# Domestic Outsourcing and Employment Security

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**Abstract**

Domestic outsourcing is known to reduce worker wages, but its effect on employment security—a key dimension of job quality—has not been studied. Using Brazil's comprehensive employee-employer data, we robustly find that outsourcing reduces exit from formal employment among cleaners and security guards during their first few years of tenure. The observed reduction in employment hazard is larger in cities with greater volatility in labor demand. Our findings are not attributable to differences in worker characteristics or changes in local market conditions. To explain the findings, we develop a search-theoretic model in which outsourcing both alters wage setting and eases reassignment across firms. The estimated model suggest that outsourcing had more positive welfare effects on workers upon job entry than implied by wage differentials alone.

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# 1 Introduction

To focus on their "core competencies," firms have increasingly relied on outsourced workers to provide professional services once performed by direct employees, such as cleaning, security, IT, and HR. In this paper, we explore how the rise of domestic outsourcing affects worker employment security and formal sector attachment. On the one hand, outsourcing may exclude workers from firm-level benefits including long-term employment security. On the other hand, professional service firms that operate large internal labor markets may prevent outsourced workers from entering unemployment through flexible reassignment of workers across client firms. As such, domestic outsourcing may redress the high levels of turnover and informality in developing labor markets (Ulyssea 2020; Donovan, Lu and Schoellman 2023). Yet, few scholars have directly measured the effects of domestic outsourcing on employment security.

Our study contributes to a growing literature on the impact of outsourcing on worker job quality (Bernhardt et al. 2016). Recent evidence shows that outsourcing is associated with reduced wages for low-wage workers, especially for workers initially at high-wage firms (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2023; Estefan et al. 2024). Domestic outsourcing is also associated with increased employment, suggesting that outsourcing may benefit firms and improve aggregate efficiency (Bertrand, Hsieh and Tsivanidis 2021; Felix and Wong 2024). However, evidence on the effects of domestic outsourcing on job transitions is scarce. Better evidence is needed to understand how domestic outsourcing affects worker welfare and labor market structure and has implications for the proper design of labor regulations.[1]

We present the first estimates of the effects of non-core activity outsourcing on worker employment security. We define *employment security* as the inverse probability of involuntary exit from formal employment. Our main finding is that outsourcing is associated with *higher* employment security, especially during the first few years of employment spells. Moreover, outsourcing reduces employment hazard more in cities with greater volatility in labor demand. These effects are robust to controlling for differences in worker characteristics or changes in

[1]In the past few years, Mexico and Peru instituted restrictions on domestic outsourcing (Jiménez and Rendon 2022; Estefan et al. 2024). Meanwhile, Brazil relaxed restrictions on outsourcing in 2017 in hopes of increasing the efficiency of labor markets.

local market conditions. Our findings suggest that outsourcing eased reassignment across firms. As a result, even though outsourced workers earn lower wages, they benefit from higher job security, resulting in less negative welfare effects than implied by wage differentials alone.

To show this, we leverage Brazil's comprehensive employment record. We focus on cleaners and security guards, two major occupations for which outsourcing status is easily identified from industry codes. We estimate a linear probability model of the effects of outsourcing on hazard from formal employment, controlling for observable worker characteristics and occupation-region-year fixed effects. For robustness, we add controls for worker unobserved ability as measured by the two-way wage decomposition proposed by Abowd, Kramarz and Margolis (1999). We also account for unobservable worker selection by investigating regression coefficient stability in response to adding controls of observable characteristics, following the method proposed by Oster (2019).

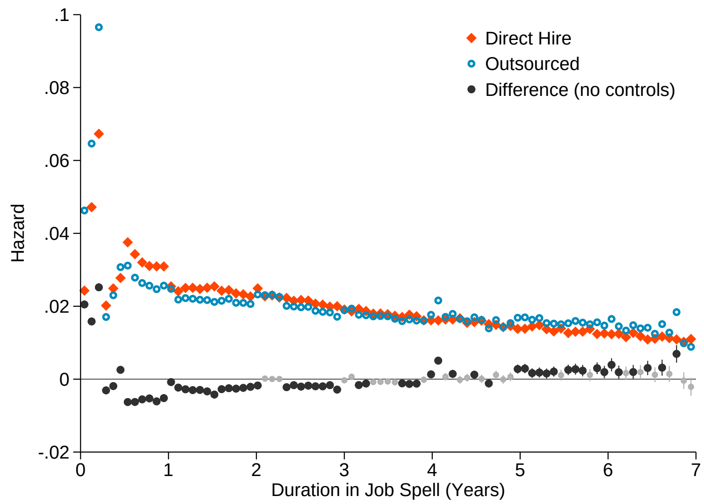
We robustly find that outsourcing is associated with reduced exit from formal employment. As shown in Figure 1, outsourced security guards have lower rates of involuntary transition from formal employment than direct-hire guards for the first six years of their employment spells. During the first year of employment spells, the predicted hazard of direct-hire security guards is roughly 50 percent higher. It is only in the seventh year of employment that their predicted hazard rates become the same. For cleaners, the hazard rates of direct-hire workers are higher than those of outsourced workers during the first three years of employment, but fall below those of outsourced workers thereafter. These differences are not explained by local labor market conditions or worker selection.

What explains the differences in employment hazard? Prior literature suggests that outsourcing helps firms overcome labor demand fluctuations by enabling easier reassignment of workers across firms (Abraham and Taylor 1996; Houseman 2001; Battiston, Espinosa and Liu 2021). Consistent with this hypothesis, we find that the observed reductions in employment hazard are larger in cities with more volatile labor demand. Specifically, we measure labor demand volatility using monthly changes in unemployment rates in six large metros. We estimate that outsourcing reduces employment hazard more in cities with greater volatility.

We also rule out an alternative hypothesis that outsourcing reduces employment hazard by

Figure 1: Hazard from Formal Employment, Direct-hire and Outsourced Workers

(a) Cleaners




(b) Security guards


Notes: Each panel plots the raw probability of involuntary exit from formal employment during each 30-day interval, separately for outsourced and direct-hire workers, conditional on being employed at the beginning of that interval. The sample includes the first full-time spells at each employer between 2003-2010. The black and gray dots plot the estimated effect of being outsourced on involuntary exit from formal employment using the linear probability model in Equation (3), without any controls. Vertical bars show 95% confidence intervals.

enabling firms to reduce wages rather than lay off workers in response to labor demand shocks. Specifically, we find that the relationship between wages and unemployment is less negative for outsourced workers, even after controlling for worker fixed effects. This finding suggests that the wages of outsourced workers do not fall more in response to negative labor demand shocks.

We supplement our hazard estimates with estimates of outsourcing wage differentials. We use a regression specification that includes controls for worker characteristics and fixed effects, following Dube and Kaplan (2010). Our wage estimates are broadly consistent with recent literature, which suggests that outsourcing reduces worker wages for low-wage workers, but less so for high-wage workers (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Spitze 2022; Drenik et al. 2023). In the low-wage occupation of cleaners, outsourcing is associated with wages that are 11 percent lower. For the more professionalized and higher-wage occupation of security guards, outsourcing is associated with wages that are only 1.3 percent lower. Following Card, Heining and Kline (2013), we use event study designs to confirm that these outsourcing wage differentials reflect the causal effect of outsourcing rather than sorting effects.

To fit our wage and hazard findings, we develop a model in which outsourcing both smooths demand fluctuations and alters wage bargaining. Our search-theoretic model features match-specific productivity, wage bargaining, and endogenous separations. In the model, outsourcing alters employment relationships by allowing the worker to be reassigned to another firm when a negative productivity shock hits. Outsourcing may also alter the bargaining power of workers. The model predicts that when reassignments are sufficiently frequent, the hazard rate of outsourced workers is lower than that of directly employed workers during the early period of employment spells. However, as time progresses, the hazard rate of outsourced workers becomes higher than direct-hire workers, so the hazard rate of direct-hire workers "crosses" from above to below the hazard rate of outsourced workers over the employment spell, as seen in the data.

We structurally estimate the model from the hazard rates and wage distributions of directly employed and outsourced workers using the Generalized Method of Moments (GMM). The estimated model fits the data very well. The model infers that the reassignment rate is positive in both occupations. The model also infers that the bargaining power of outsourced workers is lower than that of directly employed workers, and that outsourcing reduced bargaining power more for cleaners than for security guards. We conclude that the negative effect of outsourcing on worker welfare due to wage reductions is substantially offset by improvements in employment security. We robustly find that outsourced workers had higher expected utility at the start of employment spells than directly employed workers. We also find that outsourcing had more positive welfare effects in the higher-wage occupation of security guards, suggesting the effects of outsourcing are heterogeneous across occupations.

## 1.1 Related Literature and Contributions

The contribution of this paper is threefold. First and most importantly, this paper is the first to estimate the effects of domestic outsourcing on job hazards using administrative employment records (see surveys by Davis-Blake and Broschak 2009; Bernhardt et al. 2016). Batt, Doellgast and Kwon (2005) and Batt, Holman and Holtgrewe (2009) use a small sample of survey data, rather than a comprehensive employment registry, and show that outsourcing is associated with lower *perceived* job security among call center workers in the US. Our findings suggest that although outsourcing reduced wages in Brazil, it improved *actual* employment security and thereby brought benefits to workers.

Our explanation for why non-core activity outsourcing improves employment security is that outsourcing reduces labor market frictions by easing worker reassignment across firms. In closely related work, Felix and Wong (2024) study an earlier period in Brazil and focus on security guards. They estimate that outsourcing legalization led to market-level employment and job reallocation effects consistent with a reduction in labor market frictions. We complement their work by estimating the effects of outsourcing on employment security in two large occupations. Our results add to existing firm-level descriptive evidence on outsourcing determinants and job rotation (e.g. Abraham and Taylor 1996; Houseman 2001; Battiston, Espinosa and Liu 2021). We are also the first to document that the effects of outsourcing on employment hazards are larger in more volatile labor markets.

Second, we contribute novel estimates of outsourcing wage differential from a developing country context. Most existing estimates come from developed economies, such as the US (Abraham 1990; Dube and Kaplan 2010; Spitze 2022), the UK (Berlinski 2008), and Germany (Goldschmidt and Schmieder 2017). Drenik et al. (2023) study temp-agency workers in Argentina but lack information on occupation. Our evidence focuses instead on outsourced workers within specific occupations. We confirm the importance of firm-level wage premia in explaining outsourcing wage differentials and show that the wage effects of outsourcing are more negative in a low-wage occupation.

Third, we contribute a novel search-theoretic model of domestic outsourcing to explain the observed hazard profiles. Spitze (2022) uses a search-and-matching model with constant match productivity and exogenous separations and argues that outsourcing disproportionately hurts low-wage workers in the U.S.. Bilal and Lhuillier (2021) use a model with wage posting and on-the-job search to analyze the effects of domestic outsourcing and argue that outsourcing increased both aggregate output and inequality in France. Neither of these models features the possibility that outsourcing lengthens employment spells through flexible worker reassignment across firms. For this reason, they cannot explain our finding that outsourcing is associated with *higher* employment security and may understate the benefits of domestic outsourcing.

A related but separate strand of the literature focuses on *core* instead of *non-core* activity outsourcing. For instance, Estefan et al. (2024) study a ban on core activity outsourcing in Mexico that increased wages but reduced firm investment and increased firm exit. Bertrand, Hsieh and Tsivanidis (2021) study legalization of contract firm use in India that enabled manufacturers to increase in scale. Jiménez and Rendon (2022) show that Peru's limit on core activity outsourcing had little impact employment, wages, or formality. All of these studies focus on core activity outsourcing as a means to avoid labor regulation. In contrast, the differences in employment protection for outsourced and direct employees in our context are minimal. This allows us to focus on the potential benefits of flexible reassignment by contract firms.

Another related but distinct literature studies the effects of fixed-term contracts. Many studies find that the rise of fixed-term contracts is associated with *reduced* employment security among young workers in Europe (Blanchard and Landier 2002; Bentolila and Saint-Paul 1992; Cahuc and Postel-Vinay 2002; García-Pérez, Marinescu and Vall Castello 2018; Daruich, Addario and Saggio 2020). Importantly, European reforms that legalized the use of fixed-term contracts did not permit the use of professional service intermediary firms. Our evidence therefore suggests that disallowing intermediation may be a reason that the benefits of flexible worker reassignment across firm could not be realized in Europe.

The rest of the paper is organized as follows. Section 2 provides background. Section

3 estimates outsourcing wage differentials. Section 4 estimates the effects of outsourcing on employment security. Section 5 presents an interpretive framework. Section 6 concludes.

# 2 Background

## 2.1 Data and Sample Construction

We use Brazil's employee-employer matched administrative data, *Relação Anual de Informações Sociais* (RAIS), which cover the near universe of Brazil's formal-sector workers. The RAIS data include annual information on the start and end dates of employment spell, the average monthly wage over that period, and several demographic variables (such as education, gender, race, and age), which are collected through a mandatory survey administered by the Brazilian Ministry of Labor and Employment. These data are of high quality, since firms are fined for failure to report and workers cannot receive government benefits unless accurate information is reported.

We focus on data from 2003 to 2010, a period that is uncontaminated by the effects of Brazil's 1993 outsourcing legalization and has both consistent occupation codes and exact start and end dates for employment spells. To identify direct-hire and outsourced workers, we use detailed industry and occupation codes.

Despite their richness, these data have two limitations. First, we do not observe worker-firm-intermediary linkages, so we cannot characterize the match between outsourced workers and client firms.[2] Second, there is a substantial informal sector in Brazil. Only 68% and 79% of cleaners and security guards, respectively, are in the formal sector and hence covered in our sample (Appendix Table A.4). Missing observations in our data could represent either unemployment or informal employment. We address this shortcoming by leveraging information on separation reasons.

For our hazard estimation, we construct employment histories for individual workers as follows. We restrict attention to workers aged 18-65 in full-time jobs (at least 35 hours per

[2]This is a problem that plagues most administrative employment records with only one known exception (Drenik et al. 2023).

week) and exclude workers with temporary contracts.[3] We say that an employment spell ends in an *exit from formal employment* if there is more than one week between the spell's end and the start of the worker's next full-time employment spell. We count exits as *involuntary* if the spell did not end in retirement, death, or quitting, so severance compensation must be made. Appendix Figure C.3 shows that our main results remain highly similar when quits are included instead. Appendix A provides detailed data definitions.

## 2.2 Our Focus: Cleaners and Security Guards

We focus on cleaners and security guards for two reasons. First, both are large occupations where a substantial number of workers are employed by contract firms and within which the task requirements are relatively homogeneous. Second, there is a clear mapping from industry codes to contract firm status that does not exist in other occupations, so we confidently identify outsourced workers using detailed industry and occupation codes in our data.[4] These classifications are presented in Appendix A.

The comparison between these two occupations provides insights into how the effects of outsourcing may be heterogeneous across occupations. Security guards are highly professionalized, regulated, and well-paid. Because of high crime rates and inadequate public provision of policing, security guards in Brazil undergo mandatory training administered by the Brazilian government and face regulatory requirements for gun carry licenses. By contrast, cleaners are an unlicensed occupation in Brazil. They are also the lowest-paid occupation in the formal sector. The mean monthly wage of cleaners in 2010 is roughly equal to one-half of the mean monthly wage of security guards.

Table 1 shows the characteristics of the employment spells of outsourced and direct-hire workers, including age, education, gender, and race at spell start. Anticipating our main result below, the employment spells for outsourced cleaners and security guards are *less* likely to

[3]These contracts are uncommon and subject to approval by the Ministry of Labor (MTE) to meet temporary increases in demand. Many of these contracts last for three months.

[4]It is not easy to sharply identify the effects of domestic outsourcing in other occupations using industry codes. For example, outsourced drivers work in the "road transport" industry, but this category also includes drivers who work for public transportation companies.

Table 1: Summary Statistics of Employment Spells, Brazil, 2003-2010

| | Cleaners | | Security guards | |
|---|---|---|---|---|
| | Direct-hire | Outsourced | Direct-hire | Outsourced |
| Worker characteristics at spell start: | | | | |
| Age | 32.4 | 33.1 | 36.1 | 32.6 |
| | [10.1] | [9.81] | [10.8] | [7.93] |
| Years of schooling | 7.82 | 7.37 | 8.53 | 9.88 |
| | [3.25] | [3.07] | [3.53] | [2.79] |
| Male | 0.50 | 0.43 | 0.96 | 0.94 |
| | [0.50] | [0.50] | [0.21] | [0.24] |
| Non-white | 0.45 | 0.51 | 0.52 | 0.50 |
| | [0.50] | [0.50] | [0.50] | [0.50] |
| Contract hours | 43.6 | 43.7 | 43.3 | 43.7 |
| | [1.49] | [1.29] | [2.12] | [1.52] |
| Share of spells that end within: | | | | |
| one year | 0.50 | 0.59 | 0.47 | 0.38 |
| 1-2 years | 0.13 | 0.13 | 0.14 | 0.14 |
| 2-3 years | 0.06 | 0.06 | 0.06 | 0.07 |
| 3-4 years | 0.03 | 0.03 | 0.03 | 0.04 |
| 4-5 years | 0.015 | 0.013 | 0.015 | 0.019 |
| 5-6 years | 0.007 | 0.007 | 0.008 | 0.010 |
| 6-7 years | 0.003 | 0.002 | 0.003 | 0.004 |
| Share with unobserved spell end | 0.26 | 0.18 | 0.28 | 0.33 |
| Reason for spell end: | | | | |
| Involuntary exit from formal sector | 0.51 | 0.45 | 0.49 | 0.34 |
| Voluntary exit from formal sector | 0.13 | 0.15 | 0.08 | 0.08 |
| Transition to other formal job | 0.06 | 0.16 | 0.09 | 0.17 |
| Other | 0.05 | 0.06 | 0.06 | 0.07 |
| Observations | 2681874 | 1897053 | 797534 | 1201625 |

Notes: The sample is all employment spells of security guards and cleaners, respectively, between 2003 and 2010. Standard deviations are displayed in brackets.

end in involuntary exit from the formal sector than direct-hire cleaners and security guards, respectively.

