# Online Appendix
# Talent Poaching and Job Rotation

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## A.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 in 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 or 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:

### A.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?

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 is 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 would 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?

### A.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 that she was sent after joining the firm. The $F$ statistics for joint significance of these cross-section regressions are reported in the Appendix Table A.10. We find very low $F$-statistics (none is larger than 1.8 and only one -per column- is significant at 5%). We also check whether guards are rotated to better/worse buildings as their tenure within the firm increases. In Appendix Figure A.8 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 guard on a set of baseline characteristics of the guard. The results of this regression, including the estimated coefficients and the F-test of joint significance, can be found in Figure A.9. Our findings indicate that out of the 30 coefficients examined, only one is statistically significant at the 5% level (the dummy for locality 11). Importantly, the F-joint statistic is low and non-significant, suggesting that guards' baseline characteristics do not explain their assignment to either type I or II.

## A.3 Back-of-Envelope Poaching Cost Calculation

We propose that the cost imposed on the firm due to client poaching can be decomposed into 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).[1]

**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. Since a lost client will eventually be replaced by a new one, this cost is not permanent. Thus, we define the Foregone Profits as $C_\pi = 4MinW\mu$ where $\mu$ 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 equate $\mu$ to 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.** Due to the need to replace a poached guard with a new hire, the firm experiences a loss in client-specific experience, which results in lower productivity.[2] 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 $\tau$ 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 departed client with an identical one, the productivity of a new guard will be lower initially. Then, in the first period after poaching, there is an initial reduction in client-specific experience equal to $Y(\tau) - Y(0)$. Over time, the differences between the productivity of the poached guard (in a hypothetical scenario where the guard remains in the building) and the replacement guard can become smaller and eventually disappear after $T$ periods. It is important to note that our measure of productivity is limited to the monetary cost of crime, which may underestimate the

[1]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.

[2]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.

total productivity lost (as we explain in the paper, we do not observe all dimensions of productivity such as client's trust). Additionally, we simplify our analysis by assuming that the firm bears the full cost of the productivity loss.

We begin by calculating the initial percentage gap in productivity, $\hat{\beta} \times \hat{\tau}$, where $\hat{\tau}$ is the median client-experience at which poaching takes place (which corresponds to the loss of client experience endured by the firm) and $\hat{\beta}$ refers to the regression coefficient estimates from Table 2 (Panel B). We multiply this predicted percentage change in productivity by the average value of property lost that a guard would experience over a month, resulting in an approximate value of \$10. This value is interpreted as the monetary cost incurred (due to higher crime) during the first month in which the client-specific experience is lost. To account for additional costs associated with the crime that are not captured by the monetary value of the stolen property, we use information from Matthew et al. (2018) who quantify both monetary and non-monetary costs of domestic burglary in the UK. We calculate the ratio of "other costs" - which mainly include physical and emotional harm, lost output, and health costs - to the value of the property stolen, and we use this ratio to augment our estimated costs of lost property value. This allows us to obtain a more comprehensive figure that represents the full cost experienced by the client due to crime. In summary, our estimation indicates that the monthly cost of crime, specifically due to the lost of client-specific experience $(Y(\tau) - Y(0))$, is approximately \$22, and broadly 45% of this cost is the value of lost property and 55% are other costs.

The initial cost mentioned above 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). To estimate the cost for subsequent periods, we need to make further simplifying assumptions. The descriptive non-parametric estimation from Appendix Figure A.3 suggests that returns to experience in the building become relatively stable after around 3 years of experience. While this is only a descriptive pattern, 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 as a guard with 3.2 years of experience. In other words, we specify the time horizon discussed earlier as $T = 36$. For simplicity, we further assume that the initial productivity loss is reduced linearly over time and disappears after three years, i.e., $Y(\tau + t) - Y(t) = (Y(\tau) - Y(0))(1 - t/T)$.[3] The final estimated cost over the three years for which we expect productivity to differ between a poached and a new guard is $C_p = \$395$. This estimation corresponds to a single guard but the termination of the relationship with the client also results in the loss of building-specific experience for other guards assigned to that building. If we assume that 1.3 additional

[3] We do not have strong evidence to support the claim that a linear depreciation rate is realistic. The non-parametric curves are close to linear for several months, which suggests that productivity loss is reduced very little at the beginning and most catch-up occurs towards the end of the three-year period. However, we believe this pattern depends largely on the estimated functional form, which is noisy in nature. Hence, in this context, a simple linear depreciation may be a more conservative assumption.

guards are affected by the client leaving (which is the case for small buildings), the total value of productivity lost amounts to \$908.[4]

**Hiring costs.** We do not have information about the actual hiring costs of the firm before the policy change. Hiring costs encompass various expenses related to creating job openings, conducting searches, screening candidates, and providing training. 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. Our calculations are based on Manning (2011), who reports hiring costs from multiple studies (see Table 2 in that study). In the context of search-matching models, these hiring costs are typically expressed as a percentage of the total wage bill paid over the duration of the employment relation. The reported figures exhibit substantial heterogeneity, ranging from 1.5% to 11%.[5] 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.[6] 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 $C_\pi + 2.3C_p + C_h = \$5,069$ 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 on the cost incurred when rotating a guard. This cost refers to the monetary value associated with the increase in crime due to the loss of a guard's client-specific experience upon rotation. We employ an approach similar to the one used for estimating the loss of productivity due to poaching. The coefficient estimates from Table 2, $\hat{\beta}$, represents the monetary crime cost resulting from diminished client-specific experience. The reduction in client-specific experience is based on 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 adopt the same specification used in our poaching cost calculation: we disregard differences 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 \$205 in 2020 USD. This amount is significantly lower than the cost of losing the guard due to poaching.

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

[5] According to Manning (2011) this large heterogeneity is not surprising due to the difficulty in defining and measuring hiring costs, as well as the diverse countries and time periods covered by those studies

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

## A.4 Robustness Exercises Related to the Policy Change

In Column (2) of Table 5 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 2). In Column (3) of Table 5 we control for the interaction between guard fixed effects and an indicator of the two quarters after policy introduction. Results are slightly smaller for rotation, but they remain highly significant. For crime outcomes, the effects are larger than the baseline.[7]

Panel A of Figure A.5 reports the leads and lags estimates that include guard-specific linear trends. Borusyak et al. (2021) discuss a number of issues that could arise in dynamic Diff-in-Diff designs when the parallel trends assumption requires conditioning on time-varying covariates or individual trends. 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 Figure A.5, 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.[8]

Table A.6 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.

