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job rotation**

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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 increases both workers' productivity and 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 20th 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. Anticipating the client's behavior, the service firm may take costly actions to avoid poaching.¹ We argue that one of these actions consists in rotating workers from one client to another. 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.

We are not aware of any existing study that quantifies how severe the phenomenon of talent poaching from past clients is. Nevertheless, media coverage and public discussions clearly suggest that firms care about poaching and the issue is widespread. For instance, there is registered involvement of poaching past suppliers' employees for both leading companies such as Apple (Bradshaw, 2015, 2017) and less eye-catching multi-million dollar firms like Guardsmark.² More generally, the phenomenon has been documented for a diverse set of

¹This type of strategic response is a familiar problem in antitrust law. For instance, it is known that if firms are prohibited from anticompetitive behavior such as merger acquisition, price collusion or exclusive contracting, they may resort to unnecessary product differentiation to attain market power, which can in turn lead to adverse welfare consequences (see, e.g., Makadok and Ross (2013) for a formal analysis).

²See the United States District Court (E.D. Kentucky, Covington Division) case *Borg-Warner Protective Services v. Guardsmark, Inc.* 946 F. Supp. 495, 27 Nov. 1996.

occupations (both high- and low-skilled) and industries, including nursing (DLA Labor Dish Editorial Board, 2014), engineering (Chaput, 2018), cleaning (Shubber, 2018), managerial services (StevensVuaran Lawyers, 2019), game publishing (Schreier, 2020), travel advising (Pestronk, 2019), and marketing (Liffreing, 2018) among many others. It is therefore not surprising that the issue has drawn attention in many different countries, such as Australia (StevensVuaran Lawyers, 2019), Canada (Chaput, 2018) and the US (Bennet, 2018).

Despite the prevalence and importance of the poaching problem, research on this topic has been limited, probably due to the lack of a comprehensive database that allows for tracking the transition of workers across companies and industries. To advance in this direction and overcome the challenge, we take a different approach by partnering with a Colombian security-service industry firm. The empirical setting, albeit somewhat special, is adequate to study the issue of poaching for at least two reasons. First, in the middle of our sample period, the country implemented a non-poaching policy. Second, we have very rich data. 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, his/her 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 we observe 28 guards being poached by buildings and 34 formal solicitations. 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 total 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.³ 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.⁴ 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 relationship 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 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.

³Huckman 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.

⁴The exogeneity of the variation arises from the fact that the firm allocates workers on a first-come first-serve basis. In other words, the assignment of a new guard merely 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.

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.⁵ 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 afterwards. 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 talent poaching.

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

⁵During the whole sample period, the Colombian legislation plainly prohibited firms from signing contracts with other competitor-firms on poaching workers. However, before 1994, the legislation was open to interpretation when the worker is poached by a client-firm.

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; Meyer and Vickers, 1997; Arya and Mittendorf, 2004, 2006; Prescott and Townsend, 2006; Hertzberg et al., 2010; Hakenes and Katolnik, 2017).⁶ As we will show, these familiar hypotheses for job rotation do not seem to be consistent with our empirical setting.⁷ Instead, our paper proposes and demonstrates a fundamentally different rationale for job rotation: it can be used as an organizational remedy to mitigate poaching risk.⁸

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-

⁶However, 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).

⁷A relevant agency problem in our context might be the collusion between guards and criminals. However, in Section 3.2, we argue that this possibility is at odds with our empirical finding that the longer a guard has worked in a building, the less likely that a crime occurs when the guard is on duty. As for the learning aspect of rotation, we believe that it did not play a major role in our setting either, due to the type of rotation drops occurred around the policy change (see Section 4.3).

⁸The 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.

specific skill training to its workers. It has been well understood that if the firm cannot avoid poaching from its competitors (because *non-poaching agreements* between employers operating in the same product market are typically illegal), the provision of general skill training will be insufficient. We contribute to the literature by showing that in the complementary case where the firm cannot avoid poaching from its past clients, 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; Starr et al., 2020a,b; Lipsitz and Starr, 2020). 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 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 when the other employer is a client: a non-poaching policy 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 Appendices A, B and C .

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 $t = 0, 1, 2, \dots + \infty$. 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 $y_t \sim \mathcal{N}(\mu(e), \sigma^2(e))$, where $\mu(e) = \mu_0 + \alpha e$, $\sigma^2(e) = \max\{\sigma_0^2 - \beta e, 0\}$, $\mu_0, \beta \geq 0$ and $\sigma_0^2, \alpha > 0$. Here, $e \in \mathbb{N}$ is the “experience” of the worker, i.e. for how long the worker has been working for the client. Since $\mu(\cdot)$ is strictly increasing and $\sigma^2(\cdot)$ is weakly decreasing, the client-specific experience is valuable in the sense that it always increases the average output, and potentially also decreases the 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 has a discount factor $\delta \in (0, 1)$, and it charges a per-period service fee $p > 0$ for assigning a worker to the client and insuring her against the monetary consequence of the risky activity (i.e., the stochastic output y_t will be completely transferred to the firm).⁹ In contrast to the client, the firm is risk-neutral. Thus, provided that the outsourcing relationship is active, the firm’s flow payoff is equal to

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

where $w \in (0, p)$ is the per-period wage that it pays to the worker. The relative risk attitudes of the firm and the client imply that there is a potential gain from trade, and they fit in well with our empirical setting (the security firm indeed provides insurance against property loss to the buildings that it serves). The assumptions of constant wages and service fees greatly simplify the analysis and allow us to make it most evident how job rotation can balance the trade-off between poaching risk and client-specific skill. We will consider the general case where the service fees may vary across periods and are endogenously chosen by the firm in

⁹We 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). Our model also assumes, for simplicity, a monopoly service market. In a competitive setting where we may have multiple firms competing for a client, the service fee can be endogenously pinned down by the zero-profit condition of the firms.

Appendix Section B.3. The exogeneity of wages can also be relaxed, as we will discuss later (see footnote 10). The main insights of our baseline model are robust to these extensions.

In each period, the firm and the client interact with each other according to the following timeline (see Figure 1 for a graphical illustration). First, the firm 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 $\varepsilon > 0$).¹⁰ 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, while the firm may only receive its reservation payoff (which we normalize to zero).¹¹ 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. In the current baseline model, all parameters are commonly known, so we will focus on 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 $CE = \mu_y - \frac{\alpha}{2}\sigma_y^2$. Hence, provided that $\beta t \leq \sigma_0^2$, the client receives a higher instantaneous utility by hiring the worker internally than purchasing the service from the

¹⁰For simplicity, we abstract from the consideration that the worker may further negotiate with the firm or the client for his wage. One could imagine that client-specific experience or poaching risk may increase the value of a worker's outside option, and hence also his bargaining power against the firm (e.g., Nash, 1950; Rubinstein, 1982). If that is the case, job rotation will have the additional benefit of diminishing workers' bargaining power (and therefore a smaller wage bill), which should make the practice even more attractive to the firm in the presence of poaching risk.

¹¹The 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.

firm if and only if:

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

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 $\bar{T} \in (0, +\infty)$, which is strictly smaller than σ_0^2/β whenever $\beta > 0$. It is then clear that the client would prefer to bring the worker in house (and no longer transact with the firm) when it has reached 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 at most be able to collect revenue from the client for \bar{T} periods. After that, poaching takes place and the firm loses both its employee and client. Even ignoring the costs of recruiting and training a new employee, letting the client to poach the worker can still be highly undesirable if the firm sufficiently values its long-term revenue (i.e., if the firm's discount factor δ is sufficiently large; see Proposition B.1 in the appendix for a formal statement).

