Indirect Enforcement and the Transformation of Organized Crime: Evidence from the Yakuza*

Tetsuya Hoshino† Takuma Kamada‡

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Abstract

Japan's Yakuza Exclusion Ordinances (YEOs) are indirect enforcement that cuts criminal groups' lawful ties, severing revenue streams. Our model predicts the YEOs reallocate effort from traditional rackets to organized fraud schemes with non-yakuza criminal groups, increasing sophistication level without proportionate growth in case counts. This is particularly salient in low yakuza-competition areas where dominant groups internalize fraud profits. Guided by this model, we empirically find the YEOs raise financial damage while leaving case counts largely unchanged, with stronger effects in low-competition areas. Complementarity between yakuza and non-yakuza groups amplifies this shift. Arrest data confirm yakuza transitioning from extortion to fraud.

Keywords: Indirect enforcement; organized crime; yakuza; crime substitution; competition among organized crime groups
JEL codes: K4, K14, K42, L1, H7.

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†Kyoto University. Email: tetsuyahoshino137@gmail.com

‡Osaka School of International Public Policy, University of Osaka. Email: kamada@osipp.osaka-u.ac.jp

Were the Yamaguchi-gumi broken up now, public order would become much worse. Young members would be destitute. ... [W]ithout our discipline, they would do as they like. ... [T]hey would be driven to income-generating crimes.

Shinobu Tsukasa, the leader of the Yamaguchi-gumi[1]

1 Introduction

Organized crime functions as an extra-legal governance institution for millions across Latin America, Asia, and Europe, providing protection and dispute resolution while extracting rents from citizens, firms, and politicians (Balletta and Lavezzi, 2023; Blattman et al., 2025; Hill, 2003; Gambetta, 1993; Lessing, 2021; Melnikov et al., 2020; Varese, 2001). A central question is whether state interventions reduce the rents generated by criminal governance or instead displace them across illicit activities and organizational forms. Conceptually, the state can intervene along two distinct margins: direct confrontation between organized crime and law enforcement (organized crime vs. police), or policies that raise the costs for non-criminal actors to sustain relationships with organized crime (organized crime vs. society). While existing research has primarily examined direct enforcement strategies in which police actively target criminal organizations (e.g., Braga et al., 2001; Calderón et al., 2015; Chalfin et al., 2021; Dell, 2015; Dickenson, 2014; Piehl et al., 2003; Lindo and Padilla-Romo, 2018), far less is known about indirect enforcement operating through the organized crime–society margin. Understanding this margin is crucial: organized crime's economic power fundamentally depends on connections with the non-criminal world, yet interventions that restrict such interactions may not only disrupt criminal revenue streams but also shift rents toward other illicit activities or organizational forms.

We fill this gap by focusing on the *yakuza*, Japanese organized crime groups. Historically, yakuza have derived income from non-yakuza citizens, including both individuals and businesses, voluntarily or involuntarily. Their traditional rackets include extortion, civil dispute mediation, and intervention in public works projects. To combat the yakuza and maintain public safety, Japan introduced the Yakuza Exclusion Ordinances (YEOs), an indirect enforcement policy that prohibits non-yakuza citizens from associating with or providing any benefits to yakuza groups. This policy aims to sever financial flows by disrupting these relationships. The principles underlying the YEOs align with third-party policing strategies in criminology, which mobilize non-offending individuals to assume responsibility for crime control and prevention (Mazerolle and Ransley, 2004). The YEOs have immediate effects, contributing to a sharp decline in yakuza membership and accounting for approximately 26% of the reduction (Hoshino and Kamada, 2021).

However, the YEOs might give rise to new forms of crime that do not depend on links with non-criminal actors. We focus on *organized fraud*, a new form of white-collar crime, for three reasons. First, organized fraud is considered an emerging revenue source for yakuza (National Police Agency, 2015). Such schemes are often carried out by yakuza members collaborating with groups of non-

[1] Interview with a Japanese newspaper (Sankei Shimbun, 2011).

yakuza fraudsters (NHK, 2014; National Police Agency, 2014a, 2018). Second, violent crimes are relatively unprofitable in Japan due to high arrest rates, whereas organized fraud has a lower arrest rate of approximately 30% during our study period (National Police Agency, 2015). This makes organized fraud more attractive than violence, consistent with reports that yakuza economic activities have increasingly shifted from violence toward fraud. Third, organized fraud has been a serious issue in Japan, accounting for *almost half* of total financial damage from property crimes (National Police Agency, 2012, 2014b), with damage skyrocketing by approximately 140% while cases increased by approximately 70% during our study period.

Model We hypothesize that the YEOs increase financial damage from organized fraud by inducing yakuza groups to reallocate resources from traditional rackets toward fraud schemes. To formalize this hypothesis, we develop a game-theoretic model. A key feature is that yakuza and non-yakuza criminal groups, which jointly perpetrate fraud schemes, play complementary roles. Yakuza typically provide "crime capital" (initial infrastructure such as hideouts and target lists), whereas non-yakuza criminals supply "crime labor" (subordinate perpetrators). Accordingly, we assume that (i) yakuza and non-yakuza criminal groups first invest capital and labor, respectively, in a fraud venture, and (ii) the venture then chooses a fraud plan by specifying the number of cases and the per-case sophistication level. The resulting revenue is divided between the investors according to an extension of the classic Shapley value (the Aumann–Shapley rule), which allocates larger shares to those making greater contributions.

This model admits a pure-strategy equilibrium, which enables equilibrium comparative statics. The enactment of the YEOs, which lowers the profitability of yakuza's traditional rackets, induces yakuza to increase their investment in fraud. Through investment complementarities, non-yakuza criminal groups also expand their investment. Consequently, the fraud venture, now with more resources, conducts schemes more aggressively; however, because of detection risk, the number of fraud cases rises only marginally, whereas the per-case sophistication increases (Propositions 1 and 2). This mechanism yields a testable hypothesis: *the YEOs increase fraud damage while leaving the number of fraud cases largely unchanged or only marginally higher.*

We then examine how competition among yakuza groups moderates the YEOs' effects. Specifically, we investigate how the YEOs' impact differs between "monopolistic" and "oligopolistic" yakuza areas. We measure the intensity of yakuza competition using a Herfindahl–Hirschman-type index. In areas with low yakuza competition, where a few groups dominate, these groups can internalize most of the incremental returns from fraud investment once the YEOs are introduced, thereby reallocating resources toward fraud more aggressively. Consequently, the YEOs' effect should be larger in such areas (Proposition 3).

Empirical Evidence We use a staggered difference-in-differences (DiD) approach to estimate the YEOs' effects on organized fraud. We find that the YEOs significantly increase financial damage from organized fraud, while the estimated effect on the number of fraud cases is positive but statistically indistinguishable from zero, consistent with our hypotheses (Propositions 1 and 2). To ad-

dress identification concerns in staggered difference-in-differences designs (Goodman-Bacon, 2021), we confirm robustness using recently developed DiD estimators (e.g., Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2024; Gardner, 2022; Sun and Abraham, 2021) and a stacked event-study design with not-yet-treated controls. We also verify that the findings are not driven by a particular treatment cohort.

Next, we account for local competition among yakuza groups by constructing a prefecture-level Yakuza Competition Index (YCI), defined analogously to the Herfindahl–Hirschman index. We provide empirical evidence consistent with our hypothesis (Proposition 3): the YEOs' effects on financial damage are larger in areas with lower competition (lower YCI). To further examine complementarity between yakuza and non-yakuza criminal groups, we develop an index for non-yakuza criminal groups. To address the issue that data on these groups are only available in the post-YEOs period, we follow Blattman et al. (2025) to use a lasso procedure to predict non-yakuza presence from pre-treatment covariates. Estimating the interaction among the YEOs, YCI, and the non-yakuza criminal group index, we find evidence of complementarity between yakuza and non-yakuza criminal groups in generating larger fraud amounts under the YEOs. These results are robust to alternative measures of yakuza competition and alternative explanations. The observed heterogeneity could reflect weaker enforcement capacity in low-competition prefectures or more profitable illegal markets in high-competition prefectures that allow yakuza members to sustain themselves through alternative illicit activities. We rule out these possibilities by including interaction terms between the YEOs and both police per capita and an illegal market proxy.

Lastly, using arrest data from Tokyo, we show that the YEOs increase the number of yakuza arrests for fraud while decreasing arrests for extortion, a traditional income source for yakuza. This result could reflect either a behavioral shift among yakuza members or changes in policing enforcement. We address this concern by conducting a placebo regression with a crime type that the YEOs should not affect and find null effects, supporting the behavioral interpretation.

Our analysis uncovers a stark divergence between the severity and frequency of organized fraud. While the YEOs drive a substantial surge in total financial damage, the number of fraud incidents remains statistically unchanged. This finding validates our theoretical mechanism and highlights a generalizable economic principle of criminal firm behavior. When enforcement raises the cost of illicit activities, criminal groups substitute away from the extensive margin (frequency) toward the intensive margin (severity). In the presence of complementarity between organized crime groups, they concentrate resources on fewer but more sophisticated schemes that generate higher returns. This mechanism extends beyond the yakuza context to settings where enforcement differentially affects complementary criminal enterprises: raising costs for some actors while leaving others relatively unconstrained.

Our results reveal a critical policy tradeoff: indirect enforcement weakens organized crime but shifts criminal activity from regulated to unregulated actors. While the YEOs disrupt traditional yakuza operations, they inadvertently encourage fraud networks involving collaboration between yakuza and non-yakuza criminal groups. Importantly, yakuza groups operate as regulated entities

under the Anti-Yakuza Laws (Section 2.1), whereas non-yakuza criminal groups face no such regulatory constraints. Policymakers should recognize that eliminating established revenue streams may not reduce total criminal harm but rather transform it, shifting activity from regulated to unregulated criminal actors who are harder to monitor and control.

Related Literature This study contributes to the economics of crime literature, particularly on organized crime and illegal markets, in three ways. First, most research on enforcement against organized crime examines direct crackdowns led by police or the military (Braga et al., 2001; Calderón et al., 2015; Chalfin et al., 2021; Dell, 2015; Dickenson, 2014; Lindo and Padilla-Romo, 2018; Piehl et al., 2003). Organized crime is associated with societal actors—citizens, businesses, and politicians—in Italy and Latin America (Alesina et al., 2019; Blattman et al., 2025; Daniele and Dipoppa, 2023; Di Cataldo and Mastrorocco, 2021; Fenizia and Saggio, 2024; Gambetta, 1993; Melnikov et al., 2020). The yakuza similarly relies on legitimate ties. We exploit the YEOs as an opportunity to examine whether indirect sanctions that sever these ties deter crime or backfire. Only a handful of papers examine such indirect interventions (e.g., Grogger, 2002; Ridgeway et al., 2019). In Japan, Hoshino and Kamada (2021) find that the YEOs reduce yakuza membership, particularly in areas with lower yakuza competition. In the present paper, we show that this policy increases large-scale fraud as a result of shutting down established revenue streams.

Second, a growing literature examines how illegal market structures shape organized crime groups and their associated violence (e.g., Bruhn, 2021; Brown et al., 2025; Dell, 2015). The closest study to ours is Brown et al. (2025), who show that gangs in El Salvador can achieve higher profits by shifting from violent competition to collusive cooperation. Their analysis of a non-aggression pact demonstrates that when competing gangs coordinate, homicides decline but extortion payments rise in areas with intense pre-existing competition. Our study is complementary, examining a distinct mechanism. Rather than pact-induced collusion, we analyze an enforcement shock (YEOs) that pushes yakuza toward joint fraud schemes with non-yakuza criminal groups. Both settings reveal that these arrangements amplify the economic scale of organized crime. Yet the underlying processes differ sharply—collusion versus enforcement-induced reallocation. Importantly, collusive pacts increase extortion more where gang competition was previously high. That is, the marginal gains from pact-induced collusion are largest where prior inter-gang rivalry had squandered resources through conflict, and thus the pact channels resources into extortion. By contrast, the YEOs' effects on fraud damage are stronger in prefectures with initially low yakuza competition. That is, the enforcement shock facilitates resource reallocation toward joint fraud more aggressively in areas where a few groups, who can internalize most of the incremental returns from fraud investment, dominate.

Third, evidence is mixed on how organized crime groups respond when their revenues shrink. Economic shocks—whether direct price fluctuations or indirect shocks from law enforcement—affect these organizations through shifts in rents (e.g., Gavrilova et al., 2019; Sobrino, 2020) and opportunity costs (e.g., Becker, 1968; Che et al., 2018; Dell et al., 2019; Dube and Vargas, 2013;

Zhukov, 2016). Li et al. (2025) show how institutional reforms can inadvertently spawn illicit markets like human trafficking. Similarly, Battiston et al. (2024) show that enforcement crackdowns on Mexican drug trafficking organizations lead them to reallocate activity toward oil theft. Our findings reveal two forms of substitution: (i) a cross-sector shift from traditional rackets to white-collar crime and (ii) a within-sector shift from the extensive margin (frequency of offenses) to the intensive margin (severity per offense). This "quantity-for-quality" substitution is enabled by collaboration with non-yakuza groups, which facilitates greater sophistication. This shift does not represent adaptation within the traditional yakuza structure but rather a move into forms of crime conducted outside of, or after exiting, yakuza groups. Consistent with this view, we find suggestive evidence that former yakuza members have transitioned to non-yakuza criminal groups and become involved in organized fraud (Appendix B). While yakuza membership is declining and some yakuza groups have dissolved, those who remain are increasingly likely to operate alongside non-yakuza criminal groups. This reveals the strategic transformation organized crime groups undergo when indirect sanctions erode their income streams.

The rest of this paper is organized as follows. Section 2 provides the background on yakuza. Section 3 develops a simple model to formalize our hypotheses and guide our empirical analysis. Section 4 describes our data and identification strategy, and Section 5 presents our main results. Section 6 examines two potential channels. Section 7 concludes.

2 Institutional Background

This section offers institutional background relevant to our study. Section 2.1 provides background on the yakuza.[2] Section 2.2 explains organized fraud, with an emphasis on its financial damage to society and the incentives for yakuza to engage in such crimes under the YEOs. Section 2.3 provides institutional background on the Yakuza Exclusion Ordinances (YEOs).

2.1 Background on Yakuza

Organizing or joining yakuza groups is *not* illegal in Japan. Historically, the yakuza were, to some extent, tolerated in Japanese society, but increased victimization of non-yakuza citizens prompted the government to impose stricter regulations. In response, the Anti-Yakuza Laws (*Bōryokudan Taisaku Hō*) were enacted in 1992, designating yakuza groups based on specific criteria: (i) a hierarchical structure, (ii) the use of violence for financial gain, and (iii) members with criminal records. Designating yakuza groups creates a legal framework that enables authorities to swiftly and systematically issue orders, restrict, penalize, and disrupt the revenue streams of these groups. Before the YEOs' enactment, twenty-one yakuza groups were designated, and there were 82,600 yakuza members, with approximately 96% belonging to one of these designated groups.[3] This proportion remained consistent throughout our study period (National Police Agency, 2009, 2014a).

[2] For a more comprehensive review, see Hill (2003, 2004).

[3] There were 22 designated groups, but in 2011, two of them, both of which operated only in Okinawa, merged.

The yakuza have long engaged in a wide range of legal and illegal economic activities. Core revenue streams include protection rackets, extortion, methamphetamine distribution, gambling, and illegal bookmaking (National Police Agency, 2009). Their operations have diversified in response to social and economic changes (National Police Agency, 2015). For instance, they extract payments from businesses—particularly sex-industry establishments, bars, and nightclubs—through protection fees (*mikajimeryō*), lend money at usurious rates, blackmail corporations (*sōkaiya*), and intervene in civil disputes (*minbō*) (Hill, 2003, 2004; Milhaupt and West, 2000). They also exert influence in infrastructure and construction (Hill, 2003, 2004; Ramseyer, 2016). Overall, yakuza income depends heavily on interactions with non-yakuza citizens and firms. Although the threat of violence underpins these activities, increased enforcement against the yakuza has reduced overt violence and has shifted their income sources (National Police Agency, 2015). For example, they have begun operating legal front companies and have expanded into non-violent illegal activities, particularly organized fraud.

2.2 Yakuza Involvement in Organized Fraud

Organized fraud in Japan, also referred to as "communications fraud," is a category of criminal activity in which offenders illicitly obtain money primarily through the use of telecommunications media, including telephones and email. Victims are mostly the elderly, with nearly 80% of cases involving individuals aged 60 or older (National Police Agency, 2014b). In 2014 alone, total financial damage amounted to 56 billion yen (approximately 380 million U.S. dollars), accounting for *nearly half* of the total losses from property crimes in Japan (National Police Agency, 2014b).

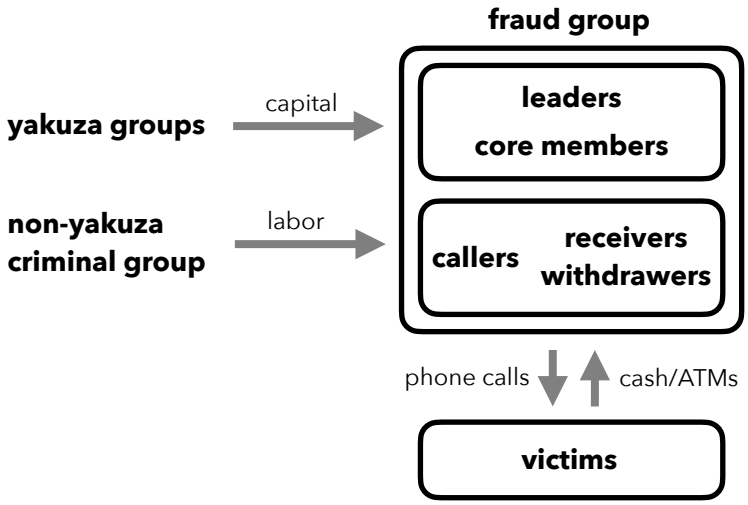
Fraud groups typically consist of both yakuza members and non-yakuza criminals working in collaboration (National Police Agency, 2014a, 2018). Such cooperation, through sharing resources and coordinating roles, enhances systemic efficiency and profitability. Figure 1 depicts a typical structure, in which high-ranking members oversee mid-ranking members, who in turn recruit and instruct low-ranking perpetrators. The latter include callers (*kakeko*), who deceive victims by telephone, receivers (*ukeko*), who collect cash, and withdrawers (*dashiko*), who extract deposited funds from ATMs.