## A.5 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 $\kappa$, for the client's poaching effort not to be futile his offer $w$ must satisfy

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

[7]The effects of the policy on crime when guard-specific trends are included are shown in Columns (6) and (7) of Table 5 and the estimations absorbing the transition period are displayed in Columns (8) and (9). Regressions at the guard-date level are reported in Table A.5

[8]Figure A.6 reports the corresponding leads and lags estimates for crime outcomes.

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

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

Intuitively, the LHS of (.1) is the total payoff that the worker will get by accepting the client's offer. In contrast, the RHS of (.1) is the worker's payoff from staying at the service firm and getting the wage $\kappa$ 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:

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

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 (.2), we can rewrite condition (.3) as follows:

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

Because the RHS of (.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 (.4) holds if and only if $e \geq T(c)$. In particular, we have $0 < T(c) < +\infty$ provided that the following condition holds:

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

In other words, provided that (.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 $0 < T_L \equiv T(c_L) < T_H \equiv T(c_H)$.

### A.5.1 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 $\lambda$ 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 a low cost before the latter is rotated.

We now argue that taking the firm's strategy as given, the decision rule described above is optimal for the client provided $\lambda$ is small enough. The crucial idea is that, even though the firm's rotation scheme is fully anticipated, the client would refrain from poaching a worker prematurely, as the additional productivity gain at such stages would not justify the associated poaching costs.

We distinguish between two cases. First, consider the scenario $c_t = c_H$, where 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 $\lambda$ 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 $\lambda$ is small enough.

The second scenario involves $c_t = c_L$, where 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 $\lambda$ 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 $\lambda$ 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. $\square$

**Remark on the Generalizability of Proposition 1.** We envision three potential generalizations of the findings in Proposition 1. First, when $\lambda$ 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. We chose to focus on the case where $\lambda$ is small in the main text, because we do observe both worker rotation and poaching in the data. Second, workers may 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. Third, we could accommodate more general payoff functions of the players. For instance, to capture the possibility that the client might have access to complimentary services to partially offset the benefits that she loses due to leaving the outsourcing firm after poaching (e.g. buildings might obtain insurance from other sources even if they hire guards directly), we could introduce a term $\phi\underline{v}+(1-\phi)\underline{v}'$, where $\phi \in [0,1]$ and $\underline{v}' < \underline{v}$, into the client's payoff. Specifically, $\phi = 1$ corresponds to the case where the client receives the same benefit as when transacting with the outsourcing firm. In contrast, $\phi = 0$ represents a scenario in which there is no complementary service available at all (in which case the term $\underline{v}'$ can be very low). Incorporating such scalars will not alter the fundamental mechanisms of the model; it will mainly involve integrating them into the relevant cutoff conditions that appear in the proof of Proposition 1.

### A.5.2 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 $\square$

### A.5.3 Proof of Proposition 3

Let $\theta$ and $\theta'$ 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 (.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 poached with a lower probability 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. $\square$

### A.5.4 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 (.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 (.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 $\underline{c}$. □

### A.5.5 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 $\gamma$ 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 realization of $\gamma$ is privately known to the worker. Let $T(c, \gamma) \in \mathbb{N} \cup \{0, +\infty\}$ be the threshold value 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 $\gamma_O$ is sufficiently small but $\gamma_I$ is moderate.

**Proposition .1.** *If both $\lambda$ 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 he 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 $\lambda$ and $g$ are small enough, 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-$\gamma_I$ 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-$\gamma_O$ 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 described above is optimal for the client if both $\lambda$ 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 $\lambda$ 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 both $\lambda$ 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 the 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 $\lambda$ and $g$ are small enough.

Next, we take the behavioral 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 $\lambda$ 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 workers. 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 lose 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. □

### A.6 The Effect of Building-Specific Experience on Crime: An IV Estimation

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.[9]

Panel A of Figure A.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 A.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.

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 section A.2, we present empirical evidence consistent with this explanation.[10]

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,

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

[10]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.

several robustness checks help alleviate some concerns such as reverse causation. Nevertheless, we acknowledge that some potential issues remain 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 that 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 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 have provided evidence supporting this claim in Section A.2.[11] 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.[12]

The results reported in Column (1) of Table A.1 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 accumulate experience at a much higher rate per period, the first stage is strong with an F statistic (Kleibergen-Paap) equal to 90.

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

[11] 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.

[12] 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.

setting, this can't be tested directly and the findings should be 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 (Figure A.10).[13] 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 that we have previously discussed, it is important to recognize other 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. Likewise, 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 work schedules and off-days for guards, which restrict the flexibility in adjusting when type-II guards are assigned to cover type-I guards' shifts – we cannot definitively rule them out. Consequently, caution is advised when interpreting our results.

## A.7 Details of the Estimation of the Event Study

In this subsection, we provide additional information regarding the event study conducted around the rotation of guards. The purpose of this study is to provide further evidence on the relationship between building-specific experience and crime.[14] 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 referred as 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 referred as the control guards). We also exclude any control guard that rotates within the comparison window. This allows for a transparent control group and it alleviates concerns regarding dynamic effects as discussed below.

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

[14] 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.

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

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

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 $\beta$ 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 $\eta_i$ and $WinRot^j_{it}$, where the latter is a fixed effect identifying observations associated with 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.[15] 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 with 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 by Borusyak et al. (2021), when treatment events are sufficiently spaced out in time such that effects dissipate or stabilize, identification can be achieved under more standard assumptions. Third, we exclude from the control group those guards that rotate during the comparison window. Finally, in Columns (4)–(6) of Table A.2 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.

Results from the estimation of (.6) are reported in Panel A of Table A.2. Estimates in Column (1) indicate an increase in crime and the value of property lost after a guard is rotated. The

[15]To control for the possibility that guards' rotation coincides with periods of high (low) crime in the building, we also run (.6) controlling for neighborhood $\times$ month fixed effects. The results we obtained (not reported) are very similar to those of Table A.2.

estimated coefficients represent 20% 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 Table A.3, we decompose the effect of $RotGuard_{it} \times PostRot_{it}$ by interacting it with two dummies, indicating if the guard has high (above the 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.

## A.8 Poaching Risk: Machine Learning Estimation

To test whether the security firm rotates more of those guards with 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 the 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 a large household because in case of the absence of a guard, she can more easily find a trustable replacement for the working shift.[16]

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 make it difficult to detect empirically which characteristics are more important for the attractiveness of the guards to the buildings. Finally, interactions between characteristics may be critical predictors of poaching (e.g. having military training matters only for young guards).

[16]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.

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 dummy taking one if the guard was poached or solicited.[17] 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.[18] [19]

Figure A.11 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). Table A.11 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.[20]

### A.9 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 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

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

[18] 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 a higher weight to the 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 with predictions with imbalanced data.

[19] Accounting for many interactions and high-order non-linearities may also help capture 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.

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

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.