It is straightforward to check that the cutoff \bar{T} is increasing in σ_0^2 , and it is decreasing in μ_0 , α and β . This is intuitive: a worker is more desirable/productive from the client's perspective if σ_0^2 is smaller, or if μ_0 , α or β 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 proceed to 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 when the risk facing her remains substantial the 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 $\bar{\sigma}_0^2 > 0$, such that if $\sigma_0^2 \geq \bar{\sigma}_0^2$ (i.e., the initial risk associated with the production activity is sufficiently large), then there exists an optimal rotation equilibrium, where the firm rotates the workers after every $T^* \leq \bar{T}$ periods.*

Proof. See Appendix Section B.1. □

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. In Appendix Section B.4, we provide an extension where the benefits from poaching a worker and performing the risky activity in house is privately known to the client.¹² In that setting, we show that poaching can take place *on* the equilibrium path despite the firm's rotation strategy, but nonetheless with a lower probability than had the firm rotated workers less frequently.

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.

¹²The information asymmetry between firm and client differentiates our study from Ciapanna (2011). In her model, Ciapanna (2011) assumes that (consulting) service firms perfectly know the benefits of different assignments and hence can make profits by facilitating the matching between consultants and clients.

Proposition 2. *The optimal frequency of rotation $1/T^*$ is increasing in α, β and μ_0 , and it is decreasing in σ_0^2 .*

Proof. See Appendix Section B.2. □

To sum up, the theoretical analysis highlights that the strategic concern of employee poaching can lead to excessive job rotation.¹³ Importantly, our model provides several clear-cut empirical predictions. First, the probability of a worker being poached should increase with his client-specific experience. Second, the higher the poaching risk of a worker, the more often he will be rotated by the firm. Finally, if employee poaching were prohibited, rotation should be merely driven by factors exogenous to our model, such as the sick leave of workers. Hence, a non-poaching policy change like the one we study in this article should result in less rotation, and the observed reduction in rotation should be more significant if the associated poaching risks are higher before the policy change. As less rotation implies larger accumulation of CSS, the policy change can increase the total surplus generated from the transaction (leading to for instance few crimes as we show in Section 4).¹⁴ In what follows, we will use a unique dataset to carefully examine our model’s predictions.

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).

¹³It 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.

¹⁴A 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 (in the hope of higher salaries, more stability, etc.), then the policy change would eliminate that incentive and thus result in a decrease in effort.

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* (6 am - 6 pm) and the following six days during the *night shift* (6 pm - 6 am). After the 12 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. Note that a single type-II guard is sufficient to cover the resting periods of two type-I guards. Thus, in a given week, a building typically needs two type-I guards and one type-II guard to cover all the shifts.¹⁵

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 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 before our sample period starts. We do not have wage information for each guard,

¹⁵Some large buildings require more than one guard working at the same time because for instance they have several entrances.

but we know that the majority of guards earn the minimum wage during the entire sample period of our study (both before and after the policy change). At the same time, the monthly service fee that the firm would charge for maintaining a guard position in a building is about 5 times of the monthly minimum wage.

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 in 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 monthly probability of 7%.

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.¹⁶

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 (which may occur, e.g., if the building's administrator is not satisfied with that 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 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 (only 2

¹⁶This approximately corresponds to 85% of the local monthly minimum wage in 1993.

out of 16 are slightly above 2). These results are consistent with the narrative that the firm allocates guards to buildings exogenously to their characteristics.¹⁷

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 flat 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 number, 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 the 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 the incidence of crime in a building during the shifts when a guard is on duty.¹⁸ In order to

¹⁷Due to the limited number of predetermined characteristics observed in the data, we cannot fully reject the possibility that endogenous matching may occur along other non-observable dimensions. However, we believe that the potential endogeneity problem is not severe as below we show that the results are robust even when we control for guard-building unobservable characteristics. In addition, as we show in Appendix Figure C3, guards do not follow a career across types of buildings. In particular, there is no evidence that guards are more likely to be allocated to high-economic-strata buildings as they acquire more experience within the security company.

¹⁸We 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 to us that crime prevention is undoubtedly the number one priority for its clients.

investigate the impact of building-specific experience on crime, we use data at the guard \times shift level to estimate the following equation:

$$Crime_{ibt} = \beta LogExpInBuilding_{ibt} + \eta LogTotalExp_{it} + \delta_{ib} + \gamma_{m(t)} + \epsilon_{ibt}, \quad (2)$$

where $Crime_{ibt}$ 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 inverse-hyperbolic-sine transformed value of property stolen if crime occurs, which we denote as Y_{ibt} .¹⁹ Our main explanatory variable $LogExpInBuilding_{ibt}$ 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 that 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 long if the firm prefers to allocate young guards to large buildings whenever there is a vacancy. For this reason, we include pair-specific fixed effects δ_{ib} and exploit the variation in building-specific experience *within* each guard-building pair over time. Finally, in order to avoid confounding the effect of building-specific experience with systematic changes in crime over time, we also includes monthly fixed effects $\gamma_{m(t)}$ in the estimation.

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 $LogTotalExp_{it}$. This variable is identified separately from time fixed effects because (i) not all guards 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

¹⁹The inverse hyperbolic sine transformation can be interpreted similarly to the logarithm, but has the advantage of being well-defined for zero and even negative values (which is important because our dependent variable has many zeros).

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 \times day of the week). These results indicate that within a given guard-building pair, crime is reduced as the guard accumulates more experience in that specific building, even controlling for the total experience as a guard.²⁰ In fact, we find that the coefficient that measure this total experience 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 C2, 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.²¹

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

²⁰The firm has told us that buildings do not provide more materials or amenities to the guards as their tenures increase, so this cannot be the reason for crime rates to decrease with larger building-specific tenure.

²¹Specifically, we run regressions of a dummy indicating the guards' types on their baseline characteristics. All coefficients are small in magnitude and non-significant at 5% level.

that the guard has 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.²²

The findings of Table 2 are important for two reasons. First, a potential reason for rotation is to avoid collusion with criminals (Choi and Thum, 2003; Abbink, 2004; Rose-Ackerman, 2010; Jia et al., 2015). Under this hypothesis, the longer a guard works in a building, the more likely he/she will be contacted and even corrupted by criminals and therefore the more likely crime will happen. However, this rationale is at odds with our finding that crime actually decreases as the guard’s time spent in the building increases. This suggests that in the current empirical setting, the main purpose of rotation is unlikely to be deterring collusion between guards and criminals. Second, the results 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.2 in the Appendix B.4), 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

²²A possible interpretation of the larger coefficients from the IV estimation is that OLS estimates are downward biased due to reverse causation. In Appendix Figure C3, 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.

relationship with the firm.²³ 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 comparing 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 would prefer to hire 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:

$$Poached_{ibt} = \beta LogExpInBuilding_{ibt} + \eta LogTotalExp_{it} + \varphi_{bm} + \eta_i + \gamma_t + \epsilon_{ibt}, \quad (3)$$

where $Poached_{ibt}$ 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 φ_{bm} . We also include guard (η_i) and week (γ_t) fixed effects. Results are displayed in Table 3.²⁴ 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.²⁵

²³In most of these cases (70%), buildings poached only one guard. A natural question is how these buildings cover all the shifts with only one poached guard - they can no longer receive service from the security firm after the poaching event. Although we do not have data to validate the anecdotal evidence, conversation with a few buildings made us conclude that when buildings poach they usually have other guards ready to cover the remaining shifts. These other guards can be workers who used to work in the building or someone with guard-experience that the building recruited, e.g., through referrals.

