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 their workers leave and work for competitors (Aghion and Bolton, 1987; Krueger and Ashenfelter, 2018; Lipsitz and Starr, 2022). 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 concern that has received less attention but is also important for service firms: their workers can leave and work for clients.

On the job, outsourced workers accumulate experience that makes 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 employees and clients, service firms may take costly actions to prevent poaching.[1] We argue that among the set of tools available to deter poaching, one of them consists of rotating workers from one client to another. By doing so, service firms hinder 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 talent poaching from clients 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

[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.

unsurprising that the issue has drawn public attention in various 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 on the transition and performance of service workers across multiple clients and their poaching behaviour. To overcome this challenge, we concentrate on the security-service industry, where we have access to detailed data from a single firm that allows for an in-depth examination of a phenomenon in its real-world context. Focusing on this particular case study provides an appropriate framework to investigate the issue of poaching for two reasons. First, in the middle of our sample period, a non-poaching policy was implemented by the government in the country where our partner firm is located, giving exogenous variation to the extent that poaching behaviour is allowed. Second, we have access to a very extensive dataset. 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. Additionally, 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. In particular, the data shows that about 8% of the guards were poached at any point before the policy change. Finally, we also have information on one of the most important measures of guards' productivity: crimes committed in buildings during guards' working shifts.

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 reduced 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 argue 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 decreases.[3] These results are robust to different alternative exercises such as an instrumental variable approach (for both crime and poaching activities as dependent variables) based on the system that the firm uses to allocate guards to shifts.

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. 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 (58%) compared to the average monthly rotation before the policy took effect. We complement this result by showing that guards who were rotated less frequently prior to the

[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.

policy change exhibited the greatest productivity increases, as evidenced by larger decreases in crime.[4]

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 took place, the firm reduced rotation allowing workers to acquire larger CSS and as a consequence, crime rates decreased. An important lesson from our results is that in environments where service companies take costly actions to avoid poaching, a policy that prohibits poaching can increase the productivity of 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 pursue two supplementary approaches.

Firstly, we present extensive survey evidence from firms in the security sector as well as anecdotal evidence from firms in other industries (e.g., legal, software development and cleaning services). With this qualitative evidence, we are able to provide arguments in favor of the validity of assumptions made in our study as well as provide arguments in favor of the generalizability of our core findings. For instance, we find that vertical poaching is a salient issue in these industries and that in many cases rotation is a managerial strategic response to the poaching problem.

Secondly, we propose a theoretical model that captures the strategic tension arising from empirical settings that as ours are prone to both poaching and rotation. In our model, a firm employs a pool of workers and transacts with a client. At the outset, the client, who lacks the necessary in-house labor, outsources a production activity to the firm. As a worker gains productivity-enhancing experience by performing the client's activity, the client may find it cost-efficient to hire that worker directly. We show that, even with other retention tools available (e.g., pecuniary incentives, amenities or non-poaching contracts), the firm may prefer to preempt poaching by inefficiently rotating workers before they reach a client-experience threshold. In equilibrium, workers with more desirable characteristics (e.g., larger

[4] A limitation of the setting is the lack of a natural control group, therefore our analysis compares the change in rotation among guards categorized ex-ante as high-risk or low-risk for poaching, based on the machine learning metrics described previously.

baseline productivity) face higher poaching risk and are rotated more frequently. Accordingly, our model corroborates that a non-poaching policy can increase worker productivity by eliminating strategic over-rotation. Finally, the model identifies several factors influencing a firm's optimal anti-poaching approach, including the client's costs associated with poaching initiation and worker preferences.

Related literature. The literature has long recognized that job rotation can impede skill accumulation and decrease job-specific productivity (Ickes and Samuelson, 1987; Groysberg et al., 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.[5] 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 their 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

[5]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 that crime decreases as guards accumulate longer building-specific tenure.

contribute to this literature by showing that in the complementary case where the firm cannot avoid poaching from its clients, the acquisition of job-specific skills 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, 2022). This type of clause limits workers from leaving their current employers and working 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).[6] 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 uses additional survey and anecdotal evidence to illustrate the generalizability of our findings. Section 7 concludes. Key figures and tables are contained in Appendix A, while additional supplementary materials, including extra figures, tables, proofs and details of different estimations can be found in the online Appendix B.

2 Institutional Setting

We partnered with a private firm in Colombia that 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 professional experience, age, gender and residential address. For each building, we know who worked

[6] The literature has also identified factors that can constrain worker mobility even in the absence of employer intervention, such as organizational status (Bidwell et al., 2015), search and switching costs (Wright et al., 1994), or limited information about outside options (Campbell et al., 2012).

there and when, where it is located, the number of flats, the required number of guards, and the type of crime that 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).[7] 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 guards working in the same building, since the rest times of the latter two are staggered. Thus, in a typical week, a building needs two type-I guards and one type-II guard to cover all the shifts.[8]

Panel A of Figure 1 illustrates a typical timetable of three guards working in the same building for a period of 16 days. The two type-I guards are labeled as e1-A and e1-B, while the type-II guard is labeled as e2. On days 7 and 8, guard e1-B rests and guard e2 covers the day shifts. On days 13 and 14, guard e1-A rests, and consequently guard e2 covers the night shifts. 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.

[7]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.

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

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 Appendix B.2, we present empirical evidence consistent with this explanation.[9]

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 another to avoid poaching. When these rotations occurred, they were typically communicated to both the building and the guard about one week prior to the rotation date.

Workers do not necessarily have the same preferences between working directly for clients and being employed by security companies. There are trade-offs to consider. Average wages are determined by the market and do not significantly differ between internal and external hiring. Working directly for clients provides guards with a more amicable environment (closer worker-client ties post-poaching) and assures that in expectation they will be working in the same place for a long period. The latter factor is appreciated by the guards, because security companies often fail to consider workers' home locations and transportation expenses in rotation planning. In contrast, employment through security companies provides advantages like better training opportunities (besides the initial training, there may be some short courses about new security services techniques), as well as job security independent of particular clients. Although we do not have information about contract specifics, our conversations with several firms in the sector indicate that the contracts that workers sign do not substantially differ whether they are with security companies or with clients directly.

[9] We do not view this decision-making process as irrational or lacking strategic considerations. On the contrary, the cost of delaying hiring to perfectly match applicants to vacancies based on specific characteristics would be prohibitively high. This is because the skills required for different guard types are very similar, and the period under analysis was characterized by a high labor tightness (Alvarez and Hofstetter, 2012, 2013). Although we lack direct evidence on how the allocation process was done in each specific case (and cannot rule out that some exceptions existed), the firm claimed that it was more efficient to fill positions quickly with the first suitable applicant. This claim is confirmed by our survey of other firms in the industry.

Conversations with buildings that initiated poaching show that usually, they have other potential guards lined up to cover the remaining shifts before poaching occurs. These potential substitutes (of the poached guard) often include former in-house guards, or referrals from those guards, residents, or new hires. 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 every poaching case, we observe the identity of the hired guard, the building that initiated the poaching, and the exact date that the guard left the firm. A building can poach a worker that is currently working or has worked for them in the past. In our data, all poaching episodes happened 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, our qualitative evidence indicates that most of the 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 for each crime episode.

Finally, buildings always provide all necessary materials and amenities (like a staircase, heater, etc) that can increase guards' productivity from the start of the contract. Failure to meet this condition will leave the building uninsured in case of crime.

Rationales for Rotation There are multiple reasons why firms in our empirical context decided to rotate workers. We classify them into two categories: strategic and non-strategic. Strategic rationales relate to the firms' motive to deter vertical poaching, while non-strategic rationales encompass motives that are unrelated to that goal. Surveyed firms reported numerous non-strategic reasons for rotation: guards requesting reassignment to buildings closer to their homes; guards seeking transfers due to personal adversaries living near their previous buildings; guards needing sick leave; and guards asking for time off to attend to personal matters, etc. We consider these reasons for rotation to be mainly idiosyncratic, resulting from changes in the guards' personal circumstances. As such, we expect them to occur from time to time throughout the sample period and therefore should not be affected by the policy change. This implies that there should be some level of rotation absent strategic motives. Our survey evidence also indicates that rotation did not damage the firm reputation, since it was a common practice in the sector in the 1990s.

2.2 Client-Specific Skills in Our Context

One of the most important tasks of a guard is to control entry into buildings and invigilate for the presence of potential criminals. When a visitor arrives, the guard contacts the resident that the visitor wishes to see and verifies if the guest 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.

According to our partner firm and surveys, the CSS of a guard also include the understanding of the inner workings of the building. Guards accumulate this knowledge over time, allowing them to prevent crime more efficiently. This is the case because criminals not only try to enter the building by registering with the guard at the entry but also by other means. The longer a guard works in a building, the more likely she would be able to detect when and how criminals try to enter the building in abusive ways. In this sense, the performance of a new guard may be different from a more experienced peer, as the latter would be more likely to anticipate the various ways in which criminals might sneak in to steal residents' property.

Finally, CSS may also include other knowledge-related aspects, such as knowing the capabilities and personalities of other guards, knowing who knows what in the building, and knowing how to cooperate with fellow workers.

3 Theory

Before proceeding to analyze the data, we present a dynamic agency model that accentuates the key tension arising from our empirical setting: the accumulation of client-specific skills increases not only productivity but also poaching risk. Our goal with this model is twofold: First, we aim to develop formal and testable predictions to guide the subsequent empirical analysis. Second, we seek to shed light on the generalizability of our proposed mechanism, specifically, under what circumstances we would expect service-providing firms to utilize rotation over alternative strategies to counter client-poaching.

Our model's proposed mechanism is underpinned by two key assumptions, which we will first validate empirically. The first assumption is that the firm-client relationship ends when poaching is attempted, irrespective of whether it is successful or not. If this were not the case, for example, if the two parties could efficiently bargain over surplus division after a poaching attempt, then the need for counter-poaching efforts like strategic rotation would greatly diminish. The second assumption is that the departure of employees to a client results in substantial costs for the firm. If poaching does not lead to significant costs, for instance, if having a former employee working on the client side can facilitate future business opportunities (e.g. Somaya et al., 2008), then the service-providing firm may actually wish to encourage rather than deter poaching.

Multiple sources of empirical evidence support the validity of the first assumption in our setting. In our data, every client that poached a worker disappeared from the sample, indicating termination of transaction post-poaching. Indeed, our partner firm confirmed that clients typically cease requiring services after poaching attempts, and conducting future business with those clients is unlikely due to the loss of trust. Our survey evidence corroborated that this reaction to clients' poaching behavior is common in the security industry. For instance, one firm stated: "*When clients poached a guard it was a headache. Not only would the good guard leave, but any business with the client would terminate.*"[10]

[10] Original Spanish: Cuando se robaban un guardia era un dolor de cabeza. No solo se iba el guardia, que era bien bueno, sino ademas se acababa el negocio con el cliente.

Next, we justify the second assumption by providing a back-of-the-envelope calculation of the cost imposed on the firm due to client poaching (see Appendix B.3 for details). We decompose this cost into three sources:

First, the firm experiences a temporary profit shortfall while searching for a new client to replace the old one. To quantify this impact, we calculate the monthly probability of acquiring a new client based on the average number of new clients gained per month by our partner firm during the sample period. Using this probability along with the typical profit generated per client, we estimate the expected foregone profits during the client replacement period to be $\$3,546$ (all figures are in 2020 US dollars henceforth).

Second, the productivity stemming from a new client-worker pair is lower than the previous, established one. This is because, initially, a worker may lack the specialized skills necessary to fully serve the needs of a client. Focusing on our empirical setting, we estimate a guard's productivity when newly assigned to a building is 36% lower compared to when her experience at that building reaches the median level of guards poached in the sample. We quantify this impact by calculating the change in the average monthly value of property lost that would occur due to the productivity decline. Additionally, we use information from Matthew et al. (2018) to account for other crime-related costs that are not captured by the monetary value of lost property. Our total estimated cost due to the loss of established client-specific human capital is $759.

Third, hiring a new worker to replace the poached one implies various expenditures – such as costs for advertising the opening, screening and selecting applicants, and training the new hires – that are difficult to observe. To overcome the data limitation, we turn to findings from prior studies that have estimated analogous hiring costs. Specifically, following the methodology used by Manning (2011), we arrive at an estimated total hiring cost $615 in our empirical setting.

Taken together, our calculation indicates that a poaching episode costs around $\$4,920$ to the firm in total. This cost is substantial, amounting to about 20 times the monthly minimum wage.[11]

[11]Using a similar methodology and assumptions, we can also estimate the cost of rotation. In this case, the main cost stems from the lost productivity that a guard experiences after being reassigned. Our back-

3.1 Model

We consider a client (or client organization, he) that repeatedly engages in a production activity at period $t = 0, 1, 2, ... + \infty$. Performing the activity requires a unit input of labor (from a worker, she) at every period. At the beginning of the game, the client does not have an in-house worker, so he outsources the activity to a service firm that specializes in providing such a workforce.[12] The productivity of a worker depends on the experience $e \in \mathbb{N}$ (i.e., the number of periods) that the worker has accumulated while serving the client. Specifically, we assume that a worker generates a surplus $z(e)$ that strictly increases in his client-specific experience. For instance, the worker may become increasingly adapted to the working environment and proficient at completing the required tasks, allowing the firm to better protect or insure the client from adverse events at a lower cost. All players are risk-neutral and share a common 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 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. The client then decides whether to accept the firm's service or not.

If the client decides to accept the service, he simply gets a fixed payoff $\underline{v}$ and pays a fee p (or any time-independent transfer of surplus) to the firm, where $0 < p < \underline{v}$, and the stage game ends. In this case, the flow payoffs accrued to the firm and the worker are given by $\pi_t = p - \underline{w} + \theta + \alpha z(e_t)$ and $u_t = \underline{w} + (1 - \alpha)z(e_t)$, respectively. Here, $\underline{w} > 0$ is the default wage that the firm pays to its employees (e.g., minimum wage as our qualitative evidence shows). The parameter $\theta \in \mathbb{R}_+$ captures the baseline productivity of a worker, while $\alpha \in (0, 1]$ measures how surplus generated from client-specific experience is divided

of-the-envelope calculation indicates the cost of rotating a guard, at the observed average rotation timing, is approximately \$199. This amount is substantially smaller than the estimated \$4,920 loss per poaching episode. We acknowledge that these calculations rely on measures of averages across guards and require many strong assumptions. However, the sizable difference between the estimates suggests that rotating guards is likely a cost-efficient strategy even when one modifies some of these assumptions.

[12] This assumption holds true in our empirical context, as buildings do not directly recruit guards from the labor market. More broadly, this assumption is reasonable if service firms exhibit increasing returns in training workers and/or have superior screening efficiency when hiring from the labor market (e.g. because they are more experienced or have specialized recruiters; see Vohra (2021) for a theory of how poaching may also impact firms' screening incentives in hiring junior workers).

between the firm and the worker.[13] To keep things simple, we do not model any *direct* costs of worker (re-)assignment (e.g. cost of rescheduling, worker annoyance, client inconvenience) beyond those impacting the accumulation and use of client-specific experience.[14]

Should the client opt not to purchase the service from the firm, he may attempt to poach the worker by proposing a wage offer w. The firm can then respond with a counteroffer w' (which encompasses both wages and amenity changes), but the worker is free to decide whether to stay or to leave. In addition, the client incurs a fixed cost $c_t = \underline{c} + \varepsilon_t \in \mathbb{R}_+$ for initiating poaching. This cost may incorporate expense for acquiring new equipment or recruiting complementary co-workers, administrative hassles, and potential loss of reputation. It can also reflect the binding nature and effectiveness of any non-poaching agreement between the involved parties: higher costs indicate situations where violating such agreements is more difficult or punitive for clients (e.g., higher legal costs in expectation). Specifically, the constant $\underline{c}$ represents baseline poaching cost, while the term ε_t captures stochastic fluctuations. For simplicity, we assume that ε_t is drawn i.i.d. across periods from a commonly known distribution $\Pr(\varepsilon_t = \varepsilon_L) = 1 - \Pr(\varepsilon_t = \varepsilon_H) = \lambda \in (0, 1)$, where $\varepsilon_L < \varepsilon_H$. In each period, the realization of ε_t is privately observed by the client before he makes the poaching decision. In the analysis that follows, we will set the values of ε_H and ε_L sufficiently apart whenever needed. This is done so that the timing of the client's poaching decision will depend in a meaningful way on which poaching cost is drawn (see Appendix B.4 for the exact parametric assumption made).

Furthermore, per the first key assumption discussed at the start of this section, the contractual relationship between the service firm and the client will end irreversibly, no matter the poaching result. In particular, if the client's recruitment attempt fails, he will thereafter receive zero payoff, while the firm will receive a constant flow payoff $\underline{\pi} - w'$, where $\underline{\pi}$ captures the firm's expected profit from finding a new client later, and w' is the wage from the counter-offer that the firm promised to the worker. Alternatively, upon successfully

[13]Assuming no client share under outsourcing is not that unrealistic in our empirical setting, because the buildings were fully insured (against losses from theft) by the security firm. Furthermore, our results will continue to hold provided that the client benefits substantially less from worker's productivity gains under outsourcing versus in-house production.

[14]Including these direct costs would be straightforward (e.g., we may assume that the firm has to pay a fixed cost every time it sends a new worker to the client). Our analysis will still hold as long as these costs are small enough compared to the cost imposed on the firm from client poaching. This appears to be the case in our empirical context, based on the back-of-the-envelope calculations in AppendixB.3.

recruiting the worker with wage w, the client will be able to produce in-house going forward. In this case, the flow payoffs accrued to the client and the worker will be $v_t = -w+\theta+\beta z(e_t)$ and $u_t = w + (1-\beta)z(e_t) + \gamma$, respectively. Here, the parameter $\beta \in (0,1]$ determines the surplus split between the client and the worker, while $\gamma \in \mathbb{R}$ represents the worker's relative preference for working for the firm versus the client. Notably, γ can incorporate both intrinsic preference and any direct cost that the worker pays by accepting the client's poaching offer, such as repayment to the firm for breaching a non-compete clause. Meanwhile, the firm, now needing to find both a new client and a new employee, will receive a constant flow payoff $\underline{\pi} - \kappa$ going forward, where $\kappa > 0$ incorporates factors such as hiring and training costs. Aligned with the second key assumption discussed earlier (that losing workers to a client is very costly for the firm), we take the value of $\underline{\pi} - \kappa$ to be sufficiently small, thereby avoiding any complications from the firm preferring its workers be poached.

3.2 Equilibrium analysis

The equilibrium analysis of our model is non-trivial because the client's poaching decision and the firm's rotation scheme are mutually influenced by each other. However, it's clear that the client's preference is to first utilize the firm's service, and then switch to in-house production later on once the assigned worker has gained enough experience to become highly skilled at the task. Leveraging this monotonicity of the client's poaching incentive, our main theoretical result below establishes the existence of an equilibrium in which the firm strategically implements a stationary rotation scheme.