### A.9.1 Representativeness of Our Partner Firm

In this section, we argue that no single relevant attribute of our partner organization makes it unique. We have surveyed more than 20 security firms to confirm that not only our partner organization is representative of a large industry, but also that the mechanisms proposed and studied here are relevant to other organizations in this industry.[21]

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 stated that vertical poaching was a tangible problem, few of them reported a concrete number of poached guards. Among the surveyed security companies that reported a number, the interviews indicate that about 30% of the workers who ever worked in these companies were vertically poached at some point (an important caveat 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.*"

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.*"

Finally, one firm expressed the following viewpoint:

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

Our qualitative evidence also indicates that rotation is one of the main tools that firms use to avoid poaching. For instance, one firm explained:

[21] Figure A.7 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.

"*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.*"

When we inquired about the specific problems they were referring to, they responded:

"*Well, that the client steals (poaches) her.*"

Another firm also shared their perspective:

"*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 changed them from one building to another one. Sometimes, the guard protested but one needs to exert authority over these things.*"

### A.9.2 Prevalence and Prevention of Poaching

**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).[22] There are two types of forces that restrict workers from moving to other employers, 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), or 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.[23] Since buildings require a guard to be present during every working shift, the insufficient-volume-of-work argument does not apply either. Finally, poaching costs were low for buildings in the 1990s Colombia because mobility-restricting 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' demands. 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.

[22]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*.

[23]In fact, our qualitative evidence indicates that clients sometimes have access to more information than the service provider firm.

**What do firms do to avoid poaching?** Having used 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 that needs 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.

**Rotation vs. legal and/or managerial practices.** Our model sheds light on when we should expect service firms to be more or less likely to use job rotation ($T_H < +\infty$) versus other legal and/or managerial practices, such as pecuniary incentives, amenities, or non-poaching agreements ($T_H = +\infty$), to address poaching concerns. 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 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 ($\theta$ and $z(\cdot)$) will both incentivize clients to make more competitive poaching offers. Similarly, a stronger worker preference for becoming an in-house employee ($\gamma$) 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.

### A.9.3 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 deepened 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.[24] 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.[25]

**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.

[24]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).

[25]Our model extension in Section A.5.5 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.

**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, as the latter alternative 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.

### A.9.4 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.

Table A.9 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 who previously worked for that client, in contrast to a lobbyist with no prior experience with the client.

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## A.10 Additional Figures and Tables

**Figure A.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 A.2:** Timing of the Stage Game

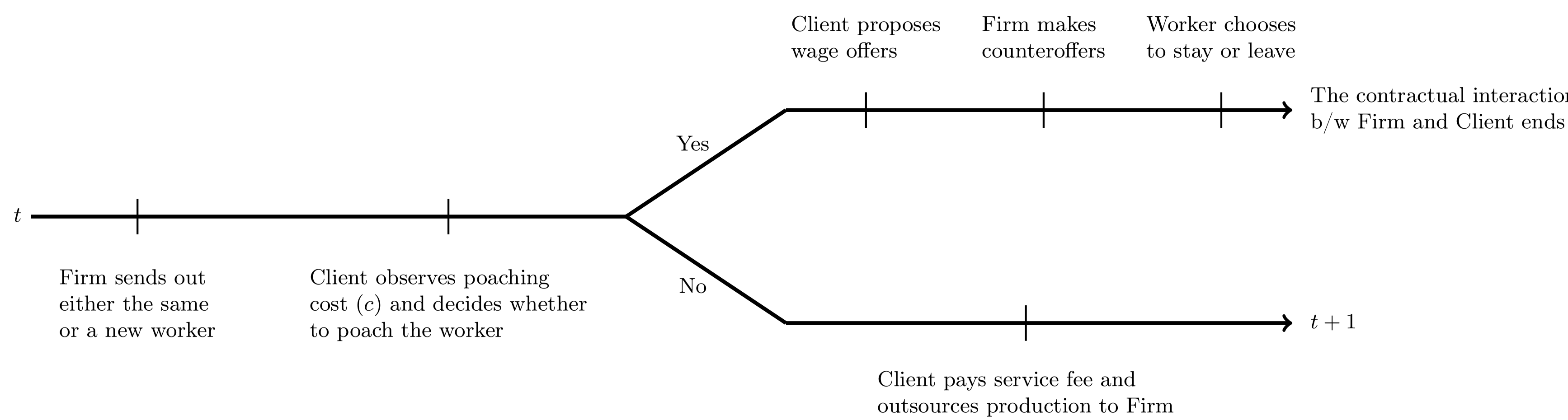
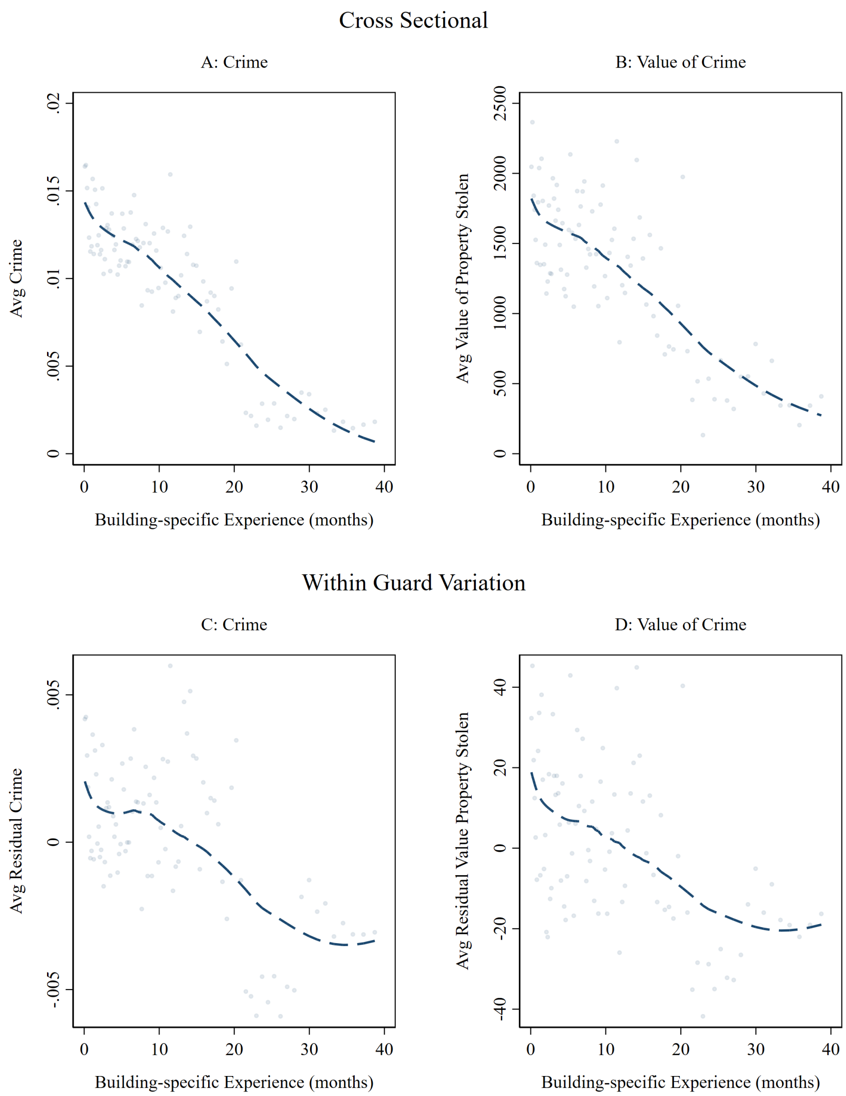
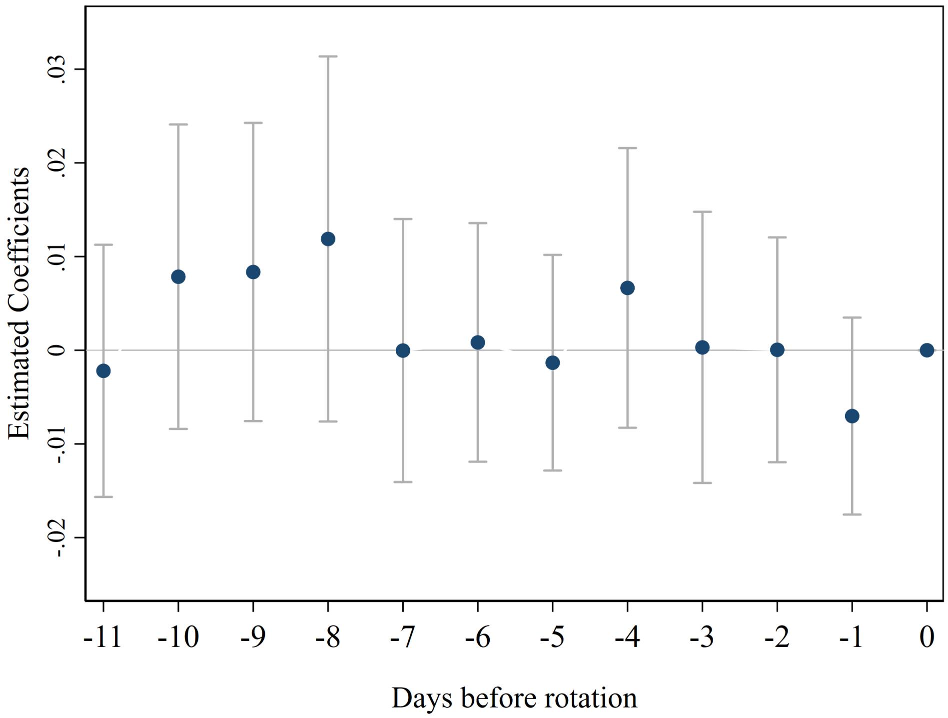