Yakuza members usually occupy higher-ranking positions, providing "crime capital," namely, initial investments for frequently changing hideouts, for procuring target lists, and for purchasing prepaid mobile phones and other infrastructure. Non-yakuza criminals typically serve as subordinate members providing "crime labor" (callers, receivers, and withdrawers). These schemes rely on a division of roles between capital and labor, both of which are essential for efficient operation.[4]

Perpetrators often operate from temporary hideouts, such as rented apartments, making phone scam calls. A common tactic is to impersonate a grandchild who urgently requires money for an alleged accident or emergency. Low-ranking operatives bear the highest risk of apprehension. By contrast, higher-ranking members remain insulated from law enforcement because lower-ranking

[4]The term non-yakuza criminals refers to criminals who have never joined a yakuza group. We distinguish them from non-yakuza citizens.

Figure 1: Structure of Organized Fraud Group



members possess limited knowledge about them (National Police Agency, 2018). This insulation likely makes organized fraud less risky and less costly for yakuza than other illegal revenue streams, such as drug trafficking (National Police Agency, 2015).

2.3 Yakuza Exclusion Ordinances (YEOs)

The Yakuza Exclusion Ordinances (YEOs) are based on the concept of "society versus yakuza." In 2007, the Minister of Justice issued guidelines to reduce victimization by the yakuza, which emphasized the importance of avoiding any association with them, as such associations could provide income to the yakuza. This approach contrasts with the traditional "police versus yakuza" approach. In principle, the YEOs are a type of third-party policing in criminology, which involves police efforts to persuade or coerce third parties to take responsibility for crime control and prevention (Mazerolle and Ransley, 2006). In the case of the YEOs, three parties are involved: police, yakuza, and citizens.

To cut off the yakuza's income sources, the YEOs prohibit non-yakuza citizens from providing any benefits to yakuza members. This includes banning non-yakuza citizens from receiving extralegal protection, providing food, drink, or office space, opening bank accounts or issuing credit cards, leasing office space, or selling insurance to yakuza members. A potential concern is that non-yakuza citizens might be reluctant to comply with the YEOs due to fear of retaliation from yakuza members. To ensure compliance, the YEOs impose penalties on non-yakuza citizens who associate with yakuza members. Authorities may publish the names of offending businesses, damaging their reputation and hindering their ability to conduct business with financial institutions. Offending businesses may also be prohibited from bidding on public construction projects. Violations of the YEOs can result in imprisonment or fines. Therefore, non-yakuza citizens face a dilemma between complying with the YEOs and risking retaliation from the yakuza.

The effectiveness of the YEOs depends on the level of enforcement through coercive methods. In

2011, it was revealed that managers of nine companies in the Onigiri Club, a golf club consisting of seventy construction companies in Fukuoka, played golf with yakuza executives. The Fukuoka Police Department reported this incident to the local government, publishing these companies' names. As a result, the nine companies were barred from public works, and two of them went bankrupt. Although quantifying enforcement levels is challenging, the National Police Agency (NPA) reports that the number of YEO offenders remained stable during the study period, increasing slightly from 62 to 71 (National Police Agency, 2014a). This indicates that the YEOs were enforced to some extent. It is possible that the actual number of YEO offenders is higher due to unreported cases, but this does not invalidate the effectiveness of the YEOs. If the YEOs were poorly implemented and few non-yakuza citizens complied, their effects would be negligible.

3 Model of Organized Fraud

To guide the empirical analysis, we develop a simple model of organized fraud. This model formalizes the yakuza–non-yakuza collaboration and links it to testable predictions. It allows for substitutability across yakuza groups and complementarity between yakuza and non-yakuza criminal groups. The model yields the following predictions (Propositions 1 to 3):

1. The YEOs shift yakuza effort away from traditional rackets (e.g., extortion) and toward organized fraud.

2. The YEOs can backfire by raising financial damage from organized fraud. Because detection risk increases with case volume, the number of cases rises little, if at all.

3. The YEOs' effect on financial damage depends on yakuza competition: the effect is weaker where competition is more intense (the Herfindahl–Hirschman-type concentration is lower).

3.1 Model

We develop a two-stage game to describe how yakuza and non-yakuza criminal groups are involved in organized fraud. In stage 1, they simultaneously invest in a joint "fraud venture," pooling resources, and in stage 2, the venture chooses a fraud case count and a per-case fraud sophistication level to maximize revenue. The resulting revenue is split across groups according to their contributions.

Stage 1 There are G yakuza groups, $g = 1, 2, \ldots, G$, and one non-yakuza criminal group, 0.[5] They simultaneously choose how much to invest in the fraud venture. Yakuza group g, endowed with a unit of criminal resources, allocates $K_g \in [0, 1]$ to the fraud venture and $1 - K_g$ to traditional rackets (e.g., extortion). The non-yakuza criminal group 0, also endowed with a unit of resources, allocates $L \in [0, 1]$ to the fraud venture and $1 - L$ to its outside option.

[5] We have a single non-yakuza criminal group to highlight the substitutability across the yakuza groups and the complementarity between the yakuza and non-yakuza criminal groups.

Yakuza investments $K_1, \ldots, K_G$ are aggregated into "crime capital." We model the aggregation by the constant-elasticity-of-substitution (CES) aggregator: $K = (\sum_g K_g^\rho)^{1/\rho}$ with a substitution parameter $\rho \geq 1$. For simplicity, let $\rho = 2$ and

$$K = \sqrt{\sum_g K_g^2}. \tag{1}$$

As we will see, this parametrization helps highlight the relationship between the aggregation and yakuza competition, measured by the Herfindahl–Hirschman-type index. Combined with non-yakuza "crime labor" L, the venture's resource is $R = AK^\alpha L^\beta$, given parameters $\alpha, \beta \in (0,1)$. Assume that this production exhibits constant or decreasing returns to scale: $\alpha + \beta \leq 1$. Here, we normalize the total factor productivity to $A = 1$. This normalization does not affect any results.

Stage 2 Given the resource R, the venture chooses a fraud case count $n \geq 0$ and a sophistication level $s \geq 0$ subject to the constraint $n + s = R$. Specifically, we take the venture's revenue to be

$$\Pi(R) = \max_{(n,s)\in\mathbb{R}_+^2} \Big\{ (1 - p(n)) \cdot n\pi(s) : n + s = R \Big\}, \tag{2}$$

where $p(n)$ is the apprehension probability of fraud being detected and $\pi(s)$ is the revenue per case. We model the apprehension probability as $p(n) = 1 - \mathsf{e}^{-cn}$ with an arrest intensity $c > 0$. We specify the per-case revenue by a concave function $\pi(s) = s^q$ for some $q \in (0,1)$. These functional forms offer a tractable equilibrium, bringing sharp implications. The constraint $n+s = R$ imposes a one-for-one trade-off between n and s, but this assumption does not affect any results qualitatively.

We define payoffs for the yakuza and non-yakuza criminal groups. They split the revenue $\Pi(R)$ according to the Aumann–Shapley rule. This is an extension of the classic Shapley value to settings where players choose continuous actions (investments). Relegating the definition and derivation to Appendix A, we provide the division formula here: yakuza group g's share φ_g and the non-yakuza group's share φ_0 are, respectively,

$$\varphi_g = \frac{\alpha}{\alpha+\beta} \frac{K_g^2}{\sum_h K_h^2} \qquad \text{and} \qquad \varphi_0 = \frac{\beta}{\alpha+\beta}. \tag{3}$$

Yakuza group g's payoff is $\Pi_g(K_1, \ldots, K_G, L) \equiv \varphi_g \Pi(R) + (1 - K_g)r$, where $r > 0$ denotes the profitability of traditional rackets. Here, $(1 - K_g)r$ is yakuza group g's revenue from the traditional rackets. Similarly, the non-yakuza group's payoff is $\Pi_0(K_1, \ldots, K_G, L) \equiv \varphi_0 \Pi(R) + (1 - L)w$, where $w > 0$ denotes the profitability of its outside option. Here, $(1 - L)w$ is the non-yakuza group's revenue from the outside option.

3.2 Equilibrium and Discussion

We focus on pure-strategy subgame-perfect equilibria. Equilibrium existence is shown in Appendix A. We discuss an interior equilibrium, in which all yakuza groups g and the non-yakuza criminal

group choose $K_g^* \in (0,1)$ and $L^* \in (0,1)$, respectively.[6]

Our goal is to formulate hypotheses about the YEOs implications. As discussed, the YEOs indirectly regulate yakuza involvement in traditional rackets, effectively raising the cost of those crimes. This is captured by a decline in the profitability r of traditional rackets.

YEOs' Effect on Organized Fraud We begin with a basic but important observation:

Proposition 1. *The YEOs' enactment increases the resource R for organized fraud.*

When the profitability r of yakuza traditional rackets falls (due to the YEOs), organized fraud becomes relatively more attractive for each yakuza group, and each K_g rises and so does the aggregate K. Through the complementarity between K and L, the non-yakuza criminal group optimally increases its investment L, further raising the resource R. Accordingly, the fraud venture adjusts its plan (n, s). Let $(n^*(R), s^*(R))$ be a solution to (2).

Proposition 2. *As the resource R increases, the optimal sophistication level $s^*(R)$ increases and is asymptotically linear in R, while the case count $n^*(R)$ increases but saturates at $1/c$.*

Propositions 1 and 2 offer a testable prediction: *the YEOs' enactment increases financial damage from organized fraud while leaving the fraud case count essentially unchanged.* The YEOs depress returns to traditional rackets, shifting yakuza capital—and, by complementarity, non-yakuza labor—into the fraud business and thus R rises. The optimal case count saturates ($n^*(R) \to 1/c$) and marginal resources load onto the sophistication level ($s^*(R) = R - n^*(R)$).

YEOs' Effect Heterogeneity by Yakuza Competition Does yakuza competition amplify, dampen, or leave unchanged the policy's effect? We measure yakuza competition with a Herfindahl–Hirschman index (HHI) based on investment shares. Each yakuza group g invests K_g units of crime capital, with its share equal to $\sigma_g \equiv K_g / \sum_h K_h$. Following the IO literature, we define the HHI as $H = \sum_g \sigma_g^2$, so a higher H indicates greater concentration (weaker competition). By capital aggregation (1), $K = \sqrt{H} \sum_g K_g$. For interpretability in the empirical analysis, we use the **Yakuza Competition Index**, defined by $\mathsf{YCI} \equiv 1 - H \in [0,1]$, where larger values denote more competition; we restate the prediction accordingly.

Since the YEOs increase the resource R available for organized fraud—and, in reality, organized fraud did increase—we examine how the effect of the YEOs is influenced by local yakuza competition when organized fraud is actively committed.

Proposition 3. *The YEOs' effect on financial damage from organized fraud is weaker in areas where yakuza competition is stronger. That is, the marginal effect of the YEOs on financial damage increases with H, i.e., decreases with* YCI.

[6] The reason why we study interior equilibria is that our policy variation enters at the margin; thus, comparative statics will be bang-bang at corner allocations ($K_g^* \in \{0,1\}$ or $L^* \in \{0,1\}$).

Table 1: YEOs' enactment dates

Group	Enactment	#Prefecture	Prefecture
1	Apr. 2010	3	Fukuoka, Nagasaki, Kagoshima
2	Aug. 2010	1	Ehime
3	Apr. 2011	24	All the other prefectures
4	Jul. 2011	6	Aomori, Iwate, Akita, Fukushima, Nara, Wakayama
5	Aug. 2011	8	Yamagata, Saitama, Niigata, Shizuoka, Toyama, Ishikawa, Shiga, Miyazaki
6	Sep. 2011	2	Chiba, Nagano
7	Oct. 2011	2	Tokyo, Okinawa
8	Jan. 2012	1	Saga

When the YEOs reduce the profitability of traditional rackets, yakuza groups g reallocate more capital toward fraud. Then, the yakuza groups with larger investment shares internalize more of the fraud surplus. In more concentrated markets (higher H, lower YCI), the post-YEOs reallocation into fraud is larger, amplifying the increase in financial damage.

4 Data and Identification Strategy

4.1 Data

YEOs' Enactment Dates The YEOs are local ordinances enacted at different times across prefectures (Table 1).[7] Most prefectures enacted the YEOs in April 2011, while others did so between April 2010 and January 2012. We identify the effects of the YEOs using the staggered enactment dates across prefectures and address concerns regarding staggered settings, as detailed in Section 4.2.[8]

Organized Fraud We use prefecture-level monthly panel data on organized fraud provided by the National Police Agency (NPA) covering the period from 2011 to 2013. Due to this data restriction, the YEO treatment dummies remain constant in the four prefectures that enacted the YEOs before 2011 (Ehime, Fukuoka, Nagasaki, and Kagoshima).

We use two outcomes: (i) financial damage from organized fraud, and (ii) the number of organized fraud cases. We apply the inverse hyperbolic sine transformation to both outcomes to accommodate zeros (Mullahy and Norton, 2024). The average damage from organized fraud is 64

[7] In several prefectures, the YEOs were amended shortly after enactment. For example, Akita enacted the YEOs in March 2011 and amended them in July 2011. We define the enactment date as the point when clauses regulating non-yakuza activity were incorporated, as our study focuses on the third-party policing aspect of the YEOs. Similar amendments occurred in Tottori, Fukuoka, and Saga.

[8] While the specific provisions vary slightly across prefectures, the basic concept is the same: non-yakuza citizens must check whether contractual counterparties have yakuza ties. In some prefectures, this applies only to current members; in Tokyo, it extends to individuals closely associated with the yakuza. In many prefectures, contracts are prohibited if fewer than five years have passed since a counterparty's retirement from a yakuza group.

million yen (Table 2). From 2011 to 2013, the average damage increased from 37 million yen to 90 million yen, an increase of roughly 140%. By contrast, the number of fraud cases per 100,000 residents is 0.466 during the study period, with an increase from 0.35 to 0.60.

These measures are based on reports from fraud victims. A concern may arise regarding victims' reporting behavior, but we argue that this concern is minimal for estimating the YEOs' effects. Increased organized fraud has led police to alert the elderly, potentially preventing them from being deceived, but this should not alter reporting behavior conditional on victimization. Additionally, as time passes, police dissemination of information about organized fraud may increase public awareness and potentially reduce victimization rates (and therefore incident rates). However, as our theoretical analysis illustrates, a countervailing force exists: criminals may refine and sophisticate their schemes in response, potentially enabling them to extract larger amounts through fraud. To investigate these competing dynamics, we conduct an event-study using an extended time window (Appendix F).

Yakuza Competition Index We construct a prefecture-level Yakuza Competition Index (YCI) analogous to the Herfindahl–Hirschman Index (HHI).[9] If each group's membership in each prefecture were known, this construction would be trivial, but such data are unavailable. We then estimate yakuza shares in each prefecture. Our baseline specification is that group g's members are distributed across the prefectures where g operates in proportion to prefecture population. Given the population q_p of prefecture p, the membership of g in p is estimated to be $N_{gp} = (g\text{'s total membership}) \times (q_p / \sum_{p'} q_{p'})$, where $\sum_{p'}$ runs over all prefectures where g operates. We then define the share $s_{pg} = N_{gp} / \sum_{g'} N_{g'p}$ if g operates in p and $s_{pg} = 0$ otherwise, where $\sum_{g'}$ runs over all yakuza groups operating in p. Then, the YCI of p is defined as $\mathsf{YCI}_p = 1 - \sum_g s_{pg}^2$.[10] Note that $\mathsf{YCI}_p = 0$ if there is only one yakuza group in p. We estimate the shares and YCIs under different specifications to examine robustness in Appendix I. We treat the YCI as time-independent and measure it using data prior to the YEOs' enactment (as of 2009).

Non-Yakuza Criminal Group Index Our model assumes complementarity between yakuza input ("capital") and non-yakuza input ("labor") by specifying a Cobb-Douglas production function. We cannot directly test this complementarity, but we examine its implications through the YCI and non-yakuza criminal group index. These measures capture the geographic and operational interdependence.

Because non-yakuza criminal groups are newer than yakuza groups and not formally designated

[9] See Brown et al. (2025) for an HHI-based gang-competition measure.

[10] An alternative index is simply the number of yakuza groups in each prefecture. However, this alternative measure has an issue. For example, both Tokyo and Kumamoto have six yakuza groups, but the number of yakuza members in Tokyo is about 15 times that in Kumamoto. Hence, the alternative measure ignores differences in the number of yakuza members. Using the YCI, we capture the differences in the number of yakuza members. For these reasons, we do not use the alternative measure for our main analysis, but our results are robust to the alternative index. In Appendix I, we conduct several robustness checks using alternative indices. We adopt alternative definitions for yakuza shares s_{pg} and the resulting YCIs. We also use alternative definitions for yakuza competition levels. Our empirical results are robust to all these specifications.

by authorities, data on these groups are informal and available only in the post-YEOs period; we source these data from YakuzaWiki (as of August 10, 2025) (Appendix N).

We provide a detailed description of our procedure to construct our measure of non-yakuza criminal groups in Appendix N. Essentially, following Blattman et al. (2025), we use LASSO regression on baseline covariates to predict this post-treatment variable, yielding a pre-treatment proxy for non-yakuza criminal groups (NYG). Specifically, we estimate a LASSO Poisson model with 10-fold cross-validation to predict the number of non-yakuza criminal groups. Our measure uses the predicted incidence rates from the cross-validation-selected model. LASSO coefficients are provided in Table A3 in the Appendix. The correlation coefficient between the actual and predicted number of non-yakuza criminal groups is 0.8013.

Control Variables We control for the following demographic and economic variables: the share of population aged 19–39, the share of population aged 65+, the share of foreign population, the unemployment rate, and GDP. The data on these variables are publicly available from e-Stat, a portal site for Japanese Government Statistics. Table 2 provides the summary statistics of these control variables.

