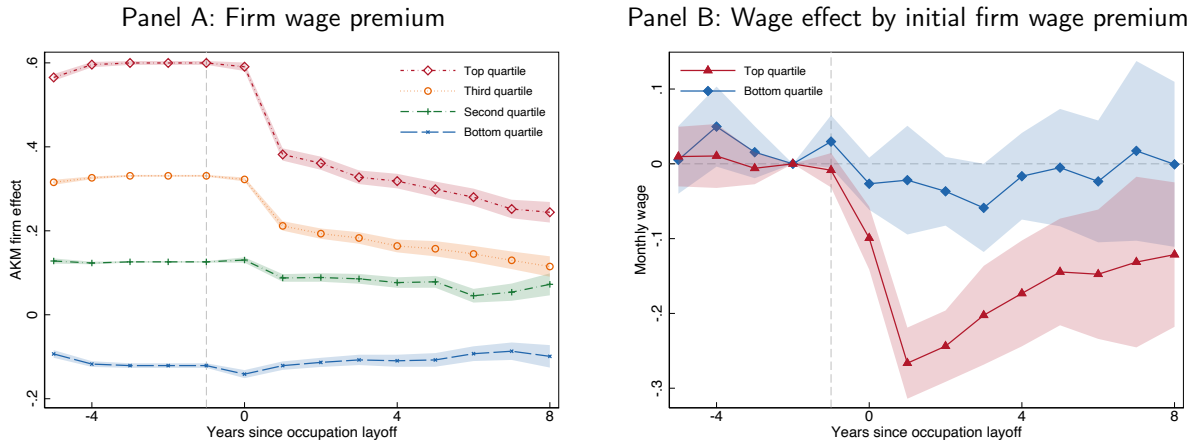
Notes: This figure plots coefficients from a linear probability model where we regress a dummy indicating the occurrence of an occupational layoff or an on-site outsourcing event on year fixed effects, relative to the omitted year of 1992, with controls for microregion and IBGE-subsector fixed effects. Our sample includes all establishment-years where the establishment had at least 10 employees and 3 security guards and its non-guard employment fell by less than 10% in the subsequent year. We exclude manufacturing establishments because they were heavily affected by Brazil's trade liberalization (Kovak, 2013; Dix-Carneiro and Kovak, 2017). Standard errors are clustered at the establishment level.

Figure 2: Effect of occupational layoffs on incumbent workers



Note: Panels A and B plot coefficients δ_k from the difference-in-differences specification in equation (1), measuring the impact of an occupational layoff on incumbent direct-hire security guards, where the control group consists of similar workers in establishments without an occupational layoff. The outcome in Panel A is formal employment; the outcome in Panel B is monthly wage as a fraction of its level two years before the event, conditional on formal employment. 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 base-year establishment level; in Panel B, standard errors are computed by a pairs cluster bootstrap (200 replications).

Figure 3: Incumbent wage reduction is related to loss of firm wage premia



Note: Panel A plots the average AKM firm effect of incumbent direct-hire security guards in the years before and after firm outsourcing decisions, conditional on their remaining formally employed. Panel B shows DD regression estimates for the effect of outsourcing decisions on incumbent monthly wage (as a fraction of wage two years before the outsourcing event) conditional on formal employment, separately for high- and low-wage outsourcing establishments. In the red series of Panel B, we include only workers initially employed at a firm in the top quartile of the AKM firm effect distribution among impacted workers; in the blue, we include only those employed in the bottom quartile. In Panel B, shaded bands indicate 95% confidence intervals, with standard errors clustered at the base-year establishment level.

Table 1: PDV of earnings losses, as multiple of baseline earnings

	Baseline	Robustness			
Interest rate r	5%	5%	3%	5%	10%
Horizon (years)	20	20	20	30	20
Post-year-8 decay	$\hat{\lambda}$	none	$\hat{\lambda}$	$\hat{\lambda}$	$\hat{\lambda}$
	(1)	(2)	(3)	(4)	(5)
Assuming zero earnings for unobserved workers	1.48 (0.15)	1.50 (0.15)	1.57 (0.18)	1.56 (0.19)	1.32 (0.10)
Assuming unobserved workers earn half min wage	1.40 (0.14)	1.42 (0.15)	1.49 (0.17)	1.47 (0.18)	1.23 (0.10)
Assuming unobserved workers earn min wage	1.31 (0.14)	1.33 (0.14)	1.41 (0.17)	1.39 (0.18)	1.15 (0.10)
Using monthly wage estimates from observed workers	1.21 (0.22)	1.26 (0.22)	1.39 (0.27)	1.40 (0.30)	0.90 (0.14)
Decay rate $\hat{\lambda}$	-0.019 (0.011)	0 –	-0.019 (0.011)	-0.019 (0.011)	-0.019 (0.011)

Notes: Entries give the present discounted value (PDV) of the earnings losses caused by an occupational layoff, expressed as a multiple of the worker's pre-layoff annual earnings, computed from equation (2) using the annual losses $\hat{\delta}_k$ estimated in equation (1). Column (1) is the baseline specification: a real interest rate of $r = 5\%$, a twenty-year horizon, and extrapolation beyond the eighth post-event year (the last we observe) at the estimated constant annualized decay rate $\hat{\lambda}$. Columns (2)–(5) vary one of those parameters at a time: column (2) holds the year-eight loss fixed instead of applying the estimated decay ($\lambda = 0$); columns (3) and (5) set r to 3% and 10%; and column (4) extends the horizon to thirty years. Because earnings outside the formal sector are not observed in RAIS, each row reports the PDV under a different assumption about the earnings of workers we no longer observe: zero earnings, half the minimum wage, the full minimum wage, or the monthly wage estimated from workers who remain observed. The bottom panel reports $\hat{\lambda}$, defined as the average annualized decay in the loss between post-event years five and eight. Standard errors in parentheses are from a pairs cluster bootstrap with 200 replications, resampling establishments as of 1999; each replication re-estimates equation (1) for all four earnings definitions and recomputes $\hat{\lambda}$ and the PDVs, so the standard errors propagate estimation error in $\hat{\delta}_k$ and $\hat{\lambda}$ jointly.

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