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

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November, 2026

Abstract

Japan's Yakuza Exclusion Ordinances (YEOs) are indirect enforcement policies that sever criminal groups' lawful ties and their revenues. We develop a stylized model in which the YEOs lower returns to traditional rackets, inducing yakuza groups to reallocate resources toward organized fraud with non-yakuza criminal groups. The model predicts higher aggregate fraud damage mainly through an increase in the amount extracted per case, with a small change in case counts. It also predicts that YEOs' effects are larger where yakuza competition is weaker. In these areas, weaker competition limits their incentive to invest in fraud to capture rivals' revenues. Lower pre-YEO investment makes additional investment less costly. Hence, the YEOs lead to larger increases in fraud losses. Difference-in-Differences results are consistent with the model prediction.

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

*Names in alphabetical order. We contributed equally to this study. We are grateful to the Editor, Timur Kuran, and three anonymous referees for helpful comments. We thank participants at various conferences and seminars, as well as colleagues for their valuable comments and suggestions. We are grateful to the National Police Agency and Prefectural Police Departments for providing data on the yakuza and organized fraud. Kamada would like to thank Grant-in-Aid from the Japan Society for the Promotion of Science (26-5010; 21K13283; 24K16362). The usual disclaimer applies.

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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 (Blattman et al., 2025; Hill, 2003; Gambetta, 1993; Lessing, 2021; Melnikov et al., 2025; Varese, 2001). A policy-relevant question is whether interventions reduce the rents generated by criminal governance or shift them across illicit activities and forms of criminal organization. The state can intervene in two ways: through direct confrontation between organized crime and law enforcement (organized crime vs. police) or through policies that raise the costs of sustaining relationships with organized crime for non-criminal actors (organized crime vs. society). Existing research has primarily examined direct enforcement strategies in which police actively target organized crime groups (e.g., Calderón et al., 2015; Chalfin et al., 2021; Dell, 2015; Piehl et al., 2003; Lindo and Padilla-Romo, 2018). Far less is known about indirect enforcement targeting the organized crime–society relationship, even though organized crime's economic activity depends on connections with societal actors. In particular, it remains unclear whether interventions that restrict such interactions reduce crime or instead transform the activities and organizational forms through which criminal revenues are generated.

We fill this gap by focusing on the *yakuza*, Japanese organized crime groups. Historically, yakuza generated revenue through non-yakuza individuals and businesses, some made voluntarily and others under coercion. 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 had immediate effects, contributing to a sharp decline in yakuza membership and accounting for 30% of the overall decline during the study period (Hoshino and Kamada, 2021).

However, the YEOs might give rise to new forms of crime that do not depend on non-criminal actors knowingly dealing with yakuza (Section 2.2). 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-yakuza fraudsters (NHK, 2014; National Police Agency,

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

2014a, 2018). Second, violent crimes are relatively unprofitable in Japan due to high arrest rates, whereas organized fraud had 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 become a serious problem in Japan: during our study period, financial damage rose by approximately 140% while the number of cases increased by approximately 70% (National Police Agency, 2012, 2014b).

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 stylized two-stage model. A key feature is that yakuza and non-yakuza criminal groups play complementary roles. As discussed in Section 2.1, this distinction reflects differences in regulatory exposure and organizational status, not intrinsically different types of offenders. Yakuza provide "crime capital" (initial infrastructure such as hideouts and target lists), whereas non-yakuza criminals supply "crime labor" (subordinate perpetrators). Accordingly, the groups first invest capital and labor, respectively, in a fraud venture. The venture then chooses the number of cases and the additional resources devoted to each case. 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. Consequently, the additional resources raise the amount extracted per case, while the risk of detection can limit the increase in the number of cases (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 the YEOs' effects vary between "monopolistic" and "oligopolistic" yakuza areas and how differences in incentives to invest in fraud to capture rivals' revenue shares explain the gap. 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 have less incentive to invest in fraud to capture their rivals' revenue shares. Their total investment in fraud is therefore lower before the YEOs, making further investment less costly. Consequently, the YEOs' effect on fraud damage is greater 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 aggregate financial damage from organized fraud, whereas the estimated effect on the number of fraud cases is not statistically distinguishable from zero. Consistent with this pattern, financial damage relative to case counts also rises, suggesting that the result operates mainly through the intensive margin, instead of the extensive margin. These results are in line with our model's predictions (Propositions 1 and 2). To address identification concerns in staggered difference-in-differences designs (Goodman-Bacon,

2021), we confirm robustness using modern staggered-adoption estimators, a stacked event study with not-yet-treated controls, and leave-one-cohort-out analyses. Baseline yakuza and enforcement measures are not statistically associated with enactment timing in our analytic sample.

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). As an extension, we examine whether the YEOs' effect on fraud damage varies jointly with regional yakuza competition and the presence of non-yakuza criminal groups, finding a positive interaction between these two regional characteristics in the YEOs' effect. While this analysis does not directly test the group-level complementarity assumed in the model's production function, it allows us to test how regional criminal-market structure shapes the policy response. To construct the non-yakuza group index, we follow Blattman et al. (2025) and use a LASSO procedure to predict post-YEO group counts from pre-treatment covariates. We interpret this index as an indirect proxy rather than an observed measure of pre-YEO group presence. The heterogeneity results remain robust after allowing the YEOs' effects to vary with enforcement capacity, illegal-market characteristics, and demographic characteristics.

Additional analysis provides evidence consistent with our model prediction. Using prefecture-by-month bankruptcy data by industry, we find that the YEOs reduce food-service bankruptcies where the share of small establishments is higher. This result is in line with the idea that food-service bankruptcies declined because the yakuza extracted less from business establishments after the YEOs. Furthermore, a descriptive analysis of ward-level arrest data from Tokyo (Appendix B) indicates an increase in yakuza arrests for fraud and a decrease in arrests for extortion, which is a traditional source of yakuza income, around the enactment of the YEOs.

Our analysis documents a divergence between the severity and frequency of organized fraud. While the YEOs increase aggregate financial damage, the estimated effect on the number of fraud cases is not statistically distinguishable from zero. This finding is consistent with our model. When detection risk sufficiently constrains case expansion, a marginal increase in fraud resources has a larger effect on the amount extracted per case than on the number of cases. This mechanism may apply beyond the yakuza context when enforcement affects complementary criminal enterprises differently: raising costs for some actors while leaving others relatively unconstrained.

Our results suggest a policy tradeoff: indirect enforcement can disrupt traditional yakuza operations while encouraging fraud involving collaboration with 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, redirecting activity toward schemes involving actors less directly exposed to regulation.

Related Literature This study contributes to the economics of crime literature, particularly on organized crime and illegal markets, in three related ways. First, most research on enforcement against organized crime examines direct crackdowns on violence led by police or the military (Calderón et al., 2015; Chalfin et al., 2021; Dell, 2015; Piehl et al., 2003; Lindo and Padilla-Romo, 2018). Yet organized crime fundamentally depends on connections with societal actors—citizens, businesses, and politicians—in Italy and Latin America (Alesina et al., 2019; Daniele and Dipoppa, 2023; Di Cataldo and Mastrorocco, 2021; Fenizia and Saggio, 2024; Melnikov et al., 2025). The yakuza similarly depend on connections with legitimate businesses and local communities, making Japan's YEOs an ideal setting to examine whether indirect sanctions that sever these ties reduce criminal activity or transform the form it takes. A closely related study by Hoshino and Kamada (2021) finds that the YEOs reduce yakuza membership, particularly in areas with lower yakuza competition. The present paper complements this work by asking what happens to criminal activity once those organizations are constrained. We show that the YEOs increase financial damage from organized fraud and, consistent with the disruption of traditional rackets, reduce bankruptcy in industries exposed to yakuza activity. Thus, our findings indicate that indirect enforcement can transform the form of criminal activity even as it weakens the targeted criminal organization.

Second, we show how criminal-market structure shapes this adaptation to indirect enforcement. A growing literature examines how illegal market structures shape organized crime groups and their associated violence (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. That is, a pact among gangs allows for cooperation in areas where gangs had previously competed. We complement this work by showing that indirect enforcement restricting established revenue sources induces a larger increase in fraud damage in areas with initially low yakuza competition. Our model explains that yakuza groups facing weaker competition have less incentive to invest in fraud to capture their rivals' revenue shares, leaving further investment less costly when returns to traditional rackets fall.

Third, we contribute to the literature on enforcement-induced criminal reallocation (Battiston et al., 2024; Cruz and Torrens, 2023; Yang, 2008). Battiston et al. (2024) show that enforcement crackdowns on Mexican drug trafficking organizations lead them to reallocate activity toward oil theft. Our study extends this literature in two ways. The first is that we find an increase in total financial damage from organized fraud but a decrease in bankruptcies in industries more exposed to the yakuza. This finding is consistent with a reallocation from traditional rackets toward organized fraud. The second is that, within fraud, criminal groups expand along the intensive margin (severity) rather than the extensive margin (frequency).

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, descriptive evidence, and identification strategy, and Section 5 presents our main results. Section 6 examines heterogeneity by yakuza competition, the role of non-yakuza criminal groups, and the effects of the YEOs on firms exposed to traditional rackets. Section 7

concludes.

2 Institutional Background

This section offers institutional background relevant to our study. Section 2.1 discusses the legal status of yakuza groups and the distinction between yakuza and non-yakuza criminal groups, and Section 2.2 describes the yakuza's economic activities.[2] Section 2.3 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 2.4 provides institutional background on the Yakuza Exclusion Ordinances (YEOs).

2.1 Legal Status and Regulatory Distinction

Organizing or joining a yakuza group is *not* itself 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 regulation. In response, the Anti-Yakuza Law (*Bōryokudan Taisaku Hō*) came into force in 1992. The law designates yakuza groups on the basis of statutory criteria, including their organizational structure, reliance on violence for financial gain, and the criminal records of their members. Designation creates a legal framework through which authorities can systematically restrict and sanction specified activities by these groups and their members. Before the enactment of the YEOs, twenty-one yakuza groups were designated, and there were 82,600 yakuza members (including quasi-members).[3] Approximately 96% of formal yakuza members belonged to one of these designated groups. This proportion remained broadly stable during our study period (National Police Agency, 2009, 2014a).

The legal distinction between members of designated yakuza groups and other criminal affiliates should not be interpreted as a distinction between intrinsically different types of offenders. Rather, formal affiliation makes yakuza members, who are more directly subject to anti-yakuza regulation, readily identifiable to law-enforcement authorities. Quasi-members and other non-registered criminal affiliates may engage in criminal activity without formally belonging to any yakuza group. Our distinction between yakuza and non-yakuza criminal groups, therefore, primarily reflects differences in regulatory exposure and organizational status. Section 2.3 discusses how this distinction operates in organized fraud, where formally affiliated yakuza members frequently work with non-yakuza criminal affiliates.

2.2 Economic Activity of Yakuza

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

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

[3] There were 22 designated groups, but in 2011 two groups, both operating only in Okinawa, merged.

social and economic changes (National Police Agency, 2015). For instance, they extract protection payments (*mikajimeryō*) from businesses, including restaurants, bars, nightclubs, and sex-industry establishments, lend money at usurious rates, blackmail corporations (*sō*kaiya), and intervene in civil disputes (*minbō*) (Hill, 2003, 2004; Milhaupt and West, 2000). In addition, the yakuza exert influence in infrastructure and construction (Hill, 2003, 2004; Ramseyer, 2016). During the government's assistance program for historically marginalized communities known as *buraku*, yakuza groups extracted rents through extortion schemes targeting businesses under the guise of advocacy for *burakumin* communities (so-called *ese-dōwa* activities) and through subsidy-funded construction projects (Ramseyer and Rasmusen, 2018). The program's termination in 2002 restricted some established sources of yakuza-linked rents. This raises the question of whether criminal groups turn to other sources of revenue when institutional changes restrict existing ones.

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. Traditional rackets, such as protection payments and intervention in civil disputes, require that the counterparty know whom it is dealing with. The counterparty pays because it recognizes the other party's yakuza affiliation. The YEOs target these recognized relationships by penalizing the non-yakuza side. Organized fraud follows the opposite logic. A fraud scheme succeeds only when the victim is unaware of the perpetrator's yakuza affiliation, and the relationship does not continue beyond the scheme. Section 6.3 examines its consequences from the perspective of legitimate businesses likely to have ties to the yakuza by analyzing bankruptcies.

2.3 Yakuza Involvement in Organized Fraud

Organized fraud in Japan is a category of criminal activity in which offenders illicitly obtain money primarily through the use of telecommunications media, including telephones and email. Most victims are the elderly, with nearly 80% of cases involving individuals aged 60 or older (National Police Agency, 2014b).[4] In 2013 alone, total financial damage amounted to about 49 billion yen, approximately 330 million U.S. dollars (National Police Agency, 2014b).[5]

Fraud groups typically consist of both yakuza members and non-yakuza criminals working in collaboration (National Police Agency, 2014a, 2018). While the division of labor varies across fraud types and methods of payment, organized fraud schemes generally involve those who coordinate the operation and those who carry out its tasks. 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 may include callers (*kakeko*), who deceive victims; receivers (*ukeko*), who

[4]This category is known as *tokushu sagi* in Japanese, and its direct translation is "special fraud" or "specialized fraud." We use "organized fraud" throughout this paper to emphasize the coordinated nature of these schemes.