Table 2: Summary Statistics of Outcome and Control Variables

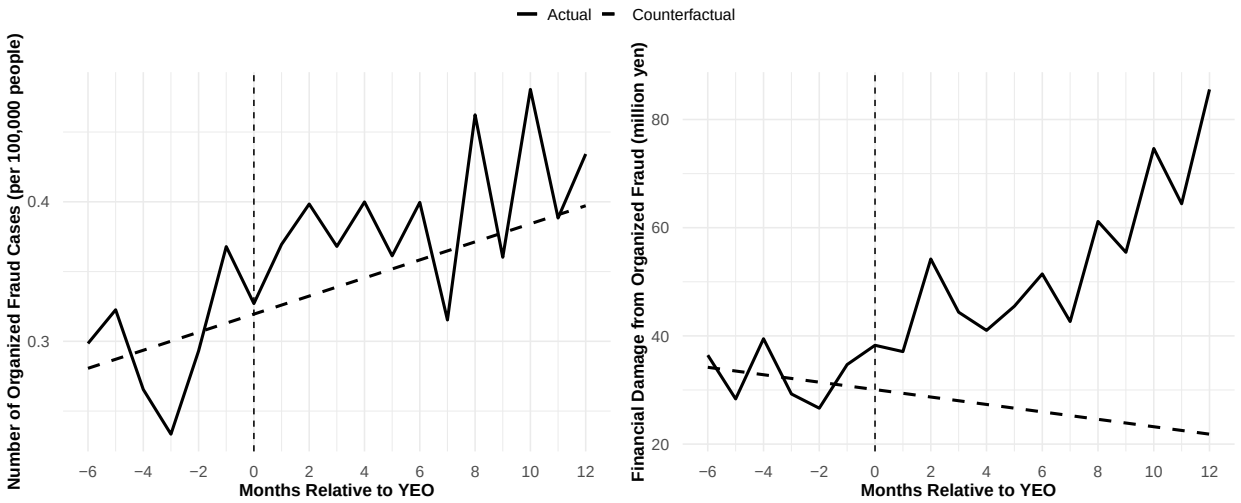
	Mean	Std. Dev.
Financial Damage from Organized Fraud (million yen)	64.7804	117.7376
Number of Organized Fraud Cases (per 100,000 residents)	0.4661	0.3496
YCI	0.3989	0.2976
NYG	1.7520	0.5539
Share of Population Aged 19-39	0.2724	0.0223
Share of Population Aged 65+	0.2558	0.0274
Share of Foreign Population	0.0122	0.0072
Unemployment Rate	0.0432	0.084
GDP (trillion yen)	11.7159	16.3485

Observations: 1,548 (excluding "always-treated" prefectures: 43 prefectures × 3 years × 12 months). Organized fraud outcomes and GDP are measured at the prefecture-month level. YCI (yakuza competition index) and NYG (non-yakuza criminal group index) are time-invariant. All other variables are measured at the prefecture-year level. **Data Sources:** Organized fraud data are from the National Police Agency. YCI is constructed using data from National Police Agency (2009). NYG is constructed using data from YakuzaWiki and lasso regression on baseline covariates (see Appendix H). Control variables are from e-Stat.

Descriptive Evidence Figure 2 shows descriptive and counterfactual trends in organized fraud. Panel A shows trends in the number of fraud cases, and Panel B shows trends in financial damage. The counterfactual trends are obtained by fitting linear time trends to the months prior to the YEOs' enactment and extrapolating these pre-treatment trends to subsequent periods to estimate

the trajectory that fraud would have followed without the policy intervention. Relative to the counterfactual trend, the actual trend in financial damage from organized fraud increases from approximately 40 million yen to 80 million yen over the 12 months following the enactment of the YEOs. By contrast, we do not observe corresponding patterns in the number of cases.

Figure 2: Descriptive and Counterfactual Trends in Organized Fraud



The counterfactual trends are obtained by fitting linear trends to pre-YEOs data and projecting subsequent values.

4.2 Research Design

Difference-in-Differences Design We estimate the effects of the YEOs using a difference-in-differences (DiD) approach that exploits monthly variation in their enactment dates across prefectures. Using the two-way fixed effects (TWFE) estimator, our benchmark DiD specification takes the following form:

$$\mathsf{Fraud}_{pt} = \alpha_1 \mathsf{YEO}_{pt} + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}, \tag{4}$$

where Fraud_{pt} is the inverse hyperbolic sine (IHS) transformation of either (i) organized fraud damage or (ii) the number of organized fraud cases per 100,000 residents in prefecture p at time t. YEO_{pt} is a treatment indicator equal to 1 if the YEOs are in effect in prefecture p at time t and 0 otherwise.[11]

Prefecture FE are prefecture fixed effects that control for time-invariant differences across prefectures. Time FE are time fixed effects, specified in alternative ways: (i) Month and year fixed effects: ξ_m (month fixed effects) and ξ_y (year fixed effects), where ξ_m captures seasonality (e.g., $t = 1, 13, 25$ all correspond to January) and ξ_y captures common shocks by year. (ii) Month and year fixed effects: ξ_m and ξ_y, together with region-specific linear trends $\sum_{r \in R} \delta_{pr} \rho_r t$, where Japan

[11] We use monthly panel data. Time t refers to the t-th month in our dataset. For example, in a dataset beginning in January 2011, $t = 1$ corresponds to January 2011, $t = 13$ to January 2012, $t = 25$ to January 2013, and so on.

is divided into seven regions $r \in R$, and $\delta_{pr} = 1$ if prefecture p is in region $r \in R$.[12] These trends control for unobservable region-specific factors that evolve over time. (iii) Region-by-month and region-by-year fixed effects: $\xi_{r,m}$ and $\xi_{r,y}$, which account for arbitrary monthly and yearly shocks at the region level. (iv) Month-by-year fixed effects: ξ_t, which fully absorb common shocks in each month-year cell, as well as region-by-month and region-by-year fixed effects. Lastly, ε_{pt} is an idiosyncratic error term.

Event-Study Design We study the dynamic effects of the YEOs on organized fraud by examining months before and after the enactment of the YEOs in an event-study framework. The analysis has two purposes. First, we indirectly assess the parallel trends assumption, the identifying assumption for the DiD strategy, by including the lead treatment indicators. The parallel trends assumption states that in the absence of a treatment, the treatment and control groups follow similar trends in the outcome of interest. This assumption could be violated, however, if the YEOs' enactment timings reflect public concern about a recent surge in organized fraud. If the assumption is satisfied, the coefficients of the lead treatment indicators should be close to zero. As future treatment status (i.e., the lead treatment indicators) would have no significant effects if the treatment has causal effects, the null effects of the lead treatment indicators serve as a placebo test. In addition, the analysis explores whether the YEOs' effects persist for the months following their enactment. We include lag treatment indicators to explore whether an increase in fraud damage or fraud cases persists after the YEOs' enactment. If the YEOs' effects are persistent, then the coefficients for the months following the YEOs' enactment should be large in magnitude and statistically significant.

We normalize prefecture p's time of observation so that the YEOs' enactment time in p is zero: t = (calendar time) − (YEOs' enactment month in p). The regression model for the event-study design is

$$\mathsf{Fraud}_{pt} = \sum_{\tau=-l}^{k} \beta_\tau \mathsf{YEO}_{pt}^{\tau} + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}, \tag{5}$$

where $\beta_{-1} = 0$ by normalization. YEO_{pt}^{τ} is the treatment indicator: for the YEOs' enactment date t_p in prefecture p, $\mathsf{YEO}_{pt}^{\tau} = 1$ if the time difference $|t - t_p|$ is either (i) equal to τ, (ii) greater than or equal to k (6 or 10), or (iii) less than or equal to $-l$ (-3 or -5). The coefficient β_τ indicates, relative to the reference period set to one month before the YEOs' enactment ($\tau = -1$), the τ-month lag effect if $\tau \geq 1$ and the $(-\tau)$-month lead effect if $\tau \leq -1$.

Potential Concerns We address three concerns regarding the YEOs' enactment across prefectures. First, a staggered DiD design relies on differential timing for identification, which raises two concerns: the "forbidden comparison" problem, where later-treated units serve as controls for earlier-treated ones, and potential bias from treatment effect heterogeneity across units and over time (Goodman-Bacon, 2021). We address these concerns in three ways. First, we use four recently

[12] The seven regions are Hokkaido–Tohoku, Kanto, Chubu, Kinki, Chugoku, Shikoku, and Kyushu–Okinawa.

developed estimators: (i) Sun and Abraham (2021), (ii) De Chaisemartin and d'Haultfoeuille (2024), (iii) Callaway and Sant'Anna (2021), and (iv) Gardner (2022); we report results in Appendix C. Second, we use a stacked event-study approach, which avoids forbidden comparisons; we present results in Appendix D. Third, we verify that the YEOs' effects are not driven by a particular treatment cohort; we report results in Appendix E.

Second, the timing of the YEOs' enactment may not be fully exogenous if local governments responded to local yakuza activity. For instance, Fukuoka, the first prefecture to enact the YEOs, had experienced severe yakuza conflicts. However, given the nationwide push to strengthen regulations, local discretion was likely limited. Conditional on prefecture and time fixed effects, we view the timing as plausibly exogenous to outcome trends. Supporting this view, lead treatment indicators in the event-study design suggest that treatment effects are not driven by pre-existing trends in organized fraud financial damage. Additionally, including observable covariates minimally changes the magnitudes of the estimated coefficients, indicating that selection on observables is unlikely to be a concern.

Third, four prefectures, including Fukuoka, enacted the YEOs before 2011. Because our data on organized fraud are only available starting in January 2011, these prefectures are "always-treated" in our sample. Throughout our analysis, we exclude these prefectures. In our robustness check, we include them and verify that our results remain robust (Appendix L).

Heterogeneity We examine treatment effect heterogeneity by allowing the impact of the YEOs to vary with cross-sectional differences in (i) yakuza competition, measured by the Yakuza Competition Index (YCI), and (ii) the presence of non-yakuza criminal groups, measured by the non-yakuza criminal group index (NYG). Our first specification is:

$$\mathsf{Fraud}_{pt} = \kappa_1 \mathsf{YEO}_{pt} + \kappa_2 \mathsf{YEO}_{pt} \times \mathsf{YCI}_p + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}, \tag{6}$$

where YCI_p is the yakuza competition index for prefecture p. The interaction term $\mathsf{YEO}_{pt} \times \mathsf{YCI}_p$ captures how the YEOs' effects vary with the level of local yakuza competition. In this framework, a negative and statistically significant κ_2 indicates that the YEOs' effect on organized fraud diminishes as yakuza competition increases. Conversely, a positive κ_2 indicates that the YEOs' effect is amplified in areas with higher yakuza competition.

Our second specification incorporates the non-yakuza criminal group index (NYG_p) and a triple interaction term:

$$\begin{aligned}\mathsf{Fraud}_{pt} = {} & \lambda_1 \mathsf{YEO}_{pt} + \lambda_2 \mathsf{YEO}_{pt} \times \mathsf{YCI}_p + \lambda_3 \mathsf{YEO}_{pt} \times \mathsf{NYG}_p + \lambda_4 \mathsf{YEO}_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p \\ & + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}.\end{aligned} \tag{7}$$

Here, NYG_p measures the predicted pre-treatment presence of non-yakuza criminal groups in prefecture p. The triple interaction term $\mathsf{YEO}_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p$ allows us to test whether yakuza and non-yakuza criminal groups act as complements in generating organized fraud. Formally, comple-

mentarity would be reflected in a positive cross-partial derivative: $\frac{\partial^2 \mathsf{Fraud}_{pt}}{\partial \mathsf{YCl}_p \partial \mathsf{NYG}_p} = \widehat{\lambda}_4 \times \mathsf{YEO}_{pt}$. A positive and significant $\widehat{\lambda}_4$ implies that the marginal effect of yakuza competition on organized fraud increases with the presence of non-yakuza criminal groups in the post-YEOs period, and vice versa, consistent with joint participation in larger or more sophisticated fraud schemes.

Throughout all analyses, standard errors are clustered at the prefecture level to adjust for serial correlation within the same prefecture.

5 Main Results

5.1 YEOs' Effects on Organized Fraud

Table 3 shows that the YEOs increase financial damage from organized fraud (Panel A) but have no measurable effect on the number of fraud cases (Panel B). Because the dependent variable is inverse hyperbolic sine (IHS)-transformed, the coefficients can be interpreted as approximate percentage changes. Column (1) presents benchmark estimates with prefecture, month, and year fixed effects. Column (2) adds observable control variables; Column (3) adds region-specific linear time trends; Column (4) replaces month and year fixed effects with region-by-month and region-by-year fixed effects; and Column (5) uses month-by-year fixed effects.

In Panel A, the estimated YEO coefficient is positive in all specifications, ranging from 0.674 to 0.797, corresponding to an increase of roughly 72–82%. The similarity of point estimates across specifications provides reassurance that the results are not sensitive to model choice. In Column (5), adding month-by-year fixed effects yields a slightly larger point estimate but also larger standard errors, reflecting the substantial variation absorbed by this specification. To explore this possibility, we use double-LASSO techniques (Belloni et al., 2014) to obtain more parsimonious time controls through an optimization procedure, and we present the results in Appendix H, where we see a slightly larger coefficient and smaller standard errors. For this reason, we treat the results from Column (4) as our preferred specification. In Panel B, the YEO coefficient remains close to zero and statistically insignificant in all specifications. These results are consistent with our model predictions: the YEOs raise the scale (financial damage) of individual fraud cases, while their effect on the frequency (number) of organized fraud incidents is negligible or only marginally positive.

Furthermore, to rule out the possibility of false discovery, we test multiple hypotheses based on the specification in Column (4) of Table 3. We estimate this specification separately for the two outcomes and then calculate family-wise adjusted p-values based on 1,000 bootstrap replications using the free step-down resampling procedure of Westfall and Young (1993), as implemented by Jones et al. (2019). The adjusted p-values are 0.009 for financial damage from organized fraud and 0.833 for the number of organized fraud cases.

Table 3: Effects of the YEOs on Organized Fraud

Panel A:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Financial Damage from Organized Fraud					
YEO	0.6742**	0.7103**	0.7201**	0.7272**	0.7969*
	(0.2847)	(0.3011)	(0.2934)	(0.2926)	(0.4340)
R-squared	0.4721	0.4781	0.4822	0.5198	0.5268
Panel B:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Number of Organized Fraud Cases					
YEO	-0.0037	0.0018	-0.0020	0.0042	0.0101
	(0.0252)	(0.0245)	(0.0244)	(0.0255)	(0.0409)
R-squared	0.5554	0.5599	0.5669	0.6078	0.6195
Observations	1,548	1,548	1,548	1,548	1,548
Prefecture Fixed Effects	✓	✓	✓	✓	✓
Month and Year Fixed Effects	✓	✓	✓	—	—
Control Variables	—	✓	✓	✓	✓
Region-Specific Linear Time Trends	—	—	✓	—	—
Month × Region and Year × Region Fixed Effects	—	—	—	✓	✓
Month × Year Fixed Effects	—	—	—	—	✓

Outcome in Panel A: The inverse hyperbolic sine (IHS) of financial damage from organized fraud. **Outcome in Panel B:** IHS of organized fraud cases per 100,000 residents. **Controls:** Share of population aged 19–39, share aged 65+, share of foreign population, unemployment rate, and GDP. **Westfall and Young's (1993) adjusted p-values based on Column (4):** 0.005 (financial damage from organized fraud), 0.056 (number of organized fraud cases). Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

5.2 Event-Study Analysis

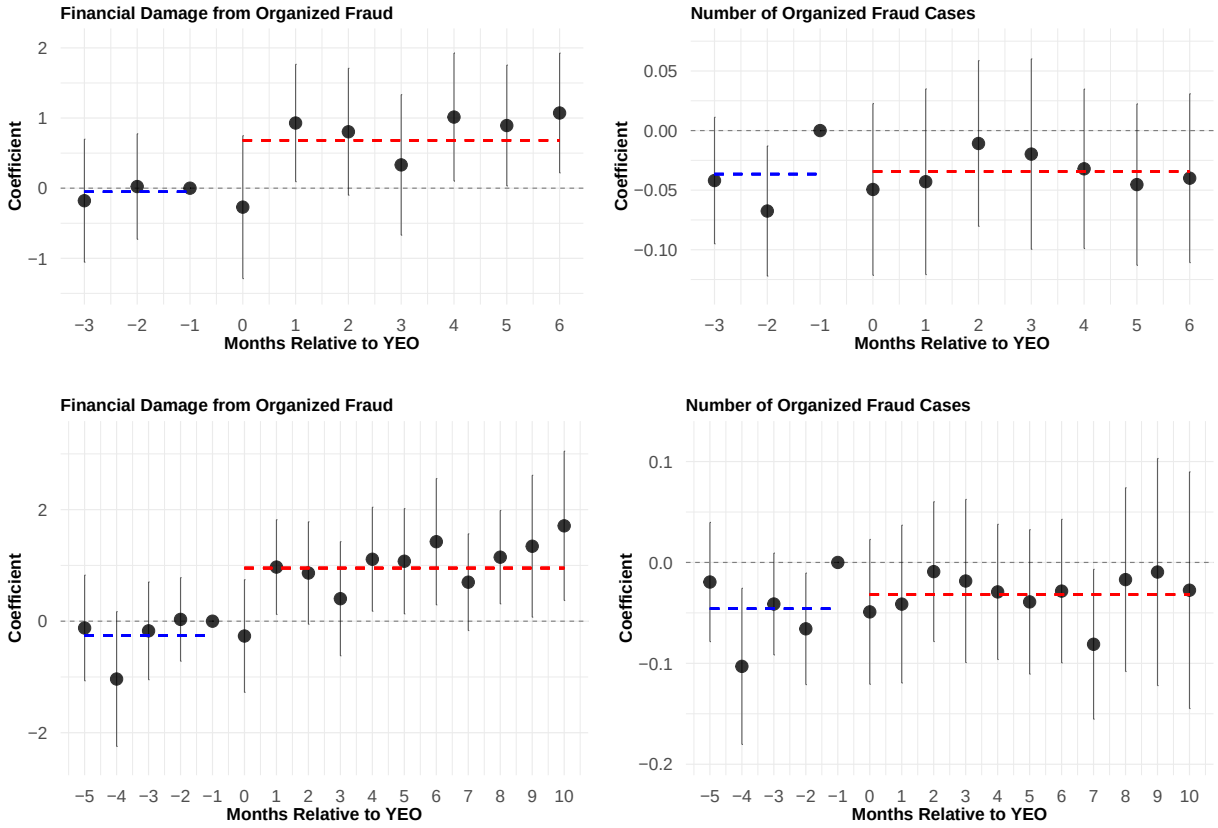
Figure 3 presents the event-study estimates from regression model (5), with 90% confidence intervals. The specification includes prefecture fixed effects, region-by-month fixed effects, region-by-year fixed effects, and control variables. The left panel reports results for financial damage from organized fraud, and the right panel reports results for organized fraud cases. In each panel, the dashed red line indicates the average of the post-treatment coefficients, and the dashed blue line indicates the average of the pre-treatment coefficients.