²⁴It is possible that total experience affects the dependent variables of our estimations in a non-(log)linear way. The concern in that case would be that the coefficient of the building-specific experience may confound the unremoved variation of the total experience. As a robustness check, we further estimate the relationships of Tables 2 and 3 by controlling the total experience non-parametrically. Results are reported in Appendix Tables C2 and C3, respectively. The estimates are very similar to those reported in the previous tables.

²⁵This magnitude is very large if we compare it with for instance the total share of guards poached during the period (0.06). Also note that, as we mentioned before, it is possible that the firm prevented some poaching events 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.

Overall, the above analysis demonstrates 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 assets, which *de facto* limits the possibility that one guard becomes an in-house worker establishing a company by herself. As a consequence, the introduction of the new law inhibited buildings from hiring guards directly.

It is important to clarify that during the whole sample period the Colombian legislation prohibited firms from using any formal contracts (e.g., non-competing clauses) to restrict the possibility of workers being poached by other firms in the same industry. However, before 1994 it was not clear from the legislation whether clients were allowed to poach employees from ex-suppliers. After 1994, the Decree 356 made clear that that latter type of poaching was not possible anymore in the security-service industry.

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, a simple 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, once the degree came into effect, for the guards who were more likely to be poached.

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 the data, are much more exposed to the risk of poaching than type-II guards.²⁶ 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 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, size of household, immigration history, military training, and working experience before joining the firm.

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.²⁷ 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).

²⁶During 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.

²⁷Note however that Proposition B.2 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.

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.²⁸ 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.²⁹ We summarize the results of the estimation in Appendix Table C4. In addition to the estimated coefficients of the regression, the table reports the Gini Importance or the mean decrease in the Gini Impurity of each variable, which measures the relative importance of that variable in predicting the poaching risk (i.e., its contribution to 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. These two variables capture different types of variation in rotation. On one hand, the dummy variable reflects the extensive margin of rotation and helps to understand how other variables (i.e. poaching risk) affect the fact that the worker has been rotated or not. On the other hand, the average shifts per building aims to exploit the intensive margin of rotation and helps to distinguish those guards that are rotated to a single new building from those that are

²⁸In Appendix Table C6 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.

²⁹To 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).

rotated to multiple buildings.

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-varying 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.

Finally, we further show in Appendix Table C5 that there is an insignificant correlation between the poaching risks of the guards that leave buildings due to rotation and those who replace them. This implies that the company is not replacing guards of high poaching risk systematically with either high or low poaching risk guards. Therefore, it is really the interaction of the ex ante poaching risk of the guards and the accumulation of building-specific experience that drives the firm’s rotation decisions.

4.3 Estimating the Effect of the Policy on Rotation

The risk of employee poaching dropped 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:

$$Rotation_{it} = \beta RiskPoaching_i \times After_t + \phi X_{it} + \eta_i + \gamma_t + \theta_i \times t + \delta_{b(it)} + \varepsilon_{it}, \quad (4)$$

where the dependent variable measures the rotation of guard i during month t . The effect of the policy (β) 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 (η_i), and account for time aggregated variation by including month fixed effects (γ_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 ($\delta_{b(it)}$).³⁰

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 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.

³⁰In 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 perfect collinearity with our main rotation measure. As a robustness check, in Appendix Table C7 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.

Figure 3 depicts the leads and lags of $RiskPoaching_i \times After_t$ 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.³¹ 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 equation (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.

We further investigate the extent to which the policy offset the differential rotation between high and low poaching risk guards. In Table 6 we extend the regression in Table 4 to include the period after the decree introduction. We find that poaching risk significantly

³¹This result also provides evidence against the idea that the firm used rotation mainly to learn about the ability of guards. If that was the case, there is no reason why the policy change would decrease the need for learning and why in particular would do it for those workers with a larger poaching index.

loses predictive power over rotation after the introduction of the decree. While the interaction between estimated poaching risk and an indicator of the period before the law is highly significant (matching the estimates in Table 4), the coefficients of interaction with an indicator for the period after the decree introduction are 8 to 9 times smaller and non-significant, suggesting that the policy offset most of the rotation gap that was due to differences in poaching risk.

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.³² 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 7, 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 7 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

³²Extending the period of analysis gives us significant but weaker results, hindering the instrumental variable exercise described later in this section.

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.

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 7. 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.³³

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

³³A 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 the problem only happens to a small number of sub-samples. In Table 7 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.

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 client-specific human capital 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 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. Although we have provided a set of anecdotal examples that indicate the prevalence of the issue across industries, the lack

of data has made it impossible to quantify exactly how spread the phenomenon is. One way to proceed is to study a panel of employer-employee matching data with information of supply chain links. In that case, the researcher can observe the transitions of workers and quantify how common that workers end up being hired by clients of their past employers. A particular prediction emerges from our analysis is that the severity of the problem of excessive job rotation (or, more broadly, strategic destruction of client-specific human capital) should depend on whether the places enforce more or less stringent no-poaching clauses (e.g., Florida v.s. California).

Finally, we believe that there are other settings, usually with high-skill workers, in which service-providing firms may be more positive about their employees being poached by client-firms, especially if these workers can assure future stream of transactions with their original employers. This setting is not appropriate to our empirical context primarily because the client-firm obtains the necessary input either all in-house or all outsourced. We expect that the benefits of client poaching are more significant in settings with other characteristics, for instance, those in which the client-firm would require a fraction of the labor force in-house and acquire the remaining labor input through outsourcing. Then, it is plausible that a service-providing firm will be more likely to seize the business opportunity if it has an ex-employee working in the client-firm. Studying and characterizing these other settings is outside the scope of this paper, but future work in this direction is guaranteed.

