Talent Poaching and Job Rotation

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Abstract

The value of a firm's service lies both in its workers and its relationship with clients. In this paper, we study the interaction between client-specific experience accumulated by workers, poaching behaviour from clients and strategic rotation of workers by firms. Using detailed personnel data from a security-service firm, we show that an increase in client-specific experience increases both the productivity of workers and their probability of being poached. The firm reacts to this risk by rotating workers across multiple clients, and more frequently so to those workers more likely to be poached. We show that after a policy change that prohibited poaching, the firm sharply decreased the frequency of rotation which in turn increased workers' productivity. We propose a theoretical model that guides the empirical patterns and allows us to argue their external validity beyond our specific empirical setting.

Keywords: talent poaching, job rotation, outsourcing

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

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1 Introduction

A well-documented and widespread feature of labor markets is that firms take actions to avoid that their workers leave and work for competitors (Aghion and Bolton, 1987; Krueger and Ashenfelter, 2018; Lipsitz and Starr, 2021). This concern has become less important overtime because across industries and countries, firms increasingly rely on service providers to undertake jobs that were previously carried by their own workers (e.g., Goldschmidt and Schmieder, 2017; Dorn et al., 2018). However, this significant labor market change increases the prominence of a less-studied but also important concern for service firms: their workers can leave and work for clients.

On the job, outsourced workers accumulate experience that make them more productive to clients. However, after a worker has acquired sufficient skills specific to a client, that client may want to hire the worker in house. Anticipating this potential loss of both employee and client relationships, the service firm may take costly actions to avoid poaching.[1] We argue that one of these strategies consists in rotating workers from one client to another. By doing so, the firm hinders workers' acquisition of client-specific skills (henceforth CSS), so that workers remain sufficiently unattractive to the clients.

We are not aware of any existing study that quantifies how severe the phenomenon of client's talent poaching is. Nevertheless, media coverage and public discussions suggest that many and various types of firms and clients do care about this type of poaching. For instance, there is registered involvement of poaching suppliers' employees for 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 occupations (high- and low-skilled) and industries, including nursing (DLA Labor Dish Editorial Board, 2014), cleaning (Shubber, 2018), engineering (Chaput, 2018), marketing (Liffreing, 2018), managerial services (StevensVuaran Lawyers, 2019), travel advising (Pestronk, 2019), and game publishing (Schreier, 2020) among many others. It is therefore not

[1] This type of strategic response is a familiar problem in antitrust law. For instance, it is known that if firms are prohibited from anti-competitive behaviour such as merger acquisition, price collusion or exclusive contracting, they may resort to other "inefficient" practices such as 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).

[2] See 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.

surprising that the issue has drawn public attention in diverse countries, such as Australia (StevensVuaran Lawyers, 2019), Canada (Chaput, 2018) and the US (Bennet, 2018).

Despite the prevalence and importance of this poaching problem, research on this topic has been limited, probably due to the lack of a comprehensive database that collects information of the transition and performance of service workers across multiple clients and its poaching behaviour. To overcome this challenge, we partner with a security-service industry firm. The empirical setting is adequate to study the issue of poaching for two reasons. First, in the middle of our sample period, the country in which our partner firm is, implemented a non-poaching policy, giving exogenous variation to the extent that poaching behaviour is allowed. Second, we have very rich data. During 74 months, the firm allocated 589 guards to 116 residential buildings on a daily basis. For each guard, we know her socio-demographic information as well as when and where she worked. For each building, we have information about its size and location. In addition, the data contains two measures of poaching intensity: whether a guard received a formal solicitation from a building, and whether a guard was hired in-house by a building. Finally, we also have information on one of the most important measure of guards' productivity: crime committed in the building while the guard was working.

We present three main empirical results. First, guards with more client-specific experience are more effective at reducing crime but are also more likely to be poached. Second, the security firm responds to this poaching concern by rotating guards across buildings, especially those with a higher poaching risk (e.g. men living in large households). Third, an anti-poaching legislation reduces both rotation and crime.

The first result studies the relationship between the CSS of a worker and the poaching decision of the client. We find that an increase in the length that the guard has worked for a specific building increases her probability of being poached by that building, even after controlling for her total working experience. We show that this is because the skill that a guard acquires by working with the same client is important for her productivity: As a guard accumulates more working shifts in a building, the probability that a crime occurs in that building and the expected value of stolen properties decrease.[3]

[3]Huckman and Pisano (2006) find a similar relationship between the quality of a cardiac surgeon's performance at a given hospital and her recent volume of surgeries at that hospital.

To address the potential endogeneity bias arising from omitted variables and reverse causation, we adopt an instrumental variable (IV) approach (for both crime and poaching as dependent variables) based on the system that the firm uses to allocate guards to shifts. We exploit the fact that the firm randomly (first-come-first-serve basis) divides guards into two types, type-I and type-II. The former type is allocated to a unique building to cover weekly shifts. The latter type is assigned to different buildings to cover daily shifts when type-I guards rest. This allocation creates a mechanical variation in the client-specific experience between the two types of workers. That is, a type-I guard accumulates more shifts than a type-II guard when both work in the same building during the same number of weeks. The IV results confirm the positive relationship between client-specific experience and observed poaching and crime. In particular, a 10% increase in the building-specific experience is associated with 6 (4) additional percentage points in the probability of being poached (witnessing a crime) by the corresponding building.

We complement the above analysis with an event study around the rotation of guards to understand better how crime rates vary before and after rotation events. As expected from the previous IV results, we show that once a guard is rotated to a new building (a reset on the accumulation of CSS), there is an increase in crime incidence and value of property lost in the building in which the rotated guard arrives. The average effect of these estimates represents about 28% of the mean of the dependent variable.

The second empirical result shows that the firm rotates more often those guards at a higher risk of being poached. To estimate the poaching risk, we exploit the fact that buildings prefer to hire directly guards with certain baseline characteristics. Based on these features, we construct a cross-section worker-specific index of poaching risk (using a machine learning approach) and we show that the rotation of guards is highly correlated with this index. 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 estimate is sizable as it corresponds to 40% of the mean of the dependent variable.

The third and last main empirical result exploits a policy change that *de facto* limited buildings from directly hiring guards in-house. If the security company rotates workers with the aim of limiting their CSS acquisition, and therefore to decrease the probability of being captured by the clients, this rotation should decrease once the policy change takes effect.

Consistent with this intuition, we show that the guards more likely to be poached before the policy change were rotated less intensively once the policy took effect. More precisely, one standard deviation increase in the poaching risk is associated with a reduction of 2 percentage points in the probability of rotation after the policy change. The magnitude of this effect is large (80%) compared to the average monthly rotation before the policy took effect. We complement this result by showing that those guards that were less often rotated were also those that increase the most their productivity after the policy change (i.e. the largest decrease in crime).

Taken together, our empirical findings suggest that the firm strategically rotated its workers excessively to avoid them being poached. Then, when a non-poaching policy takes place, the firm reduces rotation allowing workers to acquire larger CSS and as a consequence crime rates decreased. An important lesson from our results is that in contexts in which service companies take costly actions to avoid poaching, a policy that prohibits poaching talent can increase productivity of the workers.

A potential 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, we develop a theoretical model that captures the strategic tension arising from our empirical setting, and the consequent trade-off faced by the service-providing firm. We consider a firm that employs a worker and transacts with a client. The client pays a service fee for outsourcing a production activity to the firm. As the worker accumulates productivity-increasing experience by performing the client's activity, the client may find it cost-efficient to hire the worker directly. We show that the firm over-rotates its workers before they reach a client-experience threshold. In equilibrium, the workers with more desirable characteristics (e.g., larger baseline productivity –which are those with a higher poaching risk–) are rotated more often. As a result, this model shows that a non-poaching policy can increase workers' productivity by eliminating strategic over-rotation.

Related literature The literature has 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). As we will show, these familiar hypotheses do not seem to be consistent with our empirical setting.[4] Instead, our paper proposes and demonstrates a totally different rationale for job rotation — it can be used as an organizational remedy to mitigate poaching risk.

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, firms provide both general and job-specific skill training to its workers. It has been well understood that if the firm cannot avoid poaching from its competitors (because *non-poaching agreements* between employers operating in the same product market are 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 clients, the acquisition of job-specific skill may also be distorted.

Finally, it is known that the problem of firm-sponsored general-skill provision can be alleviated by *non-compete 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., 2020, 2021; Lipsitz and Starr, 2021). 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 client).

[4]For instance, a relevant agency problem in our context might be the collusion between guards and criminals (or judges and criminals – Bhuller et al. (2020) –). However, this implication is at odds with our empirical finding in which crime decreases the longer is the building-specific tenure of the guard.

However, while policy makers tend to be against non-compete clauses (e.g., Dougherty, 2017), our paper provides both a new theoretical rationale and empirical evidence to make the case for a non-poaching policy: it can enhance productivity (e.g., improve crime prevention in our setting).

The remainder of the paper is organized as follows. Section 2 describes the institutional setting of our study. In Section 3, we develop a theoretical model to accentuate the key trade-off of the setting and to guide the subsequent empirical analysis. Sections 4 and 5 present our main empirical results. Section 6 concludes. All figures, tables, proofs and additional results are contained in Appendices A, B and C .

2 Institutional Setting

We have partnered with a private security firm in 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. Our sample consists of 589 security guards allocated to 116 buildings. For each guard, we have information on when and where 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).

2.1 Relationship Between the Security Firm and Buildings

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).[5] After 12 working days, the guard rests two days. Most guards are allocated to work in a unique building for several months. However, about 15% of guards work exclusively covering the resting 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

[5] There are very few occasions when guards slightly depart from this schedule. For instance, illness episodes of one guard can result in other guards working overtime.

guards working in the same building (as they rest in different moments). Thus, in a given week, a building typically needs two type-I guards and one type-II guard to cover all the shifts.[6]

The allocation of guards to type I or II does not depends on the characteristics of the guard but it is determined by the needs of the firm when the guard starts her job. Jobs are not advertised as type I or II and contractually they do not differ. Therefore, the match between the type and the characteristics of the guard can be plausibly assumed as exogenous. In Section 4.2, we show empirically that guards' characteristics do not explain the allocated guard's type (i.e. I or II) or attributes of the building that they are assigned to.

Panel A of Figure 1 illustrates a typical timetable of three guards working in the same building in a period of 16 days. The two type-I guards are labelled as e1-A and e1-B, while the type-II guard is labelled 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. Type-II guard e2 also works 12 days in a roll before he rests for two days. Hence, as Panel B of Figure 1 illustrates, guard e2 is rotated every two days to a different building, so her full schedule of shifts is completed and once he has reached days 15 and 16, she rests (dark areas in Panel B denote resting time for guard e2).

Note that according to the above schedule, different types of guards accumulate a different number of shifts in the same building while working the same time span. In particular, during the same period of 16 days, guard e1-A accumulates 14 shifts in building 1 whereas guard e2 only accumulates 4 shifts.

The private security firm transacts with multiple residential buildings. During the whole sample period the Colombian legislation prohibited any type of firm from using any formal contracts (e.g., non-compete clauses) to restrict the possibility of workers being poached by other firms in the same product-market. However, before 1994, it was legally possible that residential buildings poached security guards. As usual in other contexts, poaching took place without the consent of the service-provider. We argue in this paper that the security firm rotated workers from one building to other to avoid poaching. In case these rotations

[6]Some large buildings require more than one guard working at the same time. The logic of allocation and replacements works in the same way.

occurred, they were typically communicated to both the building and the guard about one week before the actual rotation took place.

Conversations with buildings that poached workers show that when buildings poach, they usually have other potential guards to cover the remaining shifts. These potential workers tend to be guards that used to work for the building or others referred by residents, past guards or newly in-house guards. When a building poaches a guard, the firm terminates the contract with the building. The non-poached guards working in the poaching building are typically transferred to another building either instantaneously or after some time. For each of these poaching cases, we observe the identity of the hired guard, the building that poach her and the exact date that the guard leaves the security company. A building can poach a worker that is currently working or has worked for them in the past. In our data, all the poaching episodes occur while the guard was working in the building.

Although it was possible for the buildings to post a vacancy and hire guards directly before the policy change, some buildings preferred to outsource these positions because (i) the security company has a comparative advantage in performing the job due to the economies of scale (e.g., it may bind the needs of different clients through the training and management of a large set of employees), and (ii) the company (acting as an insurance provider) pays a fraction or the totality of the stolen items to the building if a crime occurs. The magnitude of this fraction depends on the proven responsibility that the guard had in the crime. We do not have information of the amount of money paid in each crime episode.

Finally, buildings always provide all the materials/amenities (staircase, heater, etc) that can increase guards' productivity at the beginning of the contractual relationship. Failure to fill this condition will leave the building uninsured in case of crime.

2.2 Client-Specific Skills in Our Context

One of the most important tasks of a guard is to control the entry into buildings. When a visitor arrives, the guard asks the flat that the visitor wants to go to whether or not the visitor is welcome. 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 distinguishing residents and frequent visitors from the rest. Recognizing those residents and visitors is a CSS. Naturally, this skill increases over time as guards become more familiar with the identities of residents or those who visit the building frequently. However, without sufficient experience in the building a guard is not able to screen unwanted visitors (e.g., thieves) from others. Hence, an inexperienced guard either makes everyone pay the transaction costs, or overlooks the entry of unwanted visitors.

The ability to recognize visitors and the inner workings of the building, which are accumulated over time, allow guards to prevent crime more efficiently.

3 Theory

Before proceeding to analyse the data, we present a dynamic agency model that accentuates the key tension arising from our empirical setting: the accumulation of client-specific skills and the exposure to poaching risk. Our goal with this model is two-fold: (i) to provide a rational framework to think about how service-providing firms in general, and our security firm in particular, can effectively deal with employee poaching through strategic rotation; (ii) to obtain formal and testable predictions that guide the subsequent empirical analysis.

We consider a client (or a client-firm, she) that repeatedly engages in a production activity at period $t = 0, 1, 2, ... + \infty$. The client is risk-neutral and has a discount factor $\rho \in (0, 1)$. Performing the activity requires a unit input of labour (of a worker, he) at every period. At the beginning of the game, the client does not have a worker in house, so she outsources the activity to a firm.[7] This service-firm is also risk-neutral, and has a discount factor $\delta \in (0, 1)$.