The event-study results align with the benchmark DiD estimates in Table 3: the YEOs increase financial damage, but we do not find a corresponding effect on fraud case counts. In the left panel, the pre-enactment coefficients are close to zero and statistically insignificant, while the post-enactment coefficients are large and statistically significant, providing indirect support for the parallel trends and non-anticipation assumptions. In contrast, the right panel shows that most coefficients are statistically insignificant in both the pre- and post-YEOs periods, and the averages of the pre- and post-treatment coefficients are nearly identical.

The event-study analysis reveals that financial damage from organized fraud jumps one month

after the YEOs' enactment. It is true that the prefectural authorities announced the YEOs' enactment in advance, but the financial damage was stable and close to zero before the enactment, suggesting no announcement effect. To better understand the immediacy, we note the following from both the citizens' and yakuza perspectives. From the citizens' side, businesses that fail to comply with the YEOs risk reputational damage or even bankruptcy, as illustrated by the Onigiri Club example in Section 2. This created a strong incentive for businesses to sever ties with yakuza immediately upon enactment. The YEOs were well publicized prior to enactment through extensive media coverage. For instance, two months before the YEOs' enactment in Tokyo, Shinsuke Shimada, one of the most popular comedians and TV show hosts, retired from show business after his ties with yakuza members were discovered. This scandal caused a media frenzy, impressing upon the public that association with the yakuza ruins one's reputation. Additionally, the interview with a yakuza leader (quoted in the Introduction) was released on the same day that the YEOs were enacted in Tokyo. From the yakuza perspective, organized fraud requires a variety of criminal skills (National Police Agency, 2015). Yakuza needed to prepare, for example by recruiting non-yakuza criminals to execute these schemes. This means that yakuza, anticipating future economic difficulties due to the YEOs, may have begun preparing in advance, such as finding non-yakuza criminals, so they could quickly pivot to organized fraud once their traditional ties with citizens were severed. Thus, the interval between the announcement and enactment of the YEOs serves as a period to prepare for organized fraud.

Figure 3: YEOs' Effects on Organized Fraud with 90% Confidence Intervals



Dashed red line: mean of post-treatment coefficients. **Dashed blue line:** mean of pre-treatment coefficients. **Left panel:** IHS of transformation of financial damage from organized fraud. **Right panel:** IHS of organized-fraud cases per 100,000 residents. **Top panel event window:** months −3 to +6. **Bottom panel event window:** months −5 to +10 (both relative to YEOs enactment). **Observations:** 1,548.

Addressing Identification Concerns and Robustness Our identification strategy exploits the differential timing of the YEOs' enactment across prefectures. We address two concerns in several ways: the "forbidden comparison" problem, where later-treated units serve as controls for earlier-treated ones, and potential bias from treatment effect heterogeneity across units and over time (Goodman-Bacon, 2021).

- We conduct joint-significance F-tests to validate key assumptions of our event-study design. For the lag treatment indicators, we test the null hypothesis that all coefficients are equal; failure to reject this null implies constant treatment effects over time. For the lead treatment indicators, we test the null that all coefficients are equal; failure to reject this null supports the parallel trends assumption. Table 4 presents the joint-significance F-test results. Both null hypotheses cannot be rejected across all specifications, with one exception: the specification using the number of organized fraud cases as the outcome, with 5 lead indicators.
- We address the forbidden comparison issue and potential bias due to treatment effect hetero-

Table 4: F-Test Results for Joint Significance

Outcome Variable	Specification	F-Test Results	
		Lag Indicators	Lead Indicators
Financial Damage from Organized Fraud	6 lags, 3 leads	$F = 1.51$ ($p = 0.198$)	$F = 0.08$ ($p = 0.925$)
	10 lags, 5 leads	$F = 1.37$ ($p = 0.227$)	$F = 0.66$ ($p = 0.612$)
Number of Organized Fraud Cases	6 lags, 3 leads	$F = 0.29$ ($p = 0.938$)	$F = 2.10$ (p= 0.136)
	10 lags, 5 leads	$F = 1.13$ ($p = 0.365$)	$F = 2.59$ ($p = 0.051$)

geneity by using four recently developed estimators that are robust to treatment effect heterogeneity in staggered adoption settings (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2024; Gardner, 2022; Sun and Abraham, 2021) (Appendix C). Some of these estimators are robust even when all units eventually receive treatment (De Chaisemartin and d'Haultfoeuille, 2024; Sun and Abraham, 2021). These results corroborate our main findings.

- We address the forbidden comparison issue by using a stacked event-study design that builds separate comparisons for each treatment cohort, using only not-yet-treated prefectures as controls within that cohort's calendar window (Appendix D). The results corroborate the main findings.
- We address potential treatment effect heterogeneity by examining whether the YEOs' effects are similar across treatment cohort groups (Appendix E). The results suggest that outcome changes are broadly similar across groups, indicating that treatment effect heterogeneity is unlikely to be a major concern.
- We also assess robustness to sample composition (Appendix L) and estimator choice (Appendix M); all results are reported in these Appendices.

6 Heterogeneity and Channels

Next, we examine heterogeneous effects of the YEOs by the Yakuza Competition Index (YCI) and explore potential complementarity between yakuza and non-yakuza criminal groups in response to the YEOs. We then investigate our prediction that the YEOs induce yakuza members to reallocate resources from traditional rackets toward organized fraud using data on yakuza arrests for extortion and fraud.

6.1 Heterogeneous Effects of YEOs by Yakuza Competition Index

Table 5 presents the heterogeneity analysis results across columns (1)–(3). Panel A shows results for financial damage from organized fraud, while Panel B shows results for the number of fraud cases. In Panel A, the main effect of the YEOs is positive and significant across all specifications. The interaction term between the YEOs and YCI (standardized to mean 0 and standard deviation 1) is consistently negative and significant, indicating that the YEOs' effects are stronger in prefectures with less yakuza competition. By contrast, in Panel B, the main effect of the YEOs and the interaction term between the YEOs and YCI are not statistically significant. This is consistent with

our theoretical predictions (Proposition 3): when the YEOs reduce the profitability of traditional rackets, dominant groups in less competitive prefectures internalize more of the fraud surplus and thus reallocate more resources into fraud, raising financial damage while the optimal number of cases remains capped by detection risk.

Column (1) provides the baseline test of this prediction. Column (2) examines an alternative mechanism by adding an interaction term between the YEOs and police per capita: prefectures with less yakuza competition may have weaker enforcement capacity, which could drive the observed heterogeneity. Column (3) tests another alternative by adding an interaction term between the YEOs and an illegal market index (a composite measure based on principal component analysis of prostitution arrests, methamphetamine arrests, and gun seizures): in prefectures with high yakuza competition, illegal markets may be more profitable, enabling members to sustain themselves through alternative illicit activities without shifting into organized fraud. Across both specifications, the negative interaction between the YEOs and YCI remains significant, indicating that neither enforcement capacity nor illegal market profitability explains the heterogeneous effects.

6.2 Complementarity of Yakuza and Non-Yakuza Criminal Groups in Response to YEOs

Columns (4)–(7) of Table 5 examine potential complementarity between yakuza and non-yakuza criminal groups using the YCI and non-yakuza criminal group index. Our model assumes production complementarity between yakuza and non-yakuza criminal groups. While we cannot directly test this assumption, we examine its implications by analyzing the geographic and operational interdependence of these groups. Before testing this complementarity, Column (4) verifies that heterogeneous effects by YCI are not simply driven by variation in the presence of non-yakuza criminal groups. In Panel A, the YEO–YCI interaction remains significant, whereas the YEO–NYG (non-yakuza criminal group index) interaction is small and insignificant. In Panel B, the YEO–YCI interaction is not significant, while the YEO–NYG interaction is marginally significant but with the opposite sign.

Columns (5)–(7) incorporate a triple interaction between the YEOs, YCI, and NYG to test for complementarity. The relevant measure is the cross-partial derivative: $\frac{\partial^2 y_{pt}}{\partial \mathsf{YCI}_p \partial \mathsf{NYG}_p} = \widehat{\lambda}_4 \times \mathsf{YEO}_{pt}$. In Panel A, the positive and significant coefficient on the triple interaction term indicates that the marginal effect of yakuza competition increases with the presence of non-yakuza criminal groups under the YEOs, and vice versa. Column (6) confirms this result is robust to controlling for the enforcement channel, while Column (7) confirms robustness to the alternative illegal market channel. By contrast, Panel B consistently shows that none of the relevant coefficients are significant.

Taken together, these results indicate that yakuza and non-yakuza criminal groups act as complements in generating organized fraud damage in the post-YEOs period. When both are present, the capacity to organize larger-scale and more sophisticated schemes is enhanced, increasing financial damage.

Table 5: Heterogeneity

PANEL A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable: Financial Damage from Organized Fraud							
YEO	0.6680**	0.6673**	0.6556**	0.6786**	0.3054	0.3622	0.3202
	(0.2664)	(0.2665)	(0.2672)	(0.2697)	(0.2624)	(0.2701)	(0.2568)
YEO × YCI (SD)	-0.7961***	-0.8015***	-0.7302***	-0.7480***	-0.2869	-0.2522	-0.3106
	(0.2420)	(0.2452)	(0.2400)	(0.2567)	(0.2155)	(0.2211)	(0.2012)
YEO × NYG (SD)				-0.1241	-1.1425**	-1.3565***	-1.0069**
				(0.1438)	(0.4360)	(0.4570)	(0.4411)
YEO × YCI (SD) × NYG (SD)					1.0370**	0.9386**	0.9633**
					(0.4052)	(0.4342)	(0.3967)
YEO × Police per Capita (SD)		0.0463				0.3557	
		(0.1114)				(0.3096)	
YEO × Illegal Market Index (SD)			-0.2646				-0.1350
			(0.1797)				(0.2096)
R-squared	0.5261	0.5261	0.5270	0.5263	0.5290	0.5296	0.5292
PANEL B:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable: Number of Organized Fraud Cases							
YEO	0.0029	0.0025	0.0025	0.0016	-0.0072	-0.0003	-0.0050
	(0.0244)	(0.0231)	(0.0246)	(0.0240)	(0.0275)	(0.0258)	(0.0268)
YEO × YCI (SD)	-0.0170	-0.0200	-0.0149	-0.0227	-0.0118	-0.0076	-0.0152
	(0.0223)	(0.0217)	(0.0232)	(0.0227)	(0.0275)	(0.0248)	(0.0257)
YEO × NYG (SD)				0.0146*	-0.0094	-0.0352	0.0101
				(0.0083)	(0.0302)	(0.0274)	(0.0281)
YEO × YCI (SD) × NYG (SD)					0.0245	0.0127	0.0139
					(0.0295)	(0.0276)	(0.0276)
YEO × Police per Capita (SD)		0.0250***				0.0428*	
		(0.0087)				(0.0242)	
YEO × Illegal Market Index (SD)			-0.0087				-0.0195
			(0.0165)				(0.0147)
R-squared	0.6081	0.6091	0.6082	0.6085	0.6086	0.6094	0.6090
Observations	1,548	1,548	1,548	1,548	1,548	1,548	1,548

Outcome in Panel A: IHS of financial damage from organized fraud. **Outcome in Panel B:** IHS of organized fraud cases per 100,000 residents. **YCI:** Yakuza Competition Index, defined similarly to HHI as $YCI_p = 1 - \sum_g s_{pg}^2$. **NYG:** Non-Yakuza Criminal Group Index, a pre-YEOs proxy for non-yakuza criminal groups based on a LASSO Poisson model with pre-treatment covariates. This measure uses predicted incidence rates from the cross-validation selected model. **Police per capita:** number of police officers per 1,000 residents. **Illegal market index:** composite measure based on principal component analysis of prostitution arrests, methamphetamine arrests, and gun seizures (all per 100,000 residents). All indices are standardized with mean = 0 and standard deviation = 1. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

Robustness We assess the robustness of the heterogeneity in four ways. First, we re-estimate the effects by YCI in an event-study (Appendix G). We find increases in financial damage from organized fraud in the months following the YEOs' enactment in low-YCI prefectures (below the median), with limited evidence of pre-trends; by contrast, the effects are negligible in high-YCI prefectures (above the median). Second, we test alternative competition measures (Appendix I): (i) a binary indicator for above-median YCI; (ii) a YCI that assigns each group's members equally across the prefectures where it operates; (iii) a YCI that assigns members proportionally to population density;

and (iv) a polarization-style index following Montalvo and Reynal-Querol (2005). Results are stable across all definitions. Third, we test robustness using alternative measures of non-yakuza criminal groups (Appendix J): (i) linear predictor and (ii) predicted incidence rates using a fixed penalty parameter. Our results remain robust across these two alternative measures. Fourth, we control for concurrent enforcement shocks—the Anti-Yakuza Laws revision and U.S. Executive Order 13581—by interacting each with YCI and NYG (Appendix K). Our heterogeneous findings remain robust under these controls.

6.3 From Extortion to Fraud: Yakuza Economic Activity Substitution

We investigate whether current yakuza members shift from extortion, a traditional racket, into organized fraud in response to the YEOs. Since the data from our previous analyses cannot determine whether current yakuza members engage in organized fraud, we use an additional dataset documenting arrests of yakuza members in Tokyo. We use arrests of current yakuza members for fraud as a proxy for yakuza involvement in organized fraud, and arrests for extortion as a proxy for engagement in traditional criminal income sources. Given that the YEOs were designed to disrupt traditional yakuza revenue streams, we expect the ordinances to reduce arrests of yakuza members for extortion while increasing fraud-related arrests.

Arrest data may reflect unobservable changes in law enforcement intensity rather than actual criminal behavior. If the YEOs increase police enforcement against yakuza members, changes in arrests could reflect heightened police activity rather than shifts in yakuza behavior. Under this scenario, we would expect arrests to increase across multiple crime types, not just fraud. To test this possibility, we use a placebo outcome that satisfies two key criteria: (i) yakuza members occasionally commit this crime, but it falls outside the scope of the YEOs' regulation, and (ii) police officers can readily exercise enforcement discretion in these cases. Obstruction of public order meets both criteria: when yakuza members interfere with police duties, officers may choose whether to classify this behavior as obstruction. If increased police enforcement drives the observed arrest patterns, we should see corresponding increases in yakuza arrests for obstruction of public order. However, if obstruction arrests remain stable while fraud arrests increase and extortion arrests decrease, this would suggest behavioral changes among yakuza members rather than changes in police enforcement practices.

We use ward-level data on yakuza member arrests published annually by the Tokyo Metropolitan Police Department, focusing on 23 special wards from 2008 to 2013.[13] Our research design uses an interrupted time series analysis with ward-level fixed effects, exploiting the YEOs' enactment in Tokyo in 2011. A caveat is that without cross-sectional variation, the YEOs' effects are indistinguishable from other macro shocks affecting yakuza arrests. We are unaware of other events in 2011 that would shift yakuza behavior in Tokyo, but caution is needed when interpreting the magnitudes of the empirical results. As the arrest data are count data, we use Poisson pseudo-maximum

[13] We focus on the 23 special wards to reduce geographical heterogeneity across different jurisdiction types within Tokyo metropolis.

likelihood (PPML) estimation.[14] We regress the conditional expectation as follows:

$$\ln \mathbb{E}\left[\mathsf{Arrest}_{wt} \mid Z\right] = \sum_{\tau=2008, \tau \neq 2010}^{2013} \alpha_\tau \mathsf{YEO}_{t=\tau} + \eta X_{wt} + \mu_w + \varepsilon_{ty}, \tag{8}$$

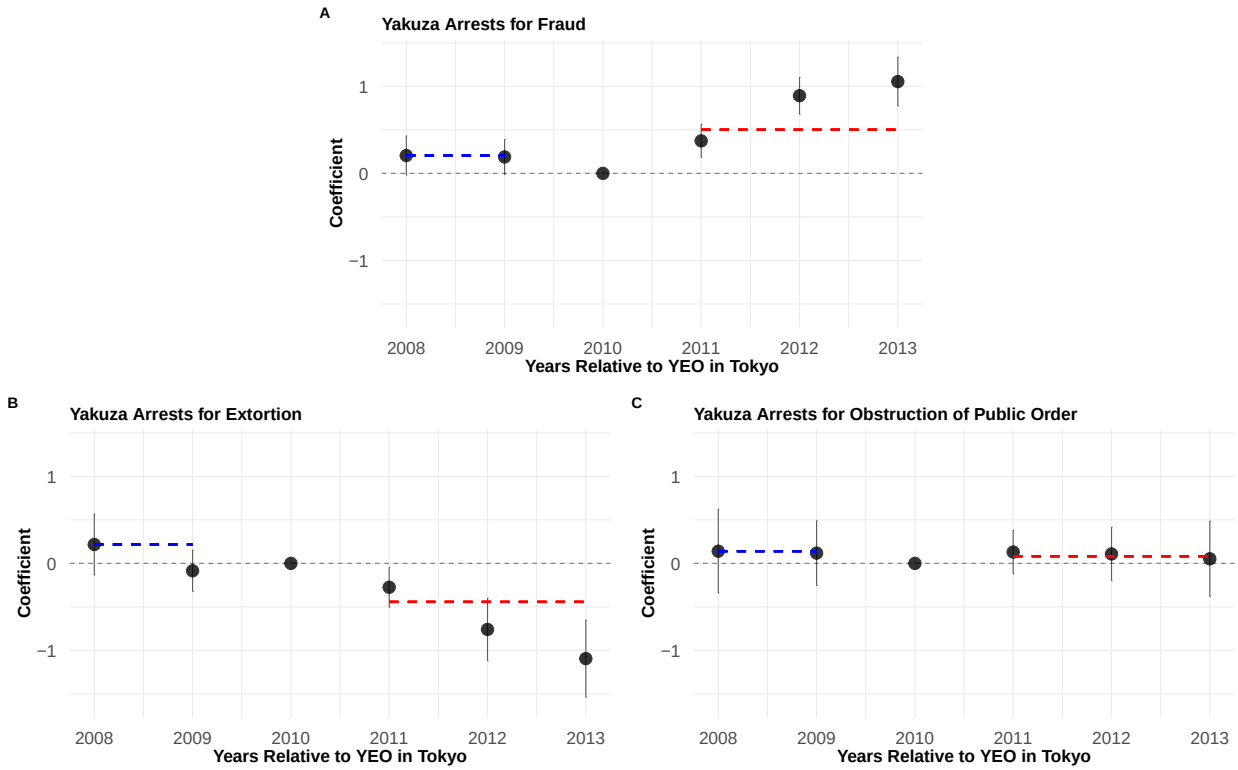
where Arrest_{wt} is the number of arrests of (current) yakuza members for fraud, extortion, or obstruction of public order in ward w in year t, and Z includes relevant covariates. Let $\mathsf{YEO}_{t=\tau}$ be the treatment indicator, which takes the value of 1 if $t = \tau$ and the value of 0 otherwise. We set 2010, one year before the YEOs' enactment in Tokyo, as the reference period. X_{wt} denotes the control variables, including the share of population aged 65+, the share of foreign population, the share of welfare-recipient households, and the log of taxable income. μ_w denotes ward-level fixed effects. ε_{wt} is an idiosyncratic error. Standard errors are clustered at the ward level.

