INCENTIVE-BASED COMPENSATION IN POLICE FORCES

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ABSTRACT

Governments worldwide are increasingly applying management principles to policing, including performance management systems and financial incentives. We study whether eligibility for performance-based bonuses tied to crime reduction improves police outcomes. Using a quasi-experimental design and administrative data from Brazil, we evaluate an incentive-based compensation program that conditioned bonuses on reductions in violent deaths, vehicle robberies, and street robberies. Our results show that police districts eligible for bonuses experienced significant reductions in both targeted and non-targeted crimes, with stronger effects at the end of semester when incentives are steeper. While there is some evidence of gaming in the reclassification of street robberies, this behavior does not undermine the program's overall effectiveness. Overall, our findings highlight the potential of financial incentives to reshape police behavior and reduce crime.

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INTRODUCTION

Concerns about crime and violence remain pressing in both developed and developing countries (Bailey & Dammert, 2006). These concerns not only shape citizens' perceptions of insecurity and influence electoral behavior (Ponce, Somuano, & Velázquez López Velarde, 2022), but also drive governments to experiment with a wide range of policing reforms. Approaches vary considerably. Some focus on community relations and the behavior of officers, such as community-oriented policing and efforts to strengthen trust between police and citizens (Banerjee, Chattopadhyay, Duflo, Keniston, & Singh, 2021). Others concentrate on targeting resources to crime hot spots or high-risk individuals (Braga, Papachristos, & Hureau, 2014; Chalfin, LaForest, & Kaplan, 2021). Still others emphasize the adoption of new technologies such as body cameras and artificial intelligence (Bromberg, Charbonneau, & Smith, 2020; Kang, 2023), and mechanisms to curb misconduct and corruption (Buntaine, Bagabo, Bangerter, Bukuluki, & Daniels, 2024; Cabral & Lazzarini, 2015; Gillanders, Ouedraogo, Maïga, & Aja-Eke, 2024). Recent debates have even extended to proposals to defund police departments as a way to reduce brutality (Eaglin, 2020). Within this broad spectrum, an emerging line of research centers on results-based management initiatives, which seek to align police performance with measurable outcomes (Parfitt, Pantaleão, & Kopittke, 2026).

Among results-based reforms, incentive-based compensation systems have gained increasing attention in law enforcement. Existing studies, however, tend to examine narrow outputs of police activity—such as drug arrests (Baicker & Jacobson, 2007), firearms seizures (Barros Jr, Delalibera, Neto, & Rodrigues, 2022), or clearance rates (Federman, 2020). Much less is known about how incentive schemes affect police teams, how they shape officers' contributions to crime reduction, and what unintended consequences they may generate. To fill this gap, we analyze whether the expectation of future financial rewards alters police behavior at the team level. Specifically, we study whether police districts that remain eligible for performance-contingent bonuses exhibit lower levels of crime, indicating greater efforts in crime reduction.

Empirically, we examine the incentive-based compensation program implemented in the police forces of the Brazilian state of Rio de Janeiro in 2009. This program is inspired by the Compstat approach, which involves clarifying missions, organizing operational command geographically, using data to identify problems, and holding interactive meetings on crime strategies. The goal is to make managers responsible for implementing crime strategies and solutions in their areas that prevent crime. However, in the context we analyze, there is an extra layer: policymakers added an incentive by offering team-based financial bonuses to police officers in districts that successfully meet their semester targets. Specifically, every six months, each of Rio de Janeiro's 39 police districts (Áreas Integradas de Segurança Pública, or AISPs) is assigned maximum allowable counts for three priority crimes: violent deaths, vehicle robberies, and street robberies. Districts that remain below these thresholds throughout the semester qualify for the bonus. The bonuses offered are appealing and are distributed evenly among all police officers in the district who met the target, regardless of wage or rank. During each incentive cycle, officers could monitor cumulative crime against the semester-specific targets and adjust their expectations of receiving the reward. We use monthly crime data from 2009 to 2015 to examine whether the expectation of receiving performance bonuses affects crime outcomes, reflecting a change in their performance.

Our identification strategy exploits within-semester variation in eligibility status to test whether remaining on track to qualify for the bonus is associated with lower crime. Specifically, our baseline two-way fixed effects (TWFE) model leverages both cross- and within-district variation. We define treatment as periods when a district remains below the cumulative threshold across all three crime categories, meaning the incentive system is binding. Finally, we exploit the timing of the program to examine whether incentive effects intensify toward the end of the semester, when the bonus deadline approaches.

We find that police districts remaining below cumulative targets in a given month are more likely to achieve better outcomes in the following month than districts that are above their thresholds and thus face little prospect of receiving bonuses. The effects of incentives are stronger as the semester progresses and are particularly pronounced in the final month of the semester. This dynamic underscores how eligibility for financial rewards can shape behavior and performance in public organizations.

Beyond our main findings, we also explore how the program's design shaped officer behavior and whether incentives generated distortions. First, we show that crime does not increase when bonuses are clearly unattainable, suggesting that police officers does not significantly change their behavior when they do not have expectation to receive the bonus. Second, we test an alternative definition of eligibility and find results very similar to the main specification. Third, we investigate potential gaming and spillovers. While there is some evidence of reclassification to street thefts, we find no systematic manipulation in other categories. Instead, reductions in non-targeted crimes, such as attempted murders, point to behavioral changes and modest positive spillovers.

We conduct a series of robustness exercises to validate our findings. First, we provide a test for parallel-trend assumption considering our reverse difference in difference setup. We show that reaching the bonus in one semester is not associated with crime reductions in the following months of a new cycle. Second, placebo regressions using pre-program data reveal no systematic effects, ruling out seasonality or autocorrelation as alternative explanations. Third, spatial analyses confirm that crime reductions in eligible districts are not offset by displacement to neighboring areas. Finally, our results remain virtually unchanged when re-estimated with a Poisson model, reinforcing that they are not sensitive to functional form.

By examining how police officers respond to incentive-based compensation programs, this study contributes to the literature on management-oriented interventions in police forces (Chalfin et al., 2021; Gillooly, 2022; Mutahi, Micheni, & Lake, 2023; Parfitt et al., 2026). By showing how the expectation of receiving financial rewards influences affects crime, reflecting both positive and negative responses from police officers, we can provide insights for developing policies to address perceived insecurity, a major challenge facing both developed and developing countries, thereby adding to existing research on financial incentives in police forces (Baicker & Jacobson, 2007; Barros Jr et al., 2022; Dincer & Johnston, 2023; Federman, 2020; Gillanders et al., 2024; Mas, 2006). Furthermore, by offering evidence based on administrative data rather than perceptual measures, this study engages in a dialogue with research advocating for a more thorough exploration of both improved outcomes and incentive distortions within pay-for-performance (PFP) programs, and more broadly with the current literature on PFP programs in public organizations (Gerrish, 2016; Heinrich & Marschke, 2010; Hood, 2006; Pham, Nguyen, & Springer, 2021).

THEORY BACKGROUND: FINANCIAL INCENTIVES IN POLICE FORCES

Structural and disruptive reforms in police forces, such as defunding police or merging police forces, are particularly difficult because these organizations leverage their control of coercion, thus constraining the policy options available to politicians and raising the threshold for reforms (González, 2020). As a response to the rising in crime records and to improve the relationships with the communities they serve, police forces around the globe has been promoting myriad of management-oriented interventions by introducing practices such as broken windows policing, problem-oriented policing, pulling levers policing, third-party policing, hot spots policing, and evidence-based policing, among others (Braga & Weisburd, 2006; Chalfin et al., 2021). Along these lines, the Compstat program developed by the New York Police department in 1994, is a stylized example of the use of managerial attributes in police forces by emphasizing aspects related to mission clarification, internal accountability, geographic organization of operational command, organizational flexibility, data driven problem identification and assessment and innovative problem-solving tactics, and external information exchange (Weisburd, Mastrofski, McNally, Greenspan, & Willis, 2003).