# 3 Effect of Outsourcing on Wages

As a first look at the effects of outsourcing in these two occupations, we measure the effect of outsourcing on worker wages. Following Dube and Kaplan (2010), we estimate the following equation using yearly panels of security guards and cleaners, respectively:

$$\ln w_{it} = \gamma O_{it} + \theta_{omt} + X_{it}'\beta + \alpha_i + \epsilon_{imt}, \tag{1}$$

where $t$ indexes year, $i$ indexes the worker, $w_{it}$ is the average real monthly wage, $O_{it}$ indicates whether the worker is outsourced, $\theta_{omt}$ is a suboccupation-year-microregion fixed effect, $X_{it}'\beta$ are the effects of time-varying observable worker characteristics (such as education and age), $\alpha_i$ controls for individual fixed effects, and $\epsilon_{imt}$ is a composite error that may include idiosyncratic worker-firm match effects.[5] We then check whether estimated outsourcing wage differentials have a causal interpretation by plotting event studies for workers who change contractual arrangements, following the method of Card, Heining and Kline (2013).

Outsourcing wage differentials may be attributable to either compensating differentials or differences in labor market rents. To understand the source of wage differentials, we investigate the extent to which outsourcing wage differentials are explained by differences in firm-level wage premia. We measure firm-level wage premia using the two-way decomposition proposed by Abowd, Kramarz and Margolis (1999) — henceforth AKM — as described in Appendix A.

We first confirm that the AKM decomposition provides a useful measure of firm-level wage premia for cleaners and security guards. After correcting for measurement error, we find that the AKM firm effects estimated using only cleaners and security guards, respectively, are very highly correlated with the AKM effect estimated for all other workers. If a firm pays 10% higher wages to other workers, it pays 6.1% higher wages to cleaners and 9.7% higher wages to security guards (Appendix Figure B.1). We then investigate whether firm-level wage premia explains

[5]An alternative approach is to follow Goldschmidt and Schmieder (2017), who estimate wage differentials using "on-site outsourcing events." We do not follow this approach for two reasons. First, Brazilian labor law prohibits nominal wage reductions through the firing and rehiring of workers at an intermediary to perform the same job. As a consequence, estimates of wage differentials using such events are likely to be biased upward. Second, as documented by Felix and Wong (2024), on-site outsourcing is exceedingly rare in Brazil. Given the rarity, wage differentials estimated using this method necessarily use a highly selected population of workers.

Table 2: Outsourcing Wage Differential, Brazil, 2003-2010

| Dep. var.: Log real wage | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Panel A: Cleaners* | | | | | |
| Outsourced | -0.194 | -0.178 | -0.110 | -0.049 | -0.072 |
| | (0.006) | (0.006) | (0.003) | (0.002) | (0.003) |
| AKM firm effect | | | | 0.413 | 0.332 |
| | | | | (0.007) | (0.007) |
| Observations | 7834355 | 7834355 | 6150003 | 6131898 | 5904192 |
| $R^2$ | 0.30 | 0.35 | 0.94 | 0.94 | 0.04 |
| *Panel B: Security guards* | | | | | |
| Outsourced | -0.173 | -0.137 | -0.013 | -0.014 | -0.003 |
| | (0.026) | (0.020) | (0.005) | (0.003) | (0.005) |
| AKM firm effect | | | | 0.690 | 0.397 |
| | | | | (0.011) | (0.012) |
| Observations | 4768878 | 4768878 | 4253501 | 4251537 | 4176299 |
| $R^2$ | 0.44 | 0.48 | 0.93 | 0.93 | 0.03 |
| Occ X Year X Microregion FE | X | X | X | X | X |
| Demographic controls | | X | X | X | X |
| Worker FE | | | X | X | X |

Notes: Sample includes all cleaners and security guards, respective, observed at year-end in RAIS between 2003-2010. Demographic controls include a full set of race X gender X education dummies interacted with age, age squared, and age cubed. AKM firm effect is estimated from a wage regression using the full worker sample in column (4), and two non-overlapping random sets of workers in column (5). Standard errors are clustered at both worker and firm level, and displayed in parentheses.

the observed outsourcing wage differential.

**Cleaners.** Table 2 Panel A displays the estimated outsourcing wage differentials for cleaners, which is -11.0 log points in our preferred specification of Column (3). Column (1) shows that, with occupation-microregion-year fixed effects, the wages of outsourced cleaners are roughly 19.4 log points lower than direct-hire cleaners. With additional demographic controls in Column (2), the estimate changes very slightly to 17.8 log points. With added individual fixed effects, as in Column (3), the wages of outsourced cleaners are smaller at 11.0 log points, suggesting that there is some unobserved selection into outsourcing.

Column (4) shows that the outsourcing wage differential is much smaller after controlling the AKM firm effect, at 4.9 log points. Column (5) uses a split sample IV approach, to remove the influence of measurement error, with AKM firm effects estimated from two equally sized samples of workers that include neither cleaners nor security guards, to remove mechanical correlation arising from using the same data on both sides of the equation (following Goldschmidt and Schmieder 2017). The correlation between cleaner wages and AKM firm effects is only somewhat attenuated in this specification. These estimates suggest that differences in firm-level wage premia substantially explain the outsourcing wage differential.

Figure 2 Panels (a) and (b) show that that cleaners who switch from direct-hire to outsourced jobs experience relative wage declines, while cleaners who switch from outsourced to direct-hire jobs experience relative wage increases. There are no significant pre-event trends during the two years prior, which implies that the estimated outsourcing wage differential is likely to capture the causal effect of outsourcing.

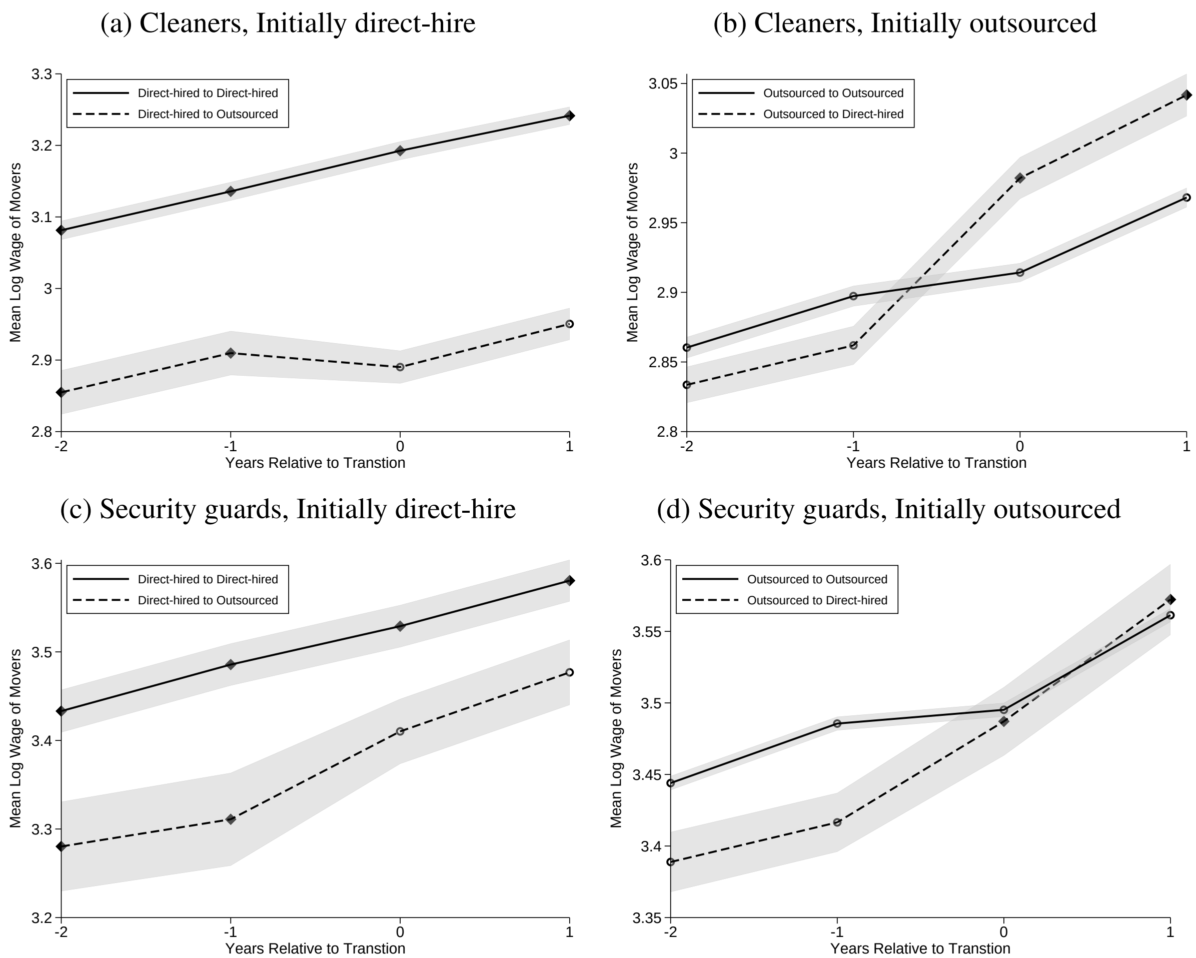
**Security guards.** For security guards, a higher-wage occupation, the outsourcing wage differential is very small. Our preferred estimate, which controls for unobservable worker heterogeneity, is -1.3 log points.

Table 2 Panel B Column (1) shows that the wages of outsourced security guards are roughly 17.3 log points lower than direct-hire security guards with occupation-microregion-year fixed effects. With additional demographic controls in Column (2), the estimate is similar, at 13.7 log points. With the individual fixed effects, as in Column (3), the estimate is only 1.3 log points. Columns (4) and (5) show that the estimated outsourcing wage differential remains small after accounting for differences in firm-level wage premia.

Figure 2 Panels (c) and (d) show that security guards who move from direct employment to outsourcing and from outsourcing to direct employment both experience a wage increase. The asymmetry in wage responses suggests some degree of endogeneity in worker mobility, but confirms that the outsourcing wage differential is likely to be small.

Appendix Figure B.2 and Table B.1 show that the negative wage effects of outsourcing increases with tenure, for both cleaners and security guards, even after controlling for demographic

Figure 2: Wage Evolution, Job Switchers




Notes: This figure shows the mean log wages of workers who transitioned between establishments in 2003-2010. We restrict the sample to job switchers who are observed not to change establishments during the two years before and during the two years after the transition. Panel (a) and (c) show cleaners and security guards, respectively, who were initially direct employees and switched to outsourcing. Panel (a) and (c) show cleaners and security guards, respectively, who were initially outsourced and switched to direct employment.

variables such as age.

# 4 Effect of Outsourcing on Exit from Formal Employment

Having examined the wage effects of outsourcing, this section estimates the effect of outsourcing on the rate at which workers exit from formal employment. These estimates are important for understanding the welfare effects of domestic outsourcing on workers. If outsourcing facilitates their transitions across firms in response to demand fluctuations, then outsourcing should reduce hazards from formal employment and thereby benefit workers. To our knowledge, this paper is the first to investigate these effects.

## 4.1 Method

We construct the first full-time spell for each worker at each employer. We estimate the hazard function only at duration less than or equal to 7 years because of small sample sizes with longer duration. We censor spells ending in a job-to-job transition, in which case we do not know when the spell would have ended in exit from formal employment.

Following Schmieder and Trenkle (2020), we estimate the hazard rate of exit from formal employment at each month $\tau = 1, \ldots, 84$ using the following regression model:

$$y_{i\tau} = \alpha_\tau + \delta_\tau O_i + X_i'\beta_t + \theta_{omt} + \epsilon_{i\tau} \mid \tau_i \geq \tau, \tag{2}$$

where $\tau_i$ is the month when individual $i$ exits formal employment. In each regression, conditional on worker $i$ has survived in an employment relationship for $\tau - 1$ months, the dependent variable $y_{i\tau}$ is a dummy indicating whether worker $i$ exited formal employment at month $\tau$. $O_i$ indicates whether worker $i$ i outsourced. $X_i$ are worker-level controls. $\theta_{omt}$ are suboccupation-microregion-year fixed effects. Estimating Equation (3) at each $\tau$ provides a vector of $\alpha_\tau$ which represents the average rate of hazard from formal employment of direct-hire workers in month $\tau$, while $\delta_\tau$ represents the shift in the hazard rate of outsourced workers in that month, which measures the effect of outsourcing on exit from formal employment.

There are two main potential sources of bias in our hazard estimates. First, outsourced workers may be more prevalent in certain locations and therefore are differentially exposed to local macroeconomic fluctuations. To account for this confounding influence, we add flexible controls for microregion-suboccupation-year fixed effects.

Second, outsourced workers may be selected. To address this concern, we use a rich set of demographic controls, including gender, age, race, and education at the start of the employment spell, as well as AKM worker effects as a proxy for unobserved worker ability. We also assess the potential impact of unobserved worker selection using the method proposed by Oster (2019). Specifically, we investigate how coefficient stability is affected by the addition of worker-level controls. Following Oster (2019), we assume that $R_{\max} = 1.3\tilde{R}$, where $R_{\max}$ is the theoretical proportion of variance explained by controls for both observed and unobserved variables, while $\tilde{R}$ denotes the proportion of variance explained by only observable variables. We plot bias-adjusted estimates $\beta^*$ assuming that the relative degree of selection on observed and unobserved variables after partialing out microregion-suboccupation-year fixed effects ($\delta$) is one.

## 4.2 Results

Figure 1 displays the raw rate of exit from formal employment of outsourced and direct-hire workers. Figure 3 shows the differences in hazard rate between outsourced and direct-hire workers with controls successively added. We first add suboccupation fixed effects, then microregion-suboccupation-year fixed effects, then observable worker demographic characteristics such as gender, age, race, and education at spell start, and finally unobservable worker ability using AKM worker effects.[6] Figure 4 shows the bias-adjusted estimates using the method pioneered by Oster (2019).

In the appendix, we report estimates with more restrictive samples or a less stringent definition of transitions from formal employment. Appendix Figure C.2 reports results when we restrict to employment spells for which the individual initially entered from outside the formal sector, to workers who are no more than 30 at the start of the spell, and to male workers, respectively. Appendix Figure C.3 reports results where transitions from formal employment due to quits are included in the hazard definition.[7] Appendix Figure C.4 shows the implied survival rates.

**Cleaners.** As shown in Figure 1, the hazard from formal employment of direct-hire cleaners is significantly higher than that of outsourced cleaners in almost all months during the first 4 years of employment, except for some unusual patterns in the first six months. For example,

[6] See Appendix A for details.

[7] Relately, we find that outsourced workers are more likely than direct employees to experience employer-to-employer transitions (see Appendix Figure C.7). This could be because outsourced workers receive lower wages and therefore are more likely to accept an outside offer.

at the one-year mark, the raw 30-day hazard rate for outsourced cleaners is 3.7 percent, while it is 4.2 percent for direct-hire cleaners. The unusual pattern in the first three months is likely attributable to tenure-dependent employment protection legislation, which caused hazards to be significantly higher prior to the three-month mark than thereafter (Arnold and Bernstein 2021).[8]

The hazard rates of outsourced and direct-hire workers become closer over the course of the employment spell. They eventually cross at around four years of tenure, with the hazard of direct-hire workers becoming lower than those of outsourced cleaners thereafter.[9] The difference in survival gradually attenuates thereafter, so outsourcing generally has a small but positive effect on employment survival probability (see Appendix Figure C.4).

Figure 3 shows that the estimates are broadly similar after successive addition of controls. This suggests that selection due to observable worker characteristics and labor market conditions do not drive these differences in hazard rates.

Figure 4 shows that bias-adjusted estimates computed using method of Oster (2019) are highly similar. As shown in Appendix Table C.2, adding observable worker controls hardly changes the estimated coefficients even though it increases the $R^2$, suggesting that unobserved worker selection is likely to be small. The estimates are also very similar when alternative sample and outcome variable definitions are used (Appendix Figure C.2 and C.3).