In every period, the players interact with each other according to the following timeline (see Figure 2 for a graphical illustration). First, the service-firm chooses a worker to assign

[7] This assumption is motivated by an empirical observation: clients do not recruit guards directly from the labour market. This assumption is satisfied if service firms have increasing returns to training guards and/or service firms are more efficient in screening workers in the labour market (because the firm is more experienced or has a specialized recruiting team; see Vohra (2021) for a theory of how poaching may also impact firms' screening incentives in hiring junior workers).

to the client. The firm can either send the same worker to the client as in the previous period, or appoint a new worker to replace the previous one. If the client decides to accept the service, she pays a fee $p > 0$ to the firm. In this case, the firm receives a flow pay-off: $\pi_t = p - w - k_s(e_t)$. Here, $w > 0$ is the wage that the firm pays to its employees. Further, $k_s(\cdot)$ captures the non-wage production cost that the firm incurs (e.g., the expected costs of protecting/insuring the client from adverse events). We assume that $k_s(\cdot)$ is a strictly decreasing function of the client-specific experience ($e_t \in \mathbb{N}$) of the worker. Thus, client-specific experience is valuable in the sense that it increases the efficiency of the worker's production (due to the worker getting more proficient at completing his task, more adapted to the working environment, and so on). For simplicity, we also assume $p > w + k_s(0)$, so that regardless of the worker's experience, it is always profitable for the firm to provide the outsourcing service.

If the client does not want to buy the service from the firm, she may hire the assigned worker by paying him wage w (or $w+\varepsilon$ for some small $\varepsilon > 0$).[8] Poaching the worker will end the contractual relationship between the service-firm and the client, so the client will have to carry out the production activity herself from then on, while the firm may only receive its reservation pay-off (which we normalize to zero). Specifically, in-house production yields a flow pay-off $u_t = v - w - k_c(e_t)$ to the client, where $k_c(\cdot)$ is a strictly decreasing function of the worker's experience specific to that client, and v measures the additional benefit/cost for the client to have the assigned worker as a regular employee rather than an outsourcing staff. For instance, in-house production may have the advantage of lower supervision costs due to better career incentives (Abraham, 1988). We assume that v is drawn according to a commonly known probability distribution with $\Pr(v = v_h) = 1 - \Pr(v = v_\ell) = q \in (0, 1)$, where $v_h > v_\ell$, and it is i.i.d. across workers (e.g., some workers may be more responsive to career incentives than others). The exact value of v is unknown to all players ex ante. However, in every period, prior to making the poaching decision the client receives a private signal ϕ (which is i.i.d. across periods and across workers) about the true match value of the worker assigned to her in that period. In particular, with probability $\lambda \in (0, 1)$, the signal is fully revealing ($\phi = v$). With the remaining probability $1 - \lambda$, the signal is

[8] The results are qualitatively similar if we instead assume that the poaching decision of the client is made after she receives the service from the firm.

uninformative ($\phi = \emptyset$).[9] After the client makes the poaching decision, the stage game ends and the instantaneous pay-offs are collected.

The equilibrium analysis of our model is non-trivial because the poaching incentive of the client is endogenous to the firm's rotation scheme. Therefore, it is useful to start by considering the simplest scenario where the firm always sends the same worker to the client. At the beginning of period t, the worker has accumulated t units of experience in performing the client's activity, which we shall refer as the worker's CSS. As a result, for a given match value v, the client receives a higher instantaneous utility by hiring the worker internally than purchasing the service from the firm if and only if:

$$-p \leq v - w - k_c(t). \tag{1}$$

Since $k_c(\cdot)$ is strictly decreasing, there exists a unique cut-off $T(v) \in \{0, 1, 2, ..., +\infty\}$, such that (1) holds if and only if $t \geq T(v)$. In particular, provided that $k_c(0) > v - w + p > \lim_{e\to+\infty} k_c(e)$, we have $0 < T(v) < +\infty$. 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 $t \geq T(v)$. 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 production cost at time $t < T(v)$, i.e., when the worker has not yet accumulated *sufficient* CSS. Hence, consistent with the empirical results (presented in Section 4.3), our model suggests that workers with larger client-specific experience are more prone to the poaching risk, in the sense that clients are more inclined to bring them in house. In particular, if the firm never rotates the worker (or if the rotation is not sufficiently frequent), it will at most be able to secure its business with the client for $T(v)$ periods. After that, poaching takes place and the firm loses both its employee and client.[10]

We now proceed to show that, in response to the employee poaching problem, the firm may strategically rotate its workers. Although there is no direct cost, rotation destructs

[9] The information asymmetry between firm and client differentiates our study from Ciapanna (2011) because she assumes that service firms perfectly know the benefits of different assignments and hence can make profits by facilitating the matching between consultants and clients.

[10] Even ignoring the costs of recruiting and training a new employee, a poaching episode 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 Appendix B.2 for a formal statement).

productivity by crippling the accumulation of CSS. This implies that the firm rotates workers to reduce employee poaching. Notice that whether the firm can indeed retain its workers by strategically rotating them is not obvious: Anticipating that the current assigned worker will be replaced later, the client might try to bring that worker in house earlier than what she would prefer, even if doing so may incur an instantaneous utility loss. Relying on the idea that a sufficiently impatient client would prefer carrying on the outsourcing relationship with the firm rather than poaching a worker prematurely, our main theoretical result below establishes the existence of a *Perfect Bayesian equilibrium* with strategic rotation.[11]

Proposition 1. *If both ρ and λ are sufficiently small (i.e., the client is sufficiently impatient and likely to be uninformed), then there exists a Perfect Bayesian equilibrium where the service-firm rotates the workers after every $\bar{T}$ periods.*[12]

Proof. See Appendix Section B.1. □

Thus, in equilibrium, the firm rotates its workers whenever they have accumulated sufficient CSS. Moreover, as shown in the proof of the proposition, had the firm rotated workers less frequently, the client would poach even when she is uncertain about the type of the assigned worker. In this sense, the firm strategically uses rotation to mitigate the poaching incentive of the client. Finally, notice that since $\bar{T}$ is decreasing in q (see footnote 11), our proposition also predicts that the frequency of rotation is increasing in the (ex ante) poaching risk of the worker.

One of the main lessons from the model is that the mere possibility of poaching may lead to destruction of workers' human capital. A non-poaching policy will stop this destruction and consequently will increase the accumulation of CSS. However, the policy may not improve

[11] In order to ease the exposition of the result, we will make a few more simplifying assumptions: (i) v_h is sufficiently large so that a worker will be poached immediately if the client knows that he is of high match value (i.e., $T(v_h) = 0$); (ii) v_ℓ is sufficiently small so that a worker will never be poached if the client knows that he is of low match value (i.e., $T(v_\ell) = +\infty$); (iii) the expected match value $\bar{v} = qv_h + (1-q)v_\ell$ is intermediate so that the cut-off period of an uninformed client is strictly positive and finite (i.e., $0 < \bar{T} \equiv T(\bar{v}) < +\infty$), and her short-term preference is strict around that cut-off: $\bar{v} - w - k_c\left(\bar{T}-1\right) < -p < \bar{v} - w - k_c\left(\bar{T}\right)$. All these technical assumptions can be relaxed without changing the main insights of our model, at the cost of a more tedious analysis.

[12] With the specification $k_c(e_t) = \alpha/(1+\beta e_t)$, where $\alpha, \beta > 0$, the condition on the client's time preference (i.e. ρ being sufficiently small) can be replaced by the technological assumption that α is sufficiently large (i.e., the cost of in-house production is substantial for the client when the worker is inexperienced).

welfare for all of the agents in the economy in the same way. To begin with, the service-firm will benefit from the policy because its business with the client will be protected and it could collect a larger surplus from the transaction due to larger CSS of the workers. By contrast, the workers may be worse-off as the policy change cuts the access to valuable outside options. Similarly, clients are also affected because they are not able to poach workers that they like. Therefore, if the additional surplus from worker's CSS accumulation goes only to the service-firm (such as in our baseline model, where the client only gets to pay a constant fee when production is outsourced), then the client will be unambiguously harmed by the policy. Nevertheless, in general the client may also benefit directly from larger CSS even when the worker is not her own employee. In that case, it is possible that the net welfare effect of the policy is positive for the client.

Robustness. The assumptions of constant wages and service fees, which are roughly consistent with what we observe in the empirical setting, greatly simplify the equilibrium analysis and allow us to focus on how job rotation can balance the trade-off between poaching risk and CSS. We will consider the general case where the service fees may vary across periods and are endogenously chosen by the firm in Appendix Section B.2 (so for example the firm has the option to charge a large fee in the beginning and then be permissive about poaching).[13] The exogeneity of wages can also be relaxed, for instance by allowing the worker to further negotiate with the firm or the client.[14] The main insights of our baseline model are robust to these extensions.

Empirical predictions. To sum up, the theoretical analysis highlights that the strategic concern of employee poaching can lead to excessive job rotation. Three clear-cut empirical predictions emerge from our model.

1. Clients are more likely to poach workers with larger client-specific experience (who are more productive).

[13]Our analysis focuses on a monopoly service market. In a competitive setting where multiple firms compete for a client, the service fee may also be pinned down by the zero-profit condition of the firms.

[14]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 (see Englmaier et al. (2014) for a model where the retention of talents requires the firm to share rents with its workers as their job performance and outside option are correlated). If that is the case, job rotation will have the additional benefit of diminishing workers' bargaining power, which should make poaching even more attractive to the firm.

2. The higher the poaching risk of a worker, the more often he will be rotated by the firm.

3. If employee poaching were prohibited, rotation should be merely driven by factors exogenous to our model (e.g. sick leave of workers, new buildings). Hence, the implementation of a non-poaching policy should decrease rotation. As less rotation implies larger accumulation of CSS, the policy change increases workers' productivity.

In the next sections, we present empirical evidence of these theoretical predictions.

4 Data and Empirical Analysis

4.1 Descriptive Statistics

Table 1 provides descriptive statistics of our database. The table summarizes some predetermined characteristics of the guards, such as previous experience working as security guard, military training and various socio-economic variables (gender, age, size of the household, migration status, income level of the neighbourhood of where they live). Most guards are male, have military training and about half of them have 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 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, but we know that the majority of guards earn the minimum wage during the entire sample period and their earnings weakly depend on their total experience and not on whether they are type-I or type-II. The monthly service fee that the firm charges for providing a guard position in a building (which requires of three guards) is about 5 times the monthly minimum wage.

Table 1 also reports variables related to the rotation of guards across buildings. On average, a type-I guard works 26 shifts a month in a building while a type-II works 9 shifts in a building/month. Type-I guards work on average in 1.03 buildings per month and only 2% of them rotate each month. This contrasts with type-II guards who work in average in 2.2 different buildings each month and rotate to a new building with a monthly probability of 4%.

Finally, the bottom part of Table 1 presents summary statistics for buildings. Buildings are relatively large, with an average of 98 flats and require 4.4 different guards to cover all the shifts during a month. The average strata of the neighbourhoods the buildings are located is 2.8. The strata value captures several measures of quality of housing ranging from 1 to 6. Neighbourhoods with larger strata tend to be safer. The average building has about 1.5 crimes in a month. The most common crime is burglary. Stolen properties frequently include items from the common space of the building (ladders, fridges, automobiles, bicycles, motorcycles) as well as electronic appliances and jewelry from flats. The average value of property stolen (when a crime occurs) is about 17 USD. This corresponds to about 16% of the 1993 Colombian monthly minimum wage.

4.2 Allocation of Guards to Buildings and Types

According to the firm, the allocation of guards to buildings and types (I vs. II) 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. In this subsection we present empirical evidence consistent with this explanation.

Guard-Building Match We conduct a number of empirical tests to investigate the magnitude to which the match between guards and buildings can be seen as endogenous based on the observable characteristics of both. Specifically, we run regressions where the dependent variable is a characteristic of the building (e.g. the size of the building, the geographical location, etc.) and the independent variables are the observed baseline characteristics of the guards that work in the building (e.g. gender, age, family size, socio-economic strata of the residence place, etc.). We perform these regressions for all observed guard-building pairs, and also separately for the matches between each guard and the first building which she was sent after 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 (none is larger than 1.8 or significant at 5%). We also check whether guards are rotated to better/worse buildings as their tenure within the firm increases. In Appendix Figure C1 we display the coefficients of a regression of the building's socio-economic strata (which proxies the quality/safeness of the building) and the tenure (quintiles) of the guard, controlling for guard and month fixed effects. Estimated coefficients reject that there is a systematic relation between the building's strata and the tenure of the guard. Altogether, these results are consistent with the fact that the firm allocates guards to buildings independent of their characteristics.

Allocation to Types We empirically test the claim that the assignment of guards to type I or II is exogenous to their baseline characteristics. We run a balance regression of the type of the guard on a set of baseline characteristics of the guard. We report the estimated coefficients of this regression along with the F-test of joint significance in Appendix Figure C2. We find that only one out of 30 coefficients is significant at 5% (dummy for locality 11). Most importantly, the F-joint statistic is low and non-significant suggesting that guards' baseline characteristics do not explain their assignment to either types I or II.

4.3 Client-Specific Experience, Guards' Productivity and Poaching

Building-specific experience and guard's productivity. Although we do not observe all the possible dimensions of guards' performance (e.g. time incurred by visitors at completing the entry registration, trust between residents and guards, etc.), we do have information about the incidence of crime. According to the security firm and buildings, crime is the single most important variable that the parties consider to measure productivity in this setting.

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

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

where $Crime_{ibt}$ is an indicator for the occurrence of crime while guard i worked 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.[15] 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). For this reason, we include pair-specific fixed effects

[15] The inverse hyperbolic sine transformation can be interpreted similarly to the logarithm, but has the advantage of being well-defined for zero and negative values.

δ_{ib} and exploit the variation in building-specific experience *within* each guard-building pair over time. Finally, we also include monthly fixed effects $\gamma_{m(t)}$ to avoid confounding the effect of building-specific experience with systematic changes in crime over time. Further, CSS can affect performance not only through its direct effect but through its indirect effect (overall experience accumulated). Therefore, in order to isolate the direct effect of CSS on performance, we control for the overall (log) experience of the guard $LogTotalExp_{it}$. This variable is identified separately from time fixed effects because not all guards joined the firm at the same time and overall experience also includes experience *prior to* joining our partner firm. We also include neighbourhood interacted with month fixed effects to control for potential trends in crime at the geographical level, 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). The estimated coefficients of building-specific experience are negative and significant. These estimates are large relative to the mean of the dependent variables. Columns (2) and (3) show that results remain almost identical when we control for narrower time fixed effects (like week or 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. In fact, the coefficient that measures the effect of overall experience on crime is not significantly different from zero in the first three columns of Table 2.

Our estimates of equation (2) remain unbiased even in the presence of endogenous matching between the characteristics of guards and buildings as we control for the guard-building pair fixed effects. 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: guards are allocated to work as type-I or type-II based on haphazard events as shown in the previous sub-section.