One of the major managerial interventions to improve performance—the use of financial incentives—remains relatively uncommon in police forces, even though it is widely applied in sectors such as education and health (Elacqua, Hincapie, Hincapie, & Montalva, 2022; Fryer, 2013; Li, 2022; Mullen, Frank, & Rosenthal, 2010; Pham et al., 2021). Yet evidence indicates that monetary incentives can shape police behavior. Mas (2006), for instance, showed that officers reduce their effort when payments fall below expected benchmarks. Baicker and Jacobson (2007) found that under U.S. forfeiture laws, police agencies that retain a substantial share of seized assets increase drug arrests, often at the expense of other crime-fighting activities. Barros Jr et al. (2022) demonstrate that monetary incentives are most effective in tasks where officers can easily translate effort into measurable outputs, such as firearm seizures.

Some factors may explain why incentive-based compensation (or PFP) programs are not widespread in police forces. A key obstacle to aligning the interests of principals and agents in police forces, which is vital for the success of PFP programs, is the risk of system gaming. Accordingly, financial incentives contingent on performance are effective when agents cannot manipulate performance metrics, when incentives do not lead to undesired comparison effects, when effort is clearly linked to observable outcomes, and when agents are unable to gauge their efforts in the present to obtain pecuniary rewards in the future (Heinrich, 2007; Hölmstrom, 1979; Zenger & Marshall, 2000). Weak accountability and inadequate control systems in public services, including policing, raise concerns about the effectiveness of incentive-based programs in achieving intended outcomes (Frey, Homberg, & Osterloh, 2013).

As in other public services, the outcomes of policing activities stem from collective efforts of individuals grouped in teams. Yet, the identification of individual contributions to the observed performance is challenging and can lead to strategic effort manipulation by team members, which may have undesirable consequences in collective PFP programs, such as output reduction, miscoordination, and incentive distortions like gaming and multitask (Frey et al., 2013; Heinrich & Marschke, 2010; Hood, 2006; Koning & Heinrich, 2013; Park & Berry, 2014). These issues are even more complex in public organizations facing low-powered incentives and multi-principal tensions (Dixit, 2002). Unsurprisingly, the results of incentive-based programs in public services are mixed, showing positive (Pham et al., 2021), negative (Park and Berry, 2014), and no conclusive results (Park, Park, & Barry, 2022).

The role of being eligible to obtain financial bonuses

Despite the controversy, financial incentives can produce positive performance effects when certain conditions are fulfilled. We argue that agents, such as police officers, are more likely to put in extra effort and turn it into better outcomes when they believe they are eligible for rewards and when the chances of receiving such rewards seem credible. Additionally, we suggest that effort levels tend to rise toward the end of the incentive period, if agents remain eligible to reap financial rewards, but decrease once agents no longer meet the eligibility requirements.

Langbein (2010) proposes that the unintended consequences of collective PFP programs can be mitigated in the presence of alignment between incentives and motivational factors. Motivational approaches, such as the expectancy theory (Vroom, 1964), have become increasingly relevant in explaining incentive-based programs dynamics, as they highlight the role of individual motivation and attitudes toward financial rewards (Kanfer, Frese, & Johnson, 2017; Kelley, Heneman III, & Milanowski, 2002) Expectancy theory posits that individuals form cognitive linkages between actions and outcomes, which influence the level of effort devoted to a task and, in turn, observed performance (Van Eerde & Thierry, 1996). In this framework, efforts are shaped by both the feasibility and the attractiveness of expected outcomes (Hackman & Porter, 1968). Furthermore, well-designed incentives can promote teamwork, which is a crucial aspect in programs where agents are grouped into teams and assessed based on collective goals (Kelley et al., 2002; Schay, 1993).

Although expectancy theory is usually applied to analyze individual PFP programs and cannot be directly disentangled or tested with the group-level administrative data available in this work, it remains particularly relevant for explaining the relationship between motivational forces, incentives, and observed performance in collective PFP settings (Nyberg, Maltarich, Abdulsalam, Essman, & Cragun, 2018). This is especially true when a clear line of sight either fosters or hinders the performance-enhancing efforts of team members (Kanfer et al., 2017). Evidence from collective PFP in education indicates that financial incentives can produce socially relevant outcomes when agents are eligible for financial rewards—that is, when they expect to receive bonuses—with no significant differences in results between group-based incentives and individual rank-ordered incentives (Pham et al., 2021).

Expectancy theory includes three connected parts—expectancy, instrumentality, and valence—that work together. First, expectancy is the chance that effort will lead to a certain level of performance. Second, instrumentality is the chance that a certain second-level outcome (e.g., financial incentives) will occur if the expected results are achieved. Third, valence concerns how attractive and satisfying the goals are perceived to be. A positive (negative) valence indicates a high (low) appeal of achieving the expected outcomes (Kanfer et al., 2017; Van Eerde & Thierry, 1996). The strength of motivational forces and the likelihood of individuals exerting effort towards specific outcomes are determined by the interplay among these components (Cadsby, Song, & Tapon, 2007). For example, the strength of financial incentives, as a tool, can boost expectancy by motivating people to put in more effort, especially if they expect to share in the results and see the financial payoff as a worthwhile reward for their work.

The connection between expectancy theory and how eligibility criteria influence behavior in incentive competitions within public services, including policing, is clear. When agents like police officers see themselves as eligible for bonuses, their expectancy increases because they believe that putting in extra effort will improve their chances of winning. Self-efficacy beliefs can affect motivation (Wood & Bandura, 1989), encouraging individuals to work harder if they feel empowered and believe they have more autonomy and control over the results they produce (Fernandez & Moldogaziev, 2013). Performance feedback related to specific goals can enhance self-regulation and strengthen perceived self-efficacy among team members, thereby positively impacting how behavioral intentions translate into performance (Kanfer et al., 2017; Wood & Bandura, 1989).

The effectiveness of police officers can be enhanced when their performance is clearly connected to the chance of receiving a reward. In this context, research from both motivation scholars (Karau & Williams, 1993) and incentive scholars (Kandel & Lazear, 1992) shows that team members tend to put in less effort when they cannot see how their individual contributions affect group outcomes. Therefore, access to performance information is essential for agents to stay aware of their eligibility and to meet their goals in incentive-based compensation programs, as demonstrated by Kelley et al. (2002) in their analysis of PFP programs in education. When eligibility is in place, instrumentality increases, leading to greater effort.

Valence emphasizes the attractiveness of variable pay and influences whether police officers see their effort as worthwhile. In this context, ensuring appropriate incentive strength (Zenger & Marshall, 2000) is essential for motivating effort in team settings and encouraging peer pressure to prevent issues like free riding. Consistent with this, Hertel, Konradt, and

Orlikowski (2004) found that when an organization sets clear goals, team members are more motivated to put in greater effort and achieve better results in collective PFP programs.

Overall, when agents see themselves as eligible, the expectancy–instrumentality–valence chain is strengthened, leading to increased effort—especially when agents receive ongoing performance feedback and believe that monetary rewards are reachable. This process helps police officers assess their efforts and enhances coordination by clarifying how close they are to reaching the expected outcome targets.

