Talent Poaching and Job Rotation

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Abstract

Firms allocate workers to clients to provide services. On the job, workers acquire skills that increase their client-specific productivity and therefore raise the probability that clients poach them. In this paper, we advance the understanding of this important, yet understudied feature of service industries. We show, both theoretically and empirically, that in order to mitigate poaching risk firms may forgo potential productivity gains by moving workers from one client to the other. Focusing on a security service-industry firm in Colombia, we find that an increase in client-specific experience both decreases crime and increases the probability that the workers are poached. After a policy change that forbids talent poaching, the firm sharply decreased the frequency of rotation, especially for workers who were more likely to be poached before the policy change. The theoretical model we propose is consistent with these empirical patterns and substantiates the broad applicability of the studied mechanism.

Keywords: talent poaching, job rotation, outsourcing

JEL Classification: D22, J24, L84, M21, M51, M54

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1 Introduction

By the end of the XX century all the service sectors of advanced economies have absorbed the largest share of value added and employment. Similar trends occur in less developed countries. The rise of service sectors has drastically transformed the labor market, in particular regarding the arrangement of employment: Across industries and countries, employers increasingly rely on service-providing firms (or independent contractors) to undertake work previously carried by their own employees (e.g., Goldschmidt and Schmieder, 2017; Dorn et al., 2018). These dramatic changes in the economy make questions on the internal workings of service sectors a primary concern.

In this article, we study a distinctive feature of service industries that has been widely discussed in the public domain, yet has received scarce attention among researchers. On the job, the outsourcing workers allocated by the service firm to clients accumulate experience that make them more productive with those specific clients. However, after a worker has acquired sufficient skills specific to a client’s needs, the client may want to bring that worker in-house as it is cost-efficient to do so.\footnote{For recent media coverage and public discussions on talent poaching from past clients, see Markopoulos (2012), DLA Labor Dish Editorial Board (2014), Bradshaw (2015), Chaput (2018), Bennet (2018) and Stevens Vuaran Lawyers (2019).} Anticipating the client’s behavior, the service firm may take costly actions to avoid poaching. We argue that one of these actions consists in rotating workers from one client to the other. By doing so, the firm hinders the workers’ skill acquisition, so that they remain sufficiently unattractive to the clients. Importantly, the excessive rotation due to the poaching concern can have policy implications as the client-specific skills that workers acquired are lost.

Our empirical analysis focuses on the security-service industry and uses a novel and detailed dataset from a private security firm in Bogota, Colombia. During the period of our analysis (74 months in total), the firm allocated 628 guards to a large sample of residential buildings (i.e., 94 clients) on a daily basis. For each guard, we have information on when and where he/she worked, previous work experience, age, gender and residential address. For each building, we have information on who worked in the building and when, where it is located, number of flats, and number of guard positions to be filled. In addition, the data contains two measures of poaching intensity: whether a guard received a formal solicitation from a
building, and whether a guard ended up being hired in-house by a building during the sample period. Finally, we also have information on an important measure of workers' productivity: crime committed in the buildings. In particular, our dataset specifies the identity of the worker who was on duty when a crime happened, and the value of the properties lost in the crime.

We present three main empirical results. The first result establishes the relationship between the client-specific skill of a worker and the poaching decision of the client. We find that even after controlling for the guard’s experience, an increase in the time that the guard has worked for a specific building increases the probability of him/her being poached by that building. We argue that this is because the skill that a guard acquires through client-specific experience is important for productivity in our setting: As a guard accumulates more working time in a building, both the probability that a crime occurs in that building and the expected value of stolen properties (when a crime does occur) decrease. These findings are robust even after controlling for the matching between buildings and guards.

To address the potential endogeneity bias arising from omitted variable and reverse causation, we further use an instrumental variable (IV) based on the system that the firm designed to allocate guards to shifts. We exploit the fact that guards are exogenously divided by the firm into two types (denoted by type-I and type-II, respectively). Specifically, type-I guards are allocated to a unique building to cover weekly shifts. By contrast, type-II guards are assigned to different buildings to cover daily shifts when their type-I co-workers rest. This allocation creates a mechanical variation in the client-specific experience. Namely, a type-I guard accumulates more shifts in a given building compared to a type-II guard working in the same building during the same period of time. The IV results confirm the positive association between client-specific experience and observed poaching. In particular, a 10% increase in the building-specific experience is associated with additional 1.8 percentage points in the probability of being poached by the corresponding building. Also echoing the previous reduced-form analysis, we find that crime drops as a result of the guard accumulating more

\[\text{Huckman and Pisano (2006) find a similar relationship between the quality of a cardiac surgeon's performance at a given hospital and his/her recent procedure volume at that hospital.}
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\[\text{The exogeneity of the variation arises from the fact that the firm allocates workers on a first-come first-serve. That is, the assignment of a new guard depends on job availability. For example, if there are already enough type-I guards for the firm to allocate to the buildings, the next guard to be hired will occupy a type-II position.}\]
shifts in the building.

Our second empirical result shows that the firm rotates more often those guards with a higher risk of poaching. To estimate the poaching risk, we exploit the fact that buildings systematically prefer to hire guards with certain baseline characteristics (e.g. young and non-migrant guards), as revealed by their observed poaching behaviour. In particular, we use a Random Forest model to construct a worker-specific index of poaching risk for type-I guards (no type-II guard has ever been poached) and we show that the rotation of guards is highly correlated with the poaching risk index. Specifically, we find that a one standard deviation increase in the estimated risk of poaching is associated with 1.5 additional percentage points in the probability of rotation.

The last empirical result exploits a policy change (the Decree 356 of 1994 in Colombia) that de facto limited the possibility that buildings hire guards in-house. If the security company rotates workers with the aim of avoiding them to acquire client-specific skills, and therefore to increase the probability of being captured by the clients, this rotation should decrease once the policy change has taken effect. Consistent with this intuition, we show that the guards more likely to be poached before the policy change were rotated less after the policy took effect. More precisely, we show that a 10% increase in our poaching risk index reduces the probability that the guard is rotated in a given month by 1.5 percentage points. The magnitude of this effect is large compared to the average monthly rotation before the policy (4%).

Finally, we show that buildings that had a larger fraction of workers with a large probability of being poached (before the policy change) were precisely those that saw the largest reduction in crime after the policy took effect. Taken together, our empirical findings suggest that the firm rotated its workers excessively to avoid them from being poached and when a non-poaching policy took place, crime rates decreased as the security firm reduced rotation, allowing the workers to acquire larger client-specific skills. The previous results have policy implications: As far as one is concerned with reducing crime rates, our setting provides a rationale to prohibit poaching talent.

At this stage, a possible concern with our results is that they may be driven by the specific empirical setting we study. To advance in the broad applicability of the mechanism
studied here, we propose a theoretical model that captures the trade-off faced by the service-providing firm. Specifically, we consider a risk-neutral firm employing a team of workers and transacting with a risk-averse client. At the beginning, the client does not have in-house workers so she pays a service fee for outsourcing a risky production activity to the firm. The client can always choose to poach the firm’s workers, who in the meantime acquire productivity-increasing experience by performing the client’s activity. We show that the firm over-rotates its workers before they reach a certain client-experience threshold. In equilibrium, the workers with more desirable characteristics (e.g., larger industry experience or baseline productivity) are rotated more often. This demonstrates that a non-poaching policy can facilitate the accumulation of client-specific skills and increase productivity (e.g., decrease crime rate as in our empirical setting) by eradicating strategic over-rotation.

**Related literature.** Economists have long recognized that job rotation can impede skill accumulation and decrease job-specific productivity ([Ickes and Samuelson] 1987; [Groysberg and Nanda] 2008; [Di Maggio and Alstyne] 2013). To rationalize the common use of rotation in organizations, a strand of the literature argues that the learning benefits of rotation can outweigh the potential productivity loss. This applies to both employee learning, which emphasizes that rotation can increase the general human capital of workers by allowing them to be exposed to a wide range of experiences ([Staats and Gino] 2012), as well as employer learning, which stresses that rotation can be an effective tool for firms to learn about relevant characteristics (e.g. productivity) of different workers and/or tasks ([Meyer] 1994; [Ortega] 2001; [Li and Tian] 2013). Differently, another strand of research focuses on the incentive aspect of rotation. The general insight is that many agency problems between firms and workers can be alleviated by including job rotation as part of the organizational design (e.g. [Ickes and Samuelson] 1987; [Arya and Mittendorf] 2004; [Prescott and Townsend] 2006; [Hertzberg et al.] 2010; [Hakenes and Katolnik] 2017). The rationale for job rotation in our paper is fundamentally different from the previous studies, as the key driving force in our case is the strategic interaction between the service-providing firm and its clients. To the best of our knowledge, we are the first to advocate job rotation as an organizational remedy.

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4 However, [Subhendu et al.] (2020) recently show that a job rotation policy can also create a moral hazard in teams problem ([Holmstrom] 1982) if the firm cannot rely on incoming workers to verify the performance of their predecessors (e.g., due to the lack of hard information).
to mitigate poaching risk.\footnote{The literature has considered other remedies that employers can invoke to forestall unwanted departure of employees, such as relational contracting (Garicano and Rayo, 2017), deferred compensation (Salop and Salop, 1976; Sim, 2020) and non-competing clauses.}

There is also a literature studying how poaching affects on-the-job training (e.g., Becker, 1964; Stevens, 1994; Acemoglu, 1997; Moen and Rosén, 2004; Leuven, 2005; Gersbach and Schmutzler, 2012). In this literature, a firm can typically provide both general and job-specific skill training to its workers. It has been well understood that if the firm cannot avoid poaching from its competitors (horizontal poaching), the provision of general skill training can be insufficient. We contribute to the literature by showing that in the complementary case where the firm cannot avoid poaching from its past clients (vertical poaching), the acquisition of job-specific skill may also be distorted.

It is known that the problem of firm-sponsored general-skill provision can be alleviated by non-competing clauses (e.g., Aghion and Bolton, 1987; Levin and Tadelis, 2005; Marx et al., 2009; Naidu, 2010; Garmaise, 2011; Mukherjee and Vasconcelos, 2012; Naidu and Yuchtman, 2013; Krueger and Ashenfelter, 2018). This type of clause limits workers from leaving their current employers and work for other firms in the same industry, sometimes within a pre-specified geographic area and period. Similarly, the employers in our setting also take actions (job rotation) to hinder workers from quitting the job and working for another employer (who in this case is a past client). However, while policy makers tend to be against (horizontal) non-competing clauses (e.g., Dougherty, 2017), our paper provides both new theoretical rationale and empirical evidence for why a policy maker would be interested in doing the opposite: adopting a (vertical) non-poaching policy that can enhance productivity (e.g., better at crime prevention).