**Figure A.3:** Productivity and Client-Specific Experience (Non-Parametric Estimation)



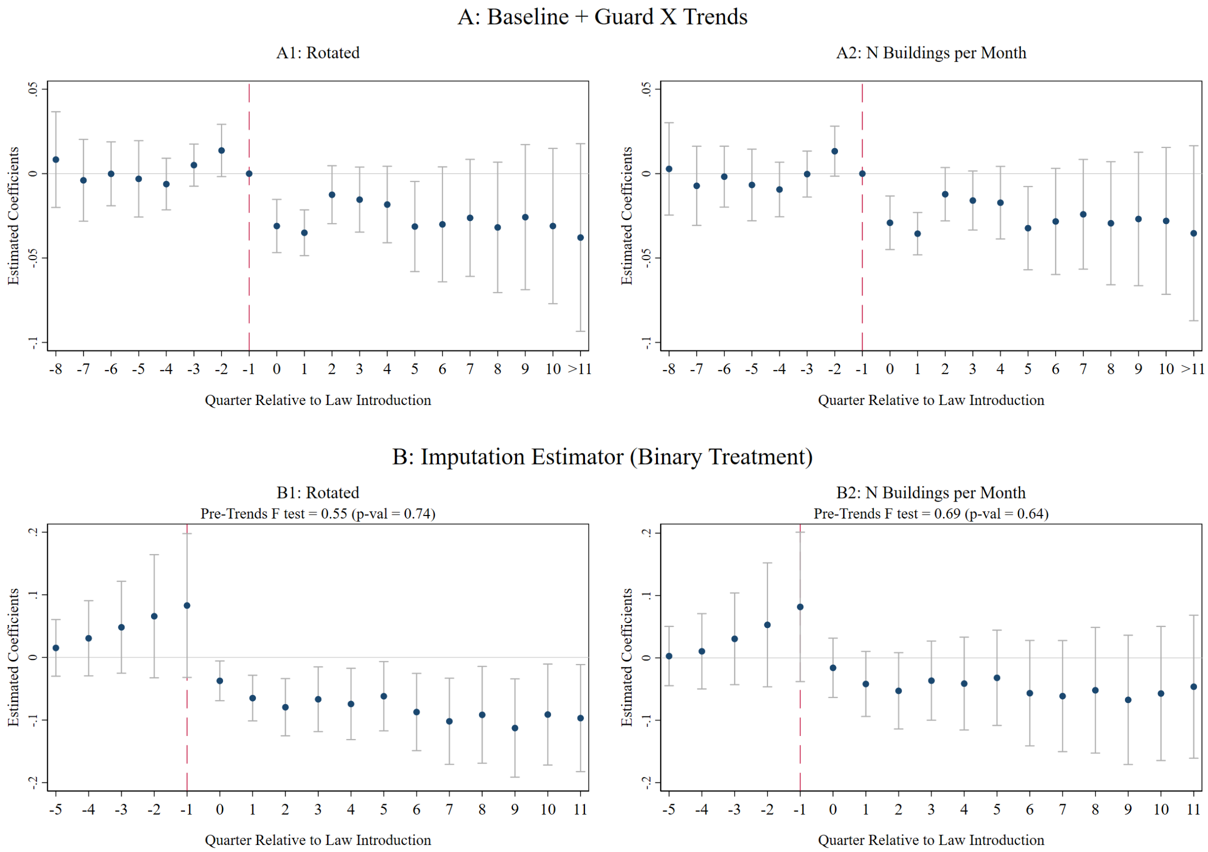

This figure displays the estimated relation between productivity variables (crime and the value of property loss due to crime) and the effective experience accumulated in the building. Observations are grouped and averaged across bins of effective building-specific experience (in months) where the bins 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 A.4:** 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. The sample is restricted to the period before the introduction of the decree. Standard errors are clustered at the guard level. $N = 208,620$.