Figure 4 reports our estimation results with 90% confidence intervals. In Tokyo, the number of arrests of yakuza members for fraud increased by more than 100% compared to the year prior to the enforcement of the YEOs. By contrast, the number of arrests for extortion decreased, down by more than 100% compared to the reference year. Lastly, our placebo outcome, yakuza arrests for obstruction of public order, did not change before and after the YEOs' enactment. Our analysis provides evidence suggesting that the YEOs did not increase the level of law enforcement but rather increased fraud committed by current yakuza members and decreased extortion, the type of crime that the yakuza would have exploited in the absence of the YEOs.

Lastly, we test multiple hypotheses based on $\ln \mathbb{E}\left[\mathsf{Arrest}_{wt} \mid Z\right] = \alpha_{\text{post}} \mathsf{YEO}_{t \geq 2011} + \sum_{\tau=2008,2009} \alpha_\tau \mathsf{YEO}_{t=\tau} + \eta X_{wt} + \mu_w + \varepsilon_{ty}$, where the coefficient of interest is α_{post}, and $\mathsf{YEO}_{t \geq 2011}$ takes the value of 1 after 2011 and the value of 0 before that. Westfall and Young's (1993) adjusted p-values are 0.005 for fraud arrests, 0.056 for extortion arrests, and 0.462 for obstruction of public order arrests.

[14] The Poisson model has several advantages (Wooldridge, 2010). It is consistent under mild distributional assumptions and does not suffer from the incidental parameters problem, allowing us to control for ward-level fixed effects.

Figure 4: YEOs' Effects on Arrests of Yakuza Members in Tokyo with 90% Confidence Intervals



Dashed red line: mean of the post-treatment coefficients. **Dashed blue line:** mean of the pre-treatment coefficients. **Outcome in Panel A:** number of yakuza arrests for fraud. **Outcome in Panel B:** number of yakuza arrests for extortion. **Outcome in Panel C:** number of yakuza arrests for obstruction of public order. **Controls:** share of population population aged 65+, share of foreign population, share of welfare-recipient households, and log of taxable income. **Observations:** 138. **Westfall and Young's (1993) adjusted p-values:** 0.005 (fraud), 0.056 (extortion), and 0.462 (obstruction of public order).

Other Results In addition to current yakuza members engaging in fraud under the YEOs, we also provide suggestive evidence of former yakuza members engaging in organized fraud in response to the YEOs. The intuition is that the YEOs reduce yakuza membership by raising the costs of affiliation but former yakuza members face poor employment prospects, leading them to engage in organized fraud. Using *changes* in the number of incarcerated yakuza members as a proxy for the number of former yakuza members, we find that decreases in incarcerated yakuza members are negatively associated with financial damage from organized fraud, suggesting that former yakuza members engage in organized fraud (Appendix B).

7 Concluding Remarks

This study examines the effects of the Yakuza Exclusion Ordinances (YEOs), which impose indirect economic sanctions on yakuza by prohibiting non-yakuza actors from providing benefits to them.

We analyze how the YEOs affect organized fraud, which has emerged as a new revenue source for organized crime groups.

We develop a model illustrating how illegal economic activity evolves following the enactment of the YEOs. Our model suggests that the YEOs increase financial damage from organized fraud by eliminating traditional revenue sources and pushing yakuza toward new types of crime—particularly organized fraud jointly operated with non-yakuza criminal groups. We further show that this increase in financial damage is primarily driven by a rise in per-case damage (i.e., greater sophistication per case) rather than an increase in case counts. In addition, the model predicts that the magnitude of this effect depends on competition among yakuza groups: the YEOs cause greater financial damage from organized fraud in areas with limited yakuza competition.

Guided by these theoretical insights, we implement a staggered difference-in-differences approach. The empirical findings are consistent with the model's predictions: the YEOs substantially increase financial damage from organized fraud, while their effect on the number of cases is small and statistically insignificant—at most marginally positive. These effects are stronger in prefectures with lower yakuza competition. Moreover, we find suggestive evidence that yakuza and non-yakuza criminal groups interact synergistically to generate greater financial damage from organized fraud under the YEOs. Lastly, using data on yakuza arrests in Tokyo, we find that yakuza arrests for extortion decline while yakuza arrests for fraud increase. The result is consistent with our prediction that the YEOs induce yakuza members to reallocate resources from traditional rackets toward organized fraud.

Our analysis offers broader insights into how indirect sanctions (that sever criminals' revenue streams by prohibiting associations between non-criminal actors and organized crime) can reshape criminal behavior. Although our analysis focuses on the yakuza context, this type of indirect intervention is relevant to many forms of organized crime whose revenues depend on interactions with non-criminal actors. Our findings highlight a policy trade-off: while indirect enforcement strategies can disrupt the traditional operations of organized crime groups, they may inadvertently redirect criminal activity toward more sophisticated and financially damaging schemes.

Finally, our findings have broader applicability to understanding the unintended consequences of enforcement interventions in other contexts. First, a shift from traditional rackets to new rackets may change the pool of potential victims from those previously connected to criminal networks to those with no prior links. For instance, Battiston et al. (2024) find that crackdowns on Mexican drug trafficking organizations shift their activity to oil theft, thereby exposing oil producers and energy companies to criminal predation. Second, enforcement-induced criminal substitution may be especially consequential when different organized crime groups play complementary roles in executing illicit activities. For instance, ransomware operations consist of specialized actors, such as initial access brokers, affiliates, and service providers, that perform complementary functions; when law enforcement targets one component, affiliates often switch to alternative ransomware brands rather than exit (National Cyber Security Centre, 2023). Third, the finding that enforcement targeting organized crime can displace rather than eliminate illicit activity underscores the need

to prevent criminal substitution. At the intensive margin, programs that support disengagement and reintegration of current affiliates of organized crime groups (e.g., rehabilitation for individuals disaffiliating from organized crime groups) can reduce the reallocation of effort toward alternative crimes. At the extensive margin, policies that lower entry into criminal markets are crucial: for example, Sviatschi (2022) shows that a conditional cash transfer for children exposed to illegal markets reduces subsequent criminal participation, mitigating these long-run, entry-side effects. Taken together, our study suggests that effectively combating organized crime requires a strategy that simultaneously disrupts existing operations while preventing both criminal substitution and the formation of new collaborations between different criminal groups.

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Online Appendix (Not for Publication)

Indirect Enforcement and the Transformation of Organized Crime: Evidence from the Yakuza

A Omitted Analysis of Model of Organized Fraud

A.1 Aumann–Shapley Value

Total profit from the joint enterprise depends on inputs from G yakuza groups and one non-yakuza group. We need an allocation rule that respects the Shapley-value logic but still works when inputs are continuous variables. The Aumann–Shapley value, which is a generalization of the Shapley value to non-atomic games, is exactly the desired sharing rule (Aumann and Shapley, 1974).

A.1.1 Definition

For an input vector $x = (K_1, \ldots, K_G, L) \in \mathbb{R}_+^{G+1}$, let $\Phi(x) \equiv \Pi(R(K, L))$, $R(K, L) = AK^\alpha L^\beta$, and $K = (\sum gK_g^\rho)^{1/\rho}$, where we fix $\rho = 2$ in the paper. Note that Φ is continuously differentiable on $\mathbb{R}_+^{G+1}$ and $\Phi(0) = 0$. For each coordinate $i \in \{1, \ldots, G, 0\}$ (with $i = 0$ denoting the non-yakuza group), we define the Aumann–Shapley payment by

$$\chi_i(x) \equiv x_i \int_0^1 \frac{\partial \Phi}{\partial x_i}(tx)\mathrm{d}t.$$

Then, we define the Aumann–Shapley value as

$$\varphi_i(x) \equiv \begin{cases} \chi_i(x)/\Phi(x) & \text{if } \Phi(x) > 0 \\ 0 & \text{if } \Phi(x) = 0. \end{cases}$$

The Aumann–Shapley value is known as a natural extension of the Shapley value. Recall that the Shapley value is defined for coalitional games in which each player decides whether to join a coalition or not (e.g., Osborne and Rubinstein, 1994, Section 14.4). The Aumann–Shapley value extends the concept of the Shapley value to non-atomic games—including our model where each player's input K_g, L is continuous. If we discretize each input into many identical "atoms" and apply the discrete Shapley value, then let atom size vanish, the expected marginal contribution converges to the above line integral along the path $t \mapsto tx$. This preserves the classic Shapley-value properties in the limit. Indeed, the Aumann–Shapley value satisfies, for instance, the efficiency, the symmetry, the linearity, and the invariance to null players.

A.1.2 Computation of Aumann–Shapley Value

We compute the Aumann–Shapley value for our setting to derive the sharing rule (3). By calculus,

$$\frac{\partial \Phi}{\partial K_g}(tx) = \Pi'(R(tx)) \cdot \frac{\partial R}{\partial K_g}(tx) = \Pi'(t^{\alpha+\beta}R) \cdot \alpha t^{\alpha+\beta-1} R \frac{K_g}{K^2}.$$

Hence the Aumann–Shapley payment to yakuza group $g = 1, \ldots, G$ is

$$\chi_g(x) = K_g \int_0^1 \frac{\partial \Phi}{\partial K_g}(tx) \mathsf{d}t = \alpha R \frac{K_g^2}{K^2} \int_0^1 t^{\alpha+\beta-1} \Pi'(t^{\alpha+\beta}R) \mathsf{d}t.$$

Substituting $u = t^{\alpha+\beta}$ so that $t^{\alpha+\beta-1}\mathsf{d}t = \mathsf{d}u/(\alpha+\beta)$, we have

$$\int_0^1 t^{\alpha+\beta-1} \Pi'(t^{\alpha+\beta}R) \mathsf{d}t = \frac{1}{\alpha+\beta} \int_0^1 \Pi'(uR) \mathsf{d}u = \frac{\Pi(R)}{(\alpha+\beta)R}.$$

Therefore,

$$\chi_g(x) = \frac{\alpha}{\alpha+\beta} \frac{K_g^2}{K^2} \Pi(R).$$

For the non-yakuza criminal group, we have

$$\frac{\partial \Phi}{\partial L}(tx) = \Pi'(t^{\alpha+\beta}R) \cdot \beta t^{\alpha+\beta-1} \frac{R}{L},$$

and thus

$$\chi_0(x) = L \int_0^1 \frac{\partial \Phi}{\partial L}(tx) \mathsf{d}t = \beta R \int_0^1 t^{\alpha+\beta-1} \Pi'(t^{\alpha+\beta}R) \mathsf{d}t = \frac{\beta}{\alpha+\beta} \Pi(R).$$

Therefore, we obtain the desired sharing rule (3):

$$\varphi_g(x) = \frac{\chi_g(x)}{\Pi(R)} = \frac{\alpha}{\alpha+\beta} \frac{K_g^2}{\sum_h K_h^2}, \qquad \varphi_0(x) = \frac{\chi_0(x)}{\Pi(R)} = \frac{\beta}{\alpha+\beta}.$$

A.2 Equilibrium Existence

Lemma 1. *There exists a pure-strategy subgame perfect equilibrium.*

Proof. By backward induction, we start with the stage-2 game. Given a resource R, the fraud enterprise maximizes its revenue (2). By substitution, we rewrite it as

$$\Pi(R) = \max_{n \in \mathbb{R}_+} \left\{ \mathsf{e}^{-cn} n \cdot (R-n)^q \right\}.$$

Since this objective is quasi-concave, it suffices to solve the first-order condition (FOC):

$$\frac{1}{n^*(R)} = c + \frac{q}{R - n^*(R)}. \tag{9}$$

Note that $0 < n^*(R) < R$ and thus $s^*(R) = R - n^*(R)$. Thus,

$$\Pi(R) = \mathsf{e}^{-cn^*(R)} n^*(R) \cdot \Big(R - n^*(R)\Big)^q. \tag{10}$$

To prove the equilibrium existence, it suffices to show that the first-stage game, with the revenue (10), has a pure-strategy Nash equilibrium. To use the Nash existence theorem (e.g., Osborne and Rubinstein, 1994, Proposition 20.3), we verify its assumptions: each player's action space is non-empty, compact, and convex; and each player's objective function is continuous in all players' actions and quasi-concave in her own action. Each player's action space $[0, 1]$ is compact and convex, and thus we only need to show that yakuza group g's objective Π_g is continuous in $(K_1, \ldots, K_G, L)$ and quasi-concave in K_g, where we recall that yakuza group g's payoff is

$$\Pi_g(K_1, \ldots, K_G, L) = \varphi_g \Pi(R) + (1 - K_g)r.$$

The continuity is immediate, and we focus on the quasi-concavity.

For the quasi-concavity, it suffices to show that both φ_g and $\Pi(R)$ are log-concave in K_g^2. Before we prove it, let us see why the log-concavity is sufficient. Once we establish the log-concavity, it follows that $\varphi_g \Pi(R)$ is log-concave in K_g^2 since the product of two log-concave function is log-concave, and thus it is quasi-concave in K_g^2. Moreover, since the mapping $K_g \mapsto K_g^2$ is increasing, the composite (thus, the first term of Π_g) is quasi-concave in K_g; moreover, as the second term is linear in K_g, it follows that Π_g is quasi-concave in K_g, as desired.

Now we establish the log-concavity. First, we prove that φ_g is log-concave in K_g^2. Letting $x \equiv K_g^2$ and $y \equiv \sum_{h \neq g} K_h^2$, we show that $x/(x + y)$ is log-concave in x. Since $x, y > 0$,

$$\frac{\mathsf{d}^2}{\mathsf{d}x^2} \log \frac{x}{x+y} = \frac{1}{(x+y)^2} - \frac{1}{x^2} < 0.$$

Second, we show that $\Pi(R)$ is log-concave in $x \equiv K_g^2$. It suffices to show that $\Pi(R)$ is log-concave in $z \equiv K^2$, which is an affine in x. It follows that

$$\frac{\partial^2}{\partial z^2} \log \Pi(R) = \underbrace{\left(\frac{\mathsf{d}^2}{\mathsf{d}R^2} \log \Pi(R)\right)}_{\leq 0} \underbrace{\left(\frac{\partial}{\partial z} R\right)^2}_{\geq 0} + \underbrace{\left(\frac{\mathsf{d}}{\mathsf{d}R} \log \Pi(R)\right)}_{\geq 0} \left(\frac{\partial^2}{\partial z^2} R\right).$$

Hence,

$$\frac{\partial^2}{\partial z^2} \log \Pi(R) \leq \underbrace{\left(\frac{\mathsf{d}}{\mathsf{d}R} \log \Pi(R)\right)}_{\geq 0} \times \frac{\alpha}{2}\left(\frac{\alpha}{2} - 1\right) L^\beta z^{\alpha/2 - 2} \leq 0,$$

where $0 < \alpha < 1$ by assumption. □

A.3 Proofs of Propositions 1 to 3

For the sake of presentation, we prove Proposition 2 first and then Propositions 1 and 3.

A.3.1 Proposition 2

By the FOC (9), we have $n^*(R) \leq 1/c$ and thus $R - n^*(R) \geq R - 1/c$. Plugging it back into (9),

$$\frac{1}{n^*(R)} = c + \frac{q}{R - n^*(R)} \leq c + \frac{q}{R - 1/c}.$$

Therefore,

$$\frac{1}{c + \dfrac{q}{R - 1/c}} \leq n^*(R) \leq 1/c.$$

As $R \to \infty$, the left-hand side approaches $1/c$.

A.3.2 Preliminaries

We state and prove preliminary results that we will use in the subsequent proofs. By the optimal revenue (10), we have

$$\Pi(R) = [e^{-cn^*(R)} n^*(R)] R^q \left(1 - \frac{n^*(R)}{R}\right)^q.$$

By Proposition 2, we have $n^*(R) \to 1/c$ as $R \to \infty$. It follows that $\mathsf{e}^{-cn^*(R)} n^*(R) \to \mathsf{e}^{-1}/c$ and $(1 - n^*(R)/R)^q \to 1$. Hence,

$$\Pi(R) = CR^q(1 + o_R(1)), \tag{11}$$

where $C = \mathsf{e}^{-1}/c$. By the envelope theorem, $\Pi'(R) = \mathsf{e}^{-cn^*(R)} n^*(R) q (R - n^*(R))^{q-1}$. As above,

$$\Pi'(R) = qCR^{q-1}(1 + o_R(1)). \tag{12}$$

Next, we consider the following expression:

$$\Pi'(R) + R\Pi''(R) = \Pi(R) \cdot \frac{q(Rn_R^*(R) - n^*(R) + qR)}{(R - n^*(R))^2},$$

where by the implicit function theorem used for the FOC (9),

$$n_R^*(R) \equiv \frac{\mathsf{d}n^*}{\mathsf{d}R}(R) = \frac{\frac{q}{(R-n^*)^2}}{\frac{1}{(n^*)^2} + \frac{q}{(R-n^*)^2}} = O(R^{-2}).$$

By Proposition 2, we have $n^*(R) \to 1/c$ as $R \to \infty$. Thus, $n_R^*(R) = O(R^{-2})$. By substitution,

$$\Pi'(R) + R\Pi''(R) \propto (1-t)^2(q-t) + qt^2(1-q+t).$$

where we write $t = n^*(R)/R \in [0,1]$. Since the right-hand side is cubic in t, it is straightforward to see that it is strictly positive for all t (and thus all R).