Formally:

H1: The expectation of receiving future monetary rewards through incentive-based compensation programs leads eligible police officers to increase their efforts to reduce crime.

While obtaining ongoing performance information is important, the timing of performance feedback in PFP programs is also crucial (Kanfer et al., 2017). Providing agents with timely performance feedback on the results of their activities can increase or decrease coordinated efforts over time (Holmström, 2017).

We argue that police officers under incentive-based compensation programs may boost their coordination efforts as they near the end of an incentive cycle, if they believe it is possible to meet the performance targets and find the financial rewards appealing. Informed team members continuously assess the feasibility of receiving bonuses and weigh the costs and benefits of exerting additional effort. As the incentive period unfolds, officers become increasingly aware of whether their efforts are likely to translate into improved performance and eligibility for rewards. Conversely, when targets appear overly demanding or unattainable, the program's motivational effect may weaken, leading officers to scale back their efforts. This dynamic is particularly pronounced toward the end of the cycle, when a perceived "lack of feasibility" is greatest, thereby reducing the effectiveness of financial incentive programs in police forces. In other words, in time-bound incentive programs with periodic performance feedback, participants are expected to intensify their efforts as the program approaches its conclusion, provided they remain eligible for rewards. For instance, in a six-month program eligible officers would be expected to exert additional efforts during the final two months. Formally:

H2: The effect of incentive-based compensation programs on crime outcomes is expected to intensify as the incentive cycle approaches its final stage.

INSTITUTIONAL SETTING

Police agencies provide an instructive context for studying managerial reforms through pay-for-performance (PFP) programs, as many have adopted performance management systems over recent decades (Baicker & Jacobson, 2007; Parfitt et al., 2026). We focus on the state of Rio de Janeiro, Brazil—home to 17 million inhabitants—where state governments oversee two distinct police institutions: the Military Police, responsible for patrolling and making arrests in flagrante delicto, and the Civil Police, responsible for crime reporting, investigation, and judicial support. Both agencies answer to the Secretary of Public Security but maintain separate leadership structures, training, and career systems. Effective crime control requires cooperation between them, yet such collaboration has historically been limited, as integration is often perceived as a threat to autonomy and organizational culture.

In 2009, the Secretariat of Public Security introduced the Integrated Target System (Sistema Integrado de Metas, SIM), an incentive-based compensation program that included both forces. Inspired by New York's CompStat model and earlier Brazilian initiatives, SIM was distinctive for explicitly incorporating financial incentives. A key first step was aligning the territorial organization of the two agencies: each of the 39 Military Police districts was matched with civil police precincts to form Integrated Areas of Public Security (Áreas Integradas de Segurança Pública, AISPs).

To foster collaboration and accountability, SIM instituted regular meetings. District-level meetings occurred monthly to address operational issues, while semiannual state-level meetings brought together senior officials, including the governor. These meetings provided a platform for coordination across agencies, performance feedback, and collective problem-solving. Complementing these managerial practices, SIM offered sizable bonus payments to all officers in districts that met their performance targets, creating strong financial incentives for collaboration.

The program focused on three priority crimes: violent deaths, vehicle robberies, and street robberies. Each semester, the Security Secretariat establishes district-specific crime limits by applying a standard percentage reduction to the previous year's crime numbers for each district.[1] Districts had to remain at or below these thresholds across the semester to qualify for the bonus. While compliance was assessed on a semester basis, monthly benchmarks were published to allow monitoring of progress. Officers had limited input into the target-setting process and did not fully understand the methodology, but they closely tracked their cumulative crime records and commonly referred to themselves as being "on" (eligible for bonuses) or "off" target (not eligible). The financial stakes were considerable. Semester bonuses ranged from US$500 to US$4,000 per officer regardless his rank, at a time when a low-ranking Military Police officer earned around US$1,000 per month. This made SIM one of the most high-powered incentive systems ever implemented in Brazilian policing.

Results-based management systems have been adopted in 11 Brazilian states since 2005 (Ricardo, Martins, Ribeiro, & Silva, 2023), beginning with IGESP in Minas Gerais (Soares & Viveiros, 2017). These initiatives typically emphasized coordination, data-driven deployment,

[1] After several years of implementation, the Secretariat revised the target-setting process by dividing districts into quartiles and assigning progressively higher percentage reductions to those in higher-crime quartiles. The objective was to ensure that the incentive remained binding even for districts with relatively low crime rates. Despite this modification, district officers continued to receive their targets in the same format: crime count caps that had to be respected in order to qualify for the bonus.

and jurisdictional redesign. Yet only three states—Rio de Janeiro, São Paulo, and Paraíba—introduced financial bonuses tied to crime reduction. Among them, Rio's program was the most ambitious: bonuses became a central pillar of the state's security strategy for at least seven years, and the program was neither interrupted nor substantially altered until 2015. This continuity makes Rio de Janeiro a unique case for evaluating the effects of incentive-based compensation in policing.

EMPIRICAL STRATEGY

We evaluate the effects of the incentive program on crime outcomes by exploiting monthly variation in whether districts remained eligible to receive bonuses. The core idea is that officers continuously monitor their cumulative crime counts against semester targets. When crime counts remain below the allowable thresholds, the bonus remains attainable, and incentives to exert effort are binding. Our identification strategy leverages this dynamic variation in *eligibility*.

Formally, at the start of each semester the Secretariat of Security announces district-level targets for violent deaths, vehicle robberies, and street robberies. Each month t, officers compare cumulative crime counts up to month $t-1$ with the semester target up to that month ($Y_{i,k,s}^{Target}$). We define:

$$On\_Target_{i,k,t} = 1\left(\sum_{j=1}^{t-1} Y_{i,k,j}^{Registered} \leq Y_{i,k,s}^{Target}\right), \tag{1}$$

On_Target_{ikt} indicates whether district i has remained under the threshold for crime k $\forall$ $k \in$ {violent death, vehicle robbery, street robbery}. The treatment variable $Elegible_{i,t,s}$ is a binary variable that takes the value of 1 if $On\_target_{i,k,t} = 1$ for all the three crime types k targeted by PFP up to month *t-1*. That is, $Elegible_{i,t,s}$ takes value 1 if district i has remained on target across all three indicators in all previous months within the same semester, and 0 otherwise. Thus, $Elegible_{i,t,s}$ captures periods in which officers have a realistic chance of receiving bonuses, which should activate effort responses. Importantly, eligibility is re-evaluated each month, generating plausibly exogenous within-district variation in incentives as districts fall on or off track.

When the semester concludes, a new incentive cycle commences, thus reinstating our main independent variable. As hypothesized in H1, we expect a negative association between $Elegible_{i,t,s}$ and the three crime variables.

We estimate the following panel model:

$$Y_{i,t,s} = \beta Eligible_{i,t,s} + X_{i,t,s}\gamma + \sum_{c=1}^{C} \delta_c D_{c,i,t,s} + \phi_i + \sigma_t + \eta_y + \varepsilon_{i,t,s}, \quad (2)$$

Where $Y_{i,t,s}$ represents crime outcomes in district *i* and month *t* of semester *s*. The coefficient of interest, β, captures the effect of being eligible to receive a bonus. All regressions include district fixed effects ϕ_i to absorb time-invariant differences in baseline crime levels, month of the semester fixed effects (σ_t) and year fixed effects (η_y) to control for seasonality.