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A Main Figures and Tables

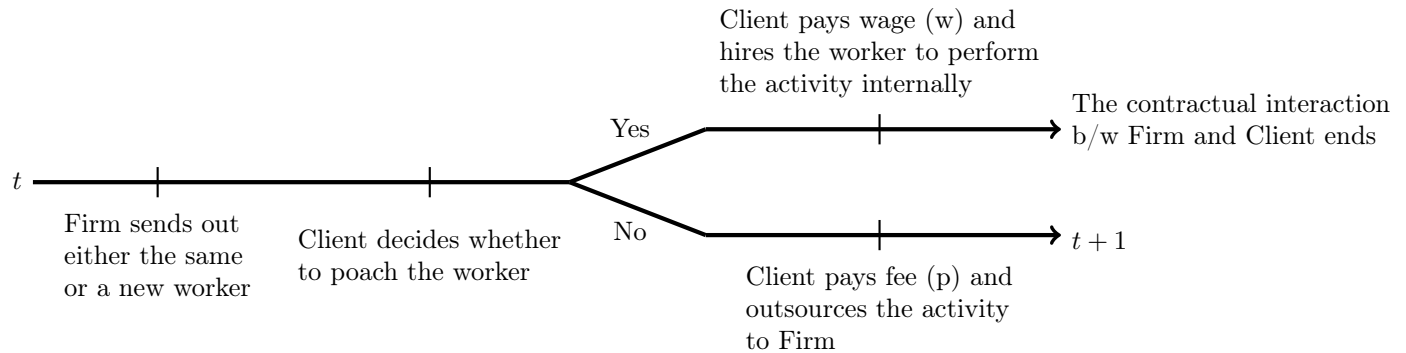


Figure 1: Timing of the Stage Game

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

Building	Shift	Week 1							Week 2							Week 3		
		1	2	3	4	5	6	7	1	2	3	4	5	6	7	1	2	
1	Day (6am-6pm)	e1-A	e1-A	e1-A	e1-A	e1-A	e1-A	e2	e2	e1-B	e1-B	e1-B	e1-B	e1-B	e1-B	e1-B	e1-A	e1-A
1	Night (6pm-6am)	e1-B	e1-B	e1-B	e1-B	e1-B	e1-B	e1-A	e1-A	e1-A	e1-A	e1-A	e1-A	e2	e2	e1-B	e1-B	

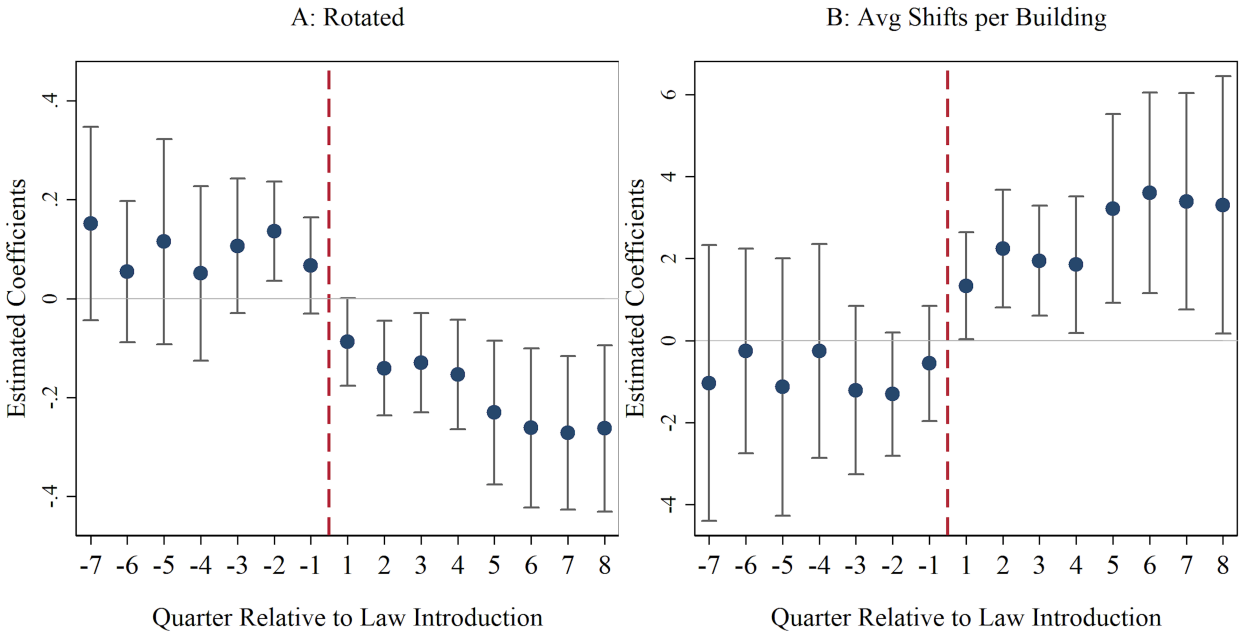
Panel B: Example of a 12-day working period for a type-II guard

Building	Shift	1	2	3	4	5	6	7	1	2	3	4	5	6	7	1	2
1	Day													e2	e2		
1	Night							e2	e2								
2	Day			e2	e2												
2	Night								e2	e2							
3	Day					e2	e2										
3	Night											e2	e2				

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

Leads-Lags \times Risk of Guard is Hired in-house



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 quarter period before the introduction of the law. 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 a given 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

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

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 upper panel of the table presents the statistics for guards (i.e., each observation is a guard). In that panel, *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 (from 1 to 6). A larger number means a higher average-income neighborhood. *Household size* is the number of people living with the guard. *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 our partner firm. The main difference between the variables *Immigrant* and *Recent immigrant* is that the latter is a category for immigrants that migrated to Bogota less than 10 years before while immigrant is for any person that 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 period. We report the average across all the buildings where the guard worked in the entire period of study. The last four variables in the panel above concern the number of buildings that a guard is assigned to and whether or not he/she is rotated to a new building in a month (for both type-I and type-II guards). The panel below presents statistics for the buildings (i.e., each observation is a building), which includes 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

	(1)	(2)	(3)	(4)
Panel A: Crime Occurred During Guard's Shift				
Log Experience in Building (\div 100)	-.013** (.0056)	-.011* (.0056)	-.011* (.0056)	-.036* (.021)
Log Total Experience (\div 100)	.0061 (.0097)	.007 (.01)	.007 (.01)	.015 (.013)
N	656,438	656,438	656,438	656,438
Panel B: IHST Value of Property Lost in Crime				
Log Experience in Building (\div 100)	-.18** (.077)	-.15** (.077)	-.15** (.077)	-.51* (.29)
Log Total Experience (\div 100)	.091 (.14)	.1 (.15)	.1 (.15)	.22 (.18)
N	656,438	656,438	656,438	656,438
Method:	OLS	OLS	OLS	IV
Guard \times Building FE:	YES	YES	YES	YES
Shift FE:	YES	YES	YES	YES
Month FE:	YES	NO	NO	NO
Building Neighb \times Month FE:	YES	YES	YES	YES
Week FE:	NO	YES	YES	YES
Shift \times Day of Week FE:	NO	NO	YES	YES

All regressions are at guard \times shift level. In Panel A, the dependent variable is an indicator for a crime occurring during the shift of the guard in the building. In Panel B, the dependent variable is the inverse hyperbolic sine transformation (IHST) of the estimated value of the property stolen or destroyed during the crime. All regressions control for the number of shifts that the guard worked during the month. In Column (4), the accumulated experience of the guard in the building is instrumented with the interaction between an indicator for guard type-II and the tenure of the guard within the firm. Robust standard errors clustered two-way at guard and at week level. First stage F statistics is 632.28. 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)				
	(1)	(2)	(3)	(4)
Log Experience in Building ($\div 100$)	.17*** (.05)	1.2*** (.35)	.24*** (.062)	1.7*** (.42)
Log Total Experience ($\div 100$)	.11 (.066)	-.066 (.051)	.18** (.08)	-.064 (.06)
N	40,099	40,099	40,099	40,099
F first-stage		401.15		403.21
Method:	OLS	IV	OLS	IV
Guard FE:	YES	YES	YES	YES
Week FE:	YES	YES	YES	YES
Building \times Month FE:	YES	YES	NO	NO
Building \times Week FE:	NO	NO	YES	YES

All regressions are at guard (building) \times week level. The dependent variable is an indicator for the week when the worker is hired in-house by the building and the sample is restricted to the period before the policy introduction. All regressions control for the number of shifts that the guard worked during the month and the share of night shifts worked in the week. In Column (4), the accumulated experience of the guard in the building is instrumented with the interaction between an indicator for guard type-II and the tenure of the guard within the firm. Robust standard errors clustered two-way at guard and at week level. Experience variables are divided by 100 (i.e. coefficients are scaled up by 100).