Proposition 1. *Provided that λ is sufficiently small, there exists a Perfect Bayesian equilibrium in which:*

(i) every worker is rotated by the firm after having served the client for T_H periods, and

(ii) poaching of a worker occurs before rotation by the firm if the client draws a low poaching cost and the worker has served for longer than $T_L < T_H$ periods,

where the values of T_H and T_L are uniquely determined by the model's parameters.[15]

[15]We make two remarks on how the findings of Proposition 1 can be generalized. First, when λ is large (meaning that drawing a low poaching cost is likely), an equilibrium exists where the firm rotates workers sufficiently often to eliminate the poaching risk entirely. Nonetheless, we choose to focus on the case

Proposition 1 highlights that even if the firm has other tools at its disposal to counter the risk of poaching, such as offering higher wages and/or better amenities to its employees, rotation may still be the preferred strategy. Intuitively, since the outsourcing relationship will end whenever poaching occurs, the maximum wage that the firm is willing to pay to retain a worker is capped at the replacement cost κ. Thus, if the productivity gain from the worker's CSS accumulation eventually outweighs even a high poaching cost, the client will outbid the firm to hire the skilled worker directly. In this scenario, the compensation package that the firm can offer will be insufficient to deter poaching. However, rotation remains an effective pre-emptive tactic against poaching: by optimally setting T_H, the firm imposes a ceiling on the attractiveness of any individual worker to the client. This allows the firm to garner some productivity gains from growing experience while keeping the poaching risk low.[16]

Next, we summarize the testable predictions that emerge from our model, each based on a comparative statics result relating to the equilibrium described in Proposition 1. The first prediction, detailed in the proposition below, is that clients are more likely to poach workers who have accumulated more client-specific experience (and who are also more productive).

Proposition 2. *As a worker accumulates more experience specific to a client, his likelihood of being poached increases upon being assigned to that same client again.*

The second prediction is that workers with higher poaching risk will be rotated more frequently by the firm. Specifically, our next proposition formalizes the driving force behind this correlation: both poaching risk and rotation frequency are positively related to a worker's baseline productivity.

where λ is small, because we do observe both worker rotation and poaching in the data. Second, workers exhibit heterogeneity in CSS accumulation, which can be captured by worker-specific surplus functions $z(\cdot)$. Intuitively, this heterogeneity implies workers with the same client tenure but different learning speeds face differential poaching risks. Following similar steps to the proof of Proposition 1, one can construct an equilibrium where the firm tailors rotation frequencies to these worker-specific risks.

[16] If the enhanced productivity specific to the client requires investment by the workers, their incentives to do so may also depend on the rotation scheme. At first glance, it may seem clear that frequent rotation negatively impacts workers' investment incentives, because it directly hinders them from reaping the benefits of such investments. However, if workers strongly prefer transitioning to a client, rotation could theoretically intensify investment incentives; otherwise, workers may be unable to become sufficiently attractive even if the client receives a low poaching cost before rotation. Since our primary goal is to make a first step in demonstrating the *applicability* of rotation as a valid strategy to counter vertical poaching, we leave for future works to explore how the optimal *intensity* of rotation will be determined by the trade-off between the poaching risks and worker incentives.

Proposition 3. *Consider two groups of workers, where workers in the first group have higher baseline productivity than those in the second group. Then, workers from the first group will also: (i) face higher risks of poaching whenever assigned to a client, and (ii) be rotated more frequently by the firm.*

Our final proposition predicts a negative relationship between rotation frequency and poaching costs.

Proposition 4. *The frequency at which the firm rotates its workers decreases as the baseline poaching cost increases. Specifically, if the poaching cost is sufficiently high, the firm will cease using worker rotation as a strategy to counter client-poaching*

To sum up, our theoretical analysis demonstrates that the threat of poaching can lead to excessive job rotation, destroying valuable human capital. Implementing a non-poaching policy would halt this vicious dynamic — if poaching were prohibited, rotation should be merely driven by factors exogenous to our model (e.g. sick leave of workers). This would enable a greater accumulation of CSS, thereby increasing the productivity of workers. However, the policy may not improve welfare equally for all agents in the economy. The firm will unambiguously benefit from the policy because its business with the client will be protected and it can capture more surplus from the transaction due to the larger CSS of the workers. In contrast, the workers could be worse-off as the policy change cuts off their access to valuable outside options. Clients would also be affected, as they would no longer be able to poach workers that they like. Overall, we caution that the net welfare impact of the policy can be ambiguous, because it may depend on the relative magnitudes of these countervailing effects.

Remark on rotation vs. legal and/or managerial practices. Our model also sheds light on when we should expect service firms to be more or less likely to use job rotation ($T_H < +\infty$) versus other managerial practices, such as pecuniary incentives, amenities or non-poaching agreements ($T_H = +\infty$), to address poaching concerns.[17] One critical factor, as suggested by Proposition 4, is the legal cost paid by clients when initiating poaching: in industries or jurisdictions where this cost is sufficiently high (for instance due to strict enforcement of non-poaching agreements), firms would be able to deter poaching solely through

[17]For further discussion on this issue, see Subsection 6.2.

their capacity to offer higher compensation to workers. A close examination of Proposition 1's proof reveals several additional factors that influence a firm's optimal choice of anti-poaching instrument. For instance, the lower net value of the outsourcing service ($\underline{v} - p$) and higher worker productivity (θ and $z(\cdot)$) will both incentivize clients to make more competitive poaching offers. Similarly, a stronger worker preference for becoming an in-house employee (γ) makes it harder for the firm to match any poaching offer. Hence, all these factors should increase the desirability of rotation relative to pure compensation adjustment as a managerial practice to combat poaching.

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 a security guard, military training, and various socio-economic variables. 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 a large variation in terms of age and migration status among the guards. On average, guards tend to share the household with 5 additional family members, 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 do not depend on building-specific experience.[18] 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, for every building that they are assigned to, a type-I guard accumulates 26 shifts per month, while a type-II accumulates 9 shifts a month. Further, type-I guards work on average in 1.03 buildings per month and only 2% of them rotate each month. This contrasts

[18] We have wage information for a small subset of guards. Although this data is limited by several measurement error issues, we find that year-fixed effects and the years since the guard joined the firm explain more than 90% of the variation in real wages (indeed, a single-year linear trend explains 73% of the variation). This is consistent with the company's narrative that wages change in a very mechanical and predictable way based on minimum wage and tenure.

with type-II guards who work on average in 2.2 different buildings each month and rotate to a new building with a monthly probability of 4%.[19]

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 neighborhoods the buildings are located is 2.8. The strata value captures several measures of the quality of housing on a scale from 1 to 6. Neighborhoods with larger strata tend to be safer. The average building experiences 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 is about 47 USD. This corresponds to approximately 21% of the 1993 Colombian monthly minimum wage.

4.2 Client-Specific Experience, Worker Productivity and Poaching

Building-specific experience and guard's productivity. As characterized by our theoretical model, client-specific experience improves workers' productivity over time. Although we do not observe all the possible dimensions of guards' performance (e.g., efficiency of visitor 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 measure of productivity in this setting.

The importance of building-specific experience for crime prevention has been emphasized both by our partner firm and by other surveyed security companies. For instance, one firm stated: "*...the best guards are those that spend a significant amount of time in the building. Spending time with a client, helps them to understand the specific location that criminals can use to enter the building.*"[20] Likewise, interviewed companies from other sectors that assign workers to clients also noted that the client-specific experience is an important determinant of worker productivity. For instance, one cleaning company stated: "*...for them to work*

[19]Figure B1 shows that the typical rotation happens before the peak of workers' performance. The figure also shows that productivity, as measured by crime incidence, decreases monotonically over several months with a change in slope only around the 20th month.

[20]"...los mejores celadores son esos que llevan bastante tiempo en un edificio. Pasar tiempo con el cliente, les ayuda a entender por donde se pueden meter los criminales."

efficiently, they need to work in the same environment consistently."

Appendix Figure B1 shows a non-parametric estimation of the relationship between crime occurrence and building-specific experience. In particular, Panel A of the figure displays the estimated cross-sectional relation, while Panel B exploits within-guard variation in experience accumulated in a given building (controlling for the total experience across buildings). Both panels indicate a sharp and significant negative relationship between productivity and client-specific experience. However, this figure is largely descriptive and does not account for confounding factors. To provide more robust evidence on the role of building-specific experience, we estimate the following equation at the guard-building-week level:

$$Crime_{ibt} = \beta ExpInBuilding_{ibt} + \eta TotalExp_{it} + \delta_{ib} + \gamma_t + \epsilon_{ibt}, \tag{1}$$

where $Crime_{ibt}$ is an indicator for the occurrence of crime in a shift when the guard i was working at building b during week t. We also consider an alternative dependent variable: the inverse-hyperbolic-sine transformed (IHST) value of property stolen if a crime occurs.[21] Our main explanatory variable $ExpInBuilding_{ibt}$ is the number of shifts that the guard worked in the building (expressed in months). 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 δ_{ib} and exploit the variation in building-specific experience *within* each guard-building pair over time. We also include week fixed effects $\gamma_{m(t)}$ to avoid confounding the effect of building-specific experience with systematic changes in crime over time. Moreover, 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 experience of the guard $TotalExp_{it}$. 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. Finally, we control for the number of days a guard worked during the week, as the likelihood of encountering a crime is higher for guards who worked more days that week.

[21] 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. As a robustness exercise, we have also estimated equation 1 with the value of property stolen in levels, and the effect relative to the mean is roughly similar.

The first column in Panels A and B of Table 2 shows the estimates of equation (1). The estimated coefficients of building-specific experience are negative and significant. Column (2) shows that results remain similar if we control for the schedule characteristics of the guard, such as the number of night shifts that the guard worked at the building during the week, or whether the guard worked during the weekend. These estimates are large relative to the mean of the dependent variables. For instance, an additional quarter of a year (three months) of experience in a building is associated with an approximately 1 percentage point reduction in the probability of crime (25% of the mean) and decreases the monetary cost of crime by more than 10%. Equivalently, an increase of one standard deviation in building-specific experience is associated with a reduction of the probability of crime equal to its mean and 40% of the monetary cost of crime.

Despite the extensive set of controls employed in equation (1), which accounts for a broad spectrum of potential confounders, the results should be interpreted cautiously since the lack of experimental variation limits the definitive establishment of a causal relationship. For instance, if guards are rotated following a crime episode, our results could be affected by reverse causation to the extent that guards who experience more crime accumulate fewer shifts in the associated buildings because they are rotated out earlier. In order to reduce this concern, in Column (3) of Table 2 we exclude from the estimation the last quarter of the guard in the building. Intuitively, crimes that prompt rotation should occur more frequently in a guard's final months in a building since if rotation is crime-driven, guards tend to be rotated soon after incidents. Estimates from this column do not change significantly with respect to the previous columns, which suggests a limited role of this type of reverse causation in explaining our results.[22]

In Appendix Table B1 we show that our estimates are broadly similar under a number of additional robustness checks. In Column (1), we use an instrumental variable approach that leverages the haphazard assignment of guards into types and the differential rate at which Type-I and Type-II guards accumulate building-experience over time. The IV approach, which we discuss in detail in Section B.7, aims to reduce concerns regarding some time-varying confounding factors that the fixed effects may not be able to absorb. Given that

[22]This finding is also consistent with the fact that the occurrence of crime is not more likely in the days before rotation, as we show in Appendix Figure B2.

equation (1) relies on linearity assumptions of the independent variables, in Appendix Table B1 (Column (2)) we also show that effects are robust to controlling non-parametrically for the total experience of the guard. In Column (3), we exclude from estimations the first month of each guard in the firm in order to address the possibility that results are driven by a period when crime could be disproportionately high due to the lack of overall experience in the job. Finally, in Column (4), we estimate equation (1) dropping all the observations corresponding to the first building in which we observe the guard.

An event study of guards' rotation. To provide further empirical evidence on the importance of guards' building-specific experience, we conduct an event study examining the evolution of crime around the time a guard is rotated to a new building. Intuitively, while guards maintain their overall experience, their building-specific human capital likely declines sharply as they move to an unfamiliar working environment. Our event study contrasts changes in crime around a guard's rotation relative to those who are not rotated. As detailed in Section B.8, our approach follows the procedure below:

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

2. We then specify a control group for this guard in this rotation episode by including all other guards that were working in either building b or building b' (the control guards).

Next, we estimate the following equation at the guard-week level by stacking all the rotation episodes:

$$\begin{aligned} Crime_{ibt} = \beta(RotGuard_{it} \times PostRot_{it}) + \eta_i \times WinRot^j_{it} \\ + \rho(PostRot_{it} \times WinRot^j_{it}) + \eta TotalExp_{it} + \delta_{b(it)} + \epsilon_{it}, \quad (2) \end{aligned}$$

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. We control for two sets of interactions: First, the interaction between the guard fixed effect η_i and $WinRot_{it}^j$, 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 $WinRot_{it}^j$, which absorbs the average change in crime after the rotation episode experienced across all guards related to such episode. We also include building fixed effects $\delta_{b(it)}$, week fixed effects, indicators for neighborhood $\times$ month, and the guard's overall experience $TotalExp_{it}$. Standard errors are clustered at the guard and $WinRotation_{it}^j$ level.

Results from the estimation of (2) are reported in Panel A of Table 3. 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 a control group only the guards who worked in the same building as the focal guard *before* rotation. Instead, in Column (3), the control group only includes guards who worked in the building where the focal guard was rotated to *after* the rotation. Results obtained in all columns are very similar. Columns (4) to (6) repeat the estimations from Columns (1) to (3) but include only guards with at least 6 months of tenure in the building. The results are about 50% larger in magnitude, suggesting a lower effect of rotation for guards with little experience in their pre-rotation building (the average effect relative to the mean of the dependent variable across all columns is 28%). In Panel B, we conduct a similar estimation using the inverse-hyperbolic-sine transformed value of property lost in crime as the dependent variable. In Appendix Table B2, 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. Consistent with the idea that the reduction in building-specific skills drives the result, the increase in crime after rotation is significantly higher for guards with relatively high experience in the building.

Overall, the event study results align with the findings of Table 2, suggesting a negative impact on guard performance due to the loss of building-specific experience after rotation. However, some limitations should be acknowledged. For example, post-rotation guards may be assigned to higher-crime shifts or decrease their effort due to lower job satisfaction. Furthermore, rotations could disrupt collaboration between guards in the same building. While it is conceivable that rotations affect non-rotating guards, we believe that such spillovers are unlikely to fully explain our results because guard shifts typically do not overlap.[23] Reassuringly, our results are robust to the exclusion of buildings with more than two guards, where the spillovers may be more likely.

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 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 notion that rotation can be inefficient as it destroys skills that positively affect productivity. Therefore, a natural question is why service firms do it. Our theory suggests that rotation can benefit the firm if the accumulation of building-specific experience, absent rotation, increases the risk of guards being poached. In the remainder of this section, we provide empirical evidence consistent with this rationale by showing that buildings prefer to poach guards with greater building-specific experience.

Building-specific experience and observed poaching. Proposition 2 of our theoretical model predicts that a higher building-specific experience increases the probability of poaching. This prediction aligns closely with the narrative presented by our partner firm and is frequently echoed in our survey responses from other security companies. For instance, one firm stated: "*We realized that buildings were poaching guards that spend significant time with them. We did not worry about the newly allocated guards.*"[24]. To empirically substantiate this relationship within our data, we leverage information from the poaching episodes where guards were hired in-house by buildings that were contractually engaged with the firm at the time of poaching.[25]

[23] While we acknowledge that it would be difficult to completely isolate the potential negative effects of rotation on non-rotating guards (if those effects do exist), the productivity drop may even be underestimated, as it is calculated alongside any disruption suffered by other guards.

[24] "Nosotros nos dimos cuenta que los edificios se robaban solamente a los guardias que llevaban bastante tiempo con ellos. Dormiamos tranquilos con los guardias que apenas habiamos mandado."

[25] In 70% of these cases, buildings poached only one guard. We lack shift data for 4 poached guards.

A simple descriptive analysis (Table 4, Panel A) underscores a strong association between a guard's building-specific experience and the poaching likelihood. At the time of poaching, a typical poached guard has 60% more building-specific experience than the median guard and falls in the top 30% of the distribution. Compared to other guards working concurrently in the same building, the poached guard has 3 times more tenure in the building. Even comparing to guards employed in that building during the quarter prior to poaching, in 71% of the cases the poached guard is either the most tenured or second-most tenured. In Appendix Table B3 we report cross-sectional regressions at the guard and guard $\times$ building levels (Panels A and B, respectively). Specifically, we find that a higher rotation of a guard is strongly negatively associated with poaching. Additionally, within a given building, a guard's accumulated experience in that building significantly correlates with poaching.

Identifying the causal connection between building-specific experience and poaching is challenging because poaching truncates the experience distribution of the poached guards. Moreover, the policy change during our sample period (see section 5) stopped poaching, rendering guards' post-policy building experience irrelevant for explaining poaching. Therefore, only the building-specific experience accumulated up to the point of the policy change should be used in our analysis. Given these considerations, we employ a duration model, a method well suited for analyzing the temporal relationship between poaching incidents and guards' rotation patterns (or their accumulated experience in a specific building). The duration model effectively characterizes the time-to-event nature of the poaching data and incorporates truncation and censoring issues more naturally (Bazen, 2011). In Panel B of Table 4, we display the estimated hazard ratios of a Cox proportional hazard model, analyzed at the week level.[26] The baseline model only controls for the total experience of the guard, but this relationship is robust even after controlling for guard and building characteristics.[27] We find that the hazard ratios are substantially larger than one for the building-specific experience and significantly lower than one for the number of times a guard was rotated. The results suggest that each rotation of a guard is associated with a 70% decrease in the baseline hazard of poaching, while each additional month of building-specific experience increases the

[26]For a discussion of duration models with time-varying controls, see Van Den Berg (2001) and Fisher and Lin (1999).

[27]The model is censored at the moment the law was introduced, as poaching was no longer possible after that point. We also account for the heterogeneity at the building level by introducing building-level random effects. We test the proportional hazard assumption using Schoenfeld residuals, and in all cases, we cannot reject the null hypothesis of proportionality (all p-values are above 10% significance level).

baseline hazard by 30%.[28]

In sum, our qualitative evidence, robust cross-sectional analysis, and duration analysis collectively and consistently point to a notable increase in the probability of poaching as building-specific experience grows. Our analysis naturally carries some limitations, given the non-experimental variation in rotation patterns. Therefore, the results should be interpreted with appropriate caution. Yet, despite these limitations, the consistency and strength of the findings still provide substantial support for a proposition widely acknowledged by firms grappling with the vertical poaching problem: poaching primarily occurs after guards have accrued a substantial amount of working experience within the building.

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. The partner firm and other interviewed firms mentioned that 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%. Figure 3 plots the time series of the average rotation across guards and provides evidence of this pattern.[29]

[28] As the baseline hazard of poaching is relatively small, these changes do not lead to dramatic shifts in the absolute likelihood of poaching.

[29] Survey evidence shows no changes around the policy change in either the violence of crime or types of

A simple before-after comparison of rotation patterns can be misleading due to time-confounding factors. A main limitation we face is the absence of a natural exogenous control group. To overcome this challenge we compare 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, which make them more or less attractive to be poached by buildings. As implied by Proposition 3, the security firm should rotate more often those guards that are more attractive to buildings – but only before the policy change, when guards could 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.