We also include commander fixed effects[2] ($D_{c,i,t,s}$) following evidence that leadership matters for policing outcomes (Masal & Vogel, 2016). This ensures our estimates are not confounded by unobserved heterogeneity in commanders' ability or motivation. In addition, $X_{i,t,s}$ controls for the number of civil police precincts operating within each military police district, which proxies for coordination difficulty between the two branches of the police.

Hypothesis 2 predicts stronger incentive effects as the bonus deadline approaches, particularly in the sixth (final) month of each semester, or incentive period. To assess how incentives evolve within the semester, we interact eligibility with the month of the incentive cycle:

[2] The commander fixed effect refers to the Military Police commander responsible for the battalion area corresponding to each AISP, which is our treatment unit. During the study period, commanders typically served for about 10 months. Each AISP/battalion area also includes civil police precincts, headed by police chiefs *(delegados)* who oversee crime registration and investigation. Our regressions control for the number of Civil Police precincts within each AISP.

$$Y_{i,t,s} = \sum_{m=1}^{6} \beta_m Eligible_{i,t,m,s} + X_{i,t,s}\gamma + \sum_{c=1}^{C} \delta_c D_{c,i,t,s} + \phi_i + \sigma_t + \eta_y + \varepsilon_{i,t,s} \quad (3)$$

We further exploit the discontinuity between the final month of a semester and the first month of the subsequent semester. Around this cut-off, the influence of financial incentives on effort may decrease abruptly at the onset of a new semester when a new incentive period commences. To capture this distinction, we restrict our analysis to the 6th month of each semester s and the 1st month of the following semester *s+1*.

$$Y_{i,t,s} = \beta_1 Elegible_{i,6,s} * 1(t = 6, s) + \beta_2 Elegible_{i,1,s+1} * 1(t = 1, s + 1) + X_{i,t,s}\gamma$$
$$+ \sum_{c=1}^{C} \delta_c D_{c,i,t,s} + \phi_i + \sigma_t + \eta_y + \varepsilon_{i,t,s} \quad (4)$$

In this regression, $Elegible_{i,6,s}$ is equal to 1 if district *i* has remained on target across all three indicators until the beginning of month 6 of semester. Here, β_1 determines whether the impact of eligibility is differential at month 6, when the incentive tournament concludes and bonuses remain attainable for eligible districts. This design helps separate genuine incentive effects from underlying crime autocorrelation: if reductions were just due to trends, we would not expect a sharp difference at this boundary. This specification is a stacked difference and difference design, so it is not subject to the potential biases in staggered treatment setups. Therefore, finding $\beta_1 < 0$ provides strong evidence that incentives, rather than crime trends, drive the observed effects.

In addition to our main specifications, we conduct several exercises to assess the plausibility of our assumptions and the robustness of our results to alternative designs and estimators as explained in the last section.

Our analysis draws on administrative data from the Institute of Public Security (ISP) in Rio de Janeiro, covering all police districts between 2009 and 2015. The resulting panel includes 3,024 district-month observations. Table 1 presents summary statistics for all variables.

INSERT TABLE 1 ABOUT HERE

RESULTS

Main effect: Eligibility to receive bonuses and future performance in PFP

Table 2 displays our main results. For each dependent variable, we use three different specifications. The first specification (Columns 1, 4, and 7) includes our main explanatory variable ($Eligible_{i,t,s}$) and all controls. The second specification (Columns 2, 5, and 8) presents regression results based on Equation 3, which examines variation within the semester, while the third specification (Columns 3, 6, and 9) is based on Equation 4, measuring the impact of bonus eligibility at the semester cutoff. All models use robust standard errors clustered at the police district level.

Consistent with H1, the coefficients for the *Eligible* variable in columns 1, 4, and 7 indicate a negative relationship between the expectation of receiving future rewards from the incentive-based compensation program and target crime indicators. Being eligible for bonuses is associated with a reduction of the of 6.9 percent in violent deaths, to a 6.3 percent decrease in vehicle robberies, and a 8.5 percent decrease in street robberies, when compared to the sample means.

In line with H2, our results show the strength of the expectation of receiving bonuses as the semester nears its end. The decline in vehicle and street robberies is more noticeable in the sixth month of the semester, the month when the incentive cycle ends. During this period, having completed the first five months under the target is associated with a 6.5 percent decrease in both vehicle robberies and street robberies.[3]

These findings are supported by the fourth exercise examining the semester cut-off. The coefficient of $Elegible_{i,6,s} * 1(t = 6, s)$ is negative and significant for vehicle robbery and street robbery (Columns 6 and 9). The results suggest that the incentive program leads to an 8.1% reduction in vehicle robberies and an 7.1% reduction in street robberies during the last month of the semester compared to being eligible in the following month. This exercise accounts for autocorrelation and provides strong evidence that the program effectively contributed to decreasing vehicle and street robberies.

INSERT TABLE 2 ABOUT HERE

The effects of eligibility for financial bonuses on violent deaths are weaker, indicating that police officers may have limited influence over this type of crime. A large portion of intentional violent deaths in Rio de Janeiro results from disputes between drug gangs, over which the police generally have limited control. To better understand this key part of the incentive-based program, we conducted further analysis. We divided this indicator into homicides and police killings, i.e., deaths caused by the police. The latter was included in the count of violent deaths starting in January 2011 to encourage a reduction in police brutality, which is especially severe in Rio de Janeiro (Monteiro, Fagundes, & Guerra, 2020).

Table 3 presents disaggregated results for violence deaths by showing the effect of eligibility for bonuses on homicides (violent deaths not caused by police) and policy killings using the same procedures as in Table 2. Column 1 indicates that the expectation of receiving

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[3] It is important to note that the observed month-6 effects should not be interpreted as a sudden surge of effort only in the final month, but rather as the culmination of cumulative efforts throughout the semester, with incentives becoming most salient as the bonus deadline approaches.

the bonus is associated with a 5.3 percent reduction in homicides. We do not observe any differential effect for this indicator when the incentive system is more binding. To assess the effect on police killings, we restrict the sample to January 2011 onward, when this indicator was added to the program, and examine both the intensive margin (levels) and extensive margin (a dummy indicating whether any police killing was registered in the district that month).

Column 4 indicates that eligibility is associated with a 16.8 percent reduction in police killings, while columns 5 and 6 show no differential effect across months. Similarly, column 7 indicates that the incentive program is associated with a 6 percentage points (or 15.6 percent) decrease in the likelihood of registering any police killing. Columns 8 and 9 indicate a larger effect in the last month of the semester (9 percentage points, or a 23 percent decrease), although it is not statistically significant at conventional levels (p-value = 0.14). This is a significant finding because police killings are an outcome that officers have substantial control over. As the perpetrators, they can modify their practices to avoid deadly encounters, such as by reducing the number of raids or changing operational strategies. The potential for such a deeply rooted and socially costly issue as policy killings to respond to incentive structures indicates that incentive-based pay could have the ability to influence police behavior.

INSERT TABLE 3 ABOUT HERE

Exploring the Eligibility Mechanism

We conducted additional analyses to examine how the program's design shaped officers' effort responses. First, we investigated whether officers reduced effort once it became clear that the bonus could not be attained. Table 4 restricts the sample to the last two months of the semester and compares districts that were already above the semester target by month 4 with those that still had a chance of receiving the bonus. We create indicators to identify districts that, by month 4, had already registered the total number of crimes expected for the entire semester, as well as districts that remained eligible (i.e., within their cumulative monthly targets). The omitted group consists of police districts that were off target but still had some probability of reaching it. We find that being above target by month 4 is not associated with any increase in crime outcomes, suggesting that officers did not reduced effort to the extent of worsening the targeted outcomes once the bonus was out of reach.