The remainder of the paper is organized as follows. In Section 2, we develop the theoretical model for analyzing talent poaching and job rotation. Followed by this we introduce the institutional setting, data and present our first empirical result in Section 3. In Section 4 we present the remaining two main empirical results. Section 5 concludes. All figures, tables, proofs and additional results are contained in the Appendices.
2 Theory

In this section, we develop a simple model to illustrate how service-providing firms can effectively contend with employee poaching from clients through strategic rotation. Specifically, we consider a risk-averse client (she) that repeatedly engages in a risky activity at period $t = 0, 1, 2, \ldots + \infty$. The client has a discount factor $\rho \in (0, 1)$, and a CARA utility function for her instantaneous payoff:

$$u(x_t) = -\frac{\exp(-ax_t)}{a},$$

where $x_t \in \mathbb{R}$ is the monetary gain/loss that the client receives in time $t$, and $a > 0$ measures her risk-aversion. Performing the activity requires a unit input of labor (of a worker, he) at every period, and it gives rise to a stochastic output $y_t \sim N(\mu(e), \sigma^2(e))$, where $\mu(e) = \mu_0 + \alpha e$, $\sigma^2(e) = \max\{\sigma_0^2 - \beta e, 0\}$, and $\mu_0, \sigma_0^2, \alpha, \beta > 0$. Here, $e \in \mathbb{N}$ is the “experience” of the worker, i.e. for how long the worker has been working for the client, and it is valuable in the sense that it both increases average output and decreases production volatility.

At the beginning, the client does not have a worker in-house, so she outsources the activity to a firm that can provide such labor service. The firm charge a per-period service fee $p > 0$, assigns a worker to the client, and bears all the monetary consequence of the activity (i.e., the stochastic output $y_t$ is completely transferred to the firm). We assume that the firm is risk-neutral, so the flow payoff it obtains by serving the client is:

$$\pi_t = p - w + y_t,$$

where $w \in (0, p)$ is the per-period wage that the firm pays to the worker. The discount factor of the firm is $\delta \in (0, 1)$.

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6We assume that the client does not recruit workers directly from the labor market. This assumption is likely to be satisfied if the firm is more efficient in screening the general skills of the workers from the labor market than the client (e.g., because the firm is more experienced or has a specialized recruiting team).

7The simplifying assumption of constant wage and service fee allows us to make it most evident how job rotation can balance the trade-off between poaching risk and client-specific skill. It is not clear that how much the firm can benefit additionally from a more flexible wage/fee schedule. For example, as wage is often quite rigid (Baker et al., 1994), the firm may end up paying the worker too much relative to his productivity if it lets the worker’s wage increase with his client-specific experience but would have to rotate him at some point (e.g., due to boredom or sick leave).
Formally, the stage game of the repeated interaction between the firm and the client goes as follows (see Figure 1 for a graphical illustration). In each period, the firm first chooses a worker to assign to the client. In particular, the firm can either send the same worker to the client as in the previous period, or appoint a new worker to perform the activity. Then, the client pays the fee $p$ to the firm if she decides to accept the service. Alternatively, the client can choose to bring the worker in-house by offering him wage $w$ (or $w + \varepsilon$ for arbitrarily small $\varepsilon > 0$). Poaching the worker will end the contractual relationship between the firm and the client, so the client will have to bear the risk associated with $y_t$ herself from then on. As a simplifying tie-breaking rule, we assume that the client will bring the worker in-house when she is indifferent between purchasing the service from the firm or not. After the client makes the poaching decision, the stage game ends and the instantaneous payoffs are collected. We are interested in the subgame perfect equilibria (SPE) of the dynamic game between the firm and the client.

To understand the poaching incentive, and also to provide a benchmark, we start by considering the scenario where the firm always sends the same worker to the client. At the beginning of period $t$, the worker have accumulated $t$ units of experience in performing the client’s activity, which we shall refer as the worker’s client-specific skill (CSS). As a result, the distribution of the worker’s output at time $t$ is $\mathcal{N}(\mu_0 + \alpha t, \max\{\sigma_0^2 - \beta t, 0\})$. Note that given the CARA-normal specification, the client’s certainty equivalent for a random output $y \sim \mathcal{N}(\mu_y, \sigma_y^2)$ is $\text{CE} = \mu_y - \frac{1}{2} \sigma_y^2$. Hence, provided that $t \leq \sigma_0^2 / \beta$, the client receives a higher instantaneous utility by hiring the worker internally than purchasing the service from the firm if and only if:

$$-p \leq -w + \mathbb{E}[y_t] - \frac{a}{2} \text{Var}(y_t) \iff t \geq T \equiv \frac{a \sigma_0^2 - 2(p + \mu_0 - w)}{2 \alpha + a \beta}. \quad (1)$$

Under the parametric assumption $\sigma_0^2 > 2(p + \mu_0 - w)/a$, which will be maintained in the rest of the section, we have $\bar{T} \in (0, \sigma_0^2 / \beta)$. It is then clear that the client would prefer to

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8For simplicity, we abstract from the consideration that the worker may further bargain with the firm or the client for his wage. More generally, it seems natural to think that client-specific experience or poaching risk should increase a worker’s bargaining power against the firm. Hence, our main insight that poaching risk can drive firms to rotate its workers excessively (see Proposition 1 below) should be robust to more general wage-determining process such as Nash (1950) or Rubinstein (1982) bargaining.

9The results are qualitatively similar if we instead assume that the poaching decision of the client is made after her transaction with the firm has been completed in the period.
bring the worker in house (and no longer transact with the firm) at time $t \geq \bar{T}$. Moreover, given that the client gets the same worker from the firm in the future if she does not poach him, it would be strictly better for her to outsource the activity and let the firm bear the risk at time $t < \bar{T}$, i.e., when the worker has not yet accumulated sufficient CSS. Hence, if the firm never rotates the worker it sends to the client (or if the rotation is not sufficiently frequent), it will only be able to collect revenue from the client for $\bar{T} < \sigma_0^2/\beta$ periods. After that, poaching takes place and the firm loses both its employee and client.

It is straightforward to check that the cutoff $\bar{T}$ is increasing in $\sigma_0^2$, and it is decreasing in $\mu_0, \alpha$ and $\beta$. This is intuitive: a worker is more desirable/productive from the client’s perspective if $\sigma_0^2$ is smaller, or if $\mu_0, \alpha$ or $\beta$ are larger. Hence, consistent with the empirical results (presented in Section 4.1), our model suggests that workers with more desirable characteristics are more prone to the poaching risk, in the sense that clients are inclined to bring them in-house earlier.

We now show that, in response to the employee poaching problem, the firm may strategically rotate its workers. To ease the exposition, we assume that $\bar{T} \in \mathbb{N}$. We first introduce the concept of rotation equilibrium.

**Definition 1.** A rotation equilibrium is a pure-strategy SPE in which the firm rotates the worker it sends to the client after every $T \geq 1$ periods, and the client always purchases the labor service from the firm. A rotation equilibrium is optimal if it maximizes the firm’s expected payoff among all rotation equilibria.

As common in repeated games, multiplicity of equilibria is difficult to rule out. However, two rotation equilibria are outcome-equivalent if they have the same frequency of rotation on the equilibrium path. Further, albeit costless, rotation destructs productivity by crippling the accumulation of CSS. Thus, it is intuitive that the firm prefers an equilibrium with least frequent rotation. This implies that the firm rotates workers more than necessary only if that can reduce employee poaching. Whether the firm can indeed retain its workers by strategically rotating them is not trivial: Anticipating that the current assigned worker will be replaced later, the client might try to bring that worker in-house earlier than what she would prefer, even if doing so may incur an instantaneous utility loss. Relying on the idea that a sufficiently impatient client would prefer carrying on the outsourcing relationship
with the firm rather than poaching a worker prematurely, our main theoretical result below establishes the existence of an optimal rotation equilibrium.

**Proposition 1.** There exists $\bar{\rho} > 0$, such that if $\rho \leq \bar{\rho}$ (i.e., the client is sufficiently impatient), then there exists an optimal rotation equilibrium, where the firm rotates the workers after every $T^* \leq \bar{T}$ periods.

All omitted proofs can be found in Appendix B. The proof of Proposition 1 reveals that in an optimal rotation equilibrium, the firm assigns a new worker to the client whenever the poaching of the preceding one is about to take place. One may further expect that more productive workers get rotated more often, since they have higher poaching risk. This is true, as we formally show in the following comparative statics result.

**Proposition 2.** The optimal frequency of rotation $1/T^*$ is increasing in $\alpha, \beta$ and $\mu_0$, and it is decreasing in $\sigma_0^2$.

To sum up, our model highlights that the strategic concern of employee poaching can lead to excessive job rotation. In particular, if employee poaching were prohibited, rotation should be merely driven by factors exogenous to our model, such as the sick leave of workers. Hence, our theory predicts that a non-poaching policy change like the one we study in this article will result in less rotation. As less rotation implies larger accumulation of CSS, the policy change can increase the total surplus generated from the transaction, leading to for instance fewer crimes as we show empirically in Section 4.

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10 In Appendix Section B.3 we provide an extension where the benefits from poaching a worker and performing the risky activity in house is privately known to the client. In that setting, we show that strategic over-rotation mitigates but does not completely eliminate poaching risk.

11 It is conceivable that in reality, firms may also counter poaching by increasing the wages of its workers. It is not clear whether this alternative measure is more cost-effective than (or to what extent it may substitute) strategic over-rotation, which actually makes it an interesting empirical question.

12 A more comprehensive analysis should take into account how workers’ effort choice (which we have not modeled) would endogenously respond to the policy change. For example, if workers exert more effort before the policy change to get poached (higher salaries, more stability, etc.), then the policy change would eliminate that incentive and thus result in a decrease in effort.
3 Data and Empirical Analysis

3.1 Institutional Setting

We have partnered with a private security firm in Bogota, Colombia. The firm provides security services to residential buildings. We have detailed 12-hours shifts data of the firm’s transactions from February 1992 to April 1998. In total, our sample consists of 628 security guards allocated to 94 buildings. For each guard, we have information on when and where he/she worked, previous work experience, age, gender and residential address. For each building, we know who worked there and when, where it is located, number of flats, required number of guards and type of crime occurred (if any).

The allocation of guards to buildings works as follows. A guard works successively for 12 days in shifts of 12 hours each: six consecutive days during the day shift (6am-6pm) and the following six days during the night shift (6pm-6am). After 12 consecutive working days, the guard rests for two days. Most guards are allocated to work in a unique building for several months. However, a fraction of guards (about 15%) are designated exclusively to cover the rest days of their colleagues. As a result, they work across multiple buildings during the 12-day period. We refer to the above two types of guards as type-I and type-II, respectively. In a given week, a building typically needs three different guards to cover all the shifts: two type-I guards and one type-II guard. A single type-II guard is sufficient to cover the resting periods of two type-I guards.

Panel A of Figure 2 illustrates a typical timetable of three guards working in the same building in a period of 16 days. The two type-I guards are labeled as e1-A and e1-B, respectively, and the type-II guard is labeled as e2. On days 7 and 8, guard e1-B rests and guard e2 covers the day shifts. On days 13 and 14, guard e1-A rests and consequently guard e2 covers the night shifts. The type-II guard e2 is also required to work 12 days in a roll before he gets to rest for two days. Hence, as Panel B of Figure 2 illustrates, guard e2 is rotated every two days to a different buildings, so that the full schedule of shifts is completed. Important for our purpose, guards are sometimes reallocated to work in a building where they have never worked before. The firm usually communicates such decision to the guard

\[^{13}\text{Some large buildings require more than one guard working at the same time because for instance they have several entrances.}\]
around a week before the rotation takes place.