[5]Throughout the paper, we convert yen to U.S. dollars at \$1 = 150 yen.

collect cash; and withdrawers (*dashiko*), who extract deposited funds from ATMs. Decisions on how much capital and labor to devote to fraud are thus made at the organizational level by high- and mid-ranking members, while low-ranking perpetrators execute assigned roles.

Yakuza members usually occupy higher-ranking positions, providing "crime capital," namely, initial investments to cover the costs of frequently changing hideouts, procuring target lists, and 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.[6]

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. Other types of fraud involve fraudulent offers of financial products or investments. Low-ranking operatives bear the highest risk of apprehension. By contrast, higher-ranking members remain insulated from law enforcement because lower-ranking 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).

This division of roles is consistent with accounts indicating that non-yakuza groups have weaker informational and logistical networks than yakuza, encouraging mutually beneficial collaboration rather than competition (Hill, 2004). Furthermore, Baradel (2026) documents that yakuza groups in Tokyo direct non-yakuza affiliates to carry out specific tasks and use their authority to extract fees. Interviews with former offenders further indicate that, because sentencing under the anti-yakuza law has become substantially harsher for members of designated yakuza groups, yakuza increasingly recruit non-yakuza members—often ex-convicts encountered in prison—for fraud operations to insulate the organization from enforcement risk (Baradel, 2021). These accounts corroborate our depiction of organized fraud as a joint venture in which yakuza provide capital while non-yakuza criminals provide labor.

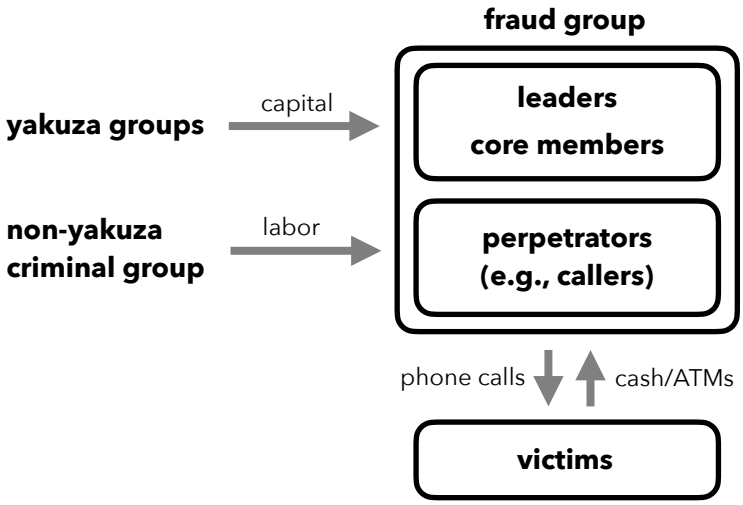
2.4 Yakuza Exclusion Ordinances (YEOs)

The Yakuza Exclusion Ordinances (YEOs) are based on the concept of "society versus 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.

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. The ordinances were part of this national shift: all 47 prefectures

[6] The term non-yakuza criminals refers to criminals outside formal yakuza membership, including former members (Section 2.1). We distinguish them from non-yakuza citizens.

Figure 1: Structure of Organized Fraud Group



enacted them between April 2010 and January 2012 (Table 1), and Section 4.2 discusses whether local crime conditions are correlated with the timing.

To cut off the yakuza's income sources, the YEOs prohibit non-yakuza citizens from knowingly providing any benefits to yakuza members. This includes banning non-yakuza citizens from receiving extralegal protection from yakuza members, providing food, drink, or office space to them, opening bank accounts or issuing credit cards for them, leasing office space to them, or selling insurance to them. 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: complying with the YEOs exposes them to possible retaliation from the yakuza, while violating the YEOs exposes them to possible penalties.

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 the incident to the local government and published the names of the nine companies. 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 organize the empirical analysis, we develop a model of organized fraud. The model is a stylized device for generating directional predictions, not a structural model of organized fraud production. The model isolates a simple mechanism: when indirect enforcement lowers the profitability of traditional yakuza rackets, yakuza groups shift resources toward fraud schemes that can be operated jointly with non-yakuza criminal groups. Because expanding case volume raises exposure to detection, this shift can increase financial damage mainly through the amount extracted per case rather than through the number of observed cases.

The model has three elements: yakuza groups allocate resources between traditional rackets and fraud; yakuza and non-yakuza criminal resources are jointly useful in fraud production; and the fraud venture chooses both case volume and additional input per case. These elements yield three directional predictions:

1. A decline in the profitability of traditional rackets shifts yakuza resources toward organized fraud.
2. Additional fraud resources increase financial damage mainly through the amount extracted per case when detection risk sufficiently constrains case volume.
3. For a fixed total yakuza resource endowment, the damage response is larger with fewer yakuza groups when marginal conversion costs are high relative to traditional returns.

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 an additional resource input per case to maximize expected 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, denoted by 0.[7] They simultaneously choose how much to invest in the fraud venture. The total resource endowment across yakuza groups is fixed at $M > 0$. Yakuza group g, endowed with $m = M/G$ units of criminal resources, allocates $K_g \in [0, m]$ to the fraud venture and $m - K_g$ to traditional relationship-based rackets (e.g., extortion). The non-yakuza criminal group 0, endowed with a unit of resources, allocates $L \in [0, 1]$ to the fraud venture and $1 - L$ to its outside option.

Yakuza investments $K_1, \ldots, K_G$ are aggregated into "crime capital." We aggregate these investments as $K = \sum_{g=1}^{G} K_g$. The venture combines crime capital K with non-yakuza "crime labor" L to obtain the resource $R = AK^{\alpha}L^{\beta}$, where $\alpha, \beta \in (0, 1)$ are given parameters. This specification

[7] The single non-yakuza group is a simplifying device. It represents the non-yakuza criminal resources available to the fraud venture, instead of a literal single organization. This abstraction allows us to focus on how yakuza competition affects the reallocation of resources toward fraud.

captures the idea that yakuza resources and non-yakuza criminal resources are jointly useful in operating fraud schemes. We adopt it as a tractable reduced-form representation of joint production. Assume that this production exhibits decreasing returns to scale: $\alpha + \beta < 1$. 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 an additional resource input per case $s \geq 0$ subject to the constraint $n(1+s) = R$. Each case requires a baseline resource input, normalized to one, in addition to the variable s, which we interpret as an input that raises the value extracted per case. Specifically, we take the venture's expected revenue to be

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

where $p(n)$ is the probability that the fraud scheme is 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 $c > 0$. This modeling of apprehension probability follows Fu and Wolpin (2018). 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.

This formulation of revenue (1) captures enforcement through detection risk. If the scheme is detected, the venture's retained fraud proceeds are zero, so $\Pi(R)$ is the expected private return net of detection risk. The reduced-form probability $p(n) = 1 - \mathsf{e}^{-cn}$ has a simple interpretation: each fraud case generates an independent operational exposure—for example, victim contact, communication records, bank transfers, ATM withdrawals, or contact with the low-ranking operatives—so the probability that at least one exposure leads to detection rises with the number of cases. By contrast, the per-case input s is the intensive margin: conditional on operating a case, the venture may target wealthier victims, sustain persuasion for longer, or use more elaborate narratives, thereby extracting a larger amount per case. These activities raise the value extracted per case $\pi(s)$ but need not proportionally increase the number of victims, transfers, etc. The assumption that p depends on n rather than directly on s is a parsimonious way to capture the idea that detection risk is generated primarily by the extensive margin of criminal operations. Unusually large transfers may attract additional scrutiny, but our baseline treats this scale dependence as second-order relative to case-level operational exposure. Beyond the forfeiture already embedded in (1), we abstract from explicit sanctions upon apprehension to keep the mechanism transparent.

Revenue Sharing and Payoffs 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: for $K > 0$,

yakuza group g's share φ_g and the non-yakuza group's share φ_0 are, respectively,

$$\varphi_g = \frac{\alpha}{\alpha+\beta}\frac{K_g}{K} \qquad \text{and} \qquad \varphi_0 = \frac{\beta}{\alpha+\beta}. \tag{2}$$

Yakuza group g's payoff is

$$\Pi_g(K_1, \ldots, K_G, L) \equiv \varphi_g \Pi(R) + (m - K_g)r - \frac{\kappa K_g^2}{2m},$$

where $r > 0$ denotes the net profitability of traditional, relationship-based rackets. These rackets include activities whose returns depend on continuing ties with non-yakuza citizens or firms, such as extortion, protection payments, and intervention in business transactions. Thus, $(m - K_g)r$ is the payoff from the resources that yakuza group g keeps outside the fraud venture. The parameter $\kappa > 0$ is common across yakuza groups. The final term is a conversion cost. The cost is measured in payoff units and incurred in stage 1 regardless of the subsequent outcome of the venture.

Similarly, the non-yakuza criminal 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. We keep w fixed in the policy experiment below because the YEOs target yakuza's relationship-based revenues rather than the outside option of non-yakuza criminal groups.

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 $K_g^* \in (0, m)$ and $L^* \in (0, 1)$, respectively. The investment bounds remain nonbinding throughout the analysis.

We interpret the enactment of the YEOs as a decline in the net profitability r of traditional, relationship-based rackets. This is a reduced-form representation of indirect enforcement: by penalizing non-yakuza actors who provide benefits to yakuza, the YEOs reduce the returns to activities that require continuing and identifiable ties with non-yakuza citizens or firms. Organized fraud may also be affected by the YEOs, for example through restrictions on lawful infrastructure or financial services. Our assumption, grounded in the institutional design of the ordinances (Section 2.2), is that the YEOs reduce the return to traditional relationship-based rackets more strongly than the return to fraud, so that fraud becomes a relatively more attractive use of yakuza resources. To isolate the resulting reallocation channel, we vary r while holding the fraud technology, detection parameter, and all other primitives fixed.

YEOs' Effect on Organized Fraud We begin with the YEOs' effect on the resources allocated to organized fraud. When the profitability r of traditional relationship-based rackets falls, organized

fraud becomes relatively more attractive for each yakuza group. Each yakuza group therefore increases its fraud investment K_g, and aggregate crime capital K rises. The total resource R available to the fraud venture therefore increases.[8]

Proposition 1. *A decline in the return r to traditional rackets increases each yakuza group's investment K_g^* (and thus the aggregate K^*) and the total fraud resource R^*.*

The fraud venture then chooses a case count and the additional resources devoted to each case. Let $(n^*(R), s^*(R))$ be a solution to the stage-2 problem. The model counterpart of aggregate financial damage is

$$D(R) \equiv n^*(R)\pi(s^*(R)),$$

whereas the fraud venture's expected private return after accounting for detection risk is

$$\Pi(R) = \mathsf{e}^{-cn^*(R)} D(R).$$

Proposition 2. *For every $R > 0$, both $n^*(R)$ and $s^*(R)$ increase with R. As $R \to \infty$, the optimal case count satisfies $n^*(R) \to (1-q)/c$, while the resources per case satisfy $s^*(R) \to \infty$.*

Remark 1. Moreover, $\mathsf{d}n^*(R)/\mathsf{d}R \to 0$ and $\mathsf{d}s^*(R)/\mathsf{d}R \to c/(1-q)$ as $R \to \infty$: when R is large, marginal increases in fraud resources are absorbed primarily by increases in resources per case rather than by case volume. The proportional response of the per-case amount $s^*(R)^q$ to a marginal increase in R exceeds that of the case count if and only if $cR > 2/(1+q)$.

Propositions 1 and 2 yield the main prediction for organized fraud. A decline in the return r to traditional rackets increases the resources devoted to fraud. This raises financial damage and the amount extracted per case. Given those resources, detection risk makes it costly to expand case volume, and thus when R is large, additional resources are allocated mainly to increasing resources per case (Remark 1). Thus, the model does not predict literally unchanged case counts. It predicts that the case-count response can be small relative to the increase in aggregate financial damage.

YEOs' Effect Heterogeneity by Yakuza Competition How does yakuza competition moderate the effect of the YEOs on organized fraud damage? In the model, we summarize concentration among yakuza groups by a Herfindahl–Hirschman-type index. Given yakuza fraud investments $K_1, \ldots, K_G$, we define group g's investment share by $\sigma_g = \frac{K_g}{\sum_h K_h}$, and let $H = \sum_g \sigma_g^2$ denote concentration among yakuza groups. A higher H means that yakuza competition is weaker. For interpretability in the empirical analysis, we use the model counterpart of the **Yakuza Competition Index**, $\mathsf{YCI} \equiv 1 - H$, so that larger values denote stronger competition.

Although first-stage investment choices are governed by the expected private return $\Pi(R)$, the heterogeneity prediction is stated in terms of aggregate financial damage, $D(R)$, because this is

[8] Non-yakuza investment L can also increase (Appendix A).

the empirical outcome in the heterogeneity analysis. Thus, the next proposition links yakuza competition to the YEOs' effect on observed damage, not directly to the venture's expected return. This result also covers the amount extracted per case $\pi(s^*(R))$. The comparison is across interior equilibria summarized by different concentration levels H; the purpose is to obtain a directional heterogeneity prediction. We write $D^*_G(r) \equiv D(R^*_G(r))$ and $\pi^*_G(r) \equiv \pi(s^*(R^*_G(r)))$ for brevity.