We start by 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 — and to focus on those guards with similar types of schedules—. We estimate the relationship between observed poaching and the predetermined characteristics of the guard. The use of these characteristics is aligned with anecdotal evidence given by our partner firm. The company stated 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 a large household because, in case of absence of the guard, she can more easily find a trustable replacement for the working shift. Since the sample of poached guards is not particularly large (28 episodes) and given the potentially large number of characteristics (and interactions between them) that could predict poaching, we use a machine learning procedure (Random Forest) to construct an index of poaching risk for each guard. The details of this procedure are described in Appendix B.9.[30]

skills that workers need. Some firms reported an increase in the demand of services. A potential concern is that the poaching risk could have increased if the law was not strongly enforced since the higher demand would lead buildings to anticipate higher fees. In response, they may have tried to poach guards before the law was fully enforced. However, this is at odds with the fact that rotation dropped immediately after the policy change, particularly for high-risk guards. We also evaluated wage data for a subset of guards from the partner company. We find that most of the variation in real wages is explained by aggregated time trends and that individual characteristics do not significantly explain the wage differences. Importantly, we find that wages did not change differentially for guards with different poaching risks, neither before nor after the policy. However, we caveat these results because of the large measurement error of wage data.

[30]To facilitate the interpretation of this section, the index is standardized to a mean of zero and a standard deviation of one.

5.1 Rotation of Guards due to Poaching Risk

In this subsection, we present some descriptive evidence consistent with the fact that, before the policy change, the firm rotated more often those guards with higher 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.

Panel A of Figure 4 shows the cumulative share of guards rotated over time before the law was introduced. Rotation patterns diverge significantly between high-risk (above median) guards and low-risk (below median) guards. As expected, those guards more likely to be poached are rotated more intensively. Panel B of the figure shows the same comparison for the period after the law took effect. There is a drop in overall rotation, but especially for the high-risk group after the policy change.

We also regress the measures of rotation on the estimated risk of poaching, controlling for time-varying characteristics of the guard and 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. As predicted by Proposition 3, 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. Specifically, 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 few guards rotated more than once 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.[31]

[31] We acknowledge that since our proxy of risk may not perfectly capture the true risk or the firm's true

5.2 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 suggests that rotation may have dropped disproportionately for guards with ex-ante high poaching risk after the policy introduction. For example, before the policy change, the monthly average rotation probabilities were 4.6% for high-risk (above median) guards and 2.3% for low-risk guards (below median). After the policy change, the rotation probability of high-risk guards dropped to 1.4%, but for low-risk guards, it remained at 2% (see last rows of Table 5).

In Columns (3) and (4) of Table 5, we repeat the estimation from Columns (1) and (2) for post-policy data. Results indicate that the relationship between rotation and poaching risk, which was large and significant the year prior to the law, became small and insignificant in the year after the policy took effect (which can also be interpreted as a placebo test). This result is consistent with the patterns shown in Figure 4.[32] We interpret this sharp change as suggestive evidence that poaching risk stopped being a determinant of rotation after the policy change. This observation also contests the hypothesis that rotation was solely determined by the firm favoring guards with specific attributes (which could make rotation less costly), such as adaptability or client-orientation. Even if these traits were confounded with our measure of poaching risk, we would expect their link to rotation to persist after the policy change. The lack of post-policy association reinforces our argument that poaching concerns substantially influenced guard rotation, a dynamic that the 1994 Decree effectively mitigated.[33] Importantly, we do not suggest that rotation should or will be entirely eliminated, as various reasons unrelated to poaching concerns could still justify rotation and remain relevant throughout our sample period.

While the previous analysis is merely descriptive, the disproportionate magnitude of the change in rotation between guards of different risk and the fact that rotation rates do not drop to zero for both groups make a purely mechanical effect from overall rotation reduction

perceived risk, the analysis may have some measurement error.

[32]Figure 4 shows that low-risk guards were rotated more often than high-risk guards in the first months after the policy was introduced. Excluding these months makes the coefficients of Columns (3) and (4) of Table 5 move even closer to zero.

[33]The effect was also unlikely to be explained by an overall change in the demand of guards, given its differential impacts.

after the law unlikely. Rather, they suggest that the firm may have purposely and selectively reduced the rotation asymmetrically. We extend this intuition to a more robust empirical framework. To this end, we leverage a *diffences-in-differences* design, which enables us to account for potential confounders and common shocks, as well as to isolate the causal impact of the policy change. The specification we use at the guard-month level is given by:

$$Rotation_{it} = \beta RiskPoaching_i \times After_t + \phi X_{it} + \eta_i + \gamma_t + \delta_{b(it)} + \varepsilon_{it}, \tag{3}$$

where the dependent variable measures the rotation of guard i during month t. The policy effect (β) is identified from the interaction between the estimated risk of poaching and a dummy taking one for post-policy periods.[34] 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).[35] 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.[36]

Table 6 reports the estimates of equation (3) (Columns 1 and 2).[37] The results confirm that guards with a larger risk of poaching were rotated less often after the policy change. A one standard deviation increase in poaching risk is associated with a 2-percentage-point reduction in the rotation probability, a very large effect relative to the 2.5% monthly average.

[34]Notice that this type of specification is not novel and resembles those from Bleakley (2010), Duflo (2001) or Card and Krueger (1994) among others. Callaway et al. (2021) argue that the magnitude of coefficients in continuous treatment 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. Reassuringly, if we assume that guards with a very low risk (e.g. below the 25th percentile) resemble 'never-treated' units and those with very high risk (e.g. above 75th percentile) resemble 'fully-treated' units, a standard diff-in-diff specification with binary treatment gives us comparable and highly significant effects of the policy. We also find similar effects from a simple 2×2 diff-in-diff with pre- and post- policy periods and the two extreme risk groups. These results are summarized in Appendix Table B6.

[35]The recent criticism over staggered Diff-in-Diff setups (e.g. Callaway and Sant'Anna, 2020; Sun and Abraham, 2021) is not a concern in this setting, since the law was introduced at a single point in time.

[36]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 B4).

[37]We report standard errors multi-way clustered at both guard and month levels, as well as two-step bootstrapped standard errors.

As before, the estimated effects are similar in magnitude for rotation probability and number of buildings worked per month.

5.2.1 Robustness

In Columns (3) and (4) of Table 6 we allow for guard-specific linear trends ($\theta_i \times t$) to identify the policy effect separately from 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).

Additionally, we estimate the policy effect excluding the transition period immediately after the law was introduced (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 policy introduction. As expected, results are slightly smaller but they remain highly significant.

Figure 5 depicts the leads and lags version of equation (3) and displays the estimated coefficients of the variables $RiskPoaching_i \times Q_t^j$ where Q_t^j is an indicator for the j^{th} quarter relative to the policy introduction ($j = 0$). The figure shows no evidence of pre-trends in rotation, but a sharp decrease for high-risk guards. The first months after the policy change display larger negative coefficients. This aligns with the descriptive evidence shown in Figure 4, where low-risk guards were rotated more intensively for a short period after the law was introduced.

Panel A of Appendix Figure B4 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 tim-varying covariates or individual trends. They propose a procedure that separates the testing of pre-trends from the estimation of dynamic effects. To deal with this concern, in Panel B of Appendix Figure B4, 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.

Appendix Table B5 presents additional robustness checks using alternative proxies for the risk of poaching. This includes an alternative estimation of the Random Forest model

(Column 1) as well as five different observable characteristics that are most associated with a higher poaching risk (Columns 2-6). As expected, the rotation decreased more after the policy change, 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. In the next subsection, we 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.

5.3 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 constrain 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 the 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 (3) but the dependent variables are the number of crimes that occurred while the guard was on duty during the month and the (IHST) value of property lost due to crime. The estimates capture the relative decrease in crime among guards with higher versus lower poaching risk. While one natural interpretation of this pattern is that decreased rotation mediates this effect, we acknowledge that our results might also capture other potential impacts of the policy beyond changes in rotation.[38]

As reported in Table 7, the estimated effect of rotation on crime, albeit less robust than the results for rotation,[39] 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 per month. Similarly, a one standard deviation increase in

[38]For instance, the policy may have changed the motivation of guards heterogeneously, though firms in the sector find this very unlikely. Since we cannot empirically verify the exact channel, we interpret the related results with this caveat in mind.

[39]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.

poaching risk is followed by a reduction in the cost of property lost in the order of 15% to 25%.[40] Appendix Figure B5 reports the corresponding leads and lags estimates when crime is the dependent variable. The policy effect on crime appears to be stronger over time.[41]

Taken all together, the results of this section provide evidence consistent with: (i) a sharp decline 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 Generalizability

We have conducted a detailed analysis of our partner firm. However, one may ask about the broader relevance of our research question and the generalizability of the findings. This section is devoted to addressing these important concerns.

We begin by noting that, while poaching is recognized as an important issue in the service sector, to the best of our knowledge it has not been quantified extensively. A way to approach this lack of data is by examining indirect measures of the poaching problem, such as how commonly employers take actions to deter their employees from being hired away by clients. One such common action is requiring employees to sign non-solicitation agreements, contractual clauses that prohibit employees from contacting former clients about providing services. A survey by Balasubramanian et al. (2021) of a large employer sample found that 77% of the firms use non-solicitation agreements, suggesting that the issue of vertical poaching from clients is important and ubiquitous.

A comprehensive understanding of the mechanism of vertical poaching requires detailed data on the interactions between workers, firms, and clients, as well as exogenous shocks (e.g. policy changes) that enable causal analysis. To the best of our knowledge, no existing dataset offers the granularity of data required for such multi-firm, multi-industry investigation. Thus, following established examples (e.g. Staats and Gino, 2012; Bidwell and Keller, 2014), we focus on a single organization that provided us with granular data on a period where an

[40]Reassuringly, the estimated effect relative to the mean is significant and roughly similar (26% of the mean) when the dependent variable is in levels.

[41]Results for the binary high-risk ($>$ 75th pctile) versus low-risk ($<$ 25th pctile) specification and the simple 2×2 specification (two groups and two periods) are reported in Appendix Table B7.

exogenous shock occurred. Although this approach has inherent limits on generalizability, it provides the required detail for studying the specific mechanisms involved.

We believe that our results carry significant implications beyond the specific conditions of our partnered firm. To substantiate the proposed mechanism and empirical findings, we have grounded the study in a theoretical framework delineating when poaching is likely to occur and when rotation can be an optimal strategy to prevent it. Additionally, we have compiled substantive qualitative evidence from other firms in the security-service industry, coupled with some anecdotal evidence from other industries where poaching is likely to present a salient issue, as predicted by the theoretical framework. In what follows, we draw on these complementary inputs to advance the issue of generalizability on three fronts: First, we argue that our partner firm is representative of the industry. Second, we analyze when poaching is an organizational problem and how rotation interacts with other potential solutions. Third, we provide empirical evidence from a different industry.

6.1 Representativeness of Our Partner Firm

In this section, we argue that no single relevant attribute of our partner organization makes it unique. For that purpose, we have conducted a survey of 20+ security firms.[42] This qualitative evidence shows not only that our organization is representative of a large industry, but also that the mechanisms proposed and studied here are relevant to other organizations.[43]

The survey gives two main lessons. First, 19 out of 23 organizations (82%) reported that the issue of vertical poaching was important or very important before the policy change as it was both frequent and costly. Second, 13 out of 19 organizations (69%) used rotation as one of the mechanisms to avoid poaching.

Our partner firm lost about 8% of the workers before the policy change due to vertical poaching. Although most of the firms think that vertical poaching was a tangible problem, few of them reported a concrete number of poached guards. Among those reporting a number, the interviews indicate that about 30% of the workers that ever worked in the surveyed security companies were vertically poached at some point (an important caveat

[42]For more information of the survey, see Section B.1.

[43]Figure B6 shows that our partner firm falls in the range of sizes for surveyed firms, neither being particularly large or small compared to others in the industry.

with this large number is that it may reflect some selection issues due to the low response rate).

Our qualitative evidence shows that the firms in our survey are aware of the vertical poaching problem, and have the expertise to recognize the associated costs. For instance, one firm stated:

> "*We tried to do whatever we could to avoid guards leaving our company because it was not easy to replace them. You need to find the right people that you can trust, train them and explain the daily routine tasks of the job.*"[44]

Another firm said:

> "*Every time a guard left it was really hard to find another one good to replace her. There is no lack of candidates, but simply it was hard to find people with experience willing to take the shifts that we needed.*"[45]

Finally, one firm stated

> "*With vertical poaching you would lose twice and it was very expensive. The guard would leave and the client would not come back.*"[46]

Our qualitative evidence also shows that rotation was one of the main tools that firms use to avoid poaching. For instance, one firm said:

> "*Whenever we saw that the worker was feeling getting along too well with the client, we prefer to rotate her to avoid potential future problems.*"[47]

When we follow with the question about what type of problems they referred to, they say:

> "*Well, that the client steals (poach) her*"[48]

[44] Haciamos lo que podiamos para que los celadores no se fueran de la empresa porque no era facil reemplazarlos. Usted tiene que encontrar la gente adecuada, ensenarles cosas y explicarle la rutina diaria del trabajo.

[45] Cada vez que un celador se iba era un calvario conseguir uno bueno para reemplazarlo. Candidatos no faltan, pero gente con experiencia que esten dispuestos a hacer los turnos como necesitabamos no estan a la vuelta de la esquina.

[46] En el robo a los celadores se perdia doble y mucho. El guardia se iba y el cliente que no volvia.

[47] Cuando veiamos que el trabajador se estaba amanando mucho con el cliente, mejor lo rotabamos para evitar dolores de cabeza despues.

[48] Pues que el cliente se lo robara.

Another firm also stated:

> "*As in many other occupations, there are good and bad guards. Clients tried to steal (poach) the good ones. When we felt that the clients could poach the good guards, we quickly change them from one building to another one. Sometimes, the guard protested but one needs to exert authority over these things.*"[49]

6.2 Prevalence and Prevention of Poaching

6.2.1 When will poaching likely be a problem?

Poaching is a major problem for service firms when their workers can move to client organizations and when this move is substantially costly. In general, the impact tends to be greater for the firm when the poached employees are more difficult to replace (e.g., due to their specialized skills and experiences).[50] There are two types of forces that restrict workers from moving to other firms, including clients: demand-side and supply-side factors (Campbell et al., 2012). On the demand side, mobility is limited when the work cultivates a large level of firm-specific skills (as opposed to client-specific skills), when service firms and their clients are asymmetrically informed about the skills of the worker, when there is not enough volume of work to justify bringing the worker in-house (these last two imply that the worker has outside options besides the employing service firm), and when the client's poaching costs are low. On the supply-side, mobility is constrained by switching costs or guards underestimating client demand.

In our context, demand side considerations do not restrict poaching because the relevant skills are CSS (Table 2) and information asymmetries between the focal firm and guards are similar to those between buildings and guards.[51] Since buildings require a guard for a whole working shift, the insufficient-volume-of-work argument does not apply either. Finally, poaching costs were low for buildings in 1990s Colombia because mobility-restricting

[49] Como en todo, hay buenos y malos celadores. A los buenos los clientes se los intentaban robar. Cuando nos oliamos que eso podia pasar, los cambiabamos de una de edificio. A veces el gaurdia nos protestaba, pero uno debe poner autoridad en estas cosas.

[50] We expect to see vertical poaching occur when the worker is relatively replaceable for the service firm but less replaceable for the client organization, *ceteris paribus*.

[51] As a matter of fact, our qualitative evidence shows that clients sometimes acquire more information than the service provider firm.

contracts were rare, and even when present, our qualitative evidence shows that courts did not enforce them.

We believe that the prevalence of poaching in our context can be attributed to the low costs of mobility and poaching, and the ease with which guards learn about clients' demand. To expand on this point, we expect that the importance of the vertical poaching problem will diminish when the mobility cost of service providers or the poaching costs of clients are high, or when service providers have a standing uncertainty about clients' demand.

6.2.2 What do firms do to avoid poaching?

Having used the qualitative evidence to confirm that poaching is prevalent and very costly for service firms in our context, we now argue that these firms may use legal and/or managerial practices to prevent their workers from being hired away.

On the legal side, firms can sign non-poaching contracts with clients (Starr et al., 2021). On the managerial side, firms can increase engagement, career prospects, rotation, incentives, or status (Bidwell et al., 2015). There is considerable heterogeneity in firms' approaches to deterring poaching. The specific solution that a firm chooses depends on the combination of managerial capability and the quality of institutions and the legal environment.

When do firms use non-poaching contracts? Vertical anti-poaching agreements between firms and clients are most feasible when the legislative environment clearly permits such contracts and institutions can strongly enforce them. In reality, even though anecdotal evidence suggests that vertical poaching is an important and common issue across many industries in the world, agreements specifically prohibiting this type of poaching remain uncommon.

Irrespective of the legality of vertical anti-poaching agreements, we think that the most important reason why they are not used in our setting and other similar ones is that, in a large part of the world, like Colombia in the 1990s, institutions could not assure strong contract enforcement and it was economically costly to litigate. For instance, in Colombia in the early 90s, the legislation prohibited firms from poaching workers from competitor firms (Article 75, Decree 410 of 1971 -Commerce Legal Code-), but it did not address poaching from clients. Furthermore, even when firms attempted to sign non-poaching contracts with clients, our anecdotal evidence shows that courts did not enforce them.

When do firms use managerial practices? There are four main factors that explain why some firms adopt managerial practices to deter poaching while others do not: (i) *Awareness.* Some service firms may think that losing workers is a natural feature of the environment and there are no effective remedies. (ii) *Capabilities.* Some firms may lack the skills to identify poaching as a problem needing to be addressed. (iii) *Incentives.* Though aware of poaching issues, some firms do not consider them costly enough to warrant intervention. (iv) *Organizational Frictions.* Other reasons, such as a lack of trust between the firm and the service provider for the implementation of relational contracts.

6.2.3 Anecdotal evidence from other industries.

To demonstrate the broader relevance of our analysis beyond the security service context, we interviewed managers in three additional industries. Their perspectives provided further qualitative insights that deepen our understanding of the prevalence of vertical poaching across sectors and the use of rotation as a preferred deterrent strategy by service firms.

Lawyers. Our interviews reveal that the issue of vertical poaching is considered important and salient among law firms. The evidence shows that these firms have used rotation as a strategy to avoid poaching only when they believe the client will not leave the firm after the rotation episode – an outcome that the interviewees indicate is actually uncommon.[52] However, the interviewees also noted that rotation is not the primary tool to avoid poaching. Instead, besides signing mobility-restricting contracts these firms also incentivize the best lawyers, for instance by giving them some shares of the company (making them partners). To sum up, providers of law services believe the issue of vertical poaching is important and, given the strength of institutions and management, they may use rotation accompanied by other tools to deter poaching.[53]

[52] It may occur only in cases in which the client approaches the law firm only to work with a specific lawyer. For a similar argument in the marketing industry, see Broschak (2004).

[53] Our model extension in Online Appendix B.6 incorporates heterogeneity in worker preferences and provides a case in which firms optimally use both rotation and other (legal and managerial) tools to deter poaching in equilibrium.

Software development. The evidence we gathered for this industry shows that managers are aware of the vertical poaching problem, and they think it is something that happens to their best workers from time to time. The interviewees tend to categorize clients into two types: large and small. The former type hires them for a set of tasks over a definite period. The latter contracts them for specific tasks that do not last for too long. Managers of software development firms stated that poaching tends to happen only with large clients, not small ones (possibly because there is enough volume of work that justifies bringing workers in-house for the former, but not for the latter). Our evidence shows that firms in this industry do use rotation to deter poaching from large clients, but importantly they also complement it with other tools such as monetary incentives.