Table 1 provides descriptive statistics of our database. The table summarizes a number of predetermined characteristics of the guards, including previous experience working as security guard, military training and various socioeconomic variables (gender, age, size of the household, migration status, income level of the neighborhood of where they live). Most guards are male with some military training and about half of them have some past experience working as security guards before joining the firm. There is large variation in terms of age and migration status. Guards tend to share the household with 4.5 additional family members on average and only 7% of them live alone. About 80% of the guards joined the firm just before our sample period starts.

Key variables related to the rotation of guards across buildings are also reported in Table 1. A guard spends on average a maximum of 17 months working in the same building but there is a large heterogeneity on the tenure across guards. Further, type-I guards work on average in 1.03 buildings per month and only 3% of them rotate each month. This contrasts with type-II guards who work in 2.4 different buildings each month and rotate to a new building with a 7% probability.

Finally, the bottom part of Table 1 reports summary statistics for the observable characteristics of the buildings. Buildings are relatively large with an average of 94 flats and require an average of 4.7 different guards to cover all the shifts during a month. The incidence of crime is relatively seldom, with a monthly probability of 5%. The average value of property stolen (when a crime occurs) is estimated to be 94.2 USD.

According to the firm, the allocation of guards to buildings does not follow any systematic criteria and is based on haphazard events like the need to allocate a guard to a new client, the starting day of a new guard, or the need to replace an existing guard (e.g., guards may be replaced when buildings’ administrators are not satisfied with guard’s performance.). Naturally, we remain skeptical about the allocation of guards to buildings being exogenous. Hence, we conduct a number of empirical tests to investigate the magnitude to which the match between guards and buildings can be regarded as endogenous based on the observable characteristics of the both. Specifically, for every baseline characteristic of the buildings in

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14 This approximately corresponds to 85% of the local monthly minimum wage in 1993.
our database, we take it as a dependent variable and regress it on the baseline characteristics of the guards. We perform these regressions for all guard-building pairs observed in the data, and also separately for the matches between each guard and the first building which he/she was sent to when joining the firm. The $F$ statistics for joint significance of these cross-section regressions are reported in the Appendix Table C1. We find very low $F$-statistics with only 2 out of 16 slightly above 2. These results are consistent with the narrative that the firm allocates guards to buildings exogenously to their characteristics.

3.2 Client-Specific Skills, Guards’ Productivity and Poaching

The importance of client-specific skills in the setting. One of the most important tasks of a guard is to control the entry into the building. When a visitor arrives, the guard communicates with the apartment that the visitor wants to visit to ask whether the visitor is welcome or not. If the reply is positive, the guard registers some basic information about the visitor (name, national id, time of arrival) and lets him/her in. This process takes about 5-7 minutes, and both guards and frequent visitors prefer skipping it due to transaction costs.

The best guards reduce transaction costs by recognizing residents and frequent visitors from the rest. Recognizing those residents and visitors is a client-specific skill. Naturally, this skill increases over time as guards become more familiar with the identities of those who enter and exit the building frequently. However, without sufficient experience in the building a guard may not be able to screen unwanted visitors (e.g., thefts) from others. Hence, an inexperienced guard either makes everyone pay transaction costs, or overlooks the entry of unwanted visitors.

Building-specific experience and guard’s productivity. Although we do not observe all the possible dimensions of guards’ performance (e.g., we do not observe the time costs incurred by visitors for completing the entry registration, the trust between residents and guards, etc.), we do have information on one important aspect of their productivity, namely

\footnote{Due to the limited number of predetermined characteristics we are reluctant to fully reject the possibility that endogenous matching may occur along other non-observable dimensions. However, we believe that the matching between guards and buildings is not endogenous as below we show that the results are robust even when we control for guard-building unobservable characteristics.}
the incidence of crime in a building during the shifts when a guard is on duty. In order to investigate the impact of building-specific experience on crime, we use data at the guard × shift level to estimate the following equation:

\[ \text{Crime}_{ibt} = \beta \text{LogExpInBuilding}_{ibt} + \eta \text{LogTotalExp}_{it} + \delta_{ib} + \gamma_{m(t)} + \epsilon_{ibt}, \] (2)

where \( \text{Crime}_{ibt} \) is an indicator for the occurrence of crime while guard \( i \) was working at building \( b \) during shift \( t \) (i.e. the date). We also consider an alternative dependent variable: the (log) value of property stolen if crime occurs, which we denote as \( Y_{ibt} \). Our main explanatory variable \( \text{LogExpInBuilding}_{ibt} \) is the (log) number of shifts that the guard worked in the building. Naturally, unobserved characteristics of the guard or the building can correlate with both crime and the accumulated experience of the guard in the building (e.g. smaller buildings may be easier to monitor). Moreover, as discussed previously, although Appendix Table C1 shows no correlation between guard and building characteristics, some concern may remain regarding guards and buildings being matched endogenously in some unobserved dimension. In such a case, the duration of the guard’s serving in the building and crime may depend on the match quality. For instance, young guards may be particularly good at preventing crime in small buildings, but at the same time, they may not stay there for long because the firm prefers to allocate young guards to large buildings whenever there is a vacancy. For this reason, we include pair-specific fixed effects \( \delta_{ib} \) and exploit the variation in building-specific experience within each guard-building pair over time. In order to avoid confounding the effect of building-specific experience with systematic changes in crime over time, our estimation also includes monthly fixed effects \( \gamma_{m(t)} \).

We expect that the performance of the guard increases with overall experience which mechanically correlates with the experience in the building. Therefore, we control for the overall (log) experience of the guard \( \text{LogTotalExp}_{it} \). This variable is identified separately from time fixed effects because (i) not all guards have joined the firm at the same time, and (ii) the measure also accounts for the previous working experience as security guard. We also control for potential trends in crime at the spatial level by having neighborhood interacted

\[ \text{Crime}_{ibt} = \beta \text{LogExpInBuilding}_{ibt} + \eta \text{LogTotalExp}_{it} + \delta_{ib} + \gamma_{m(t)} + \epsilon_{ibt}, \] (2)

We acknowledge the limitations of using crime as the main measure of productivity. First, it has limited variation as it is a relatively rare event. Second, a lower crime rate could be at the expenses of imposing higher transaction costs to residents and visitors. However, the firm has emphasized that crime is undoubtedly the number one priority for clients.
with month fixed effects. Other controls include the time of the shift (day/night) and the total number of shifts that the guard worked during the month.

The first column in Panels A and B of Table 2 shows the estimates of equation (2). All the coefficients of building-specific experience are negative and significant. Magnitudes are small in absolute terms but large relative to the mean of the dependent variables (0.0003 and 0.004 respectively) as the occurrence of crime is a rare event when measured at the guard-shift level. Columns (2) and (3) show that results remain almost identical when we control for narrower time fixed effects (like week and shift × day of the week). These results indicate that within a given guard-building pair, crime is reduced over time as the guard accumulates more experience in that specific building. Remarkably, we also find that the coefficient of the total experience of the guard across all buildings is non-significant in all the regressions.

Our estimates of equation (2) remain unbiased even in the presence of endogenous matching between guard’s and building’s fixed characteristics. However, there is still the concern that reverse causation (e.g. guards are removed from a building after a crime occurs) or some other type of dynamic selection of guards into buildings can bias the estimates. We address this concern by taking advantage of a distinctive feature of the organizational design. Namely, that guards are allocated to work as type-I or type-II based on a series of haphazard events. This initial allocation gives rise to variation in the building-specific experience across guards over time. Intuitively, a type-II guard will mechanically accumulate less experience in any given building compared to the type-I guards stationing there. To see this, note that in Figure 2 during the same period of time (16 days), guard e1-A accumulates 14 shifts in building 1 whereas guard e2 only accumulates 4 shifts in the same building. In Appendix Figure C1, we report a number of balance tests that support the claim that the assignment to type-I or type-II is uncorrelated with any baseline characteristic of the guard.

To exploit the aforementioned variation, we instrument the building-specific experience of the guard with the interaction between a dummy for type-II and the total number of shifts

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17 The firm has told us that clients do not provide more materials or amenities to the guards as they increase tenure of the firm. Therefore, this cannot explain the fact that crime rates decrease with larger client-specific tenure.

18 Specifically, we run regressions of baseline characteristics of the guards on a dummy indicating their types. All coefficients are small and non-significant at 5% level.
that the guard worked since he/she joined the firm. This interaction captures the lower (mechanical) accumulation of building-specific experience of the type-II guards compared to the type-I guards. The results are reported in Column (4) of Table 2, and they confirm the previous findings from OLS estimation. In fact, the estimated coefficients are not only significant but also larger in magnitude than those presented in Columns (1) - (3) of the table.\footnote{A possible interpretation of the larger coefficients from the IV estimation is that OLS estimates are downward biased due to reverse causation. In Appendix Figure C2 we report how crime evolves in the days before a guard is rotated, conditional on the baseline controls in equation (2). We do not find evidence of higher crime before rotation. This rules out that guards are rotated immediately after a crime occurs or that guards reduce their effort when they are informed about forthcoming rotation.}

The findings of Table 2 are important for two reasons. First, they show that the performance of a guard increases with the time spent in the building that he/she is paired with, even controlling for the guard’s total experience. Second, they are consistent with the idea that rotation can be inefficient as it destroys skills that positively affect productivity (i.e., prevention of crime). Despite the negative effect, our theoretical model suggests that rotation can still be beneficial for the firm if the accumulation of building-specific experience increases the poaching risk of the firm’s employees. We now proceed to provide empirical evidence for such correlation.

**Building-specific experience and observed poaching.** Given the analysis of equation (1) derived from our theoretical model (see also Proposition B.1 in the Appendix), we expect that the probability that a building attempts poaching a guard increases with the number of shifts that that guard worked in that building. With the help of the security firm, we are able to empirically test this theoretical prediction. In particular, we collected information of all cases of poaching prior to the introduction of the non-poaching policy: in total, there were 28 guards that were hired in-house by buildings that had a contractual relationship with the firm. For each of these cases, we observe the identity of the hired guard, the corresponding building and the last week of work of the guard as an employee of the partner firm. Interestingly, in all these cases the guard was hired while working in the building, but not after he/she had rotated to a different building. Therefore, it is conceivable that more poaching would have been observed if the firm had rotated the guards less frequently.

We then establish the link between poaching and building-specific experience by compar-
ing guards working in the same building during the same month (conditional on the fixed characteristics of the guards). Intuitively, we want to know if among the pool of guards working at the same time, the building prefers hiring those who have worked there for longer (i.e., the guards with more building-specific experience). This motivates us to estimate the following equation at the guard-week level:

\[ P_{\text{poached}}_{ibt} = \beta \log \text{ExpInBuilding}_{ibt} + \eta \log \text{TotalExp}_{it} + \varphi_{bm} + \eta_i + \gamma_t + \epsilon_{ibt}, \tag{3} \]

where \(P_{\text{poached}}_{ibt}\) is an indicator that takes one if guard \(i\) is hired by building \(b\) in week \(t\). We exploit the variation within building and month by controlling for the interaction fixed effect \(\varphi_{bm}\). We also include guard \((\eta_i)\) and week \((\gamma_t)\) fixed effects. Results are displayed in Table 3. All the coefficients of the building-specific experience are positive and significant. In particular, the IV results indicate that a 10% increase in the building-specific experience of a guard is associated with additional 1.2 to 1.8 percentage points in the probability of being poached by the corresponding building.\(^{20}\)

Overall, the above empirical results demonstrate that there is a strong association between the time that a guard spends in the building and the probability that he/she will be poached.