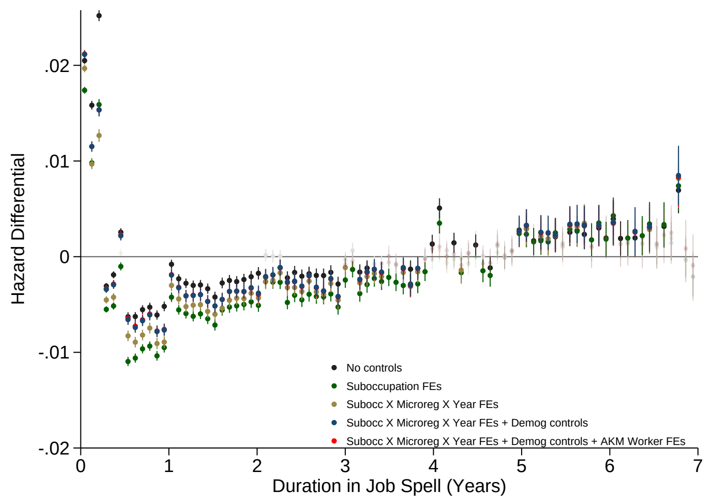
**Security guards.** The effect of outsourcing on hazard from formal employment is larger and more negative for security guards. Figure 1 shows that with the exception of the first few months, outsourced security guards have much lower probabilities of transitioning from formal employment than direct-hire workers. At the one-year mark, the raw 30-day hazard rate for outsourced workers is 3.7 percent, while it is 1.8 percent for direct-hire workers. The hazard rates of outsourced and direct-hire workers become closer over the course of the employment spell, eventually narrowing to a statistically indistinguishable difference in the sixth year of

[8]Specifically, the employer must pay a firing penalty in the event of an involuntary separation, which is equal to roughly one month of the worker's salary for every year the worker has been employed at the firm. The bulk of this penalty is paid to the worker as severance. This requirement is tenure-dependent and only applies after 3 months of employment.

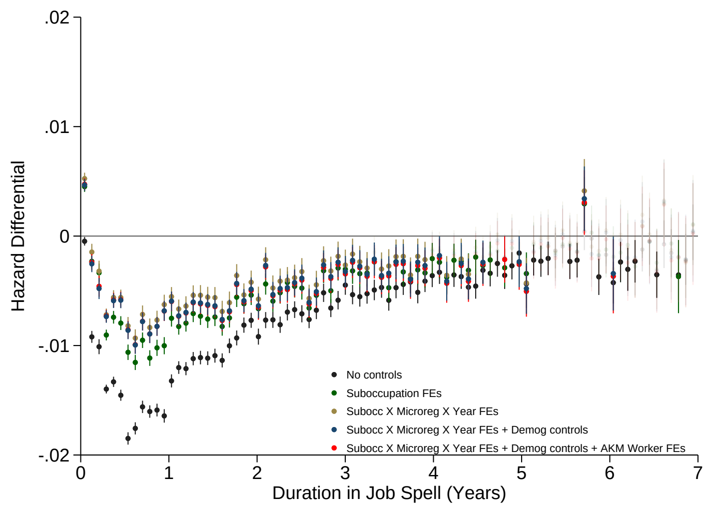
[9]As shown in Appendix Figure C.1, locally smoothed estimates of the outsourcing differential confirm that the hazard rate of outsourced cleaners "crosses" from above to below that of direct-hire security guards at around four years of tenure.

Figure 3: Effect of Outsourcing on Hazard from Formal Employment, Alternative Controls

(a) Cleaners




(b) Security guards



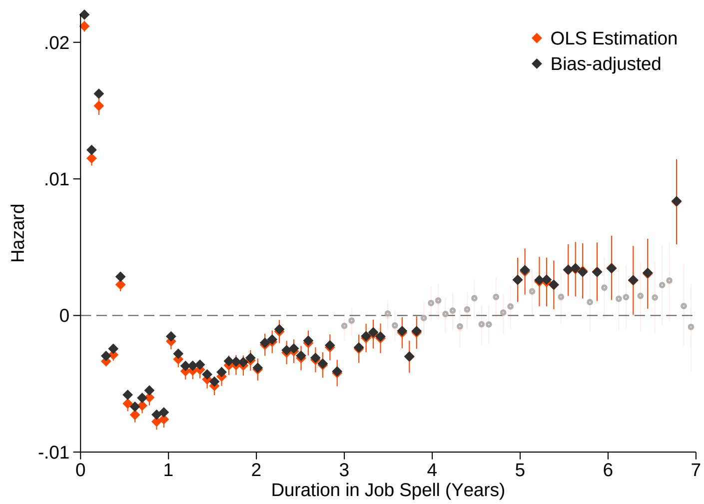

Notes: Sample includes all first full-time spells at each employer between 2003-2010. We truncate the duration at 7 years. Each hazard differential is estimated at the midpoint of the 30-day interval. The black dots display the raw difference in hazards. The green dots show estimates with only suboccupation fixed effects. The mustard dots show estimates with suboccupation X microregion X year fixed effects. The red shows the estimates with the full set of controls in our main regression. The blue dots show the estimates with the full set of controls and AKM worker effects estimated from a wage regression with the full sample. 95% confidence intervals are shown. Statistically insignificant estimates are shown in light gray.

tenure. The hazard estimates imply that outsourcing significantly increased the survival of employment spells for security guards (Appendix Figure C.4).

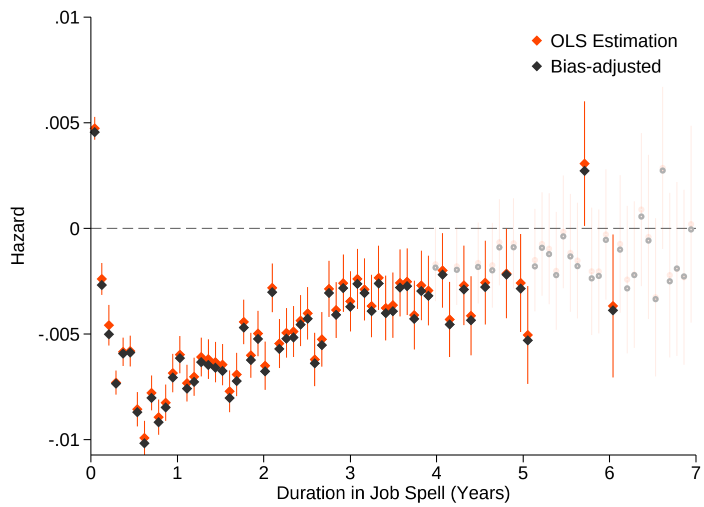
Figure 3 shows that adding suboccupation fixed effects attenuates the estimates somewhat,

Figure 4: Bias-adjusted Effect of Outsourcing on Hazard from Formal Employment

(a) Cleaners




(b) Security guards




Notes: Following Oster (2019), we plot bias-adjusted estimates assuming that $\delta = 1$ and $R_{\max} = 1.3\tilde{R}$, after partially out microregion-suboccupation-year fixed effects. The black dots display the bias-adjusted estimates. The orange dots show estimates with the full set of controls in our main regression and AKM worker effects estimated from a wage regression with full sample. 95% confidence intervals are shown for the OLS estimates. Statistically insignificant estimates are shown in light gray.

suggesting that for security guards, it is important to account for suboccupation differences. However, the effects of outsourcing on hazard from formal employment among security guards remain large even after controlling for observable worker characteristics and labor market conditions.

Figure 4 shows that estimates are highly similar after accounting for unobservable worker selection using the method of Oster (2019). Once again, adding observable worker controls hardly changes the estimated coefficients even as it increases the $R^2$ (see Appendix Table C.2). The estimates are also robust to alternative sample and outcome variable definitions (Appendix Figure C.2 and C.3).

### 4.3 Labor Demand Volatility and Employment Security

What explains the observed effects of outsourcing on the rate of exit from formal employment? Firm-level surveys show that firms outsource partly to overcome fluctuations in labor demand (Abraham and Taylor 1996; Houseman 2001).

In this subsection, we provide further evidence that outsourcing smooths labor market volatility. We measure city-level labor demand volatility, $LDV_m$, as the average of the absolute value of monthly change in city-level unemployment rate between 2003 and 2010. These measures are constructed from the PME, the Brazilian Monthly Employment Survey, which is collected in six large metropolitan areas.[10]

We then estimate the following regression:

$$y_{i\tau} = \alpha_\tau + \delta_\tau O_i + \gamma_\tau HighLDV_m + \chi_\tau HighLDV_m \times O_i + X_i'\beta_t + \theta_{ot} + \epsilon_{i\tau} \mid \tau_i \geq \tau, \qquad (3)$$

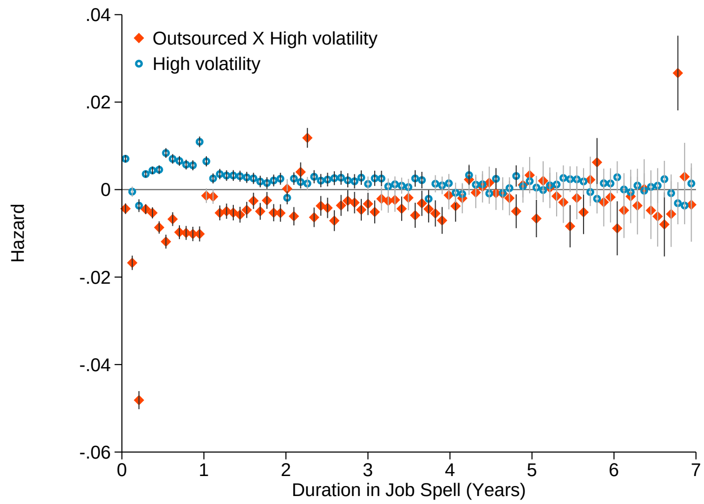
where $HighLDV_m$ is a dummy indicating whether city $m$ is among the three cities with higher labor demand volatility. Under the assumption that $O_i$ and $HighLDV_m$ are conditionally independent of $\epsilon_{i\tau}$, the coefficients $\gamma_\tau$ capture the effects of living in a high-volatility city on employment hazard, while the coefficients $\chi_\tau$ capture the additional effects of outsourcing on employment hazard in a high-volatility city. Our sample includes includes all first full-time spells at each employer between 2003-2010 in the relevant cities.

Figure 5 plots the estimated $\gamma_\tau$ and $\chi_\tau$ coefficients. We find that employment hazards for direct-hire workers were substantially higher in high-volatility cities than in low-volatility cities. For cleaners, the increases are statistically significant during almost all months in the first three

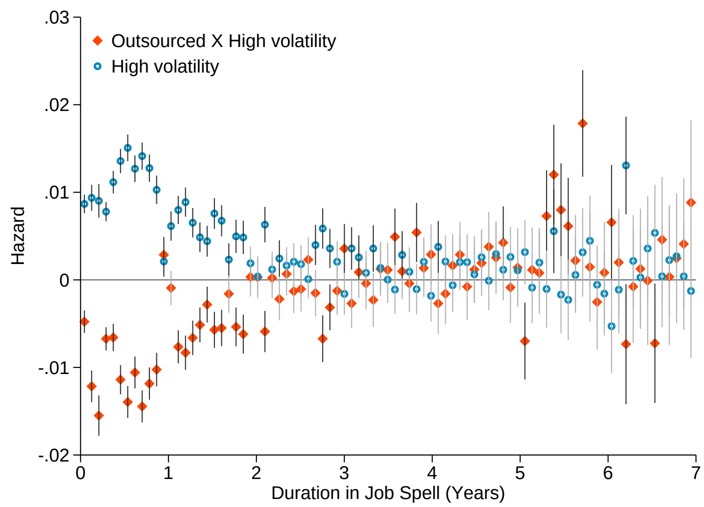
[10]Namely, Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador, and Sao Paulo.

Figure 5: Effect of Outsourcing on Employment Hazard, High vs Low Volatility Labor Markets

(a) Cleaners




(b) Security guards




Notes: Sample includes all first full-time spells at each employer between 2003-2010 in six major cities in Brazil: Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador, and Sao Paulo. Cities with higher labor market volatility than the median are classified as having high labor market volatility. The blue dot shows the estimated effect of working in cities with high labor market volatility on hazard to unemployment. The orange dot shows the added effect of working as an outsourced worker in cities with high labor market volatility on hazard to unemployment. We include the suboccupation X year fixed effects and baseline demographic controls. 95% confidence intervals are shown for the OLS estimates. Statistically insignificant estimates are shown in light gray.

years. For the security guards, the increases are statistically significant during the first two years. The differences in hazard are especially large during the first year of employment tenure.

Moveover, we find that outsourcing reduces employment hazard more in cities with high-volatility. Remarkably, the previously estimated increase in employment hazard in high-volatility cities is completely offset by outsourcing. As shown in Figure 5, the estimated $\gamma_\tau$ coefficients are the mirror image of the estimated $\chi_\tau$ coefficients, suggesting that the positive effects of outsourcing on employment security are larger in more volatile labor markets. Appendix Figures C.5 and C.6 show that these results are robust to using a continuous measure of labor demand volatility as well as additional controls.

Why does outsourcing redress labor market volatility? One possibility is that outsourcing reduces employment hazard by enabling flexible reassignment of workers across firms. Consistent with this idea, Battiston, Espinosa and Liu (2021) shows that 2-4 percent of workers in a large Columbia security service firm are rotated across clients in each month. The next section builds a model based on this possibility.

Another possible mechanism is that professional service firms facilitate downward adjustment in wages. In other words, outsourced workers may be more likely than direct hires to experience wage cuts rather than layoffs in response to reductions in labor demand. Appendix Table B.2 rules out this alternative explanation. We regress log wage on local unemployment rate and its interaction with outsourcing status, controlling for worker demographics, as well as suboccupation times microregion, and firm fixed effects. We find that the wages of outsourced workers are, if anything, more rigid than that of direct employees. Without controls for worker fixed effects, the negative relationship between wages and unemployment is similar for outsourced and direct-hire workers. With controls for worker fixed effects, the negative relationship between wages and unemployment becomes weaker for outsourced workers than for direct employees. This result suggests that outsourced contracts have greater wage rigidity, possibly due to the more standardized personnel policies of large organizations with many similar workers.

# 5 Interpretive Framework

In this section, we develop and estimate a stylized partial equilibrium search-and-matching model that formalizes the idea that outsourcing enables flexible reassignment of workers across clients. In the model, outsourcing alters the search and bargaining process in the labor market and thereby changes workers' wages and separation rates. We show that the observed hazard profiles are consistent with this model. We then use the estimated model to compare worker welfare at job start between outsourced and direct-hire workers.

## 5.1 Setup

Our model builds on Blanchard and Landier (2002), who study the effects of fixed-term contracts. The way we model reassignment of workers across firms by an intermediary is similar to models of on-the-job search (Pissarides 1994; Cahuc, Postel-Vinay and Robin 2006).[11]

A worker is matched with a firm under either employment or outsourcing. Under employment, the firm directly employs the worker. Under outsourcing, an intermediary employs the worker, but the worker is assigned to the firm. Time is continuous with a discount rate $r$. For simplicity, the choice between employment and outsourcing is assumed to be exogenous.[12]

Under each arrangement $a \in \{E, O\}$, match productivity is initially $y_a$ at $t = 0$. During the match, a single stochastic productivity shock $z$ arrives at Poisson rate $\lambda$. The match productivity then changes to $y_a + z$, where $z$ is a random variable with a continuous cumulative distribution $G(z)$.[13]

Wage is determined by Nash bargaining with worker bargaining parameter $\beta_a$ through continuous renegotiation between the firm and the worker. Since wages are bargained, we say that match-specific rents are *shared* between the worker and firm. If bargaining fails, the worker receives an outside option $\overline{W}$. Implicitly, we assume that the intermediary has zero bargaining power. This assumption is plausible since contract firms bid for service contracts competitively and the client often retains the ability to set wages for the outsourced workers.

Outsourcing has three potential effects in our model. First, outsourcing may alter worker bargaining power $\beta_a$. By allowing bargaining power to differ, we incorporate the prevailing

[11] Relatedly, Shimer (1999) and Prat (2006) study unemployment, worker turnover, and wage dispersion using models with match productivity shocks following Brownian motion. Arnold and Bernstein (2021) and Cahuc, Malherbet and Prat (2019) study the effects of discontinuities in severance pay schedules on worker hazard.

[12] Since we do not observe the identity of the end firm in our data, it is difficult for us to empirically analyze each firm's choice between outsourcing and employment.

[13] The change in productivity can be interpreted as symmetric learning about match productivity, as in Jovanovic (1979).

notion in extant literature (e.g., Dube and Kaplan 2010 and Goldschmidt and Schmieder 2017) that outsourcing lowers the rents that workers receive by circumventing within-firm fairness norms, avoiding collective bargaining agreements, or reducing efficiency wages.

Second, outsourcing allows workers to be reassigned across firms, while directly employed workers cannot. Specifically, we assume that if a worker-firm pair under outsourcing separates, then with some probability $\gamma_O$, the intermediary immediately matches the firm with a new worker and reassigns the worker to another firm. The productivity of the new match is $y_O$ and another productivity shock $z$ may arrive at rate $\lambda$. However, if worker-firm pairs under direct employment separate, then the worker receives her outside option and the firm must open a vacancy. In other words, the probability of reassignment under outsourcing is $\gamma_O \in [0, 1]$, while under employment $\gamma_E = 0$. For simplicity, we define $\gamma \equiv \gamma_O$.