Cleaning services (to companies). Our evidence shows that managers of these firms see vertical poaching as a frequent and relevant issue. However, they do not use rotation as a strategy to avoid poaching given this market's intense competition. Managers aim to please clients as much as they can, as losing one is very costly. Consequently, they prefer using other tools such as amenities over rotation, which could cause clients dissatisfaction.

Overall, the evidence from the above-mentioned three industries aligns with the main results previously reported – vertical poaching is a common and important issue and rotation is sometimes used as an anti-poaching tactic, though its implementation depends on specific market conditions and legal environments.

6.3 Poaching in the Lobbying Industry

Lastly, we study a different empirical setting to show that the issue of vertical poaching extends beyond our initial context into other high-skill sectors in developed countries. We would like to have consistent evidence of vertical relationships over time for a larger set of high-skill workers. However, to the best of our knowledge there is no comprehensive dataset on these relationships. We find a notable exception in the US federal advocacy data. The data, which is based on Blanes i Vidal et al. (2012), records employment histories of lobbyists, including their roles (in-house versus for-hire advocate), employers, and tenure. This enables us to proxy the extent of client poaching in the US advocacy industry. During the period observed, around 20% of the lobbyists initially working as external were eventually poached by a client. This fraction is twice as large as in our original setting.

Appendix Table B8 shows the relation between a lobbyist's past experience with a client and the likelihood of being hired in-house by that same client. The results show that previous client-specific experience is a statistically significant predictor of being poached. In particular, the table implies that the odds of being poached by a client are 66 times larger for a lobbyist that previously worked for that client, than a lobbyist with no prior experience with the client.

7 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 of a guard being 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 must be cautious in jumping to the conclusion that the non-poaching policy unambiguously increases welfare for at least two reasons: First, workers may derive intrinsic utilities from being direct employees of the client, and in-house relationships could lead to a higher total surplus in the long run. Second, lacking data on long-term outcomes, it is challenging to fully assess the overall implications. Hence, policymakers contemplating a non-poaching policy change should consider a more comprehensive, long-term cost-benefit analysis.

We complement the previous results in three different ways: first, by discussing anecdotal evidence from multiple industries and countries; second, by presenting a theoretical model which generalizes beyond the empirical setting that we study; and third, by providing qualitative evidence (surveys and interviews) from security and other service industries.

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 a future stream of transactions with their original employers (Somaya et al., 2008). Our setting is not appropriate to analyze that type of empirical settings, primarily because in our case the client obtains the necessary service either fully in-house or fully outsourced. We expect that the benefits of poaching are more significant in settings with other characteristics, for instance, those in which the client would require a fraction of the labor force in-house and acquire the remaining labor input through outsourcing. Exploring these other settings is outside the scope of this paper, but future work in this direction is warranted.

We end the paper by cautioning again that our empirical analysis, though comprehensive, carries limitations. First, we do not observe all dimensions of guard performance, such as the time to register guests or the ability to recognize frequent visitors. Second, the studied decision-maker is a single firm, which may limit the generalizability of our findings. Efforts to expand our scope through industry-wide surveys and cross-sector interviews have been made, yet the lack of microdata from multiple firms and sectors remains a constraint.

Third, even if our findings reveal a strong and robust link between building-specific experience and productivity, the relationship is bounded by the non-exogenous nature of rotations. We control for many potential confounders through a rich set of controls and we use alternative variations (e.g., studying crime around guards' rotation), but results should still be interpreted cautiously absent experimental rotation data. We reinforce the evidence that the relation is causal using an IV approach, although it does rely on the (untestable) exclusion restriction assumption (it is worth noting, however, that indirect evidence supports the required assumption; see Appendix Section B.7). Fourth, we have shown a strong association between a guard's tenure in a building and the likelihood of poaching. This relationship, though significant and very robust, presents analytical challenges, as detailed in Subsection 4.2. The interpretation of the results faces a similar challenge as above, given that our analysis does not leverage randomized variation.

Finally, the analysis of the non-poaching policy's effects is limited by the absence of a natural control group. To deal with this limitation, we constructed a poaching risk index for each guard using machine learning techniques and leverage variation in this measure to evaluate the effects of the policy. We posited that the policy primarily influenced rotation through the reduction in poaching risk, and substantiated this with indirect evidence (such as the lack of pre-trends), plus anecdotal and survey evidence from firms in the sector. The results should be interpreted with the caution typically exercised with Diff-in-Diff approaches, due to the counterfactual nature of the underlying identification assumption.

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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

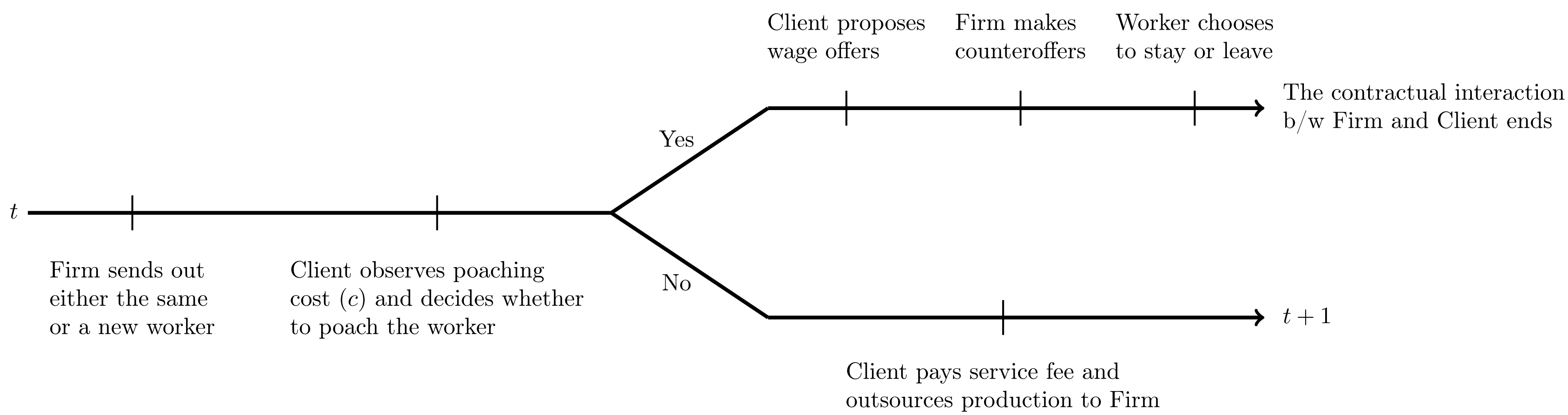
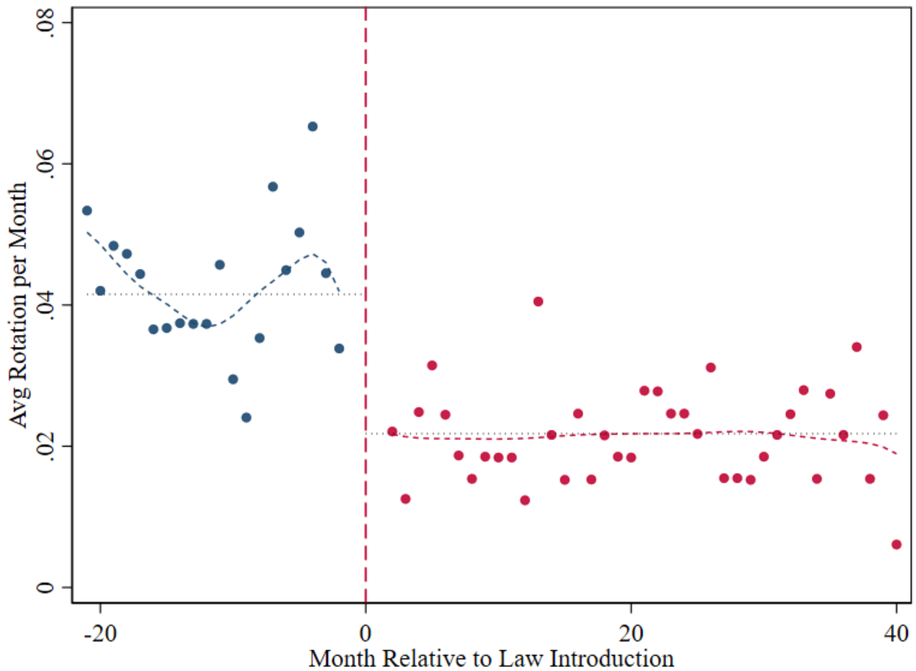
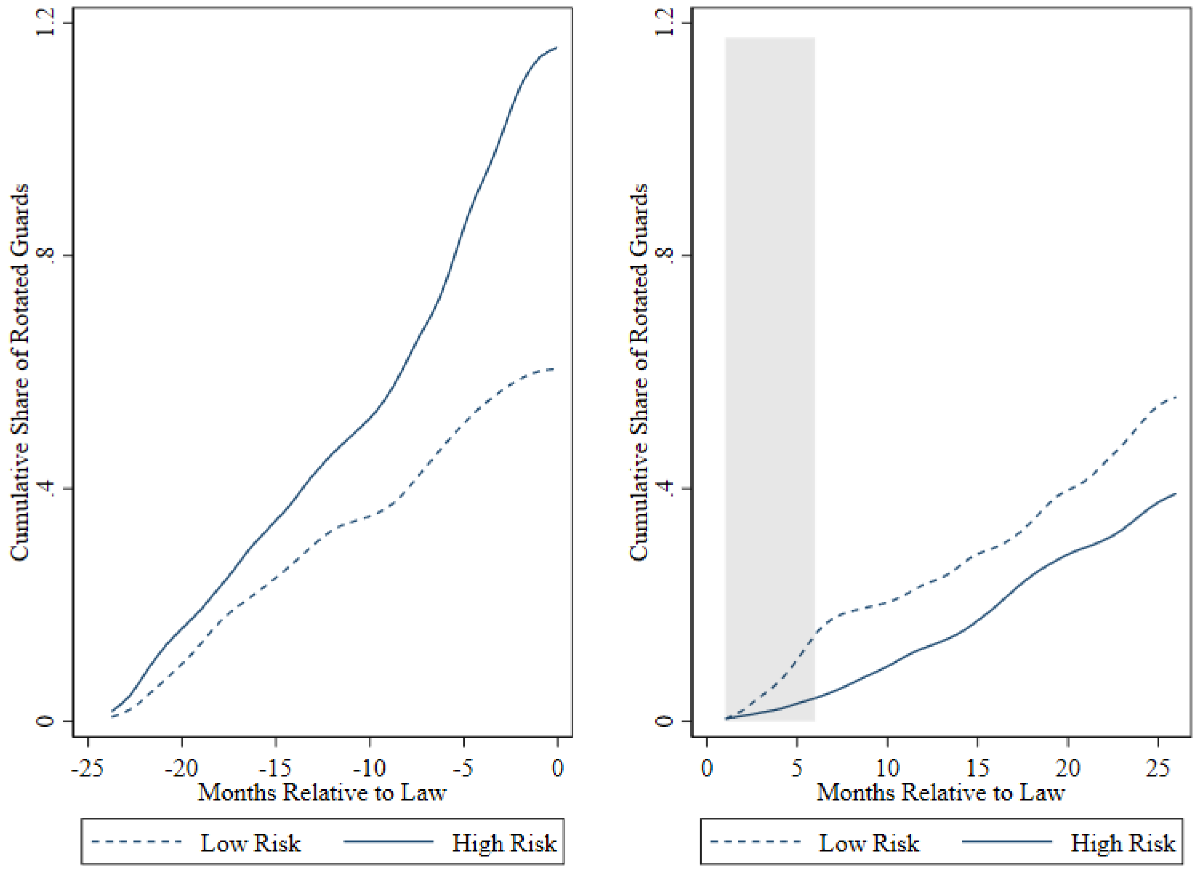


Figure 3: Evolution of Total Average Rotation



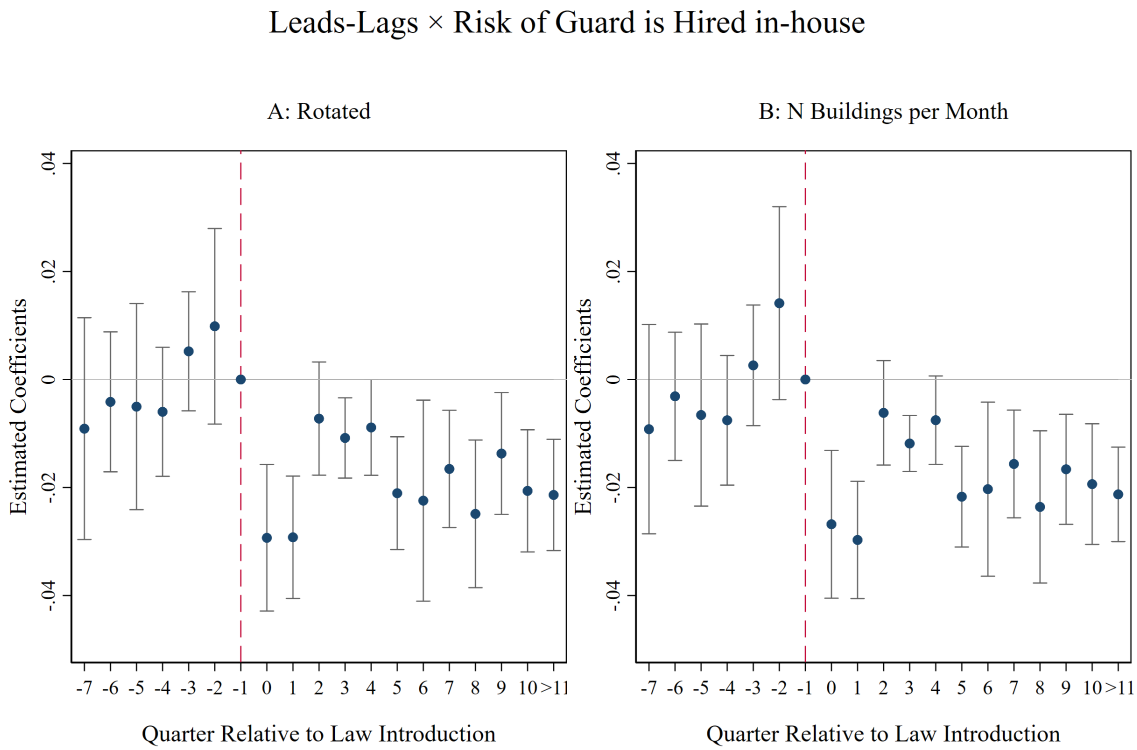
This figure displays the average rotation across all type-I guards in a given month. Each dot corresponds to the average rotation across all guards working during the corresponding month. The dashed curves display a local polynomial estimation of the evolution of average rotation over time for the periods 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.
Cumulative Rates



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 risk above the median) and low-risk guards (risk 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 the estimated 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 previous to 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, a fixed effect for the building where the guard worked most days in the month and an indicator for the first month the guard worked in the building. Observations are at the guard-month level. Standard errors are multi-way clustered at the guard-month level. $N = 15,373$.

Table 1: Characteristics of Guards and Buildings

	(1) Mean	(2) Sd	(3) Min	(4) Max
Guard Characteristics				
Number 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 crimes per month in the building	1.51	3.46	0	35
Value of property lost (usd)	46.73	119.24	0	1,421
Value of property lost (usd) if crime occur	145.12	172.85	0	1,421

Table 2: Productivity and Client-Specific Experience

	(1)	(2)	(3)	(4)
	Panel A: Crime occurred During Guard's Shift			
Experience in Building (months)	-.0025***	-.0027***	-.0031***	-.0028***
	(.00032)	(.00032)	(.00034)	(.00034)
Total Experience (months)	.00065	.00082	.0009*	.00075
	(.00054)	(.0005)	(.00053)	(.00052)
N	122,427	122,427	109,094	109,094
R2	.09	.1	.11	.16
Mean Depvar	.042	.042	.043	.043
	Panel B: IHST Value of Property Lost in Crime			
Experience in Building (months)	-.03***	-.032***	-.037***	-.034***
	(.0038)	(.0037)	(.004)	(.0041)
Total Experience (months)	.0078	.0098*	.011*	.0098
	(.0063)	(.0059)	(.0061)	(.0062)
N	122,427	122,427	109,094	109,094
R2	.088	.1	.1	.16
Mean Depvar	.5	.5	.52	.52
Guard X Building FE:	YES	YES	YES	YES
Week FE:	YES	YES	YES	YES
Days Worked Week:	YES	YES	YES	YES
Shift and Weekend controls:	NO	YES	YES	YES
Excl Last Guard-Build Quarter:	NO	NO	YES	YES
Neighb X Month FE:	NO	NO	NO	YES

N guards = 567; N buildings = 116. All regressions are at guard x week x building level. The independent variable is the acumulated experience of the guard in the building (measured in in months). In Panel A, the dependent variable is an indicator for a crime occurring during a shift when the guard was working in the building during the week. 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 total experience of the guard and number of shifts that the guard worked during the week. Columns (2) to (4) include additional controls for the share of days that the guard worked on night shifts during the week and an indicator for whether the guard worked at least one weekend shift during the week. Columns (3) and (4) exclude the last quarter the guard worked in the building. Column (4) controls for the interaction between the area of the building and the month. Robust standard errors clustered at the guard level are reported in parentheses.

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 X Rotating Guard	.013***	.013***	.013***	.023***	.019***	.018***
	(.0045)	(.0047)	(.0047)	(.0066)	(.0067)	(.0066)
Panel B:	**IHST Value of Property Lost in Crime**					
Post Rotation X Rotating Guard	.15***	.16***	.16***	.27***	.23***	.22***
	(.054)	(.056)	(.056)	(.079)	(.08)	(.08)
N	44,829	26,003	27,157	33,341	19,525	20,461
Only Guards with >6 Months of Experience in the Building	NO	NO	NO	YES	YES	YES

N guards (col 1) = 446; N buildings (col 1) = 104; Mean Depvar A (col 1) = .067; Mean Depvar B (col 1) = .803. N guards (col 4) = 426; N buildings (col 4) = 103; Mean Depvar A (col 4) = .057; Mean Depvar B (col 4) = .687. 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-week 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 (treated 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 at any shift when the guard was working during the week. 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. treated) 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 $\times$ month fixed effects and the number of days worked during the week. Additional controls are the total experience of the guard, the share of night shifts worked during the week and an indicator for the guard working the weekend. 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

	(1)	(2)	(3)	(4)
Panel A:	**Client-Specific Experience at Poaching (months)**			
		Non-Poached Guards		
	Poached Guard	**All**	**Type-I**	**Same Building**
Mean	13.27	8.79	9.38	7.4
Median	13.18	8.07	8.3	4.56
75th pctile	16.89	14.03	14.92	15.44
Panel B:	**Duration Model (Hazard Ratios)**			
Experience in Building (months)	1.3***		1.5***	
	(.16)		(.2)	
N Past Rotations		.23***		.12***
		(.13)		(.087)
p-val prop hazard	.86	.83	.10	.13
Building RE	YES	YES	YES	YES
Total Experience	YES	YES	YES	YES
Guard Chars	NO	NO	YES	YES
Build Chars	NO	NO	YES	YES

N guards = 454; N buildings = 116. This table investigates the relation between client-specific experience and poaching. The sample is for the period before the introduction of the Law. **Panel A** reports the mean, median and 75th percentile of the client specific experience (in month) at the week when poaching takes place. Column (1) refers to the poached guard. Column (2) includes all the non-poached guards during weeks when a poaching episode takes place. Column (3) accounts for all non-poached guards that are type-I during the week when poaching takes place. Column (4) refers to all non-poached guards working in the same building of the poached guard during the week when poaching takes place. **Panel B** reports the hazard ratios estimated from a Cox proportional hazards model for the time (specified in weeks) the guard spends in the firm before being poached. The model is right censored for the date when the law is introduced and includes 454 guards observed for a maximum of 109 weeks (39718 total observations). Type-II guards are assigned to the building with highest experience in the week. The model accounts for unobserved heterogeneity across buildings by incorporating a building-specific random effect. In Columns (1) and (3), the main independent variable is the building-specific experience of the guard (measured in months). In Columns (2) and (4), the main independent variable is the cumulative number of rotations prior to the observation's week. All duration models control for the total experience of the guard (measured in months). Columns (3) and (4) also control for guard characteristics (gender, previous experience, household structure, migration status and type) and building characteristics (size, tenure and socioeconomic strata of the area). The table reports the p-value of a global Chi2 test of the proportional hazard assumption of the Cox model based on Schoenfeld residuals.