**Table 4: Relation Between Rotation and
the Estimated Risk of Being Poached**

Dependent Variable	(1) Rotated	(2) Avg Shifts per Building
Poaching Risk	.092*** (.026) [.033]	-1.2*** (.35) [.45]
Log Tenure	.025 (.029)	-.03 (.41)
N	3,130	3,130

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

Dependent Variable	(1) Rotated	(2) Avg Shifts per Building	(3) N Crimes
Post \times Poaching Risk	-.15*** (.038) [.064]	2*** (.54) [.877]	-.0056 (.028) [.029]
N	14,708	14,708	14,708
Indiv Chars:	YES	YES	YES
Month FE:	YES	YES	YES
Guard FE:	YES	YES	YES
Guard Trends:	YES	YES	YES
Building (most worked) FE:	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 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: Relation between Rotation and the Estimated Risk of Guard Being Poached Before and After the Law

Dependent Variable	(1) Rotated	(2) Avg Shifts per Building
PoachingRisk \times BeforeLaw	.09*** (.025)	-1.2*** (.34)
PoachingRisk \times PostLaw	.012 (.008)	-.13 (.11)
N	17,564	17,564

This table investigates the relationship between the estimated risk of being hired by a building and the rotation of guards before and after the policy introduction. In Column (1), the dependent variable is an indicator of whether the guard worked 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 total (log) tenure of the guard in the firm, month fixed effects, the number of days 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 guard-month level. Robust standard errors clustered at guard level.

Table 7: Effect of the Policy on Buildings' Crime Measures

Dependent Variable	(1) Share of New Guards	(2) N Crimes per Month	(3) Value of Property Lost
Panel A:			
Reduced Form Results			
Post \times Avg Poaching Risk at Time of Law Introduction	-.22*** (.07) [.11]	-.41*** (.13) [.19]	-2.33** (1.05) [1.71]
N	2,465	2,465	2,461
F	9.09	10.19	4.97
Panel B:			
IV Results			
Share of New Guards (Rotation)		1.9** (.83) [1.54]	10* (5.9) [11.49]
N		2,465	2,461
Month FE:	YES	YES	YES
Building FE:	YES	YES	YES
Neighbourhood Trends:	YES	YES	YES
Avg Chars of Guards:	YES	YES	YES

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 $r_t \in \mathcal{R} \equiv \{0, 1\}$ 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 $d_t \in \mathcal{D} \equiv \{0, 1\}$ 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 the exposition by leaving the past actions of the client d_0, \dots, d_{t-1} out of the histories prior to time t : $h_0 = \emptyset$, and $h_t = \{r_0, \dots, r_{t-1}\} \forall t \geq 1$. Finally, we use \mathcal{H}_t to denote the set of all possible histories at the beginning of period t (provided that the game has not ended by then), and define $\mathcal{H} = \cup_{t=0}^{+\infty} \mathcal{H}_t$.

We are now ready for the formal analysis. As a preliminary 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:

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

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 is about to 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 σ_0^2 is sufficiently large, 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:

$$\sum_{t=T-1}^{\bar{T}} \rho^{t-(T-1)} (u(-p) - \mathbb{E}[u(y_t - w)]) > \sum_{t=\bar{T}+1}^{+\infty} \rho^{t-(T-1)} (\mathbb{E}[u(y_t - w)] - u(-p)). \quad (\text{B.2})$$

Note that for any fixed $T \in \{1, \dots, \bar{T}\}$, as $\sigma_0^2 \rightarrow +\infty$ the RHS of (B.2) goes to $-\infty$, while the LHS (B.2) converges to $+\infty$. Hence, the inequality (B.2) holds whenever σ_0^2 is sufficiently

large. Moreover, for all $\rho > 0$, (B.2) is further equivalent to

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

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 $\bar{T} \leq T$. In sum, for a fixed and sufficiently large $\sigma_0^2 > 0$, there must exist a unique cutoff $T^* \in \{1, \dots, \bar{T}\}$, such that (B.1) holds if and only if $T < T^*$.

From now on, we assume that σ_0^2 is sufficiently large 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 $r^* : \mathcal{H} \rightarrow \mathcal{R}$, 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 $d^* : \mathcal{H} \times \mathcal{R} \rightarrow \mathcal{D}$, where

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

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 $\sum_{s=t-(T^*+1)}^t r_s > 0$. 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 $\sum_{s=t-(T^*+1)}^t r_s = 0$, 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 an 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

$$\Pi(T) = \sum_{k=1}^{+\infty} \sum_{t=0}^{T-1} \delta^{(k-1)T+t} [p - w + \mu_0 + \alpha t]. \quad (\text{B.4})$$

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

$$\begin{aligned} \Pi(T+1) &= \sum_{k=1}^{+\infty} \sum_{t=0}^T \delta^{(k-1)(T+1)+t} [p - w + \mu_0 + \alpha t] \\ &= \sum_{t=0}^T \delta^t g(t) + \sum_{t=0}^T \delta^{T+1+t} g(t) + \dots + \sum_{t=0}^T \delta^{k(T+1)+t} g(t) + \dots \\ &> \sum_{t=0}^{T-1} \delta^t g(t) + \sum_{t=0}^T \delta^{T+t} g(t) + \dots + \sum_{t=0}^T \delta^{k(T+1)-1+t} g(t) + \dots \\ &> \sum_{t=0}^{T-1} \delta^t g(t) + \sum_{t=0}^{T-1} \delta^{T+t} g(t) + \dots + \sum_{t=0}^T \delta^{k(T+1)-2+t} g(t) + \dots \\ &> \sum_{t=0}^{T-1} \delta^t g(t) + \sum_{t=0}^{T-1} \delta^{T+t} g(t) + \dots + \sum_{t=0}^{T-1} \delta^{kT+t} g(t) + \dots \\ &= \Pi(T). \end{aligned}$$

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. \square

B.2 Proof of Proposition 2

Recall that T^* is the largest T that satisfies (B.1). Replacing the random variable $y_t - w$ with its certainty equivalent for the client, we can rewrite (B.1) as

$$\sum_{t=T-1}^{\bar{T}} \rho^t \left(u(-p) - u \left(\mu_0 + \alpha t - \frac{a}{2} \sigma_0^2 - \beta t - w \right) \right) \quad (\text{B.5})$$

$$\geq \sum_{t=\bar{T}+1}^{+\infty} \rho^t \left(u \left(\mu_0 + \alpha t - \frac{a}{2} \sigma_0^2 - \beta t - w \right) - u(-p) \right). \quad (\text{B.6})$$

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 α, β and μ_0 , and is increasing in σ_0^2 . It then follows that (B.5) is also decreasing in α, β and μ_0 , and is increasing in σ_0^2 , while (B.6) is increasing in α, β and μ_0 , and is decreasing in σ_0^2 . This implies that the inequality becomes more stringent as α, β or μ_0 increases, or as σ_0^2 decreases. Therefore, T^* is decreasing in μ_0, α and β , and is increasing in σ_0^2 . \square

B.3 Extension I: Endogenous Service Fees

In the main text, we made a simplifying assumption that the service fees charged by the firm are exogenous and constant over time. We will now relax this assumption and show that our main insight – that the firm may strategically use job rotation to counter the poaching risk of its employees – continues to hold with a more general contracting space.