Proposition 3. *Suppose that the resources devoted to organized fraud are sufficiently large and that the return r to traditional rackets is sufficiently low. Then, 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, $-\partial D^*_G(r)/\partial r$, decreases with* YCI*. The same holds for the marginal effect on the amount extracted per case, $-\partial \pi^*_G(r)/\partial r$.*

Proposition 3 requires that the resources devoted to organized fraud be sufficiently large and that the return to traditional rackets be sufficiently low. Both are plausible in our empirical setting. Since detection risk constrains case volume, additional resources raise aggregate financial damage mainly through the amount extracted per case (Remark 1). Our main estimates show this pattern: damage relative to case frequency rises, while the estimated case-count response is close to zero and statistically insignificant (Table 3). When the return r to traditional rackets is sufficiently low, the marginal conversion cost of investing in fraud is at least as large as the forgone return from traditional activities. Traditional rackets had been under regulatory pressure since the 1992 Anti-Yakuza Law, whereas fraud required new operations run jointly with non-yakuza partners.

When the YEOs reduce the profitability r of traditional relationship-based rackets, yakuza groups reallocate capital toward fraud. Under the Aumann–Shapley sharing rule, groups with larger investment shares internalize a larger part of the fraud surplus. Investment raises both total revenue and the investing group's share. A group that already receives a larger share has less incentive to invest to capture other groups' shares. More fragmented markets therefore have higher aggregate yakuza investment before the policy and higher marginal conversion costs. The same direct reduction in r is smaller relative to their initial marginal investment cost. Hence, under the stated conditions, in more concentrated markets (higher H, lower YCI), the damage response to a marginal decline in r is larger.

4 Data and Identification Strategy

4.1 Data

YEOs' Enactment Dates The YEOs are local ordinances enacted at different times across prefectures (Table 1).[9] Most prefectures enacted the YEOs in April 2011, while others did so between

[9] 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.

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

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.[10]

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 three outcomes. The first two are (i) financial damage from organized fraud, defined as the aggregate yen value of reported organized-fraud losses in a prefecture-month, and (ii) the number of organized fraud cases per 100,000 residents. We apply the inverse hyperbolic sine (IHS) transformation to both to accommodate zeros. The third outcome, (iii) financial damage relative to case frequency, is the IHS of financial damage minus the IHS of the raw monthly number of organized fraud cases, not the population-scaled rate used in the second outcome. For sufficiently large positive losses and counts, this difference approximates the log of damage per case. The average damage from organized fraud is 64 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 average 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).

[10] 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.

Yakuza Competition Index We construct a prefecture-level Yakuza Competition Index (YCI) analogous to the Herfindahl–Hirschman Index (HHI).[11] 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 $\mathsf{YCI}_p = 1 - \sum_g s_{pg}^2$.[12] 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 N. We treat the YCI as time-independent and measure it using data prior to the YEOs' enactment (as of 2009).[13]

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. While we cannot directly test this complementarity, we extend our heterogeneity analysis by incorporating the non-yakuza criminal group index. We examine whether the YEOs' effect on fraud varies jointly with regional yakuza competition and the presence of non-yakuza criminal groups.

Because non-yakuza criminal groups are newer than yakuza groups and not formally designated by authorities, data on these groups are informal and available only in the post-YEOs period; we source these data from YakuzaWiki, a user-maintained website about the yakuza (as of August 10, 2025) (Appendix U).

We provide a detailed description of our procedure to construct our measure of non-yakuza criminal groups in Appendix U. Essentially, following Blattman et al. (2025), we use LASSO regression on baseline covariates to predict this post-treatment variable, yielding a baseline-covariate-based index of predicted 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 counts from the cross-validation-selected model. LASSO coefficients are provided in Appendix U. The correlation coefficient between the actual and predicted numbers of non-yakuza criminal groups in the post-YEO period is 0.8013. We use baseline covariates to predict the number of non-yakuza groups operating in each prefecture. NYG is a time-invariant index based on these predictions. We use group counts because the underlying data do not report membership. NYG serves as a proxy for the non-yakuza criminal resources available to fraud

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

[12] An alternative index is simply the number of yakuza groups in each prefecture. Unlike this count, the YCI also reflects differences in estimated membership shares across groups. 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 N, 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.

[13] The model compares equally endowed groups holding their total endowment fixed (Section 3). We use estimated membership shares to proxy for local concentration in the empirical analysis.

ventures in our model (Section 3). Because we lack direct observations of these groups before the YEOs, NYG provides only an indirect indication of their pre-treatment presence.

Control Variables We control for the following demographic and economic variables: the share of the population aged 19–39, the share of the population aged 65 or older, the share of the population that is foreign, 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. Appendix D examines whether these characteristics predict the timing of the YEOs' enactment.

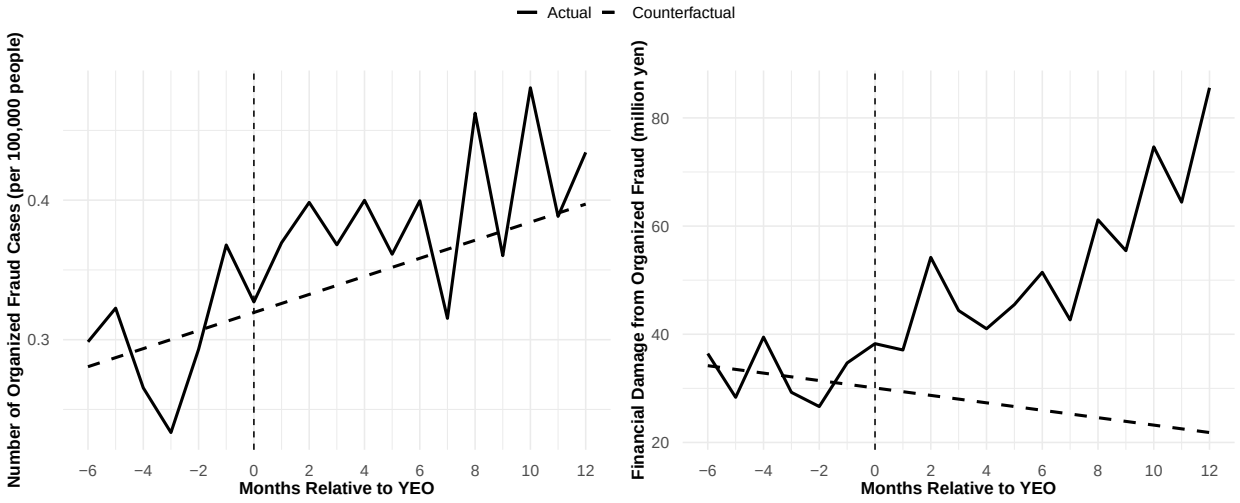
Table 2: Summary Statistics of Outcome and Control Variables

	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	0.0084
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 U). 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. Over the 12 months following the enactment of the YEOs, observed financial damage from organized fraud rises from approximately 40 million yen to 80 million yen. Observed damage is above the counterfactual trend. 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. Our benchmark DiD specification uses the two-way fixed effects (TWFE) estimator and takes the following form:

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

where Fraud_{pt} denotes one of three outcomes: (i) the inverse hyperbolic sine (IHS) of organized fraud damage, (ii) the IHS of the number of organized fraud cases per 100,000 residents, or (iii) the IHS of organized fraud damage minus the IHS of the raw monthly number of organized fraud cases 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.[14]

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 is divided into seven regions $r \in R$, and $\delta_{pr} = 1$ if prefecture p is in region $r \in R$.[15] 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

[14] 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.

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

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 timing of the YEOs' enactment reflects 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 the YEOs' enactment month in prefecture p to zero. We write t_p for the YEOs' enactment month in prefecture p. Then, $t-t_p$ denotes the number of months relative to enactment. In particular, it equals zero in the enactment month. The regression model for the event-study design is given by

$$\mathsf{Fraud}_{pt} = \sum_{\tau=-l}^{k} \beta_\tau \mathsf{YEO}_{pt}^{\tau} + \mathsf{Prefecture\ FE} + \mathsf{Time\ FE} + \eta X_{py} + \varepsilon_{pt}, \tag{3}$$

where $\beta_{-1} = 0$ by normalization. $\mathsf{YEO}_{pt}^{\tau} = 1$ if either (i) $t - t_p = \tau$ for $-l < \tau < k$, (ii) $t - t_p \geq k$ for $\tau = k$, or (iii) $t - t_p \leq -l$ for $\tau = -l$; otherwise, $\mathsf{YEO}_{pt}^{\tau} = 0$. For $(l, k) = (3, 6)$ or $(5, 10)$, relative to the reference period set to one month before the YEOs' enactment ($\tau = -1$), the coefficient β_τ indicates the τ-month lag effect for $1 \leq \tau < k$ and the $(-\tau)$-month lead effect for $-l < \tau < -1$. The coefficient β_0 indicates the effect in the enactment month.

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, in which already-treated units serve as controls for newly treated units, 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 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 E. Second, we use a stacked event-study approach, which avoids forbidden

comparisons; we present results in Appendix F. Third, we verify that the YEOs' effects are not driven by a particular treatment cohort; we report results in Appendix G.

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 outcomes. Additionally, including observable covariates minimally changes the magnitudes of the estimated coefficients, indicating that selection on observables is unlikely to be a concern. We also examine whether baseline characteristics predict the timing of enactment (Appendix D).

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 R).

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:

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

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:

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

Here, NYG_p is a time-invariant index of predicted non-yakuza group counts constructed from baseline covariates for prefecture p. The triple interaction term $\mathsf{YEO}_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p$ allows the YEOs' effect to vary jointly with yakuza competition and the presence of non-yakuza criminal groups. The cross-partial derivative of the estimated YEO effect with respect to YCI and NYG is $\widehat{\lambda}_4$. A positive and significant $\widehat{\lambda}_4$ implies that the marginal effect of yakuza competition on the YEOs' effect on organized fraud increases with the presence of non-yakuza criminal groups, and vice versa.

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). For sufficiently large values of the outcome in its reporting units, the inverse hyperbolic sine (IHS) transformation approximates the logarithm up to a constant, so coefficients can be interpreted as approximate log changes and converted into percentage changes using $100(\mathrm{e}^{\beta} - 1)$. 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, which under this large-value approximation corresponds to an increase of roughly 96–122%. 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 J, where we see a slightly larger coefficient and smaller standard errors. Because the month-by-year fixed effects in Column (5) change the point estimate only slightly and reduce precision, we use Column (4) as our preferred specification. In Panel B, the YEO coefficient remains close to zero and statistically insignificant in all specifications.

Panel C reports the results for financial damage relative to case frequency, defined as the IHS of financial damage from organized fraud minus the IHS of the raw monthly number of organized fraud cases. When both variables are sufficiently large in their reporting units, this difference approximates the log of damage per case. The coefficients range from 0.5807 to 0.6969 and are positive across all specifications.

These results are qualitatively consistent with our model predictions: the YEOs raise aggregate financial damage (Panel A), while the estimated effect on the frequency (number) of organized fraud incidents is not statistically significant (Panel B). Panel C provides additional evidence of higher damage relative to case frequency. This pattern suggests a shift toward higher-value fraud.

5.2 Event-Study Analysis

Figure 3 presents the event-study estimates from regression model (3), with 90% and 95% confidence intervals. The specification includes prefecture fixed effects, region-by-month fixed effects, region-

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
Panel C:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Financial Damage relative to Case Frequency					
YEO	0.5807**	0.6100**	0.6285***	0.6290***	0.6969*
	(0.2158)	(0.2315)	(0.2269)	(0.2246)	(0.3546)
R-squared	0.2521	0.2599	0.2669	0.3169	0.3261
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. **Outcome in Panel C:** IHS of financial damage minus IHS of the raw number of organized fraud cases (unscaled monthly count, not the per-100,000-resident rate in Panel B). **Controls:** Share of population aged 19-39, share aged 65+, share of foreign population, unemployment rate, and GDP. Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

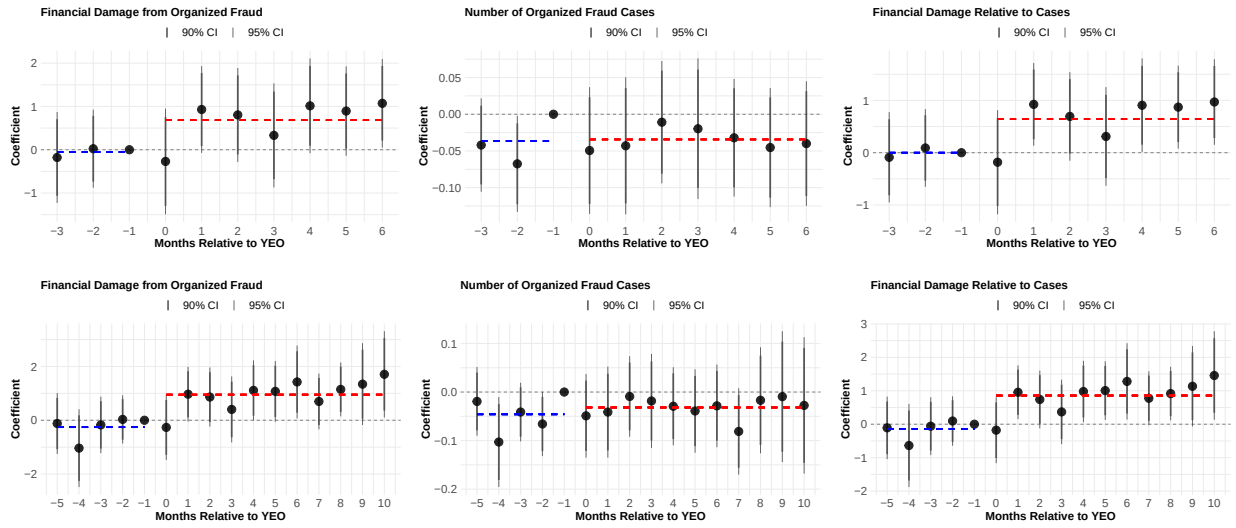
by-year fixed effects, and control variables. The left panel reports results for financial damage from organized fraud, the middle panel reports results for organized fraud cases, and the right reports financial damage relative to case frequency. In each panel, the dashed red line indicates the average 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 and right panels, 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 middle 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 and damage relative to case frequency from organized fraud jump one month after the YEOs' enactment. It is true that the prefectural authorities announced the YEOs' enactment in advance, but the pre-enactment coefficients for financial damage are stable and close to zero, suggesting no announcement effect. To better under-

stand 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 may have served as a period to prepare for organized fraud.