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
Average Rotation by Risk:				
Low (below median):	.023		.02	
High (above median):	.046		.014	

N guards = 312; N buildings = 108. This table investigates the correlation between the estimated risk of being hired by a building and the rotation of guards. The poaching risk index is standardized to a mean of zero and a SD of one. 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. The poaching risk index is standardized to a mean of zero and a sd of one. 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. The last two rows of the table display the (raw) average rotation of guards in year before/after the policy change groups by low risk of poaching (guards below the median of the risk distribution) and high risk of poaching (guards above the median of the risk distribution).

Table 6: Effect of the Policy on Guards's Rotation

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**Rotated**	**N Builds Worked**	**Rotated**	**N Builds Worked**	**Rotated**	**N Builds Worked**
Post $\times$ 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 X Transition :	NO	NO	NO	NO	YES	YES

N guards = 347; N 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 poaching risk of the guard. The poaching risk index is standardized to a mean of zero and a SD of one. 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, the log-experience of the guard and an indicator for the first month of the guard in the firm. 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. The sample period corresponds to 7 quarters before and 16 quarters after the law introduction. 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 risk and the main regression).

Table 7: Effect of the Policy on Crime

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**N of Crimes**	**IHST Value Prop Lost**	**N of Crimes**	**IHST Value Prop Lost**	**N of Crimes**	**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 X Transition :	NO	NO	NO	NO	YES	YES

N guards = 347; N 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 poaching risk of the guard. The poaching risk index is standardized to a mean of zero and a sd of one. 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 and an indicator for the first month of the guard in the firm. 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. The sample period corresponds to 7 quarters before and 16 quarters after the law introduction. 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 risk and the main regression).

B Online Appendix

B.1 Protocol of the Interview

INFORMED CONSENT TEMPLATE

Statement of the research being undertaken The following survey aims to understand the way in which Colombian service security companies managed human resources in the 1990s. For that reason, it is important to talk to someone who was employed in one of these companies in that period. We are interested to understand better how the industry worked and your participation is key for us to understand it. The main goal is to understand how Latin American service security companies used human resources (guards) and how this differs from developed countries. As an initial outcome of this research, the research team has produced a research document that can be shared with you if you want to.

Procedures and duration This survey consists of a series of questions and it is expected to last between 15-20 minutes.

Expected benefits and foreseeable risks The main benefit from this research is a deeper understanding of the inner workings of the industry. There are no associated risks to this study.

Voluntary Participation The participation of this study is voluntary, the participant can stop at any time, you do not have to answer every single question if you do not want to and withdrawal does not imply any type of penalty of loss of benefit.

Compensation There is no associated monetary compensation for participating in this study.

Deception There is no deception in this study.

I confirm that I received the information that precedes, and I declare having read and understood its content. I confirm that I am 18 years of age or older, and volunteer to take part in this research. (Consent for minors or incapacitated individuals should be obtained from their legal tutors). Taking note that my Data are processed in full compliance with the Law, I freely consent to my Data to be used in the manner and uses described. I also declare having understood my rights and limitations, as well as how to exercise them. I finally confirm my willingness for this survey to be audio-recorded.

Participant Name:
Signature:
Date:

ADDITIONAL INFORMATION: DATA PRIVACY AND MANAGEMENT

We are required to provide participants with certain information to communicate our compliance with General Data Protection Regulation n. 679/2016. —, hereby declares that it falls within the field of application General Data Protection Regulation n. 679/2016 dealing with the protection of personal data with reference to the use of the data subject's personal data that is being collected as part of this research project. Researchers' and Ethical Review Board Contact Information This research is being undertaken by —–.[1] If participants have any questions about how the research was undertaken, who will have access to and control of the data, and in case participants want to provide feedback, ask questions, or inquire about the results of the study, they should contact the Data Protection Officer of –.[2]

Confidentiality and Security Measures The surveys will be conducted by phone and audio recordings will be collected and stored in a protected folder at the computer of — office. The information will be encrypted, and the office will be locked.[3]

In the event of publication or presentation, no identifying information will be disclosed.

The data will be anonymized. There are not variables in the survey answers that allow the researcher or anyone else to bring back the respondent individually. The survey does not ask for names of people or companies.

Data Sharing The only person being able to access this information will be –.[4]

The surveyed companies are in Colombia and the data will only be transferred to –.[5]

Data about you collected for the purposes of this project and similar future projects may be transferred to and stored at a destination outside the —, for example where it is processed by an organisation operating outside the — who works for us or for one of our suppliers, or where personal data is processed by one of our suppliers who is based outside the — or who uses storage facilities outside the —. This process will be subject to appropriate safeguards to protect the security and confidentiality of your Data.[6]

[1] This information is removed to keep the anonymity of the authors' names and affiliations.
[2] This information is removed to keep the anonymity of the authors' names and affiliations.
[3] This information is removed to keep the anonymity of the authors' names and affiliations.
[4] This information is removed to keep the anonymity of the authors' names and affiliations.
[5] This information is removed to keep the anonymity of the authors' names and affiliations.
[6] This information is removed to keep the anonymity of the authors' names and affiliations.

Data Subject's Rights Data subjects shall have the rights described in the articles 15, 16, 17 and 18 of General Data Protection Regulation n. 679/2016.

You have the right to correct or erase any personal data or restrict our data processing activities. Please note that when data are processed for research purposes the above rights are not absolute, and we may be entitled to refuse requests where exceptions apply.

If you have given your consent and you wish to withdraw it, please contact the responsible of the relevant department using the contact details set out below. Please note that where our processing of your personal data relies on your consent and where you then withdraw that consent, its withdrawal shall not cause any effect in the lawfulness of the previously processed Data.

Copyright Statement Within the context of the research project, you consent that — and — edits, copies, archives, disseminates and publishes your contribution to the project. Moreover, in accepting to participate in the project you expressly waive potential copyrights that could emerge from the result of the project, granting —- and the researchers involved a non-exclusive, free, irrevocable and worldwide license to use your contribution for the purposes indicated above.[7]

If you wish to be aware of the results of the projects, the researcher will make all reasonable steps to inform you, when privacy or other legal concerns do not impede to do so.

If you have given your consent and you wish to withdraw it, please contact the researcher using the contact details set out above. Please note that where our processing of your personal data relies on your consent and where you then withdraw that consent, its withdrawal shall not cause any effect in the lawfulness of the previously processed Data.

B.1.1 Survey Questions

1. Our research shows that at the beginning of the 90s some buildings acquired security services through security agencies and that sometimes they poached some of the guards that the agencies sent to the buildings. How common was that buildings contracted directly guards? For instance, out of 100 guards, how many were contracted directly by buildings?

2. How costly or difficult was for your company that buildings were hiring directly guards and ended the contractual relationship with you?

    - Was it costly to replace guards? Why yes or why not?

[7] This information is removed to keep the anonymity of the authors' names and affiliations.

3. What were the main strategies that your firm or other firms in the sector took to avoid the poaching problem (mark as many as they are correct and explain why you used them or why you did not use them)

   - Rotate the guard.
   - Sign a contract with guards or clients.
   - Increase the wage or other types of working incentives (such as flexibility in the shifts, amenities for the relatives of the guards, etc) when there was a real threat that the guard leaves the firm
   - Try to hire guards that will never leave the firm

   Can you explain why you or you did not use each of the previously mentioned strategies? Can you explain what was the main effect of each of these strategies in the long run, in particular in terms of reputation towards clients and guards?

4. What were the main reasons to rotate a guard from one building to another?

   - When did you decide to rotate guards? How frequently you did it?
   - When were they starting their shifts with a client or when they were at their best moment in a given building?
   - Who did you decide to rotate to?
   - How long did it take for a guard to know well her new job assignment?

5. Were you able to recognize that a guard was better than others?

   - How did you know this?
   - When did you know this?

6. What were the main characteristics of guards leaving to work directly for clients?

   - Before leaving your company, how did you know they could leave and work for a client?
   - How did you know when they could leave?

7. How did you design your rotation scheme of guards across buildings?

   - How did you determine the first building that the guard should be sent to?

8. Decree 356 of 1994 prohibited clients/guards to hire directly guards.

- How did this law affect you?
- Did you think that guards will stay in your firm rather than going to clients with a larger probability after the policy change?
- Do you think this policy change had any effect in the nature of the job, the required equipment or in general the resources needed?
- Do you think this policy had any effect in the incidence of crime?
- Do you think this policy had any effect in the type of skills that clients value?

B.2 Allocation of Guards to Buildings and Types

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 B9. 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 B7 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 B8. 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.

B.3 Back-of-Envelope Poaching Cost Calculation

We start by assuming that the cost imposed on the firm due to client poaching can be decomposed to three different sources: (i) Foregone profits due to the lost client (the poacher); (ii) Productivity costs due to the lost client-specific experience of the guard; (iii) Hiring costs due to the need of replacing the poached guard (including searching, ad-posting and training costs).[8]

Foregone profits. As the fee charged by the firm is about five times the minimum wage and the guard typically earns the minimum wage, the monthly foregone profits per lost client are about four times the minimum wage. A client that leaves is eventually replaced with a new one so this cost is not permanent. Thus we define the Foregone Profits as $C_\pi = 4MinW\mu$ where μ is the expected duration time before a new client is found. We calculate the monthly probability of acquiring a new client based on the average number of new clients gained per month by our partner firm during the sample period (before the policy change). We define μ as the inverse of this probability. Our results indicate that the average time to find a new client is 4.1 months. The expected total foregone profits during the client replacement period are \$3,546 (all figures are in 2020 US dollars henceforth).

Lost productivity. As a poached guard has to be eventually replaced by a new hire there is a loss in client-specific experience which can be translated into lower productivity.[9] The main limitation is that our measure of productivity is constrained to the monetary cost of crime which underestimates the total productivity lost (as we explain in the paper, we do not observe all dimensions of productivity such as client's trust). We also make a simplifying assumption that the firm faces the full cost of productivity loss. We define the lost productivity cost of a single guard as $C_p = \sum_{t=0}^{t=T}(Y(\tau+t) - Y(t))$, where $Y(k)$ is the productivity of the guard after k periods working with the client and τ is the accumulated client-specific experienced when the guard is poached. The intuition behind this calculation is as follows: a poached guard typically has high productivity within the building due to the accumulated experience there. Even if we replace the leaving building with an identical one, a new guard will start with lower productivity. Then, in the first period after poaching, there is an initial reduction in client-specific experience equal to $Y(\tau) - Y(0)$. As time passes, the differences between the productivity of the poached guard (in the counterfactual scenario

[8]To facilitate the interpretation, we work with all the nominal monetary values expressed in 2020 constant USD and abstract from discount rates when adding up values occurring at different months.

[9]We ignore the loss of general skills suffered by the poached guard as this is included in the figures we use to calculate hiring costs below.

where the guard remains in the building) and the replacement guard can become smaller and eventually disappear after T periods.

We start by calculating the initial percentage gap in productivity as $\hat{\beta} \times \hat{\tau}$, where $\hat{\tau}$ is the median client-experience at which poaching takes place (this corresponds to the lost client experience suffered by the firm) and $\hat{\beta}$ is the regression coefficient estimates from Table 2 (Panel B). We multiply this predicted % change in productivity by the average value of property lost that a guard would experience over a month which gives us a value of approximately \$10. This value is interpreted as the monetary cost (due to higher crime) experience in the first month in which the client-specific experience is lost. In order to account for other costs related to the crime that are not captured by the monetary value of the property we use information from Matthew et al. (2018) who quantify the monetary and non-monetary costs of domestic burglary for UK. We calculate the ratio of "other costs" to the value of the property stolen.[10] We use this ratio to augment our estimated costs of lost property value and this figure corresponds to the full cost experienced by the client due to crime. The monthly monetary cost in terms of crime is estimated to be $Y(\tau) - Y(0) = \$13$ and broadly 60% of this cost is the value of lost property and 40% are other costs.

Importantly, this initial cost is not permanent in the sense that we expect that over some horizon differences in client-specific experience are less relevant (i.e. we expect that the productivity gap disappears after some time). In order to estimate the cost for subsequent periods we need to make further simplifying assumptions. The descriptive non-parametric estimation from Appendix Figure B1 suggests that returns to experience in the building become close to flat around the 3 years of experience. Naturally, this is only a descriptive pattern but we use this threshold as a lower bound and we assume that the productivity gap is fully closed in year 3 (for instance, we assume that keeping constant other characteristics, a guard with 3.5 years of experience in a building has the same productivity than a guard with 3.2 years of experience there). In terms of the parameters defined above, we assume $T = 36$. For simplicity, we further assume that the initial productivity loss is reduced linearly over time and disappears after three years. Equivalently, we assume $Y(\tau + t) - Y(t) = (Y(\tau) - Y(0))(1 - t/T)$.[11] The final estimated cost over the three years for which we expect productivity to differ between a poached and a new guard is $C_p = \$330$. This estimation corresponds to a single guard but the termination of the relation with the client implies

[10] These other costs mainly include physical and emotional harm, lost output, and health costs.

[11] We do not have strong evidence that a linear depreciation rate is realistic. The non-parametric curves are close to linear for several months which would support the claim that productivity loss is reduced very little at the beginning and most catch-up occurs by the end of the three years period. However, we believe this would rely too much on the estimated functional form which is noisy in nature. In this context, linear depreciation may be a simpler and more conservative assumption.

that the building-specific experience of other guards allocated to the leaving building also disappears. If we assume that 1.3 additional guards are affected by the client leaving, which is the case for small buildings, the total value of productivity lost is \$759.[12]

Hiring costs. We do not have information about the actual hiring costs of the firm before the policy change. Hiring costs are mainly the costs associated with creating a vacancy, searching, screening, and training the worker. These costs are typically difficult to observe and measure so we have to rely on findings from existing studies that have focused on quantifying their magnitude. We based our calculations on Manning (2011) who reports hiring costs from several studies (see Table 2 in that study). In the context of search-matching models, these hiring costs are expressed as a % of the total wage bill paid over the duration of the employment relation. Figures are heterogeneous and range between 1.5% to 11%.[13] The median of these values is about 4% so we use this number as a broad approximation. Next, we need to calculate the total wage bill paid over the whole duration of the job relation. We estimate a hiring cost $C_h = 0.04 \times MinW \times AvgDuration = 0.04 \times AvgTotalWageBill = \$367$. Alternatively, we estimate a duration model to account for the truncation of the observed duration of the employment relation and calculate the predicted median duration for each guard.[14] Using the predicted median duration instead of the observed one results in a hiring cost of \$615.

Our final calculation indicates that a poaching episode costs around $C_\pi + 2.3C_p + C_h = \$4,920$ to the firm. This number is large, approximately 20 times the minimum wage.

Cost of rotating a guard. We have also performed a back-of-envelope calculation of the cost of rotating a guard. This cost is the (monetary) excess of crime resulting from the loss of a guard's client-specific experience upon rotation. We use the same approach employed for estimating lost productivity due to poaching. The coefficient estimates, $\hat{\beta}$, from Table 2 is the monetary crime cost resulting from diminished client-specific experience. The reduction in client-specific experience is the median value observed across all rotation events. We then scale this figure up by the non-monetary cost ratio from Matthew et al. (2018) to estimate the monthly decrease in the total cost of crime. To aggregate the cost of crime over time, we also adopt the assumption used in our poaching cost calculation: we disregard differences

[12] Smaller buildings typically have two type-I guards and a type-II guard who also work in other buildings.

[13] According to Manning (2011) this large heterogeneity is not surprising given the difficulty in defining and measuring hiring costs and given the different countries and time periods of the reported studies

[14] Specifically, we use a Weibull duration model using predetermined characteristics of the guard as covariates. Then we replace $AvgDuration$ in the calculation by the median (across all guards) of the predicted duration of the estimated model.

in client-specific experience between a hypothetical non-rotated guard and the replacement guard after three years, with the assumption that these differences diminish linearly over time. This back-of-envelope calculation gives us a cost of rotating a guard (at the median observed rotation time) of \$199 in 2020 USD. This amount is significantly lower than the cost of losing the guard due to poaching.

B.4 Proofs and Additional Theoretical Results

Preliminary Analysis. To begin, consider the client's decision of whether or not to poach a worker who has performed the production activity for e periods and who is expected to be called back by the firm in the next period. Since the highest wage at which that the firm would prefer retaining the worker to letting her go is κ, for the client's poaching effort not to be futile his offer w must satisfy

$$\sum_{s=0}^{+\infty} \delta^s (w + \gamma + (1-\beta) z(e+s)) \geq \sum_{s=0}^{+\infty} \delta^s \kappa, \tag{B.1}$$

or, equivalently, $w \geq \hat{w}(e)$, where

$$\hat{w}(e) \equiv \kappa - \gamma - (1-\beta)(1-\delta) \sum_{s=0}^{+\infty} \delta^s z(e+s). \tag{B.2}$$

Intuitively, the LHS of (B.1) is the total payoff that the worker will get by accepting the client's offer. In contrast, the RHS of (B.1) is the worker's payoff from staying at the service firm and getting the wage κ thereafter. Clearly, the LHS exceeds the RHS if and only if the client's offer w is sufficiently high. It is straightforward to check that the cutoff wage $\hat{w}(e)$ decreases in e, so the client finds poaching easier when the worker has been with him longer.