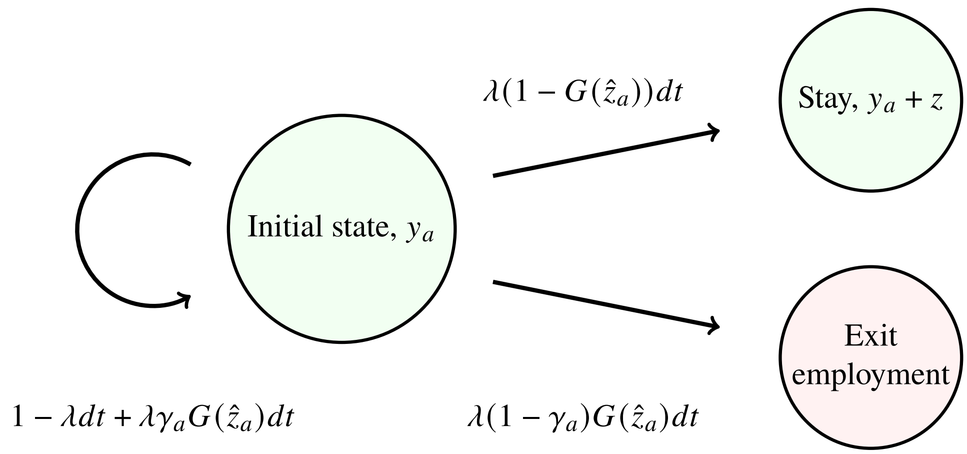
Third, outsourcing may alter the match productivity level $y_a$. This could be because the firms that select into outsourcing are systematically different. For example, Dube and Kaplan (2010) and Goldschmidt and Schmieder (2017) document that high-wage firms are more likely to outsource. It could also be that outsourced workers are positively selected due to the hiring and matching expertise of the intermediary. Yet another potential reason is that intermediaries may charge a fee for outsourced workers, which lowers the net productivity of outsourced workers. These differences in productivity levels are not important for showing why reassignment is needed to explain the observed hazard profiles, but help to fit the empirical data.

## 5.2 Equilibrium under Employment and Outsourcing

Figure 6 visualizes the state transitions under arrangement $a$. At the initial state, workers' match productivity is $y_a$. Productivity shock $z$ arrives at rate $\lambda$. There is a cutoff $\hat{z}_a$ such that separation will occur when $z$ is below the cutoff. If $z \geq \hat{z}_a$, the match continues and no further productivity shock arrives. If $z < \hat{z}_a$, then with probability $\gamma_a$, the worker is reassigned to another firm with initial match productivity $y_a$, and another productivity shock $z$ arrives at rate $\lambda$. Otherwise, the worker exits employment.

Let $\bar{V}^a$ denote the value of the firm that opens a vacancy under arrangement $a$. Let $V_0^a$ denote

Figure 6: Worker State Transitions




the value of the firm that is matched with a new worker under arrangement $a$. Let $V_1^a(z)$ denote the value of a firm that remains matched with the worker after the productivity shock $z$ arrives under arrangement $a$.

Once matched with a worker, the firm's Bellman equations before the shock is:

$$rV_0^a = \underbrace{(y_a - w_0^a)}_{\text{flow profit}} + \underbrace{\lambda \int_{\hat{z}_a}^{\infty} [V_1^a(z) - V_0^a] dG(z)}_{\text{stay after shock}}$$
$$+ \underbrace{(1 - \gamma_a)\lambda G(\hat{z}_a)(\bar{V}^a - V_0^a)}_{\text{separate after shock}} + \underbrace{\gamma_a \lambda G(\hat{z}_a)(V_0^a - V_0^a)}_{\text{re-assign after shock}}. \tag{4}$$

The flow profit for firms is productivity subtracted by wages $(y_a - w_0^a)$. With probability $\lambda$, the productivity shock arrives. When $z > \hat{z}_a$, workers will stay after the shock, and firms' utility gain is $V_1^a(z) - V_0^a$. When $z < \hat{z}_a$, with probability $1 - \gamma_a$, firms and workers are separated and firms do not get reassigned a new worker. In this case, the utility loss is $\bar{V}^a - V_0^a$. With probability $\gamma_a$, firms get a new worker and the utility change is $V_0^a - V_0^a = 0$.

If the productivity shock arrives and there is no separation, the firm's Bellman equation after the shock is

$$rV_1^a(z) = y_a + z - w_1^a(z) \tag{5}$$

The firm's flow profit is the post-shock productivity ($y_a + z$) subtracted by the post-shock wage ($w_1^a(z)$).

The worker's Bellman equation before the shock is:

$$
\begin{aligned}
rW_0^a = \underbrace{w_0^a}_{\text{flow wage}} &+ \underbrace{\lambda \int_{\hat{z}_a}^{\infty} [W_1^a(z) - W_0^a] dG(z)}_{\text{stay after shock}} \\
&+ \underbrace{(1-\gamma_a)\lambda G(\hat{z}_a)(\bar{W} - W_0^a)}_{\text{separate after shock}} + \underbrace{\gamma_a \lambda G(\hat{z}_a)(W_0^a - W_0^a)}_{\text{re-assign after shock}}
\end{aligned}
\tag{6}
$$

The flow payoff for workers is their wages. With probability $\lambda$, the productivity shock arrives. When $z > \hat{z}_a$, workers stay after the shock, and their utility gain is $W_1^a(z) - W_0^a$. When $z < \hat{z}_a$, with probability $1 - \gamma_a$, workers separate from the firm and take the outside option, and the utility loss is $\bar{W} - W_0^a$. With probability $\gamma_a$, workers are re-assigned to a new firm and their utility change is $W_0^a - W_0^a = 0$.

After the shock, if the worker remains matched with the same firm, the worker's Bellman equation is given by:

$$rW_1^a(z) = w_1^a(z). \tag{7}$$

Workers' flow utility is simply their wages. This is a self-absorbing state as no further productivity shocks will occur.

Wages are continuously negotiated through Nash bargaining, so we have that

$$
\begin{aligned}
(1-\beta_a)(W_0^a - \bar{W}) &= \beta_a(V_0^a - \bar{V}^a) & (8) \\
(1-\beta_a)(W_1^a(z) - \bar{W}) &= \beta_a(V_1^a(z) - \bar{V}^a) & (9)
\end{aligned}
$$

The free-entry condition suggests that the value of firms opening a vacancy is always zero, i.e., $\bar{V}^a = 0$.

The matched worker-firm pair is indifferent between separation and continuation at the productivity cutoff $\hat{z}_a$, so the cutoff $\hat{z}_a$ is pinned down by $V_1^a(\hat{z}_a) = \bar{V}^a$. Hence, we have

$$\hat{z}_a = -y_a + r\bar{W} \tag{10}$$

The cumulative probability of endogenous separation is

$$F_a(t) = \left[1 - e^{-(1-\gamma_a)\lambda t}\right] G(\hat{z}_a) \tag{11}$$

The hazard rate (the probability of separation in the current period conditional on being with the firm in the last period) is:

$$h_a(t) = \frac{F_a'(t)}{1 - F_a(t)} = \frac{(1-\gamma_a)\lambda G(\hat{z}_a)}{G(\hat{z}_a) + (1 - G(\hat{z}_a))e^{(1-\gamma_a)\lambda t}} \tag{12}$$

Workers' wages before and after the match productivity shock arrives are, respectively:

$$w_0^a = \beta_a y_a + (1-\beta_a)r\bar{W}, \tag{13}$$

and

$$w_1^a(z) = \beta_a(y_a + z) + (1-\beta_a)r\bar{W}. \tag{14}$$

## 5.3 Theoretical Predictions

In Section 4, we documented a striking "crossing" pattern in the hazard among cleaners in Brazil, wherein the hazard from formal employment of outsourced cleaners is initially lower than similar direct-hire cleaners but becomes higher after the first few years of employment tenure. Proposition 1 shows that this "crossing" pattern can be rationalized in our model by the possibility of flexible reassignment of workers across firms.

**Proposition 1.** *If the probability of reassignment under outsourcing, $\gamma$, is sufficiently large, then there exists some $T$ such that $h_E(t) \gtreqless h_O(t)$ if and only if $t \lesseqgtr T$. Otherwise, $h_E(t) \lesseqgtr h_O(t)$ for all $t$.*

*Proof.* See appendix. □

The intuition for Proposition 1 is as follows. Combining Equations (10) and (12), we can show that the initial hazard rate $h_a(0) = (1-\gamma_a)\lambda G(-y_a+r\bar{W})$. It follows that $h_O(0) < h_E(0)$ if either $\gamma$ is large or $y_O$ is high relative to $y_E$. In other words, transitions from formal employment are less likely at the start of an employment spell under outsourcing either if the intermediary reassigns workers across clients or if the initial worker-firm match productivity is larger for outsourced workers than direct-hire workers.

If the probability of reassignment $\gamma$ is large, however, the slope of the hazard respective to time under outsourcing flattens. This is because reassignment would lead to a new match at the initial match productivity level, and another match-specific shock may yet arrive. As a result, there is a form of dynamic selection: Conditional on survival, outsourced workers are less well protected from match-specific shocks. This causes their hazard rates to fall more slowly than those of directly employed workers.[14]

The combination of these two effects explains why there exists a cutoff $T$ such that $h_E(t) \gtreqless h_O(t)$ if and only if $t \lesseqgtr T$ only if $\gamma$ is sufficiently large. If the cutoff $T$ exists, it is possible to derive an explicit formula for it:

$$T = \frac{1}{\lambda}\log\left[\frac{\gamma}{e^{1-\gamma}(1-1/G(\hat{z}_O)) - (1-\gamma)(1-1/G(\hat{z}_E))}\right] \tag{15}$$

An immediate implication is that $T$ increases in $y_O$, but decreases in $y_E$ and $\lambda$. The relationship between $T$ and $\gamma$ is more ambiguous. It can be shown that $T$ increases in $\gamma$ if and only if $e^{1-\gamma}(1+\gamma) > \frac{G(\hat{z}_O)}{1-G(\hat{z}_O)} \cdot \frac{1-G(\hat{z}_E)}{G(\hat{z}_E)}$. Therefore, $T$ increases in $\gamma$ if $y_O \leq y_E$. However, $T$ may be decreasing in $\gamma$ if $y_O$ is much larger than $y_E$.

[14]The assumption of a one-time shock is not important for this result. As long as the probability of a subsequent negative match-specific shock falls after a match survives an initial shock, the hazard rates of the outsourced worker will fall more slowly due to the possibility of reassignment. It is only if the probability of a negative shock does not diminish conditional on survival (e.g., match productivity is drawn i.i.d. over time) that this prediction no longer applies.

Table 3: Parameters and Targeted Moments

| Parameter | Meaning | Targeted Moments |
|---|---|---|
| $\mu_E$ | average log initial match productivity of E workers | average initial wages of E workers |
| $\beta$ | bargaining power of O workers | average initial wages of O workers |
| $\sigma_E$ | std of log initial match productivity of E workers | std of initial wages of E workers |
| $\sigma_O$ | std of log initial match productivity of O workers | std of initial wages of O workers |
| $\lambda$ | arrival rate of match-specific shock | slope of the hazard rates for E workers |
| $\gamma$ | reassignment rate | slope of the hazard rates for O workers |
| $\mu_z$ | average productivity shock | average hazard rates of E workers |
| $\sigma_z$ | std of productivity shock | difference in hazard rates between E and O workers |
| $\delta$ | exogenous separation rate | long-run hazard rates of E and O workers |

E: direct-hire, O: outsourced.

## 5.4 Model Estimation

To assess the fit and quantify effects of outsourcing on worker welfare, we estimate an extended version of the above stylized model using observed wage and hazard distributions from Brazilian data. In the extended model, the initial match productivities, for both employees and outsourced workers, are drawn from a distribution $\log(y_{aj}) \sim N(\mu_a, \sigma_a)$, where $a \in \{E, O\}$. We assume that when outsourced workers are reassigned to a new firm, they are matched with a random firm, and the new match productivity is drawn from the same distribution $\log(y_{Oj}) \sim N(\mu_O, \sigma_O)$. This assumption allows us to better replicate the wage distribution observed in the data. We introduce an exogenous separation rate $\delta$ to better match the hazard rate. We discretize time and consider one period to be equivalent to one month.

Table 3 lists the parameters that we estimate using GMM and the corresponding targeted moments. The average wages of newly direct-hire workers identify their average initial match productivity. The average wages of newly hired outsourced workers determine the bargaining power of outsourced workers. The standard deviations of initial wages for direct-hire and outsourced workers reflect the standard deviations of initial match productivity for the respective groups of workers. The arrival rate of match-specific shocks, denoted as $\lambda$, is identified by the slope of the hazard rates for direct-hire workers.[15] The re-assignment rate for outsourced workers, denoted as $\gamma$, is identified by the slope of their hazard rates.[16] The average hazard rates

[15]The hazard rate in month $t$ represents the conditional probability of separating from the current employer in period $t$, given that the individual was employed in period $t-1$. A steeper hazard profile indicates a higher $\lambda$, as the match-specific shock leads to endogenous separation.

[16]A higher re-assignment rate results in a flatter hazard profile.

Table 4: Estimation Results

| Parameter | Cleaners | | Guards | |
|---|---|---|---|---|
| $\lambda$ | 0.0326 | (0.0008) | 0.0288 | (0.0013) |
| $\gamma$ | 0.3665 | (0.0241) | 0.1960 | (0.0229) |
| $\beta$ | 0.3058 | (0.1986) | 0.3985 | (0.1364) |
| $\delta$ | 0.0094 | (0.0001) | 0.0072 | (0.0000) |
| $\mu_E$ | 3.3244 | (0.2743) | 3.7197 | (0.3311) |
| $\sigma_E$ | 0.1440 | (0.0163) | 0.2241 | (0.0307) |
| $\sigma_O$ | 0.1858 | (0.0762) | 0.2558 | (0.0979) |
| $\mu_z$ | -33.7140 | (0.4958) | -28.5253 | (0.0885) |
| $\sigma_z$ | 0.2718 | (0.1136) | 0.0001 | (0.1855) |

Standard errors are reported in the parentheses.

of direct-hire workers identify the average level of productivity shock, denoted as $\mu_z$.[17] The standard deviation of productivity shock denoted as $\sigma_z$, impacts the average difference in hazard rates between direct-hire and outsourced workers.[18] The exogenous separation rate, denoted as $\delta$, is identified by examining the long-run hazard rates for both types of workers.[19]

We use predicted hazard rates holding constant worker characteristics and local labor market conditions. We drop the first three months to remove the potential effects arising from employment protection regulations, and perform local smoothing with a bandwidth of one year. We also use the counterfactual wage distribution if all observed security guards and cleaners were either outsourced or direct-hire.[20]

We calibrate parameters that cannot be easily estimated from our data. The monthly interest rate, denoted as $r$, is set at 0.0025, targeting an annual risk-free interest rate of 3%. The worker's outside option, denoted as $r\bar{W}$, is set at 70% of the value of employment at the average

[17] A higher $\mu_z$ corresponds to smaller hazard rates.

[18] Our simulation indicates that an increase in $\sigma_z$ results in a smaller gap in hazard rates between the two groups of workers.

[19] In the model, it is assumed that productivity shocks only occur once. In the long run, almost every worker has experienced the productivity shock, and separation can only be driven by the exogenous shock.

[20] We predict this counterfactual using a regression of the log real wage on the outsourced dummy, tenure dummies, and the interaction terms of outsourced and tenure dummies. The regression includes worker fixed effects, demographic controls for gender, age, age squared, race, years of schooling, and the suboccupation X year X microregion fixed effects at the spell level. We then use the regression residual (the residualized log wage) to compute the mean and standard deviation of residuals as our targeted moments. New hires are classified as workers whose duration of job spells is less than one year. The residual wage distributions are plotted in Appendix Figure B.3.

Table 5: Model Fit — Wages

| | Mean | Std | | Mean | Std |
|---|---|---|---|---|---|
| Direct-hire cleaners | | | Direct-hire guards | | |
| Data | 3.01 | 0.10 | Data | 3.46 | 0.15 |
| Model | 3.01 | 0.10 | Model | 3.46 | 0.15 |
| Outsourced cleaners | | | Outsourced guards | | |
| Data | 2.91 | 0.10 | Data | 3.46 | 0.15 |
| Model | 2.91 | 0.10 | Model | 3.46 | 0.15 |

wage.[21] The bargaining power of direct-hire workers is set at 0.5. This allows us to identify the distribution of initial match productivity of direct-hire firms from the wage distribution of newly hired direct-hire workers. The difference in average initial match productivity between outsourced and direct-hire workers is 10 log points, or $\mu_O - \mu_E = 0.1$.[22] Section E shows the robustness of our results using alternative values of the worker outside option and the productivity gap.