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



Dashed red line: mean of post-treatment coefficients. **Dashed blue line:** mean of pre-treatment coefficients. **Left panel:** IHS of financial damage from organized fraud. **Middle panel:** IHS of organized-fraud cases per 100,000 residents. **Right panel:** IHS of financial damage minus IHS of organized fraud cases. **Top panel event window:** months -3 to +6. **Bottom panel event window:** months -5 to +10 (both relative to YEOs' enactment). **Observations:** 1,548. **Confidence Intervals:** Thin and thick lines indicate 95% and 90% confidence intervals, respectively. **Joint F-tests of pre-treatment coefficients:** The null hypothesis is that all estimated pre-treatment coefficients are zero. From left to right, the p-values are 0.925, 0.136, and 0.924 for the top row and 0.612, 0.051, and 0.817 for the bottom row.

5.3 Summary of Robustness

The main finding survives alternative estimators, research designs, specifications, time controls, and sample choices. Below we discuss the most substantively important extensions.

Joint Tests of Pre-Trends We conduct joint-significance F-tests to validate key assumptions of our event-study design. For the lead treatment indicators, we test the null hypothesis that all pre-treatment coefficients are jointly equal to zero. Failure to reject this null indicates that we do not detect statistically significant pre-treatment differences from the reference period, consistent with the parallel trends assumption. The results of the joint tests of the pre-treatment coefficients are reported in the note to Figure 3. The null hypothesis is rejected at the 10% level only for organized fraud case counts in the specification with five lead indicators.

Robustness to Staggered Adoption Because identification rests on the staggered enactment of the YEOs, we assess the robustness of our results to treatment-effect heterogeneity, forbidden comparisons, and the influence of individual treatment cohorts. We first re-estimate their effects using four estimators that are robust to treatment-effect heterogeneity in staggered-adoption settings (Goodman-Bacon, 2021; Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2024; Callaway and Sant'Anna, 2021; Gardner, 2022). All four reproduce a positive and significant effect on financial damage and a null effect on the number of cases, and some remain valid even when all units are eventually treated (Appendix E). A stacked event-study design that compares each treated cohort only to not-yet-treated prefectures within its calendar window, and an event study that drops one treatment cohort at a time, both reproduce the main pattern. This indicates that neither forbidden comparisons nor any single treatment cohort drives our results (Appendices F and G).

Long-Term Window An event study using an extended time window shows that financial damage from organized fraud remains significantly elevated roughly twenty months after enactment, while the estimated effect on the number of cases is not statistically distinguishable from zero (Appendix H).

Time Controls and Estimator Replacing the most restrictive month-by-year fixed effects with parsimonious time controls selected by double-Lasso yields a slightly larger point estimate with smaller standard errors (Appendix J), and a weighted least squares estimator delivers qualitatively identical results (Appendix S).

Sample Composition The results are stable when we exclude the three prefectures most severely affected by the Great East Japan Earthquake (Miyagi, Iwate, and Fukushima) and when we add back the four "always-treated" prefectures that enacted the YEOs before 2011 (Appendix R).

Adoption Timing We assess whether local criminal conditions drove adoption timing. While the DiD design does not require covariate balance in levels, we regress the enactment month, expressed as a monthly date index, on baseline characteristics in a cross-section. In our analytic sample, none of our yakuza and enforcement measures (YCI, NYG, the illegal-market proxy index, and police per capita) predicts enactment timing. In the full sample, YCI is correlated with timing, but this is driven mainly by Fukuoka, the first prefecture to enact a YEO (Appendix D).

Subcategories of Organized Fraud We examine subcategories of fraud. Organized fraud can be categorized into two types: urgent or routine-type fraud and investment-type fraud. The first category includes relative-impersonation (family-emergency), fictitious-billing, loan-guarantee, and refund schemes. Investment-type fraud includes fraudulent financial-product or investment offers, gambling-tip schemes, and romance-introduction schemes. Investment-type fraud has higher damage per case. We find that the estimated increases are concentrated in investment-type fraud.

Placebo Exercise We use deaths from natural causes as a placebo outcome, excluding deaths from external causes such as homicide, suicide, and accidents. We expect that natural deaths have little connection to the proposed criminal-reallocation mechanism, but may reflect demographic and socioeconomic conditions relevant to organized fraud. For example, older people face higher natural mortality risks and are frequent targets of organized fraud. Although we control for the elderly population share, this exercise provides a supplementary check for broader demographic and living-condition changes not fully captured by that control. We find no statistically significant effect of the YEOs on this outcome.

6 Heterogeneity and Channels

Next, we examine heterogeneous effects of the YEOs by the Yakuza Competition Index (YCI) and extend the analysis to allow the YEOs' effect to vary jointly with YCI and non-yakuza group presence. Lastly, we study whether the YEOs disrupted traditional rackets by analyzing bankruptcies across industries.

6.1 Heterogeneous Effects of YEOs by Yakuza Competition Index

Table 4 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 have a mean of 0 and a standard deviation of 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. Panel C also shows a larger increase in financial damage relative to case frequency in prefectures with lower

yakuza competition. The damage pattern is consistent with Proposition 3: differences in revenue-sharing incentives and marginal conversion costs make the same small decline in traditional returns generate a larger increase in fraud damage when competition is weaker.

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 after allowing the policy effect to vary with these measured regional characteristics.

6.2 Heterogeneity by Yakuza Competition and Non-Yakuza Criminal Groups

We extend the heterogeneity analysis to incorporate non-yakuza criminal groups. While the model assumes complementarity between yakuza and non-yakuza criminal groups in fraud production, this regional analysis does not directly identify that production complementarity. Instead, Columns (4)–(7) of Table 4 examine whether the YEOs' effect on fraud damage varies with regional yakuza competition and the predicted presence of non-yakuza criminal groups. Columns (5)–(7) allow these two regional characteristics to interact in shaping the YEOs' effect. The coefficient on the triple interaction between YEO, YCI, and NYG captures the cross-partial derivative of the YEOs' effect with respect to these two characteristics.

First, Column (4) examines whether heterogeneous effects by YCI survive after accounting for variation in the predicted 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. Columns (5)–(7) incorporate a triple interaction between the YEOs, YCI, and NYG to examine how the YEO effect varies jointly with YCI and NYG. Because YCI and NYG are time-invariant, the baseline interaction between them is absorbed by prefecture fixed effects and is not separately identified. The cross-partial derivative of the estimated YEO effect with respect to YCI and NYG is denoted by $\widehat{\lambda}_4$. In Panel A, the positive and significant coefficient on the triple interaction term indicates that the marginal effect of yakuza competition on the YEOs' effect on financial damage increases with the presence of non-yakuza criminal groups, and vice versa. To address concerns that the two indices may be correlated with enforcement capacity and other illegal-market characteristics, Columns (6) and (7) show that the result remains robust after controlling for enforcement capacity and illegal-market characteristics, respectively. By contrast, in Panel B, none of the corresponding interaction coefficients in Columns (5)–(7) are statistically significant.

These findings are consistent with the perspective that regional criminal-market structure shapes

the response to the YEOs. To address concerns about (i) the non-yakuza criminal group index being a predicted proxy and (ii) the triple interaction potentially reflecting local demographic characteristics correlated with both indices, we perform several robustness checks summarized in Section 6.4.

Table 4: 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
PANEL C:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable: Financial Damage relative to Case Frequency							
YEO	0.5805***	0.5803***	0.5713***	0.5902***	0.2514	0.2885	0.2583
	(0.2101)	(0.2105)	(0.2102)	(0.2121)	(0.2156)	(0.2247)	(0.2152)
YEO × YCI (SD)	-0.6512***	-0.6531***	-0.6019***	-0.6075***	-0.1890	-0.1663	-0.2000
	(0.1934)	(0.1971)	(0.1913)	(0.2071)	(0.1725)	(0.1815)	(0.1671)
YEO × NYG (SD)				-0.1127	-1.0373**	-1.1771***	-0.9740**
				(0.1193)	(0.3929)	(0.4192)	(0.4033)
YEO × YCI (SD) × NYG (SD)					0.9414**	0.8772**	0.9070**
					(0.3681)	(0.3930)	(0.3683)
YEO × Police per Capita (SD)		0.0162				0.2324	
		(0.0918)				(0.2562)	
YEO × Illegal Market Index (SD)			-0.1984				-0.0630
			(0.1441)				(0.1674)
R-squared	0.3257	0.3258	0.3268	0.3261	0.3307	0.3312	0.3308
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. **Outcome in Panel C: IHS of financial damage minus IHS of raw monthly case counts. YCI:** Yakuza Competition Index, defined similarly to HHI as $YCI_p = 1 - \sum_g s_{pg}^2$. **NYG: Non-Yakuza Criminal Group Index, an index of predicted non-yakuza group counts constructed from baseline covariates using a Poisson LASSO model.** This measure uses predicted counts from the model selected by cross-validation. **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 is indicated by ***, **, and *, respectively.

6.3 Bankruptcy by Industry

Data Bankruptcy data come from the Business Mutual Aid Association and are based on surveys conducted by Tokyo Shoko Research, Ltd. The data are at the prefecture-by-month level, and we focus on the period from January 2010 to December 2013. Because the data begin before the YEOs, all 47 prefectures, including the four early adopters, have pre-enactment observations and are included in this analysis.

Our theory implies that the YEOs reallocate resources from traditional rackets to organized fraud. Traditional rackets operate in part through violent extortion and in part through involvement in legitimate firms. The YEOs prohibit non-yakuza citizens and businesses from providing financial means to the yakuza. This affects two types of firms in opposite ways (Section 2.2). On the one hand, firms that pay protection money are victims of the yakuza; the YEOs relieve them of this burden, so that their bankruptcies should decrease. On the other hand, front companies are instruments of the yakuza; the YEOs cut off the financial relationships on which they depend, and thus their bankruptcies should increase. We test these two predictions by examining the YEOs' effects on bankruptcies by industry. In both cases, the yakuza reallocate their resources toward organized fraud.

The yakuza are more likely to be involved in the industries discussed in Section 2.2, and within those industries the yakuza are more likely to exploit small firms than large ones. The data do not disaggregate by industry and firm size jointly, so we instead examine heterogeneous effects by the prefecture share of small establishments, defined as those with zero to four employees.

Table 5 shows the results. The YEOs' effect on bankruptcy frequency in food service is more negative in prefectures with a higher share of small establishments. This is consistent with a reduction in yakuza exploitation of these firms, since the ordinances make it harder for the yakuza to extract protection payments and other payments from them. Appendix T provides an event-study result.

Table 5: The YEOs' Effects on Bankruptcies by Industry

	Food	Construction	Manufacturing	Wholesale	Retail	Service
Bankruptcies per 1,000 Establishments						
YEO	-0.0049	-0.0091	-0.0076	-0.0353	0.0001	0.0057
	(0.0107)	(0.0173)	(0.0185)	(0.0221)	(0.0068)	(0.0054)
YEO × Small Share	-0.3424**	0.2078	0.2653	0.2471	-0.1001	0.1269
	(0.1514)	(0.1915)	(0.2108)	(0.3917)	(0.1005)	(0.1276)
R-squared	0.3429	0.4804	0.1843	0.2496	0.2676	0.5135
Observations	2,256	2,256	2,256	2,256	2,256	2,256
Prefecture fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Month × Region and Year × Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Outcome: IHS of bankruptcies per 1,000 establishments. **Small share:** Small Share is the industry-specific share of establishments with zero to four employees in the 2009 Economic Census. For each industry, the variable is centered by subtracting its grand mean across the 47 prefectures, but is not divided by its standard deviation. Consequently, the coefficient on YEO represents the post-YEO effect in a prefecture with the industry-average small-establishment share. The interaction coefficient represents the change in the YEO effect associated with a one-unit increase in Small Share, where the share is measured from zero to one. **Sample:** the sample covers all 47 prefectures from January 2010 through December 2013. Standard errors clustered by prefecture are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

6.4 Summary of Robustness for Heterogeneity, Channels, and Supplementary Analyses

Several analyses examine the proposed mechanisms—heterogeneity by yakuza competition, non-yakuza criminal groups, and the disruption of traditional rackets—and address alternative explanations.