4 A Non-Poaching Policy Change

At the beginning of the 1990s, the guerrilla groups in Colombia heavily victimized the country’s civil population. As a consequence, there was a civil-led initiative that advocated private security forces to provide safety services from these terrorist groups. The Colombian government supported this initiative and, in an effort to facilitate and regulate the implementation, approved the *Decree 356 of 1994*, which mandates clients interested in acquiring any type of security services to access those services only through a company. In particular, the decree makes it clear that security companies should have a large amount of financial

\(^{20}\)This magnitude is very large if we compare for instance with the total share of guards poached during the period (0.06). A caveat here is that, as we mentioned it before, it is possible that the firm prevented some level of poaching by rotating guards beforehand. Intuitively, this would attenuate the observed relationship between building-specific experience and poaching, in which case our estimates would represent a lower bound.
assets, which *de facto* limits the possibility that one guard become an in-house worker establishing a company by herself. As a consequence, the introduction of the new law inhibited buildings from hiring guards directly.

We use the above policy change to provide evidence for the central mechanism highlighted by our theoretical model. Crucially, if the security company rotates guards with the aim of trading off client-specific productivity and poaching risk, our theory predicts that this rotation decreases once the law takes effect. Indeed, after the decree was introduced, the unconditional probability that a guard rotates in a given month dropped from 4% to 2%. However, this before-after comparison can be misleading due to time confounding factors.

In the absence of an exogenous control group, we tackle this issue by comparing the change in rotation across guards that had different probabilities of being poached before the policy change. Intuitively, some guards have baseline characteristics that made it more attractive for buildings to hire them directly. According to Proposition 2, the firm should react to this differential exposure of poaching risk by assigning more attractive guards to more frequent rotating schedules (before the policy change). Therefore, to further validate the proposed theoretical mechanism, we shall examine whether the frequency of rotation dropped more for those guards more likely to be poached once the decree came into effect.

### 4.1 Poaching Risk: Machine Learning Estimation

We start by estimating the probability that a guard is poached. We focus our analysis on type-I guards who, according to both the company and our data, are much more exposed to the risk of poaching than type-II guards.\(^{21}\) Naturally, using time-dependent explanatory variables (e.g., building-specific experience or crime occurrence) is problematic as they are likely correlated with both the rotation decisions of the firm and the poaching decisions of the buildings. Instead, we estimate the relationship between observed poaching and the predetermined characteristics of the guard. For instance, as the firm communicated to us, guards living in large households are often more attractive to buildings, because in

\[\text{During our period of analysis, all poaching episodes observed in the data involved type-I guards. Our conversations with the firm also confirm that it was mainly concerned about the poaching of type-I guards. By contrast, poaching of a type-II guard was perceived as a very unlikely event. This seems natural because, by design, a type-II guard rotates across different buildings and his/her scheduling depends on factors such as the absence of another guard due to illness or leave.}\]
case of illness they can more easily find a household member to cover the shift. Overall, the explanatory variables we include are the guard’s age, gender, socioeconomic strata and neighborhood of residence, immigration history, military training, working experience before joining the firm, and size of the household.

We face two main challenges with this approach. Firstly, the total number of guards poached by buildings is small. Secondly, given that the firm would rotate guards to prevent poaching, we only observe an attenuated relation between the guards’ characteristics and poaching. The lack of variation makes it difficult to detect empirically which characteristics are more important for the attractiveness of the guards to the buildings. Moreover, it is possible that interactions between characteristics are critical predictors of poaching (e.g. having military training matters only for young guards).

To address these issues, we first augment the poaching episodes with information provided by the firm about guards receiving solicitation from buildings: A guard is solicited if a building formally expresses interest (through communication with the firm) in hiring that guard directly. We find that among the 34 guards that were solicited, 14 were also poached (14 guards were hired but not previously solicited). Therefore we consider solicitation as an informative signal about buildings’ preferences for deciding which guards to poach. We then estimate a cross-section Random Forest model, where the dependent variable is a dummy taking one if the guard was poached or solicited. This machine learning technique allows for a high sensitivity (i.e., it is better at detecting which variables are most relevant for poaching) and accounts for interactions and non-linearities among explanatory variables without running into over-fitting problems. We summarize the results of the estimation in Appendix Table C2. In addition to the estimated coefficients of the regression, the table reports the Gini Importance or the mean decrease in the Gini Impurity, which measures the relative importance of each variable in predicting the poaching risk (i.e., its contribution to

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22 Note however that Proposition B.1 shows that in general, the firm does not completely offset the poaching risk across guards. In other words, the result suggests that guard characteristics and occurred poaching should still be correlated in equilibrium.

23 In Appendix Table C3 we conduct a robustness check by excluding the solicited guards from the estimation of the risk of poaching. Our baseline findings are robust to this exclusion.

24 To prevent over-fitting, the estimation uses bootstrap aggregation with the standard rule of limiting the number of splits at each step by the squared root of the number of explanatory variables. We also use an asymmetric Gini loss function to deal with the imbalanced data problem (see Domingos 1999; Pazzani et al. 1994).
reducing the loss function across all trees). Notably, age, household composition, previous experience and immigration history are identified as the most relevant dimensions to explain that a guard is hired directly/solicited by a building.

4.2 Rotation of Guards due to Poaching Risk

We measure rotation with a dummy that takes the value 1 if the guard is reallocated to work in a new building during the month and 0 otherwise. As an alternative, we also calculate the average number of shifts per building that the guard worked during the month. The latter measure is more sensible to differences in the timing of rotation within the month, for instance it is unaffected if rotation always takes place on the first day of the month.

Table 4 confirms that prior to the policy change, the firm rotated more often those guards associated with a higher risk of being poached. Specifically, we regress the monthly measure of rotation on the estimated risk of poaching for the year prior to the policy introduction, controlling for time variant characteristics of the guard (e.g., the guard’s tenure within the firm and the total number of days the guard worked in the month) as well as month fixed effects. We find that a one standard deviation increase in the estimated risk of poaching is associated with 1.5 additional percentage points in the probability of rotation. This is equivalent to 40% of the monthly average rotation rate in the year before the policy change. In a similar vein, the correlation with the average number of shifts per building is negative and highly significant, although small in absolute magnitude (one standard deviation increase in the probability of poaching reduces the average shifts per building by 0.2).

Since the variable capturing the risk of poaching is a generated regressor, standard errors do not account for its full sampling variation. We address this concern by bootstrapping the whole two-step procedure. In each bootstrap sample, we re-estimate the Random Forest model and the main regression. We report bootstrapped standard errors in all regressions where the variable measuring the risk of poaching is part of the regressors. Table 4 shows that bootstrapped standard errors are only slightly larger compared to the baseline estimates.
4.3 Estimating the Effect of the Policy on Rotation

The risk of employee poaching drop substantially after the introduction of the decree in 1994. In fact, no poaching episode is observed in the data after the policy took effect. To investigate how this further affected the rotation of the guards, particularly those associated with a higher risk of poaching before the policy change, we estimate the following Diff-in-Diff specification at the guard-month level:

$$\text{Rotation}_{it} = \beta \text{RiskPoaching}_i \times \text{After}_t + \phi X_{it} + \eta_i + \gamma_t + \theta_i \times t + \delta_{bi(t)} + \epsilon_{it}$$ (4)

where the dependent variable measures the rotation of guard $i$ during month $t$. The effect of the policy ($\beta$) is identified from the interaction between the estimated risk of poaching and a dummy taking one for the periods after the policy change. Our estimation includes time varying controls of the guards ($X_{it}$) like the number of days worked during the month and the tenure within the firm. We absorb any permanent difference in rotation levels across guards by including guard-fixed effects ($\eta_i$), and account for time aggregated variation by including month fixed effects ($\gamma_t$). A concern remains that guards are initially allocated to rotation schedules that increase or decrease over time at different rates (for instance, rotation may be reduced faster for guards from certain localities or for guards joining at an older age). For this reason we further allow for guard-specific linear trends ($\theta_i \times t$), so that the effect of the policy besides any secular change over time can be identified. Finally, we also control for changes in rotation due to differences between buildings where the guard works by including fixed effects for the building where the guard completed most shifts during the month ($\delta_{bi(t)}$).

Table 5 reports the estimates of equation (4), which includes the standard errors obtained from bootstrapping the estimations of poaching risk and equation (4) altogether (samples clustered at the guard level). Note that our identification strategy assumes that guards with different probabilities of being poached are initially assigned to different rotation schedules (which we allow to diverge linearly over time) and that no other shock contemporaneous

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25In a given month, a type-I guard works in more than one building only if he/she is rotated. Thus, including dummies for every building where the guard worked during the month (instead of just the one where the guard spent most time) will result in perfectly collinearity with our main rotation measure. As a robustness check, in Appendix Table C4 we repeat the main analysis at the guard-week level. This allows us to absorb the full set of building-fixed effects, because type-I guards only work in one building each week. Results are significant and similar in magnitude once we scale up the coefficients to monthly equivalent units.
with the policy change affected the relative rotation of workers with higher poaching risk. We partially test these assumptions by introducing lead terms to equation (4), which allows us to reject the existence of pre-trends in the rotation of guards with different poaching risk.

Figure 3 depicts the leads and lags of \( \text{RiskPoaching}_i \times After_t \) by quarter relative to the date when the decree was introduced. The plotted estimates show no evidence of pre-trends in rotation but a sharp decrease in the rotation of guards with high probabilities of being poached. Further, the (monthly) average probability of rotation of guards above the median poaching risk decreased by 4 percentage points relative to guards below the median poaching risk. Similarly, guards above the median poaching risk experienced a relative increase of 0.6 shifts per building (2.5% in proportion to the average number of shifts per building).

### 4.4 The Effect of the Policy on Crime

The main insight of the theoretical model is that a firm may deliberately forgo potential productivity gains and *excessively* rotate workers in the presence of poaching risk, which can constraint the surplus generated from the firm-client relationship. In this sense, an important implication of non-poaching policies is that they may increase the productivity of workers by preventing strategic destruction of client-specific human capital.

We have provided evidence that reducing the risk of poaching reduces rotation. We now investigate whether the lower rotation rate is also associated with an increase in our measures of productivity, namely a decrease in crime rates and the value of property stolen.

We first estimate an equation where the dependent variable is the number of crimes occurred while the guard was on duty during the month, and the explanatory variables are the same as in (4). The results are reported in Column (3) of Table 5. The estimated effect of rotation on crime, albeit statistically non-significant, is negative and large relative to the mean number of crimes: guards above the median poaching risk reduced the number of crimes by 0.006 on average, almost 65% in proportion to the mean number of crime per guard/month.

To attain a higher statistical power in our data, we alternatively study the change in crime occurrence at the building level. We exploit the fact that we observe a large heterogeneity across buildings in the average poaching risk of the associated guards at the time when
the Decree 356 was introduced. As we have shown in Appendix Table [C1], this variation is unlikely to be related to building’s characteristics. At the same time, intuition suggests that those buildings with a larger proportion of high-poaching-risk guards should benefit more from the policy change, because the associated decrease in rotation rate is larger for the guards working there.