To exploit this 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 that the guard has worked since she joined the firm. This interaction captures the lower accumulation of building-specific experience of the type-II guards compared to the type-I guards. The results reported in Column (4) of Table 2 confirm the previous findings from the OLS estimations. The estimated coefficients of the client-specific experience are not only significant but also much larger in magnitude than those presented in Columns (1) - (3) of the table.[16] The IV estimate indicates that a 10% increase in the building-specific experience of a guard is associated with a decrease of 4 percentage points in the probability of crime.

As a robustness check, we repeat all the estimations of Table 2 but controlling non-parametrically for the total experience of the guard. Intuitively, we want to reject that the effect of building-specific experience is biased due to the presence of non-linear effects of the (log) total experience non-fully captured in equation (2). These results, reported in Appendix Table C2, are remarkably similar to those from Table 2.

An event study of guards' rotation. We conduct an event study around the rotation of guards to provide further evidence on the relationship between building-specific experience and crime.[17] Specifically, we construct a separate sample of guards by repeating the following procedure:

1. For each *rotation episode* where a guard i moved from building b to building b' at date t, we keep all the observations of guard i (hereafter the focal guard) two months before and after time t.

2. We then specify a control group for this rotation episode by including all other guards that were working in either building b or building b' during the same period of time (hereafter the control guards).[18]

[16] An interpretation of the larger coefficients from the IV estimation is that OLS estimates are downward biased due to reverse causation. Appendix Figure C3 reports how crime evolves in the days before a guard is rotated, conditional on the baseline controls in equation (2). There is no 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.

[17] Type-II guards are excluded from this exercise as they typically accumulate less building-specific experience and they can move in and out to different buildings during very short periods of time.

[18] We also exclude any control guard that rotates within the comparison window. This allows for a transparent control group and it alleviates concerns regarding dynamic effects as discussed in footnote 20.

Stacking together such treatment and control groups across all rotation episodes, we estimate the following equation at the guard-day level:

$$
\begin{aligned}
Crime_{ibt} = \beta(RotGuard_{it} \times PostRot_{it}) + \eta_i \times WinRot^j_{it} \\
+ \rho(PostRot_{it} \times WinRot^j_{it}) + \eta LogTotalExp_{it} + \delta_{b(it)} + \epsilon_{it}, \quad (3)
\end{aligned}
$$

where $RotGuard_{it}$ is a dummy taking one for the focal guard during the whole window of $t \pm 3$ months around her rotation. $PostRot_{it}$ is an indicator for the three months after the rotation of guard i (and takes one for both focal and control guards). The coefficient β captures the increase in crime that a guard experiences after she is moved to a new building, relative to control guards. Since we want to compare each focal guard with her associated control group within each rotation episode, we control for two sets of interactions. First, the interaction between the guard fixed effect η_i and $WinRot^j_{it}$, where the latter is a fixed effect identifying observations associated to each rotation episode j in the constructed sample. Second, the interaction between $PostRot_{it}$ and $WinRot^j_{it}$ which absorbs the average change in crime after the rotation episode experienced across all guards related to such episode. Naturally, we include building fixed effects $\delta_{b(it)}$ to control for the change in crime due to guards being moved between buildings with potentially different crime prevalence.[19] Finally, $LogTotalExp_{it}$ controls for the fact that even after rotation, the guard retains the overall experience gained while working in the firm. We also include indicators for neighbourhood $\times$ month and day of week $\times$ shift which are not necessary for identification but reduce the statistical noise associated to geographical or seasonal patterns (e.g. gangs may temporarily focus on some neighbourhoods or crime tend to be higher on Friday nights). We cluster standard errors (multi-way) at guard and $WinRotation^j_{it}$ level.[20]

[19] In order to control for the possibility that guards are more likely to be rotated during high (low) periods of crime in the building, we also run (3) controlling for building $\times$ week fixed effects. The results we obtained are very similar to those of Table 3.

[20] This specification is unlikely to suffer from the issues described in Borusyak et al. (2021) or Callaway and Sant'Anna (2020) for event studies. This is due to a number of reasons. First, we exploit the variation within each rotation episode (i.e. our estimation is equivalent to averaging many two-stage periods diff-in-diffs. See Gardner (2021) for a discussion of the validity of this "stacked" approach and Deshpande and Li (2019) and Cengiz et al. (2019) for empirical examples of the stacked approach in event studies). Second, the window of time we consider is relatively short and rotation is not extremely frequent. As discussed in Borusyak et al. (2021), when treatment events are sufficiently spaced out in time such that effects dissipate or stabilize, identification can be achieved under more standard assumptions. Third, we exclude from the control group those guards that rotate during the comparison window. Finally, in Columns 4-6 of Table 3 we restrict the sample to those guards that have been working in the same building for at least six months

Results from the estimation of (3) are reported in Panel A of Table 3. In Panel B, we conduct a similar estimation using the inverse-hyperbolic-sine transformed value of property lost in crime as the dependent variable. Estimates in Column (1) indicate an increase in crime and the value of property lost after a guard is rotated. The estimated coefficients represent 19% of the mean of the dependent variable. In Column (2) we repeat the exercise using as control group only the guards in the building where the focal guard worked *before* rotation. In Column (3) instead, the control group only includes the guards at the building where the focal guard worked *after* rotation. Results obtained in all columns are very similar. Columns (4) to (6) repeat the estimations from Columns (1) to (3) but including only guards with at least 6 months of tenure in the building. As expected, results are about 50% larger in magnitude, suggesting a lower effect of rotation on those guards with little experience in the building from which they are rotated from.

To better understand the previous variation and provide further evidence on the mechanisms explained in the theoretical section, we extend the specification of equation (3) and exploit the heterogeneity in the building-specific experience acquired by the guard at the time of the move. We expect that most of the variation from Table 3 is explained by those guards that acquired larger *CSS* at the time they were moved from one building to another. In Appendix Table C3 we decompose the effect of $RotGuard_{it} \times PostRot_{it}$ by interacting it with two dummies indicating if the guard has high (above median) or low (below the median) experience in the building. As expected, the results indicate that the increase in crime after rotation is high and significant for guards with relatively high experience in the building but low and non-significant for guards with low experience in the building.

Overall, the results from the event study are consistent with guards' performance being negatively affected by the loss of specific building experience after rotation.

The findings of Tables 2 and 3 are important for two reasons. First, a potential reason for rotation is to avoid collusion with criminals (Choi and Thum, 2003; Rose-Ackerman, 2010; Jia et al., 2015; Bhuller et al., 2020). Under this hypothesis, the longer a guard works in a building, the more likely she may cooperate with criminals and therefore the more likely crime will happen. However, this rationale is at odds with our findings as crime decreases

at the beginning of this window.

as guards spend more time in the building. This suggests that, in the current empirical setting, the main purpose for rotation does not seem to be deterring guards from colluding with criminals.

Second, the results are consistent with the fact that rotation can be inefficient as it destroys skills that positively affect productivity. Therefore, a natural question is why service firms do it. Our theoretical rationale suggests that rotation can be beneficial for the firm if the (absence of rotation) accumulation of building-specific experience increases the poaching risk of guards. In the next sub-section, we provide empirical evidence that this is the case by showing that buildings prefer to poach guards with large CSS.

Building-specific experience and observed poaching. To test this prediction, we use information of all the poaching episodes: in total, there were 28 guards that were hired in-house (before the policy change) by buildings that had a contractual relationship with the firm at the time of poaching.[21]

We are interested to know if among the pool of guards working at the same time, the building prefers to hire those workers with more building-specific experience. This motivates us to estimate the following equation at the guard-week level:

$$Poached_{ibt} = \beta LogExpInBuilding_{ibt} + \eta LogTotalExp_{it} + \varphi_{bm} + \eta_i + \gamma_t + \epsilon_{ibt}, \tag{4}$$

where $Poached_{ibt}$ is an indicator that takes one if guard i is hired by building b in week t. We exploit the variation brought by buildings and months 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 4. Even columns use the same IV strategy of Column (4) of Table 2.

All the coefficients of the building-specific experience are positive and significant indicating that buildings are more likely to poach guards with high CSS. In particular, the IV results of Column 4 indicate that a 10% increase in the building-specific experience of a guard is associated with additional 6 percentage points in the probability of being poached by the corresponding building.[22]

[21] In 70% of these cases, buildings poached only one guard.

[22] This magnitude is very large if we compare it with the total share of guards poached in the whole period before the introduction of the law (6%).

As a robustness check, we further estimate the relationships of Table 4 by controlling non-parametrically for total experience. Appendix Table C4 shows that this exercise gives similar results to those presented in the main text.

5 A Non-Poaching Policy Change

At the beginning of the 1990s, Colombian guerrilla groups heavily victimized the country's civil population. As a consequence, there was a civil-led initiative advocating for 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. The decree defines a security company as one with a significant amount of financial assets, which *de facto* limits the possibility that one guard establishes a security company to work as an in-house provider. As a consequence, the introduction of the new law inhibited buildings from hiring guards directly. There were no changes in guards' earnings or service fees charged to buildings around the policy change.

We use this policy change to provide evidence for the central mechanism highlighted by our theoretical model: if the security company rotates guards to trade off client-specific productivity and poaching risk, the rotation of guards should decrease 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%. This pattern is clearly observed in Figure 3 where we plot the time series of the average rotation across type-I guards.

However, a simple before-after comparison can be misleading due to time confounding factors. In the absence of an exogenous control group, we overcome this challenge by comparing the change in rotation across guards that had different probabilities of being poached before the policy change. Intuitively, we exploit the fact that guards differ by their baseline characteristics that make them more/less attractive to be poached by buildings. As implied by Proposition 1, the security firm should rotate more often those guards that are more attractive to buildings (only before the policy change: when those guards can be poached). Therefore, we examine whether the frequency of rotation dropped (relatively) more, once the degree came into effect, for guards who were more likely to be poached (before the policy change).

5.1 Poaching Risk: Machine Learning Estimation

To test whether the security firm rotates more those guards with a higher poaching risk, we first start estimating an index that reflects the probability that a guard is poached based on her observable characteristics. We focus our analysis on type-I guards who were the only ones exposed to poaching episodes. We estimate the relationship between observed poaching and predetermined characteristics of the guard. The use of these characteristics is aligned with anecdotal evidence given by our partner firm. The company argues that for instance, the size of the household of the guard may predict whether or not a building is attracted to that specific guard. Buildings prefer guards living in large household because in case of absence of the guard, she can more easily find a trustable replacement for the working shift.[23]

Overall, the predetermined variables we include in this exercise are the guard's age, gender, socio-economic strata and neighbourhood of residence, size of household, immigration history, military training, and working experience before joining the firm.

We face three challenges with this approach. First, the total number of guards poached by buildings is small. Second, given that the firm (supposedly) rotates guards to prevent poaching, we only observe an attenuated relation between the guards' characteristics and poaching. The lack of variation and the very few poaching episodes makes it difficult to detect empirically which characteristics are more important for the attractiveness of the guards to the buildings. Finally, it is possible that interactions between characteristics are critical predictors of poaching (e.g. having military training matters only for young guards).

To address these issues, we first augment the poaching episodes with information provided by the firm about guards receiving *solicitations* from buildings: A guard is *solicited* if a building formally asks the security firm to hire the guard in-house. We find that among the 34 guards that were solicited, 14 were also poached by the building writing the solicitation.

[23]We prefer to use "static" rather than time-dependent characteristics such as building specific experience or crime occurrence because the latter type of characteristics may be correlated with both rotation and poaching events.

Then, we estimate a cross-section Random Forest model, where the dependent variable is a dummy taking one if the guard was poached or solicited.[24] This machine learning approach 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.[25]

Appendix Figure C4 displays the distribution of the estimated score from the Random Forest which we use as our main measure of poaching risk (we standardize it to facilitate its interpretation). Appendix Table C5 displays the correlation between the estimated poaching risk and the observed characteristics of the guards (Column (1)) and the Gini Importance (Column (2)) which measures the relative contribution of each characteristic to the estimated poaching risk (i.e., its contribution to reducing the loss function across all trees). Results indicate that age, gender, household size and previous experience are the most relevant dimensions to predict that a guard is poached/solicited by a building.[26]

5.2 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 into a new building during the month and 0 otherwise. As an alternative, we also use an intensive margin measure that counts the number of buildings in which the guard worked during the month.

We begin our analysis by presenting some descriptive evidence consistent with the fact that (before the policy change) the firm rotated more often those guards with higher poaching risk. Panel A of Figure 4 shows the cumulative share of guards rotated over time before the law introduction. Rotation patterns diverge significantly between high-risk (above median) guards and low-risk (below median) guards. As expected, those guards more likely to be

[24] Our baseline findings are robust to the exclusion of solicited guards from the estimation of the poaching risk.

[25] Specifically, we run a Random Forest model based on Gini impurity with 500 trees (bootstrap based samples). Since our data contains few cases of poaching, we follow the standard procedure of using an asymmetric loss function that assigns higher weight to misclassification of the least prevalent event. See Pazzani et al. (1994); Domingos (1999); Sage et al. (2020) for an overview of this approach and a discussion of the problems associated to predictions with imbalanced data.

[26] The negative sign for the past experience is explained by the non-linear effect of the experience on the poaching risk. Guards with too little experience or too much (which make them expensive in the guards' market) are less preferred to those with intermediate experience. This is the motivating fact of column (6) in Table C6.

poached are rotated more intensively. Panel B of the figure shows the same comparison for the period after the law is introduced. There is a large drop in overall rotation for both groups after the policy took effect.

We also regress the measures of rotation on the estimated risk of poaching, controlling for time-varying characteristics of the guard as well as monthly fixed effects. To avoid any bias resulting from the non-random attrition, we exclude from the estimation those guards that were poached at any point. The first two columns of Table 5 show that prior to the policy change, the firm rotated more often guards with a higher risk of being poached. This table shows 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. The correlation between poaching risk and the number of buildings worked per month is positive and highly significant. Indeed, The coefficient of columns (1) and (2) are also similar in magnitude because there are few cases in which guards are rotated to more than one building in a month.

Since the measure of the risk of poaching is a generated regressor, standard errors may not account for its full sampling variation. We address this concern by bootstrapping the whole two-step procedure. That is, we re-estimate the Random Forest model and the main regression in each bootstrap sample. Although bootstrapped standard errors (reported in Table 5 with the squared brackets) are slightly larger than the baseline results, the coefficient estimates remain highly significant.

Columns (3) and (4) of Table 5 show that the relationship between rotation and poaching risk becomes small and insignificant in the year after the policy took effect. This result is consistent with the patterns shown in Figure 4 and suggests that the policy offsets most of the rotation gap due to heterogeneity in poaching risk.[27] Intuitively, as buildings cannot hire workers in-house, the security firm does not have to "differentially" rotate workers across buildings. The main source of rotation is exogenous to the type of guards.