**Figure A.5:** 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 of 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. $N = 17,119$.

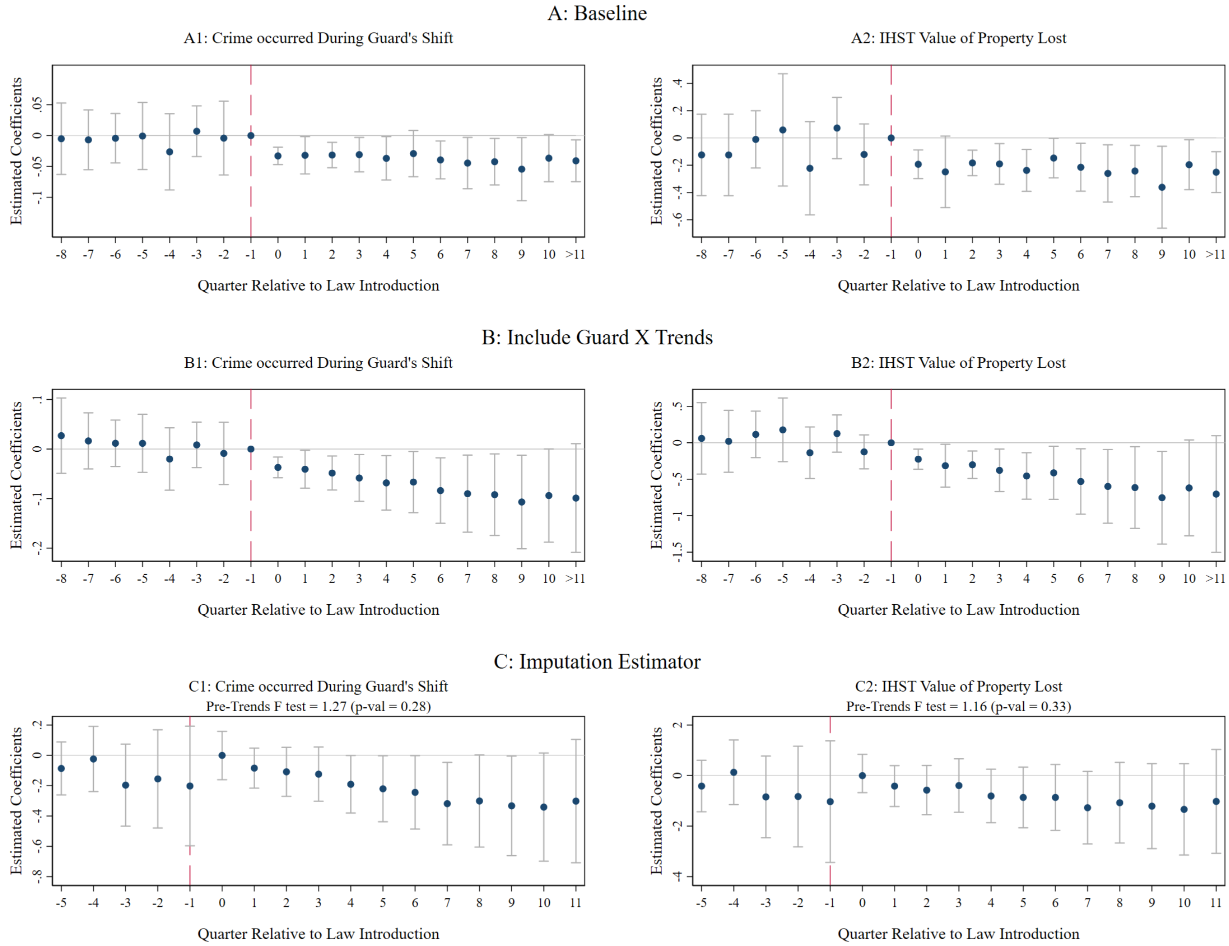
**Panel A** displays the estimated coefficients and the 95% confidence intervals of interaction between the estimated risk of being poached, with leads and lags indicators relative to the quarter when the degree was introduced. The omitted category is the interaction with the quarter previous to the 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. We exclude guards hired one month before or after the policy change. Standard errors are multi-way clustered at the guard-month level.

**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:

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

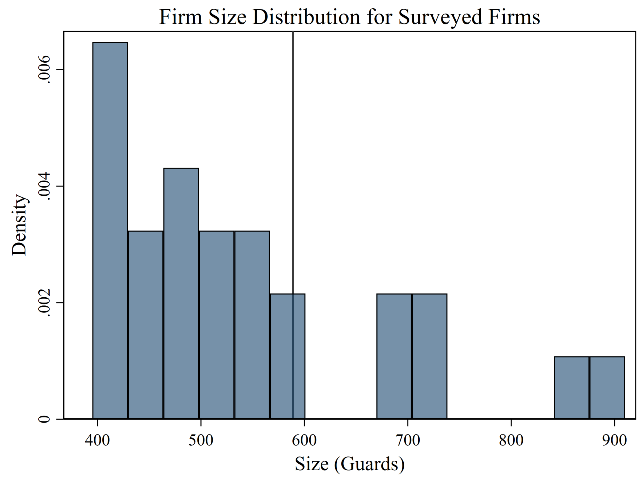
The specification is similar to the one used in Panel A and is defined over the same sample but the "treatment" is defined by the binary variable $HighRisk_i$ (which takes the value 1 if the guard is above the median of estimated poaching risk across all guards). Standard errors are clustered at the 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). 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 A.6:** Effects of the Decree 356 on Crime