Relying on the above identification strategy, we provide more definitive empirical evidence that rotation mediated the effect of the policy change on crime. Since the importance of the initial composition of a guard’s poaching risk naturally dissipates over time, we focus our analysis on a window of 6 quarters around the policy change. This is also the period for which we observe the highest correlation between the average poaching risk of guards and the frequency of rotation at the building level. More formally, we regress our main rotation measure at the building-month level (calculated as the monthly share of guards assigned to work in the building for the first time) on the interaction between a dummy taking one for the periods after the policy change and the average poaching risk of the guards worked in the building just before the policy change. The regression controls for building fixed effect and neighborhood-specific linear trends. As reported in Column (1) of Panel A in Table 6, the estimated coefficient is negative and highly significant, confirming the results we found at the individual guard level.

Columns (2) and (3) of Panel A in Table 6 use the same variation to estimate the reduced form effect of the policy change on crime at the building level. Both the number of crimes and the value of property lost due to crime significantly drop in buildings with a higher share of high-risk guards. For instance, buildings with average poaching risk among guards above the median experienced a relative drop of 0.11 crimes per month compared to those below the median. Despite the statistical significance, a potential concern interpreting these results is that rotation can change the composition of guards’ characteristics, which in turn can affect crime. Nevertheless, the effect of this channel is unlikely to impact our estimation as we controlled for the average characteristics of the guards working in the building during the month, including both fixed characteristics like the estimated poaching risk and time variant ones like tenure within the firm.

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26 Extending the period of analysis gives us significant but weaker results, hindering the instrumental variable exercise described later in this section.
Finally, we perform an IV estimation to corroborate the reduced-form results. Specifically, we instrument the rotation measure at the building-month level with the interaction between the average poaching risk of the guards worked in the building just before the policy change and a dummy taking one for the periods after the policy change. In other words, the regression in Column (1) of Panel A becomes the first stage of the reduced-form estimations in Columns (2) and (3). The results of this exercise are reported in Panel B of Table 6. The estimates indicate that increasing rotation by 10 percentage points raises both the number of crimes by 0.19 and the value of property stolen by 1 USD per building-month.  

Taken all together, the results of this section provide evidence consistent with the key predictions of our theory in the current empirical setting: (i) a sharp drop in rotation after the policy change due to the lower risk that buildings poach guards, and (ii) a consequent reduction in crime due to guards being rotated less frequently.

**Remark.** An alternative interpretation of our empirical findings is that the policy modified the incentives that the guards have to exert effort at the job. In particular, if some guards prefer to work in-house and given that the Decree limited this possibility, guards have less incentives to exert effort after the policy change. This in turn would imply that crime rate should increase after the Decree took effect. However, Table 6 shows the opposite. Hence, under this interpretation our results should be read as the potential lower bound of what the extra accumulation of CSS can do in crime rates.

5 Conclusion

In this article, we have made a first step in understanding an important issue of the service industries. Namely, how firms respond to the threat that the workers they allocate to provide services may be poached by clients.

Using detailed data from a firm operating in the security-service industry, we have shown

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27 A main caveat of this exercise is that the first stage is not strong enough to reject the hypothesis of a weak instrument. Therefore, when bootstrapping the whole procedure (i.e., the estimation of the poaching risk and the two stages of the IV regression) we usually get a few extremely large second stage estimates due to samples where the first stage is powerless. This translates into large bootstrapped standard errors even when this only happens to a small number of sub-samples. In Table 6 we report standard errors for the bootstrap subsamples where the first-stage F statistics is above one. Further increasing this threshold substantially reduces standard errors.
that the building-specific experience of a security guard decreases crime even after controlling for the guard’s total experience. As the ability to prevent crime is desirable from the buildings’ perspective, the risk that a guard may be poached by a building is also increasing in that guard’s working experience in that specific building. Anticipating the association between building-specific experience and poaching, the security firm strategically rotates its workers, at a level exceeding the one that it would choose if poaching was forbidden. The empirical analysis confirms that this was indeed what happened after a non-poaching policy came into effect.

We have also shown that the policy change reduced crime rates, suggesting that prohibiting talent poaching can have a positive effect on welfare. However, one has to be cautious in jumping to the conclusion that the non-poaching policy unambiguously increases welfare. For instance, a worker might derive intrinsic utilities from working as an in-house employee of the client, and an in-house relationship might also lead to a higher total surplus in the long run. Hence, policy makers contemplating a non-poaching policy change should consider a more comprehensive cost-benefit analysis.

As suggested by the theoretical model, the mechanism studied in this paper has broad applicability to both low- and high-skill service occupations, provided that the productivity of the outsourcing worker (or the surplus generated from the service transaction) depends significantly on the worker’s client-specific experience. Examples involving low-skill occupations include cleaning companies allocating janitors to clients who are seeking maintenance of properties, catering service firms sending waiters/waitresses to clients who are organizing gala events, or worker associations providing labor service to clients who are running fast-food restaurant franchises, etc. We think that the logic exposed here also applies to high-skill occupations such as outsourcing of legal, lobbying, IT and consulting services. We find particularly interesting to empirically test the ideas exposed here in different occupational settings.

There are however some occupations that do not seem to be represented well by the logic studied here. For instance, vertical poaching is unlikely to be a serious concern for companies providing services of tailors, flight attendants, or travel agents, among others.
References


A Main Figures and Tables

**Figure 1:** Timing of the Stage Game

<table>
<thead>
<tr>
<th>t</th>
<th>Firm sends out either the same or a new worker</th>
<th>Client decides whether to poach the worker</th>
<th>Yes</th>
<th>No</th>
</tr>
</thead>
<tbody>
<tr>
<td>t+1</td>
<td>The contractual interaction b/w Firm and Client ends</td>
<td>Client pays wage (w) and hires the worker to perform the activity internally</td>
<td>Client pays fee (p) and outsources the activity to the firm</td>
<td></td>
</tr>
</tbody>
</table>

**Panel A: Example of shift schedule of three guards in a given building**

<table>
<thead>
<tr>
<th>Building</th>
<th>Shift</th>
<th>Week 1</th>
<th>Week 2</th>
<th>Week 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Day (6am-6pm)</td>
<td>e1-A e1-A e1-A e1-A e1-A e1-A e2</td>
<td>e2 e1-B e1-B e1-B e1-B e1-B e1-B e1-B e1-B e1-B e1-A e1-A e1-A e1-A e2 e2 e1-B e1-B</td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Night (6pm-6am)</td>
<td>e1-B e1-B e1-B e1-B e1-B e1-B e1-A</td>
<td>e1-A e1-A e1-A e1-A e1-A e1-A e1-A e1-A e2 e2</td>
<td></td>
</tr>
</tbody>
</table>

**Panel B: Example of a 12-day working period for a type-II guard**

<table>
<thead>
<tr>
<th>Building</th>
<th>Shift</th>
<th>Week 1</th>
<th>Week 2</th>
<th>Week 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Day</td>
<td>e2</td>
<td>e2</td>
<td>e2</td>
</tr>
<tr>
<td>1</td>
<td>Night</td>
<td>e2</td>
<td>e2</td>
<td>e2</td>
</tr>
<tr>
<td>2</td>
<td>Day</td>
<td>e2</td>
<td>e2</td>
<td>e2</td>
</tr>
<tr>
<td>2</td>
<td>Night</td>
<td>e2</td>
<td>e2</td>
<td>e2</td>
</tr>
<tr>
<td>3</td>
<td>Day</td>
<td>e2</td>
<td>e2</td>
<td>e2</td>
</tr>
<tr>
<td>3</td>
<td>Night</td>
<td>e2</td>
<td>e2</td>
<td>e2</td>
</tr>
</tbody>
</table>

This figure shows an example of the allocation of guards to buildings in a period of 16 days. Panel A displays the timetable for a given building allocated with three guards. The two type-I guards are labeled as e1-A and e1-B, and the type-II guard is labeled as e2. Panel B provides the full shift schedule of the type-II guard during the same period of time.

**Figure 2:** Example of Guards’ Shift Schedule
This figure displays the estimated coefficients and the 95% confidence intervals of interaction between a guard’s rotation schedule and risk of being poached by a building, with leads and lags indicators relative to the quarter when the degree was introduced. The omitted category is the interaction with the three-month periods around the law introduction. The dependent variable in Panel A (left) is an indicator for whether the guard worked at more than one building during the month. In Panel B (right), the dependent variable is the average number of shifts per building worked by the guard during the month. All regressions control for guard and month fixed effects and guard-specific linear trends. Additional controls include the total number of days that the guard worked during the month. Observations are at the guard-month level. Standard errors are multi-way clustered at the guard-month level. N = 15,313.

**Figure 3:** Effects of the Decree 356 on the Rotation of Guards
### Table 1: Characteristics of Guards and Buildings

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Sd</td>
<td>Min</td>
<td>Max</td>
</tr>
<tr>
<td><strong>Guard Characteristics</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N of guards</td>
<td>628</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Type-I guard</td>
<td>0.88</td>
<td>0.33</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Male</td>
<td>0.92</td>
<td>0.28</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Military experience</td>
<td>0.67</td>
<td>0.47</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Neighborhood strata</td>
<td>1.90</td>
<td>0.58</td>
<td>1</td>
<td>5</td>
</tr>
<tr>
<td>Household size</td>
<td>5.57</td>
<td>3.39</td>
<td>0</td>
<td>11</td>
</tr>
<tr>
<td>Lives alone</td>
<td>0.07</td>
<td>0.25</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Age</td>
<td>36.74</td>
<td>9.38</td>
<td>20</td>
<td>71</td>
</tr>
<tr>
<td>Past experience as guard</td>
<td>32.41</td>
<td>52.55</td>
<td>0</td>
<td>285</td>
</tr>
<tr>
<td>Has experience as guard</td>
<td>0.48</td>
<td>0.50</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Tenure (months)</td>
<td>25.47</td>
<td>18.14</td>
<td>0</td>
<td>65</td>
</tr>
<tr>
<td>Immigrant</td>
<td>0.41</td>
<td>0.49</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Recent immigrant</td>
<td>0.70</td>
<td>0.46</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Started job before January 1992</td>
<td>0.79</td>
<td>0.41</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>N of shifts worked in the month</td>
<td>24.67</td>
<td>4.95</td>
<td>1</td>
<td>29</td>
</tr>
<tr>
<td>Max tenure in the building (in months)</td>
<td>17.23</td>
<td>18.07</td>
<td>0</td>
<td>65</td>
</tr>
<tr>
<td>N of buildings per month (Type-I)</td>
<td>1.03</td>
<td>0.16</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>N of buildings per month (Type-II)</td>
<td>2.41</td>
<td>0.77</td>
<td>1</td>
<td>5</td>
</tr>
<tr>
<td>Rotated to a new building during the month (Type-I)</td>
<td>0.03</td>
<td>0.16</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Rotated to a new building during the month (Type-II)</td>
<td>0.07</td>
<td>0.26</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td><strong>Building Characteristics</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N of buildings</td>
<td>94</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N of guards</td>
<td>4.73</td>
<td>2.72</td>
<td>3</td>
<td>14</td>
</tr>
<tr>
<td>N of flats</td>
<td>94.22</td>
<td>55.99</td>
<td>20</td>
<td>299</td>
</tr>
<tr>
<td>Neighborhood strata</td>
<td>2.85</td>
<td>1.30</td>
<td>1</td>
<td>6</td>
</tr>
<tr>
<td>N of crimes per month in the building</td>
<td>0.05</td>
<td>0.39</td>
<td>0</td>
<td>8</td>
</tr>
<tr>
<td>Value of property lost (USD)</td>
<td>2.01</td>
<td>45.76</td>
<td>0</td>
<td>1,857</td>
</tr>
<tr>
<td>Value of property lost (USD) if crime occurs</td>
<td>94.28</td>
<td>298.86</td>
<td>0</td>
<td>1,857</td>
</tr>
</tbody>
</table>