[27]Figure 4 shows that low-risk guards are rotated more often than high-risk guards the first months after the policy was introduced. When we exclude these months, the coefficients of Columns (3) and (4) of Table 5 become even closer to zero.

5.3 The Effect of the Policy on Rotation

The threat of buildings poaching guards dropped substantially after the introduction of the 1994 Decree. In fact, no poaching episode is observed in our data after the policy took effect. The descriptive evidence shown in Figure 4 and Table 5 suggests that rotation may have dropped disproportionately for guards with ex-ante high poaching risk after the policy introduction. In order to investigate more formally how the policy change affected the rotation of the guards, particularly of those with a higher risk of poaching, we estimate the following *Diff-in-Diff* specification at the guard-month level:

$$Rotation_{it} = \beta RiskPoaching_i \times After_t + \phi X_{it} + \eta_i + \gamma_t + \delta_{b(it)} + \varepsilon_{it}, \tag{5}$$

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.[28] Our estimation includes time varying characteristics of the guards (X_{it}) like the number of days worked during the month and the tenure within the firm. We absorb any permanent differences in rotation levels across guards by including guard-fixed effects (η_i), and we account for time aggregated variation by including month fixed effects (γ_t). [29]

We also include fixed effects for the building where the guard completed most shifts during the month ($\delta_{b(it)}$) to control for changes in rotation due to systematic differences between buildings where the guard works.[30]

[28]Callaway et al. (2021) argue that the magnitude of coefficients with continuous treatment from DiDs should be interpreted with caution as the interaction coefficient identifies a weighted average of the "average causal response" of the treatment along different levels of the treatment. The weights depend on the distribution of the level of the treatment variable and thus the interaction coefficient does not generally equals the overall average response effect. Since the distribution of the index of poaching risk is smoothly spread over its support and not strongly asymmetric, we follow the standard practice of interpreting the interaction coefficient as the average effect of increasing the treatment marginally.

[29]The recent criticism over staggered Diff-in-Diff setups (e.g. Callaway and Sant'Anna, 2020; Sun and Abraham, 2020) is not a concern in this setting as the law is introduced at a single date.

[30]Including dummies for every building where the guard worked during the month (instead of just the one where the guard spent most time) results in perfect collinearity with our main rotation measure. As a robustness check, we re-estimate the main specifications at the guard-date level (the advantage of this specification is that a guard can work in at most one building per day). Results are very similar to the main specification if we scale up the coefficients to the monthly level (see Appendix Table C7 which is reported at the daily level).

Table 6 reports the estimates of equation (5) (Columns 1 and 2).[31] Results confirm that guards with a larger risk of poaching were rotated less often after the policy change. An additional standard deviation in the risk of poaching is associated with a reduction in the probability of rotation of 2 percentage points. This magnitude is very large relative to the monthly average probability of rotation which is 2.5%. As in the previous table, the estimated effects for the probability of rotation and the number of buildings worked in a month are close in magnitude.

5.3.1 Robustness

In Columns (3) and (4) of Table 6 we allow for guard-specific linear trends ($\theta_i \times t$) to identify the effect of the policy besides any secular change over time. We include these controls to rule out that results are biased due to guards being initially allocated to rotation schedules that change over time at different rates (e.g. rotation may be reduced faster for guards from certain localities or for guards joining the firm at an older age).

Finally, we estimate the effect of the policy without the influence of the transition period immediately after the law introduction (see Figure 4). In Columns (5) and (6) of Table 6 we control for the interaction between guard fixed effects and an indicator of the two quarters after the policy introduction. As expected, results are slightly smaller but they remain highly significant.

Figure 5 depicts the leads and lags version of equation (5) and displays the estimated coefficients of the variable $RiskPoaching_i \times After_t$. The figure does not show evidence of pre-trends in rotation but a sharp decrease in the rotation of guards with high probabilities of being poached. The first months after the law introduction display larger negative coefficients. This coincides with the descriptive evidence shown in Figure 4 where low-risk guards are rotated more intensively for a short period after the law was introduced.

Panel A of Appendix Figure C5 reports the leads and lags estimates that include guard-specific linear trends. Borusyak et al. (2021) discuss a number of issues that could arise in dynamic Diff-in-Diff designs when the parallel trends assumption requires conditioning on time varying covariates or individual trends and propose a procedure that separates the

[31] We report standard errors multi-way clustered at guard and month level and two-steps bootstrapped standard errors.

testing of pre-trends from the estimation of dynamic effects. To deal with this concern, in Panel B of Appendix Figure C5, we report estimated pre-trends and treatment effects using the "imputation estimator" from Borusyak et al. (2021). Results from both panels are qualitatively similar to those in our baseline specification.

Additional robustness checks are shown in Appendix Table C6 where we use alternative proxies for the risk of poaching. This includes an alternative estimation of the Random Forest model as well the observable characteristics that are most associated with a higher poaching risk. As expected, the rotation decreased after the policy change, specially for immigrant, older, male guards living in larger households with intermediate previous experience.

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

5.4 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.

To explore this implication, we estimate the reduced form effect of the law on crime. We exploit the same specification as in equation (5) but the dependent variables are the number of crimes occurred while the guard was on duty during the month and the (ihst) value of property lost due to crime. The results are reported in Table 7.

The estimated effect of rotation on crime, albeit less robust than the results for rotation,[32] is negative and large relative to the mean number of crimes: an additional standard deviation of the poaching risk is associated with a monthly reduction of the number of crimes in the range of 0.026 to 0.042. This effect is about 13% to 20% of the average number of crimes

[32]Estimates are significant at 5% in Columns (2), (5) and (6) and significant at 10% in Column (1) but only marginally significant when using two-step bootstrapped standard errors.

per month. Similarly, an additional standard deviation of poaching risk is followed by a reduction in the cost of property lost in the order of 15% to 25%.[33] Appendix Figure C6 reports the corresponding leads and lags estimates when crime is the dependent variable. The effect of the policy on crime seems to be stronger over time.

Taken all together, the results of this section provide evidence consistent with: (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 and accumulating more CSS.

6 Final Discussion

In this article, we have made a first step in understanding how service-providing firms respond to the threat of clients poaching their workers. Using detailed data from a firm operating in the security-service industry, we show 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 is also increasing in that guard's building-specific experience. 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.

We also show that a policy change that forbids in-house contracting 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.

[33] As a robustness exercise that allows us to control for building fixed effects, Appendix Table C7 reports the regressions at the guard-date level. Estimates are comparable in magnitude (if scaled up to the month level) and significant at 1% when the transition period is absorbed.

We complement the previous results by providing anecdotal evidence from multiple industries and countries and presenting a theoretical model which generalizes the specific setting we study. The issue of poaching although recognized as an important feature in the service sector, to the best of our knowledge has not been quantified. A way to approach to this lack of information is to use indirect measures of the poaching problem, for instance some that show how common is that employers take actions to avoid that their employees leave and work for clients. One such common action is to ask employees to sign non-solicitation agreements. These agreements are employment contractual clauses that impede employees to contact past clients to persuade them to do business with them. Balasubramanian et al. (2021) conduct a survey to a large sample of employers and show that 77% of the firms use non-solicitation agreements. The fact that these agreements are widespread suggests that the issue of client poaching is important and ubiquitous.

Besides the previous indirect measure we take one final step to argue that the issue of client's poaching is also relevant in an empirical context different from the one we have focused on so far. For that, we use a very different source of data –a high-skill industry in a developed country– US federal advocacy data for the period 1998-2008. The data (based on Blanes i Vidal et al. (2012)) records employment histories of lobbyists, allowing us to observe whether she is a for-hire or an in-house advocate, for who they worked and when. As a consequence, we can proxy the extent of client's poaching talent in the US advocacy industry.

Appendix Table C8 shows the relation between past client experience of the lobbyist and the probability of being hired in house by the client. The results show that previous client-specific experience is a statistical and significant predictor of being poached. In particular, the table implies that the odds of being poached by a client is 66 times larger for a lobbyist that worked for that client, than for a lobbyist that never worked for the client.

We have argued that the phenomenon of poaching is relevant and widespread. However, there are other settings, in which service-providing firms may be more positive about their employees being poached by clients, especially if these workers can assure future stream of transactions with their original employers (Somaya et al., 2008). Our setting is not appropriate to analyse that type of empirical contexts, primarily because in our case the client obtains the necessary service either fully in-house or fully 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 labour force in-house and acquire the remaining labour input through outsourcing. Studying and characterizing these other settings is outside the scope of this paper, but future work in this direction is warranted.

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A Main Figures and Tables

Figure 1: Example of Guards' Shift Schedule

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-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: Timing of the Stage Game

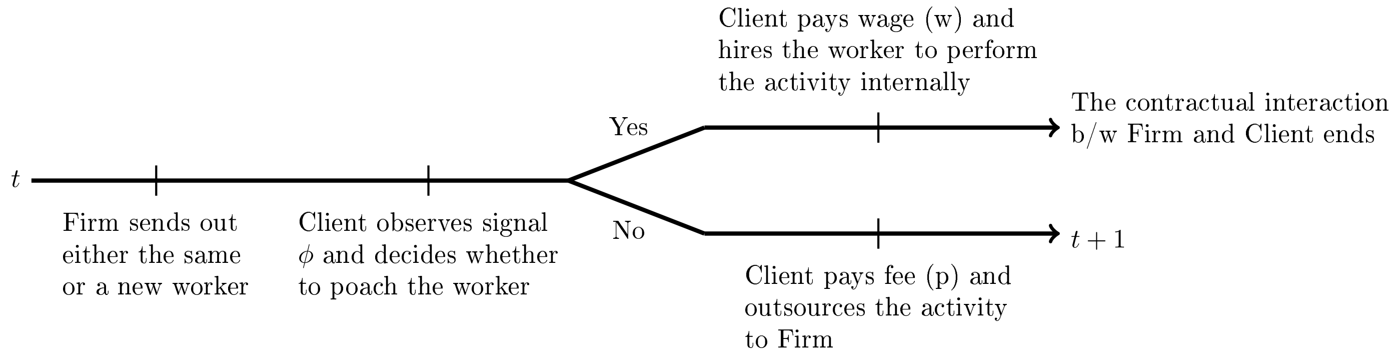
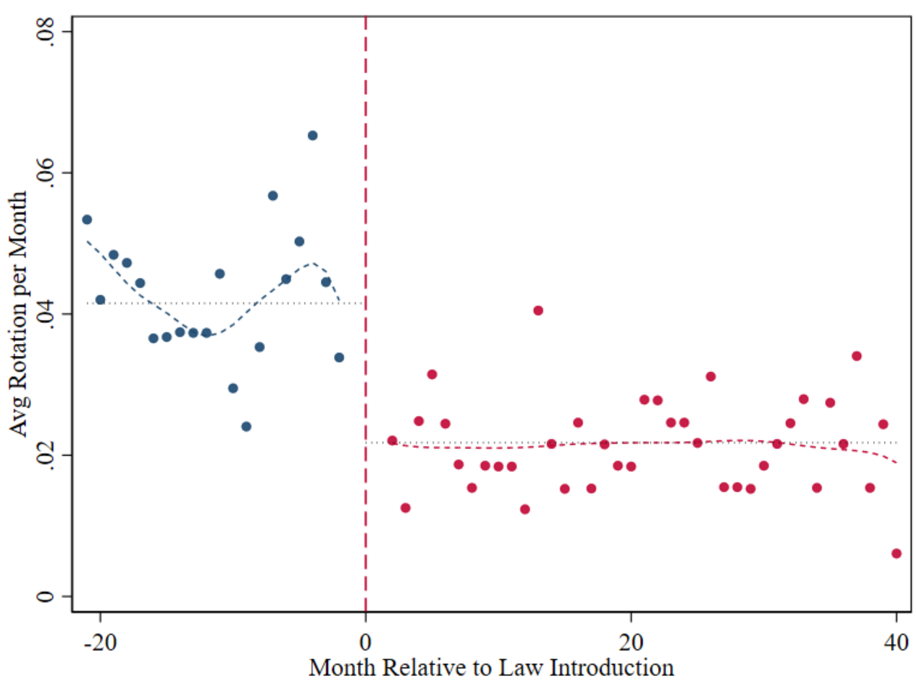
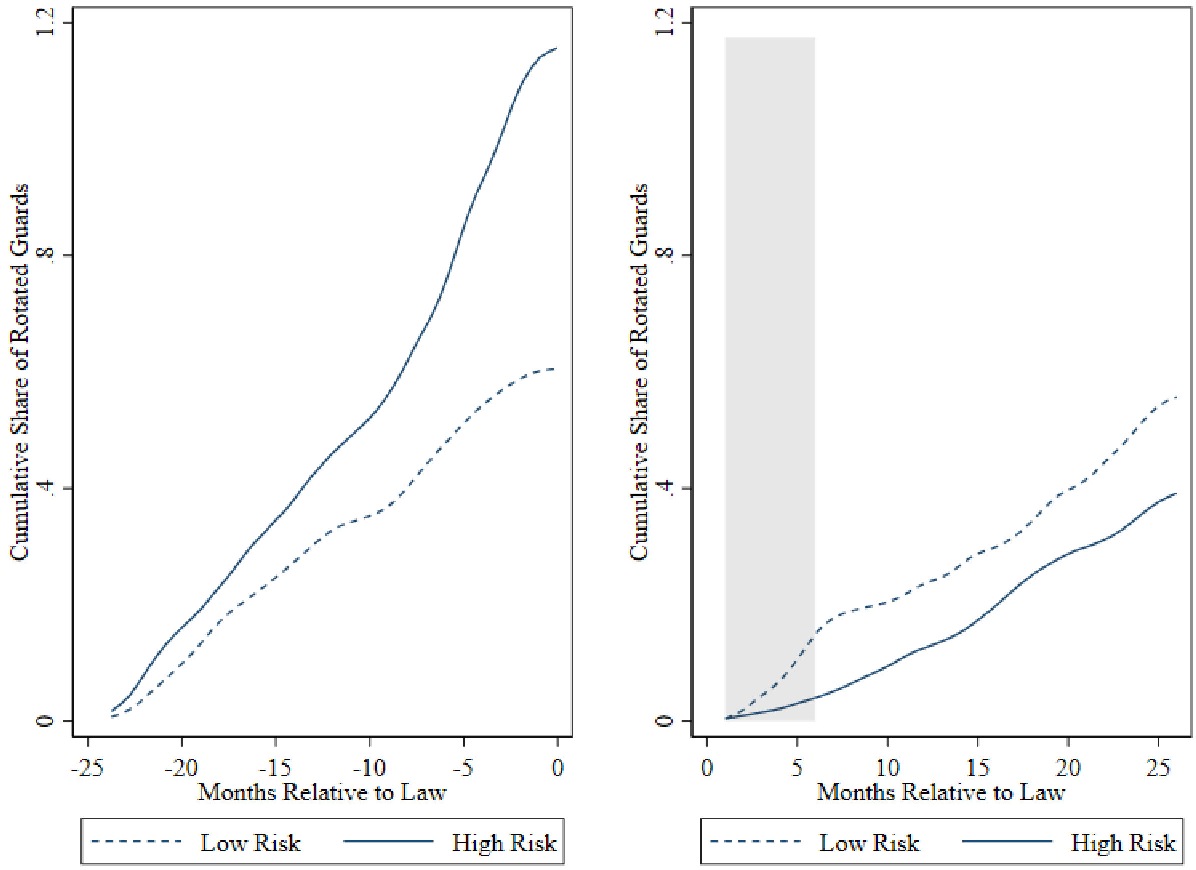