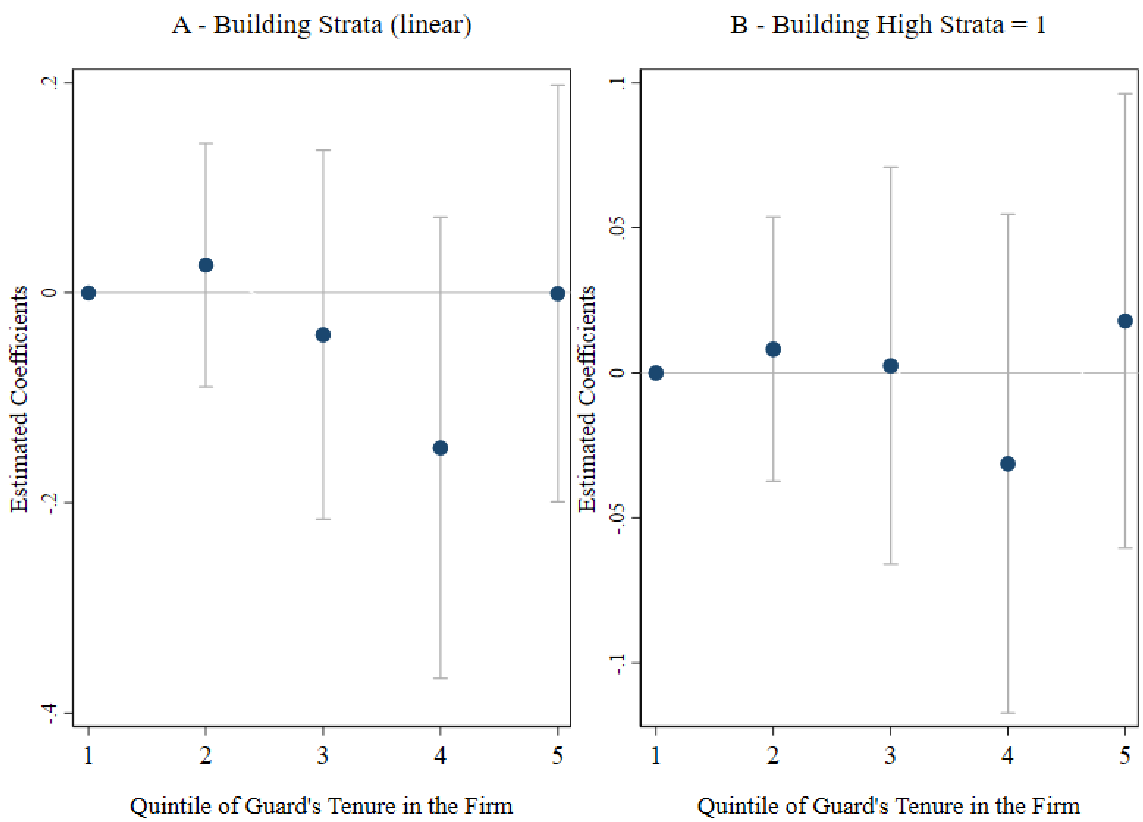

This figure shows the lead and lags effects of the Decree 356 on crime. The dependent variable in Panels A1, B1 and C1 is an indicator of whether a crime occurred during a shift where the guard was working. In Panels A2, B2 and C2, the dependent variable is the inverse-hyperbolic-sine transformed value of the property lost due to crime. $N = 17,119$. **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. We exclude guards hired one month before or after the policy change. 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.

**Figure A.7:** Firm Size Distribution



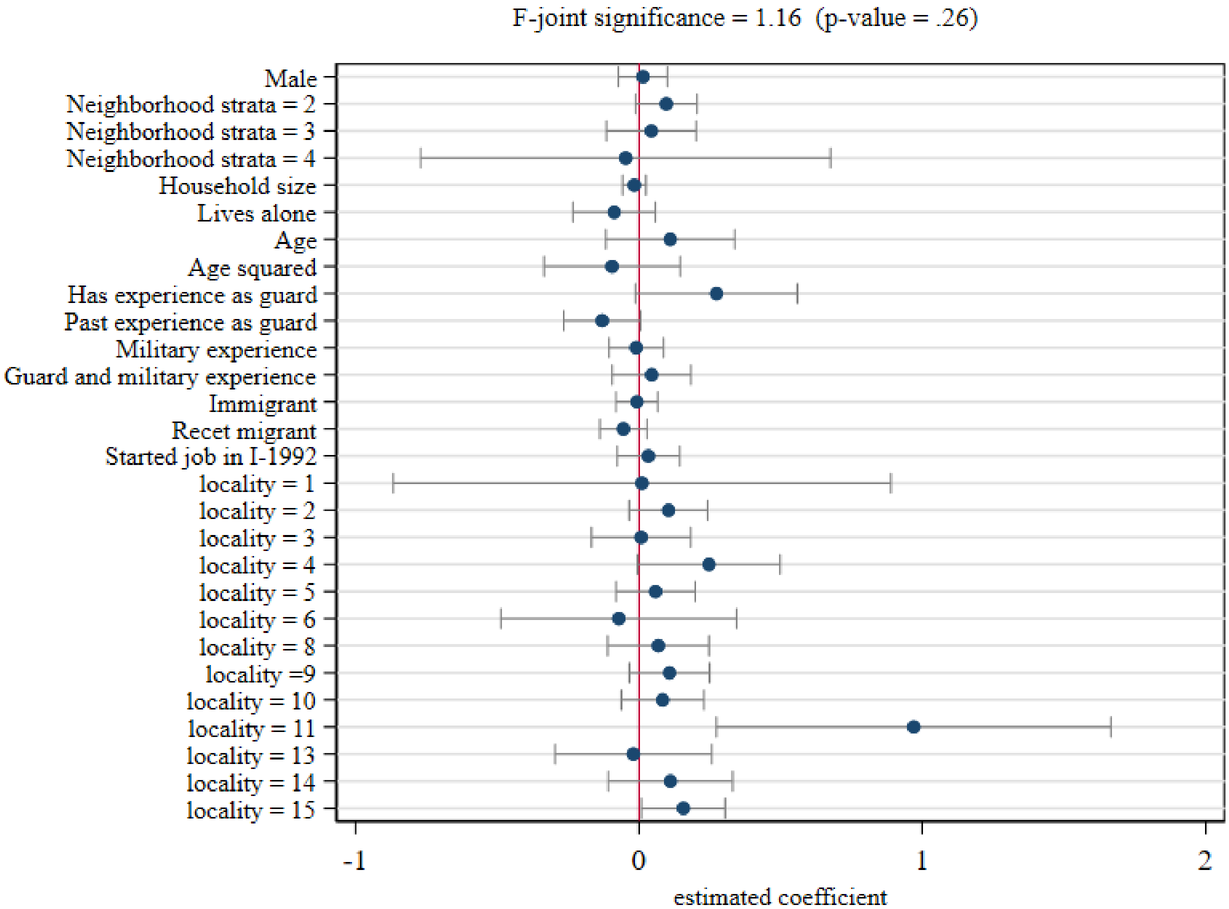

This figure shows the firms size distribution of the firms that replied to the survey. The red vertical line shows the firm size of our partner firm. $N = 23$ firms.