This table reports summary statistics for 628 guards and 94 buildings. For each of the variables we present the mean, standard deviation, minimum and maximum values. The table has two panels. The panel above presents the statistics for guards (i.e., each observation is a guard) while the panel below presents statistics for buildings (i.e., each observation is a building). Military experience is a dummy that takes the value of 1 if the guard was in the Colombian army before being hired as guard and 0 otherwise. Neighborhood strata is the socioeconomic level of the neighborhood of the guard. A larger number means a higher average-income neighborhood. Household size is the number of people living with the guard besides the guard (minimum value of this variable is 0). Lives alone is a dummy that takes the value of 1 if the guard lives alone. The dummy Has experience as guard takes the value 1 if the guard worked as a guard for another company before being hired by our partner firm. Tenure is the number of months that the guard has been with the partner firm. The main difference between the variables Immigrant and Recent immigrant is that the latter is a category for immigrants that have migrated to Bogota less than 10 years before while immigrant is for any person that has migrated to Bogota. Max tenure in the building is defined as the maximum number of months that the guard worked in a building over the whole sample. We report the average across all the buildings where the guard worked in the entire period of study. The last four variables for the guards present statistics on the number of buildings in a month and whether or not a guard was rotated to a new building for both type-I and type-II guards. The panel below present statistics for the number of guards needed in the building, number of crimes in a month and value of property lost (unconditional and conditional on witnessing a crime).
Table 2: Productivity and Client-Specific Experience

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Panel A: Crime Occurred During Guard’s Shift</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log Experience in Building ($\div 100$)</td>
<td>-.013**</td>
<td>-.013**</td>
<td>-.01*</td>
<td>-.036*</td>
</tr>
<tr>
<td></td>
<td>(.0056)</td>
<td>(.0056)</td>
<td>(.0056)</td>
<td>(.021)</td>
</tr>
<tr>
<td>Log Total Experience ($\div 100$)</td>
<td>.0061</td>
<td>.0061</td>
<td>.0077</td>
<td>.015</td>
</tr>
<tr>
<td></td>
<td>(.0097)</td>
<td>(.0097)</td>
<td>(.01)</td>
<td>(.013)</td>
</tr>
<tr>
<td>N</td>
<td>656,438</td>
<td>656,438</td>
<td>656,438</td>
<td>656,438</td>
</tr>
</tbody>
</table>

Panel B: Log Value of Property Lost in Crime

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Log Experience in Building ($\div 100$)</td>
<td>-.17**</td>
<td>-.15**</td>
<td>-.15**</td>
<td>-.48*</td>
</tr>
<tr>
<td></td>
<td>(.073)</td>
<td>(.073)</td>
<td>(.073)</td>
<td>(.27)</td>
</tr>
<tr>
<td>Log Total Experience ($\div 100$)</td>
<td>.087</td>
<td>.099</td>
<td>.099</td>
<td>.21</td>
</tr>
<tr>
<td></td>
<td>(.13)</td>
<td>(.14)</td>
<td>(.14)</td>
<td>(.17)</td>
</tr>
<tr>
<td>N</td>
<td>656,438</td>
<td>656,438</td>
<td>656,438</td>
<td>656,438</td>
</tr>
</tbody>
</table>

Method: OLS OLS OLS IV
Guard × Building FE: YES YES YES YES
Shift FE: YES YES YES YES
Month FE: YES NO NO NO
Building Neighb × Month FE: YES YES YES YES
Week FE: NO YES YES YES
Shift × Day of Week FE: NO NO YES YES

All regressions are at the guard × shift level. In Panel A, the dependent variable is an indicator for the occurrence of crime during the shift of the guard in the building. In Panel B, the dependent variable is the (log) estimated value of the property stolen or destroyed due to the crime. All regressions control for the number of shifts that the guard worked during the month. In Column (4), the building-specific experience of the guard is instrumented with the interaction between an indicator for the guard’s type (I or II) and the tenure of the guard within the firm. Robust standard errors are clustered two-ways at the guard level and at the week level. First-stage F statistics is 632.27. Experience variables are divided by 100 (i.e. coefficients are scaled up by 100).
Table 3: Poaching and Client-Specific Experience

Guard Hired by Building (Pre-Law)

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Experience in Building (÷ 100)</td>
<td>.17***</td>
<td>1.2***</td>
<td>.23***</td>
<td>1.8***</td>
</tr>
<tr>
<td></td>
<td>(.051)</td>
<td>(.35)</td>
<td>(.062)</td>
<td>(.42)</td>
</tr>
<tr>
<td>Log Total Experience (÷ 100)</td>
<td>.11</td>
<td>-.066</td>
<td>.19**</td>
<td>-.059</td>
</tr>
<tr>
<td></td>
<td>(.067)</td>
<td>(.051)</td>
<td>(.082)</td>
<td>(.06)</td>
</tr>
<tr>
<td>N</td>
<td>40,099</td>
<td>40,099</td>
<td>40,099</td>
<td>40,099</td>
</tr>
<tr>
<td>F (first-stage)</td>
<td>390.19</td>
<td>389.72</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Method: OLS IV OLS IV
Guard FE: YES YES YES YES
Week FE: YES YES YES YES
Building × Month FE: YES YES NO NO
Building × Week FE: NO NO YES YES

All regressions are at the guard/building × week level. The dependent variable is an indicator for the week when the guard is hired in-house by the building and the sample is restricted to the period before the the non-poaching policy change. All regressions control for the number of shifts that the guard worked during the month. In Column (4), the building-specific experience of the guard is instrumented with the interaction between an indicator for the guard’s type (I or II) and the tenure of the guard within the firm. Robust standard errors are clustered two-ways at the guard level and at the week level. Experience variables are divided by 100 (i.e. coefficients are scaled up by 100).
This table investigates the relationship between the estimated probability of being hired by a building and the rotation of the guard before the policy introduction. In Column (1), the dependent variable is an indicator of whether the guard was rotated to a new building during the month. In Column (2), the dependent variable is the average number of shifts per building the guard worked during the month. Each regression controls for month fixed effects, the total number of days the guard worked during the month, indicators for the starting week of the guard and average characteristics of the buildings where the guard worked during the month. Regressions are at the guard-month level and the sample is restricted to the year before the policy change. Robust standard errors are clustered at the guard level. The square brackets report the standard errors of the corresponding coefficients obtained from 200 bootstrap repetitions of the whole two-step procedure (i.e., the estimation of poaching probability and the main regression).
### Table 5: Effect of the Policy on Guards’s Rotation and Crime

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Rotated per Building</td>
<td>Avg Shifts per Building</td>
<td>N Crimes</td>
</tr>
<tr>
<td>Post × Poaching Risk</td>
<td>-.15***</td>
<td>2***</td>
<td>-.0056</td>
</tr>
<tr>
<td></td>
<td>(.038)</td>
<td>(.54)</td>
<td>(.028)</td>
</tr>
<tr>
<td></td>
<td>[.064]</td>
<td>[.877]</td>
<td>[.029]</td>
</tr>
<tr>
<td>N</td>
<td>14,708</td>
<td>14,708</td>
<td>14,708</td>
</tr>
</tbody>
</table>

Indiv Chars: YES
Month FE: YES
Guard FE: YES
Guard Trends: YES
Building (most worked) FE: YES

This table investigates the effects of the introduction of the decree on guards’ rotation (using two different measures) and crime. Each column reports the coefficient of the interaction between an indicator for the period after the law was introduced and the estimated probability that the guard is poached by a building. In Column (1), the dependent variable is an indicator of whether the guard was rotated to a new building during the month. In Column (2), the dependent variable is the the average number of shifts per building worked by the guard during the month. In Column (3), the dependent variable is the total number of crimes occurred during the shifts worked by the guard in the month. All regressions use observations at the guard-month level, and include fixed effects of guard, month and the building where the guard worked most time during the month. Additionally, all regressions include guard-specific linear trends and control for the total number of days the guard worked during the month and the log-experience of the guard. Robust standard errors are clustered two-ways at the guard-month level and are shown in parenthesis. The square brackets report the standard error of the corresponding coefficient obtained by 200 bootstrap repetitions of the whole two-step procedure (i.e., the estimation of the poaching probability and the main regression).
Table 6: Effect of the Policy on Buildings’ Crime Measures

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>(1) Share of New Guards</th>
<th>(2) N Crimes per Month</th>
<th>(3) Value of Property Lost</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Reduced Form Results</td>
<td>Post × Avg Poaching Risk at Time of Law Introduction</td>
<td>-.22*** (.07)</td>
<td>-.41*** (.13)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>[.11]</td>
<td>[.19]</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2,465</td>
<td>2,465</td>
</tr>
<tr>
<td></td>
<td>F</td>
<td>9.09</td>
<td>10.19</td>
</tr>
<tr>
<td>Panel B: IV Results</td>
<td>Share of New Guards (Rotation)</td>
<td>1.9** (.83)</td>
<td>10* (5.9)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>[1.54]</td>
<td>[11.49]</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2,465</td>
<td>2,461</td>
</tr>
<tr>
<td></td>
<td>Month FE: YES YES YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td></td>
<td>Building FE: YES YES YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td></td>
<td>Neighbourhood Trends: YES YES YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td></td>
<td>Avg Chars of Guards: YES YES YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

This table investigates the effects of the introduction of the decree on crime measured at the building level. In Column (1), the dependent variable is the share of guards that worked for the first time in the building in the corresponding month. In Column (2), the dependent variable is the total number of crimes occurred in the building during the month. In Column (3), the dependent variable is the value of the property stolen during the month (in 2010 USD). Regressions use observations at the building-month level. Panel A reports the coefficient of the interaction between a dummy taking one for the periods after the policy change and the average poaching risk of the guards worked in the building just before the policy change. Panel B shows the IV results where the independent variable is the share of guards that worked for the first time in the building during the month. The excluded instrument corresponds to the dependent variable in Panel A. All regressions include fixed effects of building and month, and the total number of guards worked in the building during the month. Additionally, all regressions control for neighborhood-specific linear trends and the average baseline characteristics of all guards worked in the building during the month. Sample is restricted to the 6 quarters around the policy change. Robust standard errors are clustered at the building level. The square brackets report the standard error of the corresponding coefficient obtained by 200 bootstrap repetitions of the whole two-step procedure (i.e., the estimation of the poaching probability and the main regression). In Panel B, bootstrap standard error is conditional on samples with a first-stage F statistics larger than one.
B Proofs and Additional Results

B.1 Proof of Proposition 1

We first introduce some formal notations. Let \( r_t \in \mathbb{R} \equiv \{0, 1\} \) be the action taken by the firm in the period-\( t \) stage game: if \( r_t = 0 \), the firm sends the same worker to the client as in period \( t - 1 \), while \( r_t = 1 \) means that the firm sends a new worker. We adopt the convention \( r_0 = 1 \). Next, let \( d_t \in \mathbb{D} \equiv \{0, 1\} \) denote whether the client decides to poach the worker assigned to her in period \( t \) \( (d_t = 1) \) or not \( (d_t = 0) \). Because the game ends whenever \( d_t = 1 \), we can simplify by leaving the past actions of the client \( d_0, \ldots, d_{t-1} \) out of the histories prior to time \( t \): \( h_0 = \emptyset \), and \( h_t = \{r_0, \ldots, r_{t-1}\} \forall t \geq 1 \). Finally, we let \( H_t \) be the set of all possible histories at the beginning of period \( t \) (provided that the game has not ended by then), and define \( H = \bigcup_{t=0}^{+\infty} H_t \).