Heterogeneity by Yakuza Competition and Non-Yakuza Groups

Event Study by YCI We re-estimate the effects by YCI in an event study (Appendix I). 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).

Alternative Competition Measures We test alternative competition measures (Appendix N): (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; (iv) a polarization-style index following Montalvo and Reynal-Querol (2005); and (v) the number of designated yakuza groups. Results are stable across all definitions, with larger effects in prefectures with fewer yakuza groups, consistent with the prediction in Proposition 3.

Alternative NYG Measures We test robustness using alternative measures of non-yakuza criminal groups (Appendix O): (i) the linear predictor, (ii) predicted counts using a fixed penalty parameter,

and (iii) leave-one-prefecture-out predictions. Our results remain robust across these alternative measures.

Potential Confounders: Concurrent Enforcement Shocks 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 P).

Potential Confounders: Demographic Characteristics We control for demographic characteristics—the share of the population aged 65 and over, population density (per habitable land area), and population size—interacted with the YEO (Appendix Q). Our heterogeneity findings remain robust to the demographic controls.

Channel

Event-Study Results for Food-Industry Bankruptcies by Share of Small Firms Appendix T provides event-study results for food-industry bankruptcies, showing that the YEOs decrease such bankruptcies in prefectures with an above-median share of small firms.

Former Yakuza Members Proxying the increase in the number of former members with the post-YEO decline in the (rank-specific) incarceration rate of current yakuza members, we find suggestive evidence that former members—especially those of regular and unknown rank, who face limited lawful employment—engage in organized fraud in patterns consistent with the roles of YCI and NYG. The pattern does not hold for high-ranking members (Appendix C).

Yakuza Arrests A descriptive analysis of ward-level arrest data from Tokyo in Appendix B indicates an increase in yakuza arrests for fraud and a decrease in arrests for extortion—a traditional source of yakuza income—around the enactment of the YEOs. We use obstruction of public order as a placebo outcome and find little change in this outcome. These results are consistent with a reallocation of yakuza activity away from traditional relationship-based rackets and toward fraud.

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 lowering the returns to traditional rackets and pushing yakuza toward new types of crime—particularly organized fraud jointly operated with non-yakuza criminal groups. Our model indicates

that when detection risk sufficiently constrains case expansion, additional fraud resources increase the amount extracted per case while having a small effect on the number of cases. It also suggests that the financial damage is larger in areas where fewer yakuza groups compete with each other.

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 not statistically significant. The effect on financial damage is stronger in prefectures with lower yakuza competition. As an extension, we find that the YEOs' effect varies jointly with regional yakuza competition and the predicted presence of non-yakuza groups; this does not directly identify the production complementarity assumed in the model. 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 higher-value and more 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 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 $K = \sum_{g=1}^{G} K_g$. Note that Φ is continuously differentiable on the region $K, L > 0$ 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

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

Then, we define the revenue share 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. If we discretize each input into the same number of identical "atoms" and apply the discrete Shapley value to these atoms, then let atom size vanish, the aggregate payment to the atoms belonging to input i converges to $\chi_i(x)$, the above line integral along the path $t \mapsto tx$. This preserves the classic Shapley-value properties in the limit. Indeed, the Aumann–Shapley payments χ_i satisfy, 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 (2). By calculus,

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

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

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

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,

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

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 (2):

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

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 $R > 0$, the fraud venture maximizes its revenue (1). By substitution, we rewrite it as

$$\Pi(R) = \max_{n\in[0,R]} \left\{ \mathsf{e}^{-cn} n^{1-q} \cdot (R-n)^q \right\}.$$

Since the objective equals zero at $n = 0, R$ and is strictly log-concave, it suffices to solve the

first-order condition (FOC):

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

Note that $0 < n^*(R) < R$ and $s^*(R) = R/n^*(R) - 1$. Thus,

$$\Pi(R) = \mathsf{e}^{-cn^*(R)} n^*(R)^{1-q} (R - n^*(R))^q. \tag{4}$$

Let $y = 1 - q - cn^*(R) \in (0, 1-q)$. The FOC gives

$$cR = \frac{q(1-q)}{y} + 1 - 2q - y, \qquad n^*(R) = \frac{1-q-y}{c}, \qquad s^*(R) = \frac{q}{y}. \tag{5}$$

By (5), y decreases with R. The envelope theorem and differentiation give

$$\eta \equiv \frac{R\Pi'(R)}{\Pi(R)} = q + y, \qquad h \equiv \frac{\mathsf{d}\log(R\Pi'(R))}{\mathsf{d}\log R} = \eta + \frac{R}{\eta}\frac{\mathsf{d}\eta}{\mathsf{d}R}.$$

Hence $q < \eta < 1$, $\mathsf{d}\eta/\mathsf{d}R < 0$, and $h < \eta < 1$, so Π is increasing and strictly concave.

For the first-stage game, let $P = \alpha\Pi(R)/[(\alpha+\beta)K]$ and $T = 1 - \alpha\eta$. For $K, L > 0$, yakuza group g's marginal payoff is

$$\frac{\partial \Pi_g}{\partial K_g} = P\left(1 - T\frac{K_g}{K}\right) - r - \frac{\kappa K_g}{m}. \tag{6}$$

Differentiating (6), using $0 < T < 1$, $K_g/K \le 1$, and $\mathsf{d}\eta/\mathsf{d}R < 0$, shows that each yakuza group's payoff is strictly concave in its own investment. The non-yakuza group's payoff is also strictly concave in L for $K > 0$, since Π is increasing and concave and $0 < \beta < 1$. By continuity, concavity extends to the boundaries, so the FOCs with boundary inequalities characterize global best responses.

At a positive equilibrium, P, T, K are common across groups. At these values, (6) decreases strictly with K_g, so the common bounds and optimality conditions imply $K_g = K/G > 0$, including when the upper bounds bind.

The non-yakuza group's optimality condition gives

$$L(R) = \min\left\{\frac{\beta^2\eta(R)\Pi(R)}{(\alpha+\beta)w}, 1\right\}, \qquad K(R) = \left(\frac{R}{L(R)^\beta}\right)^{1/\alpha}. \tag{7}$$

Since $h < \eta$ and $(\alpha+\beta)\eta < 1$, the elasticity $\mathsf{d}\log K/\mathsf{d}\log R$ equals $(1-\beta h)/\alpha > \eta$ when $L(R) < 1$ and $1/\alpha > \eta$ on intervals where $L(R) = 1$. Thus $K(R)$ strictly increases and $P(R)$ strictly decreases, with continuity preserving these monotonicities across switching points. Since $\eta(R)$ also decreases, $A_G(R) \equiv P(R)[1 - (1 - \alpha\eta(R))/G]$ strictly decreases.

Ignoring only the yakuza investment upper bound, the symmetric equilibrium condition is

$$A_G(R) = r + \frac{\kappa K(R)}{M}. \tag{8}$$

As $R \to 0$, (4), (5), and (7) imply that $\Pi(R)/R$ and $L(R)/\Pi(R)$ have finite positive limits. Thus $K(R)$ is asymptotically proportional to $R^{(1-\beta)/\alpha}$. Hence, $K(R) \to 0$ and $A_G(R) \to \infty$.

Since $K(R) \geq R^{1/\alpha}$, there is a unique R_M satisfying $K(R_M) = M$. If $A_G(R_M) < r + \kappa$, let R^* be the unique solution of (8) in $(0, R_M)$; otherwise set $R^* = R_M$. The profile $K_g^* = K(R^*)/G$, $L^* = L(R^*)$ satisfies the FOCs with the appropriate boundary inequalities and hence forms an equilibrium. □

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 (5), $y \to 0$ as $R \to \infty$. Hence, $n^*(R) \to (1-q)/c$ and $s^*(R) \to \infty$. By differentiation,

$$\frac{\mathsf{d}n^*(R)}{\mathsf{d}R} = \frac{y^2}{q(1-q)+y^2} > 0, \qquad \frac{\mathsf{d}s^*(R)}{\mathsf{d}R} = \frac{cq}{q(1-q)+y^2} > 0.$$

These derivatives are positive for every $R > 0$ and tend to 0 and $c/(1-q)$, respectively, as $R \to \infty$.

For Remark 1, the identities in (5) and the derivatives above give

$$\frac{\mathsf{d}\log(s^*(R)^q)}{\mathsf{d}\log n^*(R)} = c(R - n^*(R)) = \frac{q(1-q-y)}{y}.$$

By (5), this ratio exceeds one if and only if $cR > 2/(1+q)$.

A.3.2 Proposition 1

Since the YEOs' enactment is interpreted as a decline of the profitability r of traditional rackets, we show that $\partial R^*/\partial r < 0$. Differentiating (8) gives

$$R_r^* \equiv \frac{\partial R^*}{\partial r} = -\left[\frac{\kappa}{M}K'(R^*) - A_G'(R^*)\right]^{-1}.$$

Since $K'(R^*) > 0$ and $A_G'(R^*) < 0$, we have $R_r^* < 0$. Since $K'(R) > 0$, aggregate investment and each group's investment $K_g^* = K^*/G$ also increase when r decreases.

For the claim about non-yakuza investment, $L'(R) = Lh/R$ at an interior equilibrium. Since $[q(1-q)+y^2]h = (1-q)(q-y)^2 + (1-q)(3q-1)y + 2qy^2 + y^3$, we have $h > 0$ when $q \geq 1/3$. Under this condition, L^* also increases when r decreases.

A.3.3 Proposition 3

Recall that $D(R) = n^*(R)(s^*(R))^q$ is aggregate financial damage and that $\Pi(R) = \mathsf{e}^{-cn^*(R)}D(R)$ is the fraud venture's expected private return. The first-stage equilibrium is determined by $\Pi(R)$, while the empirical outcome corresponding to financial damage is $D(R)$. Since $K_g = K/G$ at a positive equilibrium, $\sigma_g = 1/G$ and hence $H = 1/G$; the proposition is therefore a comparison across the number G of groups, holding M and all other primitives fixed.

For the proof, we extend the equilibrium condition continuously. Let us write

$$\zeta \equiv -\frac{R}{\eta}\frac{\mathsf{d}\eta}{\mathsf{d}R} = \frac{y(1-q-y)}{q(1-q)+y^2}, \qquad u \equiv \frac{\mathsf{d}\log K}{\mathsf{d}\log R} = \frac{1-\beta h}{\alpha}.$$

Then, $h = \eta - \zeta$. Let

$$C = \frac{\kappa K}{M}, \quad z = \frac{C}{r+C}, \quad \lambda = \frac{\alpha\eta}{G-T}, \quad Q = (1+z)u - \eta + \lambda\zeta, \quad \gamma = \frac{T}{G(G-T)}.$$

Since $u > \eta$, we have $Q > 0$. Also, $\gamma > 0$ and $0 < \lambda \leq 1$. Since $R = K^\alpha L^\beta$ and $L \leq 1$, we have $C \geq (\kappa/M)R^{1/\alpha}$, so for sufficiently low r the cost condition $C \geq r$ holds in the large-R region and thus $z \geq 1/2$. Differentiating (8) gives

$$E_D \equiv -\frac{\partial D_G^*(r)}{\partial r} = \frac{RD'(R)}{(r+C)Q} > 0, \qquad \frac{1}{R}\frac{\partial R}{\partial G} = \frac{\gamma}{Q} > 0. \tag{9}$$

Let $\theta = \mathsf{d}\log(RD'(R))/\mathsf{d}\log R$. Differentiating the damage response along the equilibrium gives

$$\frac{Q^2}{\gamma}\frac{\partial \log E_D}{\partial G} = Q\left[\theta - zu + \left(\lambda + \frac{\alpha\eta}{T}\right)\zeta\right] - R\frac{\partial Q}{\partial R}. \tag{10}$$

Here partial derivatives with respect to R hold r and G fixed.

As $cR \to \infty$, $y \to 0$. Differentiating the stage-2 identities in (5) and the definitions above gives

$$\eta, \theta \to q, \qquad u \to u_\infty \equiv \frac{1-\beta q}{\alpha}, \qquad \zeta \to 0, \qquad R\frac{\partial Q}{\partial R} - z(1-z)u_\infty^2 \to 0.$$

Since $\alpha + \beta < 1$ and $q < 1$, we have $u_\infty - q = (1 - q(\alpha+\beta))/\alpha > 0$.

The right-hand side of (10) tends to $(u_\infty - q)(q - 2zu_\infty) \leq -(u_\infty - q)^2 < 0$ as $y \to 0$, uniformly over $z \in [1/2, 1]$ and $\lambda \in [0, 1]$. By continuity, $\partial E_D/\partial G < 0$ whenever cR is sufficiently large and $C \geq r$. The threshold for cR depends on α, β, q and can be chosen so that $h > 0$ also holds, since $h \to q > 0$.

Now we compare two numbers of yakuza groups. Recall that $A_G(R)$ increases with G at fixed R. The equilibrium characterization above implies that R and K increase in G. In the large-resource region, L also increases in R, since $h > 0$. Let $G_1 < G_2$ denote the numbers of yakuza groups. The interiority conditions at G_1 and G_2, together with the large-resource and cost conditions at G_1, ensure that the same conditions hold throughout $G \in [G_1, G_2]$. Integrating $\partial E_D/\partial G < 0$ over this

interval gives Proposition 3.