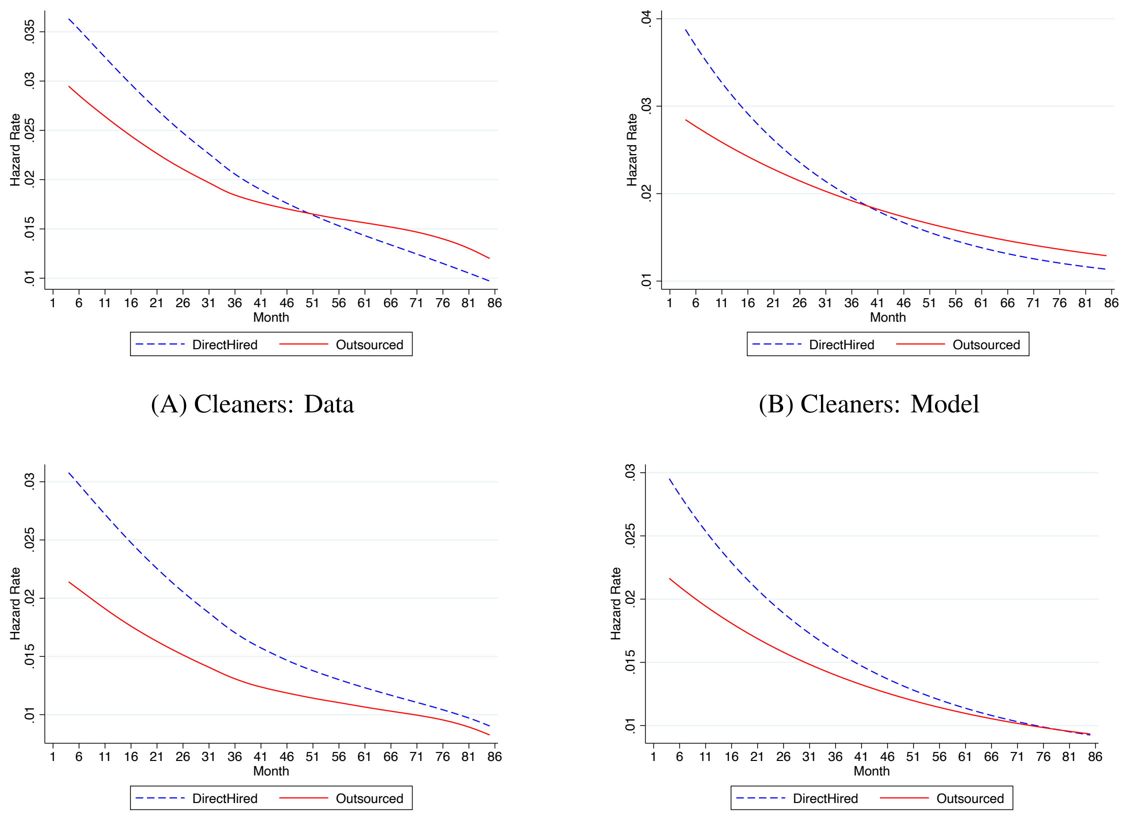
## 5.5 Parameter Estimates and Model Fit

Table 4 displays parameter estimates obtained using GMM. The estimated arrival rate of productivity shocks ($\lambda$) is roughly similar for both cleaners and guards, at approximately 3.3% and 2.9% per month, respectively. However, we estimate that 37% of cleaners are immediately re-assigned to a new firm following separation from their current client, while only 20% of guards are re-assigned. This difference arises because a crossing pattern in the hazard rates is observed for cleaners, but not for security guards (see Figure 1). As shown in Proposition 1, our model predicts that there is a crossing pattern in hazard rates only if the reassignment rate ($\gamma$) is sufficiently high.

[21] Appendix Table C.1 shows that after spells end with an involuntary exit from formal employment, a large fraction of workers are remain outside of our data even after a year. It is unclear whether they remain unemployed or are employed in the informal sector. Ulyssea (2010) estimates that a formal-informal wage gap of 15-30 percent, after controlling for observable worker characteristics. We will evaluate how our welfare estimates vary with the chosen value for the worker's outside option in a robustness check.

[22] Since we cannot observe the initial match productivity of outsourced workers and their bargaining power at the same time, we opt to calibrate the initial match productivity of outsourced workers and estimate their bargaining power using the wage distribution of newly hired outsourced workers. We later conduct a sensitivity analysis to demonstrate that our results remain robust to the chosen productivity gap between the two types of firms.

Figure 7: Model Fit — Hazard Rates




(A) Cleaners: Data

(B) Cleaners: Model

(C) Security guards: Data

(D) Security guards: Model

The estimated bargaining power ($\beta$) for outsourced cleaners is 0.31, while for outsourced guards it is 0.40. Recall that the bargaining power of direct-hire workers is set at 0.5. The estimated wage bargaining power of outsourced workers is therefore lower than that of direct-hire workers in both occupations. Outsourced cleaners also have less bargaining power than outsourced guards. This result is consistent with the fact that the outsourcing wage differential is more negative for cleaners than for guards.

The estimated model fits the wage distributions and hazard rates very well. Table 5 presents the model fit for wages. Figure 7 plots the model fit for hazard rates. Appendix E explores the robustness of our estimates to alternative calibration choices.

The implied monthly re-assignment rates from our model are 1.0% for cleaners and 0.5% for guards. These estimates are not too far from those by Battiston, Espinosa and Liu (2021), who

Table 6: Effect of Outsourcing on Worker Welfare

| | Cleaners | Guards |
|---|---|---|
| Wage | -11.0 | -1.3 |
| Welfare (in wage equivalence) | 0.4 | 7.5 |

Note: The numbers are percentage changes relative to direct-hire workers.

show that 2-4% of security guards in a Columbia firm are rotated across clients in each month.

## 5.6 Effects of Outsourcing on Worker Welfare

We leverage our structural estimates to evaluate the impact of domestic outsourcing on worker welfare. The first row of Table 6 reproduces the reduced-form estimates of the effect of outsourcing on wages from Table 2. In the second row, we report the estimated effect of outsourcing on workers' utility, measured by wage equivalence, taking into account the effect on hazards in addition to the effect on wages.

When we consider only the wage differential between outsourced and direct-hire workers, the impact of outsourcing on workers' welfare is negative. Cleaners experience an 11.0 percent reduction in welfare, while guards face a 1.3 percent reduction. The negative effect is more pronounced for cleaners, as the wage gap between outsourced and direct-hire workers is larger in this occupation compared to guards. However, when we take into account the lower hazard rates for outsourced workers, the estimated effect of outsourcing on worker welfare becomes positive for both occupations. This positive effect translates to a 0.4 percent increase in wages for cleaners and a 7.5 percent increase for security guards. These findings diverge from the estimated wage differential caused by outsourcing, which is negative for both cleaners and guards. Outsourced cleaners have less bargaining power compared to outsourced guards, but they also have higher rates of re-assignment. We observe that the welfare effect of outsourcing is greater for guards than for cleaners, suggesting that the former channel dominates the latter.

Appendix E explores the robustness of our worker welfare estimates to alternative calibration choices. We consistently find, across a wide range of calibration choices, that outsourcing has less negative effects on worker welfare than on worker wage.

# 6 Conclusion

This paper presents the first estimates of the effects of domestic outsourcing on worker employment security. Using comprehensive administrative data on security guards and cleaners in Brazil, we first confirm that outsourcing is associated with lower wages, especially for low-wage workers, as suggested by recent literature (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017; Drenik et al. 2023). We then robustly find that outsourcing is associated with a much lower rate of exit from formal employment during the first few years of employment spells. This difference is not explained by observable worker characteristics or differential exposure to labor market conditions. Moreover, we find that these effects are large in labor markets with greater demand volatility. We also provide evidence that these effects are not due to outsourcing enabling flexible wage adjustments.

To explain this novel fact, we provide a simple search-theoretic model wherein intermediary firms can both reassign outsourced workers across client firms in the event of negative productivity shocks and alter workers' wage bargaining power. The estimated model fits observed wage differentials and hazard profiles tightly. We estimate that outsourced workers in Brazil have higher welfare than comparable direct-hire employees due to improved employment security.

Our findings are important since existing literature on the aggregate effects of outsourcing ignores the potential welfare gains to workers from increased employment security and flexible reassignment of workers across firms. Our findings suggest that outsourcing has less negative consequences for workers than implied by prior literature. Future studies on the effects of domestic outsourcing on worker welfare should account for this potential benefit of domestic outsourcing, especially for non-core activities where contract-firm economies of scale in reassignment can be large and in developing country contexts where turnover is high.

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# Appendix

## A Data Definitions

**Outsourcing definition.** Appendix Table A.1 and A.2 shows our classification of occupation and industry codes. Appendix Table A.3 shows that the outsourced share of security guards steadily grew from 48 percent to 70 percent between 1998 and 2016. By comparison, there was only modest growth in outsourced employment of cleaners during the same period, which grew from 34 percent to 37 percent.

**Estimating AKM effects.** To construct worker and firm wage components, we use the two-way decomposition method of Abowd, Kramarz and Margolis (1999) (henceforth, "AKM effects"). Using data on all formal workers in RAIS spanning 1998-2016, 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, $\alpha_i$ is an 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), $\theta_t$ is a year fixed effect, $X_{it}\beta$ are the effects of time-varying observable worker characteristics (such as education and age), and $\epsilon_{ijt}$ is a composite error that may include idiosyncratic worker-firm match effects.

The estimated "AKM firm effect" ($\hat{\psi}_j$) can be thought of as representing the time-invariant pay premium of a given firm. The estimated "AKM worker effect" ($\hat{\alpha}_i$) can be thought of as representing time-invariant unobserved worker ability. 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.[23] A further concern when estimating the AKM model is limited mobility bias, which may generate misleading variance decompositions, as discussed by

[23] Identification of the AKM model requires that workers do not move across firms 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.

Andrews et al. (2008). We use a long panel so that limited mobility bias is more likely to be small (Bonhomme et al. 2023; Lachowska et al. 2023).

Table A.1: Occupation Classifications

| Classification | CBO code | Description |
|---|---|---|
| Guard | 517215 | Municipal civil guard |
| Guard | 517310 | Security agents |
| Guard | 517330 | Guards |
| Guard | 517420 | Watchpersons |
| Cleaner | 514210 | Sweepers |
| Cleaner | 514225 | General services workers (preservation, maintenance and cleaning) |
| Cleaner | 514225 | Cleaning and public welfare services worker |
| Cleaner | 514320 | Janitor |

Notes: CBO (*Classificação Brasileira de Ocupações*) is the Brazilian Classification of Occupations established by the Ministry of Labor to identify occupations in the labor market.

Table A.2: Contract Firm Classifications

| Classification | CNAE Code | Description |
|---|---|---|
| Contract firm | 74160 | Business management advisory activities |
| Contract firm | 74500 | Selection, agency and hire of labor |
| Contract firm | 74608 | Investigation, surveillance and security activities |
| Contract firm | 74705 | Activ. of hygiene and cleaning in buildings |
| Contract firm | 74993 | Other activ. of serv. provided mainly to other companies |

Notes: CNAE, National Classification of Economic Activities, is the official industry classification used by statistics and by federal, state, and municipal bodies in Brazil.

Table A.3: Trend in Outsourcing as Measured by Contract-firm Employment

| Contract-firm share of employment | | |
|---|---|---|
| Year | Cleaners | Guards |
| 1998 | 33.5% | 48.0% |
| 1999 | 33.3% | 52.1% |
| 2000 | 36.7% | 53.5% |
| 2001 | 31.2% | 55.1% |
| 2002 | 31.4% | 57.2% |
| 2003 | 33.4% | 57.9% |
| 2004 | 34.2% | 58.0% |
| 2005 | 35.0% | 58.6% |
| 2006 | 34.8% | 59.5% |
| 2007 | 34.7% | 60.0% |
| 2008 | 37.9% | 61.0% |
| 2009 | 37.6% | 62.3% |
| 2010 | 37.2% | 63.7% |
| 2011 | 37.4% | 64.6% |
| 2012 | 37.2% | 66.2% |
| 2013 | 37.6% | 67.5% |
| 2014 | 37.0% | 67.6% |
| 2015 | 36.2% | 68.6% |
| 2016 | 36.5% | 69.8% |
| Change | 3.1% | 21.9% |

Table A.4: Comparison of Formal and Informal Workers, Brazil, 2003- 2010

(a) Cleaners

| | Formally employed | Informally employed | Difference (with controls) |
|---|---|---|---|
| Age | 38.03 | 35.66 | -1.71** |
| | (11.34) | (13.58) | (0.21) |
| Years of schooling | 6.06 | 5.44 | -0.53** |
| | (3.54) | (3.61) | (0.06) |
| Male | 0.46 | 0.44 | -0.05** |
| | (0.50) | (0.50) | (0.01) |
| Non-white | 0.59 | 0.66 | 0.01 |
| | (0.49) | (0.48) | (0.01) |
| Working hours | 42.93 | 36.40 | -6.77** |
| | (8.72) | (14.00) | (0.21) |
| Log wage | 2.63 | 2.47 | -0.09** |
| | (0.36) | (0.57) | (0.01) |
| Observations | 33352 | 10737 | |

(b) Security guards

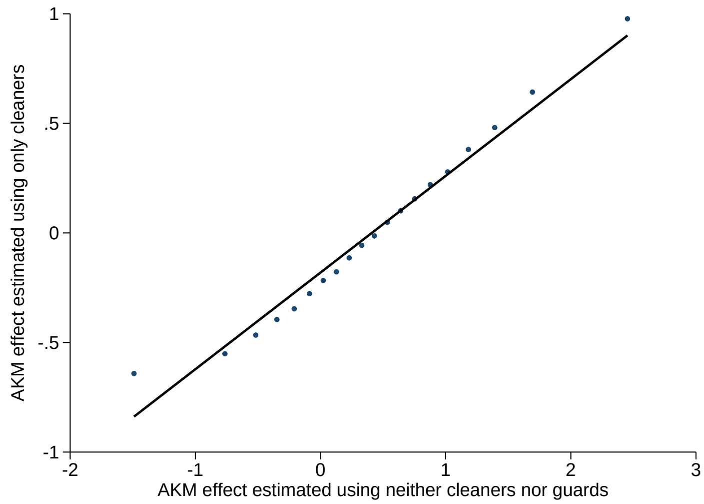
| | Formally employed | Informally employed | Difference (with controls) |
|---|---|---|---|
| Age | 38.44 | 39.82 | 1.61** |
| | (11.23) | (14.52) | (0.28) |
| Years of schooling | 7.64 | 6.03 | -1.41** |
| | (3.53) | (3.95) | (0.07) |
| Male | 0.96 | 0.96 | -0.01* |
| | (0.19) | (0.21) | (0.00) |
| Non-white | 0.55 | 0.65 | 0.03** |
| | (0.50) | (0.48) | (0.01) |
| Working hours | 47.28 | 46.94 | -0.67* |
| | (11.57) | (17.49) | (0.32) |
| Log wage | 2.90 | 2.54 | -0.21** |
| | (0.45) | (0.61) | (0.01) |
| Observations | 33892 | 7024 | |

Notes: This table shows the formally and informally employed comparison for cleaners and security guards. To define formally employed, the variable legally employed in the dataset is used. Occupational code for cleaners is 5142, while for security guards are 5173 and 5174. For controlled difference, year and state fixed effects are included. For the log wage, age, age square, years of schooling, male and non-white are included as controls, in addition to year and state dummies. Standard errors are displayed in parentheses, with ~ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level. Source: PNAD dataset (Brazil's National Household Sample Survey).

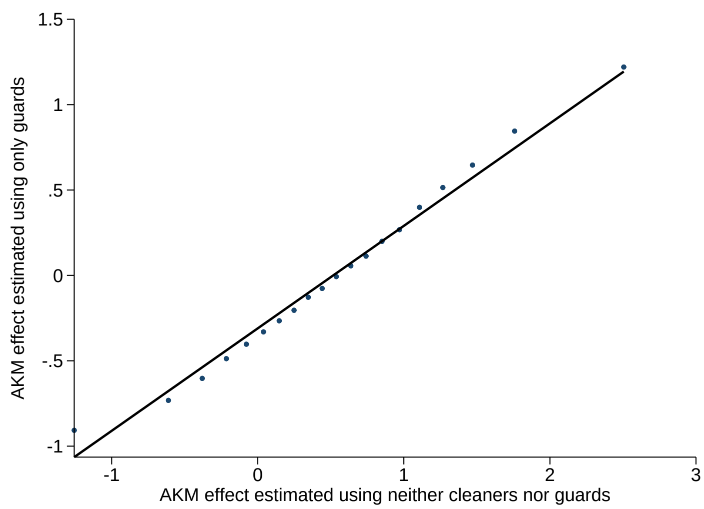
# B Additional Wage Results

Figure B.1: Comparing Estimated Firm-level Wage Premia using Different Subsamples

(a) Cleaners vs. Other workers



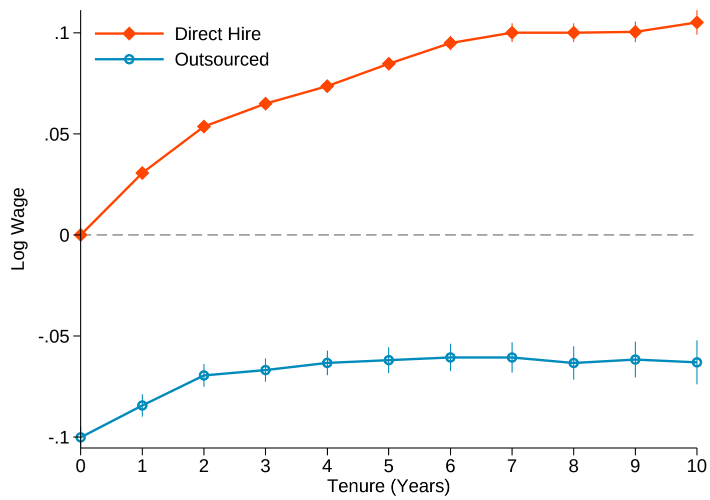

(b) Security guards vs. Other workers



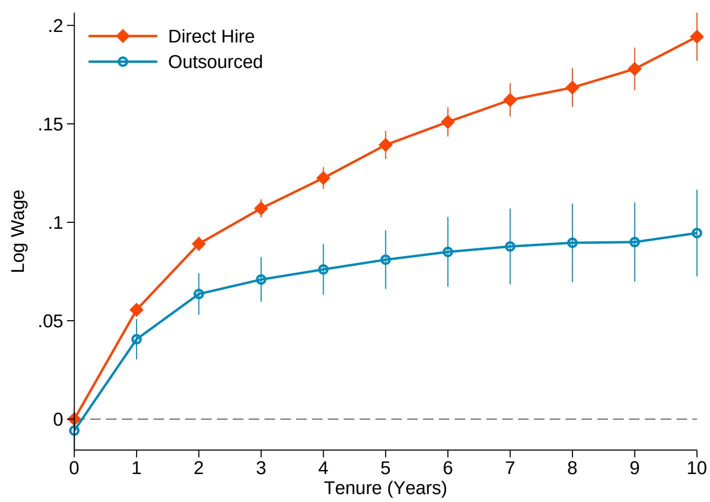

Notes: The figure shows a binned scatter plot of standardized AKM firm effects estimated using cleaners and other workers neither cleaners nor security guards, and another plot of standardized AKM firm effects estimated using security guards and other workers neither cleaners nor security guards. Each dot corresponds to 1/20 of the observations. We run a simple regression first and plot the fitting line in this figure.
Following Goldschmidt and Schmieder (2017), we use a split sample IV approach to correct measurement errors in the RHS. For cleaners, the regression coefficient of the simple regression is 0.441 (SE 0.0017). And the coefficient of IV regression is 0.606 (SE 0.0026). For security guards, the regression coefficient of the simple regression is 0.600 (SE 0.0033). And the coefficient of IV regression is 0.970 (SE 0.0050). All standard errors clustered on the firm level.