We are now ready for the formal analysis. As a first step, we derive a necessary condition for equilibrium existence. Suppose that there exists an equilibrium in which the firm rotates its workers after every \( T \geq 1 \) periods, and the client never captures the worker assigned to her. For this outcome to arise in an SPE, the following incentive constraint must hold for the client:

\[
\sum_{t=T-1}^{+\infty} \rho^{t-(T-1)} (u(-p) - \mathbb{E}[u(y_t - w)]) > 0,
\]

where \( y_t \) is normally distributed with mean \( \mu_0 + \alpha t \), and variance \( \max\{\sigma_0^2 - \beta t, 0\} \). In other words, after a worker has performed the risky activity for the client for \( T - 1 \) periods and right before he will be rotated, the client should not find it profitable to deviate from the rotation equilibrium to bring that worker in house. The constraint cannot be satisfied if \( T > \bar{T} \), because in that case it follows from (1) that all summands in (B.1) are negative. Hence, in any rotation equilibrium, we must have \( T \leq \bar{T} \).

Next, we claim that whenever \( \rho \) is sufficiently small, there exists a unique \( T^* \leq \bar{T} \), such that (B.1) holds if and only if \( T < T^* \). To show this, let us rewrite (B.1) as follows:

\[
\sum_{t=T-1}^{T} \rho^{t-(T-1)} (u(-p) - \mathbb{E}[u(y_t - w)]) > \sum_{t=T+1}^{+\infty} \rho^{t-(T-1)} (\mathbb{E}[u(y_t - w)] - u(-p)).
\]

Note that for any fixed \( T \in \{1, \ldots, \bar{T}\} \), as \( \rho \to 0 \) the RHS of (B.2) goes to 0, while the LHS of (B.2) converges to \( u(-p) - \mathbb{E}[u(y_{T-1} - w)] > 0 \). Hence, the inequality (B.2) holds whenever
\( \rho \) is sufficiently small. Moreover, for all \( \rho > 0 \), (B.2) is further equivalent to

\[
\sum_{t=T-1}^{T} \rho^t (u(-p) - \mathbb{E}[u(y_t - w)]) > \sum_{t=T+1}^{+\infty} \rho^t (\mathbb{E}[u(y_t - w)] - u(-p)) .
\] (B.3)

Note that the RHS (LHS) of (B.3) is decreasing in (independent of) \( T \). Hence, if (B.3) is satisfied for some \( T \), it will also be satisfied for all \( \tilde{T} \leq T \). In sum, for a fixed and sufficiently small \( \rho > 0 \), there must exist a unique cutoff \( T^* \in \{1, ..., \tilde{T}\} \), such that (B.1) holds if and only if \( T < T^* \).

From now on, we assume that \( \rho \) is sufficiently small so that the above cutoff \( T^* \) exists. We further argue that a rotation equilibrium with \( T = T^* \) exists. Adopting the convention \( r_s = 1 \) if \( s < 0 \), we consider the behavioral strategy of the firm \( r^*: \mathcal{H} \to \mathcal{R} \), where

\[
r^*(h_t) = \begin{cases} 
1 & \text{if } \sum_{s=(t-1)-T^*}^{t-1} r_s = 0 \\
0 & \text{otherwise}
\end{cases}
\]

and the behavioral strategy of the client \( d^*: \mathcal{H} \times \mathcal{R} \to \mathcal{D} \), where

\[
d^*(h_t, r_t) = \begin{cases} 
1 & \text{if } \sum_{s=t-(T^*+1)}^{t-1} r_s = 0 \\
0 & \text{otherwise}
\end{cases}
\]

To show that the above strategy profile is an SPE, we need to verify that there is no profitable one-shot deviation for either of the players at any history of the game. Consider first the incentive of the firm. At any period \( t > 0 \), if \( h_t \) is such that \( \sum_{s=(t-1)-T^*}^{t-1} r_s = 0 \), the worker which the firm sent to the client at period \( t - 1 \) must have accumulated at least \( T^* \) units of CSS. Given the client’s strategy \( d^* \), the firm will lose that worker for sure if it sends him again to the client. Hence, in this case the firm would indeed prefer to assign a fresh worker to the client. By contrast, if \( \sum_{s=(t-1)-T^*}^{t-1} r_s > 0 \), the firm will not need to worry about losing its employee given the client’s strategy. Since a fresh worker has lower productivity than an experienced one, it would be optimal for the firm to choose \( r_t = 0 \) in this case.

As for the client, take any \( (h_t, r_t) \) such that \( \sum_{s=t-(T^*+1)}^{t-1} r_s > 0 \). This implies that the worker assigned to the client in period \( t \) has at most accumulated \( T^* - 1 \) units of CSS. Since the inequality (B.1) is strict when \( T = T^* - 1 < T^* \), deviating from the rotation equilibrium by capturing worker is not profitable for the client. Hence, at such histories it would indeed be optimal for the client to play \( d_t = 0 \). By contrast, when \( (h_t, r_t) \) satisfies \( \sum_{s=t-(T^*+1)}^{t-1} r_s = 0 \), the worker assigned to the client will have at least accumulated \( T^* \) units
of CSS. Since (B.1) does not hold when $T \geq T^*$, we have

$$\sum_{t=T^*}^{+\infty} \rho^{t-T^*} (u(-p) - \mathbb{E}[u(y_t - w))] \leq 0.$$ 

This implies that not bringing the worker in house is not a profitable one-shot deviation in this case. Thus, at such histories it would be indeed optimal for the client to choose $d_t = 1$. In sum, $(r^*, d^*)$ is an SPE of the game. It is a rotation equilibrium because on the equilibrium path, the firm rotates its worker after every $T^*$ periods, and the client never brings the worker in house.

It remains to show that the rotation equilibrium $(r^*, d^*)$ is optimal. Consider any rotation equilibrium where rotation takes place after every $T$ periods. In equilibrium, the expected payoff of the firm is

$$\Pi(T) = \sum_{k=1}^{+\infty} \sum_{t=0}^{T-1} \delta^{(k-1)T+t} [p - w + \mu_0 + \alpha t].$$

(B.4)

Since $g(t) = p - w + \mu_0 + \alpha t$ is increasing in $t$, we have

$$\Pi(T + 1) = \sum_{k=1}^{+\infty} \sum_{t=0}^{T} \delta^{(k-1)(T+1)+t} [p - w + \mu_0 + \alpha t]$$

$$= \sum_{t=0}^{T} \delta^t g(t) + \sum_{t=0}^{T-1} \delta^{T+1+t} g(t) + \sum_{t=0}^{T} \delta^{k(T+1)+t} g(t) + ...$$

$$> \sum_{t=0}^{T-1} \delta^t g(t) + \sum_{t=0}^{T-1} \delta^{T+1+t} g(t) + \sum_{t=0}^{T} \delta^{k(T+1)-1+t} g(t) + ...$$

$$> \sum_{t=0}^{T-1} \delta^t g(t) + \sum_{t=0}^{T-1} \delta^{T+1+t} g(t) + \sum_{t=0}^{T} \delta^{k(T+1)-2+t} g(t) + ...$$

$$> \sum_{t=0}^{T-1} \delta^t g(t) + \sum_{t=0}^{T-1} \delta^{T+1+t} g(t) + \sum_{t=0}^{T} \delta^{k(T+1)-2+t} g(t) + ...$$

$$= \Pi(T).$$

Therefore, the less frequent the rotation, the higher the expected payoff of the firm. Because there cannot be a rotation equilibrium where the firm rotates even less often, $(r^*, d^*)$ is an optimal rotation equilibrium.
B.2 Proof of Proposition 2

Recall the $T^*$ is the largest $T$ that satisfies (B.1). Plugging the certainty equivalence of $y_t - w$ for the client, we can rewrite (B.1) as

\[
\sum_{t=T-1}^{\bar{T}} \rho^t \left( u(-p) - u\left( \mu_0 + \alpha t - \frac{a}{2} \sigma_0^2 - \beta t - w \right) \right) \\
\geq \sum_{t=\bar{T}+1}^{\infty} \rho^t \left( u\left( \mu_0 + \alpha t - \frac{a}{2} \sigma_0^2 - \beta t - w \right) - u(-p) \right). 
\]

(B.5)

(B.6)

By the definition of $\bar{T}$, all summands in (B.5) and (B.6) are positive. As mentioned in the main text, $\bar{T}$ is decreasing in $\alpha, \beta$ and $\mu_0$, and is increasing in $\sigma_0^2$. It then follows that (B.5) is also decreasing in $\alpha, \beta$ and $\mu_0$, and is increasing in $\sigma_0^2$, while (B.6) is increasing in $\alpha, \beta$ and $\mu_0$, and is increasing in $\sigma_0^2$. This implies that the inequality becomes more stringent as $\alpha, \beta$ or $\mu_0$ increases, or as $\sigma_0^2$ decreases. Therefore, $T^*$ is decreasing in $\mu_0, \alpha$ and $\beta$, and is increasing in $\sigma_0^2$.

\[ \square \]

B.3 Extension: Private Match Value

In this appendix, we extend the baseline model in the main text by introducing private and worker-specific match benefits for the client. As we will show, in this case both rotation and poaching can arise on the equilibrium path. In particular, poaching is more likely to take place for workers who are more experienced and who are better matched with the client.

For simplicity, suppose that the client only need to engage in the risky activity for three periods, $t = 0, 1, 2$. The per-period instantaneous payoffs of the players are the same as in the baseline model, except that the client will additionally receive worker-specific benefit $v \in \{v_{\ell}, v_h\}$ by having the activity performed internally. The benefit $v$ is i.i.d. across workers, and its prior distribution $\Pr(v = v_h) = 1 - \Pr(v = v_{\ell}) = q \in (0, 1)$ is commonly known. Further, the client privately knows the exact match value of a worker if that worker has been assigned to her in the past. Finally, to make the key message of the current extension most salient, we impose the following parametric assumption:

\[
T(v_h) < 1 < T(\bar{v}) < T(v_{\ell}) < 2, 
\]

(B.7)
where \( \bar{v} = qv_h + (1 - q)v_\ell \) and the mapping \( T \) is given by

\[
T(v) = \frac{a\sigma_0^2 - 2(p + \mu_0 + v - w)}{2\alpha + a\beta}.
\]

The assumption implies that if the firm does not do any rotation, the client will poach the worker assigned to her at period \( t = 1 \) when the match benefit is high \( (v = v_h) \), and at \( t = 2 \) when the match benefit is low \( (v = v_\ell) \).