A.3.3 Proposition 1

Since the YEOs' enactment is interpreted as the profitability r of traditional rackets decreasing, this proposition is formalized as $\partial R^*/\partial r < 0$. By the first-order conditions, we have

$$R_r^* \equiv \frac{\partial R^*}{\partial r} = -\frac{\alpha}{r}\frac{2(1-H)\Pi(R^*) + \alpha H R^* \Pi'(R^*)}{2\alpha(1-H)\Pi'(R^*) + (\alpha^2 H + \beta^2)(\Pi'(R^*) + R^*\Pi''(R^*))}. \tag{13}$$

Since $\Pi(R) > 0$ and $\Pi'(R) > 0$ by (11) and (12), the numerator is strictly positive. Similarly, since $\Pi'(R) + R\Pi''(R) > 0$, the denominator is also strictly positive. It then follows that $R_r^* < 0$.

A.3.4 Proposition 3

The heterogeneity of the YEOs' effect is formalized as how the YEOs' effect $\partial\Pi(R^*)/\partial r$ itself changes as the YCIs vary. This proposition claims that when financial damage from organized fraud is large, the YEOs' effect should be weaker when the YCIs increase. Hence, it is formalized as

$$\frac{\partial}{\partial \mathsf{YCI}}\frac{\partial \Pi(R^*)}{\partial r} > 0.$$

We begin with preliminary observations. We rewrite the first-order condition $\partial\Pi_g/\partial K_g = 0$ for yakuza group g as

$$\frac{\alpha}{\alpha+\beta}\left[\frac{\partial \sigma_g}{\partial K_g}\Pi(R) + \sigma_g \Pi'(R)\frac{\partial R}{\partial K_g}\right] = r.$$

Since $\partial R/\partial K_g = \sigma_g \alpha R/K_g$ and $\partial \sigma_g/\partial K_g = 2\sigma_g(1-\sigma_g)/K_g$, the above condition is reduced to

$$\alpha\Big[2(\sigma_g - \sigma_g^2)\Pi(R) + \alpha\sigma_g^2 R\Pi'(R)\Big] = (\alpha+\beta) r K_g.$$

Summing both sides over all $g = 1, \ldots, G$, we have

$$2\alpha\Pi(R)(1-H) + \alpha^2 R\Pi'(R)H = (\alpha+\beta) r \sum_g K_g,$$

where we recall that $H = \sum_g \sigma_g^2$ denotes the HHI among the yakuza groups. By (11) and (12),

$$(\alpha+\beta)r\sum_g K_g = \alpha C R^q\Big(2(1-H)+\alpha q H\Big)(1+o_R(1)), \tag{14}$$

Similarly, we rewrite the first-order condition $\partial\Pi_0/\partial L = 0$ for the non-yakuza criminal group as

$$\beta\Pi'(R)\frac{\partial R}{\partial L} = (\alpha+\beta)w.$$

Since $\partial R/\partial L = \beta R/L$, we have $\beta^2 R\Pi'(R) = (\alpha+\beta)wL$. By (12),

$$(\alpha+\beta)wL = \beta^2 q C R^q(1+o_R(1)). \tag{15}$$

Recall that $K = \sqrt{H}\sum_g K_g$ by (1). Substituting (14) and (15) into $R = AK^\alpha L^\beta$, we have

$$R = AH^{\alpha/2}\left[\frac{\alpha C}{\alpha+\beta}\cdot\frac{R^q}{r}[2(1-H)+\alpha q H](1+o_R(1))\right]^\alpha\left[\frac{\beta^2 qC}{\alpha+\beta}\cdot\frac{R^q}{w}(1+o_R(1))\right]^\beta.$$

Hence,

$$R = \underbrace{AH^{\alpha/2}\Big(\frac{\alpha C}{\alpha+\beta}\Big)^\alpha\Big(\frac{\beta^2 qC}{\alpha+\beta}\Big)^\beta w^{-\beta}}_{=\Xi(H)>0}\left(\frac{2(1-H)+\alpha qH}{r}\right)^\alpha R^{q(\alpha+\beta)}(1+o_R(1)).$$

Equivalently,

$$R^{1-q(\alpha+\beta)} = \Xi(H)\left(\frac{2(1-H)+\alpha qH}{r}\right)^\alpha(1+o_R(1)).$$

Taking the log on both sides, we have

$$\frac{\partial \ln R}{\partial \ln r} = -\frac{\alpha}{1-q(\alpha+\beta)} + o_R(1).$$

Multiplying both sides by R/r, we have

$$\frac{\partial R}{\partial r} = -\frac{\alpha}{1-q(\alpha+\beta)}\frac{R}{r}(1+o_R(1)).$$

Adding (14) and (15), we have

$$(\alpha+\beta)\left(r\sum_g K_g + wL\right) = \underbrace{2\alpha(1-H)\Pi(R) + (\alpha^2 H+\beta^2)R\Pi'(R)}_{\equiv F(H,R)}, \tag{16}$$

By the implicit function theorem,

$$R^*_H \equiv \frac{\partial R^*}{\partial H} = -\frac{F_H}{F_R},$$

where by calculation

$$\begin{aligned} F_R &\equiv \frac{\partial F}{\partial R} = 2\alpha(1-H)\Pi'(R) + (\alpha^2 H + \beta^2)(\Pi'(R) + R\Pi''(R)), \\ F_H &\equiv \frac{\partial F}{\partial H} = \alpha(\alpha R\Pi'(R) - 2\Pi(R)). \end{aligned}$$

We have $F_R > 0$, because $\Pi'(R) > 0$ by (12) and $\Pi'(R) + R\Pi''(R) > 0$. Moreover, we have $F_H < 0$ for any sufficiently large R because

$$F_H = \alpha\Pi(R)\left(\alpha\frac{R\Pi'(R)}{\Pi(R)} - 2\right) = \alpha\Pi(R)\left(\alpha\frac{R\cdot qCR^{q-1}(1+o_R(1))}{CR^q(1+o_R(1))} - 2\right) < 0.$$

It follows that for any sufficiently large R, $R^*_H > 0$ and thus $R^*_{\mathsf{YCI}} < 0$.

Now we evaluate the cross derivative of interest. By a direct calculation,

$$\frac{\partial}{\partial \mathsf{YCI}}\frac{\partial \Pi(R^*)}{\partial r} = \Pi''(R^*)R^*_{\mathsf{YCI}}R^*_r + \Pi'(R^*)\frac{\partial R^*_r}{\partial \mathsf{YCI}}.$$

It suffices to obtain asymptotically formulas for R^*_r and $\partial R^*_r/\partial \mathsf{YCI}$.

Evaluating (13) by using (11) and (12), we obtain that

$$R^*_r = -\frac{\alpha}{1-q(\alpha+\beta)}\frac{R}{r}(1+O(R^{-1})).$$

Hence,

$$\frac{\partial R^*_r}{\partial \mathsf{YCI}} = \frac{\alpha}{1-q(\alpha+\beta)}\frac{1}{r}R_H(1+O(R^{-1})).$$

Substituting these two, we obtain that for large enough

$$\frac{\partial}{\partial \mathsf{YCI}}\frac{\partial \Pi(R^*)}{\partial r} = -\frac{\alpha}{1-q(\alpha+\beta)}\frac{1}{r}R_{\mathsf{YCI}}[\Pi'(R) + R\Pi''(R)](1+O(R^{-1})).$$

This is strictly positive for any sufficiently large $R > 0$, because $R_{\mathsf{YCI}} < 0$ and $\Pi'(R) + R\Pi''(R) > 0$.

B Fraud by Former Yakuza Members

We argue that former yakuza members, whose numbers have increased due to the YEOs, are involved in organized fraud. Building on Hoshino and Kamada (2021), who document that the YEOs reduce the number of current yakuza members, we proxy the number of former members by the post-YEO decline in current membership. Intuitively, if the YEOs push members out of yakuza groups, then larger post-YEO declines in current membership correspond to greater exposure to former members.[15] Our data do not distinguish outflows from inflows, but this limitation is unlikely to compromise our analysis. In 2010, the year before the YEOs' enactment, 67% of yakuza members were aged 40 or older (National Police Agency, 2015). Given that yakuza initiation typically begins at younger ages (Hirosue, 2014), this age composition suggests minimal recruitment during our study period. Therefore, declines in membership likely reflect increased outflows rather than reduced inflows.

Specification We test this hypothesis by regressing organized fraud on changes in yakuza membership, acknowledging that the analysis is suggestive. If former members engage in organized fraud, we should observe a negative association between the change in yakuza rates since the YEOs' enactment and the level of organized fraud. Our focus is thus on *changes* in the independent variable and *levels* of the dependent variable. Our regression model is:

$$\mathsf{Fraud}_{pt} = \delta_1 \Delta_{pt} + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}. \tag{17}$$

We allow the effect of the IHS-transformed change in yakuza rates to vary with the YCI by including the interaction term $\Delta_{pt} \times \mathsf{YCI}_p$. We further allow for complementarity between yakuza and non-yakuza criminal groups in response to a decline in the yakuza rate by including $\Delta_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p$ and other relevant interactions:

$$\mathsf{Fraud}_{pt} = \delta_1' \Delta_{pt} + \delta_2' \Delta_{pt} \times \mathsf{YCI}_p + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}. \tag{18}$$

$$\begin{aligned}\mathsf{Fraud}_{pt} = {} & \delta_1'' \Delta_{pt} + \delta_2'' \Delta_{pt} \times \mathsf{YCI}_p + \delta_3'' \Delta_{pt} \times \mathsf{NYG}_p + \delta_4'' \Delta_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p \\ & + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}.\end{aligned} \tag{19}$$

The previous results indicate that the YEOs' effects are greater in prefectures with lower YCI. Along the lines of these results, the interaction term $\Delta_{pt} \times \mathsf{YCI}_p$ should have a positive coefficient $\delta_2' > 0$ because when former-member exposure is higher ($\Delta_{pt} < 0$), greater competition (higher YCI) dampens the effect of Δ_{pt}—thus, the effect is stronger at lower YCI. The triple interaction term $\Delta_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p$ should have a negative coefficient $\delta_4'' < 0$ because where non-yakuza groups are more prevalent, this YCI-based damping is weaker, consistent with complementarity between yakuza and non-yakuza groups.

[15] The number of former yakuza members is unknown, but the number of current members is observed. Since the YEOs reduce current yakuza membership, this implies an increase in former members. We therefore use the change in current membership as a proxy for the change in former membership.

Data We use yakuza incarceration data from the Ministry of Justice. This dataset categorizes incarcerated yakuza members by rank (high-ranking, regular-ranking, and unknown rank). High-ranking members are executives who hold leadership positions within the yakuza group. Regular members are full-fledged yakuza who maintain clear hierarchical relationships within the group. The unknown-rank category is more heterogeneous. According to our interview with the Ministry of Justice, the unknown-rank category encompasses (i) peripheral yakuza members who may not be fully initiated or hold marginal positions within the organization; (ii) yakuza members potentially affiliated with other organized crime groups, such as non-yakuza criminal groups; and (iii) those transitioning between ranks. Given our hypothesis that members who resign are likely to engage in fraud, the unknown-rank category is relevant for our analysis.

We measure rank-specific incarcerated individuals per 100,000 population using the inverse hyperbolic sine (IHS) transformation. Let $\text{asinh}(\mathit{Yakuza}_{pt})$ denote the IHS-transformed yakuza rate, and define

$$\Delta_{pt} = \begin{cases} 0 & t < \tau_p, \\ \text{asinh}(\mathit{Yakuza}_{pt}) - \text{asinh}(\mathit{Yakuza}_{p}) & t \geq \tau_p, \end{cases}$$

where τ_p is the YEOs' enactment month in prefecture p, and $\text{asinh}(\mathit{Yakuza}_{p}) = \text{asinh}(\mathit{Yakuza}_{p\tau_p})$ is the enactment-month baseline. This construction sets exposure to zero before the YEOs' enactment and, afterward, measures exposure as the deviation from the enactment-month baseline, aligning with our mechanism: before the enactment there is no policy-induced outflow; after the enactment, exposure to former members is proportional to how far current membership has fallen from its enactment-month level.

Prefecture-level membership counts are annual. To exploit within-year timing around the enactment date τ_p, we linearly interpolate the annual series to months before applying the transformation. This adds temporal variation but imposes linearity within each year, which may smooth high-frequency fluctuations and attenuate short-term effects. We therefore interpret monthly patterns with appropriate caution.

Remark We make a remark on the withdrawal of yakuza members from their groups. It is widely recognized that resigning from yakuza groups is prohibitively difficult, as such attempts often expose individuals to serious threats or violence. This concern may raise doubts about the reliability of the analysis or data. However, it is documented that the number of yakuza members has been steadily declining in recent years, as mentioned above. Moreover, the withdrawal of members from yakuza groups is formally supported through institutionalized programs. Each prefecture operates an Anti-Organized Crime Campaign Center (*Bōtsui Center*), which helps individuals seeking to leave yakuza groups. These centers work in coordination with the police, public employment offices, and related agencies under an integrated framework designed to facilitate social reintegration. The legal foundation for these initiatives is established by the Anti-Yakuza Laws (*Bōryokudan Taisaku Hō*),

which prohibit interference with withdrawal and mandate administrative efforts to assist former members in pursuing lawful livelihoods.

Result Table A1 reports estimates. Panel A presents financial damage from organized fraud, and Panel B presents the number of cases. In each panel, Columns (1)–(3) use incarceration of high-ranking members, Columns (4)–(6) regular members, and Columns (7)–(9) unknown-rank members.

The results are partly consistent with our hypothesis for regular and unknown ranks, but not for high-ranking members. For high-ranking members, the change in incarceration is positively associated with financial damage from organized fraud, and this positive association strengthens where yakuza competition is higher.

For regular-rank members, Column (4) shows a negative but insignificant association between the change in incarceration and financial damage from organized fraud. Column (5) shows that the interaction with YCI is positive and significant, indicating that more former regular-rank members engage in organized fraud in low-YCI prefectures, consistent with our main findings. Column (6) shows that the triple interaction (regular-rank incarceration $\times$ YCI $\times$ NYG) is negative, suggesting complementarity between yakuza and non-yakuza groups when exposure to former members increases.

For unknown-rank members, Column (7) shows a negative association between the change in incarceration and financial damage from organized fraud, albeit only marginally significant. This suggests that former unknown-rank members—potentially affiliated with non-yakuza groups—engage in organized fraud following the YEOs' enactment. Column (8) examines heterogeneity by YCI, showing a positive but insignificant coefficient. Column (9) indicates potential complementarity between YCI and NYG in response to increased exposure to former unknown-rank members.

Overall, while the results are somewhat noisy, they suggest that former yakuza members—especially those facing limited opportunities under the YEOs—engage in organized fraud in patterns consistent with the roles of YCI and NYG.

Table A1: Changes in Yakuza Members and Organized Fraud

Panel A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Financial Damage from Organized Fraud									
Δ Yakuza Exposure (High-Ranking)	1.3547	1.4462*	3.1243***						
	(0.8755)	(0.8222)	(0.7964)						
Δ Yakuza Exposure (High-Ranking) × YCI (SD)		1.4023	-0.8773						
		(0.9598)	(1.0712)						
Δ Yakuza Exposure (High-Ranking) × NYG (SD)			4.7268**						
			(1.8310)						
Δ Yakuza Exposure (High-Ranking) × YCI (SD) × NYG (SD)			-4.5125***						
			(1.5658)						
Δ Yakuza Exposure (Regular)				-0.5241	-0.0682	1.6647*			
				(0.7285)	(0.7249)	(0.8591)			
Δ Yakuza Exposure (Regular) × YCI (SD)					1.1903*	-0.7757			
					(0.6051)	(0.9374)			
Δ Yakuza Exposure (Regular) × NYG (SD)						2.9531**			
						(1.1689)			
Δ Yakuza Exposure (Regular) × YCI (SD) × NYG (SD)						-4.6579***			
						(1.4095)			
Δ Yakuza Exposure (Unknown Rank)							-1.4903*	-1.6105	-0.4330
							(0.7632)	(0.9898)	(1.0813)
Δ Yakuza Exposure (Unknown Rank) × YCI (SD)								-0.1388	-2.6462**
								(0.7700)	(1.2372)
Δ Yakuza Exposure (Unknown Rank) × NYG (SD)									3.0637
									(2.2458)
Δ Yakuza Exposure (Unknown Rank) × YCI (SD) × NYG (SD)									-5.5675**
									(2.4818)
R-squared	0.5172	0.5187	0.5203	0.5166	0.5190	0.5229	0.5176	0.5176	0.5196
Panel B:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Number of Organized Fraud Cases									
Δ Yakuza Exposure (High-Ranking)	0.1120	0.1048	0.3795***						
	(0.0899)	(0.0895)	(0.0918)						
Δ Yakuza Exposure (High-Ranking) × YCI (SD)		-0.1109	-0.4707***						
		(0.0696)	(0.1106)						
Δ Yakuza Exposure (High-Ranking) × NYG (SD)			0.6964***						
			(0.2304)						
Δ Yakuza Exposure (High-Ranking) × YCI (SD) × NYG (SD)			-0.7653***						
			(0.2016)						
Δ Yakuza Exposure (Regular)				-0.0540	-0.1141	-0.0873			
				(0.0743)	(0.0799)	(0.0988)			
Δ Yakuza Exposure (Regular) × YCI (SD)					-0.1570**	-0.0893			
					(0.0656)	(0.1091)			
Δ Yakuza Exposure (Regular) × NYG (SD)						-0.2244*			
						(0.1219)			
Δ Yakuza Exposure (Regular) × YCI (SD) × NYG (SD)						-0.0907			
						(0.1808)			
Δ Yakuza Exposure (Unknown Rank)							0.0819	-0.0329	-0.1010
							(0.0758)	(0.0890)	(0.1133)
Δ Yakuza Exposure (Unknown Rank) × YCI (SD)								-0.1326*	-0.3555***
								(0.0776)	(0.1257)
Δ Yakuza Exposure (Unknown Rank) × NYG (SD)									-0.3631*
									(0.1991)
Δ Yakuza Exposure (Unknown Rank) × YCI (SD) × NYG (SD)									-0.6530***
									(0.2068)
R-squared	0.6085	0.6095	0.6147	0.6082	0.6124	0.6156	0.6083	0.6092	0.6136
Observations	1,548	1,548	1,548	1,548	1,548	1,548	1,548	1,548	1,548

Δ Yakuza Exposure: the difference in the number of incarcerated yakuza members between time periods. Before the YEOs' enactment, it is defined as zero, but equals the difference after enactment. Incarceration data are available annually; we linearly interpolate to create monthly-level data. Rank: High-ranking members are executives who hold leadership positions within the yakuza group. Regular members are full-fledged yakuza who maintain clear hierarchical relationships within the group. The unknown-rank category includes (i) peripheral yakuza members, (ii) yakuza members potentially affiliated with other organized crime groups, such as non-yakuza criminal groups, and (iii) those transitioning between ranks.