Finally, equilibrium damage increases in G, while E_D decreases. Consequently,

$$-\frac{\partial \log D_G^*(r)}{\partial r} = \frac{E_D}{D_G^*(r)}, \qquad -\frac{\partial \operatorname{asinh}(bD_G^*(r))}{\partial r} = \frac{bE_D}{\sqrt{1+b^2 D_G^*(r)^2}}, \qquad b>0,$$

also decrease with G. Thus the same ordering holds for the marginal effects on log damage and on IHS damage measured in any common monetary unit.

The same argument applies to the amount extracted per case. Replacing $D(R)$ with $s^*(R)^q$ in (9) and (10) changes only θ, and differentiating the stage-2 identities in (5) shows that it also tends to q as $y \to 0$. Hence $-\partial \pi_G^*(r)/\partial r$ decreases with G under the same conditions. Since $\pi_G^*(r)$ increases in G, the marginal effect on $\log \pi_G^*(r)$ also decreases with G.

B Arrest by Yakuza Members

We provide descriptive evidence on how yakuza economic activity changed around the introduction of the YEOs. We use annual ward-level data from the Tokyo Metropolitan Police Department on arrests of current yakuza members for fraud and extortion in Tokyo's 23 special wards from 2008 to 2013. We focus on the 23 special wards to reduce geographical heterogeneity across different jurisdiction types within Tokyo metropolis. Fraud arrests provide a proxy for yakuza involvement in fraud, whereas extortion arrests capture activity in a traditional relationship-based racket.

Because the YEOs were enacted throughout Tokyo at the same time, these data do not provide cross-sectional treatment variation. The resulting before-and-after analysis therefore cannot distinguish the YEOs from other contemporaneous Tokyo-wide shocks. We consequently treat this analysis as descriptive and motivating evidence. Arrest data can reflect both unobservable changes in law enforcement intensity and actual criminal behavior. If the YEOs increase police enforcement against yakuza members, changes in arrests could reflect heightened police activity. 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.

Identifying suitable cross-sectional variation is particularly difficult for fraud. As an opportunity-based rather than territory-based offense (Baradel, 2026), fraud need not occur within a group's established territory. Consequently, differences in yakuza presence across wards need not translate into corresponding differences in exposure to yakuza fraud. Thus, 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 likelihood (PPML) estimation. 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. We regress the conditional expectation as follows:

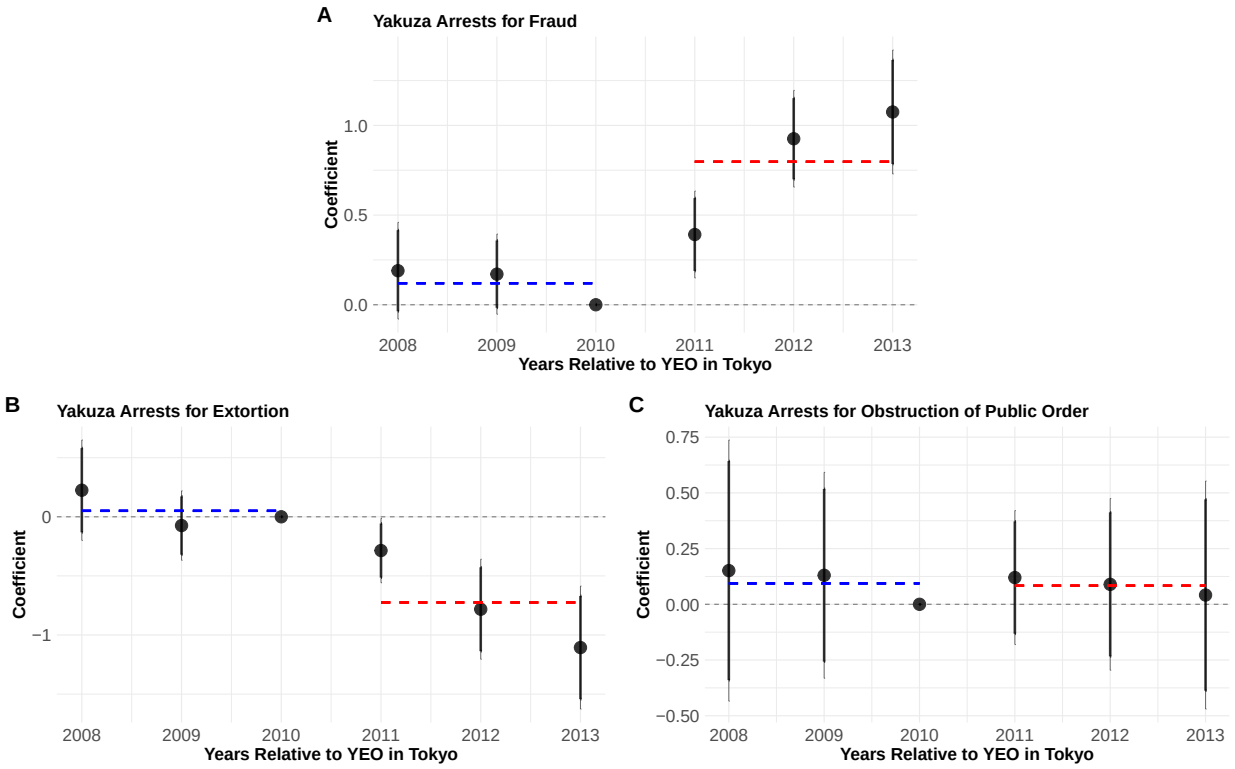
$$\ln \mathbb{E}\left[\mathsf{Arrest}_{wt} \mid Z\right] = \sum_{\tau=2008, \tau \neq 2010}^{2013} \alpha_\tau \mathsf{YEO}_{t=\tau} + \eta X_{wt} + \mu_w,$$

where Arrest_{wt} is the number of arrests of (current) yakuza members for fraud, extortion, or ob-

struction 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. Standard errors are clustered at the ward level.

Figure A1 reports the estimates with 90 and 95% confidence intervals. In Tokyo, arrests of yakuza members for fraud increased sharply after the YEOs' enactment, whereas arrests for extortion declined relative to the year before enactment. By contrast, our placebo outcome—yakuza arrests for obstruction of public order—shows no comparable change around enactment. The absence of a similar increase in the placebo outcome provides some reassurance that the divergence between fraud and extortion arrests does not simply reflect a general increase in policing intensity. These results are consistent with a reallocation of yakuza activity away from traditional relationship-based rackets and toward fraud.

Figure A1: YEOs' Effects on Arrests of Yakuza Members in Tokyo with 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 aged 65+, share of foreign population, share of welfare-recipient households, and log of taxable income. **Observations:** 138. **Confidence intervals:** thick bars are 90% and thin bars are 95% confidence intervals, based on standard errors clustered at the ward level.

C 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.[16] 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:

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

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 this effect to vary jointly with YCI and NYG by including $\Delta_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p$ and other relevant interactions:

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

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 $\Delta_{pt} \times \mathsf{YCI}_p \times \mathsf{NYG}_p$ would have a negative coefficient $\delta_4'' < 0$ if this YCI-based damping is weaker where non-yakuza groups are more prevalent, as in the triple-interaction estimates in Section 6.

[16] 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, in line with the triple-interaction estimates in Section 6.

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 association is consistent with the proposed former-member channel. Column (8) examines heterogeneity by YCI, showing a negative but insignificant coefficient. Column (9) shows a negative triple interaction, similar to that for regular-rank members.

Overall, while the results are somewhat noisy, they provide suggestive evidence consistent with the proposed former-member channel. The incarceration data do not directly identify former members or their participation in fraud.

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.

D Correlates of the YEOs Adoption Timing

We assess whether the timing of YEO adoption is systematically related to prefecture characteristics. We collapse the panel to one observation per prefecture and regress the enactment month, expressed as a monthly date index, on baseline characteristics in a cross-section. We estimate this in three samples: (i) our analytic sample, which excludes the four early-adopting prefectures (our fraud data begin in 2011, so we drop prefectures that enacted the YEOs before this period), (ii) the full sample of 47 prefectures, and (iii) the full sample excluding Fukuoka, the first prefecture to enact a YEO and historically well known for high yakuza activity. The covariates are the shares aged 19–39 and 65 or older, the foreign-resident share, unemployment, GDP, YCI, NYG, the illegal-market index, and police per capita.

Table A2 shows the results. In Column (1), the foreign-resident share enters negatively, and no other covariate is significantly related to enactment timing. In Column (2), which adds the early adopters, the coefficient on YCI becomes statistically significant, suggesting that early-adopting prefectures had a more competitive yakuza environment. In Column (3), the coefficient on YCI is no longer significant once Fukuoka is excluded. The relationship between yakuza-related factors and enactment timing is thus driven by the first prefecture to adopt. These timing results complement, but do not replace, the event-study and alternative staggered-adoption estimators reported in the main analysis and appendix.

Table A2: Correlates of the YEOs Enactment Timing

Dependent Variable: YEO Enactment Month (standardized)			
	(1)	(2)	(3)
	43 Prefecture (Analytic Sample)	47 Prefecture Sample	46 Prefecture (− Fukuoka Pref)
Share of Population Aged 19-39	-0.687	0.027	0.455
	(0.736)	(0.742)	(0.747)
Share of Population Aged 65+	-0.888	-0.169	0.154
	(0.752)	(0.717)	(0.715)
Share of Foreign Population	-0.732**	-0.451	-0.511*
	(0.328)	(0.273)	(0.273)
Unemployment Rate	-0.304	-0.420	-0.375
	(0.316)	(0.364)	(0.344)
GDP	-0.359	-0.763	-0.714
	(1.299)	(1.006)	(1.184)
YCI	-0.054	-0.558**	-0.495
	(0.265)	(0.264)	(0.306)
NYG	0.948	1.046	0.891
	(1.319)	(1.123)	(1.285)
Illegal-Market Index	-0.185	0.312	0.362
	(0.218)	(0.281)	(0.323)
Police per capita	0.148	-0.054	-0.036
	(0.342)	(0.308)	(0.346)
Observations	43	47	46
R-squared	0.432	0.374	0.337
Region Fixed Effects	✓	✓	✓

E 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 A3 presents the results, which qualitatively corroborate the main findings in Table 3.

Table A3: 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)
Panel C:	(1)	(2)	(3)	(4)
Dependent Variable: Financial Damage relative to Case Frequency				
YEO	0.5735**	1.8490*	2.1666***	1.0887***
	(0.2241)	(0.9722)	(0.6583)	(0.3067)

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)

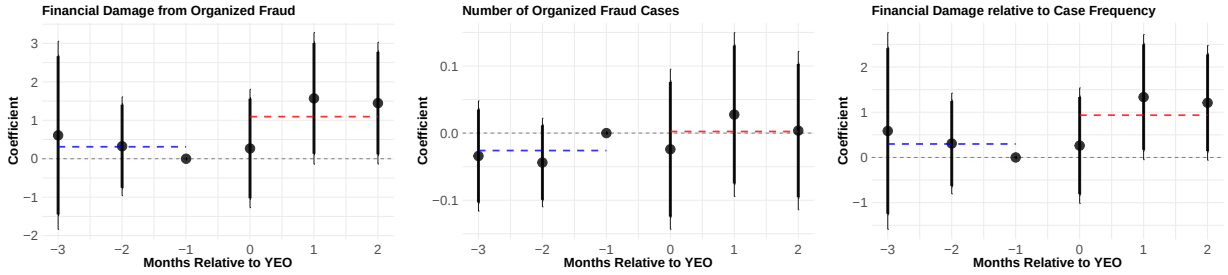
F Stacked Event-Study Design

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 A2 presents stacked event-study estimates and 90% confidence intervals, confirming the main findings.

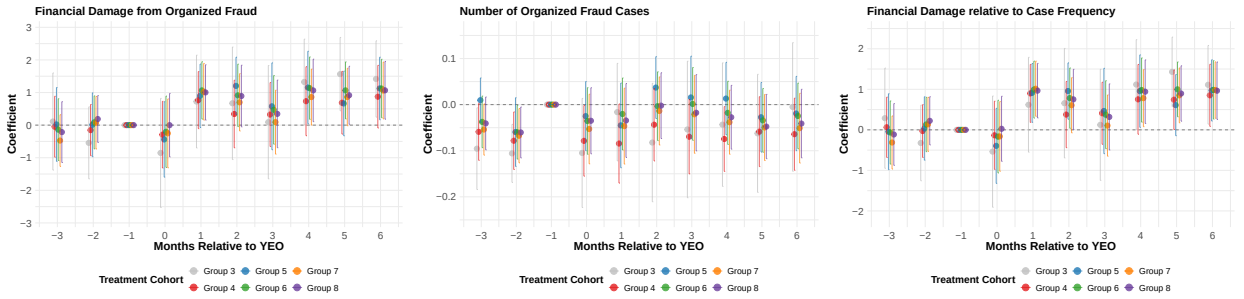
Figure A2: Stacked Event-Study Estimates



G 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 A3 reports the results. Each column corresponds to a specification that excludes one treatment timing group from the sample (and uses the remaining five). The exclusion of any treatment cohort hardly changes the behavior of the outcome.