Figure 3: Evolution of Total Average Rotation



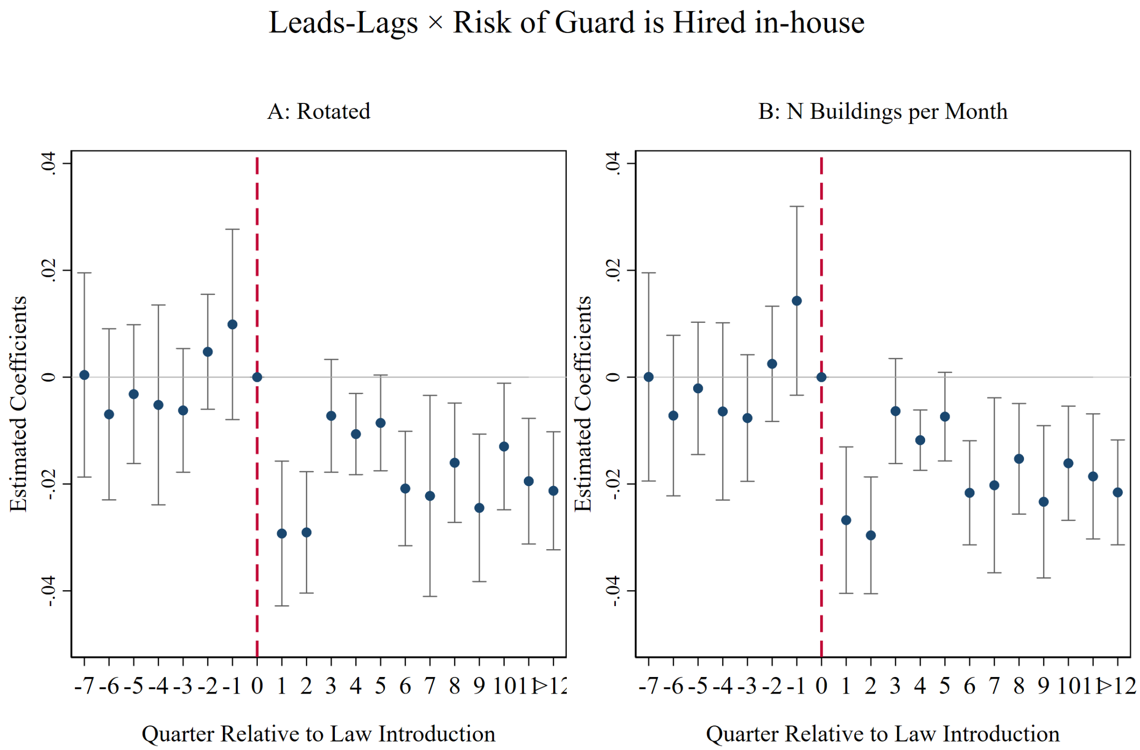
This figure displays the average rotation across all type-I guards in a given month. Each dot corresponds to the average rotation across all guards working during the corresponding month. The dashed curves display a local polynomial estimation of the evolution of average rotation over time for the periods before and after the policy change separately. The dotted lines are the average rotation for each period. The average number of guards working in a given month is 295.

Figure 4: Rotation of High vs. Low Poaching Risk Guards



This figure reports the estimated cumulative share of guards that have rotated over time. This measure is calculated as the cumulative sum of rotation episodes over the total number of guards in the sample. Time is measured in weeks and each panel corresponds to a period of 22 months. The cumulative share of guards is calculated separately for high-risk guards (those with estimated poaching probability above the median) and low-risk guards (probability below the median). The reported lines correspond to a kernel-weighted local polynomial smoothing (Epanechnikov Kernel and ROT bandwidth) estimated over the daily time series. Panel A corresponds to the period before the policy took effect and Panel B corresponds to the period immediately after the policy took effect. The shaded area corresponds to the transition period where low-risk guards are rotated relatively more intensively. Each panel starts with a cumulative share of rotated guards equal to zero on the first day of the period.

Figure 5: Effects of the Decree 356 on the Rotation of Guards



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 is an indicator for whether the guard was rotated to a new building during the month. In Panel B , 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. Additional controls include the total number of days that the guard worked during the month, the (log) tenure in the firm and a fixed effect for the building where the guard worked most days in the month. Observations are at the guard-month level. Standard errors are multi-way clustered at the guard-month level. $N = 15,313$.

Table 1: Characteristics of Guards and Buildings

	(1) Mean	(2) Sd	(3) Min	(4) Max
Guard Characteristics				
N of guards	589			
Type-I guard	0.88	0.33	0	1
Male	0.78	0.41	0	1
Military experience	0.64	0.48	0	1
Neighborhood strata	1.89	0.57	1	5
Household size	5.50	3.43	0	12
Lives alone	0.07	0.25	0	1
Age	35.93	9.15	20	71
Past experience as guard (months)	31.48	51.23	0	285
Has experience as guard	0.49	0.50	0	1
Tenure (months)	23.92	17.29	0	70
Immigrant	0.42	0.49	0	1
Recent immigrant	0.19	0.39	0	1
Started job on/before January 1992	0.80	0.40	0	1
N of shifts worked in the month	24.43	5.32	1	56
Max tenure in the building (in months)	22.04	18.19	0	65
N of buildings per month (Type-I)	1.03	0.17	1	3
N of buildings per month (Type-II)	2.22	0.78	1	5
Rotated to a new building during the month (Type-I)	0.02	0.16	0	1
Rotated to a new building during the month (Type-II)	0.04	0.21	0	1
Avg. shifts worked per building (Type-I)	26.16	2.55	1	29
Avg. shifts worked per building (Type-II)	9.06	3.86	1	24
Building Characteristics				
N of buildings	116.00			
N of guards	4.39	2.50	2	14
N of flats	98.05	57.15	20	299
Neighborhood strata	2.78	1.28	1	6
N of crimes per month in the building	1.51	3.46	0	35
Value of property lost (usd)	5.47	13.95	0	166
Value of property lost (usd) if crime occur	16.97	20.22	0	166

Table 2: Productivity and Client-Specific Experience

	(1)	(2)	(3)	(4)
Panel A:	**Crime Occurred During Guard's Shift**			
Log Experience in Building (÷ 100)	-.1***	-.073**	-.073**	-.38***
	(.038)	(.029)	(.028)	(.12)
Log Total Experience (÷ 100)	-.019	.0018	.002	.096*
	(.043)	(.039)	(.039)	(.052)
N	611,193	611,193	611,193	611,193
R2	.036	.037	.038	-
F first-stage (Kleibergen-Paap)	-	-	-	162
Mean Depvar	.009	.009	.009	.009
Panel B:	**IHST Value of Property Lost in Crime**			
Log Experience in Building (÷ 100)	-1.3***	-.91***	-.91***	-4.6***
	(.46)	(.35)	(.35)	(1.4)
Log Total Experience (÷ 100)	-.28	-.033	-.031	1.1*
	(.51)	(.46)	(.46)	(.62)
N	611,193	611,193	611,193	611,193
R2	.035	.037	.037	-
F first-stage (Kleibergen-Paap)	-	-	-	162
Mean Depvar	.11	.11	.11	.11
Method:	OLS	OLS	OLS	IV
Guard × Building FE:	YES	YES	YES	YES
Shift FE:	YES	YES	YES	YES
Month FE:	YES	NO	NO	NO
Building Neighb × Month FE:	YES	YES	YES	YES
Week FE:	NO	YES	YES	YES
Shift × Day of Week FE:	NO	NO	YES	YES

N of guards = 589; N of buildings = 116. All regressions are at guard × 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 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. Experience variables are divided by 100 (i.e. coefficients are scaled up by 100).

Table 3: Crime Behaviour after Guard's Rotation. Event Study

Control Group is Non-Rotating Guards at:	**(1) In and Out Buildings**	**(2) Only Out Building**	**(3) Only In Building**	**(4) In and Out Buildings**	**(5) Only Out Building**	**(6) Only In Building**
Panel A:	**Crime Occurred During Guard's Shift**					
Post Rotation × Rotating Guard	.0024***	.0019**	.0019**	.0034***	.0032***	.003***
	(.00078)	(.00083)	(.00084)	(.0011)	(.0012)	(.0012)
Panel B:	**IHST Value of Property Lost in Crime**					
Post Rotation × Rotating Guard	.028***	.022**	.021**	.04***	.037***	.035**
	(.0094)	(.0099)	(.01)	(.013)	(.014)	(.014)
N	277,981	161,335	168,727	207,025	121,601	127,565
Only Guards with >6 Months of Experience in the Building	NO	NO	NO	YES	YES	YES

N of guards (col 1) = 446; N of buildings (col 1) = 104; Mean Depvar A (col 1) = .012656; Mean Depvar B (col 1) = .150239; N of guards (col 4) = 428; N of buildings (col 4) = 103; Mean Depvar A (col 4) = .010781; Mean Depvar B (col 4) = .128318. This table investigates the evolution of crime occuring while the guard is on duty during the months before and after rotation. Observations are at the guard-date level. The sample is restricted to type-I guards and during a window of 3 months before/after a rotation in the sample takes place. For this window of time and for each rotation, we include all observations from the rotating guard (focal guard) and her co-workers at the rotating (in or out) building (control guards). This set of observations is labelled as a rotation episode. The regresion sample is constructed by stacking the observations for 525 rotation episodes observed after July 1992. In Panel A, the dependent variable is an indicator for a crime occurring during the shift when the guard was working. In Panel B, the dependent variable is the inverse hyperbolic sine transformation of the value of property lost due to crime. The main independent variable is a guard-level indicator for the 3 months period after rotation takes place interacted with an indicator for being the rotating (i.e. focal) guard. All regressions control for the interaction between guard and rotation episode fixed effects and the interaction between the three months after rotation and rotation episode fixed effects. We also include week fixed effects, building fixed effects, neighborhood × month fixed effects and the interaction between night-shift and day of the week fixed effects. Additional controls are (log) total tenure of the guard in the firm and shift × day of the week fixed effects. Columns (4)-(6) only include guards with more than 6 months of experience in the building. Standard errors are clustered at the guard-rotation episode window.

Table 4: Poaching and Client-Specific Experience

	Guard Hired by Building (Pre-Law)			
	(1)	**(2)**	**(3)**	**(4)**
Log Experience in Building ($\div$ 100)	.11***	.55**	.11***	.6*
	(.029)	(.26)	(.036)	(.32)
Log Total Experience ($\div$ 100)	-.04	-.099	-.044	-.1
	(.033)	(.061)	(.037)	(.068)
N	40,169	40,169	40,080	40,080
R2	.16	-	.4	-
Mean Depvar	.0006	.0006	.0006	.0006
F first-stage (Kleibergen-Paap)	-	30.18	-	25.89
Method:	OLS	IV	OLS	IV
Guard FE:	YES	YES	YES	YES
Week FE:	YES	YES	YES	YES
Building X Month FE:	YES	YES	NO	NO
Building X Week FE:	NO	NO	YES	YES

N guards = 452 ; N buildings = 116. 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 columns (2) and (4), the accumulated experience of the worker in the building is instrumented with the interaction between an indicator for workers type-II and the tenure of the worker 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 5: Correlation between Rotation and Risk of being Poached

	(1)	(2)	(3)	(4)
	Year Before Policy		Year After Policy	
Dependent Variable	**Rotated**	**N Builds Worked**	**Rotated**	**N Builds Worked**
Poaching Risk	.015***	.016***	-.0023	-.0025
	(.0029)	(.0038)	(.0027)	(.0025)
	[.0045]	[.0054]	[.0035]	[.0033]
N	3,068	3,068	3,136	3,136
R2	.013	.015	.0075	.0076
F	14	15	1.2	15
Mean Depvar	.035	1	.017	1

N of guards = 312; N of buildings = 108. This table investigates the correlation between the estimated risk of being hired by a building and the rotation of guards. Columns(1) and (2) use the sample period corresponding to one year before the policy introduction. Columns (3) and (4) repeat the estimation for the sample period corresponding to the year following the policy introduction. The sample only includes guards that joined the firm at least one year before the policy. In Columns (1) and (3), the dependent variable is an indicator of whether the guard worked was rotated to a new building during the month. In Columns (2) and (4), the dependent variable is the number of buildings in which the guard worked during the month. Each regression controls for the (log) tenure of the guard in the firm and month fixed effects. Robust standard errors are clustered at the guard level and are shown in parenthesis (with asterisks denoting significance for these s.e.). The square brackets report the standard error of the coefficient obtained by 200 boostrap repetitions of the whole two step procedure, where for each boostrap sample, in the first step we estimate of the risk of poaching and in the second step the main regression.