Formally, suppose that at time zero, the firm can offer the client (with commitment) a contract (\mathbf{p}, \mathbf{r}) specifying the service fee $p_t \in \mathbb{R}_+$ and the rotation scheme $r_t \in \{0, 1\}$ at every period $t = 0, 1, \dots + \infty$.³⁴ It is clear that if poaching is prohibited, rotation can only destroy surplus but will not bring any benefit, so at optimum the firm would always send the same worker to the client. Now suppose that, as in the baseline model, the client is free to bring the worker assigned to her in house at any time t . Thus, the firm would anticipate that any contract it offers will either induce the client to poach a worker at some finite time $T_P \in [0, +\infty)$, or to always purchase the service from the firm (in which case we denote $T_P = +\infty$). Taking the client's poaching incentive into account, the firm chooses

³⁴Allowing the firm to commit to the rotation policy enlarges the contracting space of the firm, which strengthens the optimal contracting result obtained in this section. The non-negativity constraint of the per-period service fees simplifies the analysis but is not crucial either. We can, for instance, assume more generally that $p_t \geq \underline{p}$ for some arbitrary price lower bound $\underline{p} > -\infty$ (i.e., it is sufficient that the firm cannot subsidize the client without limit).

a contract that maximizes its expected total profit. The following proposition shows that if the firm sufficiently values its long-term revenues, any optimal contract must include a positive frequency of rotation.

Proposition B.1. *Suppose that σ_0^2 is sufficiently large so that Proposition 1 applies for some constant service fees $p > w$. There exists $\hat{\delta} \in (0, 1)$, such that if $\delta \geq \hat{\delta}$, then in any optimal contract the firm will rotate its workers, i.e., $r_t = 1$ for some $t \geq 1$. In particular, any contract (\mathbf{p}, \mathbf{r}) that adopts a no-rotation policy (i.e., $r_t = 0 \forall t \geq 1$) will yield a lower expected profit than the contract $(\tilde{\mathbf{p}}, \tilde{\mathbf{r}})$ that charges a constant service fee $p > w$ and rotates the workers with the optimal frequency defined in Proposition 1.*

PROOF. Since the client is free to poach the firm's worker at any time, for poaching to take place no earlier than time $T_P \in [0, +\infty]$, it is necessary that

$$p_t \leq \bar{p} \equiv -u^{-1} \left(\sum_{s=0}^{+\infty} \rho^s \mathbb{E}[u(y_t - w)] \right) \quad (\text{B.7})$$

for all $t \leq T_P$, where $y_t \sim \mathcal{N}(\mu(t), \sigma^2(t))$. In addition, note that there must exist $\tilde{T} \leq (a\sigma_0^2 - 2\mu_0)/(2\alpha)$, such that

$$\mathbb{E}[u(y_t - w)] = u \left(\mu_0 + \alpha t - \frac{a}{2} \max\{0, \sigma_0^2 - \beta t\} \right) \geq u(0) \geq u(-p_t)$$

for all $t \geq \tilde{T}$, where $y_t \sim \mathcal{N}(\mu(t), \sigma^2(t))$. This implies that for any contract that involves a no-rotation policy, poaching will for sure take place no later than period \tilde{T} . Together with (B.7), we can obtain a uniform upper bound on the expected profit generated by any of such contracts. Specifically, for all (\mathbf{p}, \mathbf{r}) with $r_t = 0 \forall t \geq 1$, we have

$$\Pi(\mathbf{p}, \mathbf{r}) \leq \sum_{t=0}^{\tilde{T}} \delta^t (\mathbb{E}[y_t] + p_t - w) \leq \bar{\Pi}_0 \equiv \sum_{t=0}^{\tilde{T}} \delta^t (\mu_0 + \alpha t + \bar{p} - w) < +\infty.$$

Now consider a contract $p_t = p > w \forall t \geq 0$ and $r_t = 1$ if and only if $t = kT_R$ for some $k = 0, 1, 2, \dots$, where $T_R \geq 1$. Provided σ_0^2 is sufficiently large, Proposition 1 implies that there must exist $T_R^* \geq 1$, such that if $T_R \leq T_R^*$, the client will never poach the worker assigned to her. It is then straightforward to check that the expected profit that the firm

can obtain from this contract will satisfy

$$\lim_{\delta \rightarrow 1} \Pi(p, T_R^*) = \sum_{k=1}^{+\infty} \sum_{t=0}^{T_R^*-1} \delta^{(k-1)T_R^*+t} [p - w + \mu_0 + \alpha t] \geq \lim_{\delta \rightarrow 1} \sum_{t=0}^{+\infty} \delta^t (p - w) = +\infty.$$

Thus, we have shown that for δ sufficiently close to 1, any contract that does not rotate workers will be dominated (in terms of the firm's expected payoff) by a contract that does. In other words, provided the firm sufficiently values its revenues in the long run, it will include job rotation in the optimal contract. This is a strategic response to the presence of poaching risk, because, as we have argued, a no-rotation policy would have been optimal if poaching was prohibited. \square

B.4 Extension II: Private Match Value

In this section, 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:

$$T(v_h) < 1 < T(\bar{v}) < T(v_\ell) < 2, \tag{B.8}$$

where $\bar{v} = qv_h + (1 - q)v_\ell$ and the mapping $T(\cdot)$ 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.2. *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 ρ 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

$$(1 - q)(-w + p + \mu_0 + \alpha) \geq -w + p + \mu_0, \quad (\text{B.9})$$

which holds whenever q is sufficiently small. Further, provided that (B.9) holds, at $t = 1$ the firm will indeed prefer not to rotate its worker if

$$\begin{aligned} & (1 - q)[-w + p + \mu_0 + \alpha + \delta(-w + p + \mu_0)] \\ & \geq -w + p + \mu_0 + \delta(1 - q)(-w + p + \mu_0 + \alpha), \end{aligned}$$