Figure A3: Event Study with Leaving Out One Treatment Cohort at a Time

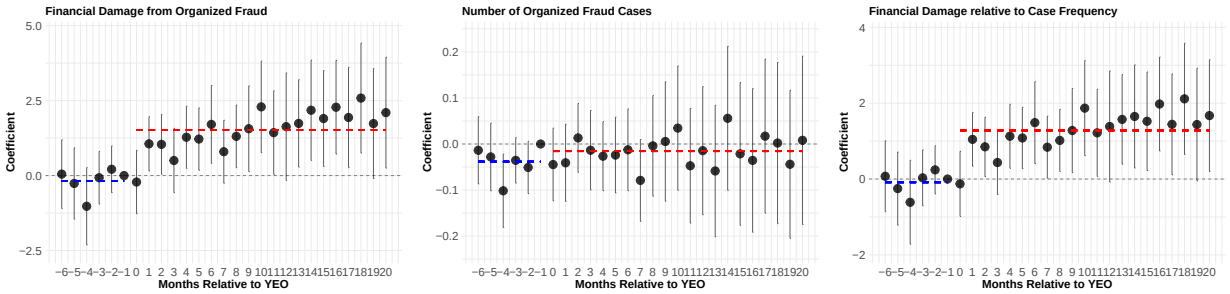


Treatment Cohort: Group 3 (Hokkaido, Miyagi, Ibaraki, Tochigi, Gunma, Kanagawa, Yamanashi, Fukui, Gifu, Aichi, Mie, Kyoto, Osaka, Hyogo, Tottori, Shimane, Okayama, Hiroshima, Yamaguchi, Tokushima, Kagawa, Kochi, Kumamoto, Oita); Group 4 (Aomori, Iwate, Akita, Fukushima, Nara, Wakayama); Group 5 (Yamagata,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

H Event Study with an Extended Time Window

We examine whether the effects of the YEOs persist over time. Figure A4 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. Nonetheless, it is important to note that this analysis only suggests persistence, because longer event-window horizons rely on comparisons among already-treated prefectures and therefore require stronger restrictions on longer effects and heterogeneity.

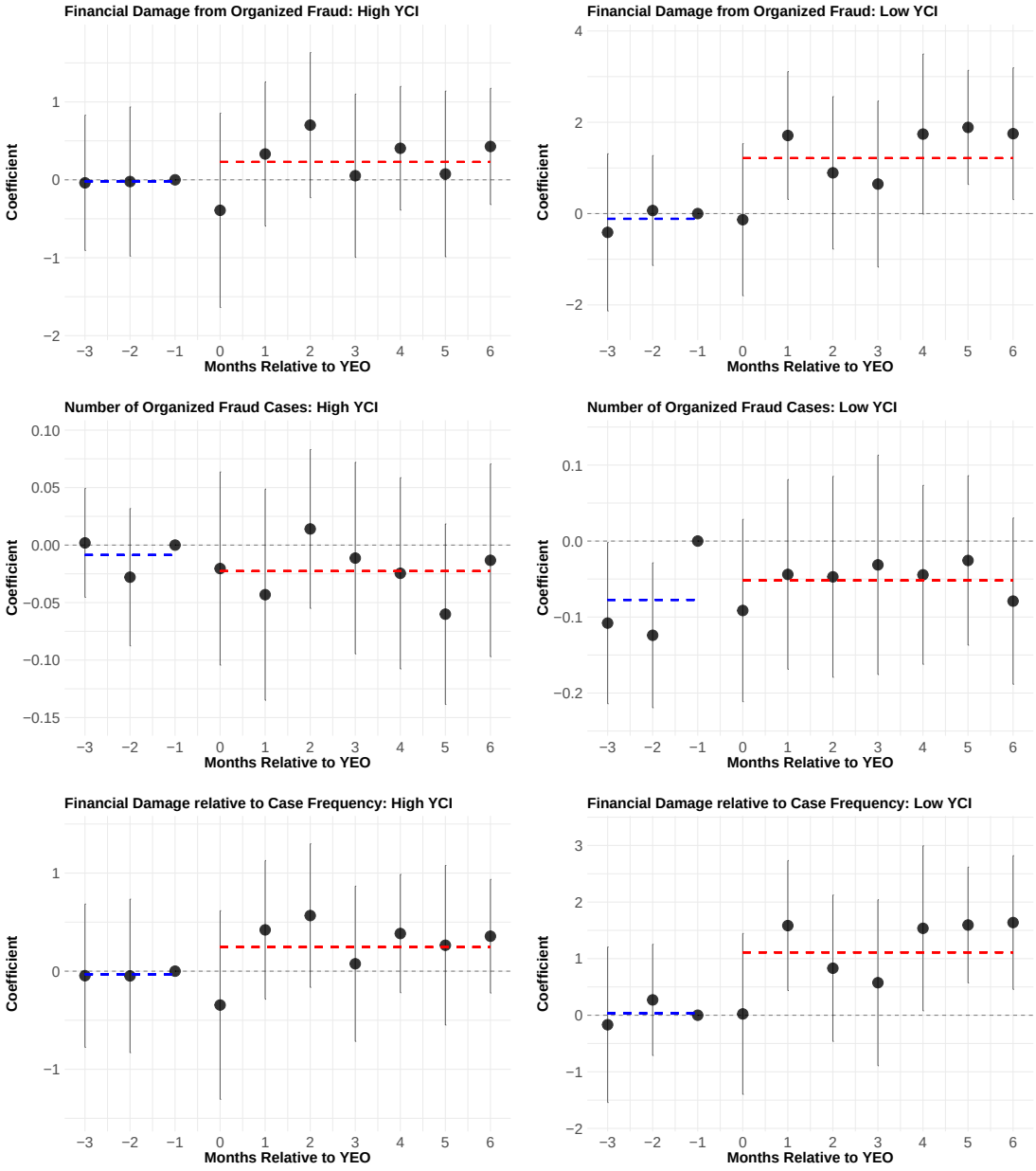
Figure A4: Event Study with an Extended Time Window



I Heterogeneity of YEOs' Effects by YCI

Table 4 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 A5 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 middle panel shows the number of organized fraud cases, and the bottom panel shows financial damage relative to case frequency. 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 A5: Event Study by YCI



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

J 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 A4 presents the results, showing a slightly larger coefficient and smaller standard errors compared to Column (5) of Table 3.

Table A4: 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)
Panel C:	(1)
Dependent Variable:	Financial Damage relative to Case Frequency
YEO	0.6928**
	(0.3309)

K Alternative Transformations of Financial Damage Relative to Case Frequency

Table A5 examines alternative transformations of financial damage relative to case frequency. Our main measure is the IHS of financial damage minus the IHS of cases, shown in Column (1). Because the IHS accommodates zeros, it uses the full analytic sample. Column (2) replaces it with the IHS of damage per case, which drops cells with zero cases. Column (3) uses the log of damage per case, which drops cells with zero cases or zero damage. Column (4) instead uses the IHS of financial damage as the dependent variable while controlling for the IHS of cases. They all show qualitatively similar results.

Table A5: Alternative Transformations of Financial Damage Relative to Case Frequency

	(1) IHS(Damage) − IHS(Cases)	(2) IHS(Avg. damage/case)	(3) log(Avg. damage/case)	(4) IHS(Damage) (control IHS(Cases))
YEO	0.6290***	0.3747***	0.3594**	0.4689***
	(0.2246)	(0.1337)	(0.1364)	(0.1636)
IHS(Cases)				2.6303***
				(0.1830)
Observations	1,548	1,479	1,474	1,548
R-squared	0.317	0.269	0.295	0.766
Sample	all cells	Cases>0	Cases>0, Dmg>0	all cells

L Subcategories of Organized Fraud

Our model predicts that the YEOs can result in shifting organized fraud toward higher-value and more financially damaging schemes. We test this prediction by examining subcategories of organized fraud. Organized fraud is broadly divided into two types: urgent or routine-type fraud and investment-type fraud. The first category includes relative-impersonation (family-emergency), fictitious-billing, loan-guarantee, and refund schemes. Investment-type fraud includes fraudulent offers of financial products or investments, gambling-tip schemes, and romance-introduction schemes. In Table A6, compared with urgent or routine-type fraud, investment-type fraud has fewer cases, slightly lower mean total financial damage per prefecture-month, and higher median total financial damage per prefecture-month. Both mean and median damage per case are higher for investment-type fraud. In Table A7, we find that the increases in both total financial damage and damage relative to case frequency are driven mainly by investment-type fraud. We interpret these results as consistent with a shift toward higher-value fraud following the YEOs.

Table A6: Summary Statistics: Subcategories of Organized Fraud

	(1) Urgent or Routine-Type	(2) Investment-Type
Panel A: Financial damage (million yen)		
Mean	34.4	30.4
Std. dev.	88.3	48.7
Median	5.6	11.7
Panel B: Cases per prefecture-month		
Mean	13.6	3.6
Std. dev.	31.7	4.4
Median	3.0	2.0
Panel C: Damage per case (million yen)		
Mean	2.18	8.17
Median	1.61	6.00
Observations (prefecture-months)	1,548	1,548
Share of all special-fraud cases (%)	79.1	20.9
Share of all special-fraud damage (%)	53.1	46.9

Table A7: Subcategories of Organized Fraud

	Urgent or Routine-Type	Investment-Type
Panel A:	(1)	(2)
Dependent Variable: Financial Damage from Organized Fraud		
YEO	-0.1330	0.9745*
	(0.3474)	(0.4996)
R-squared	0.5140	0.5018
Panel B:	(1)	(2)
Dependent Variable: Number of Organized Fraud Cases		
YEO	-0.0029	0.0048
	(0.0244)	(0.0102)
R-squared	0.6563	0.4613
Panel C:	(1)	(2)
Dependent Variable: Financial Damage relative to Case Frequency		
YEO	-0.0938	0.8888**
	(0.2766)	(0.4360)
R-squared	0.3380	0.4381
Observations	1,548	1,548
Prefecture Fixed Effects	✓	✓
Month × Region and Year × Region Fixed Effects	✓	✓
Control Variables	✓	✓

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. **Outcome in Panel C:** IHS of financial damage minus IHS of the raw number of organized fraud cases (unscaled monthly count, not the per-100,000-resident rate in Panel B).

M Placebo Outcome

We conduct a placebo exercise using deaths from natural causes, excluding external causes (accidents, suicide, homicide, and other injuries of external origin). Some external causes could potentially respond to the YEOs through changes in violence, financial distress, or other indirect channels. By contrast, we expect that natural deaths are hardly related to the YEOs. At the same time, natural mortality may reflect demographic and socioeconomic conditions relevant to organized fraud. For example, older people face higher natural mortality risks and are also frequent targets of organized fraud. Our baseline specification controls for the elderly population share, but this measure may not fully capture differences in age composition, health, and living conditions correlated with organized fraud. The placebo exercise therefore provides a supplementary check for broader contemporaneous changes that may also influence organized fraud, although it does not establish the absence of fraud-specific confounding. In Table A8, we find no statistically significant effect of the YEOs on natural deaths.

Table A8: Natural Causes of Deaths As a Placebo Outcome

	(1)
Dependent Variable:	Natural Causes of Deaths
YEO	0.0016
	(0.0064)
R-squared	0.9925

Outcome: IHS of natural causes of deaths per 100,000 residents, where natural causes of deaths are all deaths minus accidents, suicide, homicide, and other injuries of external origin. Mean: 92.27 (before IHS transformation), Std.Dev.: 54.91 (before IHS transformation). **Controls:** Share of population aged 19-39, share aged 65+, share of foreign population, unemployment rate, and GDP. **Sample and Observations**: month-year 2009-2013, 2,820.

N 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 each group's members are distributed proportionally to population across the prefectures where it operates.

- **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 5-6). Second, we assume members are distributed proportionally to population density, yielding $s_{pg}^{\text{pop density}}$ and $\text{YCI}_p^{\text{pop density}}$ (Columns 3-4).
- **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 A9 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 A9: 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
PANEL C:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable: Financial Damage relative to Case Frequency										
YEO	1.2117***	0.6823*	0.6024***	0.2858	0.6072***	0.1902	0.5998***	0.3005	0.6163***	0.3261
	(0.3554)	(0.3723)	(0.2127)	(0.2205)	(0.2122)	(0.2209)	(0.2139)	(0.2108)	(0.2131)	(0.2141)
YEO × NYG		-1.7122**		-0.9952**		-1.2320***		-0.7666***		-0.9343**
		(0.7952)		(0.3945)		(0.4363)		(0.2474)		(0.4063)
YEO × **1** (YCI > median)	-1.0317**	-0.4818								
	(0.3860)	(0.4071)								
YEO × **1** (YCI > median) × NYG		1.5996*								
		(0.7941)								
YEO × $\text{YCI}^{\text{pop density}}$			-0.6671***	-0.2195						
			(0.1972)	(0.1818)						
YEO × $\text{YCI}^{\text{pop density}}$ × NYG				0.9006**						
				(0.3651)						
YEO × $\text{YCI}^{\text{naive}}$					-0.6619***	-0.1351				
					(0.1995)	(0.1747)				
YEO × $\text{YCI}^{\text{naive}}$ × NYG						1.1014***				
						(0.4056)				
YEO × EP							-0.5889***	-0.0960		
							(0.2033)	(0.1504)		
YEO × EP × NYG								1.2884***		
								(0.4554)		
YEO × # of Designated Yakuza Groups									-0.6545***	-0.4270**
									(0.1639)	(0.1685)
YEO × # of Designated Yakuza Groups × NYG										0.5638**
										(0.2306)
R-squared	0.3219	0.3280	0.3259	0.3307	0.3254	0.3311	0.3242	0.3296	0.3257	0.3305
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 (Columns 1–2). **$\text{YCI}^{\text{pop density}}$**: Competition index computed by distributing group g's members in proportion to population density across prefectures where group g operates (Columns 3–4). **$\text{YCI}^{\text{naive}}$**: Competition index computed by distributing group g's members equally across prefectures where group g operates (Columns 5–6). **EP**: Polarization-style index analogous to the ethnic polarization measure Montalvo and Reynal-Querol (2005) (Columns 7–8). **# Designated Yakuza Groups**: number of designated yakuza groups operating in prefecture (Columns 9–10).