Figure B.2: The Effects of Outsourcing on Wage-tenure Profile

(a) Cleaners




(b) Security guards




Notes: This figure plots the estimated effects of outsourcing on wage-tenure profile. We plotted the estimated coefficients of the interaction terms between the tenure year dummies and the dummy indicating outsourcing in a wage tenure regression. We set the interaction term between the dummy indicating tenure less than 1 year and the dummy indicating workers are direct hired as zero to work as the reference group in the regression.

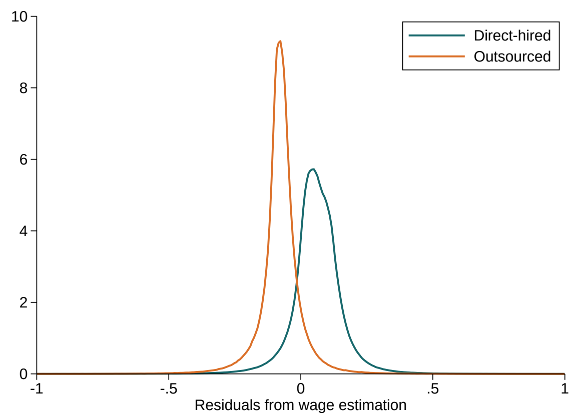
Table B.1: Wage Tenure Regression

| | Cleaners | | Security guards | |
|---|---|---|---|---|
| Outsourced | -0.110 | -0.105 | -0.013 | 0.005 |
| | (0.003) | (0.003) | (0.005) | (0.006) |
| Tenure | | 0.011 | | 0.021 |
| | | (0.000) | | (0.001) |
| Outsourced X Tenure | | -0.005 | | -0.012 |
| | | (0.001) | | (0.001) |
| Observations | 6150003 | 6150003 | 4253501 | 4253501 |
| $R^2$ | 0.94 | 0.94 | 0.93 | 0.93 |
| Occ X Year X Microregion FE | X | X | X | X |
| Demographic controls | X | X | X | X |
| Worker FE | X | X | X | X |

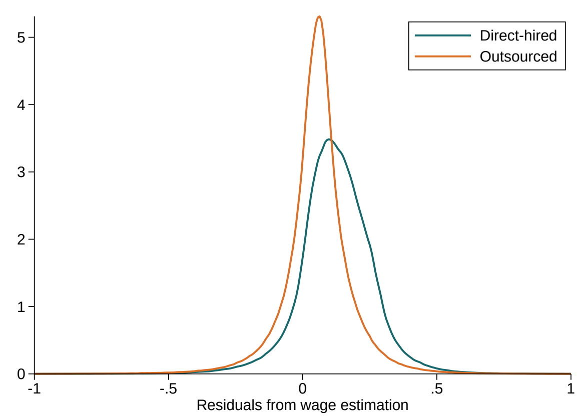
Notes: Dependent variable is log wage. Sample and controls are the same as Table 2 column (3). Tenure is measured in a continuous variable. Standard errors are displayed in parentheses, clustered at both worker and firm level.

Figure B.3: Residual Wage Distribution, Cleaners and Guards

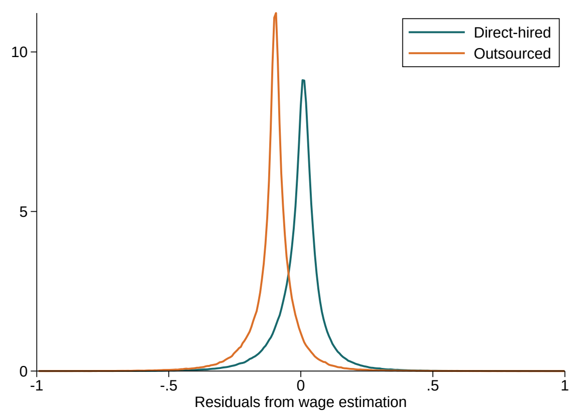
(a) All Cleaners



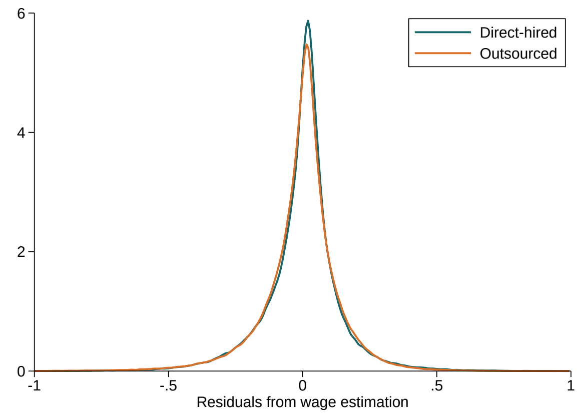

(b) All Guards




(c) Cleaners, Tenure Less than One Year




(d) Guards, Tenure Less than One Year




Notes: This figure shows the density of the residualized log wage for all and new-hire workers among cleaners and security guards, respectively. The residualized wages remove the influence of demographic variables and local labor market fluctuations. We compute the residualized wage as the sum of the residuals and the relevant estimated coefficients on the outsourced X tenure dummies from a regression of the log real wage on the outsourced dummy, tenure dummies, the interaction terms of outsourced and tenure dummies, worker fixed effects, demographic controls for gender, age, age squared, race, years of schooling, and the suboccupation X year X microregion fixed effects at the spell level.

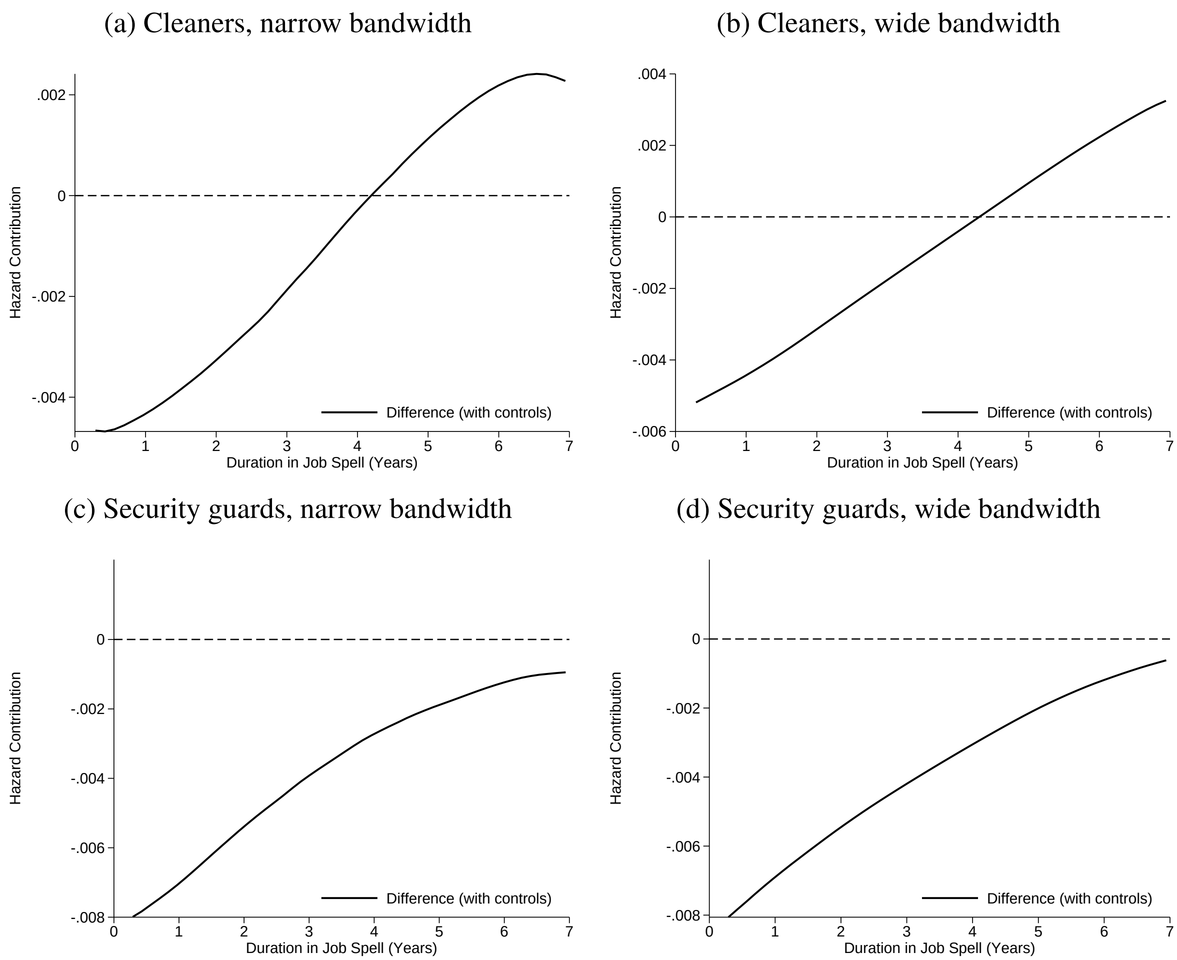
Table B.2: Wage Regression, Controlling State Level Unemployment Rate

| | Cleaners | | Security guards | |
|---|---|---|---|---|
| State Level Unemployment Rate | -5.145 | -0.980 | -5.220 | -1.614 |
| | (0.093) | (0.086) | (0.252) | (0.150) |
| Outsourced X State Level Unemployment Rate | 0.757 | 1.250 | 2.201 | 2.130 |
| | (0.203) | (0.136) | (0.369) | (0.323) |
| Observations | 7648116 | 6086694 | 4708677 | 4220416 |
| $R^2$ | 0.72 | 0.94 | 0.79 | 0.94 |
| Occ X Microregion FE | X | X | X | X |
| Demographic controls | X | X | X | X |
| Firm FE | X | X | X | X |
| Worker FE | | X | | X |

Notes: Dependent variable is log wage. The sample is the same as Table 2 column (3). We control state-level annual unemployment rates between 2003 and 2010 , which are calculated from PNAD data. We interact the unemployment rate with the outsourcing dummy. The unemployment rate in 2010 is imputed by taking the average unemployment rate between 2009 and 2011. Standard errors are displayed in parentheses, clustered at both worker and firm level.

# C Additional Hazard Results

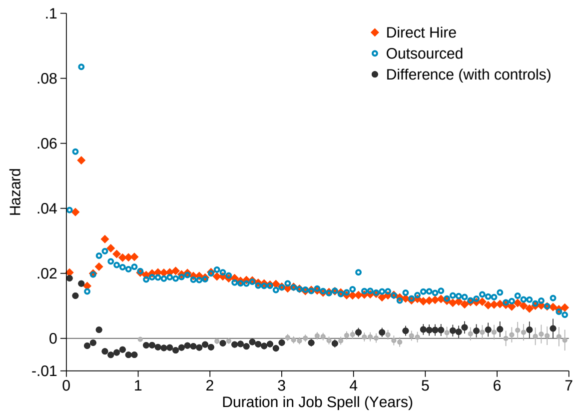
Figure C.1: Estimates of Hazard from Formal Employment, with Local Linear Smoothing



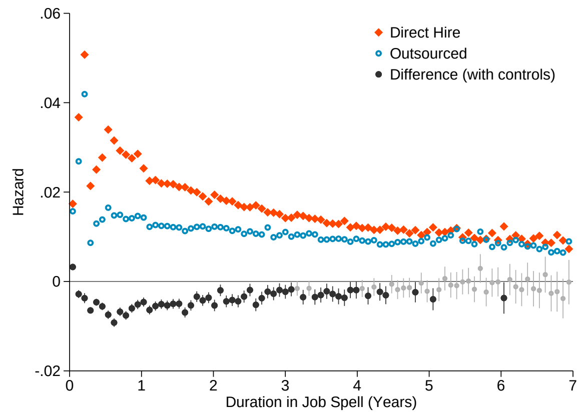

Notes: This figure plots the estimated hazard differential after linear smoothing using our main specification. The estimated hazard differential from the first three months are dropped, since employment protection legislation applies only after a three-month probationary period. Panels (a) and (c) use local linear smoothing with a bandwidth of 1 year. Panels (b) and (d) use a bandwidth of two years.

Figure C.2: Effect of Outsourcing on Hazard from Formal Employment, Alternative Samples

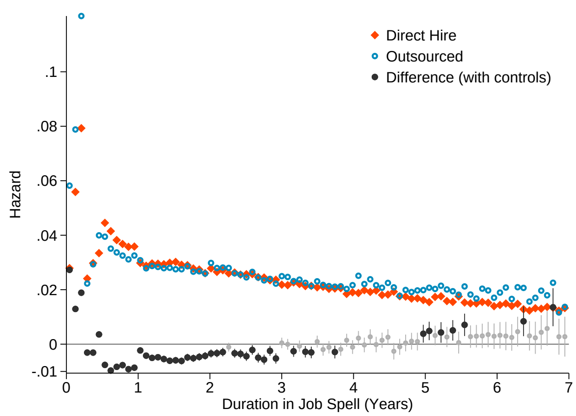
(a) Cleaners, Initially outside formal sector