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

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

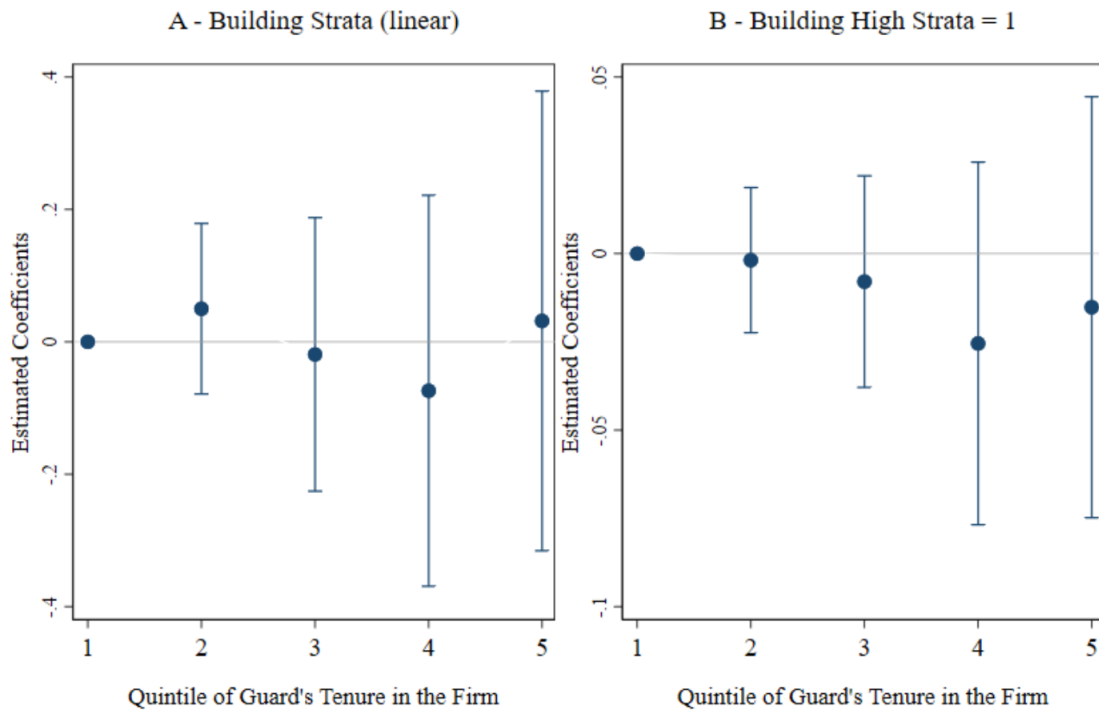
which is guaranteed to hold given the assumption $T(\bar{v}) > 1$. If the firm did not rotate the worker in the beginning of the period, then the client must have learned her private match benefit with that worker. Given $T(v_h) < 1$, the client will for sure prefer to bring that worker in house if $v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

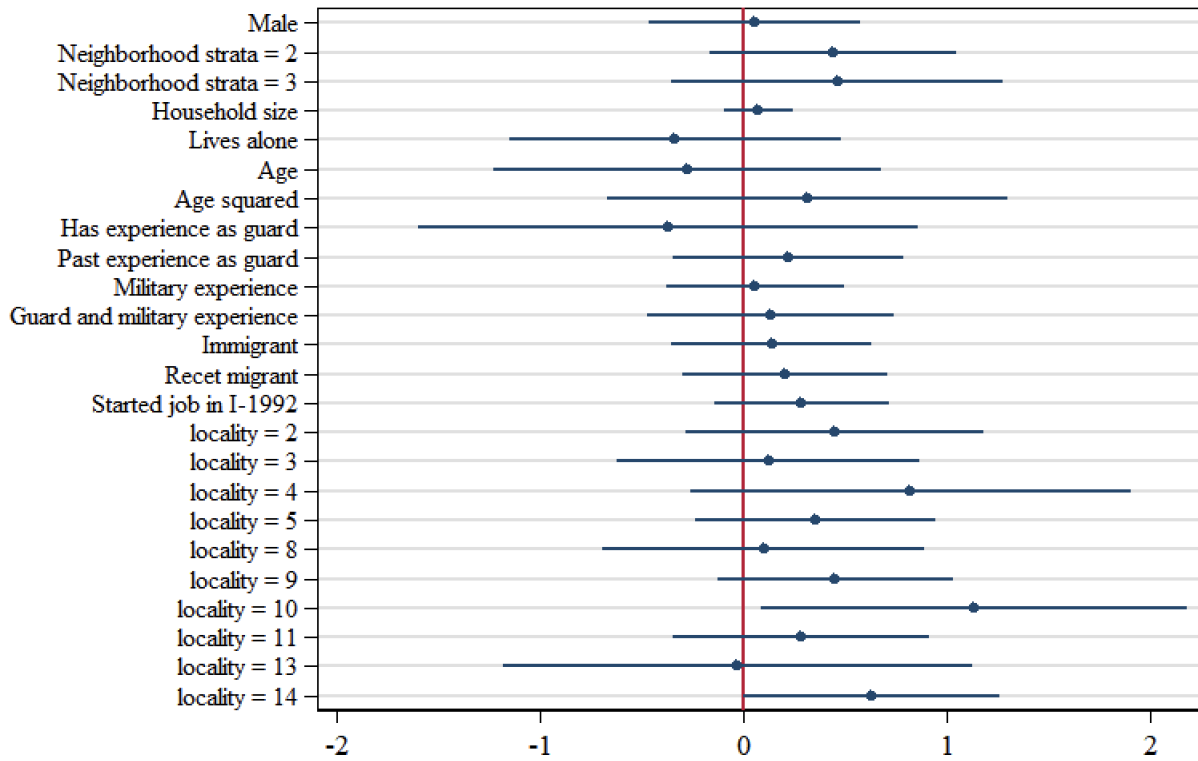
Finally, it is also straightforward to check that condition (B.8) guarantees that the client will not have the incentive to deviate to poach the fresh worker assigned to her at $t = 0$. \square

C Additional Figures and Tables



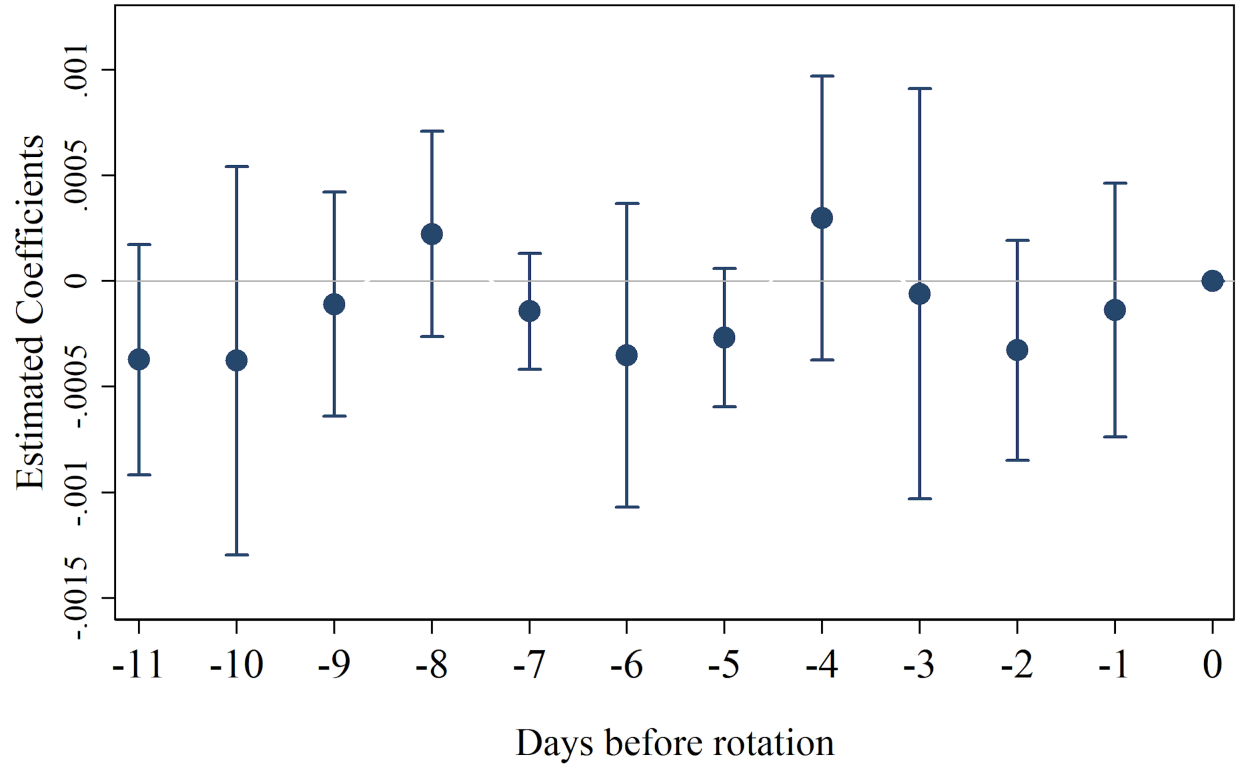
This figure displays the estimated coefficient and the 95% confidence intervals of regressions of the building's strata and indicators for the quintile of guard's tenure within the firm. The regressions have controlled for both guard fixed effect and month fixed effect. In Panel A, the dependent variable is the socioeconomic strata of neighborhood where the building is located (which takes values 0 to 6). In Panel B, the dependent variable is an indicator of building located at a high socioeconomic strata (stratas 5 and 6). Standard errors are clustered at the guard level. $N = 656,438$.