Now, suppose that the client is facing a worker who has served him for e periods and a poaching cost c. Then, for the client to benefit from hiring that worker internally rather than transacting with the firm, the following condition must hold:

$$\sum_{s=0}^{+\infty} \delta^s \left(\underline{v} - p\right) < -c + \sum_{s=0}^{+\infty} \delta^s \left(-\hat{w}(e) + \theta + \beta z(e+s)\right). \tag{B.3}$$

That is, compared to continually contracting with the firm and forgoing all surplus from the accumulation of CSS, the client is better served by hiring the current worker at wage $\hat{w}(e)$ and conducting production internally going forward. Using (B.2), we can rewrite condition

(B.3) as follows:

$$\underline{v} - p < -(1-\delta)c - \kappa + \gamma + \theta + (1-\delta)\sum_{s=0}^{+\infty} \delta^s z(e+s). \tag{B.4}$$

Because the RHS of (B.4) strictly decreases in e, for every $c \in \mathbb{R}_+$ there is a unique cut-off $T(c) \in \{0, 1, ..., +\infty\}$ such that (B.4) holds if and only if $e \geq T(c)$. In particular, we have $0 < T(c) < +\infty$ provided that the following condition holds:

$$(1-\delta)\sum_{s=0}^{+\infty} \delta^s z(s) < \underline{v} - p + (1-\delta)c + \kappa - \gamma - \theta < \lim_{e \to +\infty}(1-\delta)\sum_{s=0}^{+\infty} \delta^s z(e+s). \tag{B.5}$$

In other words, provided that (B.5) is satisfied, poaching becomes a potentially profitable "one-shot deviation" for the client when the worker's experience with him exceeds a finite threshold. To simplify the discussion going forward, we rule out some boundary cases by directly assuming $0 < T_L \equiv T(c_L) < T_H \equiv T(c_H)$.

B.4.1 Proof of Proposition 1

Let e_t be the units of experience that the assigned worker at period t has accumulated for serving the client, and c_t be the realized poaching cost. We will show that, provided that λ is sufficiently small, the following profile of behavioural strategies and associated beliefs constitute an equilibrium: For the client, he will poach the assigned worker at time t with a wage offer $w = \hat{w}(e_t)$ if and only if either (i) $c_t = c_L \equiv \underline{c} + \varepsilon_L$ and $e_t \geq T_L$, or (ii) $c_t = c_H \equiv \underline{c} + \varepsilon_H$ and $e_t \geq T_H$. As for the service firm, at the beginning of period t, it will send out a fresh worker if and only if $e_{t-1} \geq T_H$, i.e. the worker from period $t-1$ has accumulated at least T_H units of experience with the client. Further, if the client makes a poaching offer $w < \hat{w}(e_t)$, the firm will counter with some $w' < \kappa$ to make the worker strictly prefers to stay. However, if the client's offer satisfies $w \geq \hat{w}(e_t)$, then the firm will not make any counter-offer. Finally, each worker chooses the offer that gives her a higher payoff, with a tie-breaking rule favoring the client.

Note that the strategy profile above generates an equilibrium path as described in our proposition: the firm routinely rotates workers after every T_H periods, while poaching occurs whenever the client has a chance to poach a sufficiently skilled worker at low cost before the latter is rotated.

We now argue that, taking the firm's strategy as given, the decision rule above is optimal for the client if λ is small enough. We distinguish two cases.

Case 1: $c_t = c_H$, i.e., the client's poaching cost is high at period t. Then, poaching the assigned worker right away is suboptimal for the client if $e_t < T_H - 1$, because he can strictly improve his payoff by postponing and poaching the same worker in the next period. When $e_t = T_H - 1$, the payoff per period following the proposed strategy converges to $\underline{v} - p$ as $\lambda \to 0$. Thus, given how T_H is constructed, not poaching the worker at this stage is sequentially rational for the client provided that λ is sufficiently small. By an analogous limiting argument, one cannot have a profitable one-shot deviation from poaching when the client faces a worker with $e_t \geq T_H$, as long as λ is small enough.

Case 2: $c_t = c_L$, i.e., the client's poaching cost is low at period t. Then, poaching the worker right away is suboptimal for the client if $e_t < T_L$, because the resulting total payoff will be lower than always transacting with the firm. By contrast, when $e_t \geq T_L$, the client's payoff per period from a one-shot deviation – not poaching now and then returning to the proposed strategy from next period on – converges to $\underline{v} - p$ as $\lambda \to 0$. Hence, given how T_L is constructed, poaching a worker with $e_t \geq T_L$ is optimal for the client if λ is small enough.

Next, we take the behavioural strategy of the client as given and consider the incentive of the firm. When $e_{t-1} < T_H$, poaching only takes place when the cost of doing so is low for the client. Hence, the poaching risk will be small if λ is sufficiently small. 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. However, when $e_{t-1} \geq T_H$, the firm will for sure lose both its business and employee if it assigns the same worker to the client as before. Thus, given $\underline{\pi} - \kappa < p - \underline{w}$, the firm will strictly prefer to use rotation to mitigate the very substantial poaching risk that it faces at this stage. □

B.4.2 Proof of Proposition 2

Consider the equilibrium in Proposition 1. Note that, on the equilibrium path, each worker will at most be assigned to a client for T_H periods. Let $e \in \mathbb{N}$ be the units of experience that a worker has accumulated for serving the client. If $e < T_L$, the probability that the worker will be poached is zero. If $T_L \leq e < T_H$, the probability that the worker will be poached is $\lambda > 0$. Hence, the likelihood of poaching is always (weakly) increasing with the worker's client-specific experience □

B.4.3 Proof of Proposition 3

Let θ and θ' be the baseline productivity parameters of the workers from two different groups. Further, let $\{T_L, T_H\}$ and $\{T'_L, T'_H\}$ be the threshold values of client-specific experience associated with these two groups, respectively. Given (B.4), $\theta < \theta'$ implies that both $T_L \geq T'_L$ and $T_H \geq T'_H$ hold. Hence, conditional on having accumulated the same units of client-specific experience e, workers from the first group will be lower than those from the second group: when $e \in (T_L, T'_L)$, the probability is 0 for the first group but $\lambda > 0$ for the second group; when $e \notin (T_L, T'_L)$, the probability of poaching is the same for both groups. The statement that the frequency of rotation is higher for the second group also immediately follows. $\square$

B.5 Proof of Proposition 4

In the equilibrium that Proposition 1 describes, the frequency of worker rotation is inversely related to T_H. At the same time, it is clear from condition (B.4) that the value of T_H will increase as the baseline poaching cost $\underline{c}$ decreases. Moreover, as $\underline{c}$ becomes sufficiently large, there will not be any finite T_H that satisfies (B.4). In this case, we effectively have $T_H = +\infty$, which is equivalent to the firm never rotating workers. Thus, Proposition 4 follows directly from the relationship between T_H, rotation frequency, and the baseline poaching cost $\underline{c}$. $\square$

B.6 Extension: Uncertain Worker Preferences

In the baseline model, workers always leave the firm and become an in-house employee of the client when poaching occurs *on the equilibrium path*. However, in the data, we observe some workers declining the client's offer and staying with the firm. To rationalize this fact, we extend the model by allowing for uncertainty in workers' preferences. As an additional benefit, the extension also illustrates a scenario in which firms optimally utilize both rotation and complementary managerial practices (such as monetary incentives and amenities) in equilibrium to deter poaching.

Specifically, we now suppose that each worker's preference parameter γ is uncertain and is drawn from a commonly known distribution $\Pr(\gamma = \gamma_O) = 1 - \Pr(\gamma = \gamma_I) = g \in (0, 1)$, where $\gamma_I > \gamma_O$. The true value of γ is privately known to the worker. Let $T(c, \gamma) \in \mathbb{N} \cup \{0, +\infty\}$ be the cutoff such that:

$$\underline{v} - p + (1 - \delta)c + \kappa - \gamma - \theta < (1 - \delta) \sum_{s=0}^{+\infty} \delta^s z(e + s)$$

holds if and only if $e \geq T(c, \gamma)$. To simplify the discussion going forward, we rule out some boundary cases by directly assuming $0 < T(c_L, \gamma_I) < T(c_H, \gamma_I) < T(c_L, \gamma_O) = T(c_L, \gamma_O) = +\infty$, which holds whenever γ_O is sufficiently small but γ_I is moderate.

Proposition B.1. *If both λ and g are sufficiently small, there exists a Perfect Bayesian equilibrium in which the service-firm rotates the workers that it sends to the client after every $T(c_H, \gamma_I)$ periods, while the client poaches a worker whenever she draws a low poaching cost and that worker has served her for more than $T(c_H, \gamma_L)$ periods. On the equilibrium path, workers with $\gamma = \gamma_I$ will leave the firm when they are poached by the client, but workers with $\gamma = \gamma_O$ will not.*

PROOF. Let e_t be the units of experience that the assigned worker at period t has accumulated for serving the client, and c_t be the realized poaching cost. We will show that, provided that both λ and g are sufficiently small, the following profile of behavioural strategies and associated beliefs constitute an equilibrium: For the client, he will poach the assigned worker at time t with the wage offer

$$\hat{w}_I(e_t) \equiv \kappa - \gamma_I - (1-\beta)(1-\delta)\sum_{s=0}^{+\infty} \delta^s z(e_t + s).$$

if and only if either (i) $c_t = c_L$ and $e_t \geq T(c_L, \gamma_I)$, or (ii) $c_t = c_H$ and $e_t \geq T(c_H, \gamma_I)$. As for the firm, at the beginning of period t, it will send out a fresh worker if and only if $e_{t-1} \geq T(c_H, \gamma_I)$, i.e. the worker from period $t-1$ has accumulated at least $T(c_H, \gamma_I)$ units of experience with the client. Further, if the client makes a poaching offer $w < \hat{w}_I(e_t)$, the firm will counter with some $w' < \kappa$ to make a type-γ_I worker strictly prefers to stay. If $\hat{w}_I(e_t) \leq w < \hat{w}_O(e_t) \equiv \kappa - \gamma_O - (1-\beta)(1-\delta)\sum_{s=0} \delta^s z(e_t + s)$, the firm will counter with some $w' < \kappa$ to make a type-γ_O worker strictly prefers to stay. If $w \geq \hat{w}_O(e_t)$, then the firm will not make any counter-offer. Finally, each worker chooses the offer that gives her a higher payoff, with a tie-breaking rule favoring the client. Note that this strategy profile will generate an equilibrium path as described in our proposition.

We now argue that, taking the firm's strategy as given, the decision rule above is optimal for the client if both λ and g are small enough. We distinguish two cases.

Case 1: $c_t = c_H$, i.e., the client's poaching cost is high at period t. Then, poaching the assigned worker right away is suboptimal for the client if $e_t < T(c_H, \gamma_I) - 1$, because he can strictly improve her payoff by postponing and poaching the same worker in the next period. When $e_t = T(c_H, \gamma_I) - 1$, the payoff per period following the proposed strategy converges to $\underline{v} - p$ as $\lambda \to 0$. At the same time, by poaching the worker with a wage offer $\hat{w}_I(e_t)$, the client will be able to bring the worker in house for sure if $\gamma = \gamma_I$, so her payoff will converge to $-c_H + \sum_{s=0}^{+\infty} \delta^s(-\hat{w}_I(e_t) + \theta + \beta z(e_t + s))$ as $g \to 0$. Note that it would always be suboptimal for the client to offer wage $w \neq \hat{w}_I(e_t)$. Hence, given how $T(c_H, \gamma_I)$

is constructed, poaching will be suboptimal for the client provided that both λ and g are small enough. By an analogous limiting argument, one cannot have a profitable one-shot deviation from poaching when the client faces a worker with $e_t \geq T(c_H, \gamma_I)$, as long as bath λ and g are small enough.

Case 2: $c_t = c_L$, i.e., the client's poaching cost is low at period t. Then, poaching the worker right away is suboptimal for the client if $e_t < T(c_L, \gamma_I)$, because the resulting total payoff will be lower than always transacting with the firm. By contrast, when $e_t \geq T(c_L, \gamma_I)$, the client's payoff per period from a one-shot deviation – not poaching now and then returning to the proposed strategy from next period on – converges to $\underline{v} - p$ as $\lambda \to 0$. Hence, given how $T(c_L, \gamma_I)$ is constructed, poaching a worker with $e_t \geq T(c_L, \gamma_I)$ by making a wage offer $w = \hat{w}_I(e_t)$ is optimal for the client as long as both λ and g are small enough.

Next, we take the behavioural strategy of the client as given and consider the incentive of the firm. When $e_{t-1} < T(c_H, \gamma_I)$, poaching only takes place when the cost of doing so is low for the client. Hence, the poaching risk will be small if λ is sufficiently small. 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. However, when $e_{t-1} \geq T(c_H, \gamma_I)$, the firm will for sure lose the client if it assigns the same worker to the client as before, and almost sure losing the worker as well when g is sufficiently small. Thus, given that the value of $\underline{\pi} - \kappa$ is sufficiently small, the firm will strictly prefer to use rotation to mitigate the very substantial poaching risk that it faces at this stage. $\square$

B.7 IV Approach for Estimating the Effect of Building-Specific Experience on Crime

Our estimates of equation 1 remain unbiased in the presence of endogenous matching between the characteristics of guards and buildings as we control for the guard-building pair fixed effects. Alongside our rich set of controls, which absorb a wide range of potential confounders, several robustness checks help alleviate some concerns such as reverse causation. Nevertheless, we acknowledge that some potential issues persist unaddressed by the OLS regression. For example, if a guard takes a leave due to illness, their building-specific experience will be relatively low. Upon returning to work, they may not be fully recovered, potentially affecting their efficiency in preventing crime. Likewise, temporary shocks at the building level could influence the schedules of multiple guards (e.g., new building administrators might request more frequent rotation of guards), potentially correlating with overall crime rates for the period. To address some of these concerns, we leverage a source of variation which likely influences a guard's actual experience in buildings but is plausibly uncorrelated with crime outcomes. As we outline below, this instrumental variable (IV) approach rests on necessary assumptions and is not without its limitations. Therefore, we interpret the IV results not as a definitive proof, but as reassuring evidence that provides an additional layer of robustness to our main results.

We take advantage of the guard allocation process, which typically assigns guards to types without considering guards' characteristics. We provide evidence supporting this claim in Appendix B.2.[15] Consider two guards, one of type-I and the other of type-II, starting work at the same building on the same day. Due to the nature of their schedules, after a specific number of calendar days, the type-I guard will have accumulated more shifts compared to the type-II guard. As this temporal progression is purely mechanical, the interaction between a calendar linear trend and the guard type serves as a relevant instrument that is likely exogenous to crime outcomes. Intuitively, the actual experience of a guard may depend on several factors, some being subject to exogenous events, and others possibly endogenous to crime. However, experience also depends on the simple passage of time and its interaction with the guard type. Our instrument essentially isolates the variation arising from this last source.[16]

The results reported in Column (1) of Appendix Table B1 confirm the previous findings from the OLS estimations. The estimated coefficients of the client-specific experience are very similar in magnitude. Consistent with the fact that type-I guards accumulates experience at a much higher rate per period, the first stage is strong with an F statistic (Kleibergen-Paap) above 300.

The fundamental identification assumption is that building-specific skills and experience are primarily driven by the effective time a guard spends in a building, rather than simply by the passage of calendar time. This means that the acquisition of building-specific knowledge and the strength of the relationships developed with the building and its residents are directly tied to the total amount of time a guard physically spends in that building. Although this is a plausible assumption in this setting, this can't be tested directly and the findings should be

[15]It is worth noting that we do not argue or believe this allocation to be irrational from the firm's perspective. In fact, our partner firm has highlighted that due to labor market tightness, any delay in filling a vacancy can be costly. Consequently, the typical strategy is to assign the best candidate from the pool of applicants, or the first applicant who meets the minimum requirements, to the vacancy.

[16]It's important to note that the linear time variable is usually absorbed by (or highly collinear with) the controls of the baseline regression, making it irrelevant. Additionally, using either a common linear trend (i.e., calendar time) or tenure within the building yields similar results. This is because tenure in the building equals calendar time minus the date the guard started working in the building, which is absorbed by the guard-building fixed effects.

interpreted bearing this caveat in mind. A second more nuanced concern is that the learning or skill acquisition rates differ across types for the same effective time worked in a building. In essence, the IV estimates might capture not just the impact of experience (measured by the number of shifts worked in a building), but also a potentially more efficient learning process about the client among Type I guards. An indirect check of this assumption relies on the fact that the evolution of crime over effective time spent in the building appears broadly similar for guards of different types (Appendix Figure B9).[17] While this check is indirect and largely descriptive, it helps to alleviate potential concerns about differential learning rates influencing the results.

In addition to the necessary assumptions we have already discussed, it's necessary to acknowledge additional potential threats to the validity of the IV. For instance, a concern could arise if Type I or Type II guards are systematically assigned to work during shift-building periods with relatively higher crime rates. Similarly, an increase in crime rates at a building may prompt a change in the rotation pattern for one type of guards, such as requiring Type II guards to work more or less frequently during these periods. While we consider these scenarios as very unlikely – primarily due to the typically rigid patterns of work schedules and off-days for guards, which restrict the flexibility in adjusting when Type II guards are assigned to shifts to covering Type I guards' off-days – we cannot definitively rule them out. Consequently, caution is advised when interpreting our results.

B.8 Details of the Estimation of the Event Study

In this subsection, we provide additional details of the event study around the rotation of guards which provides further evidence on the relationship between building-specific experience and crime.[18] 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). We also exclude any control guard that rotates within

[17] We also find no statistically significant difference in the regression slope between productivity and effective building-specific experience across guard types.

[18] 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.

the comparison window. This allows for a transparent control group and it alleviates concerns regarding dynamic effects as discussed below.

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

$$
\begin{aligned}
Crime_{ibt} = \beta(RotGuard_{it} \times PostRot_{it}) + \eta_i \times WinRot^j_{it} \\
+ \rho(PostRot_{it} \times WinRot^j_{it}) + \eta TotalExp_{it} + \delta_{b(it)} + \epsilon_{it}, \quad \text{(B.6)}
\end{aligned}
$$

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, $TotalExp_{it}$ controls for the fact that even after rotation, the guard retains the overall experience gained while working in the firm and we also include indicators for neighborhood $\times$ month 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 neighborhoods). We cluster standard errors (multi-way) at guard and $WinRotation^j_{it}$ level.

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

[19] In order to control for the possibility that guards' rotation coincides with periods of high (low) crime in the building, we also run (B.6) controlling for neighborhood $\times$ month fixed effects. The results we obtained (not reported) are very similar to those of Table 3.

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 at the beginning of this window.