**Proposition B.1.** There exist \( \hat{\rho}, \hat{q} \in (0, 1) \), such that if \( \rho \leq \hat{\rho} \) and \( q \leq \hat{q} \), then there exists a subgame perfect equilibrium where the firm rotates its workers at and only at \( t = 2 \), and poaching takes place at \( t = 1 \) when the private match value for the client is high.

**Proof.** We argue that if \( \rho \) and \( q \) are sufficiently small, then the following strategy profile constitutes an SPE: For the firm, its strategy is to rotate the current worker if and only if \( e = 2 \), i.e., that worker has been assigned to the client at both periods 0 and 1. For the client, she will bring the assigned worker in house either if \( e = 2 \) or if \( e = 1 \) and she has learned that \( v = v_h \). It is clear that in such an equilibrium, both rotation and poaching can arise on the equilibrium path as described in the proposition.

First, consider the incentive of the firm. Taking the strategy of the client as given, the firm will strictly prefer to send out a fresh worker at \( t = 2 \) if there was no rotation at \( t = 1 \), because otherwise it will for sure lose both its employee and client. However, if rotation already took place at \( t = 1 \), then the firm would not want to make another replacement at \( t = 2 \) provided that

\[
(1 - q) (-w + p + \mu_0 + \alpha) \geq -w + p + \mu_0, \tag{B.8}
\]

which holds whenever \( q \) is sufficiently small. Further, provided that (B.8) holds, at \( t = 1 \) the firm will indeed prefer not to rotate its worker if

\[
(1 - q) [-w + p + \mu_0 + \alpha + \delta(-w + p + \mu_0)] \\
\geq -w + p + \mu_0 + \delta(1 - q) (-w + p + \mu_0 + \alpha),
\]

which also holds whenever \( q \) is sufficiently small.

Next, consider the incentive of the client. At period \( t = 2 \), if the assigned worker is a rookie \( (e = 0) \), then poaching is not a best response for the client given that \( T(\bar{v}) > 1 \). If \( e \geq 1 \), then the client must have learned her private match benefit with the worker. Given the assumption \( T(v_h) < 1 < T(v_\ell) < 2 \), the client will strictly prefer to bring the worker
in house if either $e = 2$ or if $e = 1$ and $v = v_h$, while outsourcing to the firm will still be preferred if $e = 1$ and $v = v_\ell$.

Taking the above continuation strategy at $t = 2$ as given, we then consider the client’s incentive at $t = 1$. If the worker assigned to the client in this period is a rookie, then, anticipating that the same worker will be assigned in the next period, the client would prefer to carry on the outsourcing relationship with the firm if

$$-p - \rho \left( (1 - q)p + q \left( -w + v_h + \mu_0 + \alpha - \frac{a}{2}(\sigma_0^2 - \beta) \right) \right) \geq -w + \bar{v} + \mu_0 - \frac{a}{2}\sigma_0^2 + \rho \left( -w + \bar{v} + \mu_0 + \alpha - \frac{a}{2}(\sigma_0^2 - \beta) \right),$$

which is guaranteed to hold given the assumption $T(\bar{v}) > 1$. If the firm did not rotate the worker in the beginning of the period, then the client must have learned her private match benefit with that worker. Given $T(v_h) < 1$, the client will for sure prefer to bring that worker in house if $v = v_h$. If $v = v_\ell$, the client would refrain from poaching the worker if

$$-p - \rho p \geq (1 + \rho) \left( -w + v_\ell + \mu_0 + \alpha - \frac{a}{2}(\sigma_0^2 - \beta) \right) + \rho \left( \alpha + \frac{a}{2} \beta \right),$$

which, given that $T(v_\ell) > 1$, holds whenever $\rho$ is sufficiently small.

Finally, it is also straightforward to check that condition (B.7) guarantees that the client will not have the incentive to deviate to poach the fresh worker assigned to her at $t = 0$. \(\square\)
C  Additional Figures and Tables

The figure displays the estimated coefficients and the 95% confidence intervals of a probit regression, where the dependent variable is an indicator of the guard being type-II and the explanatory variables are predetermined characteristics of the guard. Non-dummy variables are standardized. Robust standard errors are used. \( N = 534 \).

Figure C1: Balance of Type-I vs. Type-II Allocation
The figure displays the estimated coefficients and the 95% confidence intervals of a regression, where the dependent variable is an indicator of whether a crime occurred during the shift of the guard, and the explanatory variables are dummies indicating the days before the guard is rotated to a different building. The regression controls for fixed effects for week, shift (day or night), guard-building pair, and interactions between the neighborhood of the building and the month. Sample is restricted to the period before the introduction of the decree. Standard errors are clustered at the guard level. $N = 213,344$.

**Figure C2:** Evolution of Crime Before Rotation
Table C1: The Matching Between Guards and Buildings

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All Pairs of Guard-Building</strong></td>
<td><strong>F</strong> (Prob F $&gt;$ 0)</td>
<td><strong>F</strong> (Prob F $&gt;$ 0)</td>
</tr>
<tr>
<td>N of Flats in the Building</td>
<td>0.85 (0.63)</td>
<td>0.88 (0.60)</td>
</tr>
<tr>
<td>N of Required Guards</td>
<td>1.50 (0.11)</td>
<td>1.46 (0.13)</td>
</tr>
<tr>
<td>Socioeconomic Strata of Neighborhood</td>
<td>0.59 (0.89)</td>
<td>1.64 (0.07)</td>
</tr>
<tr>
<td>High Strata of Neighborhood</td>
<td>1.01 (0.45)</td>
<td>2.04 (0.02)</td>
</tr>
<tr>
<td>City Area = South</td>
<td>2.03 (0.02)</td>
<td>1.07 (0.40)</td>
</tr>
<tr>
<td>City Area = Center</td>
<td>0.92 (0.56)</td>
<td>0.85 (0.63)</td>
</tr>
<tr>
<td>City Area = West</td>
<td>0.41 (0.98)</td>
<td>0.40 (0.98)</td>
</tr>
<tr>
<td>City Area = East</td>
<td>0.91 (0.57)</td>
<td>0.79 (0.70)</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>1,559</td>
<td>625</td>
</tr>
</tbody>
</table>

**Dependent Variable:**

Guard Characteristics (controls): Gender, age, age squared, household size, immigration status, military training, previous working experience, dummy for living alone, dummies for the strata of the neighborhood and for the city area where the guard lives.

This table reports the F-statistics and the corresponding p-values for cross-section regressions of building characteristics (dependent variable in each row) on guard characteristics. Each cell refers to a different regression. In Column (1), the regressions include all the observed combinations of guards and buildings (cross-section). In Column (2), observations are restricted to the first building where the guard was assigned to work when joining the firm. Standard errors are clustered at the building level.
Table C2: Predicted Poaching Risk

<table>
<thead>
<tr>
<th></th>
<th>(1) Correlation with Baseline Chars</th>
<th>(2) Gini-Based Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male</td>
<td>.14*** (0.0094)</td>
<td>0.051</td>
</tr>
<tr>
<td>Military Experience</td>
<td>.012* (0.0073)</td>
<td>0.022</td>
</tr>
<tr>
<td>Neighborhood Strata</td>
<td>-.0087 (0.009)</td>
<td>0.029</td>
</tr>
<tr>
<td>Household Size</td>
<td>.028*** (0.0046)</td>
<td>0.110</td>
</tr>
<tr>
<td>Lives Alone</td>
<td>-.041*** (0.014)</td>
<td>0.016</td>
</tr>
<tr>
<td>Age</td>
<td>-.012*** (0.0042)</td>
<td>0.171</td>
</tr>
<tr>
<td>Past Experience</td>
<td>-.023*** (0.0074)</td>
<td>0.130</td>
</tr>
<tr>
<td>Had Experience as Guard</td>
<td>.042*** (0.01)</td>
<td>0.025</td>
</tr>
<tr>
<td>Immigrant</td>
<td>.02* (0.012)</td>
<td>0.023</td>
</tr>
<tr>
<td>Years Since Migration</td>
<td>-.073*** (0.012)</td>
<td>0.169</td>
</tr>
<tr>
<td>Recently Migrated</td>
<td>.066*** (0.019)</td>
<td>0.032</td>
</tr>
<tr>
<td>Neighborhood of Residence FEs (Std Error/Combined Importance of FEs)</td>
<td>.016</td>
<td>0.221</td>
</tr>
</tbody>
</table>

This table displays the relation between the predicted probability that a guard is hired in-house (estimated using a Random Forest model) and the baseline characteristics of the guards. Column (1) shows the estimated coefficients (and their standard deviations) of a regression using the predicted score as the dependent variable. The regression also includes fixed effects for the neighborhood where the guard lives. Column (2) shows the mean decrease in the Gini Impurity of each variable, which is a measure of the relative importance of the variable in predicting the poaching risk. For the neighborhood of residence, we report the sum of the Gini-based importance across all the neighborhood indicators.
This table investigates the effects of the introduction of the decree on guards’ rotation (using two different measures) and crime. Each column reports the coefficient of the interaction between an indicator for the period after the law was introduced and the estimated probability that the guard is poached by a building. The poaching risk is estimated using an alternative specification as illustrated in the main text. In Column (1), the dependent variable is an indicator of whether the guard was rotated to a new building during the month. In Column (2), the dependent variable is the average number of shifts per building worked by the guard during the month. In Column (3), the dependent variable is the total number of crimes occurred during the shifts worked by the guard in the month. All regressions use observations at the guard-month level, and include fixed effects of guard, month and the building where the guard worked most time during the month. Additionally, all regressions include guard-specific linear trends and control for the total number of days the guard worked during the month and the log-experience of the guard. Robust standard errors in parentheses are clustered two-ways at the guard-month level. The square brackets report the standard error of the corresponding coefficient obtained by 200 bootstrap repetitions of the whole two-step procedure (i.e., the estimation of the poaching probability and the main regression).
### Table C4: Effect of the Policy on Guards’ Rotation and Crime
Regressions at the guard × week level

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post × Poaching Risk</td>
<td>-0.026***</td>
<td>1.2***</td>
<td>-0.0043</td>
</tr>
<tr>
<td></td>
<td>(.0078)</td>
<td>(.34)</td>
<td>(.007)</td>
</tr>
<tr>
<td>N</td>
<td>389,164</td>
<td>389,164</td>
<td>389,164</td>
</tr>
</tbody>
</table>

Indiv Chars: YES YES YES
Week FE: YES YES YES
Guard FE: YES YES YES
Guard Trends: YES YES YES
Building FE: YES YES YES
Building Trends: YES YES YES

This table investigates the effects of the introduction of the decree on guards’ rotation (using two different measures) and crime. Each column reports the coefficient of the interaction between an indicator for the period after the law was introduced and the estimated probability that the guard is poached by a building. In Column (1), the dependent variable is an indicator of whether the guard was rotated to a new building during the week. In Column (2), the dependent variable is the average number of shifts per building worked by the guard during the week. In Column (3), the dependent variable is the total number of crimes occurred during the shifts worked by the guard in the week. All regressions use observations at the guard-week level, and include fixed effects of guard, week and building. Additionally, all regressions include guard-specific linear trends and control for the total number of days the guard worked during the week and the log-experience of the guard. Robust standard errors in parentheses are clustered two-ways at the guard-week level. The square brackets report the standard error of the corresponding coefficient obtained by 200 bootstrap repetitions of the whole two-step procedure (i.e., the estimation of the poaching probability and the main regression).