INSERT TABLE 4 ABOUT HERE

Second, we test an alternative definition of eligibility. Because bonuses depended on the end-of-semester outcome, small deviations around the target during the semester could still be corrected. In Table A1, we replicate the main analysis but allow police districts up to 5 percent above the target to count as eligible. Semester-average results remain similar to the main specification, but the month 6 effects shrink in size and precision, consistent with narrower adjustment margins late in the semester.

Gaming and Spillover to Other Crimes

Given the financial incentives at stake, it is plausible that police officers could attempt to game the system by misreporting targeted crimes or shifting them into untargeted categories. If this were the case, observed reductions in crime would reflect manipulation rather than genuine improvements in performance. To test this possibility, we examine five outcomes that could reveal gaming behavior (see Table 5).

We first consider two related indicators of violent death: (i) cadavers and bones discovered, and (ii) attempted murders. Cadavers and bones are recorded when a body is found but the cause cannot be confirmed as violent. Although these cases are rare (roughly one per district per month, compared to ten violent deaths), they create scope for reclassification. Column 1 does not indicate a statistically significant relationship between bonus eligibility and the records of cadavers and bones found, while models in Column 2 and 3 reveal a positive and marginally significant association in the final month of the semester. Overall, the evidence of gaming in this dimension is inconclusive. Furthermore, given that cases of cadavers and bones found are relatively infrequent (about one per district per month, compared to ten violent deaths), an eventual increase would be unlikely to create substantial distortions in the measurement of violent deaths and influence the allocation of financial bonuses to police officers.

For attempted murders, manipulation might occur if officers failed to update cases when victims later died. However, across all specifications (columns 4–6), the program is linked to a statistically significant decrease in attempted murders. Column 4 shows a 12.2 percent decline when districts are eligible for the bonus. This effect is stronger in the last month of the semester, with estimated reduction varying between 14.8% and 15.8%. Rather than suggesting gaming, this pattern indicates positive spillover effects: incentives that reduce targeted crimes also lower closely related non-targeted offenses.

Next, we examine three categories where reclassification is more feasible: (i) *other robberies*, (ii) *vehicle thefts*, and (iii) *street thefts*. "Other robberies" is a catch-all category that excludes the targeted types. It includes, for example, cargo robberies, robberies to commercial stores, and armed robbery of property. Results in Column 7 indicate an average reduction of 5.8 percent in this indicator, while Columns 8 and 9 do not indicate differential effects across months. These findings may suggest the presence of positive spillovers to non-target crimes rather than manipulation in crime registration.

Turning to thefts, we find no evidence that eligibility affects vehicle thefts. In contrast, street thefts show a 12.1 percent increase in the final months of the semester, precisely when incentives are strongest (see Column 14). This offsetting pattern suggests some reclassification of robberies as thefts, likely because the line between the two is blurry and victims may not always distinguish between them. Importantly, this potential gaming appears limited to street thefts and does not extend to other robbery categories.

Finally, we consider the possibility that crimes were simply not recorded. Here the insurance context provides a strong check: victims of vehicle robberies almost always file insurance claims requiring an official police report, leaving little room for underreporting. The consistency of our vehicle robbery results across multiple specifications strengthens our confidence that the observed reductions reflect declines rather than artifacts of suppressed reporting.

Overall, while we find some evidence of gaming in the classification of street robberies, through reclassification as street thefts, we do not observe systematic manipulation in other categories. On the contrary, reductions in non-targeted crimes point to behavioral changes. These findings suggest that the incentive program may have contributed to improving crime outcomes and, in turn, the conditions of citizens residing in districts eligible for performance-contingent bonuses.

INSERT TABLE 5 ABOUT HERE

Robustness Checks

In addition to the procedures described above, we conducted several complementary checks to assess the validity and robustness of our results.

Parallel trends. Our identification strategy requires that, conditional on fixed effects and controls, crime trends in eligible and ineligible district-months would have evolved similarly without the financial incentives. We cannot conduct the usual event study since, in a sense, our estimation strategy is a reverse difference-in-differences. To test the parallel trend assumptions, we then estimate the following specification:

$$Y_{i,t,s+1} = \beta_1 Elegible^{\#}_{i,6,s} * 1(t = 1, s + 1) + \beta_2 Elegible^{\#}_{i,6,s} * 1(t = 2, s + 1) + \phi_i + \phi_t + \eta_y + \varepsilon_{i,t,s+1} \qquad (5)$$

for months $t = 1$ and 2 in semester $s+1$. In that specification, $Elegible^{\#}_{i,6,s}$ indicates that district i ended the previous semester within its target, i.e., it received the bonus. If our results are truly driven by the PFP program, the effect should be indistinguishable from zero. This is because receiving the bonus in the previous semester should not be related to crime trends in the early months of the following semester. Table A2 shows that the parallel trends hypothesis holds for nine of the eleven crime indicators used in the analysis. Importantly, the results indicate that the observed decreases in violent deaths and vehicle robberies—two crimes targeted by the incentive-based compensation program—as well as the reduction in police killings, cannot be attributed to preexisting crime trends.

Placebo tests. Second, we conducted a placebo analysis using crime data from 2005–2008, prior to the introduction of the program (see Table A3). We simulated a fictional PFP system by applying the program's average target reduction to historical crime rates. If the coefficient of $Elegible_{i,6,s} * 1(t = 6, s)$ were negative and significant, this would suggest that our estimated effects could be driven by seasonality or autocorrelation rather than the program. As shown in Table A3, the placebo regressions reveal no systematic effects, providing additional reassurance that the crime reductions observed after 2009 reflect the actual incentive-based compensation program.

Spatial dependence. We considered whether our results could be driven by spatial displacement of crime. Enhanced policing in one district could, in principle, push crime into neighboring areas, rather than reducing it overall. To test this, we employed a Global Moran's I test with a queen 1 contiguity matrix, which indicates that crime is spatially correlated across districts. We then estimated spatial lag and spatial error models (Table A4) following Anselin, Florax, and Rey (2013). These models explicitly consider spatial correlation in outcomes across districts. The results from these models closely match our baseline fixed effects estimates, even though Lambda is significant for vehicle and street robberies, indicating that spatial dependence does not affect our main findings. Taken together, these results suggest that increases in contiguous districts do not offset reductions in crime within eligible districts.

Functional form. We re-estimate our main results using a Poisson model, that treats crime outcomes as counts. As shown in Table A5, the results closely mirror those reported in Table 2 under our preferred specification.

DISCUSSION

Our results, supported by a battery of robustness tests, show that incentive-based compensation in police forces are likely to reduce crime when officers remain eligible for financial bonuses. Under this condition, we find consistent declines in two of the three targeted crimes—street and vehicle robberies. We also find that the effects are non-linear over time, intensifying toward the end of the incentive cycle—particularly in the final month of the semester. Although violent deaths remain unaffected in the last months of the semester, the program appears to affect police killings, a category more directly influenced by police officers. We also find positive spillovers, with declines in attempted murders and non-targeted robberies such as cargo robbery and armed robbery of property. Yet, our results suggest potential manipulation in the classification of street thefts during the final months of the semester. While such gaming practices may offset some of the gains in reducing street robberies and must be acknowledged, the overall evidence supports our framework: incentive-based compensation programs in police forces generate net positive effects when officers are eligible for financial rewards.