B.9 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.[20]

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. Then, we estimate a cross-section Random Forest model, where the dependent variable is a

[20]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.

dummy taking one if the guard was poached or solicited.[21] 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.[22] [23]

Appendix Figure B10 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 B10 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.[24]

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

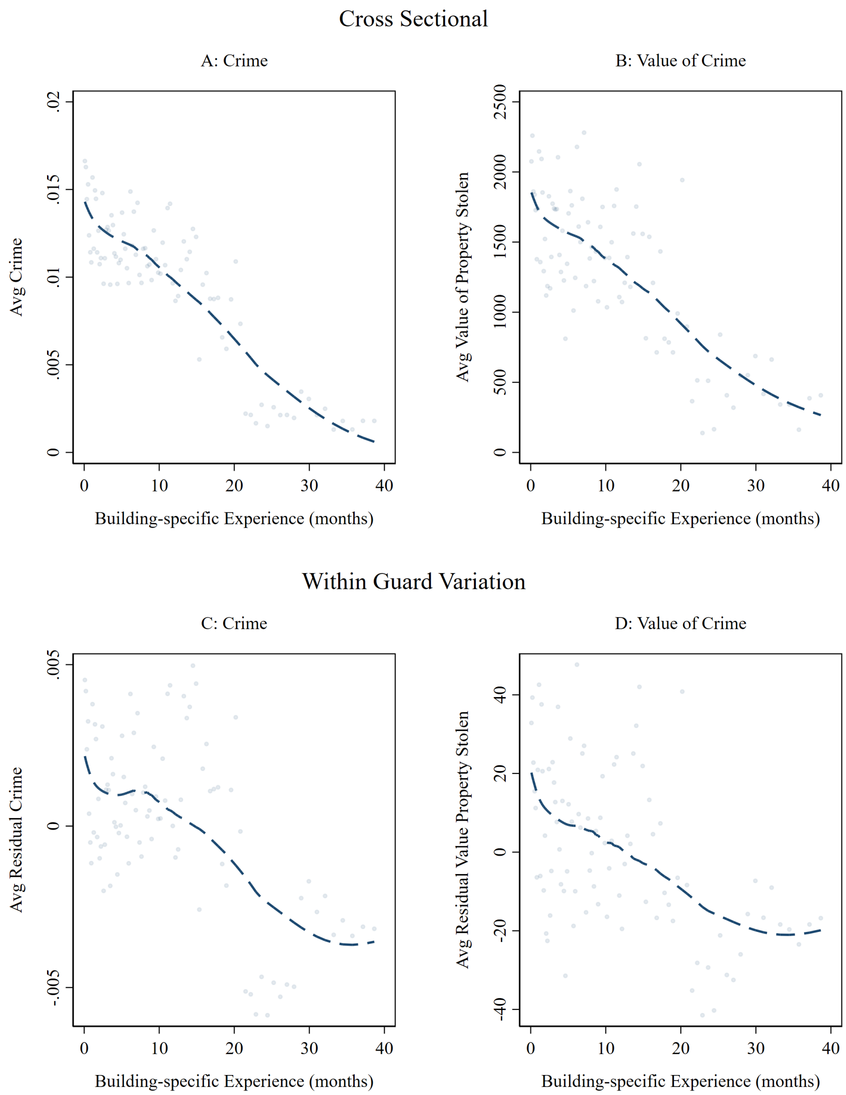
[22] Specifically, we run a Random Forest model based on Gini impurity with 500 trees (bootstrap based samples). Since our data contains few cases of poaching, we follow the standard procedure of using an asymmetric loss function that assigns higher weight to misclassification of the least prevalent 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.

[23] Accounting for many interactions and high order non-linearities may also help capturing non-observable features of the guards related to poaching, however, our approach relies on observable characteristics and therefore it does not necessarily encapsulate all the determinants of the true risk of poaching.

[24] 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 B5.

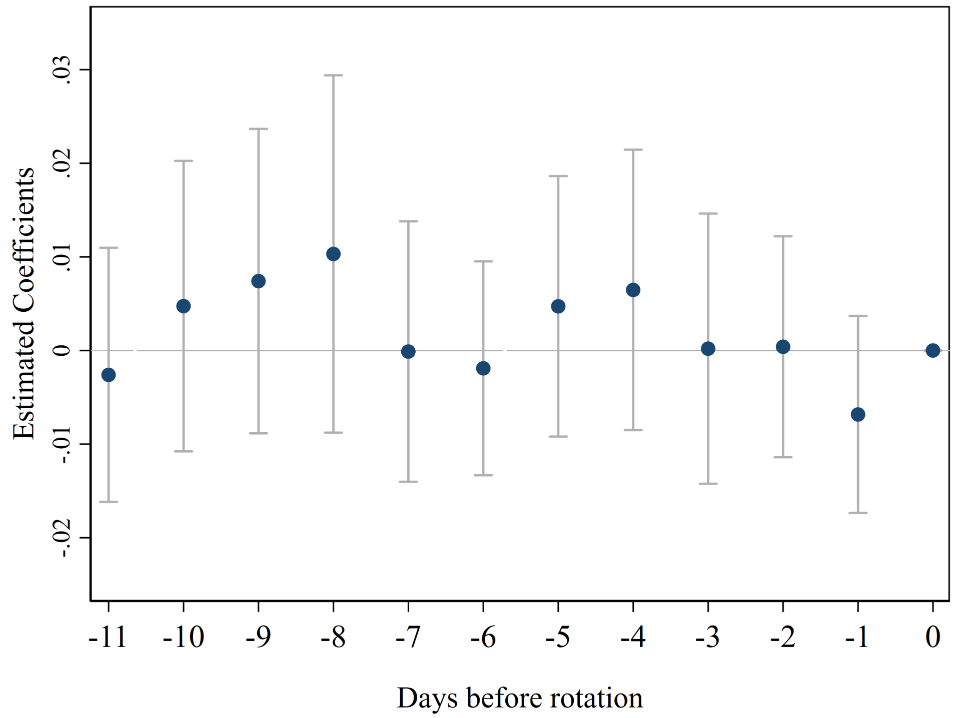
B.10 Additional Figures and Tables

Figure B1: Productivity and Client-Specific Experience (Non-Parametric Estimation)



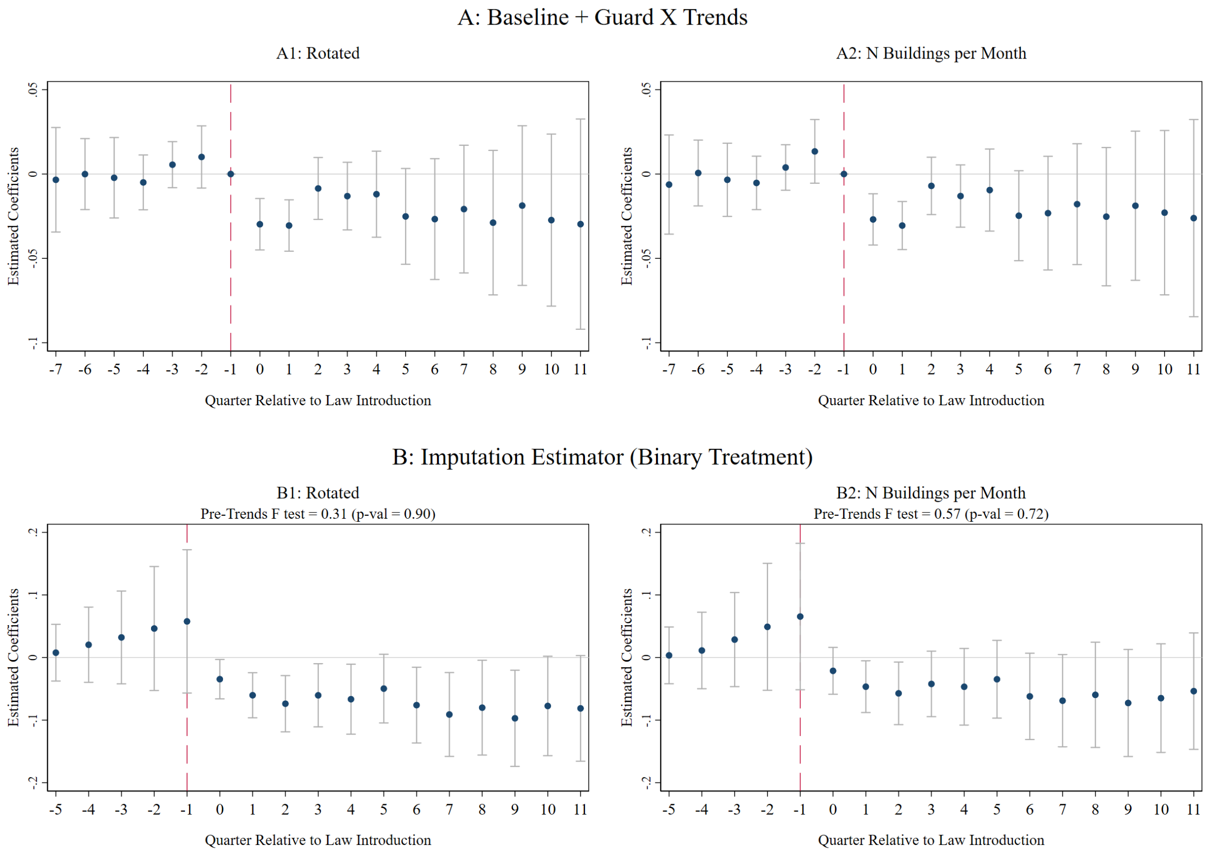
This figure displays the estimated relation between productivity variables (crime and the value of property loss due to crime) and the effective experience accumulated in the building. Observations are grouped and averaged across bins of effective building-specific experience (in months) where the bins correspond to percentiles of the distribution of building-specific experience. In Panels A and B, the dependent variable corresponds to the raw average for the bin as observed in the data. In Panels C and D, the relation is estimated within-guard by first residualizing the dependent variable to remove guard fixed effects and the total experience of the guard. Each dot corresponds to a different bin of building specific experience. The curves are estimated using non-parametric local polynomial regressions.

Figure B2: 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 B3: 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.

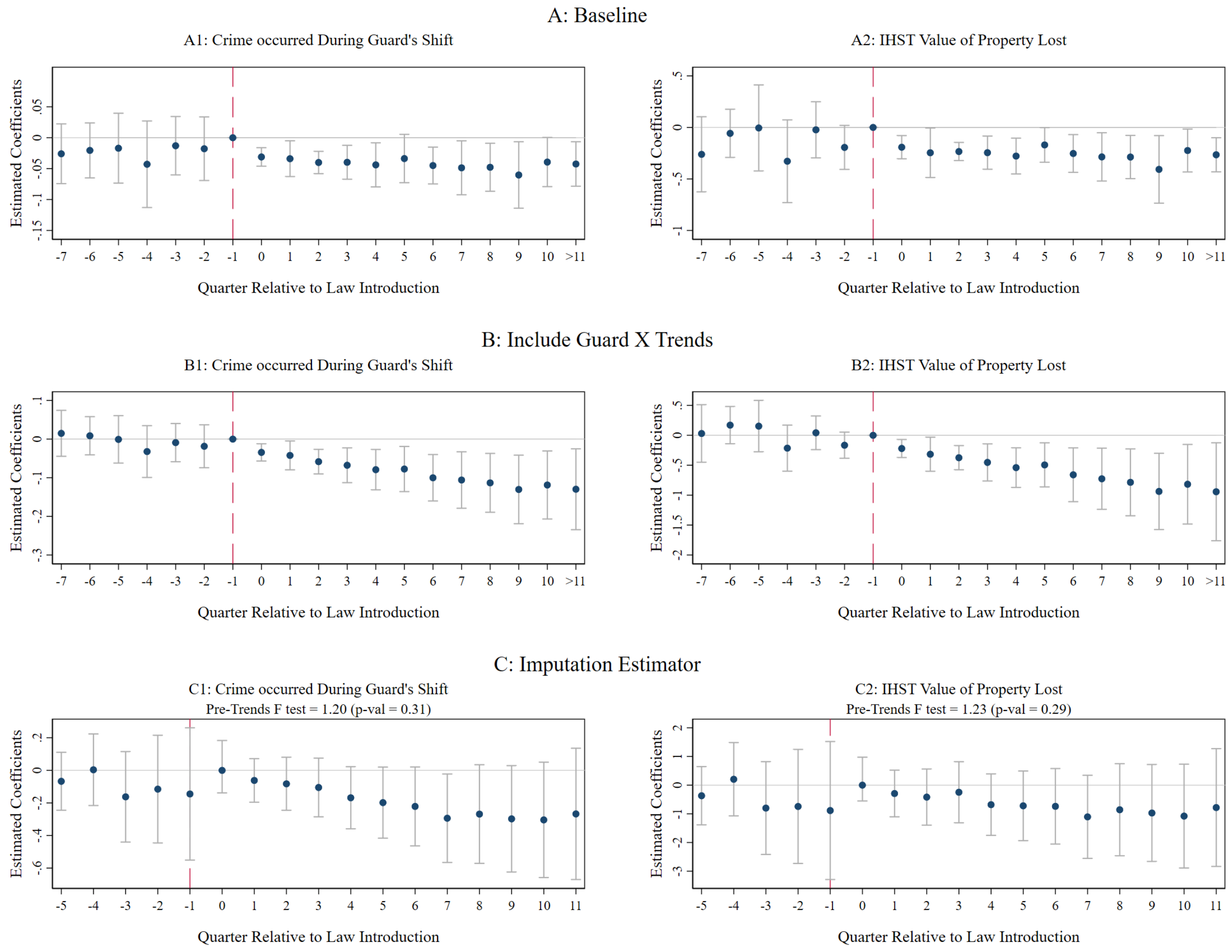
Panel A displays the estimated coefficients and the 95% confidence intervals of interaction between the estimated risk of being poached, with leads and lags indicators relative to the quarter when the degree was introduced. The omitted category is the interaction with the quarter previous to the introduction 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. $N = 15,373$.

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:

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

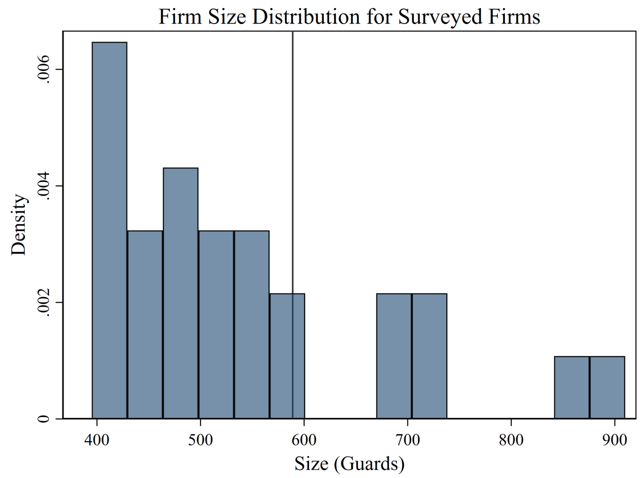
The specification is similar to the one used in Panel A and is defined over the same sample but the "treatment" is defined by the binary variable $HighRisk_i$ (which takes the value 1 if the guard is above the median of estimated poaching risk across all guards). Standard errors are clustered at the 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 B4: Effects of the Decree 356 on Crime



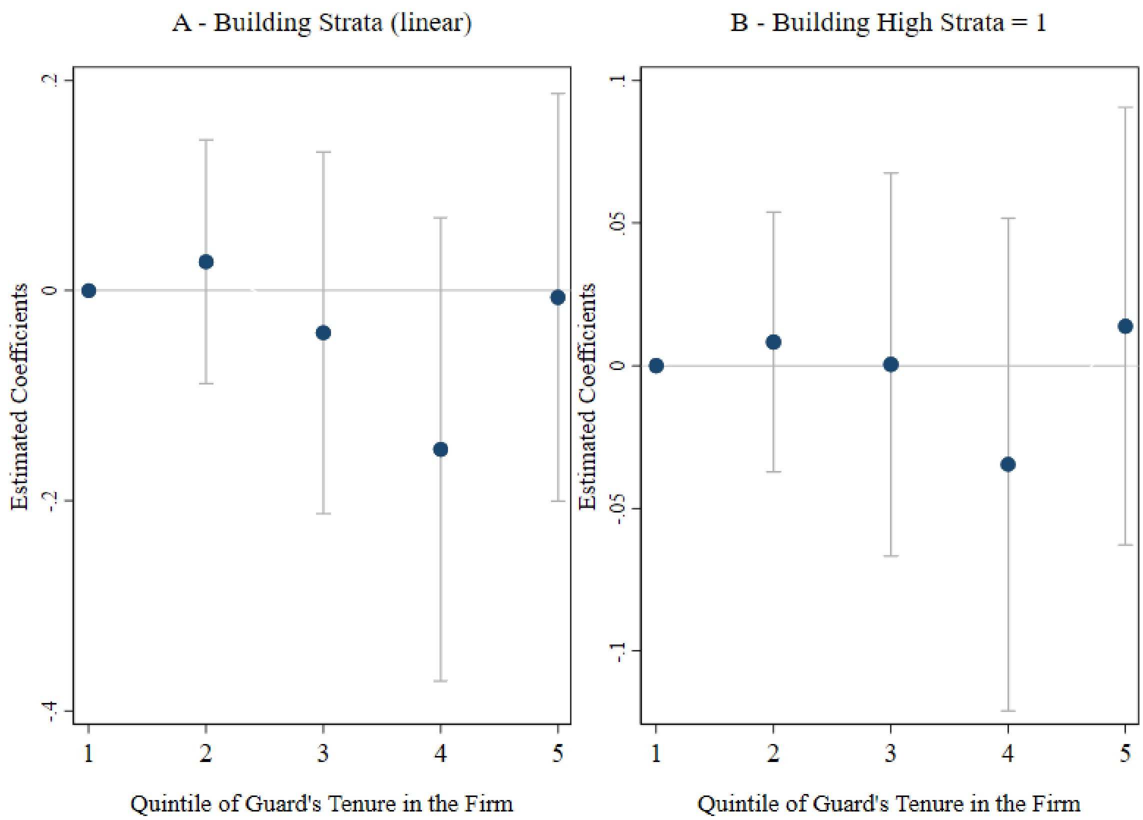
This figure shows the lead and lags effects of the Decree 356 on crime. The dependent variable in Panels A1, B1 and C1 is an indicator for whether a crime occurred during a shift where the guard was working. In Panels A2, B2 and C2, the dependent variable is the inverse-hyperbolic-sine transformed value 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 the quarter previous to 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. $N = 15,373$.

Figure B5: Firm Size Distribution



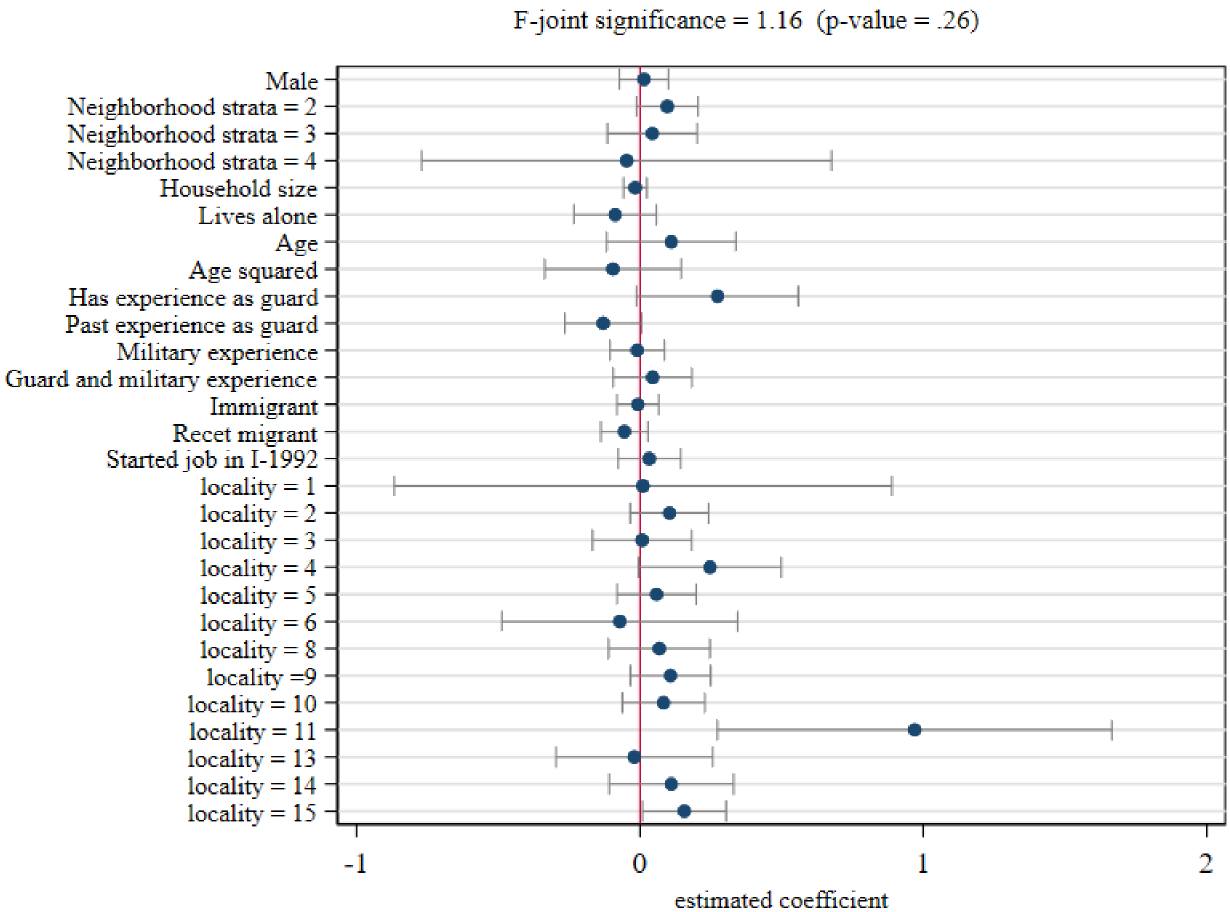
This figure shows the firms size distribution of the firms that replied the survey. The red vertical line shows the firm size of our partner firm. $N = 23$ firms.