**Figure A.8:** Building Socio-economic Strata and Guard's Tenure



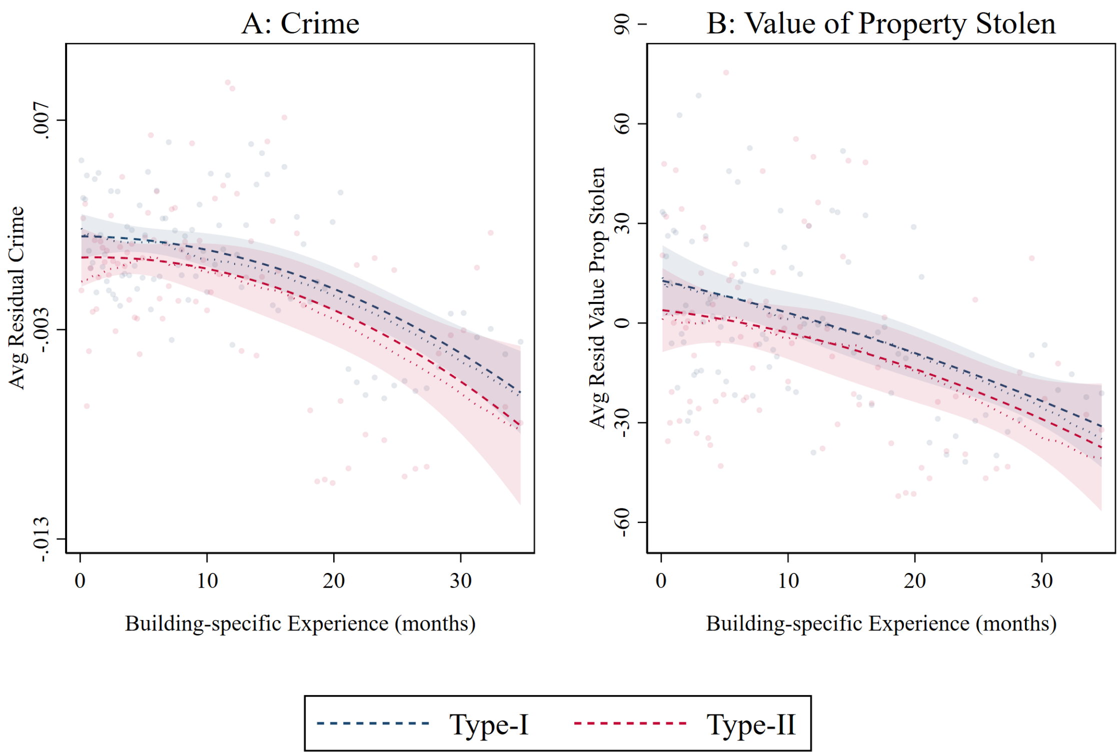

This figure displays the estimated coefficients and the 95% confidence intervals of regressions of the building's strata and indicators for the quantile of the guard's tenure within the firm. The regressions have controlled for both guard fixed effect and month fixed effect. In Panel A, the dependent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

**Figure A.9:** Balance Tests for Type-I vs. Type-II Allocation



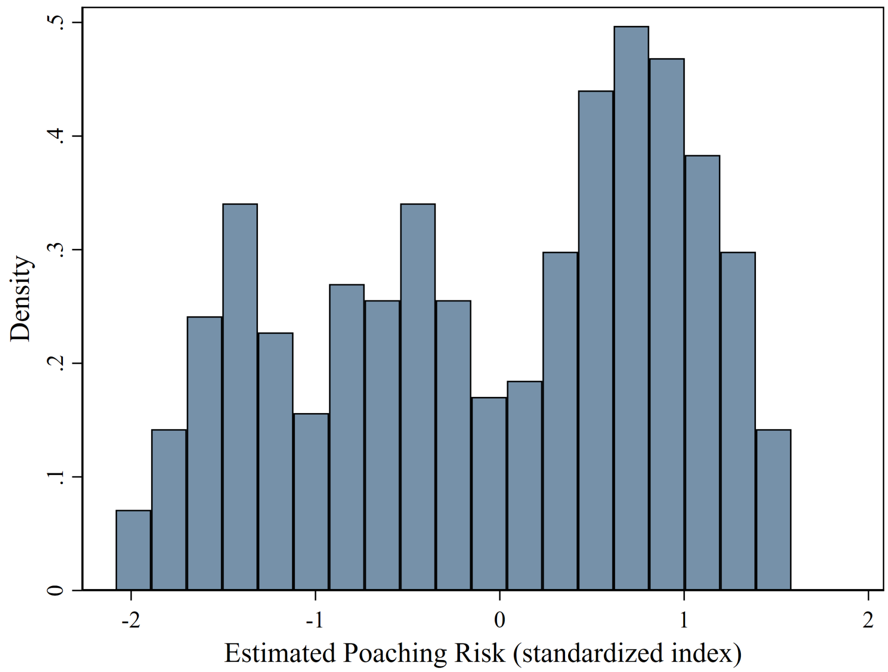

The figure displays the estimated coefficients and the 95% confidence intervals of a regression, where the dependent variable is an indicator of the guard being type-II and the explanatory variables are predetermined characteristics of the guard. Non-dummy variables are standardized. The figure also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

**Figure A.10:** 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 A.11:** 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 A.1: Productivity and Client-Specific Experience. Additional Robustness

| | (1)<br>IV | (2)<br>NonParm<br>TotExp | (3)<br>Exclude<br>1st Month | (4)<br>Exclude<br>1st Build |
|---|---|---|---|---|
| | **Panel A: Crime occurred During Guard's Shift** | | | |
| Exp in Building (months) | -.0027*** | -.0025*** | -.0029*** | -.0025*** |
| | (.00045) | (.00026) | (.00031) | (.00037) |
| N | 107,921 | 107,921 | 105,469 | 58,826 |
| R2 | .0039 | .1 | .1 | .09 |
| F first-stage (K-P) | 90 | - | - | - |
| Mean Depvar | .044 | .044 | .042 | .029 |
| | **Panel B: IHST Value of Property Lost in Crime** | | | |
| Exp in Building (months) | -.034*** | -.031*** | -.035*** | -.03*** |
| | (.0052) | (.0031) | (.0036) | (.0044) |
| N | 107,921 | 107,921 | 105,469 | 58,826 |
| R2 | .0039 | .1 | .1 | .088 |
| F first-stage (K-P) | 90 | - | - | - |
| Mean Depvar | .53 | .53 | .51 | .35 |
| 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 accumulated experience of the guard in the building (measured in in months). In Panel A, the dependent variable is an indicator of 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 months 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 A.2: 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*** | .014*** | .014*** | .022*** | .019*** | .019*** |
| | (.0046) | (.0047) | (.0048) | (.0067) | (.0068) | (.0067) |
| **Panel B:** | **IHST Value of Property Lost in Crime** | | | | | |
| Post Rotation X Rotating Guard | .15*** | .16*** | .16*** | .27*** | .23*** | .22*** |
| | (.055) | (.057) | (.057) | (.08) | (.081) | (.08) |
| N | 44,596 | 25,936 | 26,957 | 33,217 | 19,512 | 20,354 |
| Only Guards with >6 Months of Experience in the Building | NO | NO | NO | YES | YES | YES |