Implications

The results of this study contribute to multiple streams within the policy analysis and management literature. First, we extend research on management-oriented interventions in police forces (Banerjee et al., 2021; Chalfin et al., 2021; Gillooly, 2022; Mutahi et al., 2023; Parfitt et al., 2026) by highlighting the role of financial incentives in generating outcomes that benefit the communities served by law enforcement agencies. Drawing on incentive-based and motivational perspectives, our theorized and tested argument shows that the expectancy of receiving bonuses can enhance policing outcomes and improve crime records. When civil servants such as police officers perceive that their actions advance organizational goals while also yielding personally valuable monetary rewards, they adjust their behavior in ways that align individual, collective, and public interests. Such behavioral changes are evidenced by the non-linear patterns observed during the incentive cycle, with eligible police officers intensifying their collective efforts as the cycle approaches completion.

The paper also highlights how financial incentives can reshape the behavior of eligible police officers working within rigid institutional structures, such as police forces, fostering the creation of value that benefits both individuals and society at large. This aligns with ongoing calls to recognize the role of management attributes in spurring public value creation and delivering benefits to a wide range of stakeholders in public services more broadly (Cabral, 2024; George, Fewer, Lazzarini, McGahan, & Puranam, 2024).

Second, our work adds to the emerging literature on incentives in police forces (Baicker & Jacobson, 2007; Barros Jr et al., 2022; Dincer & Johnston, 2023; Federman, 2020; Gillanders et al., 2024) by examining the conditions under which financial rewards shape both positive and negative responses among police officers. While incentives can mobilize individual and collective efforts that translate into improvements in targeted and non-targeted outcomes, especially toward the end of the incentive cycle, collateral effects may also emerge, such as potential gaming in crime records that are more difficult for victims or audit bodies to detect timely. Despite these drawbacks, our findings suggest that incentives did spur enhanced outcomes in eligible districts, as reflected in the positive effects on both targeted and non-targeted outcomes. Along these lines, performance feedback structures that inform agents of their progress toward expected goals serve as important instruments for guiding the actions of police officers in incentive-based compensation programs.

Third, our work contributes to the broader pay-for-performance (PFP) literature (Gerrish, 2016; Heinrich & Marschke, 2010; Hood, 2006; Pham et al., 2021). Drawing on administrative data and a quasi-experimental design—rather than perceptual measures or approaches without counterfactual assessment—this study offers new evidence of both the positive and negative consequences of financial rewards in public services, with a particular focus on policing. Beyond the limited evidence of incentive-based compensation programs in police forces, our analysis shows the effects of actions of eligible officers on collective performance, moving beyond the individual dimension. This aligns with a discussion in the management literature on the need for greater attention to collective incentive schemes (Nyberg et al., 2018; Perry, Engbers, & Jun, 2009), which have been more extensively studied in education (Pham et al., 2021) and health (Mullen et al., 2010). By investigating both the effects and potential distortions in collective PFP programs, our work responds to recent calls for deeper cross-disciplinary dialogue on how such programs influence motivational forces and performance (Bae, 2021; Cadsby et al., 2007; Elacqua et al., 2022; Langbein, 2010; Nyberg et al., 2018), particularly when agents like police officers believe that coordination with peers and alignment with targeted indicators are instrumental to achieving their goals. Finally, our work provides a nuanced analysis of gaming in incentive schemes, demonstrating how the strength of incentives can influence the behavior of civil servants and encourage strategic manipulation, while also highlighting their unintended consequences and the limitations of such practices. By doing so, we aim to contribute to prior work in public management that examines incentive distortions and their consequences (Heinrich & Marschke, 2010; Hood, 2006; Koning & Heinrich, 2013; Park & Berry, 2014).

Limitations and Future research

Building on our work, several paths for future research become apparent. Although our study uses data from one country, the police forces we examine share key features with agencies in both developed and developing nations, such as incentive levels, bureaucratic constraints, and competition for resources and legitimacy among agencies. At the same time, we acknowledge that cross-country differences may introduce distinct nuances. Future studies could leverage alternative datasets and diverse contexts to assess the extent to which eligibility for bonuses effectively enhances performance across varying institutional settings.

Experimental approaches could be used to disentangle the role of each expectancy component on individual and collective behavior, which was not possible with the administrative data available in this study. Further research could examine how different expectancy features interact with various types of incentives to influence motivation and performance. For example, there is a need to investigate how rewards are distributed (i.e. equally or equitably) among team members in incentive programs and the effects on motivation and outcomes.

Our study lacked access to granular individual-level information, such as data on police district commanders, who likely play a critical role in shaping the effectiveness of the incentive program. Commanders are not randomly assigned to districts; rather, appointments could eventually reflect prior performance, experience, or political considerations. As a result, substantial variation may exist in the strategies commanders employ and in their effectiveness. While our inclusion of commander fixed effects accounts for time-invariant heterogeneity across leaders, it cannot fully rule out bias if assignments are systematically correlated with evolving crime trends. Our identification strategy therefore relies on within-commander and within-district variation in eligibility status, which mitigates—though does not entirely eliminate—this concern. Future studies could explore how the varying capabilities of both leaders and team members influence the link between effort and performance, as well as strategies to mitigate frustration among team members who are not eligible for bonuses, both in incremental police reforms and in public service programs more broadly.

In our study, we observe crime outcomes rather than direct measures of police effort. To address police effort indirectly, we employed several procedures, including exploiting within-district transitions in eligibility status, comparing outcomes across the semester boundary (month 6 versus month 1), and examining spillovers in other nontargeted crimes. Despite these efforts, we acknowledge that future research with access to direct indicators of policing activities—such as patrol intensity, arrests, or civilian complaints—would be valuable for further isolating how incentive schemes translate into changes in police behavior.

Last, while we did not find evidence of worsening crime outcomes among those no longer eligible for financial rewards, our data do not allow us to explore the mechanisms underlying this phenomenon. Building on our findings, future research could advance through either quantitative or qualitative approaches to examine the role of career concerns, intrinsic motivation, or forward-looking incentives among officers who became fully ineligible during the incentive cycle.

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Table 1 – Descriptive Statistics

Type	Variable	N	Mean	St. Dev.	Min	Max	Description
Independent Variable	Eligible	3,024	0.695	0.46	0	1	1 if a given police district was eligible to bonus in the previous month (eligible to bonus); 0 otherwise
Dependent Variables	Violent deaths	3,024	10.922	11.427	0	73	Number of violent deaths (by police district per month)
	Vehicle robbery	3,024	54.304	75.759	0	408	Number of vehicle robbery)
	Street robbery	3,024	163.36	181.652	0	971	Number of street robbery
Violent Death Components	Homicides	3,024	9.757	10.476	0	71	Number of homicides
	Police killing	2,378	1.108	2.036	0	18	Number of deaths by police
	Police killing (dummy)	2,378	0.396	0.489	0	1	Number of deaths by police (dummy)
Alternative dependent variables (not included in the incentive system)	Cadavers and Bones Found	3,024	1.214	1.414	0	12	Number of bodies and bones found
	Attempted Murders	3,024	10.702	9.576	0	72	Number of Attempted Murders
	Other robberies not included in the target	3,024	108.68	120.512	0	633	Number of other types of robbery not targeted in the PFP program (by district per month)
	Vehicle theft	3,024	37.096	30.905	0	182	Number of vehicle thefts
	Street theft	3,024	135.14	141.406	1	2,206	Number of street thefts
Controls	Maximum prize (R$ 1000)	3,024	9.427	4.546	1.5	13.5	Monetary value of the bonus received by the best performing police district in the observed semester
	District population (1000 hab)	3,024	417.97	236.591	32.94	1,136.55	Inhabitants of the observed district in the observed month
	Number of precints	3,024	3.478	1.876	1	10	Number of civil police precincts in a given police district
	Month (dummy)	-	-	-	-	-	Calendar month
	Commander (dummy)	-	-	-	-	-	Battalion Commander unique identifier

Note: This table presents the descriptive statistics of units that participated in in the PFP program between the second semester of 2009 and second semester of 2015, in the state of Rio de Janeiro.