C Alternative Difference-in-Differences Estimators

Recent developments in the DiD literature reveal that treatment effects may be biased when treatment timing is staggered across units and treatment effects are heterogeneous (Goodman-Bacon, 2021). This section examines the robustness of our event-study results using four recently developed estimators: (i) Sun and Abraham (2021), (ii) De Chaisemartin and d'Haultfoeuille (2024), (iii) Callaway and Sant'Anna (2021), and (iv) Gardner (2022). To be consistent with the main specification, we account for observed covariates. Since some estimators do not allow for covariate inclusion, we use residuals from regressing the outcome on observed covariates. Table A2 presents the results, which qualitatively corroborate the main findings in Table 3.

Table A2: Effects of the YEOs on Organized from Alternative DiD Estimators

Panel A:	(1)	(2)	(3)	(4)
Dependent Variable: Financial Damage from Organized Fraud				
YEO	0.6618**	2.2662*	2.7689***	1.4928***
	(0.2887)	(1.1958)	(0.8708)	(0.3598)
Panel B:	(1)	(2)	(3)	(4)
Dependent Variable: Number of Organized Fraud Cases				
YEO	-0.0028	0.0297	0.0865	0.0580
	(0.0247)	(0.0531)	(0.0647)	(0.0364)

Column (1): Sun and Abraham (2021)

Column (2): De Chaisemartin and d'Haultfoeuille (2024)

Column (3): Callaway and Sant'Anna (2021)

Column (4): Gardner (2022)

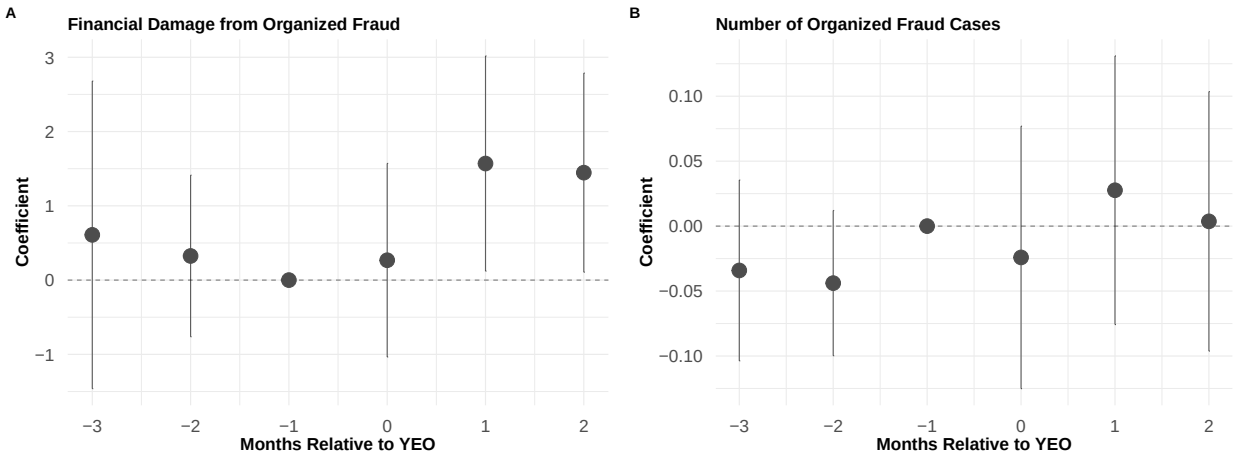
D Stacked Event-Study Design

Standard two-way fixed-effects event studies can inadvertently use already-treated units as controls when treatment is staggered. To avoid this, we implement a stacked event-study design that builds separate comparisons for each treatment cohort, using only not-yet-treated prefectures as controls within each cohort's calendar window.

Specifically, for each YEO enactment month, we form a self-contained cohort block covering a common event window around that cohort's treatment date. Within each block, treated prefectures are compared only to prefectures that have not yet enacted the YEOs but will do so later. Because YEO enactments occur within a relatively short time horizon, we use a narrow window from three months before to two months after enactment for each cohort. We then stack these cohort-specific datasets into a single combined dataset.

Our stacked specification includes cohort-by-prefecture fixed effects and cohort-by-calendar-month fixed effects, ensuring that all comparisons are made within cohort and common time periods. Standard errors are clustered at the prefecture level. Figure A1 presents stacked event-study estimates and 90% confidence intervals, confirming the main findings.

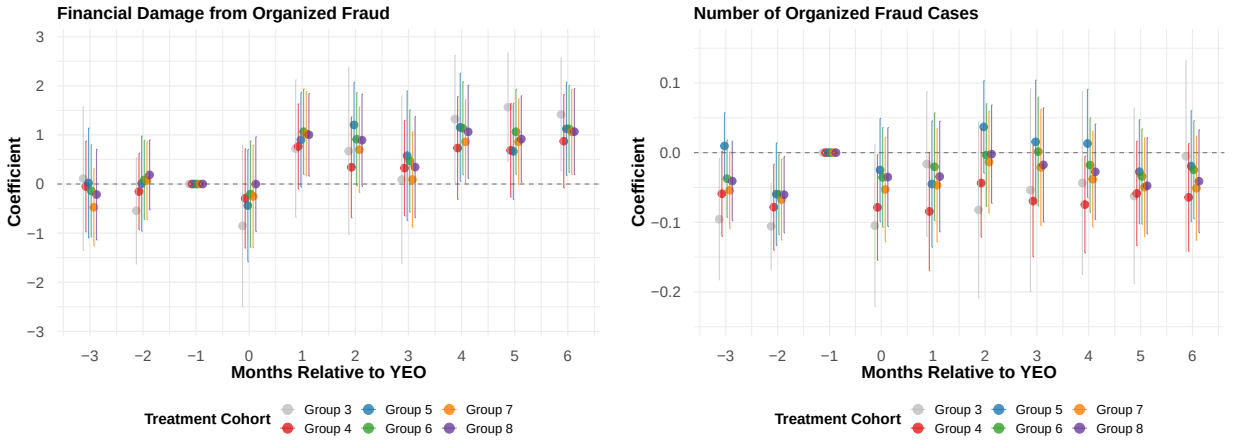
Figure A1: Stacked Event-Study Estimates



E Event Study with Leaving Out One Treatment Cohort at a Time

We run an additional test to see if the YEOs' effects are driven by particular treatment timing groups. Because we are concerned with potential bias in the presence of treatment effect heterogeneity in staggered DiD settings, we examine whether the YEOs' effects are similar across treatment cohort groups by excluding one treatment cohort at a time. Figure A2 reports the results. Each column corresponds to a specification that excludes one treatment timing group from the sample (and uses the remaining seven). The exclusion of any treatment cohort hardly changes the behavior of the outcome.

Figure A2: Event Study with Leaving Out One Treatment Cohort at a Time

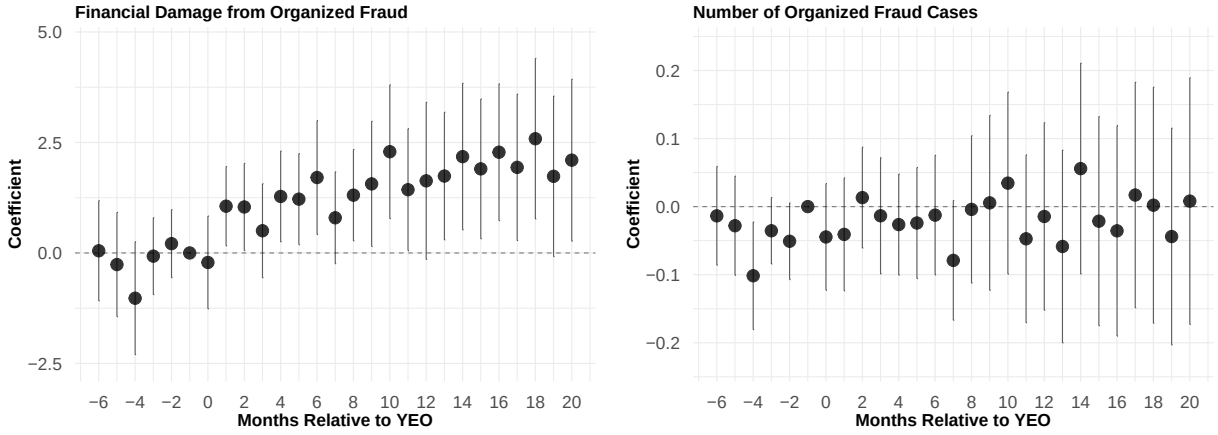


Treatment Cohort: Group 3 (Hokkaido, Miyagi, Ibaraki, Tochigi, Gunma, Kanagawa, Yamanashi, Fukui, Gifu, Aichi, Mie, Kyoto, Osaka, Hyogo, Tottori, Shimane, Okayama, Hiroshima, Yamaguchi, Tokushima, Kagawa, Kochi, Kumamoto, Oita); Group 4 (Aomori, Iwate, Akita, Fukushima, Nara, Wakayama); Group 5 (Yamagata, Saitama, Niigata, Shizuoka, Toyama, Ishikawa, Shiga, Miyazaki); Group 6 (Chiba, Nagano); Group 7 (Tokyo, Okinawa); and Group 8 (Saga).

F Event Study with an Extended Time Window

We examine two competing forces through which the YEOs affect organized fraud over time. On the one hand, police dissemination of fraud-related information may heighten public awareness and reduce victimization rates, thereby lowering the incidence of fraud cases. On the other hand, as our theoretical model demonstrates, criminals may respond by developing more sophisticated schemes that enable them to extract larger sums from each victim. Figure A3 presents event-study estimates using an extended time window, revealing that financial losses from organized fraud remain significantly elevated 20 months after the YEOs' enactment, while the estimated effects on the number of fraud cases are not statistically distinguishable from zero.

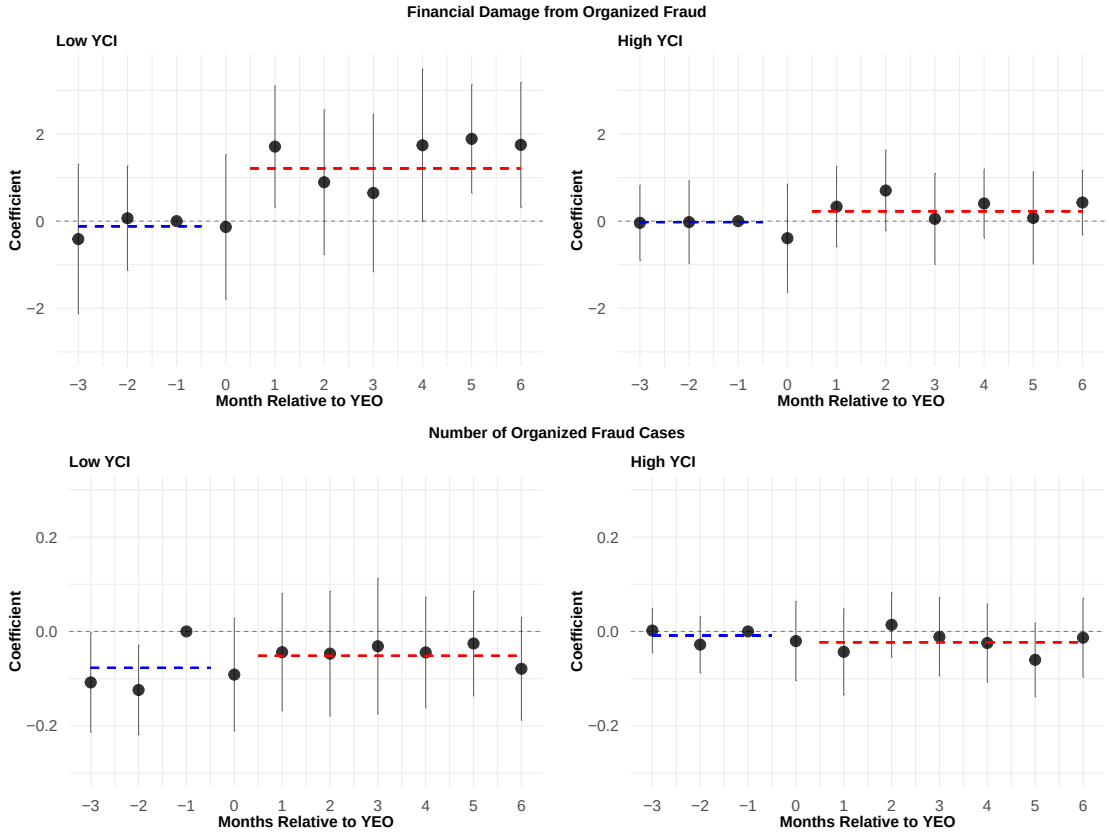
Figure A3: Event Study with an Extended Time Window



G Heterogeneity of YEOs' Effects by YCI

Table 5 shows the heterogeneous effects of the YEOs by YCI using a difference-in-differences specification. Here we present the results in event-study form, which allows us to test for pre-trends in the outcome variables. Figure A4 presents event-study results by YCI level. Low YCI is defined as below the median value, while high YCI is defined as above the median. The top panel shows financial damage from organized fraud, and the bottom panel shows the number of organized fraud cases. Large increases in financial damage are observed in low-YCI prefectures following the YEOs' enactment, while such effects are absent in high-YCI prefectures. The results for the number of cases show no significant effects in either low-YCI or high-YCI prefectures during the post-YEOs period.

Figure A4: Event Study by YCI



Low YCI: defined as below-median YCI. **High YCI:** defined as above-median YCI.

H Flexible Time Fixed Effects via Double-Lasso

Column (5) of Table 3, where we add month-by-year fixed effects, yields a slightly larger point estimate but also larger standard errors, reflecting the substantial variation absorbed by this restrictive specification. To address this issue, we use double-LASSO techniques (Belloni et al., 2014) to obtain more parsimonious time controls. Double-LASSO provides a systematic method for covariate selection by identifying the most relevant controls through an optimization procedure that penalizes large coefficients while shrinking irrelevant parameters to zero. This approach results in a more parsimonious model that focuses on the most important control variables. Table A3 presents the results, showing a slightly larger coefficient and smaller standard errors compared to Column (5) of Table 3.

Table A3: Flexible Time Fixed Effects via Double-Lasso

Panel A:	(1)
Dependent Variable:	Financial Damage of Organized Fraud
YEO	0.8093**
	(0.4109)
Panel B:	(1)
Dependent Variable:	Number of Organized Fraud Cases
YEO	0.0164
	(0.0393)

I Robustness Check to Alternative Measures of Yakuza Competition

We examine the robustness of the heterogeneous effects to alternative measures of yakuza competition. Our main specification uses a yakuza competition index (YCI) where the share of each yakuza group is defined proportionally to prefecture-level population.

- **Binary Competition Measure.** Columns (1-2) test robustness using a binary indicator equal to one for prefectures with above-median YCI values.
- **Alternative Distribution Assumptions.** The YCI requires computing the share s_{pg} of yakuza group g in prefecture p. Our main analysis assumes members of organization g are distributed proportionally to population across all prefectures where g operates, yielding YCI^{pop} (used in the main text). We test two alternative assumptions: First, we assume members of g are distributed equally among all prefectures where g operates, yielding share s_{pg}^{naive} and the resulting index $\text{YCI}_p^{\text{naive}}$ (Columns 3-4). Second, we assume members are distributed proportionally to population density, yielding $s_{pg}^{\text{pop density}}$ and $\text{YCI}_p^{\text{pop density}}$ (Columns 5-6).
- **Alternative Index Definition.** Columns (7-8) report results using a YCI defined analogously to the Ethnic Polarization Index (Montalvo and Reynal-Querol, 2005).
- **Yakuza Group Count.** Columns (9–10) use the raw number of designated yakuza groups operating in each prefecture.

Table A4 presents the results. All specifications yield results consistent with our main analysis, confirming the robustness of our findings to alternative measures of yakuza competition.