Figure B6: Building Socio-economic Strata and Guard's Tenure



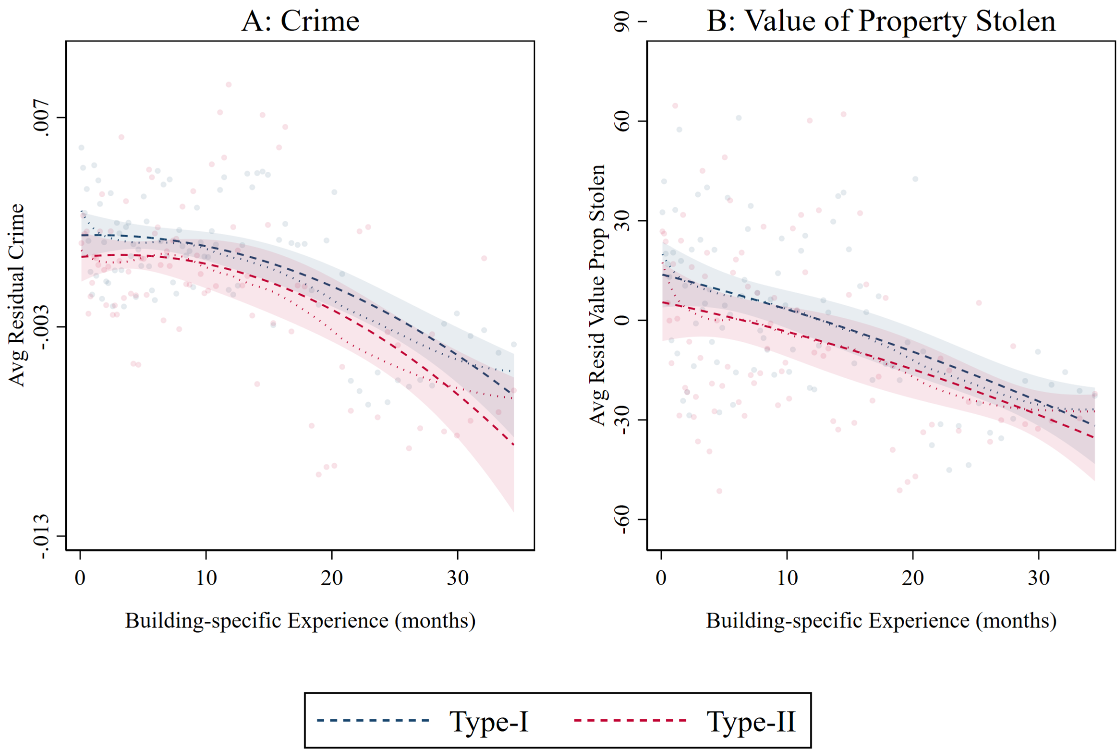
This figure displays the estimated coefficients and the 95% confidence intervals of regressions of the building's strata and indicators for the quantile of guard's tenure within the firm. The regressions have controlled for both guard fixed effect and month fixed effect. In Panel A, the dependent variable 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 B7: Balance Tests for Type-I vs. Type-II Allocation



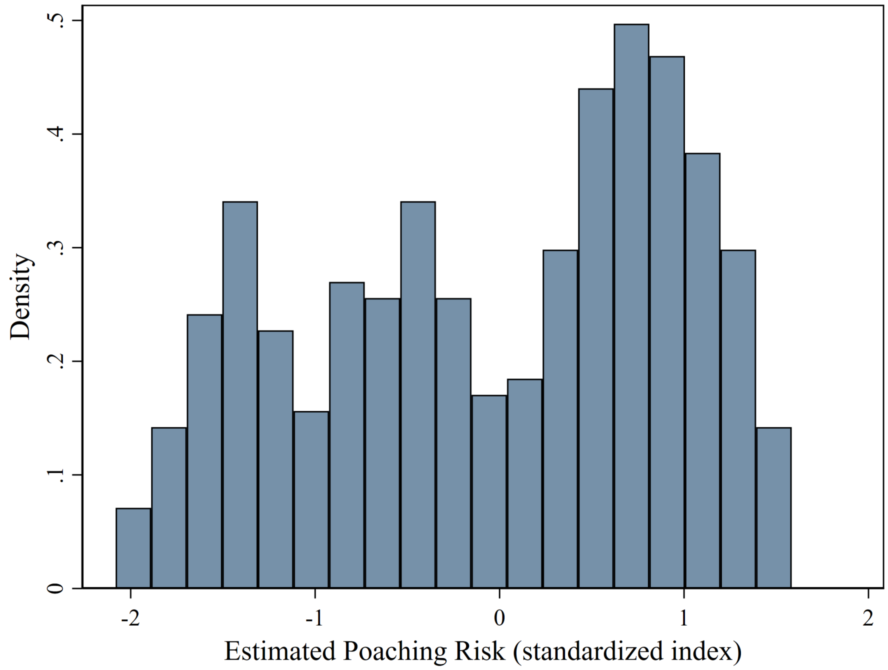
The figure displays the estimated coefficients and the 95% confidence intervals of a regression, where the dependent variable is an indicator of the guard being type-II and the explanatory variables are predetermined characteristics of the guard. Non-dummy variables are standardized. The figure also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

Figure B8: Productivity and Client-Specific Experience by Type of Guard



The figure displays the estimated relation between the productivity variables (crime and the value of property loss due to crime) and the effective experience accumulated in the building. The dependent variable is first residualized to remove guard fixed effects and the total experience of the guard interacted with and indicator for the type of guard. Observations are grouped and averaged across bins of effective building-specific experience (in months) where the bins correspond to percentiles of the distribution of building-specific experience. The dashed line and 95% confidence interval corresponds to estimates of a quadratic polynomial relation. The dotted line corresponds to the fully non-parametric estimation using local polynomial regressions. The relation is estimated for the common support of both types of guards (35 months of effective experience) and extreme values larger than two times the 99th percentile of the dependent variable are excluded from the estimation.

Figure B9: 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 score 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 index is standardized with zero mean and unit standard deviation.

Table B1: Productivity and Client-Specific Experience
Additional Robustness

	(1) IV	(2) NonParm TotExp	(3) Exclude 1st Month	(4) Exclude 1st Build
	Panel A: Crime occurred During Guard's Shift			
Exp in Building (months)	-.0031***	-.0027***	-.0031***	-.0026***
	(.00043)	(.00025)	(.00034)	(.00041)
N	109,094	109,094	106,633	59,715
R2	.0041	.11	.11	.09
F first-stage (K-P)	338	-	-	-
Mean Depvar	.043	.043	.042	.028
	Panel B: IHST Value of Property Lost in Crime			
Exp in Building (months)	-.037***	-.032***	-.037***	-.03***
	(.0051)	(.003)	(.004)	(.0048)
N	109,094	109,094	106,633	59,715
R2	.004	.1	.1	.088
F first-stage (K-P)	338	-	-	-
Mean Depvar	.52	.52	.5	.34
Method:	IV	OLS	OLS	OLS
Guard X Building FE:	YES	YES	YES	YES
Week FE:	YES	YES	YES	YES
Days Worked Week:	YES	YES	YES	YES
Shift and Weekend controls:	YES	YES	YES	YES
Total Experience:	LIN	BINS	LIN	LIN
First Month Guard:	YES	YES	NO	YES
First Build of Guard:	YES	YES	YES	NO

N guards = 567; N buildings = 116. All regressions are at guard x week x building level. The independent variable is the acumulated experience of the guard in the building (measured in in months). In Panel A, the dependent variable is an indicator for a crime occurring during a shift when the guard was working in the building during the week. 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: Guard-Building fixed effects, week fixed effects, the number of shifts that the guard worked during the week, the share of night shifts and an indicator for whether the guard worked during the weekend. Columns (1), (3) and (4) controls for the total experience of the guard measured in month and Column (2) includes separate dummies for each quintile of the distribution of total experience. In Column (1) the accumulated experience of the worker in the building is instrumented with the interaction between the tenure of the guard in the building (in months) and an indicator for the type of the guard. The F-Statistics (Kleibergen-Paap) for the first stage of the IV regression is displayed in the table. Column (3) excludes from the sample any observation corresponding to the first month worked by the guard after joining the firm. Column (4) excludes from the sample any observation corresponding to the first building where the guard was allocated.Robust standard errors clustered at the guard level are reported in parentheses.

Table B2: 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 X Rotat Guard X:			
High Exp in Building	.03***	.026***	.027***
	(.0077)	(.008)	(.0082)
Low Exp in Building	.012	.0084	.0066
	(.017)	(.016)	(.017)
N	33,065	19,250	20,185
Panel B:	**IHST Value of Property Lost in Crime**		
Post Rotation X Rotat Guard X:			
High Exp in Building	.36***	.32***	.33***
	(.093)	(.097)	(.1)
Low Exp in Building	.13	.089	.068
	(.2)	(.2)	(.2)
N	33,065	19,250	20,185

N guards = 416; N buildings = 103; Mean Depvar A = .057; Mean Depvar B = .685. 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 (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 at any shift when the guard was working during the week. 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 number of days worked during the week. Additional controls are the total experience of the guard, the share of night shifts worked during the week and an indicator for the guard working the weekend. Standard errors clustered at the guard-rotation episode window.

Table B3: Poaching and Client-Specific Experience. Cross Sectional Correlation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Guard Level Correlation								
Avg Rotation	-.5***	-.6***	-.56***					
	(.14)	(.18)	(.16)					
Mean Depvar	.053	.053	.053					
Guard Chars:	NO	YES	YES					
Poach Risk:	NO	NO	YES					
Panel B: Guard × Building Level Correlation								
	All Building and Guards				**Only Poaching Building**			
(max) Experience in the Building	.018***	.021***	.018***	.019***	.033***	.034***	.029***	.02**
	(.0039)	(.0049)	(.0051)	(.005)	(.0062)	(.0074)	(.0082)	(.0076)
Total Experience:	YES	YES	YES	YES	YES	YES	YES	YES
Building FE:	NO	YES	YES	YES	NO	YES	YES	YES
Guard Chars:	NO	NO	YES	YES	NO	NO	YES	YES
Poach Risk:	NO	NO	NO	YES	NO	NO	NO	YES

This table explores the cross-sectional correlation between poaching and measures of building specific experience. The sample period corresponds to the months before the policy change was introduced. **Panel A** shows the estimates of a regression between a dummy for the guard being poached and the average rotation (share of months when the guard was rotated) of the guard for the whole period when is observed. Each observation (N = 454) is a guard. Column (2) control for guard characteristics (gender, previous experience, household structure, migration status and type) and column (3) also controls for estimated risk of poaching index of the guard. **Panel B** uses observations at the guard × building pair level and displays the estimates of a regression between a dummy taking one if the guard was poached by the building and the (maximum) building-specific experience accumulated at the corresponding building measured in months. Columns (1) to (4) includes all the observed guard-building pairs (N = 926) and columns (5) to (8) only include the set of buildings that poached a guard and the guards that have worked for that building in the month prior to poaching (N = 98). Columns (2) to (4) and (6) to (8) control for building fixed effect. Columns (3), (4), (7) and (8) also control for guard characteristics (gender, previous experience, household structure, migration status and type). Columns (4) and (8) additionally control for the estimated risk of poaching index of the guard. Robust standard errors displayed in parentheses.

Table B4: Effect of the Policy. Day level regressions

Dependent Variable	(1) Rotated	(2) N of Crimes	(3) Rotated	(4) N of Crimes	(5) Rotated	(6) N of Crimes
Post × Poaching Risk	-.0008***	-.0012*	-.00093***	-.0012*	-.00071***	-.0019***
	(.000092)	(.00065)	(.00018)	(.00068)	(.00021)	(.00068)
N	399,781	399,781	399,781	399,781	399,781	399,781
Mean Depvar	.00094	.0079	.00094	.0079	.00094	.0079
Indiv Chars:	YES	YES	YES	YES	YES	YES
Date FE:	YES	YES	YES	YES	YES	YES
Guard FE:	YES	YES	YES	YES	YES	YES
Building FE:	YES	YES	YES	YES	YES	YES
Guard Trends:	NO	NO	YES	YES	YES	YES
Building Trends:	NO	NO	YES	YES	YES	YES
Guard X Transition:	NO	NO	NO	NO	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 the estimated poaching risk of the guard. The poaching risk index is standardized to a mean of zero and a SD of one. In Columns (1), (3) and (5) the dependent variable is an indicator of whether the guard was rotated to a new building that date. In Columns (2), (4) and (6) the dependent variable is the number of crimes that occurred in the building in 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, a shift (day or night) indicator and an indicator for the first month of the guard in the firm. Columns (3) and (4) include guard-specific and building-specific monthly linear trends. Columns (5) and (6) control for the interaction between guard fixed effect and an indicator for the two quarters after the law was introduced. The sample period corresponds to 7 quarters before and 16 quarters after the law introduction. Robust standard errors are clustered two-ways at the guard-month level and are shown in parenthesis.

Table B5: Effect of the Policy on Rotation. 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	-.011
	(.0021)	(.012)	(.0064)	(.00085)	(.0054)	(.0075)
N	15,161	17,599	17,599	17,599	17,599	17,599
Mean Depvar	.025	.024	.024	.024	.024	.024
Panel B:			**N 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 guards = 347; N 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 the predicted score of a Random Forest model 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 11 years of past experience when joining the firm (the interval span the years with highest positive correlation with the 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, the log-experience of the guard and an indicator for the first month of the guard in the firm. The sample period corresponds to 7 quarters before and 16 quarters after the law introduction. Robust standard errors clustered two-ways at guard and month level are shown in parenthesis.

Table B6: Effect of the Policy on Rotation Using High vs. Low Risk

	Monthly Obs		2x2 D-i-D	
	(1)	**(2)**	**(3)**	**(4)**
Dependent Variable	**Rotated**	**N Builds Worked**	**Rotated**	**N Builds Worked**
Post × High Risk	-.045***	-.047***	-.053***	-.056***
	(.009)	(.01)	(.0078)	(.0076)
N	7,418	7,418	294	294
R2	.081	.072	.13	.14
Mean Depvar	.025	1	.027	1
Indiv Chars:	YES	YES	NO	NO
Month FE:	YES	YES	NO	NO
Guard FE:	YES	YES	NO	NO
Building (most worked) FE:	YES	YES	NO	NO

This table investigates the effects of the introduction of the decree on guard's rotation using an alternative diff-in-diff specification where the control units are defined as those guards with estimated poaching index to be below the 25th percentile of the index distribution and treated units correspond to guards above the 75th percentiles of the distribution (N=167). Guards in between the 75th and 25th percentile are not in the estimating sample. Each column reports the coefficient of the interaction between an indicator for the period after the law was introduced and an indicator for the guard being above the 75th percentile of the risk distribution. In Columns (1) and (3) the dependent variable is an indicator of whether the guard was rotated to a new building during the month. In Columns (2) and (4) the dependent variable is the average number of buildings in which the guard worked during the month. In Columns (3) and (4) the dependent variable is averaged over the period before and after the policy introduction. Columns (1) and (2) 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. Columns (1) and (2) additionally control for the total number of days the guard worked during the month, the log-experience of the guard and an indicator for the first month of the guard in the firm. In Columns (3) and (4) there are only two observations per guard, corresponding to the periods before and after the policy introduction. The only control in Columns (3) and (4) are indicators for the guard being above the 75th percentile of the distribution and an indicator for the period after the policy introduction. The sample period corresponds to 7 quarters before and 16 quarters after the law introduction. Robust standard errors are clustered two-ways at the guard-month level in Columns (1) and (2) and clustered at the guard level in Columns (3) and (4).

Table B7: Effect of the Policy on Crime Using High vs. Low Risk

	Monthly Obs		2x2 D-i-D	
Dependent Variable	(1) N of Crimes	(2) IHST Value Prop Lost	(3) N of Crimes	(4) IHST Value Prop Lost
Post × High Risk	-.13**	-.72**	-.085*	-.66***
	(.054)	(.28)	(.047)	(.25)
N	7,418	7,418	294	294
R2	.26	.25	.43	.53
Mean Depvar	.2	1.4	.29	2
Indiv Chars:	YES	YES	NO	NO
Month FE:	YES	YES	NO	NO
Guard FE:	YES	YES	NO	NO
Building (most worked) FE:	YES	YES	NO	NO

This table investigates the effects of the introduction of the decree on crime using an alternative diff-in-diff specification where the control units are defined as those guards with estimated poaching index to be below the 25th percentile of the index distribution and treated units correspond to guards above the 75th percentiles of the distribution (N=167). Guards in between the 75th and 25th percentile are not in the estimating sample. Each column reports the coefficient of the interaction between an indicator for the period after the law was introduced and an indicator for the guard being above the 75th percentile of the risk distribution. In Columns (1) and (3) the dependent variable is the number of crimes that occurred in the building in the shifts when the guard was working. In Columns (2) and (4) 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. In Columns (3) and (4) the dependent variable is averaged over the period before and after the policy introduction. Columns (1) and (2) 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. Columns (1) and (2) additionally control for the total number of days the guard worked during the month, the log-experience of the guard and an indicator for the first month of the guard in the firm. In Columns (3) and (4) there are only two observations per guard, corresponding to the periods before and after the policy introduction. The only control in Columns (3) and (4) are indicators for the guard being above the 75th percentile of the distribution and an indicator for the period after the policy introduction. The sample period corresponds to 7 quarters before and 16 quarters after the law introduction. Robust standard errors are clustered two-ways at the guard-month level in Columns (1) and (2) and clustered at the guard level in Columns (3) and (4).

Table B8: 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.

Table B9: Investigating the Matching Between Guards and Buildings

	(1) All Pairs of Guard-Building		(2) 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 B10: 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 risk that a guard is hired in-house (estimated using a Random Forest model) and the baseline characteristics of the guards. The poaching risk index is standardized to a mean of zero and a SD of one. 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.