Table 2 - Main Results: Effect of expectancy of receiving bonuses on crime rates

	Number of occurrences								
	Violent deaths			Vehicle robbery			Street robbery		
	OLS	OLS	DD	OLS	OLS	DD	OLS	OLS	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible	-0.752***	-0.781**		-3.326**	-2.249		-13.651***	-12.173*	
	(0.245)	(0.318)		(1.608)	(2.402)		(3.859)	(6.094)	
Eligible* Month 2		1.687*			11.965**			25.188	
		(0.905)			(5.891)			(19.084)	
Eligible* Month 3		-0.752			7.221*			11.733	
		(1.081)			(3.929)			(10.867)	
Eligible* Month 4		1.079*			4.656			9.632	
		(0.584)			(3.315)			(8.945)	
Eligible* Month 5		-0.003			-2.617			2.320	
		(0.370)			(2.132)			(4.866)	
Eligible* Month 6		-0.206	-0.197		-3.447*	-4.298**		-10.387**	-11.260**
		(0.471)	(0.453)		(1.980)	(1.751)		(4.846)	(4.623)
Month FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Y mean	10.92	10.92	10.72	53	53	53.26	161.21	161.21	157.7
Number of police districts	39	39	39	39	39	39	39	39	39
Observations	2,790	2,790	930	2,790	2,790	930	2,790	2,790	930
Adjusted R^2	0.854	0.854	0.842	0.932	0.932	0.925	0.959	0.959	0.949

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015. Columns 3, 6, and 9 restrict the sample to the last and first month of each semester. The main variables include eligibility for the bonus, its interactions with semester–month indicators, and an interaction with the final month of the semester. Controls in all columns include the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, month of the semester and year. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3 – Disaggregation of Violent Deaths

	Number of occurrences								
	Homicides			Police killing			Police killing (dummy)		
	OLS	OLS	DD	OLS	OLS	DD	OLS	OLS	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible	-0.520***	-0.508*		-0.183*	-0.178		-0.061**	-0.031	
	(0.191)	(0.264)		(0.107)	(0.128)		(0.029)	(0.046)	
Eligible * Month 2		0.980			0.380**			0.195***	
		(0.840)			(0.152)			(0.052)	
Eligible * Month 3		-0.596			-0.248			-0.033	
		(1.165)			(0.465)			(0.137)	
Eligible * Month 4		0.769			0.275*			0.062	
		(0.533)			(0.153)			(0.070)	
Eligible * Month 5		-0.185			0.061			-0.041	
		(0.393)			(0.187)			(0.055)	
Eligible * Month 6		-0.106	-0.144		-0.149	-0.073		-0.090	-0.090
		(0.430)	(0.437)		(0.223)	(0.232)		(0.060)	(0.059)
Month FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Y mean	9.79	9.79	9.54	1.09	1.09	1.08	0.39	0.39	0.37
Number of police districts	39	39	39	39	39	39	39	39	39
Observations	2,790	2,790	930	2,144	2,144	740	2,144	2,144	740
Adjusted R^2	0.851	0.851	0.836	0.510	0.510	0.587	0.437	0.437	0.454

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015. Columns 3, 6, and 9 restrict the sample to the last and first month of each semester. The main variables include eligibility for the bonus, its interactions with semester–month indicators, and an interaction with the final month of the semester. Controls in all columns include the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, month of the semester and year. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 4 - Effect on crime outcomes after police officers are no longer eligible

	Number of occurrences		
	Violent deaths	Vehicle robbery	Street robbery
	OLS	OLS	OLS
	(1)	(2)	(3)
Eligible	-0.926**	-4.678**	-18.528***
	(0.385)	(1.860)	(6.253)
Above Target by Month 4	-0.122	-1.192	-5.453
	(0.589)	(3.036)	(9.492)
Month FE	Yes	Yes	Yes
Y mean	10.73	52.36	157.27
Number of police districts	39	39	39
Observations	930	930	930
Adjusted R^2	0.859	0.932	0.956

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015. Columns 1 to 3 restrict the sample to the fifth and sixth month of each semester. Main variables include eligibility for the bonus, its interactions with an indicator if the crime levels of the district were above the target by the fourth month of the semester. Controls include the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, year and month of the semester. Robust standard errors clustered at the district level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 5 - Expectancy of receiving bonuses and gaming

	Number of occurrences														
	Cadavers and Bones Found			Attempted Murders			Other robberies not included in the target			Vehicle theft			Street theft		
	OLS	OLS	DD	OLS	OLS	DD	OLS	OLS	DD	OLS	OLS	DD	OLS	OLS	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Eligible	-0.077	-0.242**		-1.286***	-0.589		-6.156**	-6.022*		-0.129	-0.682		5.670*	-5.113	
	(0.073)	(0.099)		(0.268)	(0.467)		(2.285)	(3.054)		(0.574)	(1.151)		(2.855)	(5.229)	
Eligible * Month 2		0.981***			1.464			14.746*			3.748*			79.646	
		(0.197)			(1.642)			(8.088)			(2.039)			(52.249)	
Eligible * Month 3		0.004			-3.968			8.930			0.523			15.400*	
		(0.380)			(2.716)			(5.538)			(2.668)			(8.181)	
Eligible * Month 4		0.397**			0.715			6.774			2.899*			14.305	
		(0.188)			(0.864)			(4.059)			(1.553)			(9.659)	
Eligible * Month 5		0.185			-0.608			-0.892			0.444			13.866**	
		(0.176)			(0.752)			(2.679)			(1.439)			(6.638)	
Eligible * Month 6		0.237	0.278*		-1.665**	-1.553**		-2.621	-3.326		0.414	0.082		16.283***	13.405***
		(0.146)	(0.151)		(0.662)	(0.647)		(2.707)	(2.603)		(1.192)	(1.157)		(5.328)	(3.984)
Month FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Y mean	1.22	1.22	1.2	10.54	10.54	10.51	106.05	106.05	107.17	37.14	37.14	36.87	134.38	134.38	133.89
Number of police districts	39	39	39	39	39	39	39	39	39	39	39	39	39	39	39
Observations	2,790	2,790	930	2,790	2,790	930	2,790	2,790	930	2,790	2,790	930	2,790	2,790	930
Adjusted R^2	0.255	0.255	0.279	0.645	0.646	0.632	0.942	0.942	0.939	0.891	0.891	0.883	0.742	0.742	0.856

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015. Columns 3, 6, and 9 restrict the sample to the last and first month of each semester. The main variables include eligibility for the bonus, its interactions with semester–month indicators, and an interaction with the final month of the semester. Controls in all columns include the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, month of the semester and year. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A1 - Effect of expectancy of receiving bonuses on crime rates with flexible targets