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

As detailed in Appendix U, 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 three alternative measures: (i) the linear predictor from the cross-validation-selected model, which captures predictions on the log scale rather than the count scale, (ii) predicted incidence rates using a fixed penalty parameter ($\lambda = 0.1$), instead of the cross-validation-selected value, which tests sensitivity to the regularization strength, and (iii) out-of-sample predicted values from a leave-one-prefecture-out procedure. For (iii), we re-estimate the Poisson LASSO model, 47 times, each time omitting one prefecture, and predict the omitted prefecture's count from the model fitted to the remaining 46. A prefecture's own observed number of groups therefore is not included as its own predicted value. This can alleviate a potential concern that the in-sample prediction can reflect prefecture-specific outcomes measured after the ordinances. We standardize the out-of-sample measure using the mean and standard deviation of the main measure, so that the coefficients are comparable to those in Table 4. Column (3) excludes Tokyo: when Tokyo is held out, its GDP lies far outside the range of the remaining prefectures, and the model extrapolates to an implausible predicted count (172, against an observed count of 14). For the other 46 prefectures, the out-of-sample and in-sample predictions are nearly identical.

Table A10 presents the results of these robustness checks. Across these alternative measures, the coefficient on the interaction term between the YEOs, YCI, and non-yakuza criminal group index remains positive and significant.

Table A10: Robustness to Alternative Measures of Non-Yakuza Criminal Group Index

Panel A:	(1)	(2)	(3)
Dependent Variable: Financial Damage from Organized Fraud			
YEO × YCI (SD)	-0.3063	-0.5930**	-0.3728**
	(0.2040)	(0.2269)	(0.1832)
YEO × NYG (Linear Predictor, SD)	-0.9104***		
	(0.3227)		
YEO × YCI (SD) × NYG (Linear Predictor, SD)	0.7696**		
	(0.2979)		
YEO × NYG (Fixed, SD)		-0.4936	
		(0.3031)	
YEO × YCI (SD) × NYG (Fixed, SD)		0.5138*	
		(0.3010)	
YEO × NYG (Out-of-Sample, SD)			-1.0859**
			(0.4818)
YEO × YCI (SD) × NYG (Out-of-Sample, SD)			0.9041*
			(0.4699)
R-squared	0.5295	0.5268	0.5092
Panel B:	(1)	(2)	(3)
Dependent Variable: Number of Organized Fraud Cases			
YEO × YCI (SD)	-0.0100	-0.0071	-0.0193
	(0.0275)	(0.0241)	(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)		-0.0182	
		(0.0217)	
YEO × YCI (SD) × NYG (Fixed, SD)		0.0471*	
		(0.0241)	
YEO × NYG (Out-of-Sample, SD)			-0.0187
			(0.0330)
YEO × YCI (SD) × NYG (Out-of-Sample, SD)			-0.0052
			(0.0455)
R-squared	0.6085	0.6088	0.5483
Panel C:	(1)	(2)	(3)
Dependent Variable: Financial Damage relative to Case Frequency			
YEO × YCI (SD)	-0.2132	-0.4792**	-0.2491*
	(0.1552)	(0.1780)	(0.1411)
YEO × NYG (Linear Predictor, SD)	-0.8113***		
	(0.2822)		
YEO × YCI (SD) × NYG (Linear Predictor, SD)	0.6970**		
	(0.2626)		
YEO × NYG (Fixed, SD)		-0.4214*	
		(0.2448)	
YEO × YCI (SD) × NYG (Fixed, SD)		0.4236	
		(0.2626)	
YEO × NYG (Out-of-Sample, SD)			-0.9604**
			(0.4066)
YEO × YCI (SD) × NYG (Out-of-Sample, SD)			0.9018**
			(0.4135)
R-squared	0.3314	0.3268	0.3301
Observations	1,548	1,548	1,512
Tokyo Excluded	—	—	✓

NYG (Linear Predictor): the linear predictor $x\hat{\beta}$ (the log of the predicted count, $\log \hat{\mu}$) from the model selected by 10-fold cross-validation. **NYG (Fixed)**: Non-Yakuza Criminal Group Index, an index of predicted non-yakuza group counts constructed from baseline covariates using a Poisson LASSO model. This measure uses predicted incidence rates with a fixed penalty parameter ($\lambda = 0.1$). **NYG (Out-of-Sample)**: Non-Yakuza Criminal Group Index, an index of predicted non-yakuza group counts constructed from baseline covariates using a Poisson LASSO model. This measure uses out-of-sample predicted values, obtained by re-estimating the cross-validation selected model for each prefecture on the remaining 46 prefectures. Column (3) excludes Tokyo.

P 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 A11 presents the results of these robustness checks. In Panels A (financial damage) and C (damage relative to case frequency), the YEO–YCI interaction remains negative and significant in Columns (1) and (3), and the triple interaction remains positive and significant in Columns (2) and (4). The only exception is Column (4) in Panel C. The heterogeneous effects with respect to YCI and NYG are largely robust to the inclusion of these additional enforcement measures.

Table A11: 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
Panel C:	(1)	(2)	(3)	(4)
Dependent Variable: Financial Damage relative to Case Frequency				
YEO	0.5739***	0.3829*	0.5753***	0.3254
	(0.2063)	(0.2099)	(0.2089)	(0.2244)
YEO × YCI (SD)	-0.6212***	-0.1663	-0.5743**	-0.1993
	(0.1787)	(0.1581)	(0.2401)	(0.2173)
YEO × NYG (SD)		-0.9555***		-0.8311*
		(0.3530)		(0.4587)
YEO × YCI (SD) × NYG (SD)		0.8844***		0.7073
		(0.3246)		(0.4440)
Anti-Yakuza Laws Revision × YCI (SD)	-0.1286	0.1016		
	(0.1264)	(0.1553)		
Anti-Yakuza Laws Revision × NYG (SD)		-0.5563**		
		(0.2282)		
Anti-Yakuza Laws Revision × YCI (SD) × NYG (SD)		0.3780*		
		(0.1981)		
Obama Enforcement × YCI (SD)			-0.1073	0.0072
			(0.1957)	(0.2185)
Obama Enforcement × NYG (SD)				-0.2373
				(0.3503)
Obama Enforcement × YCI (SD) × NYG (SD)				0.2804
				(0.3257)
R-squared	0.3263	0.3219	0.3259	0.3311
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.

Q Robustness to Heterogeneity Conditional on Demographic Characteristics

The main text examines heterogeneity conditional on other criminal activity and enforcement capacity. Here we examine heterogeneous effects of the YEOs by YCI and NYG conditional on demographic characteristics: the share of the elderly population, population density in habitable land, and total population. These variables also capture criminal opportunity. The elderly are the primary targets of organized fraud, and opportunities are generally more ample in urban and more populous areas. Table A12 shows the results. The heterogeneous effects by YCI, and by YCI and NYG jointly, survive conditioning on demographic characteristics.

Table A12: Robustness to Heterogeneity Conditional on Demographic Characteristics

PANEL A:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Financial Damage from Organized Fraud						
YEO	0.6774**	0.6718**	0.6778**	0.3629	0.2975	0.3338
	(0.2635)	(0.2689)	(0.2724)	(0.2520)	(0.2612)	(0.2780)
YEO × YCI (SD)	-0.7939***	-0.7523***	-0.6828**	-0.4064*	-0.2682	-0.2882
	(0.2276)	(0.2526)	(0.2652)	(0.2051)	(0.2104)	(0.2222)
YEO × NYG (SD)				-0.8573*	-1.2708**	-0.7599
				(0.4527)	(0.5315)	(0.5610)
YEO × YCI × NYG				0.8695**	1.0731**	0.9293**
				(0.4061)	(0.4167)	(0.4435)
YEO × Age (65+)	0.4846***			0.3857**		
	(0.1515)			(0.1746)		
YEO × Urban		-0.1706			0.1298	
		(0.1689)			(0.3390)	
YEO × Population			-0.2803			-0.3808
			(0.1936)			(0.2852)
R-squared	0.5284	0.5264	0.5269	0.5302	0.5291	0.5293
PANEL B:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Number of Organized Fraud Cases						
YEO	0.0038	0.0024	0.0026	0.0019	-0.0094	-0.0044
	(0.0240)	(0.0243)	(0.0243)	(0.0240)	(0.0278)	(0.0286)
YEO × YCI (SD)	-0.0168	-0.0227	-0.0202	-0.0307	-0.0066	-0.0119
	(0.0203)	(0.0225)	(0.0230)	(0.0228)	(0.0287)	(0.0276)
YEO × NYG (SD)				0.0356	-0.0453	0.0277
				(0.0286)	(0.0433)	(0.0474)
YEO × YCI × NYG				-0.0019	0.0346	0.0141
				(0.0270)	(0.0289)	(0.0336)
YEO × Age (65+)	0.0448***			0.0609***		
	(0.0161)			(0.0147)		
YEO × Urban		0.0221**			0.0362	
		(0.0097)			(0.0286)	
YEO × Population			0.0079			-0.0370
			(0.0149)			(0.0292)
R-squared	0.6101	0.6086	0.6082	0.6115	0.6089	0.6088
PANEL C:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Financial Damage relative to Case Frequency						
YEO	0.5867***	0.5847***	0.5891***	0.2806	0.2525	0.2729
	(0.2081)	(0.2109)	(0.2128)	(0.2137)	(0.2155)	(0.2259)
YEO × YCI (SD)	-0.6498***	-0.6033***	-0.5524**	-0.2497	-0.1916	-0.1899
	(0.1857)	(0.2030)	(0.2150)	(0.1763)	(0.1664)	(0.1765)
YEO × NYG (SD)				-0.8923**	-1.0193**	-0.7483
				(0.4139)	(0.4835)	(0.4971)
YEO × YCI × NYG				0.8563**	0.9364**	0.8601**
				(0.3768)	(0.3824)	(0.3972)
YEO × Age (65+)	0.3161**			0.1961		
	(0.1235)			(0.1466)		
YEO × Urban		-0.1871			-0.0183	
		(0.1469)			(0.2709)	
YEO × Population			-0.2444			-0.2877
			(0.1586)			(0.2391)
R-squared	0.3278	0.3265	0.3269	0.3314	0.3307	0.3310
Observations	1,548	1,548	1,548	1,548	1,548	1,548

R 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 A13 presents the results. We obtain qualitatively similar results to our main specification, confirming the stability of our findings.

Table A13: 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).

S 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.

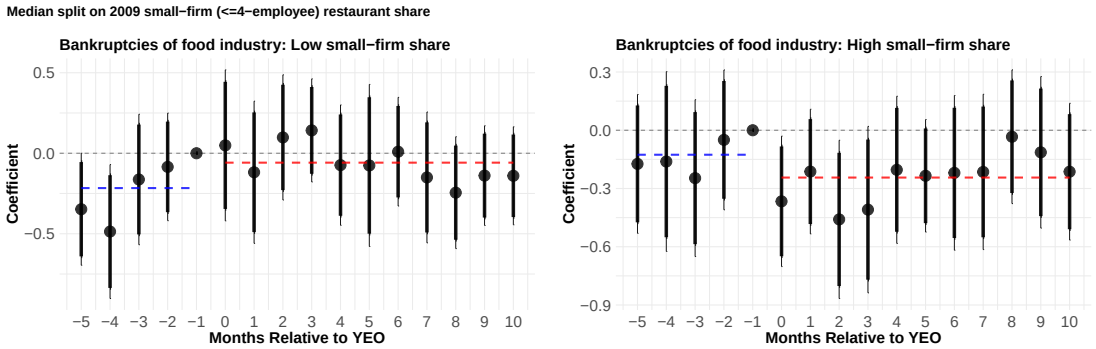
Table A14: Robustness to Alternative Estimator

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

T Event-Study Results for Food-Industry Bankruptcies

This section presents event-study results for food-industry bankruptcies in prefectures with high and low shares of small firms. Figure A6 corroborates the main findings in Table 5, indicating that the YEOs reduce food-industry bankruptcies in prefectures with a high share of small firms.

Figure A6: Event Study by Share of Small Firms



Low Share: Prefectures with a below-median share of small firms. **High Share:** Prefectures with an above-median share of small firms.

U 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. These figures should therefore be treated as indicative. 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 the NYG index, 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 A15 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 A15: Baseline Predictors of Non-Yakuza Criminal Groups

Predictors	Lasso Coefficient
Serious Violent Crime	0.0814
GDP	0.1580
Constant	0.5274

V 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 A16 lists all the designated yakuza groups. In the table, "#pref. of influence" denotes the number of prefectures in which the corresponding yakuza group operates.[17]

Table A16: 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		

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

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

Table A17: 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.