**N guards (col 1) = 447; N buildings (col 1) = 105; Mean Depvar A (col 1) = .066; Mean Depvar B (col 1) = .803. N guards (col 4) = 426; N buildings (col 4) = 103; Mean Depvar A (col 4) = .056; Mean Depvar B (col 4) = .686.** 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 of 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 A.3: 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*** |
| | (.0078) | (.0082) | (.0084) |
| Low Exp in Building | .013 | .0085 | .0073 |
| | (.017) | (.016) | (.017) |
| N | 32,949 | 19,245 | 20,086 |
| **Panel B:** | **IHST Value of Property Lost in Crime** | | |
| Post Rotation X Rotat Guard X: | | | |
| High Exp in Building | .36*** | .32*** | .34*** |
| | (.095) | (.099) | (.1) |
| Low Exp in Building | .14 | .087 | .071 |
| | (.2) | (.2) | (.2) |
| N | 32,949 | 19,245 | 20,086 |

**N guards = 416; N buildings = 103; Mean Depvar A = .056; Mean Depvar B = .683.** This table investigates the evolution of crime ocurring 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 labeled as a rotation episode. The regression sample is constructed by stacking the observations for 525 rotation episodes observed after July 1992. In Panel A, the dependent variable is an indicator of 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 A.4: Poaching and Client-Specific Experience. Cross Sectional Correlation**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | **Panel A: Guard Level Correlation** | | | | | | | |
| Avg Rotation | -.5*** | -.61*** | -.58*** | | | | | |
| | (.15) | (.18) | (.17) | | | | | |
| 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*** | .032*** | .031*** | .028*** | .019** |
| | (.0039) | (.005) | (.0053) | (.0052) | (.0062) | (.0074) | (.0079) | (.0075) |
| 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 = 453) 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 = 919) 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 = 96). 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 A.5: 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 $\times$ Poaching Risk | -.00076*** | -.0013* | -.00095*** | -.0015** | -.00074*** | -.0022*** |
| | (.0001) | (.00066) | (.00018) | (.0007) | (.00021) | (.00068) |
| N | 451,481 | 453,545 | 451,481 | 453,545 | 451,481 | 453,545 |
| Mean Depvar | .001 | .0096 | .001 | .0096 | .001 | .0096 |
| 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 = 360; N buildings = 116.** 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. We exclude guards hired one month before or after the policy change. Robust standard errors are clustered two-ways at the guard-month level and are shown in parenthesis.

**Table A.6: 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 $\times$ Poaching Risk | -.011*** | -.022* | -.056*** | -.0018** | -.013** | -.0092 |
| | (.0021) | (.012) | (.0054) | (.00075) | (.0052) | (.0078) |
| N | 17,119 | 19,419 | 19,419 | 19,419 | 19,419 | 19,419 |
| Mean Depvar | .027 | .026 | .026 | .026 | .026 | .026 |
| **Panel B:** | | | | **N Buildings per Month** | | |
| Post $\times$ Poaching Risk | -.011*** | -.02 | -.062*** | -6.8e-06 | -.025** | -.014 |
| | (.0036) | (.014) | (.008) | (.0018) | (.012) | (.011) |
| N | 17,119 | 19,419 | 19,419 | 19,419 | 19,419 | 19,419 |
| 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 = 356; N buildings = 116.** 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. We exclude guards hired one month before or after the policy change. Robust standard errors clustered two-ways at guard and month level are shown in parenthesis.

Table A.7: 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 | -.051*** | -.054*** | -.049*** | -.052*** |
| | (.0093) | (.0099) | (.0072) | (.0071) |
| N | 8,444 | 8,444 | 300 | 300 |
| R2 | .072 | .067 | .13 | .14 |
| Mean Depvar | .025 | 1 | .026 | 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 an 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=171). 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 controls 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. We exclude guards hired one month before or after the policy change. 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 A.8: Effect of the Policy on Crime Using High vs. Low Risk**

| | Monthly Obs | | 2x2 D-i-D | |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| **Dependent Variable** | **N of Crimes** | **IHST Value Prop Lost** | **N of Crimes** | **IHST Value Prop Lost** |
| Post $\times$ High Risk | -.13** | -.64** | -.082* | -.59** |
| | (.05) | (.28) | (.047) | (.28) |
| N | 8,371 | 8,371 | 298 | 298 |
| R2 | .27 | .27 | .43 | .51 |
| Mean Depvar | .24 | 1.7 | .3 | 2.1 |
| 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 crime using an alternative diff-in-diff specification where the control units are defined as those guards with an 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=170). 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 during 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 that 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 controls 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. We exclude guards hired one month before or after the policy change. 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 A.9: Client Experience and Poaching in the Lobbying Industry**

| **Dependent Variable** | Lobbyist Hired In-House |
|---|---|
| Previous Client Experience | 4.2*** |
| | (.17) |
| N | 1138444 |
| Client FE: | YES |
| Lobbyist FE: | YES |

**N clients = 990; N lobbyists = 1181.** 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 the 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 A.10: 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 | 0.91 | (0.56) | 0.90 | (0.57) |
| N Required Guards | 1.53 | (0.10) | 1.70 | (0.05) |
| Socioeconomic Strata Neighborhood | 1.51 | (0.10) | 0.95 | (0.52) |
| High Strata Neighborhood | 1.85 | (0.03) | 1.02 | (0.44) |
| City Area = South | 1.28 | (0.22) | 1.52 | (0.10) |
| City Area = Center | 1.05 | (0.42) | 1.33 | (0.19) |
| City Area = West | 0.40 | (0.98) | 0.47 | (0.96) |
| City Area = East | 0.90 | (0.58) | 0.55 | (0.92) |
| N | 1,424 | | 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 A.11: **Estimated Poaching Risk and Guard's Characteristics**

| | (1) Correlation with Baseline Chars | (2) Gini-based Importance |
|---|---|---|
| Male | 1.8*** | 0.184 |
| | (.058) | |
| Military Experience | .091* | 0.022 |
| | (.051) | |
| Neighborhood Strata | -.049 | 0.030 |
| | (.051) | |
| Household Size | .097*** | 0.101 |
| | (.034) | |
| Lives Alone | -.43*** | 0.014 |
| | (.088) | |
| Age | .022 | 0.133 |
| | (.029) | |
| Past Experience | -.23*** | 0.168 |
| | (.049) | |
| Had Experience as Guard | .31*** | 0.023 |
| | (.069) | |
| Immigrant | .14 | 0.020 |
| | (.084) | |
| 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 | 10.71 | |
| N | 389 | |
| R2 | .79 | |
| F | 87 | |

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 the dependent variable. The regression also includes 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.