Table 6: Effect of the Policy on Guards' Rotation

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**Rotated**	**N Builds Worked**	**Rotated**	**N Builds Worked**	**Rotated**	**N Builds Worked**
Post × Poaching Risk	-.02***	-.02***	-.023***	-.022***	-.015***	-.015***
	(.003)	(.0035)	(.0051)	(.0056)	(.005)	(.0057)
	[.006]	[.006]	[.008]	[.008]	[.007]	[.007]
N	15,161	15,161	15,161	15,161	15,157	15,157
R2	.066	.14	.094	.17	.12	.19
Mean Depvar	.025	1	.025	1	.025	1
Indiv Chars:	YES	YES	YES	YES	YES	YES
Month FE:	YES	YES	YES	YES	YES	YES
Guard FE:	YES	YES	YES	YES	YES	YES
Building (most worked) FE:	YES	YES	YES	YES	YES	YES
Guard Trends:	NO	NO	YES	YES	YES	YES
Guard × Transition :	NO	NO	NO	NO	YES	YES

N of guards = 347; N of buildings = 113. This table investigates the effects of the introduction of the decree on guard's rotation. 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 Columns (1), (3) and (5) the dependent variable is an indicator of whether the guard was rotated to a new building during the month. In Columns (2), (4), (6) the dependent variable is the the average number of buildings in which the guard worked during 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. All regressions also control for the total number of days the guard worked during the month and the log-experience of the guard. Columns (3), (4), (5), (6) include guard-specific linear trends. Columns (5), (6) control for the interaction between guard fixed effect and an indicator for the two quarters after the law was introduced. Robust standard errors are clustered two-ways at the guard-month level and are shown in parenthesis (with asterisks denoting significance for these s.e.). 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 7: Effect of the Policy on Crime

Dependent Variable	(1) N of Crimes	(2) IHST Value Prop Lost	(3) N of Crimes	(4) IHST Value Prop Lost	(5) N of Crimes	(6) IHST Value Prop Lost
Post × Poaching Risk	-.032*	-.19**	-.026	-.15	-.042**	-.25**
	(.017)	(.092)	(.019)	(.097)	(.019)	(.099)
	[.026]	[.135]	[.026]	[.143]	[.027]	[.147]
N	15,161	15,161	15,161	15,161	15,157	15,157
R2	.25	.25	.29	.29	.31	.3
Mean Depvar	.21	1.5	.21	1.5	.21	1.5
Indiv Chars:	YES	YES	YES	YES	YES	YES
Month FE:	YES	YES	YES	YES	YES	YES
Guard FE:	YES	YES	YES	YES	YES	YES
Building (most worked) FE:	YES	YES	YES	YES	YES	YES
Guard Trends:	NO	NO	YES	YES	YES	YES
Guard × Transition :	NO	NO	NO	NO	YES	YES

N of guards = 347; N of buildings = 113. This table investigates the effects of the introduction of the decree on 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 Columns (1), (3) and (5) the dependent variable is the number of crimes that occurred in the building in the shifts when the guard was working. In Columns (2), (4), (6) the dependent variable is the (ihst) value of the property lost in the month for the crimes occurred in the building during the shifts when the guard was working. 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. All regressions also control for the total number of days the guard worked during the month and the log-experience of the guard. Columns (3), (4), (5), (6) include guard-specific linear trends. Columns (5), (6) control for the interaction between guard fixed effect and an indicator for the two quarters after the law was introduced. Robust standard errors are clustered two-ways at the guard-month level and are shown in parenthesis (with asterisks denoting significance for these s.e.). 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).

B Proofs and Additional Results

B.1 Proof of Proposition 1

Let μ_t be the client's posterior expectation about the type of the worker assigned to her after she observes the private signal at time t. We want to show that the following profile of behavioural strategies and the associated beliefs constitute an equilibrium: At the beginning of any period t, the service-firm assigns a new worker to the client if and only if $e_{t-1} = \bar{T}-1$, i.e. the worker it sent out at period $t-1$ has accumulated $\bar{T} = k_c^{-1}(qv_h+(1-q)v_\ell-w+p) \in \mathbb{N}$ units of client-specific experience. As for the client, she will poach the worker assigned to her at time t if and only if either (1) $\mu_t = v_h$, or (2) $e_t \geq \bar{T}$ and $\mu_t = \bar{v}$. Note that if all players follow this strategy profile, the client's continuation pay-off at any period $n\bar{T}$, where $n \in \{0, 1, 2, ...\}$, equals to

$$\bar{V} = \sum_{t=0}^{\bar{T}-1} \rho^t \gamma^t \left(-\gamma p + (1-\gamma) \sum_{j=t}^{+\infty} \rho^{j-t} (v_h - w - k_c(j)) \right) + \rho^{\bar{T}} \gamma^{\bar{T}} \bar{V},$$

where $\gamma = 1 - \lambda q$. Solving the above recursive equation, we have

$$\bar{V} = \frac{1}{1-(\rho\gamma)^{\bar{T}}} \sum_{t=0}^{\bar{T}-1} (\rho\gamma)^t \left(-\gamma p + (1-\gamma) \sum_{j=t}^{+\infty} \rho^{j-t} (v_h - w - k_c(j)) \right). \tag{B.1}$$

We start by examining the incentive of the client. At any period t, if the client has received a signal indicating that the worker assigned to her is with high match value v_h, then poaching that worker is a dominant strategy for the client provided that v_h is sufficiently large (i.e., $v_h - w - k_c(0) > -p$), which we have assumed in the main text (see footnote 11). Similarly, when the client has learned that the worker assigned to her is with low match value v_ℓ, outsourcing to the firm is a dominant strategy provided that v_ℓ is sufficiently small ($v_\ell - w - \lim_{e\to+\infty} k_c(e) < -p$), which we have also assumed in the main text. For an uninformed client, we argue that for the desired outcome to arise in equilibrium, the following incentive constraint must hold:

$$-p + \rho\bar{V} > \sum_{t=0}^{+\infty} \rho^t \left(\bar{v} - w - k_c \left(\bar{T} - 1 + t \right) \right). \tag{B.2}$$

In words, condition(B.2) requires that after a worker has performed the production activity for the client for $\bar{T}-1$ periods (so $e_t = T-1$) and right before he is about to be rotated, provided that the client is still uncertain about the worker's type ($\mu_t = \bar{v}$), she will not find it profitable to deviate from the proposed strategy to bring that worker in house. Note that as $\rho \to 0$ the LHS of (B.2) goes to $-p$, while the RHS (B.2) converges to $\bar{v} - w - k_c(\bar{T} - 1)$. Further, if $e_t \leq \bar{T} - 2$, poaching that worker is suboptimal for the client given that (i) $\bar{v} - w - k_c(\bar{T} - 2) < -p$, and (ii) the same worker will be assigned to her in the next period. Finally, if $e_{t-1} \geq \bar{T}$ (which can happen off the equilibrium path), the one-shot deviation principle implies that poaching the assigned worker is optimal for the uninformed agent if

$$\begin{aligned} &\sum_{t=0}^{+\infty} \rho^t \left(\bar{v} - w - k_c \left(\bar{T} + t\right)\right) \\ > &-p + \rho \left[\sum_{s=0}^{+\infty} \rho^s \left((1-\lambda)\bar{v} + \lambda q v_h - w - k_c \left(\bar{T} + 1 + s\right)\right) - \lambda(1-q) \sum_{s=0}^{+\infty} \rho^s p\right], \end{aligned}$$

which is the case whenever ρ is sufficiently small (since $\bar{v} - w - k_c(\bar{T} + t) > -p$).

Now we take the behavioural strategy of the client as given and consider the incentive of the service-firm. Suppose that a worker has accumulated $e \leq \bar{T} - 1$ units of client-specific experience in the beginning of period t. Regardless of whether the worker is rotated or not, the poaching decision of the client will not be affected: The client will not poach the worker assigned to her if she receives $\phi \in \{\emptyset, v_\ell\}$, and she will poach the worker if $\phi = v_h$. As a result, the cost of rotation – that it destroys the stock of CSS and decreases productivity – becomes the dominant force, so the firm would indeed prefer not to rotate the worker.

Next, suppose that the worker has accumulated $e = \bar{T}$ units of client-specific experience in the beginning of period t. If the firm choose not to rotate in this period, the according to the client's strategy, the worker will be poached even when the client is still uncertain about the true match value, in which case the firm will lose both its client and employee. If the firm chooses to send out a new worker, then the client will be discouraged from poaching, in which case the firm will be able to collect more profits. Thus, when λ is sufficient small, the firm would prefer to rotate this worker to mitigate the very substantial poaching risk that it faces at this stage.

Finally, if the firm did not rotate a worker with experience $e \geq \bar{T}$ (which can happen off the equilibrium path) but the client did not poach the that worker, then we assume that the firm would assign probability one that the client has learned that the match value of that worker is v_ℓ. In that case, it would indeed be sequentially rational for the firm to continue to assign this worker to the client. □

B.2 Extension: 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 and optimally, use job rotation to counter the poaching risk of its employees – continues to hold with a more general contracting space. For simplicity, we assume that the benefit of in-house production v is deterministic for the client, and it satisfies (i) $v < k_c(0) - k_s(0)$ (i.e., there is a positive gain from trade, at least in the beginning of the transaction between firm and client); and (ii) $v > w + \lim_{t \to +\infty} k_c(t)$ (i.e., the value of in-house production is positive for the client provided that the worker is sufficiently experienced).

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 $t = 0, 1, ... + \infty$.[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. Taking the client's poaching incentive into account, the firm chooses 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. *If δ is sufficiently large and ρ is sufficiently small, then in any optimal contract the service-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 a contract $(\tilde{\mathbf{p}}, \tilde{\mathbf{r}})$ that charges a constant service fee $p > w + k_s(0)$ and rotates the workers with the frequency $1/T(v)$.*

Proof. Consider any contract that adopts a no-rotation policy. Since the client is free to poach the firm's worker at any time, for poaching to take place no earlier than time $T \in [0, +\infty]$, it is necessary that

$$p_t \leq \bar{p}_t \equiv \sum_{s=t}^{T} \rho^{s-t} \left(k_c(s) + w - v\right), \ \forall t \leq T. \tag{B.3}$$

[34] Allowing 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.

In addition, since $v > w + \lim_{t\to+\infty} k_c(t)$, there must exist a finite cut-off date $\hat{T}$ such that $\bar{p}_t \leq 0$ for all $t \geq \hat{T}$. Together with (B.3), this implies that for any contract that involves a no-rotation policy, poaching will for sure take place no later than period $\hat{T}$. We can then obtain a uniform upper bound on the expected profit generated by any of such contract. Specifically, for all $(\mathbf{p}, \mathbf{r})$ contract with $r_t = 0\ \forall t \geq 1$ that induces poaching at some period $T \in \mathbb{N}$, the expected profit of the firm must satisfy

$$\Pi(\mathbf{p}, \mathbf{r}) = \sum_{t=0}^{T} \delta^t (p_t - w - k_s(t)) \leq \sum_{t=0}^{\hat{T}} \delta^t \bar{p}_t,$$

so it is bounded by $\sum_{t=0}^{\hat{T}} \bar{p}_t < +\infty$ as $\delta \to 1$.

Now consider a contract with a constant service fee $p_t = p\ \forall t \geq 0$, where

$$p \in (\max\{k_c(1) + w - v, k_s(0) + w\}, k_c(0) + w - v)$$

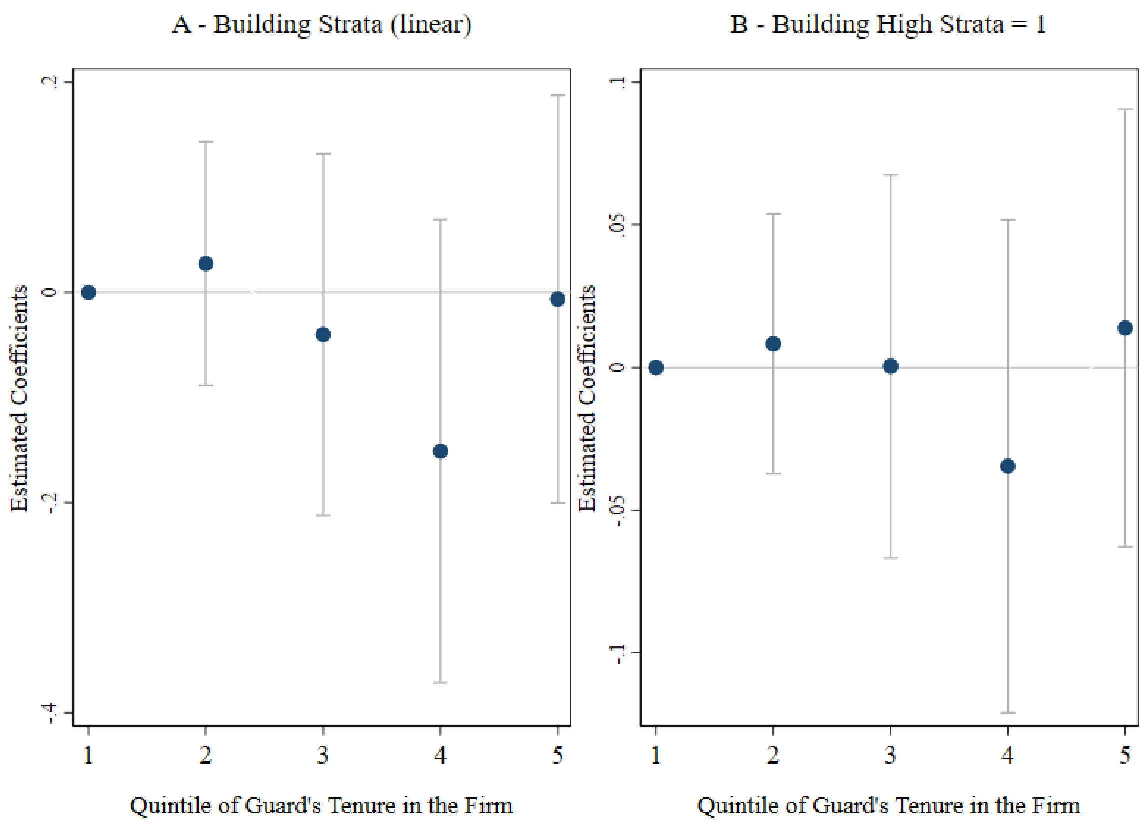
is well-defined given the parametric assumptions, and a rotation scheme $r_t = 1$ for all $t \geq 1$ (i.e., the firm rotates its workers after every period). Provided that ρ is sufficiently small (or α is sufficiently large if we employ the specification that $k_c(e_t) = \alpha/(1 + \beta e_t)$), we can replicate the arguments in Proposition 1 and show that 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

$$\lim_{\delta\to 1} \Pi_1 = \lim_{\delta\to 1} \sum_{t=0}^{+\infty} \delta^t = [p - w - k_s(0)] = \sum_{t=0}^{+\infty} [p - w - k_s(0)] = +\infty.$$

This implies that for δ sufficiently close to 1, any contract that does not rotate workers will be dominated (in terms of the firm's expected pay-off) by a contract that does. In other words, as long as 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. $\square$