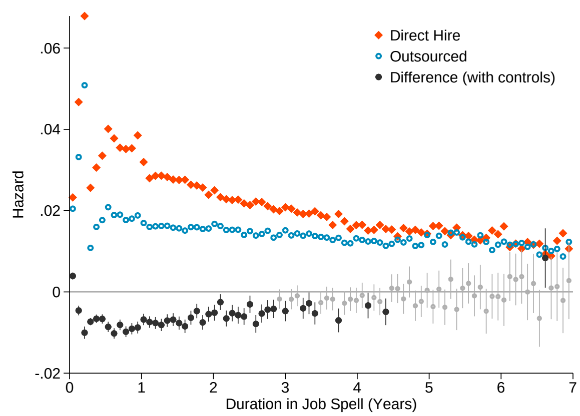

(b) Security guards, Initially outside formal sector



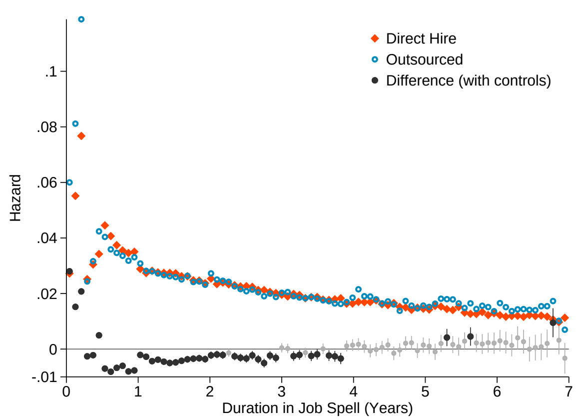

(c) Cleaners, Age ≤ 30 at spell start



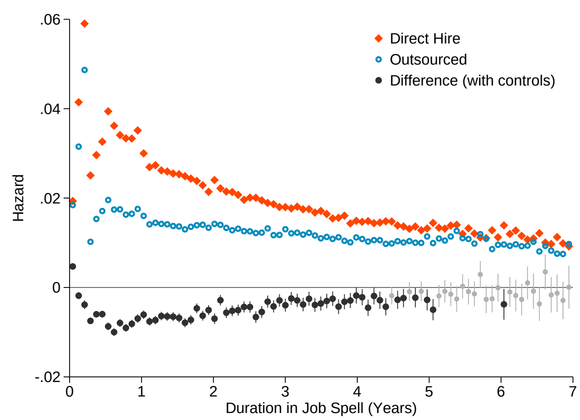

(d) Security guards, Age ≤ 30 at spell start




(e) Cleaners, Male



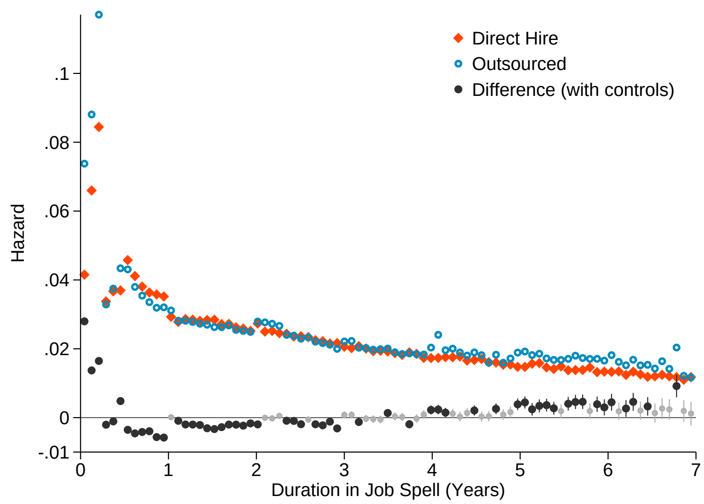

(f) Security guards, Male



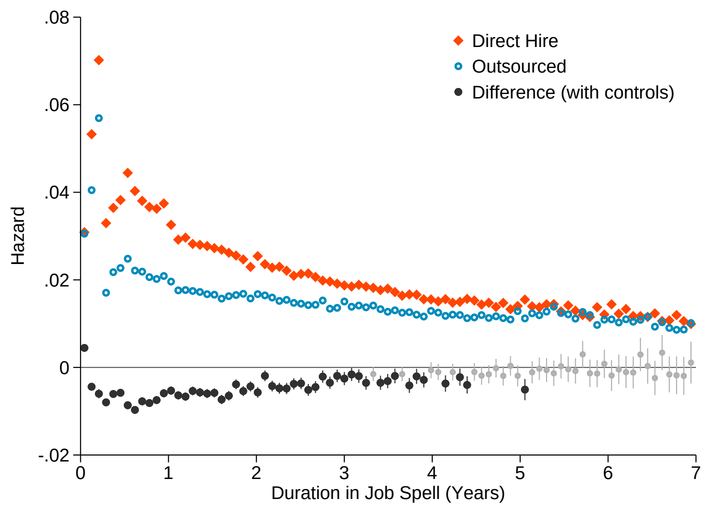

Notes: This figure replicates Figure 1 for subsamples. Panels (a) and (b) are restricted to workers who were not employed in the formal sector for at least seven days prior to the beginning of the spell. Panels (c) and (d) are restricted to workers who were age 30 or below. Panels (e) and (f) are restricted to male workers.

Figure C.3: Effect of Outsourcing on Hazard from Formal Employment, Including Quits

(a) Cleaners

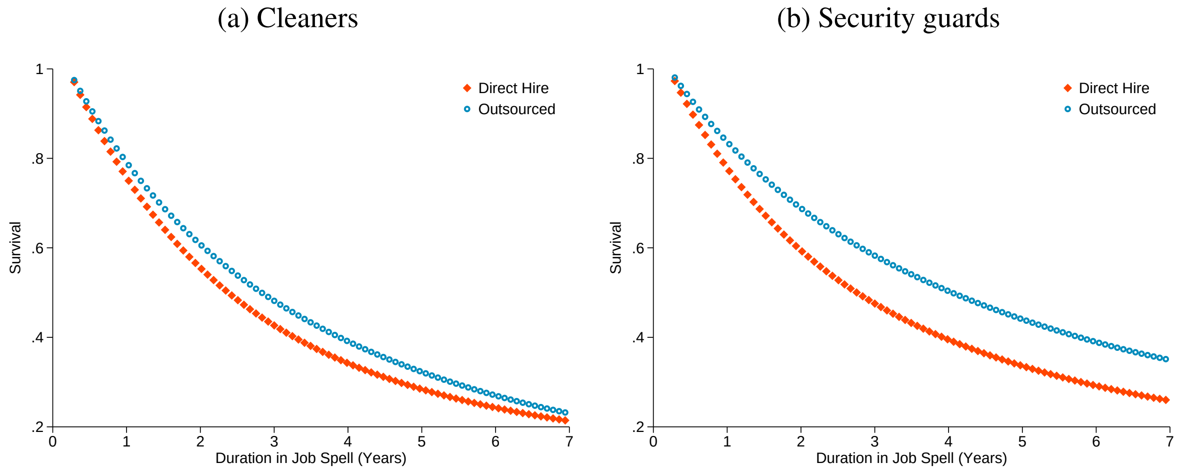



(b) Security guards




Notes: This figure replicates Figure 1 but does not censor quits.

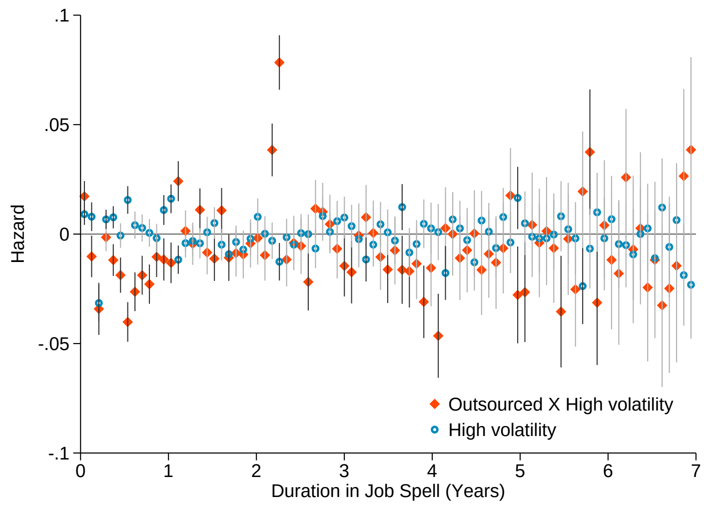
Figure C.4: Survival Function Based on Estimated Hazard from Formal Employment



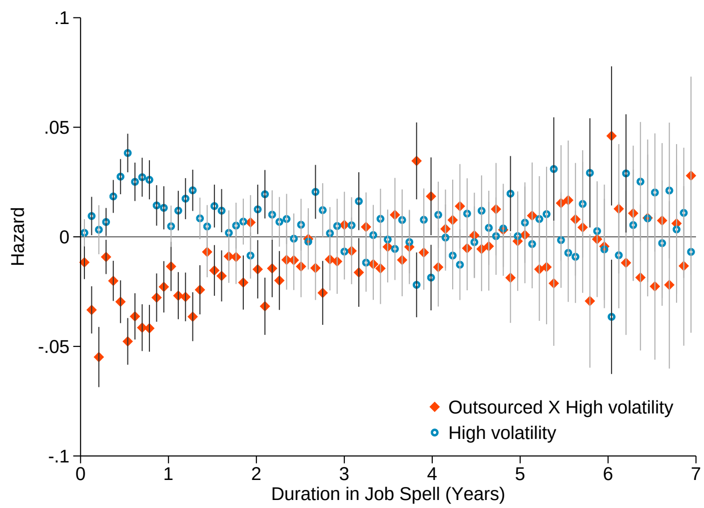

Notes: This figure shows the survival function implied by the hazard estimates from our main specification, after dropping the first three months and smoothing with a bandwidth of one year. The levels are calculated using the predicted mean hazard if observed workers were instead either all outsourced or directly employed.

Figure C.5: Effect of Outsourcing on Hazard from Formal Employment, Interacting with Raw Labor Market Volatility

(a) Cleaners



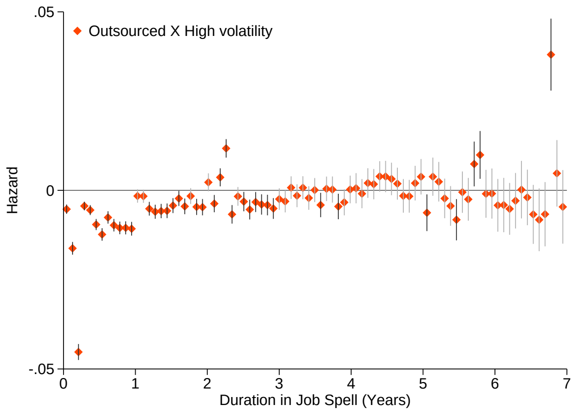

(b) Security guards



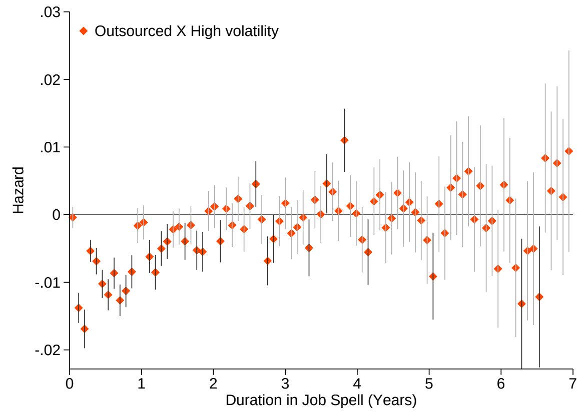

Notes: This figure replicates Figure 5 but instead uses the raw labor market volatility numbers in each city.

Figure C.6: Effect of Outsourcing on Hazard from Formal Employment, Interacting with Labor Market Volatility, Alternative Controls

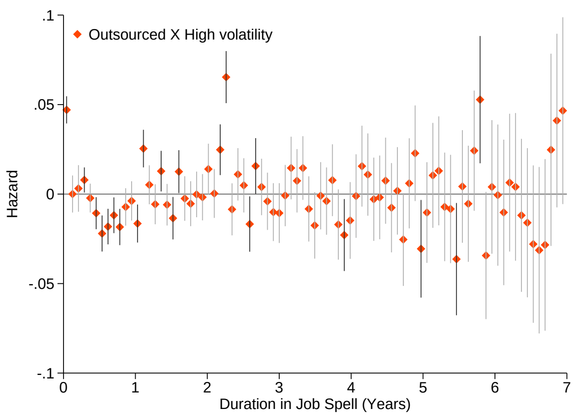
(a) Cleaners, dummies



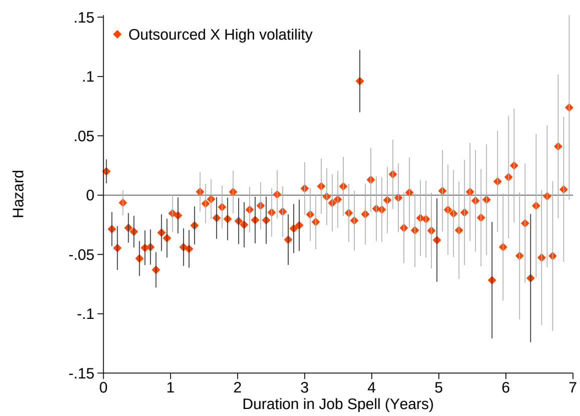

(b) Security guards, dummies




(c) Cleaners, raw LDV



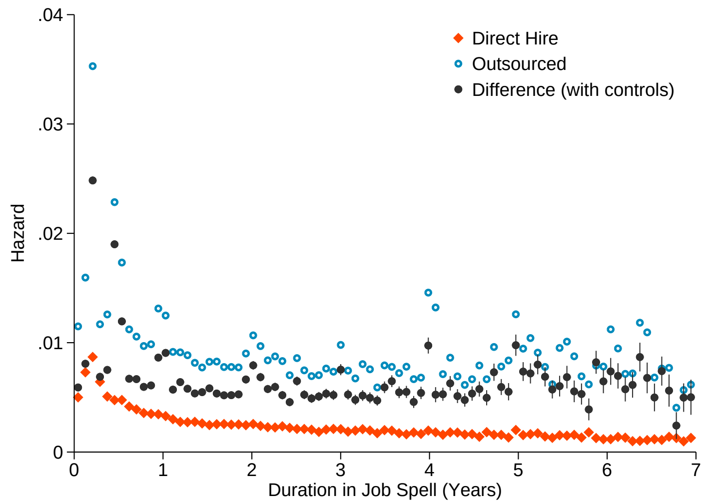

(d) Security guards, raw LDV



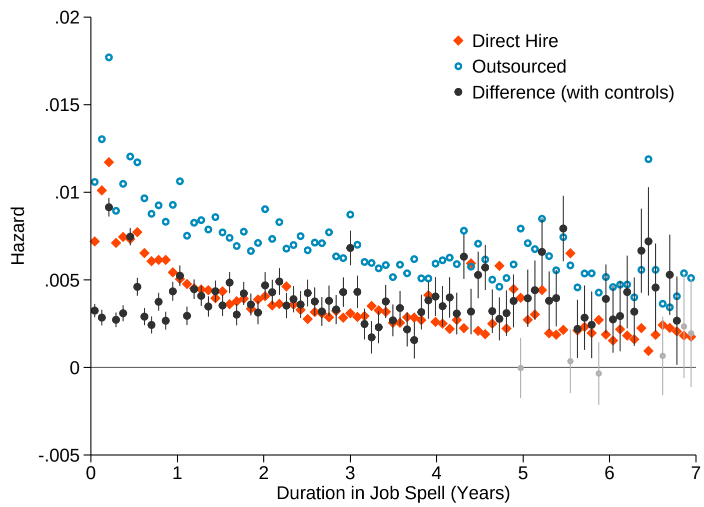

Notes: This figure replicates Figure 5 but controls the suboccupation X microregion X year fixed effects and baseline demographic controls. We only plot the coefficient of the interaction between outsourced and labor market volatility.

Figure C.7: Effect of Outsourcing on Transitions to Other Jobs

(a) Cleaners




(b) Security guards




Notes: This figure replicates Figure 1 but instead shows transitions to other jobs as the hazard outcome.

Table C.1: Employment Status, One Month or Year after Involuntary Exit from Formal Employment

| | Cleaners | | Security guards | |
|---|---|---|---|---|
| | Direct-hire | Outsourced | Direct-hire | Outsourced |
| One month after | | | | |
| Total formally employed | 0.05 | 0.08 | 0.05 | 0.09 |
| Same occupation, direct hire | 0.01 | 0.01 | 0.01 | 0.01 |
| Same occupation, outsourced | 0.00 | 0.02 | 0.01 | 0.04 |
| Different occupation | 0.04 | 0.05 | 0.03 | 0.03 |
| Observations | 1341210 | 845388 | 386633 | 400652 |
| One year after | | | | |
| Total formally employed | 0.36 | 0.38 | 0.32 | 0.39 |
| Same occupation, direct hire | 0.06 | 0.04 | 0.08 | 0.04 |
| Same occupation, outsourced | 0.02 | 0.08 | 0.04 | 0.19 |
| Different occupation | 0.29 | 0.26 | 0.20 | 0.16 |
| Observations | 1120331 | 718937 | 319257 | 332758 |

Table C.2: Effects of Outsourcing on Hazard from Formal Employment at year 2, 4 and 6, Alternative Controls

(a) Cleaners

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Effect at 2 years | -0.002 | -0.005 | -0.004 | -0.004 | -0.004 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $R^2$ | 0.000 | 0.011 | 0.070 | 0.071 | 0.072 |
| Effect at 4 years | 0.001 | -0.000 | 0.001 | 0.001 | 0.001 |
| | (0.000) | (0.001) | (0.001) | (0.001) | (0.001) |
| $R^2$ | 0.000 | 0.013 | 0.086 | 0.088 | 0.088 |
| Effect at 6 years | 0.004 | 0.004 | 0.004 | 0.004 | 0.003 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| $R^2$ | 0.000 | 0.014 | 0.088 | 0.090 | 0.090 |
| Suboccupation FEs | | X | | | |
| Subocc X Microreg X Year FEs | | | X | X | X |
| Demographic controls | | | | X | X |
| AKM Worker FEs | | | | | X |

(b) Security guards

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Effect at 2 years | -0.009 | -0.007 | -0.006 | -0.006 | -0.007 |
| | (0.000) | (0.000) | (0.001) | (0.001) | (0.001) |
| $R^2$ | 0.001 | 0.004 | 0.033 | 0.034 | 0.034 |
| Effect at 4 years | -0.004 | -0.002 | -0.001 | -0.001 | -0.002 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| $R^2$ | 0.000 | 0.003 | 0.040 | 0.042 | 0.042 |
| Effect at 6 years | -0.004 | -0.002 | -0.003 | -0.003 | -0.004 |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.002) |
| $R^2$ | 0.000 | 0.005 | 0.050 | 0.053 | 0.053 |
| Suboccupation FEs | | X | | | |
| Subocc X Microreg X Year FEs | | | X | X | X |
| Demographic controls | | | | X | X |
| AKM Worker FEs | | | | | X |

Notes: This table shows the estimation of effects at 2 years, 4 years and 6 years in Figure 3. Different sets of fixed effects and controls are included.