	Number of occurrences								
	Violent deaths			Vehicle robbery			Street robbery		
	OLS	OLS	DD	OLS	OLS	DD	OLS	OLS	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Almost Eligible (5%)	-0.705***	-0.607**		-3.843***	-3.473		-13.530***	-17.260***	
	(0.190)	(0.235)		(1.383)	(2.260)		(3.456)	(6.054)	
Almost Eligible (5%)* Month 2		0.591			2.180			5.539	
		(0.593)			(2.897)			(6.051)	
Almost Eligible (5%)* Month 3		0.049			2.550			11.997*	
		(0.472)			(2.701)			(6.486)	
Almost Eligible (5%)* Month 4		0.223			-0.322			7.237	
		(0.463)			(2.250)			(4.426)	
Almost Eligible (5%)* Month 5		-0.765			-4.578*			1.565	
		(0.467)			(2.352)			(3.593)	
Almost Eligible (5%)* Month 6		-0.640	-0.752		-1.853	-2.882*		-3.017	-4.821
		(0.442)	(0.452)		(1.529)	(1.707)		(3.865)	(3.591)
Month FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Y mean	10.92	10.92	10.72	53	53	53.26	161.21	161.21	157.7
Number of police districts	39	39	39	39	39	39	39	39	39
Observations	2,790	2,790	930	2,790	2,790	930	2,790	2,790	930
Adjusted R^2	0.855	0.855	0.842	0.932	0.932	0.924	0.959	0.959	0.949

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015. Columns 3, 6, and 9 restrict the sample to the last and first month of each semester. The main variables include almost eligibility for the bonus, its interactions with semester–month indicators, and an interaction with the final month of the semester. Controls in all columns include the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, month of the semester, and year. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A2 – Parallel-trend test

	Number of occurrences										
	Violent deaths	Vehicle robbery	Street robbery	Homicides	Police killing	Police killing (dummy)	Cadavers and Bones Found	Attempted Murders	Other robberies not included in the target	Vehicle theft	Street theft
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Eligible (at t=6) * Month 7	-0.357	-3.305	-20.651***	-0.277	0.061	0.005	-0.032	-0.378	-7.703**	-0.179	-4.307
	(0.303)	(2.130)	(4.817)	(0.308)	(0.161)	(0.047)	(0.090)	(0.579)	(2.934)	(1.036)	(7.478)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y mean	10.88	53.48	162.3	9.6	1.15	0.4	1.25	10.59	107.02	36.97	143.62
Number of police districts	39	39	39	39	39	39	39	39	39	39	39
Observations	930	930	930	930	778	778	930	930	930	930	930
Adjusted R^2	0.851	0.929	0.951	0.846	0.550	0.429	0.285	0.594	0.938	0.868	0.683

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015. The main variables include eligibility for the bonus in semester s and its interactions with month 1 at semester s+1. The sample includes the months one and two for semester s+1. The regressions control for the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, year and month of the semester. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A3 - Effect of expectation of receiving bonuses on crime rates (Placebo analysis between 2005 and 2008)

	Number of occurrences					
	Violent deaths		Vehicle robbery		Street robbery	
	OLS	DD	OLS	DD	OLS	DD
	(1)	(2)	(3)	(4)	(5)	(6)
Elegible	-0.168		0.495		-2.218	
	(0.276)		(1.310)		(2.147)	
Elegible * Month 6		-0.189		-3.856		-4.886
		(1.442)		(3.402)		(4.355)
Month FE	Yes	No	Yes	No	Yes	No
Y mean	13.4	13.4	66.41	69.78	152.76	152.69
Number of police districts	39	39	39	39	39	39
Observations	1,736	555	1,736	555	1,736	555
Adjusted R^2	0.880	0.870	0.962	0.961	0.970	0.970

Note: This table reports regression results for district-level crime from the first semester of 2005 through the second semester of 2008. Columns 3, 6, and 9 restrict the sample to the last and first month of each semester. The main variables include eligibility for the bonus and an interaction with the final month of the semester. Controls in all columns include the number of civil police precincts, commander fixed effects, and fixed effects for district, month of the semester, and year. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, **p<0.05, *** p<0.01.

Table A4– Robustness: Effect of expectation of receiving bonuses on crime rates (Spatial Lag and Spatial Errors Regressions)

	Number of occurrences					
	Violent deaths		Vehicle robbery		Street robbery	
	SAR	SAR	SAR	SAR	SAR	SAR
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible	-0.804***	-1.18**	-4.466***	-5.223**	-17.97***	-16.523***
	(0.274)	(0.487)	(1.423)	(2.54)	(2.707)	(4.82)
Eligible * Month 2		3.414		12.496		30.044
		(3.35)		(17.275)		(33.043)
Eligible * Month 3		0.64		11.432		14.911
		(1.648)		(8.513)		(16.265)
Eligible * Month 4		2.015**		9.602**		14.895
		(0.938)		(4.886)		(9.283)
Eligible * Month 5		0.207		-0.552		-3.191
		(0.723)		(3.788)		(7.172)
Eligible * Month 6		0.224		-1.932		-9.577
		(0.675)		(3.539)		(6.696)
Lambda	-0.229*	-0.237**	0.079	0.084	0.043	0.049
	(0.117)	(0.109)	(0.071)	(0.072)	(0.066)	(0.074)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Y mean	10.94	10.94	50.7	50.7	159.03	159.03
Number of police districts	37	37	37	37	37	37
Observations	2664	2664	2664	2664	2664	2664
R2 adjusted	0.75	0.75	0.86	0.86	0.92	0.92

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015, using a Spatial Autorregressive Model. The main variables include eligibility for the bonus, its interactions with semester–month indicators. Controls include the maximum bonus, number of civil police precincts, and fixed effects for district, year and month of the semester. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A5 - Robustness: Poisson Regressions

	Number of occurrences								
	Violent deaths			Vehicle robbery			Street robbery		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible	-0.081***	-0.096***		-0.080***	-0.039**		-0.113***	-0.096***	
	(0.020)	(0.033)		(0.009)	(0.015)		(0.005)	(0.008)	
Eligible * Month 2		0.502			-0.199			-0.180*	
		(0.721)			(0.516)			(0.104)	
Eligible * Month 3		-0.268*			0.001			-0.071*	
		(0.153)			(0.112)			(0.043)	
Eligible * Month 4		0.139			-0.025			-0.002	
		(0.086)			(0.046)			(0.021)	
Eligible * Month 5		0.025			-0.070***			0.022*	
		(0.048)			(0.022)			(0.012)	
Eligible * Month 6		0.010	-0.0001		-0.059***	-0.065***		-0.063***	-0.086***
		(0.043)	(0.046)		(0.020)	(0.021)		(0.011)	(0.012)
Month FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Y mean	10.92	10.92	10.72	53	53	53.26	161.21	161.21	157.7
Number of police districts	39	39	39	39	39	39	39	39	39
Observations	2,790	2,790	930	2,790	2,790	930	2,790	2,790	930

Note: This table reports regression results for district-level crime from the second semester of 2009 through the first semester of 2015, using a Poisson Model. Columns 3, 6, and 9 restrict the sample to the last and first month of each semester. The main variables include eligibility for the bonus, its interactions with semester–month indicators, and an interaction with the final month of the semester. Controls in all columns include the maximum bonus, number of civil police precincts, commander fixed effects, and fixed effects for district, month of the semester, and year. Robust standard errors, clustered at the district level, are reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.