Table A4: Robustness to Alternative Measures of Yakuza Competition

PANEL A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable: Financial Damage from Organized Fraud										
YEO	1.4624***	0.8422**	0.6550**	0.3387	0.6862**	0.2308	0.6854**	0.3648	0.6713**	0.3464
	(0.4360)	(0.4154)	(0.2652)	(0.2675)	(0.2674)	(0.2671)	(0.2720)	(0.2725)	(0.2674)	(0.2588)
YEO × NYG		-2.0051**		-1.0254**		-1.3494***		-0.8370***		-1.0329**
		(0.8076)		(0.4312)		(0.4749)		(0.2925)		(0.4348)
YEO × **1** (YCI > median)	-1.3018***	-0.6597								
	(0.4740)	(0.4648)								
YEO × **1** (YCI > median) × NYG		1.8803**								
		(0.8044)								
YEO × $\text{YCI}^{\text{pop density}}$			-0.7978***	-0.3253						
			(0.2423)	(0.2247)						
YEO × $\text{YCI}^{\text{pop density}}$ × NYG				0.9480**						
				(0.4060)						
YEO × $\text{YCI}^{\text{naive}}$					-0.7992***	-0.2254				
					(0.2448)	(0.2190)				
YEO × $\text{YCI}^{\text{naive}}$ × NYG						1.2030***				
						(0.4372)				
YEO × EP							-0.6687***	-0.1515		
							(0.2372)	(0.1866)		
YEO × EP × NYG								1.3445**		
								(0.5168)		
YEO × # of Designated Yakuza Groups									-0.8306***	-0.5706**
									(0.2196)	(0.2216)
YEO × # of Designated Yakuza Groups × NYG										0.6696**
										(0.2567)
R-squared	0.5236	0.5276	0.5262	0.5288	0.5260	0.5294	0.5247	0.5279	0.5262	0.5294
PANEL B:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable: Number of Organized Fraud Cases										
YEO	0.0240	0.0067	0.0026	-0.0038	0.0031	-0.0101	0.0032	0.0013	0.0031	-0.0102
	(0.0389)	(0.0392)	(0.0243)	(0.0267)	(0.0244)	(0.0288)	(0.0246)	(0.0267)	(0.0246)	(0.0255)
YEO × NYG		-0.0530		0.0005		-0.0163		0.0096		-0.0171
		(0.0330)		(0.0283)		(0.0342)		(0.0200)		(0.0254)
YEO × **1** (YCI > median)	-0.0351	-0.0231								
	(0.0462)	(0.0467)								
YEO × **1** (YCI > median) × NYG		0.0686**								
		(0.0340)								
YEO × $\text{YCI}^{\text{pop density}}$			-0.0173	-0.0162						
			(0.0222)	(0.0266)						
YEO × $\text{YCI}^{\text{pop density}}$ × NYG				0.0148						
				(0.0286)						
YEO × $\text{YCI}^{\text{naive}}$					-0.0199	-0.0126				
					(0.0228)	(0.0291)				
YEO × $\text{YCI}^{\text{naive}}$ × NYG						0.0312				
						(0.0325)				
YEO × EP							-0.0149	-0.0158		
							(0.0203)	(0.0252)		
YEO × EP × NYG								0.0044		
								(0.0399)		
YEO × # of Designated Yakuza Groups									-0.0164	-0.0184
									(0.0229)	(0.0242)
YEO × # of Designated Yakuza Groups × NYG										0.0236
										(0.0159)
R-squared	0.6081	0.6088	0.6082	0.6085	0.6082	0.6088	0.6081	0.6083	0.6081	0.6089
Observations	1,548	1,548	1,548	1,548	1,548	1,548	1,548	1,548	1,548	1,548

1{YCI$_p$ > median(YCI)}: 1 for prefectures above the sample median of YCI (Column 1). **$\text{YCI}^{\text{pop density}}$**: Competition index computed using shares $s_{pg}^{\text{pop density}}$ proportional to population density across prefectures where group g operates (Column 2). **$\text{YCI}^{\text{naitve}}$**: Competition index computed using equal shares $s_{pg}^{\text{naive}} = 1/N_g$ across prefectures where group g operates (Column 3). **EP**: Polarization-style index analogous to the ethnic polarization measure Montalvo and Reynal-Querol (2005) (Column 4). **# Designated Yakuza Groups**: number of designated yakuza groups operating in prefecture (Column 5).

J Robustness Check to Alternative Measures of Non-Yakuza Criminal Groups

As detailed in Appendix N, our main measure uses predicted values of non-yakuza criminal groups from a Poisson LASSO model with 10-fold cross-validation. As robustness checks, we examine two alternative measures: (i) the linear predictor from the cross-validation-selected model, which captures predictions on the log scale rather than the count scale, and (ii) predicted incidence rates using a fixed penalty parameter ($\lambda = 0.1$) rather than the cross-validation-selected value, which tests sensitivity to the regularization strength.

Table A5 presents the results of these robustness checks. Across both alternative measures, the coefficient on the interaction term between the YEOs, YCI, and non-yakuza criminal group index remains positive and significant, confirming the complementarity between yakuza and non-yakuza criminal groups.

Table A5: Robustness to Alternative Measures of Non-Yakuza Criminal Group Index

Panel A:	(1)	(2)
Dependent Variable: Financial Damage from Organized Fraud		
YEO × YCI (SD)	-0.3063	-0.2869
	(0.2040)	(0.2155)
YEO × NYG (Linear Predictor, SD)	-0.9104***	
	(0.3227)	
YEO × YCI (SD) × NYG (Linear Predictor, SD)	0.7696**	
	(0.2979)	
YEO × NYG (Fixed, SD)		-1.1425**
		(0.4360)
YEO × YCI (SD) × NYG (Fixed, SD)		1.0370**
		(0.4052)
R-squared	0.5295	0.5290
Panel B:	(1)	(2)
Dependent Variable: Number of Organized Fraud Cases		
YEO × YCI (SD)	-0.0100	-0.0118
	(0.0275)	(0.0275)
YEO × NYG (Linear Predictor, SD)	-0.0096	
	(0.0236)	
YEO × YCI (SD) × NYG (Linear Predictor, SD)	0.0219	
	(0.0237)	
YEO × NYG (Fixed, SD)		-0.0094
		(0.0302)
YEO × YCI (SD) × NYG (Fixed, SD)		0.0245
		(0.0295)
R-squared	0.6085	0.6086
Observations	1,548	1,548

NYG (Linear Predictor): Non-Yakuza Criminal Group Index, a pre-YEOs proxy for non-yakuza criminal groups based on a LASSO Poisson model with pre-treatment covariates. This measure uses the linear predictor (log scale) from the cross-validation selected model. **NYG (Fixed)**: Non-Yakuza Criminal Group Index, a pre-YEOs proxy for non-yakuza criminal groups based on a LASSO Poisson model with pre-treatment covariates. This measure uses predicted incidence rates with a fixed penalty parameter ($\lambda = 0.1$).

K Robustness Check to Additional Enforcement When Examining Heterogeneity

We examine the robustness of the heterogeneous effects to alternative measures of enforcement. In the main text, we test the robustness using baseline police per capita interacted with the YEO. In this section, we test two alternative enforcement measures that occurred during our study period:

- Anti-Yakuza Laws Revision (October 2012): The Anti-Yakuza Laws were amended in October 2012. This revision expanded the designation of yakuza groups in conflict and imposed additional restrictions on their activities, including limitations on using their own offices. We create a treatment variable AYL_t, which equals 1 after October 2012 and 0 otherwise. We then include the corresponding interaction terms with YCI and NYG.
- U.S. Executive Order 13581 (July 2011): Executive Order 13581, issued by the Obama administration in July 2011, freezes the property and assets of transnational organized crime groups, explicitly including the yakuza. We create a treatment variable $\mathsf{USEnforcement}_t$, which equals 1 after July 2011 and 0 otherwise, and include the corresponding interaction terms with YCI and NYG.

Table A6 presents the results of these robustness checks. Across all specifications, the main results remain consistent with our baseline findings. The coefficients on the YEOs and the interaction term between YEOs and YCI maintain their significance and expected signs. The heterogeneous effects with respect to YCI and NYG are robust to the inclusion of these additional enforcement measures, confirming that our main results are not driven by concurrent policy changes or enforcement actions.

Table A6: Robustness to Additional Enforcement Measures

Panel A:	(1)	(2)	(3)	(4)
Dependent Variable: Financial Damage from Organized Fraud				
YEO	0.6679**	0.4675*	0.7122**	0.4148
	(0.2647)	(0.2498)	(0.2939)	(0.2734)
YEO × YCI (SD)	-0.7957***	-0.2854	-0.7734**	-0.3477
	(0.2297)	(0.2060)	(0.3103)	(0.2708)
YEO × NYG (SD)		-1.0964**		-0.9493**
		(0.4083)		(0.4622)
YEO × YCI (SD) × NYG (SD)		1.0211***		0.8240*
		(0.3762)		(0.4554)
Anti-Yakuza Laws Revision × YCI (SD)	-0.0015	0.2058		
	(0.1767)	(0.1885)		
Anti-Yakuza Laws Revision × NYG (SD)		-0.5475***		
		(0.1939)		
Anti-Yakuza Laws Revision × YCI (SD) × NYG (SD)		0.3476**		
		(0.1671)		
Obama Enforcement × YCI (SD)			-0.0314	0.0861
			(0.2917)	(0.3179)
Obama Enforcement × NYG (SD)				-0.2389
				(0.4136)
Obama Enforcement × YCI (SD) × NYG (SD)				0.2585
				(0.3837)
R-squared	0.5261	0.5173	0.5261	0.5292
Panel B:	(1)	(2)	(3)	(4)
Dependent Variable: Number of Organized Fraud Cases				
YEO	0.0055	0.0004	-0.0065	-0.0275
	(0.0242)	(0.0264)	(0.0289)	(0.0290)
YEO × YCI (SD)	-0.0304	-0.0230	-0.0206	-0.0060
	(0.0209)	(0.0256)	(0.0270)	(0.0299)
YEO × NYG (SD)		-0.0188		-0.0398
		(0.0284)		(0.0380)
YEO × YCI (SD) × NYG (SD)		0.0329		0.0615*
		(0.0281)		(0.0352)
Anti-Yakuza Laws Revision × YCI (SD)	0.0569***	0.0452**		
	(0.0211)	(0.0187)		
Anti-Yakuza Laws Revision × NYG (SD)		0.0129		
		(0.0302)		
Anti-Yakuza Laws Revision × YCI (SD) × NYG (SD)		-0.0075		
		(0.0251)		
Obama Enforcement × YCI (SD)			0.0050	-0.0087
			(0.0228)	(0.0228)
Obama Enforcement × NYG (SD)				0.0370
				(0.0418)
Obama Enforcement × YCI (SD) × NYG (SD)				-0.0447
				(0.0345)
R-squared	0.6135	0.6094	0.6083	0.6091
Observations	1,548	1,692	1,548	1,548

AYL: Anti-Yakuza Law revision in October 2012. **USEnforcement**: Executive Order 13581, issued by the Obama administration in July 2011.

L Robustness Check to Alternative Samples

We examine the robustness of our findings to alternative samples. Our main specification excludes "always-treated" prefectures, which enacted the YEOs before January 2011. In this exercise, we perform two robustness checks:

- Column (1): We exclude prefectures affected by the Great East Japan Earthquake that occurred on March 11, 2011. It is often said that yakuza members engaged in business in the affected areas after the earthquake. For example, some participated in the repair of nuclear reactors (Ramseyer, 2016) and in the restoration of the affected areas. These economic opportunities might affect yakuza activities. To eliminate potential confounding effects of the earthquake, we exclude the three prefectures that were most severely affected by this earthquake (Miyagi, Iwate, and Fukushima) from our dataset.
- Column (2): We include all prefectures in the sample, including "always-treated" prefectures that enacted YEOs before January 2011 (i.e., Fukuoka, Nagasaki, Kagoshima, and Ehime).

Table A7 presents the results. We obtain qualitatively similar results to our main specification, confirming the stability of our findings.

Table A7: Robustness to Alternative Samples

Panel A:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Financial Damage from Organized Fraud						
YEO	0.8245***	0.8231***	0.7005***	0.7396***	0.3528	0.3548
	(0.2944)	(0.2851)	(0.2529)	(0.2535)	(0.2480)	(0.2468)
YEO × YCI (SD)			-0.7786***	-0.8191***	-0.2746	-0.2787
			(0.2315)	(0.2471)	(0.1986)	(0.2180)
YEO × NYG (SD)					-1.1202**	-1.2070***
					(0.4294)	(0.4383)
YEO × YCI (SD) × NYG (SD)					1.0162**	1.0781**
					(0.4022)	(0.4081)
R-squared	0.5408	0.5044	0.5471	0.5108	0.5501	0.5138
Panel B:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Number of Organized Fraud Cases						
YEO	0.0060	0.0053	0.0033	0.0034	-0.0071	-0.0058
	(0.0276)	(0.0244)	(0.0262)	(0.0234)	(0.0290)	(0.0267)
YEO × YCI (SD)			-0.0170	-0.0180	-0.0135	-0.0134
			(0.0232)	(0.0221)	(0.0284)	(0.0270)
YEO × NYG (SD)					-0.0067	-0.0084
					(0.0302)	(0.0298)
YEO × YCI (SD) × NYG (SD)					0.0227	0.0228
					(0.0292)	(0.0292)
R-squared	0.6006	0.6038	0.6009	0.6041	0.6014	0.6045
Observations	1,440	1,692	1,440	1,692	1,440	1,692
Sample	Exclude GEJE	All	Exclude GEJE	All	Exclude GEJE	All

GEJE: sample excluding prefectures severely affected by the Great East Japan Earthquake (i.e., Miyagi, Iwate, and Fukushima).
All: sample including "always-treated" prefectures (i.e., Fukuoka, Nagasaki, Kagoshima, and Ehime).

M Robustness Check to Alternative Estimator

In the main specification, we use OLS. In this robustness check exercise, we report results using the Weighted Least Squares (WLS) estimator. Across three models, the results are qualitatively similar to the main results.

Table A8: Robustness to Alternative Estimator

Panel A:	(1)	(2)	(3)
Dependent Variable: Financial Damage from Organized Fraud			
YEO	0.4512**	0.6727***	0.5688***
	(0.1985)	(0.2098)	(0.1973)
YEO × YCI (SD)		-0.6980***	-0.5356***
		(0.1962)	(0.1602)
YEO × NYG (SD)			-0.6190*
			(0.3112)
YEO × YCI (SD) × NYG (SD)			0.6137**
			(0.2924)
R-squared	0.6585	0.6638	0.6661
Panel B:	(1)	(2)	(3)
Dependent Variable: Number of Organized Fraud Cases			
YEO	-0.0016	0.0026	-0.0174
	(0.0244)	(0.0251)	(0.0258)
YEO × YCI (SD)		-0.0132	-0.0214
		(0.0231)	(0.0246)
YEO × NYG (SD)			-0.0099
			(0.0217)
YEO × YCI (SD) × NYG (SD)			0.0295
			(0.0208)
R-squared	0.7706	0.7707	0.7720
Observations	1,548	1,548	1,548
Estimator	WLS	WLS	WLS

N Data Construction on Non-Yakuza Criminal Groups

Non-yakuza criminal groups consist of loose, younger crime networks that lack the yakuza's hierarchical structure but often collaborate with them. Because these crews are fluid and newer than yakuza groups, data on them are difficult to obtain; estimates are typically based on police briefings and media reports rather than formal registries. These figures should therefore be treated as indicative rather than definitive. Accordingly, we obtain data on the presence of these groups from YakuzaWiki, a user-maintained website about the yakuza (as of August 10, 2025). Notably, data on these groups are only available for the post-YEOs period.

To construct a pre-YEOs proxy for non-yakuza criminal groups, we use baseline covariates to predict post-YEOs counts in the spirit of Blattman et al. (2025). Specifically, we fit a Poisson LASSO with 10-fold cross-validation, where each fold holds out a random tenth of the sample for testing while training on the remaining nine tenths.

Candidate predictors (all standardized) include: log population; share aged 65+; share aged 19–39; share under 18; share male; share foreign; traffic-fatality accidents per capita; counts per capita of serious violent crime (homicide, robbery, arson, sex crime), assault, and property crime; per-capita delinquent arrests for serious violent crime, assault, and property crime; arrests for methamphetamine; arrests for prostitution; gun seizures; and police per capita.

Table A9 presents the variables with non-zero coefficients and their estimates from the cross-validation-selected model. The correlation between observed and predicted non-yakuza group presence is $\rho = 0.8013$.

Table A9: Baseline Predictors of Non-Yakuza Criminal Groups

Predictors	Lasso Coefficient
Serious Violent Crime	0.9573
Share of Male	0.3249
Arrests for Methamphetamine	0.1521
Arrests for Prostitution	0.1470
Gun Seizures	0.0804
GDP	0.1259
Share of Foreign Residents	0.0056
Constant	-0.6456

O Designated Yakuza Groups

The Anti-Yakuza Laws "designate" 21 yakuza groups during our study period. The criteria for the designation are that (i) the yakuza group has a hierarchical organizational structure, (ii) its members use violence to make money, and (iii) its members have criminal records. Table A10 lists all the designated yakuza groups. In the table, "#pref. of influence" denotes the number of prefectures in which the corresponding yakuza group operates.[16]

Table A10: Designated Yakuza Group

yakuza group	#pref. of influence	yakuza group	#pref. of influence
Yamaguchi-gumi	45	Shinwa-kai	1
Inagawa-kai	19	Sōai-kai	2
Sumiyoshi-kai	18	Kyokudō-kai	5
Kudō-kai	3	Taishū-kai	1
Kyokuryu-kai	1	Sakaume-gumi	1
Aizukotetsu-kai	2	Kyokutō-kai	15
Kyōsei-kai	1	Azuma-gumi	1
Gōda-ikka	3	Matsuba-kai	10
Kozakura-ikka	1	Fukuhaku-kai	4
Asano-gumi	2	Namikawamutsumi-kai	6
Dōjin-kai	4		

[16]There were 22 designated yakuza groups, but in 2011, two of them, both of which operated only in Okinawa, merged.

P Summary Statistics of Outcome and Control Variables: Ward-Level Analysis

Table A11: Summary Statistics of Outcome and Control Variables: Ward-Level Analysis

	Mean	Std. Dev.
Yakuza Arrests for Fraud	16.7463	8.1912
Yakuza Arrests for Extortion	10.8261	7.4402
Yakuza Arrests for Obstruction of Public Order	4.7896	3.2767
Share of Population Aged 65+	0.1992	0.0253
Share of Foreign Population	0.04674	0.0255
Share of Welfare-Recipient Households	0.0164	0.0081
Taxable Income (billion)	846.5690	454.9371

Observations: 138 (23 wards × 6 years). **Data Sources:** Data on yakuza arrests are obtained from the Tokyo Metropolitan Police Department. All other control variables are obtained from e-Stat.