Table C.3: Average Labor Market Volatility in Six Main Cities, 2003-2010

| | Labor Demand Volatility |
|---|---|
| Recife | 0.748 |
| Belo Horizonte | 0.552 |
| Salvador | 0.527 |
| Sao Paulo | 0.517 |
| Porto Alegre | 0.472 |
| Rio de Janeiro | 0.402 |

Notes: This Table shows the average labor market volatility in Figure 5. Labor market volatility is the average absolute month-to-month change in unemployment rate between 2003 and 2010. The unit of labor market volatility is percentage point.

# D Proofs

## Proof of Proposition 1

Let $G_a = G(\hat{z}_a)$ and $\eta = 1-\gamma$. Let $L(t)$ be the ratio between two hazard rates:

$$L(t) \equiv \frac{h_O(t)}{h_E(t)} = \frac{\eta G_O}{G_E}\frac{G_E + (1-G_E)e^{\lambda t}}{G_O + (1-G_O)e^{\eta\lambda t}} \tag{16}$$

Let $\xi(t) \equiv \frac{G_E+(1-G_E)e^{\lambda t}}{G_O+(1-G_O)e^{\eta\lambda t}}$. We can then write $L(t) = \frac{\eta G_O}{G_E}\xi(t)$.

We first prove two useful lemmas.

**Lemma D.1.** *$L(T) = 1$ if and only if $T = \frac{1}{\lambda}\log\left[\frac{\gamma}{e^{1-\gamma}(1-1/G(\hat{z}_O))-(1-\gamma)(1-1/G(\hat{z}_E))}\right]$.*

*Proof.* By equation (16), $L(T) = 1$ if and only if

$$e^{\lambda T} = \frac{1-\eta}{(1-1/G_O)e^{\eta} - (1-1/G_E)\eta} \tag{17}$$

□

**Lemma D.2.** *$\xi'(t)$ is decreasing in $\eta$ for all $t$.*

*Proof.* Note that $\xi(t) > 0$, so $\frac{d\log\xi(t)}{dt} = \frac{\xi'(t)}{\xi(t)}$ has the same sign as $\xi'(t)$. Taking logarithm, we have:

$$\log\xi(t) = \log[G_E + (1-G_E)e^{\lambda t}] - \log[(G_O + (1-G_O)e^{\eta\lambda t}] \tag{18}$$

It follows that

$$\frac{d\log\xi(t)}{dt} = \frac{(1-G_E)\lambda e^{\lambda t}}{G_E + (1-G_E)e^{\lambda t}} - \frac{(1-G_O)\eta\lambda e^{\eta\lambda t}}{G_O + (1-G_O)e^{\eta\lambda t}} \tag{19}$$

Notice that only the second term is related to $\eta$. Letting $c = \eta\lambda$, the second term can be rewritten as $-\frac{(1-G_O)ce^{ct}}{G_O+(1-G_O)e^{ct}}$. Taking derivative over $c$ and noting that $G_O \in (0,1)$, we have:

$$-\frac{\partial \frac{(1-G_O)ce^{ct}}{G_O+(1-G_O)e^{ct}}}{\partial c} = -\frac{\overbrace{e^{ct}\overbrace{(G_O-1)}^{<0}[e^{ct}\overbrace{(G_O-1)}^{<0} - \overbrace{G_O(1+ct)}^{>0}]}^{>0}}{[-e^{ct}(G_O-1)+G_O]^2} < 0 \tag{20}$$

If $\eta$ increases, then $c = \eta\lambda$ increases, and thus $\frac{d\log\xi(t)}{dt}$ decreases. $\square$

The desired proposition follows from combining the following lemmas.

**Lemma D.3.** *If $\eta = 1$, then $h_O(t) \lesseqgtr h_E(t)$ for all $t$ if and only if $y_O \gtreqless y_E$.*

*Proof.* At $\eta = 1$, we have:

$$\begin{aligned}
\frac{d\log\xi(t)}{dt} &= \frac{(1-G_E)\lambda e^{\lambda t}}{G_E+(1-G_E)e^{\lambda t}} - \frac{(1-G_O)\lambda e^{\lambda t}}{G_O+(1-G_O)e^{\lambda t}} \\
&= \frac{(1-G_E)\lambda e^{\lambda t}[G_O+(1-G_O)e^{\lambda t}] - (1-G_O)\lambda e^{\lambda t}[G_E+(1-G_E)e^{\lambda t}]}{[G_E+(1-G_E)e^{\lambda t}][G_O+(1-G_O)e^{\lambda t}]} \\
&= \frac{[G_O(1-G_E) - G_E(1-G_O)]\lambda e^{\lambda t}}{[G_E+(1-G_E)e^{\lambda t}][G_O+(1-G_O)e^{\lambda t}]} \\
&= \frac{(G_O - G_E)\lambda e^{\lambda t}}{[G_E+(1-G_E)e^{\lambda t}][G_O+(1-G_O)e^{\lambda t}]}
\end{aligned} \tag{21}$$

The sign of $\frac{d\log\xi(t)}{dt}$ is the same as that of $G_O - G_E$. When $\eta = 1$, $L(0) = \frac{G_O}{G_E}$. If $y_O \gtreqless y_E$, then $G_O \lesseqgtr G_E$, so $\frac{d\log\xi(t)}{dt} \lesseqgtr 0$ and thus $\frac{dL(t)}{dt} \lesseqgtr 0$. Since $L(0) \lesseqgtr 1$, $h_O(t) \lesseqgtr h_E(t)$ for all $t$. $\square$

**Lemma D.4.** *If $\eta \in \left(\frac{G_E}{G_O}, 1\right)$ and $y_O < y_E$, then $h_O(t) > h_E(t)$ for all $t$.*

*Proof.* When $y_O < y_E$, $\hat{z}_O > \hat{z}_E$, $G_O > G_E > 0$, and $\frac{G_E}{G_O} \in (0,1)$. Notice that $\xi(0) = 1$, so $L(0) = \eta\frac{G_O}{G_E}$. Since $\eta \in \left(\frac{G_E}{G_O}, 1\right)$, $L(0) > 1$. At $\eta = 1$, since $G_O > G_E$, $\frac{d\log\xi(t)}{dt} > 0$ by Equation (21), which implies that $\xi'(t) > 0$. From Lemma D.2, $\xi'(t)$ is decreasing in $\eta$, so $\xi'(t) > 0$ for all $\eta \in (0,1)$. Therefore $L'(t) > 0$. We conclude that $L(t) > 1$, so $h_O(t) > h_E(t)$ for all $t$. $\square$

**Lemma D.5.** *If $\eta < \frac{G_E}{G_O}$ and $y_O < y_E$, then there exists $T$ such that $h_O(t) < h_E(t)$ for all $t < T$ and $h_O(t) > h_E(t)$ for all $t > T$.*

*Proof.* Since $\eta < \frac{G_E}{G_O}$, $L(0) < 1$. Note that $L(t)$ increases in $t$ for all $t$ (by the same logic as the previous proof). By Equation (16), $L(t) \to \infty$ as $t \to \infty$. Since $L(t)$ is continuous and monotone, there exists $T$ such that $L(t) < 1$ for all $t < T$ and $L(t) > 1$ for all $t > T$. □

**Lemma D.6.** *If $\eta < 1$ and $y_O \geq y_E$, then there exists $T$ such that $h_O(t) < h_E(t)$ for all $t < T$ and $h_O(t) > h_E(t)$ for all $t > T$.*

*Proof.* Since $y_O \geq y_E$, $G_O \leq G_E$. Since $\eta < 1$, $L(0) = \eta \frac{G_O}{G_E} < 1$. By Equation (16), $L(t) \to \infty$ as $t \to \infty$. By the continuity of $L$, there exists some $T_1$ such that $L(t) < 1$ for all $t < T_1$, and $T_2$ such that $L(t) > 1$ for all $t > T_2$. By Lemma D.1, there is a unique $T$ such that $L(T) = 1$, so $T_1 = T_2$. □

## Proofs for additional results

**Lemma D.7.** *$T$ increases in $y_O$, but decreases in $\lambda$ and $y_E$.*

*Proof.* This follows from Equation (15) and noting that $G_a = G(-y_a + r\bar{W})$. □

**Lemma D.8.** *$T$ increases in $\gamma$ if and only if $e^{1-\gamma}(1+\gamma) > \frac{G(\hat{z}_O)}{1-G(\hat{z}_O)} \cdot \frac{1-G(\hat{z}_E)}{G(\hat{z}_E)}$.*

*Proof.* Equation (17) implies that

$$\frac{de^{\lambda T}}{d\eta} = \frac{(1 - 1/G_O)e^{\eta}(\eta - 2) + (1 - 1/G_E)}{((1 - 1/G_O)e^{\eta} - (1 - 1/G_E)\eta)^2}. \tag{22}$$

Therefore, $\frac{dT}{d\gamma} \gtreqless 0$ if and only if

$$e^{1-\gamma}(1+\gamma) \lesseqgtr \frac{(1 - 1/G_E)}{(1 - 1/G_O)}. \tag{23}$$

□

# E Robustness: Structural Estimation

We perform two robustness checks to assess the sensitivity of our estimation results to alternative calibration values. First, we re-estimate the model for various values for the difference in initial match productivity between outsourced and direct-hire workers ($\mu_O - \mu_E$). Table E.4 presents the estimation results.

For cleaners, we find that key parameters such as re-assignment rates ($\gamma$) and productivity shock arrival rates ($\lambda$) are highly robust to values of the productivity gap ranging from 0 to 20 log points.[24] However, the estimated bargaining power of outsourced workers, $\beta$, falls with the productivity gap. This is intuitive, since a higher productivity gap implies that outsourced workers have higher initial match productivity on average, so to match the observed wages, the bargaining power of outsourced workers must be lower. Despite this, the estimated bargaining power of outsourced cleaners is still consistently lower than that of direct-hire cleaners in all specifications.

For guards, we find that the estimated re-assignment rates range from 7% to 20% when the productivity gap ranges from 10 to 20 log points, indicating a consistently low reassignment rate. However, the model fit substantially deteriorates as the productivity gap declines from 10 log points to 0. As shown in the last column of Table E.4, the average distance between the model's predicted hazard and the actual hazard in the data exceeds 1 percent when the productivity gap is smaller than 10 log points. The model also fails to predict the no-crossing pattern in the hazard rates when the productivity gap is below 10 log points. This suggests that the productivity gap needs to be at least 10 log points for the model to fit the observed hazard rate patterns.

Second, we re-estimate the model with different values of the worker's outside option. In our baseline specification, we calibrated the outside option ($r\bar{W}$) to be 70% of the value of employment at the average wage. Here we use a range of values for the outside option, ranging from 60% to 90% of the value of employment at the average wage. The results are presented in Table E.5. The estimated average initial match productivity of direct-hire workers declines as the value of the worker's outside option increases, as indicated by Equation 13. This will

[24]The estimated values of $\gamma$ range from 0.372 to 0.366 as the productivity gap changes from 0 to 0.2.

lead to a decrease in the average initial match productivity and a decline in the bargaining power of outsourced workers. However, the model fit and estimates of re-assignment rates for both cleaners and guards are broadly similar across different choices of the value.

In Table E.6, we present welfare estimates from alternative models as robustness checks. We explore different calibration methods, such as setting the initial match productivity gap between outsourced and direct-hire workers to 0.05 or 0.15 instead of 0.1, and adjusting the value of the worker's outside option to be 50 to 90 percent of the value of employment at the average wage. Our results demonstrate that the welfare implications for outsourced cleaners and guards remain consistent across different calibration assumptions of the productivity gap. As the value of the worker's outside option increases, the welfare benefit of being an outsourced worker decreases for both cleaners and guards, as the advantage of re-assignment becomes smaller. Across various calibration methods, the welfare effect ranges from -6.6 percent to 4.1 percent for cleaners and from 4.5 percent to 11.6 percent for security guards.

Table E.4: Robustness Checks — Productivity Gap

| Productivity gap | $\lambda$ | $\gamma$ | $\beta$ | $\delta$ | $\mu_E$ | $\sigma_E$ | $\sigma_O$ | $\mu_z$ | $\sigma_z$ | Fit error |
|---|---|---|---|---|---|---|---|---|---|---|
| A. Cleaners | | | | | | | | | | |
| 0 | 0.032 | 0.372 | 0.367 | 0.0093 | 3.324 | 0.144 | 0.172 | -52.90 | 0.514 | 0.377 |
| 0.05 | 0.033 | 0.369 | 0.334 | 0.0093 | 3.324 | 0.144 | 0.179 | -32.63 | 0.041 | 0.376 |
| 0.1 | 0.033 | 0.366 | 0.306 | 0.0094 | 3.324 | 0.144 | 0.186 | -33.71 | 0.272 | 0.375 |
| 0.15 | 0.033 | 0.366 | 0.281 | 0.0094 | 3.324 | 0.144 | 0.192 | -36.58 | 0.738 | 0.375 |
| 0.2 | 0.033 | 0.366 | 0.258 | 0.0094 | 3.324 | 0.144 | 0.199 | -40.00 | 1.299 | 0.375 |
| B. Guards | | | | | | | | | | |
| 0 | 0.025 | 0.438 | 0.493 | 0.0056 | 3.720 | 0.224 | 0.229 | -65.20 | 0.021 | 1.501 |
| 0.05 | 0.026 | 0.377 | 0.442 | 0.0060 | 3.720 | 0.224 | 0.243 | -36.48 | 0.000 | 1.334 |
| 0.1 | 0.029 | 0.196 | 0.399 | 0.0072 | 3.720 | 0.224 | 0.256 | -28.53 | 0.000 | 0.668 |
| 0.15 | 0.031 | 0.072 | 0.361 | 0.0080 | 3.720 | 0.224 | 0.268 | -26.72 | 0.001 | 0.289 |
| 0.2 | 0.028 | 0.144 | 0.329 | 0.0071 | 3.720 | 0.224 | 0.280 | -33.51 | 0.003 | 0.206 |

Fit error presents the average percentage difference between the model's predicted and the data's actual hazard rates.

Table E.5: Robustness Checks — Worker Outside Option

| Worker outside option (as % of value of employment at average wage) | $\lambda$ | $\gamma$ | $\beta$ | $\delta$ | $\mu_E$ | $\sigma_E$ | $\sigma_O$ | $\mu_z$ | $\sigma_z$ | Fit error |
|---|---|---|---|---|---|---|---|---|---|---|
| A. Cleaners | | | | | | | | | | |
| 0.5 | 0.033 | 0.366 | 0.358 | 0.0094 | 3.450 | 0.127 | 0.140 | -38.90 | 0.002 | 0.375 |
| 0.6 | 0.033 | 0.366 | 0.337 | 0.0094 | 3.389 | 0.135 | 0.159 | -35.99 | 0.092 | 0.375 |
| 0.7 | 0.033 | 0.366 | 0.306 | 0.0094 | 3.324 | 0.144 | 0.186 | -33.71 | 0.272 | 0.375 |
| 0.8 | 0.033 | 0.366 | 0.256 | 0.0094 | 3.255 | 0.154 | 0.236 | -33.82 | 0.422 | 0.375 |
| 0.9 | 0.033 | 0.366 | 0.159 | 0.0094 | 3.180 | 0.166 | 0.390 | -46.56 | 1.632 | 0.375 |
| B. Guards | | | | | | | | | | |
| 0.5 | 0.029 | 0.179 | 0.428 | 0.0073 | 3.866 | 0.194 | 0.207 | -41.01 | 0.000 | 0.601 |
| 0.6 | 0.029 | 0.193 | 0.417 | 0.0072 | 3.796 | 0.208 | 0.228 | -34.99 | 0.000 | 0.644 |
| 0.7 | 0.029 | 0.196 | 0.399 | 0.0072 | 3.720 | 0.224 | 0.256 | -28.53 | 0.000 | 0.668 |
| 0.8 | 0.029 | 0.193 | 0.363 | 0.0072 | 3.637 | 0.243 | 0.302 | -22.14 | 0.000 | 0.651 |
| 0.9 | 0.028 | 0.204 | 0.260 | 0.0069 | 3.547 | 0.265 | 0.441 | -18.94 | 0.001 | 0.518 |

The worker outside option in the baseline model is 70% of the value of employment at the average wage. The fit error presents the average percentage difference between the model's predicted and the data's actual hazard rates.

Table E.6: Robustness Checks on Welfare

| Worker outside | Productivity gap | | |
|---|---|---|---|
| option | 0.05 | 0.1 | 0.15 |
| Cleaners | | | |
| 0.6 | 4.1 | 3.6 | 3.6 |
| 0.7 | 0.4 | 0.4 | 0.4 |
| 0.8 | -2.8 | -2.8 | -2.8 |
| 0.9 | -6.4 | -6.4 | -6.6 |
| Guards | | | |
| 0.6 | 11.6 | 9.0 | 10.1 |
| 0.7 | 10.6 | 7.5 | 9.2 |
| 0.8 | 7.5 | 6.3 | 8.5 |
| 0.9 | 4.5 | 5.5 | 5.5 |

Note: The numbers are percentage changes relative to direct-hired workers. We vary the worker outside option from 60% to 90% of the value of employment at the average wage. We also vary the productivity gap from 5% to 15%.