Figure C1: Building Socioeconomic Strata and Guard's Tenure



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 C2: Balance Tests for 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 C3: Evolution of Crime Before Rotation

Table C1: The Matching Between Guards and Buildings

	(1)		(2)	
	All Pairs of Guard-Building		Only First Building Assigned	
	F	(Prob F>0)	F	(Prob F>0)
Dependent Variable:				
N of Flats in the Building	0.85	(0.63)	0.88	(0.60)
N of Required Guards	1.50	(0.11)	1.46	(0.13)
Socioeconomic Strata of Neighborhood	0.59	(0.89)	1.64	(0.07)
High Strata of Neighborhood	1.01	(0.45)	2.04	(0.02)
City Area = South	2.03	(0.02)	1.07	(0.40)
City Area = Center	0.92	(0.56)	0.85	(0.63)
City Area = West	0.41	(0.98)	0.40	(0.98)
City Area = East	0.91	(0.57)	0.79	(0.70)
N	1,559		625	

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 clustered at the building level.

**Table C2: Productivity and Client-Specific Experience
(non-parametric control for total experience)**

	(1)	(2)	(3)	(4)
Panel A: Crime Ocurrred During Guard's Shift				
Log Experience in Building ($\div 100$)	-.012** (.0054)	-.0097* (.0055)	-.0096* (.0055)	-.036* (.021)
N	656,438	656,438	656,438	656,438
Panel B: IHST Value of Property Lost in Crime				
Log Experience in Building ($\div 100$)	-.16** (.073)	-.14* (.075)	-.14* (.075)	-.51* (.29)
N	656,438	656,438	656,438	656,438
Method:	OLS	OLS	OLS	IV
Total Experience Quintiles:	YES	YES	YES	YES
Guard \times Building FE:	YES	YES	YES	YES
Shift FE:	YES	YES	YES	YES
Month FE:	YES	NO	NO	NO
Building Neighb \times Month FE:	YES	YES	YES	YES
Week FE:	NO	YES	YES	YES
Shift \times Day of Week FE:	NO	NO	YES	YES

All regressions are at guard \times shift level. In Panel A, the dependent variable is an indicator for a crime occurring during the shift of the guard in the building. In Panel B, the dependent variable is the inverse hyperbolic sine transformation (IHST) of the estimated value of the property stolen or destroyed during the crime. All regressions control for the number of shifts that the guard worked during the month. In Column (4), the accumulated experience of the guard in the building is instrumented with the interaction between an indicator for guard type-II and the tenure of the guard within the firm. Robust standard errors clustered two-way at guard and at week level. First stage F statistics is 632.28. Experience variables are divided by 100 (i.e. coefficients are scaled up by 100). All regressions include dummies for the quantiles of total experience.

**Table C3: Poaching and Client-Specific Experience
(non-parametric control for total experience)**

Guard Hired by Building (Pre-Law)				
	(1)	(2)	(3)	(4)
Log Experience in Building ($\div 100$)	.18*** (.053)	1.2*** (.34)	.25*** (.066)	1.7*** (.41)
N	40,099	40,099	40,099	40,099
F first-stage		401.15		403.21
Method:	OLS	IV	OLS	IV
Total Experience Quintiles:	YES	YES	YES	YES
Guard FE:	YES	YES	YES	YES
Week FE:	YES	YES	YES	YES
Building \times Month FE:	YES	YES	NO	NO
Building \times Week FE:	NO	NO	YES	YES

All regressions are at guard (building) \times week level. The dependent variable is an indicator for the week when the worker is hired in-house by the building and the sample is restricted to the period before the policy introduction. All regressions control for the number of shifts that the guard worked during the month and the share of night shifts worked in the week. In Column (4), the accumulated experience of the guard in the building is instrumented with the interaction between an indicator for guard type-II and the tenure of the guard within the firm. Robust standard errors clustered two-way at guard and at week level. Experience variables are divided by 100 (i.e. coefficients are scaled up by 100). All regressions include dummies for the quantiles of total experience.

Table C4: Predicted Poaching Risk

	(1) Correlation with Baseline Chars	(2) Gini-Based Importance
Male	.14*** (.0094)	0.051
Military Experience	.012* (.0073)	0.022
Neighborhood Strata	-.0087 (.009)	0.029
Household Size	.028*** (.0046)	0.110
Lives Alone	-.041*** (.014)	0.016
Age	-.012*** (.0042)	0.171
Past Experience	-.023*** (.0074)	0.130
Had Experience as Guard	.042*** (.01)	0.025
Immigrant	.02* (.012)	0.023
Years Since Migration	-.073*** (.012)	0.169
Recently Migrated	.066*** (.019)	0.032
Neighborhood of Residence FEs (Std Error/Combined Importance of FEs)	.016	0.221
N	526	
R ²	.72	

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.

**Table C5: Correlation between Poaching Risk of the Guards
Rotating In and Out of the Building**

	(1) Risk of Incoming Guard	(2) Risk of Incoming Guard
Poaching Risk of the Guard	.054 (.035)	-.018 (.035)
N	784	784
R2	.005	.14
Building FE:	NO	YES

This table investigates the correlation between the estimated poaching risk of the guard rotating out from a building with the estimated poaching risk of the substitute guard rotating into the building. All regressions include a dummy for the period before the non-poaching law. Standard errors clustered at the building level.

**Table C6: Effect of the Policy on Guards’s Rotation and Crime
Alternative Estimation of Poaching Probability**

Dependent Variable	(1) Rotated	(2) Avg Shifts per Building	(3) N Crimes
Post × Poaching Risk	-0.11*** (.036) [.057]	1.4** (.53) [.78]	-.0024 (.039) [.036]
N	13,750	13,750	13,750
Indiv Chars:	YES	YES	YES
Month FE:	YES	YES	YES
Guard FE:	YES	YES	YES
Guard Trends:	YES	YES	YES
Building (most worked) FE:	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. 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 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 C7: Effect of the Policy on Guards' Rotation and Crime Regressions at the guard \times week level

Dependent Variable	(1) Rotated	(2) Avg Shifts per Building	(3) N Crimes
Post \times Poaching Risk	-.026*** (.0078)	1.2*** (.34)	-.0043 (.007)
N	389,164	389,164	389,164
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 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).