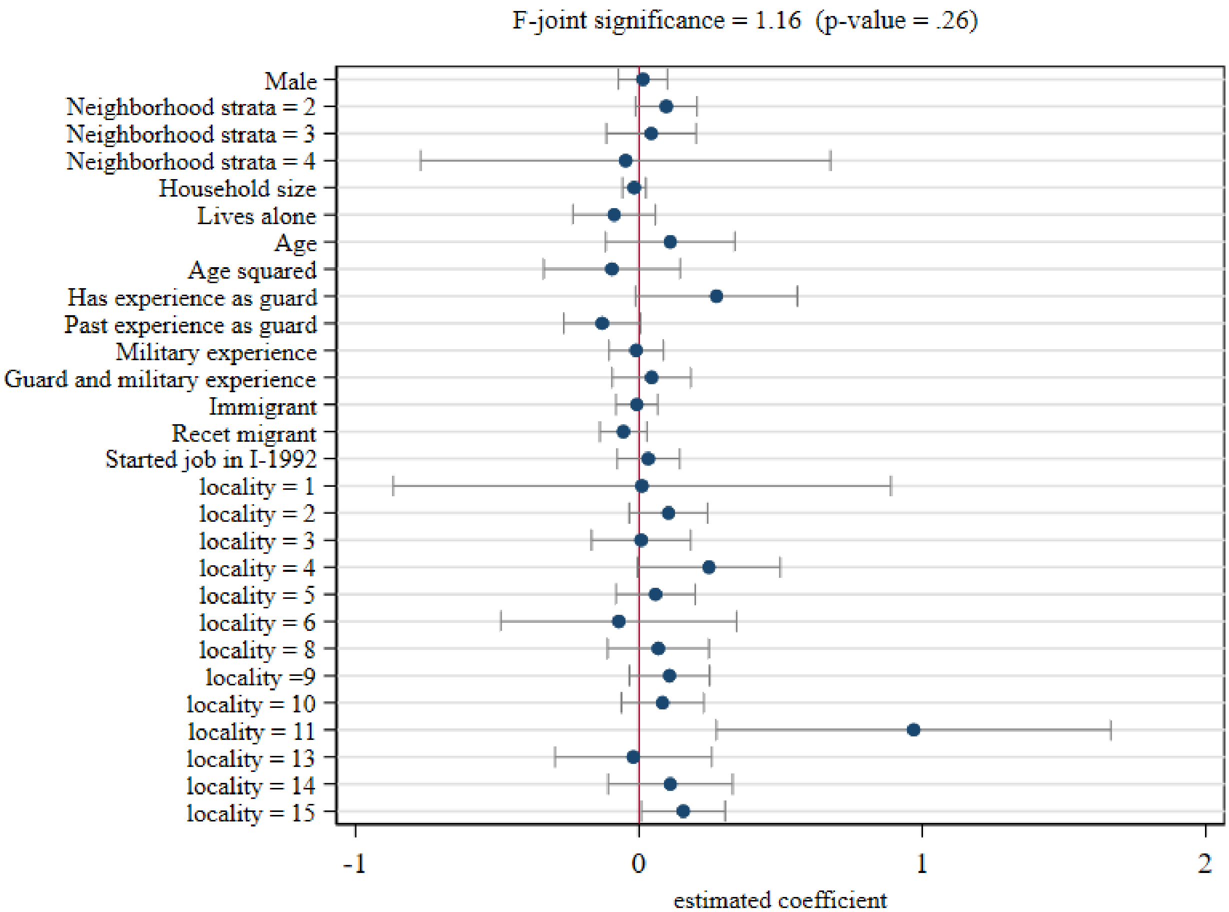
C Additional Figures and Tables

Figure C1: Building Socio-economic Strata and Guard's Tenure



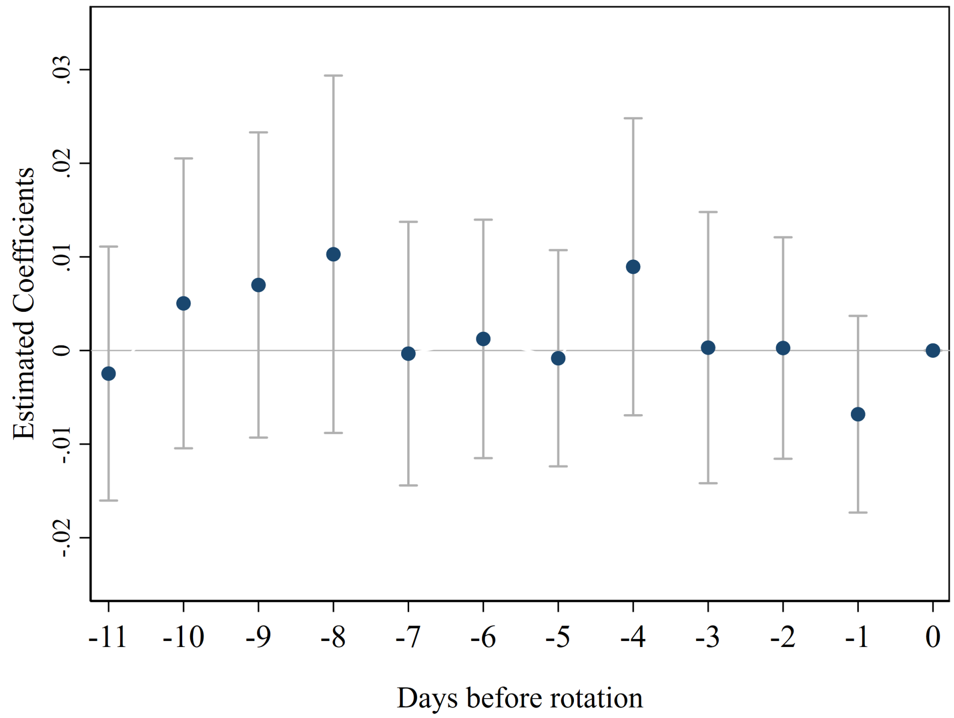
This figure displays the estimated coefficients and the 95% confidence intervals of regressions of the building's strata and indicators for the quantile of guard's tenure within the firm. The regressions have controlled for both guard fixed effect and month fixed effect. In Panel A, the dependent variable is the socio-economic strata of neighbourhood 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 socio-economic strata (stratas 5 and 6). Standard errors are clustered at the guard level. $N = 656,438$.

Figure C2: Balance Tests for Type-I vs. Type-II Allocation



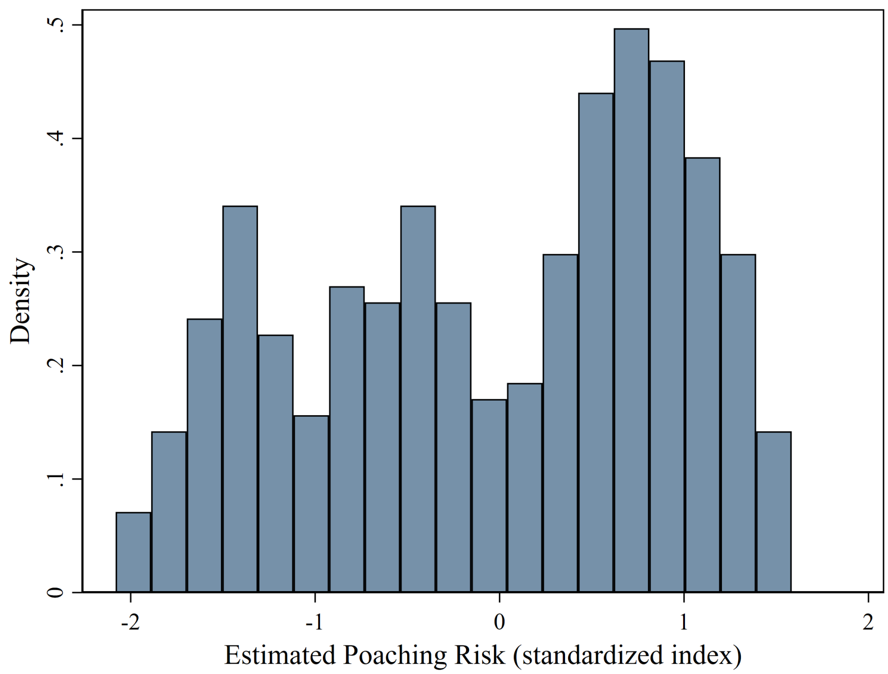
The figure displays the estimated coefficients and the 95% confidence intervals of a regression, where the dependent variable is an indicator of the guard being type-II and the explanatory variables are predetermined characteristics of the guard. Non-dummy variables are standardized. The figure also reports the F statistic of a joint significance test for all coefficients being equal to zero and the associated p-value. $N = 534$.

Figure C3: Evolution of Crime Before Rotation



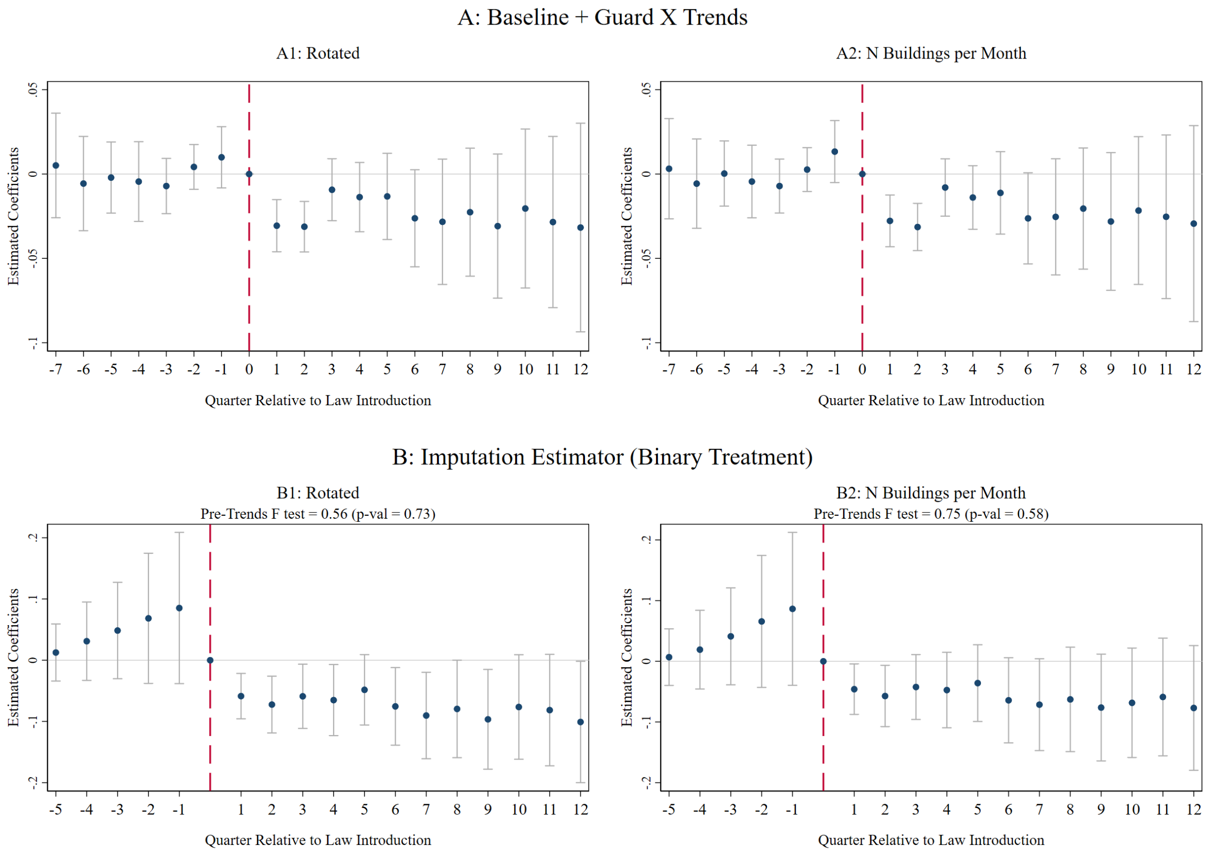
The figure displays the estimated coefficients and the 95% confidence intervals of a regression, where the dependent variable is an indicator of whether a crime occurred during the shift of the guard, and the explanatory variables are dummies indicating the days before the guard is rotated to a different 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. $N = 208,620$.

Figure C4: Distribution of the Poaching Risk Index (standardized). Random Forest estimation



The figure displays the distribution of the estimated index of poaching risk at the guard level. The index is constructed as the predicted probability from a Random Forest estimator (calculated as the average voting across 500 trees). The Random Forest model uses two categories (poached/solicited vs. non-poached/non-solicited) and is based on a Gini impurity loss function with bootstrapped samples and asymmetric weights to account for the imbalanced (i.e. few) number of poaching episodes. The estimated score is standardized with zero mean and unit standard deviation.

Figure C5: Effects of the Decree 356 on Rotation. Lead-Lags controlling for guard linear trends



This figure shows the lead and lags effects of the Decree 356 on the rotation of guards. The dependent variable in Panels A1 and B1 is an indicator for whether the guard rotated to a different building during the month. In Panels A2 and B2, the dependent variable is the number of buildings in which the guard worked during the month.

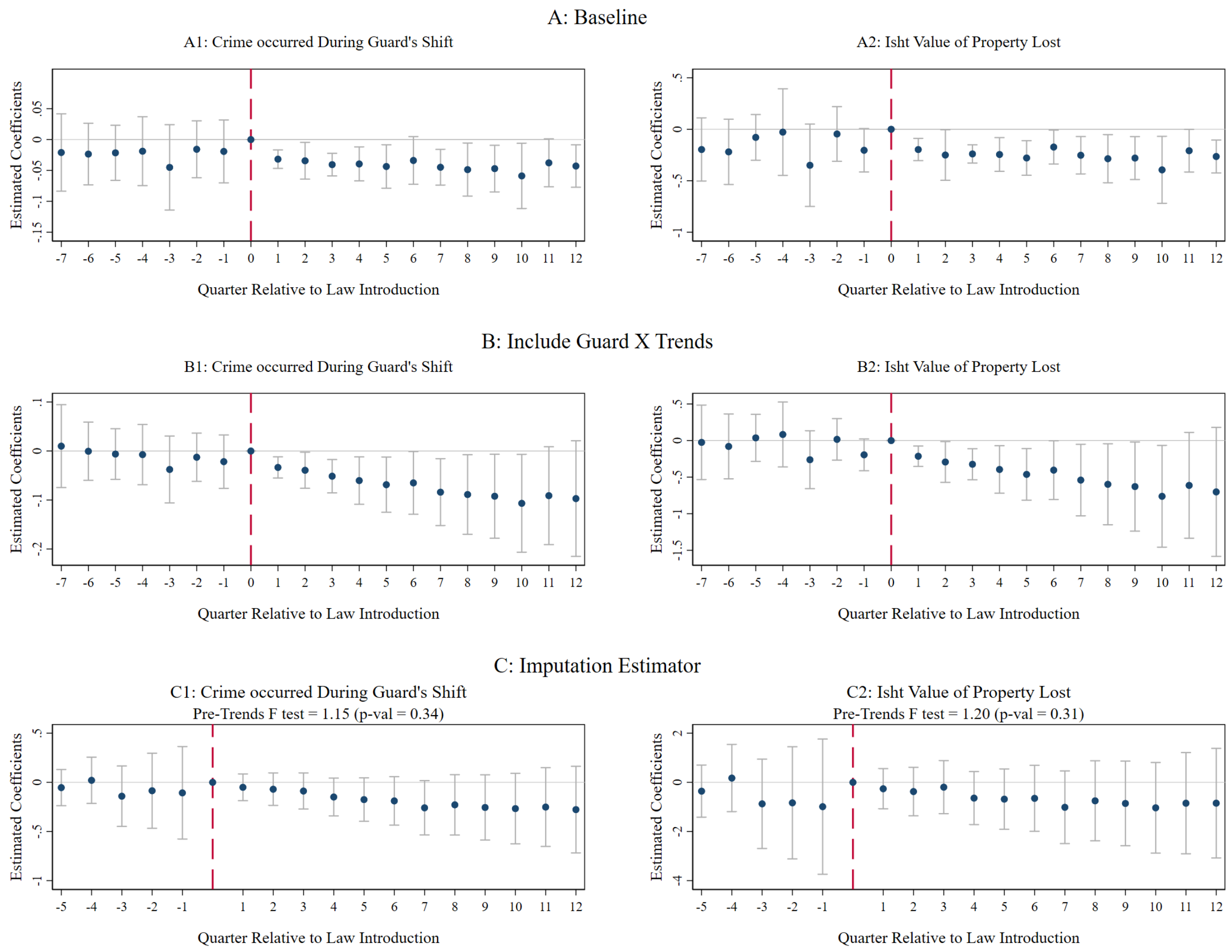
Panel A displays the estimated coefficients and the 95% confidence intervals of interaction between the estimated risk of being poached, with leads and lags indicators relative to the quarter when the degree was introduced. The omitted category is the interaction with the quarter period of the introduction of the law. All regressions control for guard fixed effects, month fixed effects and guard-specific linear trends. Observations are at the guard-month level. Standard errors are multi-way clustered at the guard-month level. $N = 15,313$.

Panel B reports the pre-trends and treatment effects using the imputation estimator proposed in Borusyak et al. (2021). Specifically, the estimation is based in the following equation:

$$Y_{it} = \sum_{j=-K}^{+K} \left(\beta^j HighRisk_i \times After_t^j \right) + \phi X_{it} + \eta_i + \gamma_t + \theta_i \times t + \varepsilon_{it}.$$

The specification is similar to the one used in Panel A and is defined over the same sample but the "treatment" is defined by the binary variable $HighRisk_i$ (which takes the value 1 if the guard is above the median of estimated poaching risk across all guards). Standard errors are clustered at the guard-level. We also report the F-statistic (and p-value) for testing parallel pre-trends following the procedure discussed in Borusyak et al. (2021). $N = 15,313$. A limitation of this approach is that it requires defining sharp treatment and a control groups, which we emulate by dividing guards into high (above median) and low (below median) poaching risk groups. We test for the existence of pre-trends using only five lead periods and we obtain a non-significant F statistic.

Figure C6: Effects of the Decree 356 on Crime



This figure shows the lead and lags effects of the Decree 356 on crime. The dependent variable in Panels A1, B1 and C1 is an indicator for whether a crime occurred during a shift where the guard was working. In Panels A2, B2 and C2, the dependent variable is the (ihst) value of the property lost due to crime. **Panel A** displays the estimated coefficients and the 95% confidence intervals of interaction between the estimated risk of being poached, with leads and lags indicators relative to the quarter when the degree was introduced. The omitted category is the interaction with the quarter period of the introduction of the law. All regressions control for guard fixed effects and month fixed effects. Observations are at the guard-month level. Standard errors are multi-way clustered at the guard-month level. **Panel B** is similar to Panel A but regressions also control for guard-specific linear trends. **Panel C** reports the pre-trends and treatment effects using the imputation estimator proposed in Borusyak et al. (2021). The reported coefficients corresponds to the interactions between the leads and lags indicators with a binary variable taking one when the guard is above the median of the estimated poaching risk. $N = 15,313$.

Table C1: Investigating 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 Flats in the Building	0.96	(0.50)	0.92	(0.55)
N Required Guards	1.42	(0.14)	1.60	(0.07)
Socioeconomic Strata Neighborhood	1.23	(0.25)	0.98	(0.48)
High Strata Neighborhood	1.67	(0.06)	1.08	(0.38)
City Area = South	1.29	(0.21)	1.52	(0.10)
City Area = Center	1.01	(0.46)	1.30	(0.20)
City Area = West	0.40	(0.98)	0.47	(0.96)
City Area = East	0.88	(0.60)	0.60	(0.89)
N	1,437		589	

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

This table reports the F-statistic and the corresponding p-value for cross-section regressions of building characteristics (dependent variable in each row) on guards' characteristics. Each cell refers to a different regression. In Column (1), observations are 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 occurred During Guard's Shift**			
Log Experience in Building ($\div$ 100)	-.11***	-.076***	-.076***	-.38***
	(.04)	(.028)	(.028)	(.12)
N	611,193	611,193	611,193	611,193
F first-stage	-	-	-	162
Mean Depvar	.009	.009	.009	.009
Panel B:	**IHST Value of Property Lost in Crime**			
Log Experience in Building ($\div$ 100)	-1.3***	-.95***	-.95***	-4.6***
	(.48)	(.34)	(.34)	(1.4)
N	611,193	611,193	611,193	611,193
F first-stage	-	-	-	162
Mean Depvar	.11	.11	.11	.11
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

N guards = 589; N buildings = 116. 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 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 162.06. All regressions control for dummies of quintiles of total experience.

Table C3: Crime Behaviour after Guard's Rotation. Event Study. Interaction with Experience in the Building

Control Group is Non-Rotating Guards at:	(1) In and Out Buildings	(2) Only Out Building	(3) Only In Building
Panel A:	**Crime Occurred During Guard's Shift**		
Post Rotation × Rotat Guard ×:			
High Exp in Building	.0035***	.0041***	.0042***
	(.0012)	(.0013)	(.0013)
Low Exp in Building	.0032	.00088	.00038
	(.0032)	(.0032)	(.0032)
N	205,254	119,830	125,794
Panel B:	**IHST Value of Property Lost in Crime**		
Post Rotation × Rotat Guard ×:			
High Exp in Building	.042***	.049***	.051***
	(.015)	(.016)	(.016)
Low Exp in Building	.034	.0085	.0025
	(.038)	(.038)	(.038)
N	205,254	119,830	125,794

N of guards = 417; N pf buildings = 103; Mean Depvar A = .011; Mean Depvar B = .128. This table investigates the evolution of crime occuring while the guard is on duty during the months before and after rotation. Observations are at the guard-date level. The sample is restricted to type-I guards and during a window of 2 months before/after a rotation in the sample takes place. For this window of time and for each rotation, we include all observations from the rotating guard (treated guard) and her co-workers at the rotating (in or out) building (control guards). We exclude guards with less than 6 months of experience in the building. This set of observations is labelled as a rotation episode. The regresion sample is constructed by stacking the observations for 525 rotation episodes observed after July 1992. In Panel A, the dependent variable is an indicator for a crime occurring during the shift when the guard was working. In Panel B, the dependent variable is the inverse hyperbolic sine transformation of the value of property lost due to crime. The main independent variables are the triple interaction between an indicator for the 3 months period after rotation takes place, an indicator for being the rotating (i.e. treated) guard and an indicator for the guard being above (first row) or below (second row) the median of building-specific experience. The regressions also control for the double interaction between the 3 months period after rotation takes place and the indicator for the guard being above the median of building-specific experience. All regressions control for the interaction between guard and rotation episode fixed effects and the interaction between the three months after rotation and rotation episode fixed effects. We also include week fixed effects, building fixed effects, neighborhood × month fixed effects and the interaction between night-shift and day of the week fixed effects. Additional controls are (log) total tenure of the guard in the firm and shift × day of the week fixed effects. Standard errors clustered at the guard-rotation episode window.

Table C4: 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)	.1***	.55**	.11***	.61*
	(.03)	(.25)	(.036)	(.32)
N	40,169	40,169	40,084	40,084
F first-stage	-	29.28	-	24.87
Method:	OLS	IV	OLS	IV
Total Experience Quantiles:	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

N guards = 452; N buildings = 116. All regressions are at guard (building) $\times$ week level. The dependent variable is an indicator for the week when the guard is hired in-house by the building and the sample is restricted to the period before the 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 workers type 3 and the tenure of the guardr 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 C5: Estimated Poaching Risk and Guard's Characteristics

	(1) Correlation with Baseline Chars	(2) Gini-based Importance
Male	1.8***	0.184
	(.059)	
Military Experience	.091*	0.022
	(.051)	
Neighborhood Strata	-.057	0.030
	(.052)	
Household Size	.095***	0.101
	(.034)	
Lives Alone	-.43***	0.014
	(.086)	
Age	.02	0.133
	(.029)	
Past Experience	-.23***	0.168
	(.049)	
Had Experience as Guard	.31***	0.023
	(.069)	
Immigrant	.2**	0.020
	(.088)	
Years Since Migration	-.47***	0.116
	(.044)	
Neighborhood of Residence FE's (Std Error /Combined Importance of FE's)	.363	0.171
Joint F Residence FE's	11.16	
N	389	
R2	.79	
F	94	

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 of a regression using the predicted score as dependent variable. The regression also include fixed effects for the neighborhood where the guard lives and we report the standard deviation of the estimated coefficients. Column (2) shows the Mean Decrease in Gini Impurity of each variable, which is a measure of the relative importance of each 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 C6: Effect of the Policy on Guards's Rotation and Crime
Alternative Measures of Poaching Risk

Risk Measure	(1) Alternative Risk Index	(2) Age	(3) Male	(4) Household Size	(5) Immigrant	(6) Medium-High Experience
Panel A:			**Rotated**			
Post × Poaching Risk	-.011***	-.03**	-.055***	-.0015*	-.0077	-.0086
	(.0021)	(.012)	(.0064)	(.00085)	(.0054)	(.0082)
N	15,161	17,599	17,599	17,599	17,599	17,599
Mean Depvar	.025	.024	.024	.024	.024	.024
Panel B:			**N of Buildings per Month**			
Post × Poaching Risk	-.012***	-.023*	-.055***	-.0011	-.012*	-.017*
	(.003)	(.012)	(.007)	(.00091)	(.0058)	(.0097)
N	15,161	17,599	17,599	17,599	17,599	17,599
Mean Depvar	1	1	1	1	1	1
Indiv Chars:	YES	YES	YES	YES	YES	YES
Month FE:	YES	YES	YES	YES	YES	YES
Guard FE:	YES	YES	YES	YES	YES	YES
Building (most worked) FE:	YES	YES	YES	YES	YES	YES

N of guards = 462; N of buildings = 113. This table investigates the effects of the introduction of the policy on two measures of guards' rotation. Each column reports the coefficient of the interaction between an indicator for the period after the law was introduced and a different measure of the risk that the guard is poached. In Panel A, the dependent variable is an indicator for whether the guard is rotated to a new building during the month. In Panel B, the dependent variable is the number of buildings in which the guard worked during the month. In Column (1), the measure of risk is a random forest prediction where solicited guards are excluded from its estimation. In Columns (2) to (6), the measure of risks are the age, an indicator for the guard being male, the size of the household, an indicator for the guard being immigrant and an indicator for the guard having between 4 and 10 years of past experience when joining the firm (where the interval corresponds to the deciles of past experience with the highest positive orrelation with the poaching risk index). All regressions include fixed effects of guard, month and the building where the guard worked most time during the month. Additionally, all regressions control for the number of days the guard worked during the month and the log-experience of the guard. Robust standard errors clustered two-ways at guard and month level are shown in parenthesis.

Table C7: Effect of the Policy. Day level regressions

Dependent Variable	(1) Rotated	(2) N of Crimes	(3) ishtVal PropLost	(4) Rotated	(5) N of Crimes	(6) ishtVal PropLost	(7) Rotated	(8) N of Crimes	(9) ishtVal PropLost
Post $\times$ Poaching Risk	-.0008***	-.0012*	-.012*	-.00093***	-.0012*	-.012*	-.00071***	-.0019***	-.018***
	(.000092)	(.00065)	(.0059)	(.00018)	(.00068)	(.0062)	(.00021)	(.00068)	(.0063)
N	399,781	399,781	399,781	399,781	399,781	399,781	399,781	399,781	399,781
Mean Depvar	.00094	.0079	.07	.00094	.0079	.07	.00094	.0079	.07
Indiv Chars:	YES	YES	YES	YES	YES	YES	YES	YES	YES
Date FE:	YES	YES	YES	YES	YES	YES	YES	YES	YES
Guard FE:	YES	YES	YES	YES	YES	YES	YES	YES	YES
Building FE:	YES	YES	YES	YES	YES	YES	YES	YES	YES
Guard Trends:	NO	NO	NO	YES	YES	YES	YES	YES	YES
Building Trends:	NO	NO	NO	YES	YES	YES	YES	YES	YES
Guard X Transition:	NO	NO	NO	NO	NO	NO	YES	YES	YES

N guards = 348 ; N buildings = 113. This table investigates the effects of the introduction of the decree on guard's rotation and crime using guard-date level observations. 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 Columns (1), (4) and (7) the dependent variable is an indicator of whether the guard was rotated to a new building that date. In Columns (2), (5) and (8) the dependent variable is the number of crimes that occurred in the building in the shift when the guard was working. In Columns (3), (6) and (9) the dependent variable is the (ihst) value of the property lost for the crimes occurred in the building during the shift when the guard was working. All regressions use observations at the guard-date level, and include fixed effects of guard, date and the building. All regressions also control for the total log-experience of the guard and a shift (day or night) indicator. Columns (4) to (9) include guard-specific and building-specific monthly linear trends. Columns (7) to (9) control for the interaction between guard fixed effect and an indicator for the two quarters after the law was introduced. Robust standard errors are clustered two-ways at the guard-month level and are shown in parenthesis.

Table C8: Client Experience and Poaching in the Lobbying Industry

Dependent Variable	Lobbyist Hired In-House
Previous Client Experience	4.2***
	(.17)
N	1141761
Client FE:	YES
Lobbyist FE:	YES

N clients = 992 ; N lobbyists = 1183. This table shows the relation between past client experience of lobbyist and the probability of being hired in house by the client. The sample consists of all possible client-lobbyist pair combinations (including only those lobbyists who worked for a lobbying company and switched to working in-house for a client). The table reports the estimates of an Alternative-Specific Conditional Logit (McFadden, 1984) and includes client and lobbyist fixed effect. The independent variable is a dummy indicating that the lobbyist worked for the client in the past before being hired in-house. Standard errors clustered at the lobbyist level.